

**Manufacturing sector productivity in
South Africa in the 1980s:
Error and ideology in a contested terrain**

Volume I

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ABSTRACT

Estimates of the value of manufacturing sector output enter into many economic indices, especially those measuring productivity. The South African Central Statistical Services has twice made substantial errors in the output series. Revisions to correct the first of these raised the growth rate in manufacturing over the period 1970-80 from 2,6 per cent per annum (compound) to 5 per cent. This episode is not common knowledge.

After examining the conceptual difficulties involved in producing output estimates, a practical technique for detecting errors in the series, the Euler Consistency Test, is presented. Developed, refined, and then applied to the South African data, it predicted, retrospectively, the first set of errors (using only the information available at the time those errors were made), then detected another set of errors, not previously known to exist. The study records the process by which the CSS was made to concede this second error.

Acknowledgement only came after protracted correspondence and an examination conducted by a special committee formed to investigate my complaints. With 1979 set equal to 100, the output level in 1988 was originally given as 113,8. After investigation, the CSS raised this to 126,1. The magnitude of this second error is equivalent to the omission of the total output of the two SASOL plants commissioned during the early 1980s.

Estimates of productivity growth by the National Productivity Institute using these incorrect figures are shown to have created a misleading picture of the sector's performance, especially in the sensitive debate over the relationship between wage and productivity growth. An attempt is made to lay the groundwork of an analytical framework for comprehending (from a Marxist point of view) the activities of ideological state apparatuses like the NPI.

A review of the literature on theory choice is conducted, and the necessarily political nature of this activity is explored. The relative impotence of 'science' in the face of ideology in a conflict-ridden society is considered. The question of the significance of disagreements between economists is examined, and prospects for convergence and consensus on certain issues are weighed.

This work is dedicated to**Liz Johnston**

Without her sacrifices - the willingness to do almost all the child care, to work extra hours to make ends meet - the opportunity for me to complete a primary degree in mid-life would simply not have arisen. Nor would the chance to change careers - made possible by the honours degree which could be pursued full-time because of her assumption of the additional role of breadwinner. Her generosity created a 'university family' - Linda, Deanna, Paula and Fiona, our daughters, are all graduates of this university. Higher learning has become a way of life for each of them. Although they may have been able to achieve this anyway, it would have been at much greater cost - Liz' willingness to consider herself last of all, has eased for them, the difficult route to academic success.

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sympathetic and supportive. Nick Wilkins sorted out every computer hassle that plagued me, was coffee maker during the lonely night shifts, and good companion to boot. Finally, to Nic Amin - sparring partner in the field of political economy, friend, reader of last resort.

In accordance with the regulations of the University of Natal, I certify that the contents of this thesis are my own original work unless specifically indicated to the contrary in the text.

I further declare that this thesis has not been presented to any other university.

Signed..... 

Date..... *23 June 1995*

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ANC	African National Congress
BLS	Bureau of Labor Statistics
CEAS	Central Economic Advisory Service
CEO	Chief executive officer
CES	Constant elasticity of substitution
CLMPC	Canadian Labour Market and Productivity Council
Cosatu	Congress of South African Trade Unions
CPI	Consumer Price Index
CPS	Centre for Policy Studies
CSS	Central Statistical Services
CSO	Central Statistical Office
DA	Domestic Absorption
EASy	Economic Analysis Systems
EAC	Economic Advisory Council
ECT	Euler Consistency Test
EDP	Economic Development Programme
ERD	Exchange Rate Deviation index
ET	Economic Trends
Fosatu	Federation of South African Trade Unions
GDI or GDY	Gross Domestic Income
GDP	Gross Domestic Product
GDP(E)	Gross Domestic Product (estimated by the expenditure method)
GDP(I)	Gross Domestic Product (estimated by the income method)
GDP(P)	Gross Domestic Product (estimated by the production method)
GDP(A)	Gross Domestic Product (the average of these three)
GNP	Gross National Product
GNU	Government of National Unity
HM	Her Majesty's (Government)
ICOP	International Comparison of Output and Productivity
IDC	Industrial Development Corporation
IEA	Institute of Economic Affairs
IGDPD	Implicit Gross Domestic Product Deflator
IMF	International Monetary Fund
ISA	Ideological state apparatus

ISI	Import substituting industrialisation
ISP	Industrial Strategy Project
MSI	Monthly Sales Inquiry
MW	Minimum wage
Naledi	National Labour and Development Institute
NEDLC	National Economic Development and Labour Council
NEF	National Economic Forum
NFB	Net Foreign Balances
NIC	Newly-industrialising country
NIESR	National Institute for Economic and Social Research
NMC	National Manpower Commission
NPI	National Productivity Institute
NPI, AR	National Productivity Institute, Annual Report
NUM	National Union of Mineworkers
NUMSA	National Union of Metalworkers of South Africa
OECD	Organisation for Economic Co-operation and Development
OC	Other Chemicals
PAC	Productivity Advisory Council
PC or P.C.	President's Council
PDE	Perverse Deflator Effect
PPI	Production Price Index
PPP	Purchasing Power Parity
PVP	Physical Volume of Production
PVMP	Physical Volume of Manufacturing Production
RDP	Reconstruction and Development Programme
RGDP	Real Gross Domestic Product
RGNP	Real Gross National Product
REALST	Resource Allocation Strategist
RNDI	Real National Disposable Income
SA or S.A.	South Africa
SABC	South African Broadcasting Corporation
SABS	South African Bureau of Standards
SACTU	South African Congress of Trade Unions
SARB	South African Reserve Bank
SIC	Standard Industrial Classification (of all economic activities)
SNA	System of National Accounts
SNR	Statistical News Release

ToT	Terms of Trade
Tucsa	Trade Union Council of South Africa
UK	United Kingdom
UN	United Nations
US or USA	United States of America

PART I

METHODOLOGY, IDEOLOGY AND THE ABUSE OF STATISTICS

"The trouble with people is not that they don't know but that they know so much that ain't so." (Josh Billings, cited in Atkinson, 1982, p49)

Chapter 1-1

Aims and structure of the study

Research into the extent of error in official statistics is comparatively rare,¹ primarily because of the extreme difficulty of producing reliable estimates of the quality of these artifacts. It may be an indication of the inherent problems of the topic that Morgenstern's classic work, *On the Accuracy of Economic Observations* first published in 1963, still towers above almost everything else in the field. Progress has been made, but many of the problems with which Morgenstern was concerned remain unresolved today.² Research into the misuse of statistics, either deliberate or unintentional, in an academic discipline like economics is also relatively rare. Paradoxically, ordinary citizens (ie, those without specialised academic training), or their political representatives, seem much more ready to challenge the authorities over suspected abuses of statistics. The controversy surrounding the 1991 Population Census, or the doubts raised in recent years about the accuracy of the Consumer Price Index are cases in point. This dissertation deals primarily with problems that have arisen in the estimation of manufacturing sector output. It looks briefly as well, at the (mis)use by the National Productivity Institute (NPI) of a set of manufacturing sector output estimates acknowledged by the Central Statistical Services (CSS) (after a lengthy battle) to be incorrect. The specific focus is the period 1979-89, although it is necessary to go further back (to about 1970) to savour fully the extent of these errors.

-
- 1 Because of their rather obvious political connotations in South Africa, statistics in this country have attracted more than the usual number of critics. The saga of the unemployment statistics and the rôle of Charles Simkins in it is fairly well known (Meth, 1988). Solid work on a range of problems, many of them still unresolved today, has been done by Roukens de Lange. His early work on employment and income contains detailed and sensible recommendations for improvements (1979, pp27ff), most of which, unfortunately, have not been taken up.
 - 2 A review of developments since the publication of Morgenstern's pathbreaking working may be found in Griliches (1986). See Mizon (1991) for a discussion of the impact of poor measurement on theory.

The way in which the material with which this study is concerned, manufacturing sector output statistics, came gradually to dominate the larger research project of which it is but one component - an examination of the quality and use of productivity statistics in South Africa - was quite roundabout. In going back over that history to lay bare the process of selection which yielded the results reported in the study, a number of questions on research method are raised. To some of these, there are no easy answers. A secondary focus of the study is thus on matters methodological and political,³ in particular, in the manner in which commitment to a particular analytical framework comes to be influenced by the material setting in which research is conducted.

As research topics go, the study of errors in a single statistical series is not especially, or maybe not even at all, outlandish - many, far more obscure areas have come under detailed scrutiny. Outlandish or not, there is some point in attempting to understand how a particular field of inquiry comes to be settled upon. The precise ways in which research questions are formulated are not always, or possibly seldom straightforward. Clearly, the motives of someone who has replicated a study performed elsewhere, worthy though the results of such an enterprise may be, would not excite much attention. If the driving force is idiosyncratic, as might be the case where a researcher has some or other personal connection with the subject, the attempt to understand this would draw more on psychology than on economics. Where, however, the sheer complexities of socio-economic phenomena and the difficulties of comprehending them theoretically⁴ play an important part in shaping the topic, theory, empirical work and the politics of research can interact in quite interesting ways.

3 Henceforth, when the terms 'politics' and 'economics' are used in this study, unless the context suggests otherwise, they are intended to be understood in a specific - Marxist - way. Marxist analysis insists that:

"...the separation between the political, economic, social, and cultural parts of the social whole is artificial and arbitrary, so that, for instance, the notion of 'economics' as free from 'politics' is an ideological abstraction and distortion. There is no such thing as 'economics' - only 'political economy', in which the 'political' element is an ever-present component.

On this view, politics is the pervasive and ubiquitous articulation of social conflict, and particularly of class conflict, and enters into all social relations, however these may be designated."

To prevent this "pervasiveness" of the political from paralysing social inquiry,

"...it is perfectly possible to treat politics as a specific phenomenon, namely as the ways and means whereby social conflict and notably class conflict is manifested." (Miliband, 1977, p6)

Note that this definition leaves plenty of space for conflicts other than class struggle, eg, race or gender struggles, which may, at times, assume greater prominence. The insistence on class conflict, though, is a reminder that in class societies like capitalism, class conflict can never be eradicated. Thus in South Africa, even if the rather obvious hand of the apartheid régime in almost every sphere of the labour market had not been present, there would still be good reason to describe wage and other struggles as 'political'.

The concept of ideology will be addressed at some length in Chapter 1-5. For the meanwhile, the following definition, drawn from Larrain (1979, p210), makes it clear what is intended by the use of the term:

"Ideology is not a simple error. It is a particular kind of distortion, dependent upon real contradictions, which demands their solution in practice before it can be overcome. Science itself cannot overcome ideology. Yet it is through science that ideology can be understood..."

4 'Theory' is used loosely here, indeed, there is no simple agreed meaning to the term as it is used in the social sciences. An attempt to separate theory from analytical framework and both from model, following Leibenstein (1976), is made in Chapter 1-4.

The structure of the dissertation follows the logic of discovery, rather than some neat order, generated externally and imposed *ex post facto* on the material. This is discussed a little further in Chapter 1-4. Suffice it at this point to say that the study commences with an exploration of the background, empirical, methodological and historical, of the circumstances that led to the uncovering of the errors. This is followed by an attempt to get to grips with the errors themselves, also in the historical sequence in which the lengthy debate with the CSS over those errors unfolded. Idiosyncratic though this order will seem at times, the structure mirrors quite faithfully my own development in working through the topic. It would have been possible to have obliterated all or most traces of this process by writing a literature review which made it look as though the final result of reading and reflection was an empirical examination guided by theory. This has not been done, and, in many ways therefore, this document constitutes a sort of intellectual biography - brought to life through the history of a project worked on sporadically over a period of more than 11 years.

As is so often the case, the original aim of the dissertation - a critical evaluation of the activities of the National Productivity Institute (NPI) coupled with a critical analysis of the manufacturing sector output figures - was far too ambitious. Not until the final writing-up stage, however, was it recognised that to do justice to both topics would add roughly another four 30-page chapters to the present work.⁵ Acknowledging the unwieldiness of this has entailed vigorous pruning and rearranging.

The somewhat circuitous route to the manufacturing sector output estimates was via an attempt to demonstrate that many of the NPI's statements about the productivity performance of the sector, based as they were on thoroughly unreliable statistics, must themselves be unreliable. It is a relatively simple matter to show that the NPI has repeatedly used the CSS' indifferent quality statistics to make a case which no careful economist would endorse without qualification. Part of the excess of ambition consisted in a desire to explain why individuals within particular institutions are constrained to behave in ways which many of the people at the NPI have done. A moment's thought should have been all that was necessary to show that the journey into theories of the state, theories of institutions, not to mention economic psychology, was going to be arduous and lengthy.

Excess of ambition was also manifest in the desire to confront the conventional wisdom in such areas as the significance of wage claims in cost-push inflation, the impact on growth of redistribution, or inadequately explored areas such the causes of increasing capital-intensity in

5 These exist as rough drafts.

South African manufacturing,⁶ the competitiveness of South African exports, or indeed the reasons why most South African manufacturers have traditionally not exported⁷ - all topics on which the NPI has articulated quite strong views. To have entered this terrain would have meant engaging in arguments that are almost certainly interminable, not least because of the poor quality of the available data.

A compromise has had to be sought between, on the one hand, writing the productivity question and the NPI out of the study altogether, and, on the other, turning in an even longer work which makes obligatory detours into all of this material. As it is, the work runs to two volumes anyway. This is not as bad as it seems - **Volume I** contains the substantive arguments and **Volume II** the appendices. Part of the reason for the length of the appendices is the stress laid in the study on replicability⁸ - to tax others with producing work that cannot be replicated and then do the same oneself, is problematic.

To prevent the study from getting completely out of hand, a decision has been taken to restrict the empirical examination of the NPI to a brief overview of the relevant activities of that organisation during the period in question. Little more is offered than is necessary to establish that the NPI has used the weak manufacturing statistics to promote the view that the relationship between aggregate wage and productivity growth⁹ in the economy has been

6 See the study by Welcher (1991) on this question. It relies on sorting through management 'perceptions' rather than the rigorous analysis of changing relative prices of factors of production and the estimation of elasticities of substitution that many economists would prefer.

7 Explanations relying on descriptive terms such as 'bias against exports', insofar as they neglect the impact of rent-seeking activities by various fractions of the capitalist class on the forms of regulatory structures that the state puts in place, are somewhat less than useful. To unpack the content of a notion such as 'bias against exports' entails a more than superficial confrontation with theories of the capitalist state.

8 To assist in this, a diskette containing the workings of Appendices 2-4; 2-5; 2-6 and 2-7 is provided in a pocket inside the cover of Volume 2.

9 The NPI invariably uses the notion in a relative sense, frequently by reference to the 'country's' competitors, but some of the less well-informed consumers of this piece of information, particularly in the business community, interpret this as meaning that productivity is poor in some absolute sense. Here are two examples of such a misunderstanding, one recent, the other not:

"South Africa's productivity is among the lowest in the world." (Bas Kardol, sometime Chairman of the Barlow Rand Corporate Consulting Group, cited in *Sunday Tribune*, May 20 1984, p32)

"Productivity in South Africa, among the world's worst, would have to improve dramatically before any meaningful foreign investment could be expected, management consultant firm P-E Corporate Services MD Martin Westcott said yesterday." (*Business Day*, Thursday, October 17 1991, p3)

The NPI does not make this elementary error; on the question, for example, of manufacturing sector productivity growth, it is reported that South Africa "...does not compare too badly with Canada and the USA..." (*Productivity Focus 1986*, p22). In *Productivity Focus 1989*, even the incorrect output data place South Africa above some of the countries in a 14-country comparison of private-sector productivity (see pp32-33, especially Figure 35).

Readers ought to be wary however, of any international comparison which purports to be able to rank countries in terms of relative productivities. Very little serious work in this field has yet been published. The authors of one of the largest studies done to date, Szirmai and Pilat, 1990, provide a select bibliography and refer as well to an International Comparisons of Output and Productivity project (ICOP), which apparently is still in its early stages. In methodological terms, it appears that the studies that do exist have, despite their complexity, only reached the stage of binary or bilateral comparisons.

As an example of a pair of binary comparisons between the USA and Japan, and the USA and Korea, the work by Szirmai and Pilat illustrates very well the difficulties involved. Data selection and reconciliation was a major problem - the basic data are available from the authors in a 300-page annex to the project report. The authors compared 28 "sample industries" (3-digit SIC classification) between the USA and Japan, and the USA and Korea.

unsatisfactory.¹⁰ This does not mean that it is sought to show that the productivity performance of the economy has been satisfactory - merely that the NPI has persistently overstated the case for management in an area of some sensitivity - wage negotiations. This is done in **Chapter 1-2**.¹¹ After an introductory glance at some of the difficulties of producing productivity statistics, the chapter reveals how large the errors in the output statistics have now been acknowledged by the CSS to be. It then shows how, on two separate occasions, the NPI has constructed an incorrect account of the performance of the economy on the basis of incorrect CSS output estimates. On the second occasion, ample warnings of the likelihood that the figures were wrong went unheeded. In **Chapter 1-3**, the climate in which the NPI has operated is discussed. This is followed by a brief history of the organisation in which the structures and finances are examined. The chapter ends with a look at the way in which the NPI's efforts have created or reinforced the perception that wage claims by unionised workers have been excessive.

Abandoning the bulk of the empirical material on the NPI to its fate in various pieces, both published (Meth, 1990a; 1990b; 1991a; 1991b) and unpublished (Meth, 1990c; 1991c), raised the question of what to do with the corresponding theoretical work. It has been decided to present some of this material at the end of **Chapter 1-4**, a general introduction to the theoretical and methodological problems facing the study. The chapter considers the (necessarily?) un-methodical nature of the project and provides the first demonstration of the fact that neither theory nor statistics are 'innocent' (divorced from politics). This is followed by the briefest of reviews of the history of the debate on the difficulties of valuation in manufacturing - one aspect of the broader 'index number problem'. Continuing with an

(continued)

is gathered in each of the countries concerned, they managed to make 126 matches in the USA/Japan comparison involving 220 products in Japan and 493 in the USA. These accounted for 22,4 percent of gross output in manufacturing in Japan and 21,9 percent in the USA. For the USA/Korea comparison, products accounting for 46,0 percent of gross output in Korea, and 22,4 percent in the USA were matched (1990, p2). Clearly, it will be a long time before much reliance can be placed in international comparisons of labour productivity.

Note that acronym 'SIC' refers to the Standard Industrial Classification of all Economic Activities. It is based upon a standardised international classification system devised by the United Nations. Using the system, economic data on 'industries' may be presented at differing levels of aggregation. The 1-digit level (major divisions) lists whole 'sectors' of the economy, eg, SIC 3 'Manufacturing', or SIC 2, 'Mining and quarrying'. The 3-digit (major group) level refers to 'industries', eg, SIC 351 'Manufacture of Industrial chemicals'. Fine detail is given at the 5-digit (sub-group) level, eg, SIC 35110, 'Tanning extract'.

- 10 Productivity analysis in South Africa is not in its infancy, having been conducted on a systematic basis by the NPI for more than 25 years. The quality of the workmanship varies enormously, depending on the type of study being conducted. Some of the firm- or industry-level studies have been competently executed, but many of the macro-economic analyses are not good, and are ideologically slanted as well.
- 11 Chapter numbering may seem a trifle odd. The first digit in the chapter number refers to the part of the study in which the chapter is situated - either Part 1 or Part 2. The second digit locates the chapter in relation to the other chapters in that part of the study. The same convention is used for tables and appendices. This has been done because Part 2 of the study was completed quite a while before Part 1. Even with smart search facilities, renumbering every chapter, every appendix, every table and every reference to any of these in the study, the possibility of introducing errors exists. Chapter numbers have thus been left as they were. The reader's indulgence is begged for the small inconvenience involved.

exploration of those parts of Marxist theorising about the capitalist state which could be applied to an institution like the NPI, the chapter leaves incomplete the task of presenting a fully-worked out theoretical position.

Most of **Chapter 1-5** is devoted to an exploration of what is sometimes called the 'competing paradigms' problem in the social sciences. Seemingly remote from the, at times, nuts and bolts practicality of the debates around output valuation, the competing paradigms story is important because, it is argued, without an explicit commitment to one of those world views, Marxist analysis, the study would probably not have been performed at all. Not only that, the chapter pushes the question of choice between competing frameworks to its limits to try and uncover how choices between analytical frameworks are made. Were it not for the fact that every one of us makes such a choice, either consciously or unconsciously, every time we undertake a piece of analysis or research, the chapter, having been written, could possibly have been consigned to the scrapheap, or to the store containing those things which everyone should confront at least once. The justification for leaving the chapter in the study is the questions it raises about the process of choice.

The chapter has been reconstructed from arguments in the (ambitious) first draft which attempted to draw together the many threads necessary to present a viable analytical framework, from a Marxist point of view, to explain the forces that incline (not oblige or compel!) individuals within particular structural locations to intervene in the economy in the way that they do.¹² Relocating the remnants of this endeavour to the previous chapter has allowed the engagement with the 'competing paradigms' debate to be used to cast light on the mechanisms through which the selection of a particular theoretical framework influences research agendas.

Looking back over the way in which a commitment to a particular analytical approach, in my case to Marxism, has influenced the shape of the research project, some awkward truths have had to be faced. Frequently, possibly even inevitably, confrontations between theory and the real world modify theory, especially if these confrontations take place over a lengthy period. In my own case, the encounter has brought about a movement from a position where I would have insisted that the distance between what are called the 'competing paradigms' in

12 It is clear that constructing an adequate analytical framework for understanding the rôle of individuals in the propagation of ideologies in and through institutions like the NPI would be a multi-disciplinary affair. Drawing on psychology, sociology and political science (as it would have to) such a daunting task is precisely the sort challenge from which economists have shrunk in the past. This study continues that tradition - it offers some reasons why the servants of the NPI should have chosen to act in the way in which they have. These, however, are but loosely integrated into an understanding of the broader socio-economic structures within which these actors are located.

economics is so large that none but the most trivial conversations between them were possible, to one in which I would concede that although this may well be true for some aspects of the competing theories, they occupy a common terrain on which nothing in the theories themselves prevents them asking at least some of the same questions. That being so, it is argued, political affiliation becomes a determining factor in the choice of many research topics (and of the analytical frameworks that underpin them). Disagreements between economists are shown to be of consequence, not so much because devotees of different worldviews cannot, in principle, ask the same questions, but rather because different analytical frameworks present differing opportunities and imperatives for policy formulation.

In the spirit of the discussion above about the apparently idiosyncratic ordering of the material, this discussion on methodology, which some writers would have placed at the beginning of the study (or possibly even omitted altogether!), is located at the end of the first part of it. The logic of this is simple - it is only in the final stages of writing up that this confrontation actually occurred. For most of the period during which this research was conducted, and during which the series of skirmishes with the authorities reported in these pages took place, I operated with a fairly 'vulgar' Marxist analytical framework which seldom needed to be refined to meet the demands of a complex and nuanced reality. Those were the days of opposition. The changing climate in South Africa, one which has required contributions in the field of policy formation, has stripped away the luxury of being able to rely on such simple-mindedness.

I would claim though, with undiminished vigour, that I view productivity in the way that I do because I use Marxist analysis to do so. That said, however, it must be acknowledged that the immersion in the debate on the nature of theoretical choice, the practical politics of attempting to effect improvements in productivity statistics, as well as the reading of the latest Marxist work in the field have enforced a movement away from the illusory comfort of a now dated position such as that to be outlined immediately below. This shift notwithstanding, it will, I hope, become clear that it is not immodest to claim a victory over the CSS, and above all, the NPI, in the engagement recounted in these pages.

Poor productivity elsewhere

There are few works of which I am aware that tackle the ideology of poor worker productivity by performing a critical examination of the documents used to produce and sustain that ideology. One of them is by a Marxist sociologist - Theo Nichols (1986). Another left-wing

arena in which this question has been aired, although in somewhat less detail than in Nichols' work, is the journal *Monthly Review*. The June 1980 edition contains a short article (pp1-9) called "The Uses and Abuses of Measuring Productivity", written by the editors. This was a follow-up to an article called "Productivity Slowdown: A False Alarm" that had appeared in the journal the previous year. An interesting feature of the 1980 article was the reference to the scepticism expressed in certain sections of the business community to the reported declines in productivity. *Business Week*, for example, in its edition of February 25 1980, ran a piece called "A Productivity Drop that Nobody Believes". Concern at the time that the US was falling behind its competitors led to a rash of such endeavours. One of them, by Stanley Henrici, published in the *Harvard Business Review* (November-December 1981), suggests that the productivity problem might not exist at all, and recommends disregarding national statistics on productivity altogether, on the grounds of their extreme unreliability (p129).

Similar articles have made their appearance in *The Economist*, which displays a lively interest in the quality of statistics, often running provocative pieces on the topic of errors in statistics. A recent example, headed "More than meets the eye" (December 26th-January 8th 1993, p87) reported the work of Michael Darby, who has argued of late argued that the US output and productivity slowdown may be accounted for to some important extent by measurement errors. Two types error are discussed - under-valuation of service sector output, and problems with changing relative prices.¹³

In language which many Marxists would find hard to resist, the editors of the *Monthly Review* article cited above dismissed reports of a productivity slowdown in the US in the late 1970s as "statistical sleight-of-hand." They wrote that:

"Capital's demand for kickbacks - to reverse labor's hard-won gains on working conditions - has been spreading from one industry to another. And the groundwork for this shift in the class struggle has been laid in the so far successful brainwashing of the public to believe that the United States is in trouble because of sagging labor productivity.

The most remarkable aspect of all this hullabaloo about productivity is that it is based on absolutely phoney statistics, which owing to their frequent repetition and prominent display in the press, are accepted as valid and meaningful measures. A striking example of how misleading and false these figures can be is provided by the construction industry. The official data, as computed by the government's statisticians, show that productivity in the construction industry rose over 70 percent from 1949 to 1967, and since then declined by over 20 percent. In other words, construction workers today presumably produce one fifth less in a hour than they did in 1967.

Experts in this industry, however, are becoming increasingly aware that these numbers have no relation to reality... In fact, field studies of changes in the labor requirements for specific types of construction,

13 I have not been able to trace this particular piece, but in 1984, Darby cast doubt on the statistical evidence for productivity slowdown, suggesting that the viewers of it might be suffering from myopia.

conducted by the U.S. Labor Department, have revealed that productivity has indeed been rising during the same period that the index of productivity for the industry as a whole has reported a precipitous decline." (1980, p2)

Would that things were so simple. The reality, as Szymanski (1984) (another Marxist) has argued, is much more complex. Thus the work of *Monthly Review* stalwarts such as Harry Magdoff, whose own involvement in research on productivity dates back to before World War II, and others like Victor Perlo (1982) ("The False Claim of Declining Productivity and its Political Use"), whilst making important and valid critiques of what are generally regarded as quite reputable statistics, ie, those produced by the US Bureau of Labor Statistics, overstates the case for the opposition. As Szymanski insists:

"Something fundamental happened to the rate of productivity increase in the 1960s and 1970s [in the US]. And this must be explained." (1984, p297)

It is not the intention of this study to explain why productivity in South Africa has declined. What is attempted, taking a leaf out of both Szymanski's work and the *Monthly Review* tradition, is to show not that the decline did not occur at all, but that it was not as serious as has been reported, and that furthermore, the 'fragile inferences'¹⁴ habitually drawn by the NPI from decidedly weak statistics have repeatedly been used as a weapon against working class organisations in South Africa.

So much for the first part of the study. After the necessarily brief encounter with the theoretical wellspring of the study, attention shifts in Part II to the central topic to be investigated - the delinquent manufacturing sector output estimates. To introduce the problem, the history of a debate conducted with the Central Statistical Services (CSS) during the past three years or more over this issue is recounted. *En route* to the main question of the causes of the errors in the series, a few important side issues are tackled. These include explorations of the problems of dealing with non-cooperative bureaucracies, as well as a brief foray into a question for which few answers have been provided, namely, how useful are particular statistical series, and to whom?

Structure once more

The study divides into two parts. **Part I** contains the introduction, the encounter with the NPI and the exploration of some of the theoretical and methodological issues raised by a study such

14 The term is from an article on sensitivity analysis by Leamer (1985). He observed that a "...fragile inference is not worth taking seriously". (p308)

as this. **Part II** looks in detail at the construction of output estimates in manufacturing. It also contains a summary of the findings and a set of policy recommendations.

Both parts of the study are supplemented by appendices - those relevant to Part I include **Appendix 1-1** - a reproduction of a paper I wrote more than ten years ago (Meth, 1983) at the instance of worker leadership in Fosatu (the Federation of South African Trade Unions).¹⁵ Published by Fosatu as an occasional paper,¹⁶ this polemical document contains, in embryo, many of the principal conclusions defended in this study. **Appendix 1-2** is an unpublished response to it by two officials of the South African Reserve Bank (SARB) (Swanepoel and van Dyk, 1983), and **Appendix 1-3** a brief response from the NPI (1983) replicating (incorrectly in places) some of the Reserve Bank's arguments. **Appendix 1-4** reports the findings of a content analysis of media reports on productivity over the period under consideration.

Part II - an overview

The remainder of this chapter is devoted to a short introduction to the contents of Part II of the study - the detailed structure of which is described in Chapter 2-1. The overview given below contains a summary of the main arguments and findings, and some reference to the general problems encountered in conducting the research.

In the first draft of the study, an attempt was made to gather together, in one or more chapters, all of the relevant theoretical material relating to the valuation and measurement problems for the manufacturing sector study. The intention was to draw on the appropriate (clusters of) theory as each different empirical section or chapter was tackled. In the event, although this did serve to highlight how many different theoretical fields were involved, the process of writing became cumbersome. It seems that only with difficulty can some of the theoretical and empirical material be pried apart. The approach adopted for the most part therefore combines relevant theory (or speculation) with the material to which it refers.

15 Fosatu, formed in 1979, was one of the major forerunners of the present-day Cosatu, the Congress of South African Trade Unions.

16 The paper (which is referred to in this study as the Fosatu Challenge), with its Marxist underpinnings, belonged in the category of left-wing work designed for popular, as opposed to academic audiences. As such, certain simplifications had to be made. It was felt at the time, however, that these could be defended in a more rigorous academic environment. Worked on sporadically over the years, much of the technical part of that defence is to be found within the pages of Part II of this study. Various other aspects of that defence have been published (Meth, 1990a; 1990b; 1991a; 1991b). Others exist in unpublished form, the most substantial of these (Meth, 1991c) tackles such issues as capital productivity; job loss associated with productivity improvements; wages, inflation and productivity; productivity and international competitiveness, as well as the problems of international comparisons more generally. Output valuation is a central issue in many of these works. Problems with labour estimates were tackled in Meth (1988). A rigorous defence of the Marxist stance adopted has yet to appear - this study contains the beginnings of such an endeavour.

Inaccuracies in the manufacturing sector output estimates are neither novel nor unexpected, and it would be unreasonable to demand of the South African national accounting statisticians levels of accuracy which their counterparts in more developed countries cannot always sustain. It is not easy to prevent bias from creeping into large-scale surveys conducted regularly on a constantly changing population, especially when extreme difficulties attend the execution of the periodic censuses required to establish benchmarks for the surveys. The errors exposed in this study in the constant price output estimates for the sector for the period after 1980 however, have their roots in faulty procedures as well as in sample bias.

Part II of the study is based on the material in two monographs on the topic, the second of them (Meth 1993a) being a rejoinder to the official response to the first of the two (Meth 1992). Combining and reworking these two pieces into one for the purposes of this study has not been an easy task. There is a certain artificiality involved in converting what was a halting process of discovery - essentially the record of a long dialogue between myself and the CSS, with occasional interjections from the SARB - into a single, apparently seamless narrative. To preserve the somewhat stumbling character of the actual research process, the blind alleys, errors and surprises are referred to at appropriate points in the story.

In the first of the two works in question, errors in the estimates were revealed by an examination of the way in which the CSS had 'benchmarked'¹⁷ the manufacturing census output estimates for the years 1982 and 1985 back to the 1979 figures. The examination showed that output levels for the period after 1980 were seriously underestimated. A set of alternate estimates was presented, and the rudiments of an 'early warning' technique for uncovering errors (the Euler Consistency Test) were presented. In the second of these two publications, the technique outlined above was refined and the explanations of the errors offered by the CSS were shown to be false. By means of a set of simulations on some hypothetical data, the potential impact of rebasing on output estimates was revealed. The actual South African data were then scrutinised and the possible sources of error located. The need for an entirely different approach to the measurement of output estimates was discussed, and certain alternatives were presented. The results presented in this dissertation represent an attempt to develop still further the general approach of the earlier work. This development work has taken the research deeper in the problematic literature on the valuation of net output, an endeavour whose first fruits were presented at a conference in November 1993 (Meth,

17 Benchmarking is a process in which indirect volume indices, obtained by deflating manufacturing census values of gross output by Production Price Indices (PPIs), are used as reference points through which the trend lines plotting the growth of the Physical Volume of Manufacturing Production (PVMP) are forced. The process of benchmarking is referred to again in Chapter 1-2 and then discussed at greater length in Chapter 2-3. As a first approximation, one may say that PVMPs are an attempt to measure output volumes 'directly', ie, they are concerned with actual physical quantities of commodities produced.

1993a). The last word on the matter has not been heard - not only has output been incorrectly estimated, the problems of aggregation are so severe that, as will be seen, the use of simple production functions like the Cobb-Douglas in analyses of the South African manufacturing sector is almost certainly inappropriate.

Of the numerous appendices to Part II, only one, the first, will be referred to here. The remainder are described briefly in Chapter 2-1. Appendix 2-1 contains an examination of the impact of rapid changes in the price of gold on estimates of Real¹⁸ National Income. Originally slated for inclusion in the main body of the study, this examination has been relegated to an appendix because its concerns, although relevant to the problem of output valuation, are somewhat peripheral to the analysis of the manufacturing sector performance figures - the central concern of Part II of the study. This appendix is noteworthy because it draws attention to an apparently unreported phenomenon, namely the negating on occasion, by means of what I have called the Perverse Deflator Effect, of the adjustments made to the national accounts to accommodate changes in the terms of trade.

18 Khamis (1984, p197) has argued that the use of the term 'real' to describe estimates of constant price magnitudes is misleading, and should cease. The SNA gives precise meaning to the two terms - thus:

"The changes over time in the current value of flows of goods and services and of many kinds of assets can be decomposed into changes in the prices of these goods and services and changes in their volumes. Flows or stocks at constant prices take into account the changes in the prices of each item covered. They are said to be in *volume* terms. However, many flows or stocks do not have price and quantity dimensions of their own. Their current values may be deflated by taking into account the change in the prices of some relevant basket of goods and services or assets, or the change in the general price level. In that case, flows or stocks are said to be in *real* terms (at constant purchasing power). For example, the System provides for the calculation of income in real terms. (UN, 1993, para 2.77, p25) (Emphasis in original)

As will be seen in Part II of the study, this will require one to speak of constant price gross output and real net output. It seems to me that a little permissiveness in this matter will not do great harm. Accordingly, whilst this distinction will be noted, rigorous use will not be made of it, unless a failure to do so undermines a particular argument.

Chapter 1-2

Making the worst of it: CSS error compounded by NPI insensitivity

How to Statisticulate

(being the opening paragraphs of Chapter 9 of Darrell Huff's book *HOW TO LIE WITH STATISTICS*, 1954, [1988])

"Misinforming people by the use of statistical material might be called statistical manipulation; in a word (though not a very good one), statisticulation.

The title of this book and some of the things in it might seem to imply that all such operations are the product of intent to deceive. The president of a chapter of the American Statistical Association once called me down for that. Not chicanery much of the time, said he, but incompetence. There may be something in what he says, but I am not certain that one assumption will be less offensive to statisticians than the other. Possibly more important to keep in mind is that the distortion of statistical data and its manipulation to an end are not always the work of professional statisticians. What comes full of virtue from the statisticians desk may find itself twisted, exaggerated, over-simplified, and distorted-through-selection by salesman, public-relations expert, journalist, or advertising copywriter.

But whoever may be the guilty party in any instance, it is hard to grant him the status of blundering innocent. False charts in magazines and newspapers frequently sensationalize by exaggeration, rarely minimize anything. Those who present statistical arguments on behalf of industry are seldom found, in my experience, giving labour or the customer a better break than the facts call for, and often they give him a worse one. When has a union employed a statistical worker so incompetent that he made labour's case out weaker than it was?

As long as the errors remain one-sided, it is not easy to attribute them to bungling or accident..."

Setting the scene

Ever-present in the background of this study is a concern with the quality of productivity statistics and the way in which these generally unreliable figures have been used in South Africa. Apart from the treatment which this chapter gives to the matter, however, the quality of the productivity statistics published by the NPI will not be referred to other than peripherally in the remainder of the study. The NPI is discussed at some length in Chapter 1-2 and the proselytising or propagandistic activities of that institution are squeezed through some of the material on theoretical frameworks considered in Chapters 1-4 and 1-3, but thereafter, the NPI will also fade from view.

In this chapter, the first encounter with the CSS' inaccurate manufacturing sector output estimates takes place. It is perhaps not as widely known as it ought to be, that the CSS' attempts to measure output levels in the 1970s contained errors even more spectacular than those made in the 1980s. After a short introduction to some of the difficulties involved in measuring productivity, the CSS' 1980s errors are looked at. The 1970s errors are embedded in a discussion a little further on in the chapter about the way the NPI used these incorrect estimates to produce the misleading labour productivity statistics¹ on which its 'poor productivity' case was based.

When it comes to understanding how the NPI's offerings came either to constitute the dominant view, or possibly to be absorbed into and to reinforce that view, especially on the wage/productivity relation, an awareness of the ideological climate into which the organisation has released its findings is vital.² So too is some idea of the form of the organisation. Once this has been presented, the NPI's analytical endeavours are approached via a short hypothetical detour through Ruritania, where, by coincidence, a similar set of errors to those made in South Africa have been found to have infected the statistics. This detour is followed by a glance at the CSS' error-laden 1970s figures and the NPI's use of them. Then comes an examination of the implications of the 1980s CSS errors, especially for the wage/productivity debate. An overview of the NPI's contribution to the 'poor' productivity debate concludes the chapter.

Although it is the labour productivity estimates on which attention is concentrated here, one could, of course, go much further, and show how the errors in the output estimates have spilled over into almost every aspect of the 'official' productivity statistics. It is possible to show, for example, that the capital and the multifactor productivity figures, and most of the estimates which purport to represent South Africa's productivity performance in an international context are also misleading.³ Those conclusions follow automatically from the

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- 1 The term 'labour productivity' as used here has its conventional meaning - units of output per unit of labour input. No causal relations are implied, ie, reported changes in productivity levels pose questions as to what may have brought such changes about, they do not provide explanations. These questions are sometimes technical in nature, but often they are not. Non-technical or ideological aspects enter the debate when one or other party starts making claims which have policy implications, especially when such claims rest on a slender statistical basis. Income distribution is among the more obviously sensitive areas, but problems relating to workplace practice (the labour process) can also give rise to energetic struggles. Repeated assertion of the belief that wage increases regularly outstrip productivity improvements, for example, is typical of an apparently innocent empirical finding which has important distributional implications. A claim such as this is likely to exert strong influence on those whose vested interests it serves.
 - 2 The approach adopted is one that I have used elsewhere (Meth, 1991a), namely that of casting the debate over the relationship between changes in wages and productivity within the increasingly conservative (in the Thatcherite sense) economic climate that came to dominate policy formation in South Africa from the early 1980s onwards.
 - 3 That has been tackled in one of the collection of unpublished manuscripts generated by this project - Meth (1991c).

fact that output has been incorrectly estimated. It is not necessary, however, to belabour the issue - the points about the NPI's use of statistics that need airing can readily be made using the labour productivity estimates only - other productivity figures (eg, capital and multifactor) simply confirm the analysis made here. More importantly, though, the labour productivity estimates are the most highly politicised, for the obvious reason that they stand at the nexus of struggles between workers and employers.⁴

Measuring labour productivity - some of the obstacles

Estimates of labour productivity provide a record of the units of output produced per unit of the labour inputs used in production. The definition and measurement of both output and inputs is not without difficulty - it is to problems of measurement, especially of output that this study owes its existence.⁵ Since labour (and all other) productivity statistics⁶ are derived statistics, a critical analysis of these statistics must perforce focus on the quality of estimates of output and of labour inputs. The substantial errors in the output estimates for the South African manufacturing sector in the period 1979-88 disclosed below were uncovered by a combination

4 Capital productivity is also political - in the sense that rising capital intensity (allegedly accompanied in the South African case by falling capital productivity!) is attributed in part to a business response to rising levels of worker militancy (class struggle). It is less directly so, however, because a decision to mechanise or automate seldom has the same newsworthiness as a confrontation over conditions of employment.

5 In Chapter 2-1 reference is made to an article by Griliches (1985) on the ambivalence of economists towards the imperfections which make their jobs "difficult and often impossible", namely that: "...these imperfections give us our legitimacy in the first place." It is no exaggeration to state that the present study would not have been worthwhile conducting were it not for the occasionally extreme difficulties of measurement and hence the likelihood of large error.

6 Productivity estimates of this sort are simple ratios - units of output per unit of labour, or capital. There is disagreement on what constitutes the most appropriate index to use in measuring productivity. The literature is replete with the results of attempts to break out of the straitjacket of the simple ratio method of estimating productivity. It has long been known that this can produce misleading results. Clemhout (1963) shows that the linear production function implicit in the use of the ratio method to estimate productivity can reveal technological regression under conditions where a Cobb-Douglas production function would show technological progress. He observes that:

"It is then clear that the ratio method is an elementary and dangerous way of estimating productivity. One immediately comprehends the precariousness of such ratios as elements of policy-making, whether economic, social or political." (pp359-360)

The older literature is reviewed in an article by Kennedy and Thirlwall (1972), which gives a useful and accessible discussion on productivity and production functions, as well as one on technical progress. Nadiri's (1970) piece on total factor productivity covers some of the same ground, but goes much more deeply into the technical problems. One interesting aspect of the Nadiri article is its review (see pp1167ff) of the Jorgenson-Griliches dismissal of Denison's claims about growing total factor productivity (the Abramowitz residual). Nadiri discusses the intense sensitivity to choice of conventions for measuring total factor productivity (p1169). A comparison of the tentative nature of 'knowledge' on this question with the NPI's crude assertions about the (lack of) growth of total factor productivity is instructive. See NPI (1994b, pp9-13).

The recent literature delves into the use of more exotic indices and techniques for measuring productivity, both for domestic calculation, and for the more problematic international comparisons. See Caves *et al* (1982a; 1982b), Diewert and Morrison (1986), Färe and Grosskopf (1992). In practice, the complexities of this somewhat abstruse debate tend to be ignored for the simple reason that the statistics required to derive 'correct' indices are not available in many countries.

of luck, doggedness and faith in the efficacy of a test⁷ that shows when national accounting output estimates deviate too much from their 'true'⁸ values.

It may well be that the denominator (labour inputs) in a labour productivity estimate is as difficult to measure as the numerator (output). Aspects of this question will be referred to below in passing, but in the study itself, the bulk of the attention is concentrated on just one of the many problem areas encountered in the measurement of output, namely, the difficulties of preparing constant price output estimates in the manufacturing sector. The relevant statistics are produced chiefly by the CSS.

Apparently neutral terms such as the 'units of output' and 'units of labour', commonly encountered in theoretical works, may disguise for a while the problems involved in specifying what these units are or should be. Confrontations with the real world, however, compel the economist to work with imperfect data, often collected for uses other than those to which they are put. As far as the production of commodities is concerned, attempts are made to represent output levels by means of indices that measure physical volumes.⁹ When this proves to be difficult, or even impossible, as will be the case in an industry with a heterogeneous and rapidly changing output mix, indirect volume measures have to be devised.¹⁰ The path to reliable indirect volume measures is, however, blocked by numerous conceptual and practical obstacles, some of which are explored in Part II of the study.

As to the denominator in labour productivity estimates, there may be occasions when the difference between using a crude concept such as the number of workers employed, as opposed

7 A rudimentary version of this test, which I have called the Euler Consistency Test, appeared in my first foray into this field - the Fosatu Challenge - ie, the paper that called into question the accuracy of labour productivity estimates published by the NPI (Meth, 1983). More on this paper, especially its ideological and political implications, appears in Chapters 1-4 and 1-5.

8 It is not easy to define 'true' adequately, let alone to determine the 'true' value of some or other magnitude. 'True' is used here in the sense of 'consistent with the other magnitudes in a set of national accounting estimates'.

9 The discussion in Eisner (1988, pp1616-1621) on the difficulties of measuring final product provides an excellent example of the conceptual difficulties encountered in attempting to classify an apparently simple activity such as the production of a commodity like clothing.

10 Valuing services poses problems of an even more intractable nature. Criticising the current state of economics in 1972, Morgenstern, observed that about 60 percent of GNP in the USA consisted of services of one sort or another, but that the theory of the firm was anchored firmly in notions appropriate to the study of physical output. It is not clear that theory offers much better guidance today. Somewhat tongue-in-cheek, Morgenstern asked:

"How is productivity of an orchestra, a school, a law firm, a church (input sinners - output saints?) measured? Is a productively played quintet first played by 5, then by 4, by 3...artists? Or do they play louder, faster...? What does productivity of a nation mean under these circumstances?" (p1183)

Usually, more is regarded as better. Private sector service output is measured by a variety of surveys and censuses. Governments in some highly developed states specify elaborate performance indicators to measure productivity. Some of them may be reasonable - others are probably less so. A glance at the 300-page plus guide to output indicators for the US Federal Government suggests that many of them are suspect, eg, the number of snow survey forecasts issued (1993, p172).

to a more complex measure such as hours actually worked, becomes significant. 'Hours worked' is also, however, not a wholly satisfactory measure, as the debate over Leibenstein's (disputed) notion of X-efficiency (1966) quite clearly shows.¹¹ Even if it were, further problems arise from the fact that an hour's worth of skilled labour is very different from the same quantum of unskilled.¹² Thus, even though at first sight labour productivity indices may seem to be uncomplicated measures, behind this apparent simplicity lurks a host of difficulties. Of these, no more will be said here. Instead, we turn to the series on which the study is concentrated, the output errors. After a brief scrutiny of these figures, the wage/productivity debate and the NPI's rôle in it will be joined.

Errors in the CSS output series

Attempts by the CSS to estimate volumes of manufacturing production have not enjoyed great success. Since March 1992, the figures have twice been revised upwards. The first of these revisions represented an initial attempt by the CSS to 'benchmark'¹³ the monthly survey figures to the results yielded by the 1985 Census of Manufacturing. The second revision, made

11 There is a sizable literature in this and related areas. For a direct response to Leibenstein, see Stigler (1976). See Shen (1985) for an attempted examination of the compatibility of the X-[in]efficiency postulate with industrial psychology theories of worker behaviour. Essentially, the argument is that the disutility of work constitutes a strong incentive to shirk. Twenty five years after the notion of X-inefficiency was first advanced, the subject was allocated its own slot at the annual meeting of the American Economic Association. See Leibenstein and Maital (1992); Frantz (1992), and Button and Weyman-Jones (1992). Frantz makes the important point that setting up a theoretical model in a particular way limits the questions it can ask of the world. Apropos of Leibenstein's critics, he observes that:

"...they have given us mostly assumptions about why X-inefficiency cannot exist. In fact, X-inefficiency cannot exist only within their own models and given their own methodologies and use of language. The economics profession has the capacity to entertain alternative models and methodologies." (1992, pp437-438)

On a somewhat different tack, the relationship between unemployment and the notion of 'contested exchange' has, in recent times, been explored by Bowles (1985) and Bowles and Gintis (1990), while the size of the efficiency wage necessary to ensure that a 'no-shirk condition' is met has been examined by Shapiro and Stiglitz (1984). On this topic see also Carlin and Soskice (1992, pp404ff).

12 The Bureau of Labor Statistics (BLS) of the United States Department of Labor is currently examining the problem of attempting to allow for skill differences in the labour force in productivity estimates. Interview with Mr Charles Ardolini, Chief, Division of Industry Productivity and Technology Studies, Washington DC, March 1992.

13 Benchmarking, as noted in Chapter 1-1, is a process of correction performed on time series survey results, such as the manufacturing sector output figures, or the employment estimates. Monthly, quarterly or any other regular survey results record trends, but over time, and for a number of reasons, bias can distort these results. In national statistical agencies worldwide, it is customary to perform censuses at regular intervals (in the present case, triennially) and to use the hopefully more accurate observations obtained as 'benchmarks' or pegs, as it were, on which to hang the previously detected trend lines. National accounting statisticians operate under the assumption that they can measure trends reasonably accurately, especially over relatively short periods of say two to three years, even if they cannot get absolute levels correct. Benchmarked figures are thus usually bodily displacements of trend lines, upwards or downwards, as dictated by census results. As far as the ability of national accounting statisticians to measure output accurately is concerned, Siegel (1994) has claimed that the Production Price Index (PPI), the standard deflator applied to gross output estimates, "...misses about 40 percent of change in quality..." He also claims, however, that the mismeasurement is constant over time (1994, p11).

in November 1993, in which the figures were reconstructed using a different estimating technique, raises aggregate output levels significantly.¹⁴

The November 1993 figures are an improvement on the pre-March 1992 results, but even these latest figures are incorrect. Census (financial) year and corresponding calendar year¹⁵ results for these three endeavours are given in Table 1-2.1 below. As may be seen, the 1982 error was trivial, the 1985 error was in the region of 13 per cent, and the 1988, not much less.

Table 1-2.1 Three attempts by the CSS to estimate manufacturing sector output levels

Financial year estimates	1978/79	1981/82	1984/85	1987/88
Unbenchmarked (pre-March 1992)	100,0	124,2	112,5	114,8
Benchmarked (March 1992)	100,0	125,8	117,6	118,9
Reconstructed (November 1993)	100,0	124,1	121,9	125,5
Calendar year estimates	1979	1982	1985	1988
Unbenchmarked	100,0	113,8	104,7	113,8
Benchmarked	100,0	116,7	109,5	118,9
Reconstructed	100,0	115,7	117,8	126,1

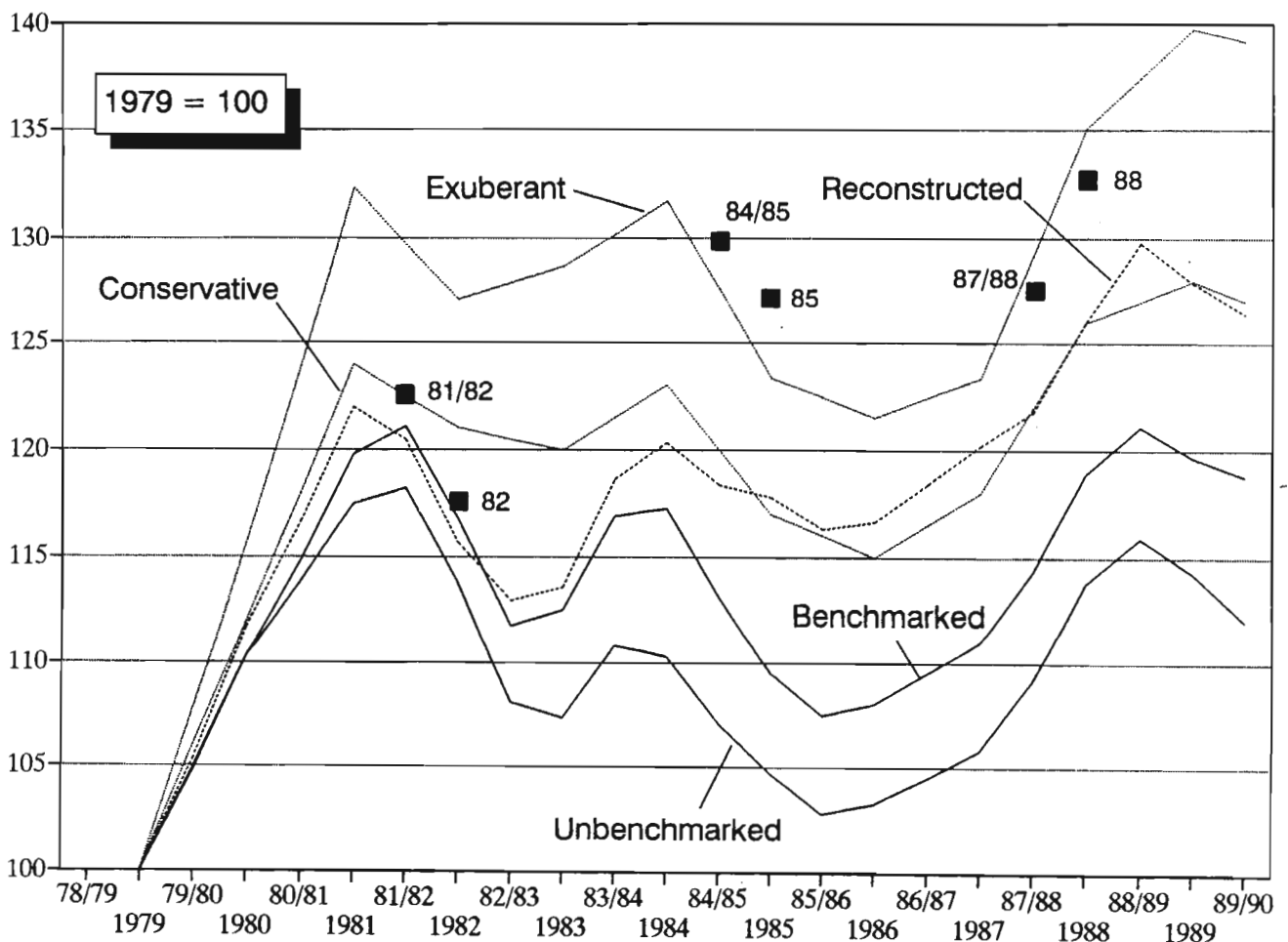
Sources: Estimated from SNR P3041.3, 12 September 1990 - Unbenchmarked
 SNR P3041.3, 9 March 1992 - Benchmarked
 SNR P3041.3, 12 November 1993 - Reconstructed

So convinced was I of the appropriateness and reliability of the technique that I had developed for performing consistency tests on the manufacturing sector output estimates, that in 1991, long before the CSS conceded existing estimates were wrong, I persuaded the *Financial Mail* (November 1 1991, p40) to publish a pair of estimates I had made, demarcating a region within which, it was argued, the 'true' values were likely to lie.¹⁶ These values were

- 14 My first attempt at interpreting these results was in a paper presented at the end of November 1993 (Meth, 1993a). The final draft of the paper had already been completed when the Statistical News Release containing the reconstructed results (SNR P3041.3 of 12 November 1993) was published. Its results for the manufacturing sector as a whole were quickly inserted - I took it for granted that the increases in aggregate output resulted from the fact that the CSS had recognised the error of their ways and had incorporated, at long last, what is argued below to have been one of the major causes of the errors - the huge volume of output that flowed from the Chemical Industry when Sasol II and III plants came on stream in the early 1980s. This, as the analysis below reveals, is apparently not what has happened. The long and short of it is that it is not known by what means the CSS has produced the reconstructed figures. The question will not be pursued in this dissertation - as is disclosed in Chapter 2-1, it is hoped that this matter will soon become the subject of an inquiry to be conducted by international experts. It is sufficient that the existence of large errors has finally been accepted by the CSS.
- 15 Several censuses conducted by the CSS, for example, the manufacturing census, cover the period from 1 July in one year to 30 June in the following year. Thus what is called the 1985 Manufacturing Census covers the period from 1 July 1984 to 30 June 1985. For convenience sake, I shall call this a 'financial' year. This usage is adopted as a shorthand method of distinguishing census years from calendar years (1 January to 31 December). Some of the confusions caused by a failure to note this distinction are addressed in Chapter 2-3.
- 16 In Part II of the study it is shown that this confidence, based as it was on a partly faulty understanding of the procedures for producing national accounts, was somewhat misplaced. By way of consolation, however, it appears that the correction which this misunderstanding entailed is more than compensated for by the true extent of the CSS' error.

subsequently incorporated into an occasional paper called, somewhat provocatively, "South Africa's Thatcherite Productivity Miracle: 1979-89" (Meth, 1991a).¹⁷ Plotted in Figure 1 below - they create an area bounded by two series labelled respectively 'Exuberant' and 'Conservative' (the upper fine-dotted lines). Also shown in this diagram are the three CSS estimates in Table 1-2.1 above, and my most recent estimates¹⁸ (the square markers) of the approximate regions within which 'true' output levels for the years 1981/82; 1982; 1984/85; 1985; 1987/88 and 1988 are likely to be located. The CSS figures have inched closer to my original estimates, and some of their latest estimates now lie within the range demarcated in 1991.

Figure 1
Manufacturing Output



One intention of this study is to demonstrate that the 'true' trend line of manufacturing output (insofar as can be established) passes near or through the square markers in the diagram above.

17 This paper was published by the Economic Trends Research Group - a network of left-wing economists centred around the Development Policy Research Unit in the University of Cape Town.

18 The explanation of the way in which these have been made is given in Chapter 2-7.

That the CSS has conceded as much as it has to date is gratifying - it will be more satisfactory still when it is acknowledged finally, that the existing estimates (the 'reconstructed' figures) are still too low. When that occurs, a further reconstruction of the output trend line will be required. The information presented in this diagram suggests that such a reconstruction could well show that the recession of 1981-82/83 was not as severe as the CSS has indicated, and that the recession of 1984-86 was, at worst, more of a levelling out rather than a decline, and may, in fact, also not have occurred at all.

Gathering statistics in Ruritania (and South Africa)

Suppose that an economist were analysing the productivity performance of the manufacturing sector of the Ruritanian economy over a nine-year period for a group of (paying) corporate clients numbering a couple of thousand. Taking a generous view of matters, the clients have no objection to our economist making public the results of the analysis, nay, more than that, they are community-minded and insist that the results be disseminated as widely as possible. The chief statistician of Ruritania gives out statistical information to our analyst (and to anyone else who wants it) which makes it possible to estimate that output over the relevant period grew at a compound rate of 2,6 per cent *per annum*.

Using the only yardsticks available to him/her, performance relative to that of the manufacturing sector in other times, or in other places, the economist concludes that the productivity record has been poor and makes known his/her findings to all and sundry. The press and the television services, being short of human interest stories on the day on which this is made known, give wide coverage to an otherwise dull economic fact. Engaging in speculation on the causes and consequences of this lamentable result, unfortunately not from a particularly learned perspective, the media succeeds in arousing some response from a public generally indifferent to such arcane matters. The switchboard of a phone-in programme on the national radio is jammed for about 10 minutes.

Meanwhile, the chief statistician, whose Olympian neutrality is assured by an informal rule which debars him from conducting anything more than the most superficial analysis of the figures produced by his department, has set his workers the task of reconditioning the statistical mechanisms used to create economic data. These worthies suspect that bias, the bane of a statisticians' existence, may have crept into the surveys, and they suspect furthermore, that the periodic manufacturing censuses, not having been overhauled for more than 15 years, could be miscounting. At considerable cost, a thorough census is conducted, and it is

discovered that output levels (as disclosed by a survey conducted scarcely one month before the census) have been rather sadly under-estimated.

Recognising the impossibility of returning to all of the respondents from whom information had either not been obtained at all, or had been incorrectly captured, the chief statistician applies to the new census results, the techniques devised for just such eventualities by the international authorities in this field. This enables him to reconstruct a historical record which is somewhat more satisfactory than the existing story. Going back as far as seems prudent, about nine years, the chief statistician causes a revised set of estimates to be made for every industry in the sector.

Being a sensitive and discreet fellow, and not wishing to create alarm and despondency, not to mention anger amongst the chattering classes by letting it be known that his department is using large amounts of tax money to misinform that public (albeit unintentionally), he does not trumpet the news of this error abroad. He does, however, as required by law, publish the information in a collection of tables containing large numbers of other estimates as well. (Relations between certain sections of the public and some uncouth members of the media on the one side, and the chief statistician on the other, are not always cordial, but it is not polite to inquire into the reasons for this).

A disinterested observer, using the new information, would discover that the growth rate of output (in aggregate) was actually 5,0 per cent rather than the paltry 2,6 per cent it was thought previously to be. If the observer were reasonably informed, they would recognise that a growth rate of this magnitude is quite respectable - being not far below the economy's historic best, and well up in the chart that records international performances. Clearly, a calculation of this sort has the power to upset. People who matter have become accustomed to feeling bad about the economy's course - it were a pity to disturb their equilibrium with mere good news. Fortunately, the media are fully occupied at the time with other baleful events in the wider society, such as the high level of irritability over certain proposed changes to the structures of representation. Launching the figures into the public domain in their dazzlingly pure numerical form, without noisome and distracting comment, ensures that the good news remains safely locked up in its technical straitjacket.

Arcadia, the capital of Ruritania, is a modest city and the chief statistician knows our economist reasonably well. This does not, however, affect the commitment of either to the pursuit of pure knowledge. To say sooth, though, our economist is a little put out, because every laborious calculation he has performed (user-friendly computers having yet to make their

appearance in Arcadia) has to be redone. Then there is the small matter of a public which has been assured that things are bad and getting worse. For purposes of his own, it actually suited our economist to be able to claim as much anyway. Whatever is to be done? One possible recourse is to real life, to see if it has any lessons to teach fiction.

Stranger than fiction

Perceptions of productivity growth in the last decade or so as being poor (worse even than what it was in the previous decade) have been built partly on the basis of the error made by the national accounting statisticians that is the subject of this study. This error has permeated many aspects of the productivity statistics prepared and published by the NPI.¹⁹ By coincidence, the NPI used the similarly incorrect output series produced by the CSS to establish or bolster the view that productivity growth in the 1970s was poor as well. One can see the first signs of the record of misfortune being created, in good faith, by the CSS in the sparse comment issued with some of the output statistics. For example, on the basis of the best information available in 1978 (the 1970-based national accounts), the CSS informed users that:

"The real value added of manufacturing, commerce and finance increased rapidly during the period 1960 to 1970 and average annual growth rates of 8,4; 7,8; and 5,7 per cent, respectively, were maintained. For the years 1970 to 1976 the average growth rates of these three major divisions dropped to 3,9; 3,2; and 3,9 per cent, respectively." (SNR P12.1, 23 March 1978)²⁰

There being no other source of national accounting information, the NPI had, and still has perforce to make use of the CSS' offerings. The following example is typical of the many misleading NPI press releases based on the national accounting data:

"Figures from the National Productivity Institute (NPI) show that between 1970 and 1980 the real gross domestic product per economically active person in S.A. grew by a low 4,6% - an average of 0,2% a year. Since 1974, productivity has actually declined. Countries like Sweden, Japan and Taiwan have shown a consistent high rate of growth in per capital (*sic*) gdp. SA's poor performance is disturbing, particularly since its slow average rate of growth is tending to fall even further behind the more industrialised countries." (*Financial Mail*, Supplement, 27 August 1982, p49).

GDP *per capita* is not a productivity indicator, it provides a rough guide to welfare levels. GDP per economically active person is doubly unreliable, because in the South African case, it used the incorrect numerator (inaccurate output estimates) as well the wrong denominator.

19 As demonstrated in Meth (1991c), all of the aggregate capital/output ratios for the period after 1980 are incorrect and hence all capital productivity statistics are wrong. Some (unknown) number of industry ratios (which are not wonderfully reliable in the first place) are definitely incorrect. Unit labour costs estimates, whose impact on the competitiveness of South African manufacturers has been seriously exaggerated, are also incorrect.

20 The 1960-70 manufacturing sector growth rates look a little suspect, but that is of little concern here.

Rather obviously, any estimate which contains a larger number of unemployed (an important component of the economically active) at the end of the period than it did at the beginning must understate productivity growth. Disagreements about unemployment levels in South Africa notwithstanding, there seems little doubt that it was much higher in 1980 than in 1970.²¹

Figures like the CSS' 1970-based manufacturing sector output estimates were obviously central to the NPI's early attempts at interpreting trends. These yielded conclusions which will be shown below to be incorrect. Here, from the same source cited above, one sees the Executive Director providing easily digestible but 'phony' information to the business community, the primary audience of a periodical like the *Financial Mail*:

"An analysis of the manufacturing sector from 1972-81 ... shows that at the end of the 10-year period, employees were being paid 20% more for producing only 4% more. As NPI executive director Dr. Jan Visser comments, 'This discrepancy between wage increases and productivity performance is too big for comfort'. It is also one of the reasons for this country's soaring inflation." *Manpower Survey*, supplement to *Financial Mail*, August 27, 1982, p49.

Interspersed among the regular (crude) reports of wage growth exceeding productivity growth by some depressing percentage have been the results of the apparently more sophisticated calculations performed with the aid of the NPI's REALST (Resource Allocation Strategist) model.²² Soon, statements of the following type began to make their appearance:

"Figures released by the National Productivity Institute (NPI) show that between 1970 and 1982 improvements in labour productivity played little rôle in economic growth.

On average there was an improvement of 0,28% a year in the productivity of black labour and 0,06% and 0,05% in Asian and coloured labour. The productivity of whites in the same period rose an annual 1,25%.

However with salaries and wages rising faster than product prices in the non-agricultural sectors the recovery of black labour costs fell 0,92% a year. For Asians, coloureds and whites respectively decline (*sic*) of 0,16%, 0,18% and 0,32% were recorded.

In line with the rising cost of labour and the minimal gain in productivity the cost effectiveness of black, Asian and coloured labour fell 0,64%, 0,09% and 0,12% a year over the period.

Given the somewhat stronger improvement in white productivity, the cost effectiveness of this category of labour showed an annual 0,92% increase, resulting in an overall rise in the cost effectiveness of all labour of a mere 0,68% a year.

Even this figure, however, has to be qualified. Gains in white productivity were accentuated because wages and salaries at current prices for this group grew at a slower rate than gross domestic product. The negative cost effectiveness of the other groups is linked to a greater percentage increase in remuneration.

At the same time it can be seen that although white wages and salaries tended to grow more slowly, the recovery of labour costs from product prices for this group declined rapidly as a result of historically high income levels". (*Financial Mail*, 3 February 1984, p45) (Emphasis added)

21 Since the NPI was not in the habit of supplying any details of its sources, it is impossible to discover what estimates of the economically active population in South Africa were used to arrive at these conclusions. Whatever figures were used, they will have been, as I have pointed out elsewhere (Meth, 1988), so erratic as to be wholly unreliable.

22 For a full discussion of this model, see van Loggerenberg, 1986. Descriptions of it are reproduced in many NPI publications.

Apart from the fact that the figures used to draw these conclusions are incorrect, no mention is made in this seemingly rigorous analysis of the well-known fact that it is impossible to measure individual contributions to production, and hence impossible to measure productivity or 'cost-effectiveness' by race or by any other category into which one may divide the workforce (Thurow, 1980).²³ In the previous chapter, a passage from a *Monthly Review* article on the abuse of productivity statistics was quoted, along with a warning that it overstated the case. There are times however, when a conclusion like this:

"...the groundwork for [a] shift in the class struggle has been laid in the so far successful brainwashing of the public to believe that [South Africa] is in trouble because of sagging labor productivity.

The most remarkable aspect of all this hullabaloo about productivity is that it is based on absolutely phoney statistics, which owing to their frequent repetition and prominent display in the press, are accepted as valid and meaningful measures..."²⁴

seems justified. The NPI claims highlighted in bold above are 'absolutely phoney', and one can but wonder at the intentions of anyone who attempts to pass off such results as being valid. In any case, if movement up the job hierarchy, with the likely productivity improvements which this implies, was taking place at the rate some observers suggest that it was (Knight and McGrath, 1987) then it may well have been the case that black worker productivity was increasing faster than white. Although it is, of course, impossible to discover whether or not relative productivities changed in this way, there is some fairly substantial evidence available on changes in the racial structure of the occupations. This is considered in Chapter 1-3.

There is an important difference between the act of creating a 'phoney' statistic like the group (racial) productivities that come out of the REALST package, and the relatively innocent pastime of churning out standard but incorrect (also 'phoney') productivity indices on the basis of national accounting data supplied by the authorities. Both may have been done in good faith, but the REALST calculations contain such a primitive theoretical error, and that in an

23 Terms used by the NPI such as 'over-recovery' (essentially, increasing price by more than the rate of inflation), 'under-recovery' (the opposite), or 'negative cost effectiveness' (presumably, being paid more than is merited) have, if not a scientific ring to them, at least the appearance of being grounded in good accounting practice, not to mention a series of powerful prescriptive connotations as well, so it is with some interest that the following interchange is reported. After giving details of the positive or negative performances of the various race groups to four decimal places (RP 41/1984, Table 7/4), the National Manpower Commission (NMC) annual report states that NPI data show that "...increases have been recorded in the labour productivity of each group" (RP 41/1984, p273). Then, in an interesting footnote - interesting because different elements of the bureaucracy seldom criticise one another publicly, the NMC comments that:

"Difference of opinion can exist on the question of whether it is in fact possible to make this kind of allocation between population groups". (RP 41/1984, p273n)

It is not, and it is precisely for this reason that the struggle for a living wage cannot be dismissed, as the NPI tries to, simply by asserting repeatedly that wages are out of line with productivity improvements.

24 Recall that this quote, adapted here to the South African case, is from the 1980 *Monthly Review* editorial cited in Chapter 1-1.

area of great sensitivity, that the authors of such nonsense should not be allowed to escape censure. Trade union hostility to the NPI was based on perception that the organisation was biased in favour of management - the REALST package, designed as a cost-accounting tool, is one of the ways in which that bias was made to operate.

In mid-1982, the CSS announced a rebased (to the year 1975) and revised set of results (SNR P12.1, 4 June 1982). These had the effect of changing growth rates in manufacturing, commerce and finance for the period 1960-70 from the 1970-based estimates reported above of 8,4; 7,8; and 5,7 per cent, to 6,5; 6,8 and 6,2 per cent respectively. Previously estimated growth rates of 3,9; 3,2; and 3,9 per cent respectively for the period 1970-76 for these major divisions now changed to 5,4; 6,0 and 3,8 per cent.²⁵ The Fosatu Challenge drew attention to the impact of rebasing and revision on manufacturing sector output estimates - revising the estimates caused the growth rate of manufacturing sector output between the years 1970-79 to rise from the 2,6 per cent *per annum* yielded by the 1970-based figures to 5 per cent *per annum*.²⁶

Because the sharp increases in growth rates for the sectors listed above were offset by an apparent decline in the contribution of the gold mining sector,²⁷ these revised results had little impact on the way that the performance of the economy was perceived. The guidance offered by the CSS to users on this question was as follows:

"Except for the effect of the weighting factor for gold mining, the effect of rebasing on the total growth was small.

Apart from rebasing, certain revisions to the gdp estimates were effected in the light of better data which became available and these revisions also had a **considerable** effect on the growth rate. The principal change brought about as a result of improved data, is the revision of the major division manufacturing. Recently the indexes of physical volume of manufacturing production were completely revised and rebased on 1975 and these results were used to revise the gdp estimates of manufacturing." (SNR P12.1, 4 June 1982) (Emphasis added)

The Fosatu Challenge accuses the CSS of making these changes 'quietly' (Meth, 1983, p1) and the SARB of introducing the changes in their publications with a "...wonderfully bland statement..." (p16). Not surprisingly, the authorities were not pleased by these suggestions. Swanepoel and Van Dyk (1983, p6) commented that:

25 Estimated from *South African Statistics 1982*, p21.7. The rebased and revised results given in Table 2.2 of SNR P12.1 of 4 June 1982 are identical to those in this latter source for the period 1969-81.

26 Endpoint selection here was somewhat arbitrary, as is the case in many of the NPI reports. More rigorous selection - peak-to-peak, for example, does not weaken the case. The years 1971, 1974 and 1981 mark upper turning points. Using the figures in *South African Statistics 1982*, it is a simple matter to show that the 1971-81 growth rate is 5,5 per cent and the 1974-81 rate is 5,1 per cent *per annum* (compounded). (p21.7)

27 Part of the decline was caused by changes (increases) in the price of gold and the valuation problems associated with this - see Appendix 2-1 below.

"Om dit as "stilweg" te bestempel, terwyl dit interdaad wyd bekendgemaak is, is nie alleen onbillik nie, maar bevat ook kwaadwillige insinuasies."

These two authors seem to confuse the process of distributing, to a large number of people and institutions on a circulation list, an official statistical release having in its small print an explanatory note which contains the adjective 'considerable', with the very different process of attracting people's attention to what was, when all is said and done, a major error. I held the view then, and I continue to hold it now, that the errors disclosed by the revisions and made public at the time of rebasing the statistics from the year 1970 to 1975 were so large that a press conference should have been called by the head of the CSS to explain how this happened. Economists of every persuasion should have been warned that part of the recent South African economic history needed to be rewritten. Instead, the event appears to have been almost completely ignored by the media.

To check the impact of the CSS' attempts at making these changes "...wyd bekend...", (widely known) every issue of the *Financial Mail* between the beginning of June 1982 and the end of June 1983 was scanned for evidence of the message about the errors in the statistics having had some impact. Apart from the publication of a summary of the findings of the Fosatu Challenge in the issue of April 29, 1983, there is no sign at all that the *Financial Mail* was aware of the errors. In the September 24 1982 edition there is an article titled "More pay for less work - why?" which repeats the NPI claim cited above about "...employees being paid 20% more for producing only 4% more..." Commenting on the inflationary tendencies to which this gave rise, the author of the article observed that "...[T]he key lies in raising wages at the same rate as productivity grows." The article states that:

"From 1970 to 1980 the country enjoyed an average real growth of 3,1% a year, yet productivity growth lagged behind at only 0,3% a year."

Adding that:

Less than 10% of our development over the decade grew out of better labour productivity; the rest was little more than good luck in commodity markets." (*Financial Mail*, September 24 1982, p1474)

These conclusions all rest on the incorrect (1970-based) statistics. If what is arguably South Africa's most influential weekly periodical, and one which moreover has displayed a keen interest in matters statistical over the years, is unaware of corrections which the CSS allegedly made widely known, then there is something wrong. The strong likelihood, given the CSS' reluctance to acknowledge error, is that the CSS simply did not take any of the obvious steps that could have been taken to ensure that the revised results did indeed become widely known.

If the intention to do so had been serious, a press conference such as that suggested above, would have ensured that a matter as important as this received the wide publicity it merited.

The Fosatu Challenge was accorded fairly wide publicity, and in the face of this, the NPI sought to reassure its clientele with a refutation of the claims made about the poverty of their workmanship. It appeared in an article in *Productivity SA* (NPI, 1983), a draft of which had been circulated earlier in the year. The NPI effort was based largely on the document written by two staff members of the SARB (Swanepoel and Van Dyk, 1983).²⁸ The NPI used the occasion to affirm their faith in the quality of the information made available by the national accounting statisticians in South Africa, stating that:

"It must be stressed that the NPI uses official publications (mainly from the CSS and the SARB) in calculating productivity statistics. Although we cannot guarantee the accuracy of these figures, we do place very great store on the authority of these two institutions and we have never had any reason to challenge their figures."

Responding to the fact that the revised output estimates raised the productivity growth rate for the period 1970-79 from 1,1 per cent *per annum* to 2,3 per cent, the NPI stated that this was:

"...still not a growth rate to be proud of and which would certainly not have changed any of the conclusions drawn previously." (NPI, 1983, p7)

If the appropriate standards by which to judge were the performances of Japanese, Korean or Taiwanese economies, the NPI might have been able to make some sort of case. When it is recalled, however, that the period 1970-79 encompassed two downswings, one of them, the almost three-year long 'Soweto' depression, the unwillingness on the part of the NPI to look anew at the results pointed to something other than a commitment to scientific method.

"Hegel remarks somewhere that all facts and personages of great importance in world history occur, as it were, twice. He forgot to add: the first time as tragedy, the second as farce." (Marx, *The Eigtheenth Brumaire of Louis Bonaparte*)

History as farce

More than a little literary license is necessary to elevate the activities of the NPI to events of world historic significance. So also, is a certain relaxation of standards required to see their

28 Recall that the three pieces of work under discussion here are reproduced as Appendices 1-1; 1-2 and 1-3 and the end of the study.

erroneous report of the productivity performance of the manufacturing sector during the 1970s as tragedy. Nonetheless, their activities are not without consequence, and there is a grim humour in the fact that having constructed an incorrect interpretation of the 1970s on the basis of a set of incorrect figures supplied by the CSS, the NPI has repeated the performance for the 1980s.

This time, however, the (acknowledged) absolute error in output levels over the period under consideration (1979-89) - about 13,6 percentage points - is smaller than it was in the period 1970-79 (a whopping 28,6 percentage points),²⁹ and hence the impact on the NPI's productivity calculations is apparently less severe. There is, however, no cause for the NPI to celebrate. Referring to Figure 1 above, it may be seen that the high upper turning point reached in 1981/82 makes peak-to-peak growth rates thereafter either quite modest, or turns them into declines. The official NPI story up until the present (November 1994) still has output falling over the period 1981-89 as it does in the 'Unbenchkmarked' line in Figure 1 (NPI, 1994b, p35). The peak-to-peak decline registers about 0,3 per cent *per annum*. If my estimates of the 1981/82 and 1988 'benchmarks' of about 123 and 133 respectively are correct, then output probably peaked in 1989 at something in excess of 136-137. This would give a peak-to-peak output growth rate of about 1,2 to 1,3 per cent *per annum*. Employment fell slightly over the period, so productivity growth was probably in the region of about 1,3 to 1,4 per cent. Not, as the NPI has previously said, a growth rate of which to be proud, but given the circumstances (the insurrectionary phase after 1985 and another three-year downturn), the turnaround from falling productivity to slightly positive growth is quite a respectable performance. In the 1970s the NPI error in estimating the productivity growth rate was 1,2 per cent *per annum* (2,3 vs 1,1); for the 1980s, it could be as high as 1,6 per cent *per annum* (-0,3 vs 1,3).

This, however, is only half of the story. When confronted by critics of their workmanship, national accounting statisticians sometimes claim that although they may be unable to measure absolute values with any certainty, their estimates of trends are usually reliable. A fairly elastic notion of reliability is necessary to make the 1970-based estimates acceptable, and an equally relaxed set of standards is required to reconcile the 'pre-March 1992' and the 'Reconstructed' figures in Figure 1 above. It is true that the two series move in roughly the same direction, but the magnitude of the relative movements is important for another reason. In the paper called "South Africa's Thatcherite Productivity Miracle: 1979-89" (Meth, 1991a) I argued that

29 With 1970 set equal to 100, the 1970-based output figure for 1979 was 126,4 whereas the 1975-based level was 155. These figures are estimated from *South African Statistics 1980*, p21.7 and *South African Statistics 1982*, p21.7 respectively.

comparisons between two, possibly equally unreliable output series were not the proper way to gauge the significance of the errors made by the CSS and the NPI.³⁰ The appropriate comparison (and then only after sufficient *caveats* have been issued) is rather between the (reasonably reliable) wage growth figures and the (frequently unreliable) output growth figures. The difficulty in performing this comparison lies in the problem of choosing a deflator for nominal earnings.

There is a debate on whether the real compensation ought to be measured by the basket of goods produced in a country, or that which is consumed (Lawrence and Slaughter, 1993, p169). If the latter, then the correct deflator to use is the Consumer Price Index (CPI); if the former, then the Production Price Index (PPI) should be used. Both measures will be estimated below, first the consumption wage (CPI deflator) and then the product wage (PPI deflator). It is common practice to refer to the consumption wage (loosely) as either the real wage, or sometimes real earnings. Drawing attention to the distinction between the product wage and the consumption wage, in effect signals the need for greater than usual care to be exercised in the way in which analytical categories are handled.³¹ This is of more than mere terminological significance - Lawrence's and Slaughter's basic hypothesis, for example, is that contrary to

30 Some of the reasons for comparing trends in South Africa and Britain will be referred to in the next chapter.

31 Despite the fact that the topic has been on economists' research agendas for decades, the relationships between real (consumption) wages, and the product wages which underlie real unit costs are not easy to disentangle, nor do they relate to the business cycle in any simple way. Modern analytical technology may have facilitated a breakthrough, but I have not come across it. Certainly, the following claim, made nearly fifty years ago, could probably have been advanced with equal confidence today. In it, the author observed that:

"...in a world where imperfection of competition is a prevalent phenomenon... it is impossible to predict, on purely *a priori* grounds, the manner in which the fluctuations of the average rate of real wages... would be correlated with the short period fluctuations in employment." (Tsiang, 1947, p40)

Movements in product wages likewise can only be understood by empirical examination. Although Tsiang finds that product wages "...in the system as a whole..." (in Britain and the USA) appear to be "...positively correlated with the percentage of unemployment" (p56), manufacturing data for the two countries fail to display stable relations of this type. He observed that:

"...there is no definite correlation, positive or negative, between the productivity of labour and product wages, nor between productivity and employment or output." (p67)

Fluctuations in the product wage in manufacturing industries were due largely to "...variations of raw material prices relative to money wages" (p67).

There are two lessons to be drawn from this work. In the first place, the importance of the rôle of inputs other than labour and capital in productivity studies in South Africa has been neglected, or had lip service paid to it for too long. It has been acknowledged, for example, by van Loggerenberg that:

"...the signs and extents of productivity change and price recovery change could vary if materials were included in the analysis. Indeed," he continues, "the values [of productivity estimates] are sensitive to the inclusion of materials in the analysis." (1986, p43)

Materials (and energy) have not been neglected elsewhere - two interesting examples of the use of the so-called 'K,L,E,M' (capital, labour, energy, materials) approach to productivity analysis, may be found in the studies by Duggan and Clem (1985) and by Norsworthy and Malmquist (1983).

The second lesson to be drawn from the Tsiang piece comes from the reference to the difficulties of *a priori* prediction of short-period movements in a world characterised by imperfect competition. This raises the question of the need for a counterweight in the form of event-related analysis, to trend analysis, the currently dominant form. It appears that little if any work of the type done by Tsiang has been performed for the South African economy, so his results spell out an interesting research agenda. Tackling such questions is, however, a major task in its own right.

popular belief, the 'real wage' in the USA has not declined as much in recent times as is claimed (1993, p169). The conventional comparison between the consumption wage and output levels which discloses an output/wage gap is argued to be inappropriate. When the product wage is used, the gap almost disappears. In passing, it is also argued that no major redistribution away from labour towards capital occurred.

In South Africa, the argument has been conducted differently - here, the NPI has generally used comparisons between the wage in nominal terms and output in real terms. Apart from the fact that this avoids the complexities of the debate over the appropriate deflator for wages, it also generates much starker, and hence more newsworthy, disparities. In any event, the NPI has used the low (incorrect) output growth figures to insist that wage and productivity growth were so out of balance as to have had serious consequences for the South African manufacturing sector. Using the higher levels of output growth now published by the CSS, it is clear that the NPI's conclusions are suspect. As stated in Chapter 1-1, it is not claimed in this study that productivity performance has been satisfactory - merely that the NPI has persistently overstated the 'poor productivity' case. When one uses the higher estimates in Figure 1, (the estimates defended in this study), which probably lie closer to the truth of the sector's performance, the longer term performance appears almost respectable. The responsibility for the misrepresentation of the 'true' performance of the sector lies both with the NPI and the CSS - the latter for producing seriously incorrect basic data, and the former for using it uncritically. As is shown below, the NPI were warned as early as 1987 that the manufacturing sector figures were beginning to drift.

So much for the propaganda aspects of the matter - the next thing is to spell out briefly some of the characteristics of the business cycle and the political climate within which those results are embedded. This provides a context within which to view the evidence of changing productivity and wage levels. Although the character of the business cycle has changed in recent times, movements in economic variables during the different phases of the cycle have about them some predictability. One would expect, for example, that as the upper turning point is reached, wage growth would outstrip productivity growth. The rate of growth of production may be expected to fall rapidly, often becoming negative. The overhang of wage claims from the rapid growth phase of the cycle, however, takes time to work its way through. By contrast, in the early stages of an upswing, with labour having been disciplined by a phase of depressed activity levels, one would expect wage growth to be outstripped by productivity growth. Overlay this pattern with episodes like the rebirth of African unionism in the early 1970s (and its resurgence in the 1980s), the Soweto and the Rubicon crises, and one can make a reasonable guess at what the figures for the South African manufacturing sector should look like. The results neither surprise nor disappoint.

This may be seen by examining the results in Table 1-2.2 below. The table contains five sets of estimates of the level of output per worker, with 1970 set made equal to 100. It will be observed that for the period 1970-79, the results in Series 2, 3, 4 and 5 are identical. This is because there are only two sets of results available for the period 1970-79 - the old 1970-based figures in Column 1, and the 1975-based results that replaced them.³²

Table 1-2.2 Output per worker, consumption wage and product wage 1970-89

	<----- Output/worker series ----->					<----- 'Real' wage ----->	
	1970-based 1	Unbench- marked 2	Exuber- ant 3	Conserv- ative 4	Recon- structed 5	Consumption wage 6	Product wage 7
1970	100,0	100,0	100,0	100,0	100,0	100,0	100,0
1971	98,2	102,5	102,5	102,5	102,5	102,7	103,6
1972	98,6	104,5	104,5	104,5	104,5	103,4	104,7
1973	101,2	108,4	108,4	108,4	108,4	104,8	103,5
1974	101,4	109,0	109,0	109,0	109,0	108,3	100,8
1975	99,8	109,1	109,1	109,1	109,1	111,1	100,6
1976	95,5	107,6	107,6	107,6	107,6	114,7	101,7
1977	90,9	105,6	105,6	105,6	105,6	114,9	100,0
1978	94,1	112,2	112,2	112,2	112,2	115,7	101,4
1979	97,8	118,6	118,6	118,6	118,6	117,1	102,3
1980		125,6	131,6	127,5	127,2	121,3	104,6
1981		126,4	142,3	133,4	131,3	125,2	109,6
1982		120,5	134,6	128,1	122,4	129,4	114,0
1983		118,8	142,3	132,7	125,7	129,5	116,0
1984		120,5	143,7	134,2	131,3	133,0	122,8
1985		117,2	138,2	131,0	131,9	128,2	119,3
1986		116,6	137,2	129,9	131,8	123,0	114,3
1987		118,7	138,3	132,2	134,7	122,4	114,9
1988		125,8	149,3	139,3	139,5	126,6	117,6
1989		125,4	153,3	140,5	140,4	129,6	117,9

Notes:

Employment estimates for the five different productivity series are from *Productivity Focus 1994*, p35.

Output estimates for the different series are derived from the following sources:-

Series 1 - the 1970-based figures in *South African Statistics 1980*, p21.7.

For Series 2-5, the 1970-79 values may be taken from any of several sources - those in this table come from *Productivity Focus 1994*, p35. The estimates for Series 3, 4 and 5 do not go back further than 1978 or 1979. The 1975-based figures for the years 1970-79 are the same, with minor revisions, as those given in most versions of the Unbenchmarked estimates.

For the period 1979-89, the output estimates are derived from the following sources:

Series 2 - Unbenchmarked: SNR P3041.3, 12 September 1990.

Series 3 - Exuberant: Meth (1992, Table 12, Column 7).

Series 4 - Conservative: Meth (1992, Table 12, Column 8).

Series 5 - Reconstructed: SNR P3041.3, 12 November 1993

Consumption wage is the set of estimates of 'real earnings' in *Productivity Focus 1994*, p37 (nominal wage deflated by the CPI).

Product wage is the NPI's nominal wage estimate (*Productivity Focus 1994*, p37) deflated by the PPI. The PPI's are from *South African Statistics 1986* and *1990*, p8.6.

32 These latter figures have had minor facelifts performed on them by the various authorities that publish and republish them, but essentially, they remain in the form that appears in *South African Statistics 1982*.

Had they been correct, the Column 1 figures would have warranted fully, the drawing of the most pessimistic conclusions. The under-estimate of the productivity level in 1979 of almost 21 percentage points (Columns 1 and 2) is, however, a spectacular reminder of just how wrong the CSS figures can be. Certain other features of the figures in this table stand out quite prominently. Firstly, by the end of the first half of the period, consumption wage and productivity growth had just about kept pace with each other, despite the major recession and the wage surge of the mid-1970s. Secondly, by 1989, the end of the period, productivity growth exceeded consumption wage growth, even as measured by the Conservative and the Reconstructed estimates, and this notwithstanding the massive output drop reported in 1981-83.³³ Even more surprising is the performance of the product wage. Over the nine-year period starting in 1970, it barely grew at all, whereas the period 1980-84 saw it go leaping up by about 18 percentage points while labour productivity, according to the most optimistic estimates grew by 12 percentage points, and by the most pessimistic estimate, actually declined. Viewed in peak-to-peak terms, ie, from 1974 to 1981, and from 1981 to 1988, labour productivity in the earlier period grew by somewhere between 13 and 31 percentage points (Reconstructed vs Exuberant estimates), while the product wage grew by about nine points. In the later period; both labour productivity and the product wage grew by about nine points, clearly, not a disaster, but equally clearly also not a sparkling performance. One would expect the 1989/90 results to be more disappointing than those for 1988/89. With output falling, employment rising slightly and the consumption wage stabilising,³⁴ this is indeed what happens.

Comparisons of the relative growth of consumption wages and labour productivity on a year-on-year basis are given in Table 1-2.3, and of product wage and labour productivity in Table 1-2.4. A positive (bad) result means that wage growth exceeded productivity growth, and vice-versa for a negative (good) result. To summarise the year-on-year changes in Table 1-2.3 for the earlier period, the nine observations in Column 1 figures have three 'good' years, five very 'bad' ones, and one mediocre. The Column 2 figures by contrast, have one very 'bad' one, three middling 'bad' ones, one mediocrity and four 'good' ones, two of them extremely so. Note the match of these relative movements against the pattern predicted by the experience of previous business cycles. With the exception of the one very 'bad' year, one might almost say that the results are 'normal', and that for a highly abnormal society in the throes of its deepest depression in about forty years.

33 This is shown in Figure 1 above. Whether or not it actually occurred is a matter of some doubt, a question that has been raised above.

34 According to the NPI's *Productivity Focus 1994*, real output fell from 174,0 to 169,7 (1970=100), labour input rose slightly from 139,8 to 140,1 (p35), and real earnings were stable at 129,6 (p37).

Table 1-2.3 Year-on-year change in consumption wage per worker minus change in output per worker (per cent)

	1970-based 1	1975-based 2		Unbench- marked 3	Exuber- ant 4	Conserv- ative 5	Recon- structed 6
1970-71	4,5	0,2	1979-80	-2,8	-8,7	-4,6	-4,4
1971-72	0,3	-1,2	1980-81	3,1	-6,9	-2,0	-0,2
1972-73	-1,2	-2,5	1981-82	10,1	12,0	9,5	13,0
1973-74	3,3	2,9	1982-83	1,8	-7,7	-4,5	-3,2
1974-75	4,3	2,7	1983-84	1,8	2,2	2,0	-2,0
1975-76	7,9	5,1	1984-85	-1,6	0,7	-1,6	-5,4
1976-77	4,8	2,2	1985-86	-4,5	-4,3	-4,1	-5,1
1977-78	-2,4	-5,8	1986-87	-2,7	-1,6	-2,9	-3,5
1978-79	-2,3	-5,0	1987-88	-2,9	-6,8	-2,8	-0,6
			1988-89	3,4	-1,0	1,8	2,1

Note: The 1975-based series in Column 2 is the same as the Unbenchmarked series.
 Sources: The six series are obtained by subtracting the estimates of output per worker in Table 1-2.2 above from the figure for consumption wage per worker (nominal wage deflated by the CPI) in Column 6 of that table.
 The series for the period 1970-71 to 1978-79 in Columns 1 and 2 use the output estimates in Columns 1 and 2 of Table 1-2.2 above. Those for the period 1979-80 to 1988-89 in Columns 3-6 are obtained from the series in Columns 2-5 of Table 1-2.2 above.

Table 1-2.4 Year-on-year change in product wage per worker minus change in output per worker (per cent)

	1970-based 1	1975-based 2		Unbench- marked 3	Exuber- ant 4	Conserv- ative 5	Recon- structed 6
1970-71	5.5	1.1	1979-80	-3.4	-9.3	-5.2	-4.9
1971-72	3.2	1.7	1980-81	2.9	-7.1	-2.3	-0.5
1972-73	1.0	-0.3	1981-82	9.6	11.4	8.9	12.5
1973-74	3.4	3.0	1982-83	5.3	-4.1	-1.0	0.4
1974-75	5.2	3.6	1983-84	2.0	2.3	2.2	-1.9
1975-76	8.0	5.2	1984-85	6.9	9.2	6.8	3.0
1976-77	8.3	5.6	1985-86	4.3	4.6	4.7	3.7
1977-78	0.5	-3.0	1986-87	1.5	2.6	1.3	0.8
1978-79	-0.1	-2.8	1987-88	-3.5	-7.4	-3.4	-1.2
			1988-89	4.0	-0.3	2.4	2.7

Note: This table differs from Table 1-2.4 only in that the product wage (nominal wage deflated by the PPI) in Column 7 of Table 1-2.2 is used in place of the consumption wage in Column 6 of that table.

After 1979, the series become more interesting. The 'Unbenchmarked' results in Column 3 of Table 1-2.3 also provide ample cause for gloom, and once again would justify pessimism. The Column 4, 5 and 6 results (respectively Exuberant, Conservative and Reconstructed) suggest otherwise. The 'bad' 1981/82 results, the outcome of a four-percentage point rise in real earnings after the upper turning point had been passed, coupled with the huge, but unsubstantiated drop in output growth has a significant influence on the results. It is important, as noted above in the discussion on my benchmark figures (the square markers in Figure 1) to

sort out the question of the position of the 1981/82 markers as well as the experience in the period immediately thereafter. Even after the body blow which the 'bad' 1981/82 result delivers to the results, all three of the Column 4, 5 and 6 figures in Table 1-2.3 (respectively Exuberant, Conservative and Reconstructed) yield a 'good' outcome in a peak-to-peak analysis for the years 1981-89. In each case, the percentage increase in productivity is greater than that in the consumption wage.

For the period 1982-1989, (ie, ignoring for the moment the 1981/82 figures), the labour productivity/consumption wage comparison yields five 'good' years out of seven for the Column 4 figures (the exuberant estimates). The Column 5 figures (Conservative) the same, and the column 6 figures (Reconstructed) six out of seven. The distribution of 'good' and 'bad' years is roughly what one would expect. There was a mini-boom in 1984 that punctuated the downturn - this saw a rise in the consumption wage from the level at which it had stabilised after the excesses of 1981/82. After 1984, the consumption wage declines sharply until 1987, yielding the string of 'good' years which last all the way to the upper turning point in 1988/89. It fell between 1985 and 1987, while productivity was either static or climbing ever so slightly. In the earlier period, it would seem from the figures in Column 2 it would appear that most of the relative gains that workers made on the swings during the years 1973/74 to 1976/77 (mainly years of downswing) they lost on the roundabouts in the following years.

The product wage comparison in Table 1-2.4 tells a somewhat different story. Taking all ten observations for the period from 1979-80 onwards, the Unbenchmarked figures suggest that there were eight bad years and two 'good'. The Exuberant estimates yield five 'good' years and five bad; the Conservative six 'bad' and four 'good', and the Reconstructed, seven 'bad' and three 'good'. As noted above in the discussion to Table 1-2.2, even the Exuberant figures do not give a sparkling performance. The difference between the performance reported by the Unbenchmarked figure over the cycle 1981-88/89 - a small decline in labour productivity coupled with an eight-percentage point rise in the product wage would have constituted grounds for grave concern, had it occurred. The six-, seven-, or eight-percentage point productivity improvements registered by the other measures over the cycle make the results tolerable, even if only barely so. All the more reason then, that a thorough investigation be undertaken to establish output levels as accurately as possible, and to look as well into the PPI for any signs of erratic performance. As will be seen in Part II of the study, this index is not above suspicion.

Three unheeded warnings

It is clear from Table 1-2.2 above that productivity performance estimates based on the now-supplanted 'Unbenchmarked' series (Column 2) are considerably below the new official estimates in Column 5 (the 'Reconstructed' figures). The error in 1989 reaches 15 percentage points once more. If, as seems likely, the true value lies somewhere between the 'Exuberant' and the 'Conservative' values, that error will be larger still - it could easily be nudging the 20 percentage point mark.

As before, one cannot claim that the NPI has falsified the record. That does not mean, however, that the organisation is blameless. By using the CSS output series uncritically once again to paint a picture of unrelenting gloom the NPI is back in the Ruritanian position. This time, however, they ignored *en route* several clear warnings (to be described below) that the CSS' figures were once again sliding into error. As explained towards the end of Chapter 1-5, the outcome of attempts this time to extract a retraction has been slightly better, and promises to become better still. This has come about primarily because of a change in personnel at the NPI, and of course, because of the changes in the political system to one that is now much more sensitive to Cosatu's position.

A lesson to be learned from this débacle is that national accounting data are often not a reliable enough vehicle on which to rest weighty conclusions. Analysts venturing into sensitive areas like the wage/productivity relation have several obligations. One of these is to be mindful of the slightest suggestion that error is creeping into the national accounting data. National accounts are not like lightning - error can strike twice in the same place. It does so for the simple reason that both employment and output are difficult to measure.

Failure by the NPI to heed the three warnings of potential error in the data they have used to produce the existing incorrect analysis amounts to negligence. Admittedly, the warning signs appeared first in the employment figures, but since employment is easier to measure reliably than output, alarm bells should have started ringing - having been caught flatfooted once, the NPI should have requested an immediate investigation by the Statistics Council. The first warning came as early as 1987. It was contained in a news release giving preliminary estimates of the 1985 Manufacturing Census results. SNR P10 of 20 August 1987 suggested that total employment was 1 515 000 instead of the 1 327 000 that the survey results were reporting at the time.³⁵ The second came when the final results of the census were incorporated into a news

³⁵ This latter figure continued to be accepted for quite a while afterwards - see, for example, *South African Statistics 1988* (p7.15). The CSS drew attention to the preliminary nature of the figures by underlining the word in the explanatory notes, but once again provided somewhat less than satisfactory service in failing to draw user's attention to this very large error. Some users see many news releases each month - it is not easy to discover when major changes take place unless attention is drawn to them.

release on employment and wages which, in setting the 1985 employment at 1 428 988, showed the existing official estimate to be incorrect by nearly eight percent - this was presented in Statistical News Release (SNR) P0242.2 of 19 October 1989. This benchmark employment figure naturally made all other existing figures for the five years or so on either side of it incorrect as well. Then in May 1990, I presented a paper at a conference on errors in productivity statistics (Meth, 1990b) at which the NPI was represented. This paper drew attention to the errors in employment estimates and warned of the likelihood of further errors in the output figures. A shortened version was published in *Indicator SA* (Meth, 1990a). The journal carried a response from the NPI (du Plooy, 1990), which instead of addressing the question of error seriously, sought to argue the essentially apolitical nature of the NPI's productivity campaign.³⁶ The NPI blundered on, still using the incorrect employment figures until about 1992, when they changed over to Roukens de Lange's (1990) Standardised Employment Series (SES).³⁷

A second obligation on the part of researchers working in sensitive areas is to use whatever corroborating evidence is available to support their conclusions. In Part II of the study, considerable effort is devoted to demonstrating that consistency checks can be performed on the national accounting magnitudes (and should be performed wherever possible) - the same applies to the productivity statistics.

One indicator of conditions in the manufacturing sector is the distribution of value added between labour and capital. We will look now at signs of a significant redistribution against wages (remuneration) and in favour of profits (surplus). The results may not have been sufficient to stop the NPI's anti-redistribution campaign, but they should at least have been sufficient to change the slant of it. More importantly, they should also have aroused suspicion about the quality of the output estimates.

To him who hath

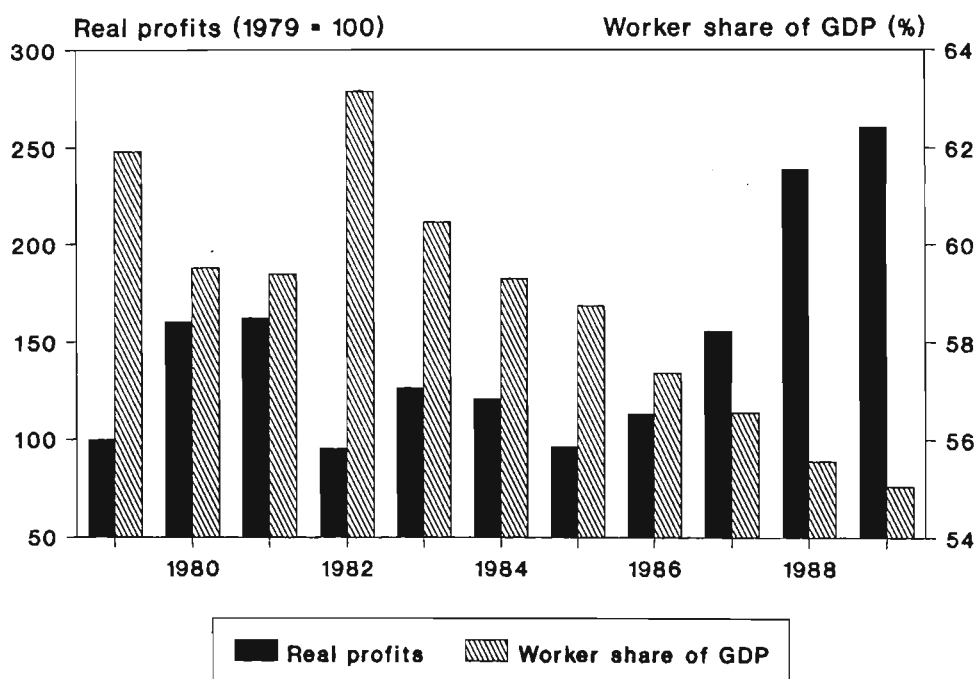
One part of the story - cast in terms of the relationship between output per worker and real (consumption) wage - has been related above. What is equally interesting, and what imparts a distinctly Thatcherite stamp to the period is the distributional impact of the early-80s recession

36 The case put up by du Plooy was weak - it misquoted Lenin in an attempt to establish a convergence of views on the question of productivity improvement. I responded to this in Meth (1991a).

37 *pers comm.* Ms K Liebenberg (NPI), September 1994. At the time of writing (November 1994), the incorrect output estimates (the 'benchmarked' series) still formed the basis of the NPI's productivity estimates.

and the late-80s growth spurt. Although the official estimates are a little suspect, it does seem that apart from the deep recession years of 1982-83, and 1984-85, capital appears to have done better during the troubled 1980s than labour.³⁸ Despite the collapse of the gold price (and with it, the economy) in 1981, and the onset of the insurrectionary phase in South African politics following P W Botha's 'Rubicon' speech in 1985, there is evidence to suggest that the absolute level of profits in real terms was considerably higher in 1989 than it had been a decade earlier. This was caused in part by a redistribution against workers. Figure 2 shows this rise in real profits and the redistribution of 'factor incomes' from labour to capital.

Figure 2 Real profits and worker share of GDP, 1970-89



The 'real' profit estimates in this figure are drawn from a data set compiled by the Industrial Development Corporation (IDC), primarily from information supplied by the CSS.³⁹ They

38 Some care needs to be exercised when talking about profit levels - partly because the data are of dubious reliability, and partly because profits in an economy characterised by highly uncompetitive markets need bear but scant relation to changes in productivity. Specific conditions may permit rapidly rising profit levels and redistributions from one class to the other, but when these change, the gains can readily be reversed. As noted below, the course charted by profits in the period under consideration is, in many ways, an index of the malaise besetting the economy.

39 No official estimates in 'real' or constant price terms of the value of gross surplus are made. Reference is made in Chapter 2-2, under the heading 'A digression on the use of the CPI as deflator', to the difficulties of estimating GDP(I) (the factor income approach to estimating GDP) in constant price terms. In essence, the problem arises because suitable deflators for the components of value added, and especially of gross surplus, are difficult to determine. This discussion is pursued in Chapter 2-7. As noted in Chapter 2-2 below, however, a rough-and-ready technique for estimating 'real' profits is suggested in the 1981 guide to the British national accounts. See Copeman, 1981, p3-5. It is interesting to note that the IDC (1992) simply ignores these problems, and produces a data set which is

show that the slump of 1982-85 was followed by a surge in real profits to record levels in 1989. The IDC estimates suggest that profits (net of interest and depreciation) totalled about R5,5 billion (in constant 1990 prices) in 1979. They grew to about R9 billion in the quite exceptional year of 1981, after which they plunged to R5,3 billion in 1982. The fitful recovery of 1983/84 saw a slight improvement, but 1985 profits were only a little above the disastrous 1982 figures. Profits then soared to R14.6 billion in 1989 (IDC, 1992, p6.3). Corroboration of these findings may be found in a study by Kantor (1989) that looks at rates of (inflation-adjusted) growth of earnings of companies included in the industrial share index on the Johannesburg Stock Exchange. According to Kantor, rapid growth occurred after 1987, to place 1989 earnings (in absolute terms) almost on a par with the "...extraordinary peak of 1981" (1989, p3).

This increase in the mass of profits (and probably in the rate of profit as well) resulted from two processes. The first of these was the significant change in the division of net output between remuneration and gross operating surplus depicted in Figure 2 above. The figures used to generate this chart are the Remuneration and GDP from *South African Statistics 1990* (pp21.9 and 21.11). The increase in the worker's share from 59,4 percent in 1981 to 63,1 percent in 1982 is in line with the rise in real earnings (the consumption and product wage) coupled with the drop in productivity recorded in all three productivity series. The year in which workers secure a maximum share of output coincides with the year in which employment peaks.⁴⁰ Workers' share is dependent upon the absolute size of the workforce and the wage level, but despite the fact that both the consumption and product wages (the last two columns of Table 1-2.3) continue to rise after 1982 (until 1984), they do not do so rapidly enough to offset the redistribution towards capital that commences once employment levels start to fall. From 1982 onwards (the point at which Thatcherism in South Africa begins to bite?) the share of remuneration⁴¹ in net output declines steadily. A decade of vigorous trade union action (1979-89) saw the aggregate average consumption wage (real earnings) rise by about 12,5 percentage points (the product wage by about 16 percentage points) while output per worker rose by somewhere between 22 and 30 percentage points. Retrenchment, which affected African workers most,⁴² was thus coupled with rapidly rising profits - the stereotypical Thatcherite outcome.

(continued)

components, from which they produce estimates of one component - profits - suitably deflated, to render it in constant price terms.

40 Employment estimates are from SNR P0242.2, 19 October 1989.

41 Problems exist in these estimates as well - these were discussed in Meth (1991d and 1992).

42 Employment of African workers in manufacturing peaked at 838 757 in 1982 and fell to 771 300 in 1988, a decline of eight per cent. For whites, employment also peaked in 1982 (at 329 438), then fell to 312 400 in 1987, a decline of about five per cent. See *South African Statistics 1990*, p7.15.

The second cause of the surge in profits reported above results from a somewhat unhealthy redistribution within total gross operating surplus. This points to another important feature of this phase of South African economic history - an investment strike by capital that has continued almost until this day.⁴³ Surplus consists of interest payments, allowances for depreciation and profits (on which taxes are paid). According to the IDC, profits leapt from 24,7 percent of gross operating surplus in 1985 to 53,3 percent in 1989. Over the same period, allowances for depreciation fell from about 47 percent of gross surplus to about 36 percent, and interest paid on loans and debentures from about 34 percent to less than 17 percent. The fall in allowances for depreciation is consistent with a running down of the capital stock, and the fall in interest paid is the obvious concomitant of the fact that less borrowing for investment purposes was taking place.⁴⁴ Bearing in mind the difficulties of estimating the value of gross surplus in 'real' terms are referred to in Footnote 39, to the extent that one is able to obtain some sort of indication of real changes in gross surplus, it would appear that the falls in the relative magnitudes of depreciation allowances and interest paid were not large to offset the huge growth in profits. Gross surplus in 'real' terms (constant 1990 prices) obtained using the deflator the IDC used to estimate real profits, rose from about R21,7 billion in 1985 to about R27,3 billion in 1989. Using the price index for imported inputs, this shrinks a little from about R22,2 billion in 1985 to about R27 billion in 1989. Applying the same pairs of indices to the depreciation estimates, it would appear that the allowance was roughly the same in 1985 as it was in 1989, ie, about R10 billion.⁴⁵

Changes in profit rates, the relative shares of capital and labour, and in the relative wages of black and white workers are dealt with at some length in Nattrass (1992, Ch. 1). Even without access to the upwardly revised output estimates now available, she concluded that:

"While it is true that wage growth faster than that of productivity has contributed to the profit squeeze, it is not true that this has been the 'fault' of workers.

For all the fine words we hear from the private sector about the marvels of free enterprise and path-breaking entrepreneurs, the truth of the matter is that South African industrialists are over-protected and hamstrung by poor (often racist) management. The pickings have been too good for too long from the domestic markets and patronage networks with the state. The development of a dynamic and robust export sector has suffered as a result.

43 Most of such investment as did take place in the manufacturing sector was skewed towards highly capital-intensive strategic installations designed to permit the economy to withstand conditions approaching siege. These investments would have had the effect of raising labour productivity, but if the capital goods were poorly utilised, multi-factor productivity would have suffered. Until such time as reliable capital stock estimates are available, particularly for the two Sasols and for the Armscor projects which absorbed much of the investment, the question of the impact on capital productivity must remain unanswered.

44 The fact that the three components of gross surplus for which percentages are given here do not total to 100 percent is not a cause for concern. A category called 'Other' needs to be added in, and it is also necessary to deduct interest received on investments. See IDC, 1992, pp1.5-1.6.

45 The data used to make these estimates is given in IDC, 1992, pp1-5.

Rather than languishing behind high tariff walls, South African manufacturing must be further exposed to the cold winds of international competition. This can be done gradually to minimize disruption in production but it must be done steadily. Very little mercy must be shown to those capitalists who run bleating to Pretoria for protection." (1992, p22)

Corroboration of her finding that the profit share as well the rate of profit rose during the period 1981-88,⁴⁶ coupled with upwardly revised output figures which ameliorate the declining capital productivity figures she reports (Nattrass, 1992, Table 3, p10), serves to strengthen the conclusion that despite the adverse conditions, manufacturing capital prospered after the mid-point of the decade. It did so in much the same way as those capitals that survived the first few years of Thatcherism. This may not be the healthy prosperity one would like to have seen, but it is an interesting demonstration of the capability of capitalism to survive major crises, political and economic. Starry-eyed Marxists, pitting crude analytical tools against 'monopoly capitalism' and engaging in fantasies about the impending victory of socialism in South Africa, as many did in the late 1980s, were once again hopelessly wrong.

To conclude this section of the chapter we consider the obvious question which the discussion on net output (value added) raises. 'Real' profits (and 'real' gross surplus) and both the consumption and product wages increased over the decade 1979-89, and, at the same time, a major shift in the distribution of net output between labour and capital was taking place (labour's share was about 62 per cent of the total in 1979 and 55 per cent in 1989). That being so, one must ask how this could be reconciled with the slow growth in output reported by the CSS and inserted by the NPI into its productivity estimates?⁴⁷ It is not possible to reward the factors of production in the way that has reportedly been done out of a volume of output that grows as slowly as the CSS used to allege that it did. A simple consistency check of this sort would have added to the alarm raised by the discovery by the CSS that the employment figures were incorrect.

Woods and trees: a disaggregated view

Aggregate figures always conceal details, that is, after all, what the aggregation process is supposed to do - suppress extraneous information so that the broader picture may be seen. The use of aggregates is not, however, always innocent. Sometimes there may be a certain sleight of hand involved (more 'statisticulation'). The analysis above has been conducted in aggregate terms because much of what the NPI says about productivity at a national level is cast in those

46 Nattrass' profit rate findings are generated using a different data set.

47 See for example, the output series in the NPI's *Productivity Focus 1990*, pp52-53.

terms. There is, of course, much more to the productivity and wage story than the set of aggregate comparisons reviewed above. As everybody knows, the rates of change of wages for African and white workers over the period have been strikingly different. The beginnings of an attempt to explain why this is so appear below - the topic is pursued in the following chapter. For the meanwhile, to offset any false impression which may have been created by the relatively favourable wage/productivity comparisons performed above, it is necessary to show how rapid increases in African wages were counterbalanced either by moderate increases, or at times, by decreases in white wages.

Precisely what the significance was of the different patterns of wage increases for employment of the two groups of workers, it is difficult to say. It is known that wage demands by African workers aroused widespread hostility and that this probably contributed to increasing capital intensity (Welcher, 1991). Whether this substitution was based on the kinds of calculations which 'firms' make in the idealised world constructed by economists, or whether it was based on the loosely grounded "negative perceptions" referred to by Welcher (1991, p92) is not known. Until such time as it is, there is a good possibility that policy prescriptions which flow from the only information to which economists have access could be incorrect. This question will be returned to in Chapter 1-3 where some of the attempts to offer policy prescriptions are reviewed. The pattern of relative wage and salary changes over the period 1970-89 is presented in Table 1-2.5. The indices in the table are derived by deflating estimates of the salary and wage bill by the CPI to yield consumption wages (commonly referred to as real wages). For the earlier period (1970-79) these have to be calculated from estimates of employment, of wage and salary bills and from estimates of the CPI for different income groups. For the later period, estimates of monthly salaries and wages in constant price terms are available.

The table has been broken up (somewhat arbitrarily) into blocks with years ending in 5 or 0 set equal to 100. This has been done so that relative rates of change are readily visible. Probably the most striking feature of the results is the fact that wages for African workers grew by nearly 90 per cent in real terms over the period, whereas those for whites increased by 11 per cent or so. Such, however, is the racial disparity in earnings that even at the end of the period, that whites, who constituted only 21,5 per cent of the total workforce, still received nearly half of the wage bill (47,6 per cent). African workers, by contrast, made up 53,3 per cent of the workforce, but received only 33,8 per cent of the wage bill.⁴⁸

48 Estimated from SNR P0242.2, 19 October 1989, p7.

Table 1-2.5 Consumption wage increases, white and African workers in manufacturing, 1970-89

	White	African	White	African	White	African	White	African
1970	100.0	100.0						
1971	104.8	101.2						
1972	105.4	104.1						
1973	104.7	112.3						
1974	108.0	123.2						
1975	108.6	127.0	100.0	100.0				
1976	108.1	136.8	99.0	107.4				
1977	106.4	139.8	97.9	110.3				
1978	107.7	142.5	99.1	112.6				
1979	109.5	146.7	100.9	116.3				
1980	115.2	153.4	106.1	121.2	100.0	100.0		
1981	119.2	158.1	109.7	124.9	103.4	103.1		
1982	120.0	165.0	110.4	130.4	104.1	107.6		
1983	120.5	169.0	110.9	133.5	104.5	110.2		
1984	121.5	172.6	111.9	136.4	105.5	112.5		
1985	115.4	169.3	106.3	133.8	100.2	110.4	100.0	100.0
1986	109.4	166.1	100.7	131.2	94.9	108.3	94.8	98.1
1987	108.5	169.3	99.9	133.8	94.2	110.4	94.0	100.0
1988	110.8	181.3	102.0	143.3	96.2	118.2	96.0	107.1
1989	111.1	188.9	102.2	149.3	96.4	123.2	96.2	111.6

Source: For the period 1970-79, salary and wage bill figures and employment estimates are from *South African Statistics 1982*, p7.10. The CPI is from *South African Statistics 1988* pp8.21-8.22. The 1980-88 figures are from SNR P0242.2, 19 October 1989; and the 1989 figure from SNR P0242.1, 16 November 1989 and 27 February 1990.

Looking at the four sub-periods into which the table has been divided, it may be seen that falling or static white wages offset the rapid growth of African wages in certain years, but that when increases in the white wage do occur, they are normally accompanied by even larger increases in the African wage (1981-82 and 1983-84, or in the earlier period, 1973-74). Sometimes, pent-up demand for skilled labour would seem to be responsible for rapid increases in white wages (1979-80 and 1980-81 being a case in point) which then exceed the rate of growth of African wages. By comparing these disaggregated figures with the results for the total workforce in Column 6 of Table 1-2.2 above one may see that although there is a tendency for the growth of the total wage to be moderated by slower growth of the white wage, this did not happen during and in the aftermath of each growth spurt. Rather, obviously, rapidly growing wages for both groups of workers is what gives rise to the 'bad' performances reported in Tables 1-2.3 and 1-2.4 above.

The well-known fact that slower white wage growth did, on occasion, offset the much more rapid growth in African wages, and in so doing served to keep wages and productivity more-or-less in line one with the other, is quite remarkable, and not all that well explained. As will be seen in Chapter 1-3, the reverse was predicted. That being so, it is worthwhile spending a short while looking at such economic evidence as does exist to explain the phenomenon - especially since there is no shortage of commentators ready to ascribe all or most of the

increase in black wages to 'arbitrary, political factors'. Despite all that is said about the 'political' nature of African worker wage demands, it is clear from Table 1-2.5 that in periods of slow growth or in the downward phases of some of the business cycles, the growth of African wages becomes negative. This happened in 1975-77, and more prominently from 1984-86. Lost ground was however, recovered fairly rapidly in 1988 and 1989.

Conventional economic theory would lead one to expect that during periods of slow growth and even more so, of negative growth, wage levels would either stabilise or fall. One might have thought also that the sheer numbers of unemployed flooding the market since the late 70s would have exerted a downward pressure on wages. Where this has not happened, the reflex response - the 'political' activities of workers - is not necessarily correct.⁴⁹ Two separate but related processes that started in earnest in the 1970s appear to have gathered momentum over the period. Together, they could possibly account for some of the residual growth in wages during the downswings in the cycle. The first of these was that employers either stopped hiring large numbers of workers of the lowest grade - so-called labourers, or they cut down on the numbers employed in this category. This shakeout of unskilled workers was accompanied by a process of grade mobility of African workers - upward movement through the ranks of the unskilled into semi-skilled and even some into skilled positions.

Evidence of this process is sketchy and difficult to interpret (for a start, the degree of 'skill' is difficult to measure) but a number of different sources all lead to broadly similar conclusions. As far as the skill composition of the workforce is concerned, Schneier (1983, p28) has reported that the proportion of unskilled workers (African) in the so-called modern sector of the economy fell substantially over the period 1969-79.⁵⁰ This could have resulted both from

49 Despite the fact that the marginal productivity propositions upon which conventional economics relies so heavily for a theory of wages are mere tautology at the high level of aggregation at which they are applied (Thurow, 1980), economists who operate within that framework are still prepared to assert that concepts such as the 'warranted wage' are valid. An attempt by Artus (1984) to discover whether real wages in the manufacturing sectors of various countries in Europe were above this level is a typical example of the approach. The technique adopted entailed the assumption of a constant elasticity of substitution production function, with an "innovative element" in the form of an allowance "...for changes in the rate of "disembodied" productivity growth" as well as allowances for changes in the relative weights of capital and labour. A variant included energy as a specific factor (p11). Making the truly heroic assumption that workers were paid their marginal product during the latter half of the 50s and the 60s, and plugging in the relevant variables on this base, the model yielded the appropriate results - ie, "...strong reasons to believe that in France, Germany and the United Kingdom,, the real wage has been too high in recent years, in the sense of being incompatible with high employment." (1984, p13) It will surely not be too long before somebody performs a similar exercise on the South African data.

50 This trend had been picked up in the late 1970s in the *Manpower Surveys* conducted by the then Department of Labour. Translated into labour force projections in the Economic Development Programme produced by the Prime Minister's economic advisor, a prediction emerged that of approximately 980 000 African workers who would take up modern-sector employment in the decade 1977-87, a mere 50 000 would have been labourers (unskilled workers) (EDP, Vol 2, p454). In the event, according to *South African Statistics 1990* (p7.6), a mere 265 000 of these 980 000 jobs materialised - probably not many of them for labourers.

the downturn in the economy and from union demands to decrease differentials in the wages of skilled, semi-skilled and unskilled workers. Using the scanty evidence available to test which of these was the more important, a recent study has found that for the modern sector as whole, the move to compress differentials has had but little effect. On the contrary, according to Hofmeyr (1991), real wage growth rates for unskilled and semi-skilled workers were negative over the decade 1975-85, ie, the consumption wage for a given occupation fell over the period. Such wage increases as were observed could, according to Hofmeyr, be attributed largely to two factors, namely grade mobility, and gender-specific changes in differentials. There appears to have been some widening of the differentials between unskilled, semi-skilled and skilled workers, with unskilled workers faring particularly badly (Hofmeyr, 1991, p29). Gender-specific changes are likely to have been of some significance in manufacturing, where women constituted about 25 percent of the total workforce, the bulk of them being production workers.⁵¹

Under the conditions of rising unemployment which, all commentators agree, characterised the South African economy from 1976 onwards, one could argue that there would not (or should not) have been much upward pressure on real wages within particular occupational grades. In general, Hofmeyr's findings suggest that real wages for women seem either to have grown faster than those for men - for many of whom, wages fell quite substantially. At very least, women's wages appear not to have fallen as fast as men's. Where increases for women are not accounted for by grade mobility, they may be explained by changes in hours worked and/or by the social pressure (accompanied by changes in the law) to pay equal wages for equal work. The data are not good enough to permit these sometimes contradictory movements to be disentangled, so much of what is said by any commentator must be speculative.

Some worker successes may have been scored in the struggles over job evaluation, with jobs previously rated as low-skilled on the basis of traditional *ad hoc* grading schemes being regraded, and with them the size of the wage packet - indeed, such a process might account for some substantial proportion of the upward mobility in manufacturing observed by both Hofmeyr and Knight and McGrath (1987). The latter study, using a different data set, found evidence of some increase in occupational real wage rates, but attributed about 60 percent of observed real wage increases to grade mobility.⁵²

51 The figure of 25 percent is for the year 1982 - see Report No. 30-01-01(1982), Tables 7.1 and 7.2. It is unlikely that this percentage has changed dramatically over time.

52 The Knight and McGrath results (see p56 of their paper) are reviewed in Hofmeyr (1991, p30). Differences between their findings and those of Hofmeyr are traced to their possible roots in sampling procedures.

Occupational change in manufacturing, especially in the metal-working industries, has been of great importance in the last 15 years or so. This is illustrated in Table 1-2.6 below. Statistics in South Africa are not noted for their excellence, but when independently collected series dealing with roughly similar phenomena all move in the same general direction, some confidence may be placed in them. The *Manpower Surveys* on which the figures in this table are based have been notorious for their errors, but reading the results reported in the table above in conjunction with the work of Hofmeyr and of Knight and McGrath, one can argue that the likelihood that they are picking up an important tendency is much greater. For the purposes of this study, all of the occupational changes observable above are significant, in the sense that they would all contribute to a higher average wage for a relatively smaller workforce. In the administrative and technical class occupational categories 01 and 02, the highest skill grouping, expand the most. In production, group 05 grows slightly, group 06 stays roughly the same, and group 07 grows extensively, chiefly at the expense of group 11 in the category service and other workers. The number of workers in this latter category (*Manpower Survey* occupation number 685 - labourer) in the four industries as a whole fell from 83 000 in 1977 to 36 000 in 1985.⁵³ This decline might be explained in part by reclassification, but much of it is likely to be due to less profligate use of labour-power by capital.

The *Manpower Survey* figures do not match the CSS employment figures on which they were supposed to be based, or at least none of those given in any edition of *South African Statistics*. The differences are irritatingly large for individual industries, but for the four industries as a whole, total employment and, more importantly, the change in employment were roughly the same. As may be seen from the last row of the table, total employment rose by about 10 000 or so. With the number of labourers declining by 47 000, this has to mean fairly significant upward movement by African workers, at least to the semi-skilled grades.

Such evidence on relative wage movements in specific industries within the manufacturing sector as is available is, however, somewhat out of line with Hofmeyr's findings. His results are based on data for major divisions (sectors) of the economy, rather than for major groups (industries). The contrary evidence available does not necessarily negate his conclusions - what it suggests is that in a climate of overall depression, significant victories were scored by workers in certain industries, victories which could have had important bearing on the attitudes of employers and investors. In the metal and engineering industries for example, wages of the

53 Basic metals have been omitted from this comparison because of a discrepancy between the CSS and Manpower Survey estimates of the size of the workforce. The *Manpower Survey* figure for 1977 is incorrect by more than 100 000, the error consisting chiefly of an over-estimate of the number of labourers (occupation number 685).

lowest paid group 'Lower skilled operators and labourers' grew much faster between 1975-85 than those of 'Higher skilled operators' and of 'Artisans'. Wage indices for the three groups of workers were respectively 46,2; 55,0 and 54,5 in 1975 (1980=100). By 1985, these had climbed to 218,7; 160,9 and 175,2 (*South African Labour Statistics 1987*, Table 4.1.17.2, p397).

Table 1-2.6 Comparative occupational structures - South African engineering industries, 1977 and 1985

	Metal Products		Machinery		Electrical Machinery		Transport Equipment	
	1977	1985	1977	1985	1977	1985	1977	1985
ADMINISTRATIVE and TECHNICAL								
01	2,7	2,0	3,0	3,5	2,3	3,4	3,0	3,2
02	2,1	3,8	4,1	8,3	6,2	6,0	3,8	6,0
03	6,5	6,8	10,0	10,6	9,8	10,2	12,4	11,8
04	0,8	0,9	2,6	2,3	1,8	1,6	1,6	1,6
Sub-total	12,1	13,6	19,7	24,8	20,1	24,8	20,8	22,7
PRODUCTION								
05	2,3	2,7	2,2	3,2	3,8	5,0	5,1	5,1
06	10,8	9,6	15,1	14,9	10,8	10,1	11,7	11,8
07	40,0	51,5	34,5	44,1	35,1	49,3	37,8	46,3
Sub-total	53,1	63,9	51,8	62,2	49,7	64,4	54,6	63,2
SERVICE and OTHER								
08	2,4	3,1	3,5	3,2	6,9	2,3	2,8	2,7
09	1,1	5,3	1,1	3,2	1,0	1,6	0,8	2,9
10	0,5	0,6	0,3	0,4	0,6	0,3	0,2	0,2
11	28,3	13,4	22,3	6,2	19,1	10,3	19,0	8,3
Sub-total	32,3	22,5	27,2	13,0	27,6	14,4	22,8	14,1
TOTAL EMPLOYMENT								
(thousands)	134	126	76	68	62	71	87	102

Sources: *Manpower Survey*, 1977 and 1985, Vols. 1 and 2.

Key:	ADMINISTRATIVE and TECHNICAL	PRODUCTION	SERVICE
	01 Managerial	05 Foremen and Supervisors	08 Stores
	02 Professional and Technical	06 Artisans and Apprentices	09 Drivers
	03 Clerical	07 Other Production Workers	10 Catering
	04 Sales		11 Labourer

Notes: Occupations within the different groups are based loosely on the International Standard Classification of Occupations. Certain occupations have been reallocated from the groups in which they were placed by the Department of Manpower - eg, 'SUPERVISORS' have been separated from 'OTHER SKILLED and SEMI-SKILLED EMPLOYEES (nec)' in Group 16, to reflect more accurately the numbers of supervisors. Columns for 1977 do not total to exactly 100 per cent because of difficulties in allocating some workers to occupational groups. The differences are small. Details are available on request.

This accords with what one might have expected - the relative scarcity of artisans ensured that their wage grew somewhat faster than the group of skilled operatives immediately below them in the job hierarchy. Worker organisation in the industry grew significantly in strength over the period - MAWU, the Metal and Allied Workers Union, being one of the more militant and

successful unions in the old federation, Fosatu. That tradition has been maintained by NUMSA (the National Union of Metalworkers of South Africa) MAWU's successor in the new federation, Cosatu. Fosatu was committed to an overall compression of wage differentials, and Cosatu has followed the same line. The results of that commitment are visible in the pattern of wage differentials for the higher- and lower-operatives and labourers, market forces or no market forces.⁵⁴ Presumably, higher skilled work merits higher wages - yet in all of the many NPI statements dealing with wage and productivity that I have seen, not one has referred to this simple economic justification for the wage increases of one group of workers exceeding the rate of growth of output of workers as a whole. Having considered the economic evidence available, let us turn now to a consideration of the structure of the NPI and to the ideology of wages which has grown up in the period that the NPI has been active.

54 A lack of readily available data on occupational differentials makes it difficult to assess the extent to which this story was repeated in other industries where organisation is strong, eg, in the chemical or textile industries. It is even more difficult to measure capital's response to the struggles waged in these industries.

Chapter 1-3

The NPI: Context, structure, function and ideology

On the importance of being unimportant

"Everybody is a flaming wizard when it comes to flogging the poor slob on the shop floor who's adding value to the product. But walk into any plant and you will find direct labour represents only about 10 percent of total manufacturing cost." (Robert L Callahan, President of Ingersoll Engineers, cited in NPI, 1985, p62).

Introduction

Statistical 'facts' are not neutral phenomena waiting to be collected from a storehouse of knowledge. After they have been manufactured (and it is clear that the question of which statistics are produced is itself the product of specific struggles) they frequently become the objects of struggles whose form, ie, interpretation or analysis, is importantly shaped by the ideological preconceptions of the participants. In these struggles, merely to refrain from critical engagement with the data is in itself a potentially political act. In its failure to engage critically with the statistical 'facts' that purport to represent economic reality in South Africa, the NPI has failed to treat both its data inputs and its own output with the care such fragile artifacts deserve. This omission is argued to have advanced the cause of the dominant classes in South Africa.

This chapter looks in some little detail at the NPI, at the wider economic setting in which it has operated, and at the way in which the debate over 'excessive' wage claims by unionised (primarily African) workers¹ has developed. Without examining the relevant aspects of the NPI as an institution in its historical setting, it is difficult to understand its dubious handling of dubious statistics. What follows makes no pretence to being a history of the NPI,² nor does it make any claim to being an adequate account of the struggles over wages and working conditions that took place during the period under consideration. Space constraints limit the offering here to the barest of sketches - all that is provided is the information required to make the beginnings of an informed assessment of the relevant NPI activities. It is hoped that what

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- 1 The arguments advanced in this chapter against the conventional wisdom on this matter must not be misinterpreted. It is not suggested that 'excessive' wage claims will not ultimately damage the social fabric - clearly they will. All that is argued is that several of the influential commentators on this matter have oversimplified what is a complex and contentious matter. In the process, due weight has not been given to the difficulties of settling competing claims to the total social product.
 - 2 There is a reasonably comprehensive history of the NPI in the report by the President's Council on a "Strategy and Action Plan to Improve Productivity in South Africa" (P.C. 1/1989, pp136-146).

follows will, despite its brevity, provide an *entrée* of sorts into the debate on institutional conduct in capitalist social formations.³

Of humble origins: an institute for fostering productivity growth

On 1 July 1968, the National Productivity Institute formally commenced operations under the administrative wing of the South African Bureau of Standards. Its first, and to date, only executive director is Dr. Jan Visser - nearly 30 years after its formation, the institution is still under his leadership. From a modest start, a study into productivity in the men's and boys' clothing industry, whose unfavourable findings reportedly did not endear the fledgling NPI to 'industrialists', not, that is, until they had been "...convinced of the correctness of the data..." (NPI, 1988, p6), the organisation grew to a point where in its heyday in the late 1980s, it employed more than 180 people (NPI, AR 1988/89, p3). In the early 1970s, as the South African economy began to show signs of sclerosis, and the first rumblings of black worker activism began to be felt, the need for official intervention increased.

Apart from the intermittent pressure which initially fragile 'non-white' unions themselves brought to bear on employers and the apartheid colossus, the 1960s and 70s witnessed an increasing interest among activists, social scientists and other commentators on the South African scene in the very low levels of the wages paid to black (particularly African) workers.⁴ Calls for wage increases from a predominantly white intelligentsia became more and more insistent. Usually, these came with the proviso that a *quid pro quo* in the form of improved worker productivity was necessary. Black workers, on the other hand, some of the time aided by rapid economic growth, gradually took matters into their own hands and fought with growing tenacity (and success) for wage increases. The question for them of whether or not these were related to productivity improvements appears not to have entered into the deliberations in any significant way. As the strength of Black working class organisation grew, so too did demands for improvements in conditions of employment. Issues of health and safety in the workplace, struggles over control of pension funds, and challenges to managerial prerogative at other levels began to be voiced. An important event was the formulation of a demand by Fosatu in the early 80s for a 'living wage'. This echoed the earlier campaign by the trade union federation SACTU (the South African Congress of Trade Unions) around the 'pound-a-day' (*iponto asinamali*) rallying cry, only this time on a much more secure

3 This matter is pursued a little further in Chapter 1-4.

4 See the articles by Graham (1961); Horwood (1962), and Hume (1970). There are references to further work of this kind in various issues of *A Survey of Race Relations in South Africa*. The 1973 edition refers to separate articles by Pursell; Spandau and Kessel (p209).

organisational footing (Luckhart and Wall, 1980; Meth, 1985; Jack, 1987). Wage and other demands by workers, chiefly those organised by the Cosatu affiliates, came to dominate the debate.

Proposals for a "productivity policy" in South Africa are traced by Bell (1975) to the report of the Reynders Commission (1972). This report, and an article by Botha (1972) both argued that productivity growth was low and declining (Bell, 1975, p479). The slender (and inappropriate) statistical basis on which the poor productivity ideology was founded was subjected by Bell to searching critique and its shallowness exposed. Arguing that a slower growth rate of labour productivity in South Africa than in the advanced capitalist economies was entirely predictable, and not "...a cause for pessimism about South Africa's long term international competitiveness", Bell challenged the complaint by Botha that rapid growth had been bought at the cost of "lavish" use of production factors, especially labour, with the retort that:

"A "lavish input" of labour in South African circumstances is surely to be applauded, not decried." (Bell, 1975, p485)⁵

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- 5 In the discussion of Bell's paper that followed, Visser of the NPI made a remark which shows how crude statistics can be abused to bolster an argument. On the question of the employment creation, Visser said that: "As ons kyk na die deelnemingskoers van ons bevolking in die ekonomie, vind ons dat ons nie ver afsteek by die res van die wêreld nie. As my inligting korrek is dan is ons ekonomies bedrywige bevolking omtrent 34 persent van die totale bevolking, vergeleke met 36 persent in die V.S.A. M.a.w. ons begin die stadium nou baie vinnig bereik waar ons dit nodig het om ons groei te stimuleer deur produktiwiteitsverhoging en nie deur addisionele indiensneming van produksiefaktore nie." (p510)
- Visser's information is not correct, and substantially so. In the mid-1970s in the advanced capitalist economies the proportion of the total population that was economically active ranged from a low of about 38 per cent in the Netherlands, to a maximum of about 51 per cent in Denmark. For the USA in 1977, the figure was 46,0 per cent (ILO, 1978, pp55-64). Developing country ratios were typically in the high 20s or low 30s. If one wishes to indulge in this sort of exercise (often a dubious undertaking) numerous corrections are required to render the figures comparable. If, for example, the proportion of the population aged 15 years or less had been excluded Visser may have been able to begin making meaningful statements. What was, and still is required, of course, is accurate information on what the economically active population was engaged in at the time this interchange took place, and what proportion of the non-economically active would have liked to work had they been able to. In particular, it would be useful to know more about unemployment than Hofmeyr's (1993) undoubtedly excellent work can tell us. The supply of prime-age males might well have begun to dry up in the early 1970s, but some significant proportion of the economically active population was outside of the formal sector, engaged in activities which generated a mere pittance.
- The rapid process of modern-sector employment creation to which Bell obviously refers was instrumental in improving welfare, a fact which seems to have escaped Visser's attention. Bell's enthusiasm needs, of course, to be tempered by a recognition of the realities of production in a market economy. If the wages paid to the 'lavish input' of labour are too far out of line with the dictates of the competitiveness constraint, certain painful readjustments will become unavoidable, a point that Visser and many others have made. The problem with the way in which this point usually is made is that seldom is serious consideration given to the distributional implications.
- Some of the productivity improvements for which Visser was pleading, appear, according to the results presented in this work, to have been forthcoming. Unfortunately, they were accompanied not by growing employment, but by massive unemployment. Using the strict definitions introduced by the Current Population Survey in 1978, a mere three years after the interchange reported above, the unemployment rate amongst African men was estimated at 6,7 per cent and amongst women 17,7 per cent, or an overall figure of 10,1 per cent of the economically active (*South African Statistics 1986*, p7.37).
- Causality in economic growth being notoriously difficult to establish, it is incumbent upon all who make policy recommendations, that they consider fully the implications of what it is they recommend.

Bell's argument is compelling - his critique of the theoretical failure of the Reynders Commission in elevating productivity growth to an "...entirely autonomous factor determining the rate of growth of output in general and exports in particular..." (p490) is not easy to fault. But the fate of his work - its academic excellence notwithstanding - is instructive. The productivity policy set in motion by the theoretical failure he identifies, ignores his warning that bodies such as the NPI, with their concentration on:

"..factors hampering productivity ...in the area of finance, administration, personnel management, production, selling and/or management generally... may be capable of playing only a small, perhaps a negligibly small, part in raising productivity" (p491).

This analysis may be excessively pessimistic - apart from anything else, the NPI appears to have achieved a level of competence which enables it to survive in the market for the non-public good part of its activities (consulting on range of issues related to productivity improvement).⁶ Irrespective of whether or not this is so, the NPI's success in creating a distorted perception of the productivity performance of the economy is anything but negligibly small. The development of the NPI's capacities commenced against the backdrop of a tapering off of the growth boom based on Import Substituting Industrialisation (ISI) and a rise in worker militancy. Direct interest in black wage levels at the macro level, where present, was muted rather than shrill, as it was later to become. Nonetheless, the broad outline of what was later to become the standard NPI call had already been articulated by respected academics. In his 1972 paper, Botha, for example, stated that:

"The task facing industry is to support and promote attempts at raising productivity by institutions such as the Productivity and Wage Association and the National Productivity Institute. Then African wages can increase against higher productivity rather than as a result of philanthropic pleas. However commendable, an overall increase can only be inflationary, and will deleteriously affect our international competitive position, unless the White section is prepared to make sacrifices, which appears to be an idle hope." (1972, pp207-208)⁷

In the early days, the NPI appears to have concentrated on attempts to improve the competitiveness of South African manufactures to facilitate entry into export markets, a concern it continues to display to this day. Detailed industry studies conducted in the 1970s pointed to inefficiencies so extreme in some cases that one could but marvel at the continued

6 It is not known for certain whether or not these activities are subsidised from the funds granted by the state for the pursuit of its public good production activities (the fostering of a climate conducive to productivity improvements). The NPI's annual financial reports are not sufficiently detailed to allow this question to be addressed.

7 As has been demonstrated in the previous chapter, white and African wages grew at very different rates, for reasons that are not entirely obvious. The end-result was a decline in racial differentials. The 'White section' may not have been prepared to make sacrifices, but the outcome of two decades of faltering growth was a change in the distribution of earnings that made it look as though they had been prepared to make at least some concessions. See Table 1-2.5 above.

existence of many of the firms surveyed.⁸ The NPI's increasing interest in rapidly rising wage levels (especially those of black workers) and its warnings of the 'dangers' this posed for the economy dates from the late 1970s, the warnings becoming more and more strident in the early 1980s.

Overlaying these events, but present as background rather than as a causal mechanism *per se*, was a much-publicised and heavily-researched international 'productivity slowdown'. The 'slowdown' dates from the late 1960s onwards, and affected all but a few of the major economies.⁹ South Africa (a dependent social formation?)¹⁰ shared the fate of the dominant economies, although the exact nature and the implications of South Africa's 'dependence' have been treated in somewhat cavalier fashion by economists.¹¹ The claims of black workers to a larger share of the wealth they help to produce has thus been made within the context of a collapse of economic growth both nationally and internationally.

8 Amongst the published studies were reports on the knitting industry (1972); spinning and weaving (1973); footwear (1975), and furniture (1976). A large unpublished study on the clothing industry was undertaken in about 1980.

9 The literature on the slowdown is enormous. Over the years innumerable articles have appeared in which mystified economists attempt to explain the sudden disappearance of growth. After an exhaustive scrutiny of the causes of the 'slowdown' (ie, the partial disappearance of the 'Abramowitz Residual') Denison, for example, finally concluded that:

"(of the) 17 alleged causes of the productivity slowdown...a few could be rejected, many were demonstrably unimportant, and a few eluded quantitative appraisal. None could be shown to account for much of the drop in the growth rate of the residual so I described its cause as a mystery. That mystery remains." (Denison, 1983, p68).

A sample of some of the work in the field may be found in Baumol (1986); Fischer (1987); Giersch and Wolter (1983); Lindbeck (1983); Maddison (1987) (a lengthy survey); Matthews (ed) (1982) (a collection of articles); or Romer (1987), who offered what he described as a 'crazy explanation of the slowdown'. The proceedings of a symposium on the slowdown published in 1988, included papers by Griliches, Jorgenson, Mancur Olson and Michael Boskin. The tone of some of the findings may be gauged from the title of Griliches' paper "Productivity Puzzles and R & D: Another Nonexplanation".

Working within a Marxist framework, Weisskopf; Bowles and Gordon (1983) attempted to broaden the analysis of the slowdown (in the USA) by concentrating on two "social" variables, namely "declining work intensity" and "lagging business innovation". In so doing, they make Denison's 'mystery' disappear - their social variables are sufficient to explain the slowdown. They claim that the "underspecification bias" of existing models "...strengthen tendencies to blame the unskilled and women for the underlying causes of stagnation", as well as reinforcing the "anti-Keynesian" trends in macroeconomic analysis and policy" (p438). Taking issue with the mainstream proposition that:

"...models explaining productivity growth would be incapable of accounting *endogenously* for the productivity slowdown and that recourse to external factors would be necessary to explain changes in the performance of the economy, particularly after 1973..."

they argue that:

"Our historical analysis leads us to an opposite conclusion. We argue that the structural forces that provided the basis for postwar prosperity also led to the stagnation of the late 1960s and 1970s; that is, the structure of postwar prosperity itself eventually generated declining productivity growth." (p422) (Emphasis added)

This dialectical outcome is what one would expect from a Marxist analysis - the very success of capitalist growth and accumulation (a contradictory process) in one phase generates the crisis of the next.

10 This unfelicitous term is used by Davies (1979) to describe South Africa's relationship with the 'dominant' social formations - the advanced capitalist economies.

11 The poverty of the debate on capital goods and 'dependence' in South Africa and elsewhere provides a good example of the knee-jerk response of economists to one of the major problems of development economics. See Meth, 1990c.

Productivity and the media

The events described immediately above form the backdrop against which the NPI has conducted the publicity campaign whose aim it is to popularise the view that productivity in South Africa is poor and needs urgently to be improved. The NPI's views can readily be discerned by analysing a work like *Productivity Focus*. That, however, is not what is of interest here - what is necessary is some measure of the impact of the NPI's campaign. This is no simple matter, primarily because there is little reliable evidence about what views people hold¹² about productivity and still less about why they hold the views they do. In the absence of being able to conduct the comprehensive survey that could settle this matter, resort to a surrogate in the form of a content analysis of newspaper articles on productivity stretching back to the early 1970s will have to suffice. This appears not to be such a poor substitute - there is considerable congruence between the views articulated in the press by business and the state, with those of the NPI. The central sentiments of the campaign orchestrated by the NPI appear thus to have been energetically accepted and promoted by a wide-ranging audience drawn from these quarters. Clearly, the NPI has always wished to influence unions, not to mention the ordinary person in the street. Of the former it may be said quite confidently that there is almost no evidence whatsoever of a positive response by the progressive unions.¹³

Content analysis is beset by a number of methodological problems. An examination of these may be found in Krippendorff (1980). Experienced practitioners of this arcane specialisation may raise an eyebrow at the somewhat cavalier method used to carry out the survey, but I have confidence that an attempted replication would not disagree too wildly with the results reported below.¹⁴ Many of the articles were short, containing at most, two or three pieces of information. About the longer articles there would probably be differences of interpretation. The results of the survey are summarised in Table 1-3.1 below. The technique adopted was to scan each article for 'characteristics' (discreet ideas or pieces of information being conveyed). The process of selecting characteristics or 'ideas' is a little arbitrary, but it is not wholly so, one begins to see certain patterns emerging. The characteristics emerge, in the first instance

12 The study by Welcher (1991) is one of the very few academic works of which I am aware that has attempted to gauge people's perceptions. The exercise was limited to relatively senior management. Welcher's survey was quite large - the response rate to the questionnaire was 19,5 per cent (587 respondents) (1991, p63). Another source of perceptions about productivity (but less generous) is P.C. 1/1989 - a report by the President's Council into productivity. Some of the evidence from this is cited below.

13 The *Daily News* of 30 August 1988 quoted Visser as claiming that:
 "Trade unions are coming round to the realisation that the creation and distribution of wealth depend on raising productivity... More rational negotiations between employers and labour representatives illustrate this."
 No other evidence is offered in support of this claim.

14 To make possible a replication of the results, the full list of sources (newspaper articles) examined and the characteristics detected in each article are given in Appendix 1.4.

from the sorts of claims made in NPI publications such as *Productivity Focus*. The short list which this generated was added to as the scan proceeded. One of the characteristics in the list below, Number 9 - Negative union rôle - emerges from a reading of P.C. 1/1989, as well as a number of comments by the NPI on this issue. In the event, there turned out to be no instances of this in the clippings examined. The only remark which approximated to such a category is the statement by Dr Visser to the effect that:

"In addition to its low quality, labour is further making a contribution to the digging of its own grave by sometimes demanding unrealistic wage increases." (*Daily News*, 5 September 1986)¹⁵

Clearly, this belongs in category Number 4 below. All told, 102 articles covering the period 1972-89, with the bulk of the articles (92) dating from 1979 onwards were examined. These articles were clipped by Natal Newspapers (whose co-operation is gratefully acknowledged). Every usable article was analysed. An article was not considered usable if it simply referred to the achievements of a single firm or a few firms, nor was it used if it merely advertised some event related to productivity. The broad criterion for inclusion was that it had to comment on productivity performance and/or offer some critical observation on the topic of productivity performance. Four sources or types of commentator could be distinguished. The 'A' group of articles have an NPI spokesperson, often Dr Visser, as primary source. Businessmen¹⁶ (usually high-level managers) or business consultants form the 'B' group, whilst the 'D' group contains senior political figures - mostly cabinet ministers. The 'C' group consisted of those who did not fit into any of these three groups. A small minority (8 articles), they were mainly academics or overseas commentators. The numbers of articles attributable to each source is given in the first row of the table. Business garnered most space, followed by the NPI, and then by the state.

By examining the figures in the column headed 'N' one may see which characteristics appeared most frequently. Characteristics 1, 2, 3, 5, 8, 18 and 20 are the most important. Far and away the most quoted of these was Characteristic 1 - Poor productivity performance. More than half of the articles contained some reference to this, often under quite lurid headlines - 'Atrocious', 'Disastrous', being typical examples of the sort of newspaper hyperbole used to

15 In the earlier period, some commentators were not so restrained. One article cited the NPI as claiming that "...South Africans are working at half-pace." (*Daily News*, 17 January 1978). Under the heading "Economist lays into workers", another article cited Lawrence McCrystal, who headed the anti-inflation campaign in the mid-1970s and who frequently commented on productivity in those days, as claiming that South Africans were lazy workers. (*Natal Mercury*, 22 January 1976). This kind of confrontational approach was soon abandoned to all but politicians, who seem to be able to adopt a hectoring tone towards their audience with relative impunity.

16 No gender bias is intended here - the comments analysed have, as far as can be determined, all been made by men. Where women have been involved, it has been in a supporting rôle, as for example when women's organisations of one sort or another exhort women (especially housewives) to join in the drive to improve productivity. See *Natal Mercury*, 8 August 1989.

inflate the importance of the reports. Of the 31 'NPI' articles, 14 (45 per cent) contained this feature. An even larger proportion of the 'Business' articles (26/41 - 63 per cent) and a still larger proportion of the 'State' articles (16/22 - 73 per cent) were of this type.

Next came a group of characteristics (2, 3 and 4) which link wage demands to productivity growth and inflation. Many of the Number 2 statements compared wage growth in nominal terms with productivity growth in real terms.¹⁷ Another marked feature of the B group articles was the (hardly surprising) tendency to cite NPI results in support of the arguments made. Without conducting a proper statistical analysis of these results it is not easy to say whether the slight differences in responses between the A, B and D commentators is significant. There is however, some suggestion that the NPI refers more readily to the problems in this regard than do either of the other commentators. If a larger sample of clippings were used, say from the INCH microfiches from the University of the Orange Free State, this would be the sort of hypothesis one might wish to test. For our purposes here, it is sufficient that the characteristics 2 and 3 appeared quite frequently. The gloomy conclusions promoted by this characteristic were reinforced by the occasional reference to Characteristic Number 8 - Declining international competitiveness.

Characteristic Number 5 - Management responsible for poor performance - behaves in an interesting manner. The business commentators in particular, were quite keen to lay the blame for poor productivity performance straight at management's door. Criticisms came in a variety of forms - 'captains of industry' "...who do not complain about import restrictions and tariff protection..." (*Daily News*, 5 June 1986) were one target. Poor relations between top management and supervisors was another. One of the more startling findings was the result of a survey conducted by the NPI which suggested that 65 per cent of managers did not know how to set about raising productivity (*Daily News*, 2 September 1986). The frequency of complaints about management imparts a sense of balance to the debate, as do occasional claims like this statement by Dennis Etheridge, sometime Chairman of Anglo-American's Gold and Uranium Division who said that:

"...South Africa had the world's worst productivity rate (*sic*) - and that it could not be blamed on blacks. Statements made by many a chairman and CEO of companies that blacks should not receive further real increases in wages unless they improve their productivity are nonsense." (*Sunday Times*, 25 July 1982)

Of course, the mining houses were involved at the time in a fairly energetic struggle with white workers who were attempting to obstruct change, but even so, the statement is quite strong, given that it was made at a time when growth in black wages was rapid.

17 Some of the statements were fairly strongly worded. A recent article had du Plooy of the NPI saying that "...South Africa's productivity is sick compared to average pay increases." (*Daily News*, 8 January 1989)

Table 1-3.1 Content analysis of articles on productivity in the press, 1972-89

	N	A	B	D
Number of newspaper articles	102	31	41	22
Characteristic				
1 Poor productivity performance	57	14	26	16
2 Wage growth exceeds productivity growth	22	11	8	3
3 Wage growth and inflation linked	20	9	7	4
4 Unreasonable wage demands	2			
5 Management responsible for poor performance	22	4	10	7
6 Increased mechanisation	2			
7 Poor capital productivity	3			
8 Declining international competitiveness	9	3	2	3
9 Negative union rôle	0			
10 Declining GDP/C linked to poor prod growth	4			
11 Desirability of incentives to improve prod	4			
12 Reference to a productivity improvement	3			
13 Specifically generous to labour	4			
14 Under-utilisation of capacity	1			
15 Productivity and education linked	2			
16 Critique of productivity measures	1			
17 Improvement in quality	1			
18 Training recommended	15	5	7	3
19 Skill shortage and/or excess sup of u/s wkrs	6			
20 Poor attitude and/or ignorance about prod	12	2	4	4
21 Managers positive, supervisors not	1			
22 Poor living conditions of workers	1			

Key: N is the total number of articles in the sample
A is the number of these using an NPI spokesman as sole or main source
B is the number of these in which a business spokesperson(s) is the main source
D is the number of articles in which the main spokesperson was a senior political figure

Characteristics 20 and 21 offer attempts at explaining why productivity is poor, the former 'Poor attitude and/or ignorance about productivity' being the more frequently cited. Finally, Characteristic Number 18 - Training recommended - was the most commonly cited cure. The odd dissenting voice warned against regarding training as a panacea (*Daily News*, 18 August 1985) but insofar as workers were seen to be part of the problem, training was seen as the answer.

Two common themes, one with relatively direct bearing on the matter, the other without, also featured quite prominently in the articles. The first of these was a number of references, especially in NPI type articles, to the fact that output growth in South Africa had been secured almost entirely through increases in inputs rather than through improvements in productivity, an obvious consequence of the failure to measure output levels correctly, especially in the earlier period (1970-80). A second feature was the repeated reference to South Africa's high birth rate. This was frequently offered in support of the argument that higher economic growth was an absolute necessity.

This, in short, is my interpretation of the way in which the productivity debate is presented in the South African media. Tentatively, I would argue that the stylised facts embodied in these claims form a part of the intellectual reflexes of those with 'knowledge' of this aspect of the South African economy.

In analysing the way in which the information presented influences viewpoints, rather obviously, both text (what is said) and context (by whom and to whom it is said) are important. The following example gives some insight into the operation of the mechanisms by which prejudices are made and/or reinforced. It does so by considering the structure of some stereotypical statements made by the NPI, and the likely responses of the audience to which they are addressed.

In the days before the NPI's annual publication *Productivity Focus* was introduced, the NPI annual reports were used to convey some of the information which subsequently found its way into that publication. In the annual report for 1981/82, for example, the following two statements may be found:

"It is clear that the manufacturing sector experienced a three-year period (1975 to 1978) when output per person employed or labour productivity continued to remain negative, i.e. less and less was produced per person employed. Salaries and wages per employee, however, did not adjust to this decline in productivity at all. At one stage during this period an 18 per cent increase in wages coincided with a two per cent drop in labour productivity." (NPI, AR 1981/82, p3)

Further on, in a discussion on the managerial corps available to the South African economy, the NPI reproduces the skill shortage numbers popular at the time and then states that:

"An aggravating fact is that many of the managers in the RSA are not productivity conscious, while many middle managers have not had formal management training. A change in attitude is observable, however, in many cases and it is hoped that this effect will snow-ball in the foreseeable future." (NPI, AR 1981/82, p5)

The intended audience of the annual report is the corps of managers (the primary readers of *Productivity Focus* as well). It is unlikely that as readers, they approach the topic free of any prior judgement. Their own experience, either at first or second hand, of the difficulties of negotiating with awkward trade-unionists, few of whom have any formal training in business economics, is unlikely to predispose them favourably to the movement. Their views of their own competence, by contrast, even, or especially if they 'have not had formal management training', are unlikely to be as unfavourable. Of value in helping to confirm the presence of 'hard' scientific information in the form of quantitative data, the unreasonable behaviour of workers juxtaposed against 'soft' (socio-economic) managers, especially when that ends on the optimistic note reported

Attention to this phenomenon (in a different context) has been drawn by Bresnahan (1993), in the discussion on a paper by Baily (1993) that compared productivity in four service industries in Europe, Japan and the USA. He observed that:

"Economics is often accused of being the unthinking instrument of the political right. Economists are very good at quantifying the benefits of competition - productivity, efficiencies and so forth - but tend, as this paper shows, to leave the costs in terms of quality or whatever in vague qualitative terms. Thus, economists offer hard facts and data about the things the political right cares about, and then "Gee, it is just too hard to quantify" all the things the political left thinks are decisive on the issues." (Bresnahan, 1993, pp149-150)

Bresnahan had no disagreement with Baily's commitment to the virtues of competition as a means to improving productivity (the topic under consideration was the replacement of small retail outlets by large supermarket or discounting operations), but his sensitivity to the negative impact of such developments distinguishes Bresnahan from others who celebrate rising productivity with no thought of its total effect.

Meaning and moral hazard

The general proposition that improvements in productivity are desirable is not in dispute.¹⁸ Apart from the fact that productivity improvement could be argued to be desirable in and of itself, a number of other imperatives for fostering such improvements may be adduced. Rapidly growing productivity has the potential, for example, to reduce the social tension over competing claims to shares of total output made by the different classes (and fractions of classes) in the economy. The desirability of the end (improving productivity) obviously does not, however, justify the use of almost any and every means to achieve it. Sullyng a noble end is all too easy - the single-mindedness of the NPI's propaganda campaign is one way to do so.

The 'awareness' campaign is an enterprise aimed at creating or altering perceptions.¹⁹ Specifying the form and content of the messages required to make a campaign of this sort succeed is no simple task. The advertising and public relations industries expend vast amounts

18 This statement needs, however, to be tempered by a recognition of the fact that specific productivity improvements can, and frequently do yield both winners and losers in the short- to medium-term. The actual and potential job loss associated with productivity improvements has not been properly treated by the NPI. Under certain conditions, this loss may be extensive. See Meth, 1994. Welfare losses may even extend into the long-term in certain cases, as happens, for example, when a productivity gain entails an irreversible ecological loss.

19 In the organisation's mission statement, the belief is expressed that greater productivity can be achieved if "...a proper attitude of mind is adopted..." Given this, it is not surprising to read in the document that: "[The NPI's] first and foremost obligations are, therefore, to foster such an attitude of mind - i.e. to increase the *awareness* and *appreciation* of productivity among all producers and all consumers - and to provide them with the guidance they need in order to improve productivity." (NPI, AR 1984/85, p2) (Emphasis in original)

on research designed to disclose what will 'work' and why. Without venturing anywhere near the huge literature which undoubtedly exists on this topic, one would have thought that a *sine qua non* for the success of an undertaking of the sort tackled by the NPI is an ability to show that the condition to be modified is real, that it has significance for its intended audience, and that it is capable of being changed. Although it is not known what professional assistance, if any, the NPI has had over the years, it is hard to imagine any expert advice being given which did not include these conditions.

To meet the first of them, ie, to impress upon the populace at large a need to improve productivity, one must be able to demonstrate that productivity is indeed unsatisfactory. This may be in either or both of the dimensions in which it may be measured - in a relative sense - productivity growth is poor, and in an absolute sense - the level of output per worker (and per unit of capital) is low. Clearly, one can only make sense of either condition by comparing current performance either with some previous period in a country's history, or by comparing it with the performance in other countries. The productivity crisis of the 1970s and 80s in the advanced capitalist economies was in the first of these dimensions only - output per worker in most of them was at an all-time high. In South Africa, by contrast, productivity growth and the absolute level of productivity were both much less satisfactory.

If, in the course of conducting the campaign, evidence is disclosed of satisfactory productivity performance in many industries, or if there are readily comprehensible explanations for poor productivity which make it seem unlikely that individual commitment to improving matters would make any difference, the campaign is almost certain to run into difficulties. Rather obviously, where a temptation to produce results of the 'appropriate' kind exists, a moral hazard of some severity may be seen to exist.²⁰

It would be noteworthy if the treatment of statistics by the institution or individuals concerned in this situation were not uncritical, in the sense that results which appear to bolster the case would tend to be accepted with little or no demur. Results which contradict the preconceived basis of the campaign are, of course, potentially an embarrassment. Evidence will be offered below that suggests an inability on the part of the NPI to maintain the objectivity demanded of social scientists.²¹ A reliance on shaky data to produce the 'right' conclusions, as well as an

20 Problems of agency and of moral hazard are yet another field where economists only ventured some time other practitioners. A possible explanation for this could be the direct involvement of say, for example, the business accounting profession in day-to-day problems of this type.

21 It might be objected that the NPI, formally a non-profit company, one of whose major goals it is to raise awareness levels, should not have to meet such stringent criteria. This would be valid if the NPI were viewed purely and simply as a lobbyist. The fact that the NPI is the primary source of productivity statistics on a national basis disqualifies it from seeking to escape the standards one would apply in judging the workmanship of any reputable research institute.

unwillingness to concede that 'official' sources often are deeply flawed are the telltale signs of this failing. Without wishing to suggest malfeasance on the part of any of the officials of the NPI, it is submitted that of the limited range of possible behaviours of people placed in such a situation, suppression or denial of 'unfavourable' results is an ever-present temptation.

As stated at the outset, it is not the intention to argue that productivity in South Africa, conceived of as output per worker and viewed in aggregate, is satisfactory.²² In parts of what is termed the 'modern sector', it probably is, whereas in nearly all of the so-called 'informal sector' and some significant parts of the 'modern sector', it is not. Rather, the point is that the missionary zeal with which the NPI has approached its task has occasioned some loss of the sense of balance and proportion so necessary to scientific work.²³ A hint of this absence of balance is visible in the following remark reportedly made by the chief executive of the NPI, Jan Visser. He is trying here to explain the poor productivity of black workers:

"We must never forget that 150 years ago when the whites moved into this region, that was the first time that the indigenous population had seen a wheel. It's a hell of a jump from that to the electronic age." (*Manpower Survey*, supplement to *Financial Mail*, September 16, 1983, p21)²⁴

Although the implicit racism of statements like this (of which more examples will be offered in Chapter 1-5) is deplorable, what is of greater interest here is that crude stereotypes like this are evidence of a mind set firmly on a particular view - in this case, that the productivity of an easily identifiable group of workers is poor. It may seem unfair to single out comments of this sort for criticism, but they are one of the major transmission mechanisms for ideologies. Statements like this link up with the shared prejudices (including racism) of some large

22 One meaning of the notion 'satisfactory' (there are others) is hinted at above. An industry's productivity performance may be said to be satisfactory if its wage/productivity relation enables it to compete effectively, ie, without substantial protection, in the international market. Judged by this criterion, the productivity performance of, say, the Paper and Pulp industry in South Africa is said to be satisfactory, whereas that of the Textile industry is not.

23 Although what follows is harshly critical of the NPI, this criticism is not meant to detract from the achievements of that body in those fields where it has demonstrated its competence. Improvements in productivity, mostly solid and sustained, and sometimes spectacular, have resulted from the activities of the NPI at firm-level, and in the public sector, at departmental level. These are reported in the NPI publications and the press. Some of them are referred to below.

24 This paternalism echoes a statement made by Mentz, Chairman of the Central Native Labour Board in Cape Town in 1956. That worthy told employers that:

"Africans were 'too childish' to master trade unionism, using as 'evidence' the 150 years it had taken Europeans to do the same." (Luckhart and Wall, 1980, p112)
Stereotypes about the qualities of 'native' workers ring down the ages - in 1956 the president of the FCI observed that:

"The increased mechanization of manufacture has created for the Bantu a permanent niche in production as a repetitive worker... In fact it has been mentioned on good authority that a well-trained Native is probably the best repetitive worker in the world." (Cited in Lewis, 1984, p123)

proportion of South Africa's managers - the creation and reproduction of shared meanings is what ideology is all about.²⁵

Meanings are constructed at several levels - the NPI has been active on several of them. On the one hand, one sees attempts to gain recognition in the scientific community, as evidenced by, for example, the touting of the NPI's elaborate cost-accounting model called REALST (REsource ALlocation STRategist) as a contribution to the growth accounting debate in economics.²⁶ On the other, one finds evidence of attempts to move the debate from the strictly academic to a more 'homely' atmosphere in which 'common sense' rather than the rigour normally associated with science (somewhat of a hindrance) can prevail. An instance of this may be found in the editorial comment of a special edition of the NPI's in-house glossy magazine *Productivity SA* published in April 1985. Noting that although the survey the magazine contained had been "...richly researched, it in no way aspired to academic certainty", the editors observed that:

"The over-emphasis on scientific accuracy in so many of our publications is both a symptom of the institutionalisation of the spirit of free enquiry and a barrier to open and creative discourse."

The editor's intention, by contrast, was to "...provide a platform for considered dialogue..." (NPI, 1985, p2). There comes a point when the pursuit of precision degenerates into pedantry - this study, with its enforced examination of detail, must get close to it in many places. Unfortunately, one has to be vigilant when it comes to relaxing the standards of evidence. This is because discourses between apparent equals often disguise the power relations behind them. Thus if 'open and creative discourse' is based on impressions rather than critically evaluated

25 Writing in the early 1980s on the ways in which the relationship between capitalist growth and apartheid have been mystified, David Yudelman commented that:

"Even those liberal Anglophone academics who genuinely abhor race discrimination share the responsibility for unconsciously obfuscating its origins, development, and contemporary function. Politically isolated and with very little chance of widespread support from any significant sector of the white electorate, the liberals have attempted to avoid ivory tower isolation by passing lightly over the rôle of their fellow Anglophones. The great majority of these, particularly those who have been large-scale employers of Africans, supported and still support race discrimination." (1984, p14)

The problems involved in understanding shared values are not trivial. Therborn (1978, p135) observed that: "Little is to be gained by the observation that, apart from interlocking membership in cohesive power groups, another kind of interpersonal identity exists: the sharing of ideas, a consensus of values."

At that point in the text he cites a writer on democratic theory who claimed that:

"...democratic politics is merely the chaff, it is the surface manifestation, representing superficial conflicts. Prior to politics, beneath it, enveloping it, restricting it, conditioning it, is the underlying consensus among a predominant portion of the politically active members."

To which claim he poses the following question:

"But what if 'consensus' is the surface manifestation of something else, which 'envelops', 'restricts' and 'conditions' both the consensus and electoral politics?" (Therborn, 1978, p135n)

Rejoining the argument, he states that:

"For in contemporary societies, such a consensus is extremely general and abstract, and the precise modes of its emergence, functioning and maintenance still have to be explained."

26 See, for example, the article by van Loggerenberg (1986) in the *South African Journal of Science*, a somewhat unusual venue for a piece on accounting procedures.

evidence, it is the impressions of the powerful that are likely to prevail. Impressionistic analyses have their place in the scheme of things - it is sometimes useful to call upon stylised facts - but impressions, in the hand of the powerful, are all too easily elevated to the level of dogma. When this happens, the result is not science, but superstition. Paradoxically therefore, the plea can foreclose on debates through two interlocking mechanisms - in the first place, the relaxed discourses called for would appear to rule out certain lines of inquiry, such as the careful digging required to show that some of the NPI's major conclusions rest on a faulty basis. In the second place, the power relation enables the stronger participant to dictate the terms of the engagement. My attempts to persuade the NPI to look critically at the statistical sources they used were given short shrift. 'Free enquiry' and 'creative discourse' could have taken place once the NPI's basic premises, namely that (a) worker productivity is poor, and (b) workers are granted wage increases not justified by productivity improvements been accepted. Of course, this is now changing, but it was changing power relations (Larrain's 'solution of contradictions') more than anything else which has brought this about. The debate on productivity in South Africa, such as it is, has taken place on a terrain marked out by the NPI. Within its boundaries, the state and the capitalist class have hitherto enjoyed the privilege of 'open and creative discourse' on productivity.²⁷ Scientific scepticism is necessary (but not sufficient) to dismantle the bounded reality so created. The workmanship of the NPI has been shown to be shoddy, yet that output has been eagerly embraced by both capital and the state. It is difficult not to draw the conclusion that this was because of its ideological implications.

As a creation of the apartheid state which enjoyed increasingly enthusiastic support from the capitalist class, the NPI has always been a contradictory organisation. Thus the context in which it has operated provided scope for certain kinds of activities at the same time as it limited the effectiveness of others. The institutional structures of the NPI reflected these constraints and possibilities. Let us look briefly at them.

The NPI - structure and constraint

The NPI has a multiple rôle, the execution of which is rendered difficult by the conflicting demands which it must meet. Its own conception of its "...first and foremost obligations" being to foster a particular set of attitudes which will improve attitudes to productivity

²⁷ This does not mean that the NPI has not enjoyed the freedom to make harsh criticisms of certain business practices. Using findings from the REALST package, for example, the NPI has frequently condemned the price 'over-recovery' (inflationary price increases) made possible by a lack of competition. Managers have also been criticised on numerous occasions for their lackadaisical or indifferent attitude to productivity improvement. In Chapter 1-4, the relative autonomy of state institutions which permits such freedoms is discussed.

improvement, provides a pointer to the moral hazard to which it must fall prey. As already noted, good news cannot be expected to hold the interest of the target audience for long, so there is considerable incentive for the NPI to produce bad news, especially when it necessary to "...convince [potential customers] to embark on productivity work with [the NPI's] help" for which help, "...professional fees are charged" (NPI, AR 1984/85, p4). This activity is likely to impinge on one of the other important functions of the NPI, namely the preparation and publication of productivity statistics from basic data generated by others, primarily the CSS.

The extent to which the CSS can be trusted to be wholly impartial in its data-gathering activities is a matter of dispute, as is the question of its ability to adhere at all times to acceptable standards of scientific practice. Moll (1984, p129) argues that although for a variety of reasons, the CSS seems to be "...influenced more by 'modernisation', a desire to conform to international (Western) standards, general economic practices and sheer inertia than the desire to produce any specific political impact", there is little doubt that "...coverage and bias strongly favour the status quo". Pressures to produce politically acceptable results in such technical areas as the census of manufacturing, are, however, minimal, and the same may apply to estimates of employment. Given that the CSS performs almost no analysis of much of the information it publishes, the resistance to acknowledging error probably stems from the embarrassment of being shown to be incompetent. It is unlikely that bureaucrats elsewhere have much enthusiasm for similar public demonstrations.

An organisation such as the NPI, whose controlling organs are dominated by its largest customer, has little chance of attaining the levels of objectivity achieved of the CSS, limited though these may be. Whatever its weaknesses and shortcomings, and they are many, the CSS has a formal commitment to neutrality which the NPI can never have, given its existing mission. The CSS has the virtue of not having any direct links to the capitalist class, or for that matter, to organised labour. Such linkages, when they exist, are highly likely to distort research output.

In South Africa, the wheel has come the full circle - the official producers of statistics, the CSS, now republish some of the output of the NPI in their annual *South African Labour Statistics*. This is an undesirable state of affairs. It is imperative that the function of measuring productivity be separated from that of encouraging productivity improvement. As long as it not, the temptation to cloud the issue by painting an overly-pessimistic picture must be considered to be an ever-present danger. Requiring the NPI to execute both functions is like getting the police to both try and punish the suspects they apprehend. Impartiality is impossible

under such conditions. The CSS is far from ideal, indeed, as this study quite clearly demonstrates, they have much to do to get their house in order, but they are preferable to the NPI as it is presently constituted.

Funding, consulting and information production

The NPI is not a simple organ of the state like the CSS, staffed by civil servants - its current status is that of state-subsidised, private non-profit company (company or association registered not for gain - a Section 21 company). This was attained in 1976. Having started life as an institute within the South African Bureau of Standards, the NPI was moved to the Department of Manpower in 1980. It now falls under the Minister without Portfolio - in practice, the Minister responsible for the Reconstruction and Development Programme.

Early funding was quite generous - under the 'Industries' vote in the Budgets for the years ending March 31 1974, 1975 and 1976, amounts of up to R450 000 each year were set aside, to be used by the SA Bureau of Standards for the 'Promotion of Productivity'. Of these amounts, R200 000 in each year was earmarked for 'Investigation in regard to promotion of productivity in export industries'.²⁸ In the early 1970s, the average wage of an academic researcher would not have exceeded about R10 000 per annum, so the funds available could, at that time, purchase quite a lot of research. In the budget for the year ending March 1977, the National Productivity Institute appears as an independent entry for the first time, still under the vote for 'Industries'. The amount budgetted was R482 000 and the note earmarking R200 000 for promotion of productivity in export industries still appeared (R.P. 2&5 - 1976, pp30-8). A similar vote appears in the budget for the following year, by which time the NPI was allotted R689 000 (R.P.2&5-1977, pp30-8).

Latterly, the NPI has received a grant which is part of the vote of the Department of Manpower, under whose control it was listed as a 'Statutory Institution' (RP 77/1981 pxi), having been transferred there from its previous home in the Department of Industries, Commerce and Tourism in 1980 (RP 77/1981 pv). The NPI's financial fortunes are charted in Table 1-3.2.²⁹ In the first column, the value of the states' grant (R millions in constant 1990 prices) is given. The second column shows how much income, also in constant 1990 prices,

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- 28 The source of this information is: R.P. 2&6 - 1973, p139; R.P. 2&6 - 1974, p303 and R.P. 2&6 - 1975, p298.
- 29 Table 4.2 in P.C. 1/1989 presents some, but not all of this information given in Table 1-3.2. The table in P.C. 1/1989 does not give any indication of the size of the income generated by the NPI's own consulting activities.

the NPI has succeeded in generating through its own activities (chiefly consulting and training).

Several features of these results strike the eye - probably the single most outstanding being the virtual trebling of the state's contribution in the fiscal year 1981/82. This made possible an increase in employment from about 40 in 1981 to 80 in 1983.³⁰ Almost as prominent (and of almost equal absolute size) was the decline in the value of the state grant from its peak in 1986/87 to a low point in 1991/92. As far as the NPI's efforts at revenue generation are concerned, their performance over the period 1981/82 to 1989/90 (roughly the same period as the peak-to-peak in the business cycle) was quite satisfactory. A business which reported an eightfold increase in turnover (in real terms), especially if profits grew correspondingly fast would be an interesting proposition. In keeping with this rapid growth was an apparently rapidly declining dependency on the state. This must imply that the bulk of the NPI workers are or were engaged either in consultancy-related (income generating) or in administrative work. Like all businesses, however, the NPI has been deeply affected by the recession - the decline in real revenues of almost 25 per cent must be cause for concern (and also for retrenchment).³¹

Table 1-3.2 The NPI's economic performance, 1980/81 to 1992/93 (Rm in constant 1990 prices)

	Value of State Grant	NPI Own Income	State grant as % of Total revenue
1980/81	3,64	2,45	59,8
1981/82	9,90	1,90	83,9
1982/83	8,74	3,57	71,0
1983/84	9,98	4,31	69,8
1984/85	11,07	6,40	63,4
1985/86	10,62	5,77	64,8
1986/87	11,12	6,02	64,9
1987/88	10,71	9,27	53,6
1988/89	9,34	10,92	46,1
1989/90	8,50	16,24	34,4
1990/91	7,72	13,31	36,7
1991/92	6,40	13,66	31,9
1992/93	8,71	11,73	42,6

Sources: NPI total income is from the NPI Annual Reports for the relevant years.

State grants are from R.P. 1 or R.P. 2&4 for the years in question.

Note: The values of the state grants and of NPI own income are R millions expressed in constant 1990 prices. These estimates have been obtained using the CPI as deflator. The CPI from *South African Statistics 1993*, p8.20.

The figures given in the column headed state grant are the budget figures. They do not include, for example, extra payments such as that of R2,2 million made in 1989/90 to cover the drop in the real value of the state's contribution (NPI, AR 1989/90, no page numbers).

30 *Productivity SA*, June/July 1988, p7. The temptation to invoke a public-choice theoretic explanation of the NPI's growth as bureaucratic rent-seeking will be resisted.

31 Employment fell from an all-time high approaching 200 at the end of 1988 (*Productivity SA*, June/July 1988,

It is not known exactly how the state grant is spent, and this cannot be ascertained by scrutinising the NPI's annual reports, at least not the reports that are made available to the public. With the largest ever grant in hand, the NPI commented that the 1984/85 moneys went, partly, if not wholly, into "Research into matters of national importance" (NPI, AR 1984/85, p4). If one compares, say, the 1992/93 annual report (NPI, AR 1992/93) with that for 1988/89 (NPI, AR 1988/89) one notices a certain deterioration in that information on the sources of revenue and the size of the workforce given in the earlier report does not appear in the later report. It would seem that the minimalist stipulations of the Companies Act make this possible. This minor annoyance (the size of the grant is readily ascertained by examining the budget for the Department of Manpower) is made possible by the ambiguous status of the NPI (a non-profit company). It constitutes yet another of the mechanisms by which the public, whose taxes are used to support the activities of the organisation, are prevented from obtaining easy access to information. This is not necessarily sinister, but it is irritating. A more detailed report of the activities of the organisation is made available to the members of Productivity Advisory Council,³² but this is not a public document.³³ Presumably, the Council would grant access to anyone interested, but that is beside the point - taxpayers should not have to go to such lengths to obtain information on how their money is spent. If the allegation made in this study that state funds have been used to manufacture propaganda can be sustained, then the lack of transparency in accounting for those funds becomes a matter for some concern.

In its early days, the NPI used to publish industry studies, but this has long since ceased. These provided a wealth of information not available anywhere else. The accent now appears to be on commissioned work, the results of which are made available only to the commissioners of the research. Currently, the NPI publications consist of the bi-monthly glossy magazine called *Productivity SA* and two major annuals - *Productivity Statistics* and *Productivity Focus*. These latter two publications (and the frequent speeches made by Visser) are the main sources of information on the productivity performance of the economy. *Productivity Statistics* first appeared in 1982 and *Productivity Focus* in 1984 or 1985. It is unlikely that the state grant is even dented by the production costs of the NPI's two major

32 Cosatu could, of course, have gained access to the document by taking a place on the Council, something which it has steadfastly declined to do. On this, more below.

33 These reports are referred to in P.C. 1/1989, para.4.96, p142. That document notes that the Department of Manpower had no control over the way in which the money was spent. Departmental representation on the NPI's board was not obligatory. Responsibility for ensuring that the money was spent judiciously rested with the NPI's board and the Productivity Advisory Council. A suggestion was made to the committee drawing up P.C. 1/1989 (on behalf of the Department of Manpower) that:

"...where the State makes a grant to an outside body, such body should be obliged to submit a detailed report on how the money was used via the department through which the money is allocated. This report should be tabled in Parliament, where it can be debated." (P.C. 1/1989, para.4.98, p143)

This suggestion appears not to have been taken up. The more comprehensive report was submitted to the Minister of Manpower, the Director-General and his staff, but it did not go to Parliament.

annual publications on national productivity. Since the early 1980s the NPI has employed more than 100 staff, but the work required to produce these annuals is done essentially by one person. Up until 1990, this was done by Dr R du Plooy.³⁴ He was assisted by Ms K Liebenberg, who took over the job when du Plooy resigned in 1990. Ms Liebenberg is responsible for compilation of the tables, the graphs and commentary as well as the production of much of the finished artwork for the two publications. Total time taken to produce the finished works each year is about six months. The two publications, which are big money-spinners for the NPI, because of the subscriptions which private business pays, were based, in the first instance, on similar journals published by the American Productivity Association. The NPI publications were initially as crude as their US exemplars, but in recent times, Ms Liebenberg has proved to be receptive to criticism (chiefly from Dr T Moll and myself) and has begun to give detailed references to sources. This makes replication of the NPI results as well as checking of the original sources possible. Much of the data, both local and overseas, is supplied on diskette by Economic Analysis Systems (EASy), a Pretoria-based private consultancy that buys data from the CSS and the SARB, performs certain cleaning operations on it, and then retails the product, chiefly to corporate clients.³⁵

By 1985, the NPI claimed, not unsurprisingly, that the "business community" was its largest customer (NPI, AR 1984/85, p2). As has been pointed out, the NPI functions as a major secondary producer of statistics and as a primary producer of analyses. It does so, however, without the financial independence so vital to ensuring balance in these activities. Of course, the NPI can point to the numerous occasions when it has criticised 'management' for failing to take the proper steps to improve productivity.³⁶ There is a substantial difference between such criticisms, laudable and overdue though they undoubtedly are, and the development of a truly critical stance both towards business and the state.

At its founding the NPI was very much a creature of the state (a smallish research institute within a government department), but has long since outgrown that constraint. The NPI is now the dependent of the clients for whom it performs consulting services and of the state to which

34 See Chapter 1-4 for a discussion of the way in which the activities of specific individuals are conceptualised and examined in this study. At this point it will suffice to point out that the often harsh criticisms of statements made by the individuals concerned is not intended to impute any motive to them - the criticism is solely of their activities as bearers of social relations.

35 *pers. comm.* Ms K Liebenberg, August 1994.

36 A typical example is a statement of the following type:

"Our long experience ... suggests that ... the single most important factor contributing to labour under-utilisation is the quality of management; while the single most important factor contributing to labour inefficiency is the quality of labour. To be more specific, our experience suggests that lack of managerial skills particularly at supervisory and middle management levels results in very poor supervision and in greatly deficient production planning and control. These are responsible in turn for a great deal of idle time. With respect to inefficiency and its underlying poor quality of labour, our biggest problem lies in the very low level of education received by a large proportion of the labour force." (NPI, 1985, p7)

it is beholden to top up its purse when times are hard - as they have been since the long recession that began in 1989. The unending imperative to produce information of the right sort (the problem of moral hazard) coupled with a need not to offend 'business', its major client, are bound to produce the kind of institution we have before us today. Consultancy and public relations (unpaid advertising) are the lifeblood of the organisation. There is no reason to expect academic rigour or neutrality from such a body.

Controlling structures

The history of the NPI given in P.C. 1/1989 includes an examination of its organisational structure as well as some discussion on its relations with the three 'social partners'³⁷ - employers, the state, and worker representatives, all of whom are represented on the Productivity Advisory Council (PAC).³⁸ Up until recently, the members of the Council were appointed by the Minister of Manpower. This council lays down the broad policy of the 'company'. The NPI executive is accountable to a board of directors drawn from the ranks of this council. On both bodies the capitalist representatives, whilst not numerically preponderant, are dominant. The size of the Council varies somewhat, but on average, consists of 21 members including the Chairman.³⁹ There are 15 alternate members. Eight of the Council

37 Relations between the three partners, business, labour and the state, are complex. Analyses of these relations, unless cast in a comparative framework, leaving the reader to choose the interpretation that suits them best, almost always expose the theoretical commitment of the analyst. One view, popular among certain factions on the left, is that unions that co-operate (collaborate?) with the state and the capitalist class are in danger of being co-opted. The danger varies with the type of union concerned. Bureaucratic unions, unions in which a large gap has opened between the membership and the elected officials, are prone to being absorbed into tripartite structures in which they are the most junior partners. It could be argued that the unions that have accepted places on the NPI's Advisory Council are of this type. Yet ultimately, unions do, as they must, relate to both business and the state. This raises questions about the necessity for various forms of social compacts. The literature on this is huge - for an introduction to some of the issues in the South African context, see Natrass (1992), especially Chapter 2. Some of the dangers of corporatism are examined in Habib and Desai (1994).

38 The Council has an interesting history. Apropos of its launching in 1969, a *Financial Mail* article headed "Productivity or fireworks" commented that if the Council held together it would be a triumph of "...tact and trust." Noting that Tucsa, then the country's largest labour body was known to be disgruntled and suspicious because "...labour men are outnumbered 8 - 3 by representatives of employer organisations..." the article went on to cite one labour leader as saying that:

"If this is going to set the pattern of the Council, it's dead even before it begins." (*Financial Mail*, February 9 1968)

This hostility soon gave way - in 1970, Robert Kraft, a senior Tucsa official made an impassioned plea for greater financial support from the State for the NPI (*Daily News*, 16 November 1970). Looking back on the relations between unions and the NPI, P.C. 1/1989 reports that:

"The Committee was informed that the NPI initially enjoyed the full support of the union movement because union leaders were convinced that increased productivity constituted a means by which a better dispensation for their members could be negotiated. During the formative years the NPI found that union leaders were, generally speaking, more enthusiastic about productivity projects than employers. Later, with the advent of greater militancy in some unions and union federations, productivity began to be viewed with suspicion. A well-defined information programme to promote productivity awareness among unions is now clearly called for." (para.4.107, p145)

39 Names and constituencies of the Council members are given in the NPI annual reports. The figures quoted here are from NPI, AR 1990/91, p4.

members are the direct representatives of capitalist interests (employer associations). No significant sector of organised business is missing. The Council has or recently had on it four (or five) representatives from employee associations (trade unions). With the exception of the Boilermakers Union, the unions represented on it are predominantly conservative.⁴⁰

The Council is similar, in certain limited respects, to the many tripartite institutions found in South Africa and elsewhere to regulate conflict and to transform adversarial relations, if possible, into courses of action which advance the 'general' or 'national' interest. A major difference between the Council and, say, the industrial councils however, is that the former is not a negotiating body, answerable directly to its constituencies. Unlike an industrial council, a body which requires balanced representation, the NPI apparently sees no need for such balances. One result is the chronic under-representation, one might even say misrepresentation, of worker interests. These two features of NPI structures - the purely advisory function of the Council and the under-representation of union interests, account for the persistent refusal of the democratic trade unions to accept the invitation to take a seat on the Council. According to du Plooy (1990), the NPI sought to expand trade union representation on the Productivity Advisory Council (PAC) from five to eight members:

"...but some trade union federations blatantly ignored invitations from the NPI to serve on the PAC."⁴¹

Attempts by the NPI to persuade the 'progressive' unions, chiefly representatives of Cosatu affiliates, to take their place on the Council predate this, however. Had efforts to persuade Cosatu officials to take this step had been successful, it would have been a coup for the NPI - because of the increase in legitimacy entailed. Not surprisingly, Cosatu has refused consistently to be incorporated in this manner. Undeterred, the NPI soldiered on, and in the continuing struggle had progressed by 1992 to a point where officials of Cosatu agreed to permit the first ever high-level contacts between officials of the two bodies. After preliminary discussions "...on the possibility of cooperation between [the] two organizations..." the then

40 Only in a crude consensus model of the world (and in certain unique circumstances) is conflict absent from the relations between workers and the capitalist class. There are, however, certain groups of workers who, after initial struggles against their employers, manage to occupy, sometimes for lengthy periods, a privileged space, usually because of their monopolisation of certain skills. Such groups tend, the world over, to be represented by conservative trade unions. South Africa is no exception. It comes as no surprise to discover that in the past, union representatives on the Council came from worker organisations of this type. In 1985, for example, there were three union representatives (and alternates), one each from the now defunct Tucsa, from the South African Confederation of Labour and from the Federation of Salaried Staff Associations of South Africa. All of these federations represented conservative groupings (Tucsa somewhat less than the other two). Their histories do not suggest that they could mount any sort of challenge against the combined might of capital and the state, although Tucsa did at times protest rather lamely against some of the sillier views advanced by the NPI (see, for example, *Labour Mirror*, March 1985, p2).

41 P.C. 1/1989 comments that it is obvious that "...unions are not represented adequately", and notes that five additional unions had been invited to nominate representatives to the Productivity Advisory Council. In the committee's view, the long-term interests of both unions and their members would have been served if these invitations had been accepted. (para. 4.108, p145).

Director of Business Development of the NPI wrote a letter to Cosatu outlining a draft Position Statement on a number of issues that had been raised. Amongst the comments offered under the heading "THE NOTION OF PRODUCTIVITY" the letter contained the following statement:

"An area where the NPI has been particularly active is that of productivity statistics. Statistics have been generated for two reasons, viz, to raise awareness within a wide range of audiences about productivity and its rôle in economic upliftment and to identify areas of economic activity that would benefit from some sort of productivity enhancing intervention...

Most of the statistics published have been derived using public domain data and therefore entrench the same intrinsic deficiencies. Nevertheless, despite this impediment, it has become clear that South Africa's productivity and living standards are not improving at a rate that will guarantee international competitiveness and social wellbeing. Major shortcomings in the interpretation of productivity statistics in the past have been the tendencies to assign to labour the responsibility for low labour productivity and to overplay the negative aspects of prevailing trends." (NPI, June 1992)

Such conciliatory language as that contained in the last paragraph was far from the NPI's spirited rejection (NPI, 1983) of earlier claims in the Fosatu Challenge that the NPI was guilty of precisely the offence of using the weak official statistics to foster the beliefs now acknowledged to be 'major shortcomings of interpretation'. So too, was the acknowledgement of 'tendencies to assign to labour the responsibility for low labour productivity'. Although collaboration in certain research activities has taken place since that approach was made, the initiative did not deliver the much sought after agreement on Cosatu's part to take their place on the Council. The strong likelihood is that in today's more democratic climate, the NPI will be radically restructured, and that Cosatu will then take its rightful place on the controlling body. This matter is addressed briefly at the end of Chapter 1-5.

What emerges from this analysis of the nature of the NPI is a picture of a body that is an awkward amalgam of public and private sector components. As such, it is both advantaged and disadvantaged by its form. State funding, but of a steadily decreasing real value, is coupled with limited accountability to the broader public. Limited autonomy provides some scope for the NPI to criticise both state and capital, but financial dependence on both, as well as formal dominance by both through the Council circumscribe the NPI's ability to act independently. The NPI's failure to condemn the apartheid régimes' strategic investment programme, the major cause of falling capital productivity, is conclusive evidence of this.

Legitimacy of the NPI in the eyes of popular forces in South Africa, especially the trade union movement, could never be achieved. The NPI's past (acknowledged) anti-worker bias and its close ties to capital and the state prevented the organisation from taking the (social democratic) steps necessary to accommodate worker aspirations. The growing intensity of popular struggles

before February 1990 heightened the dilemma faced by the NPI, squeezed as it was on the one side by falling revenues and hence forced deeper into the arms of paying clients, and on the other by growing radicalisation of the worker movement. Both sets of pressures were increased by the recurrent and extended crises through which the economy staggered.

Lacking as well the formal independence of a properly constituted national statistical bureau, many of the NPI's attempts at statistical production were bound to be suspect, if not on occasion, mere special pleading. The moral hazard of being involved in both the production of statistics and the advocacy of policy on the basis of those statistics is sometimes almost insuperable. Under conditions of growing prosperity and limited strife neutrality is not impossible to maintain, but rising social tensions of the type experienced in South Africa drown cool counsels. Issuing invitations to take part in 'open and creative discourses' under such circumstances is fruitless.

Struggles over wage increases in South Africa

There are a limited number of ways in which incorrect statistics can come to have an importance beyond their trivial foundations in a weak data set.⁴² One of these is when they are deployed, particularly in the absence of countervailing information, in the wage/productivity debate. When the record is distorted by errors in the basic data from which it is constructed, much mischief can be done. In the first place, propaganda based on incorrect analysis is likely to raise tempers unnecessarily in hostile climates like that of South Africa's strongly adversarial industrial relations system. Secondly, to the extent that the imbalance between the rate of growth of wages and of labour productivity growth has been overstated by the NPI's incorrect statistics, many capitalists could have been replacing workers with machines for reasons unconnected with relative price changes in the factors of production labour and capital. In management circles, it is common, when speaking of labour, to refer to the 'hassle factor' involved in dealing with worker demands. Adverse publicity could exacerbate this view. Welcher (1991), for example, suggests that capital intensity has been increasing in part because management's **perception of worker productivity** (not their measurement of it) is negative.⁴³ In the face of what are perceived as 'political' wage demands, some employers in the economy have espoused views which favour a reduction of the rate of growth of employment, especially of black workers.

42 An attempt to address the question of the importance of errors in national statistics is made in Chapter 2-1.

43 See the commentary in Meth (1991c) on Welcher's work.

Economic policies to combat social ills are neither easy to introduce nor to sustain, even when the illfare they seek to address is so obvious that there is no gainsaying it. Furnishing adequate explanations for socio-economic illfare is also not a simple matter. Painful truths such as high infant mortality in certain sections of a population are readily comprehended in physiological terms. Economics can contribute to an understanding of the phenomenon, but the difficulties of devising and putting in place appropriate solutions means that people often are abandoned to the impersonal workings of long run 'economic' forces. Occasionally, very occasionally, it may be possible to say that one understands, tolerably well, a particular economic phenomenon. Most of the time, however, the evidence is inconclusive - several theoretical explanations can usually be provided for the same event.⁴⁴ Because the mechanisms for choosing between competing explanations are imprecise, care needs to be exercised in interpretation. Only those whose business is advocacy or evangelism enjoy the luxury of being able to dispense with the caution that should be the stock in trade of every economist.

Two brute facts cast a shadow over every aspect of the debate over wages in South Africa. The first of these is that the period under consideration - roughly from the early 1970s until about 1989 - was characterised by inflation, most of the time in double figures. Everybody knows that under inflationary conditions, wages and commodity prices in nominal prices terms rise more rapidly than does output in real terms - that is one definition of inflation. Comparisons between qualitatively different variables like real wages and commodity prices in general, if made at all, need to be made with care.⁴⁵ The (repeated) statement that wage growth is

44 Several reminders of the highly tentative nature of much of what passes for knowledge in economics (the 'received wisdom') may be found in the comprehensive two-volume survey on the then state-of-the-art theory in labour economics (to take but one of many fields) edited by Ashenfelter and Layard (1986). Despite energetic attempts to analyse the demand for labour "...a remarkable amount is still unknown." (Hamermesh, 1986, p467). Attempts to model firms' factor demand strategies yield intellectual structures that are probably unlike those used by "...rather few, if any..." firms. This raises questions about the validity of "as if" modelling which "...will probably remain on the agenda for a long time to come." (Nickell, 1986, p521). Studies on the labour supply are slightly more advanced, primarily because of the greater availability of data, but their state is still fragile - the standard model has never been tested adequately - Pencavel (1986, p94) says that:

"...few scholars have conducted their research with the aim of testing the theory; most have been interested in quantifying a relationship whose existence is presumed to be true."

Research into trade unions is not in much better shape - despite the progress that has been made, Farber concludes that:

"...there remains an extensive agenda for further research that needs to be addressed before economists can claim a real understanding of union behavior." (1986, p1085)

Kennan states flatly that: "There is no commonly accepted economic theory of strikes." (1986, p1091). If there were, the parties could predict when strikes would occur and hence take action to avoid them. Kennan notes that this could be called the "Hicks Paradox". This trap cannot be avoided by bourgeois economics as long as it remains contemptuous of political economy, sociology and the other disciplines required to comprehend a dense social reality. Economics, with its vain search for rigour, is almost powerless in the face of such complex phenomena. Many more examples can be provided, if necessary, to caution the profession against the making of excessive claims about what the discipline 'knows'.

45 The inflationary pressures that began to assert themselves in the early 1970s have yet to be brought fully under control. It is not the intention to enter the complex debate on the causes of this social malaise here, other than to comment on the fact that the NPI does not show similar restraint. Almost since that institution first began operations it has published its wage and productivity estimates in a way that invites readers to accept a simple cost-push explanation of the phenomenon. Innumerable comparisons of the real increase in output with the increase in nominal wage or earnings have appeared in the press. To attempt to offset this bias, the analysis in this study compares real with real. Questions of the causes and impact of inflation are

outstripping productivity growth, for example, contains both an implicit analysis (wage demands are exerting upward pressure on prices) and an implicit policy prescription - the wage (in nominal terms) should be as close to productivity growth (in real terms) as possible. Certainly, it should be below the rate of inflation (plus the rate of growth of productivity?) if there is to be any hope of reducing inflation. Once, however, an inflationary spiral has started, or even if steady state inflation exists, individuals who fall behind suffer a loss of real welfare. The implicit policy therefore has distributional implications.

The problem with inflation is that its causes are not very well understood. Based on some of the theoretical explanations, certain 'cures' are posited - crudely speaking, the monetarist school urges control of the money supply, whilst cost-push theorists demand wage, and less frequently, price restraint. Variants of the former seem to 'work', at least for a while, but at tremendous social cost. The resulting burdens are not evenly distributed in the society at large. A policy of wage restraint, if implemented, would result in substantial redistribution against wage earners, unless bolstered by other measures.

Unlike politicians, economists cannot speak with excessive confidence in such circumstances. In this part of the study the presence of inflation is taken as a given, as is the possibility that excessive wage claims could exacerbate the inflation. What is not accepted is the proposition that workers organised by trade unions affiliated to Cosatu are largely responsible for inflation. This has not been established with confidence by any study conducted in South Africa. Endless numbers of correlations have been published by the NPI⁴⁶ but available data are not accurate enough to permit researchers to pinpoint causes.

Regardless of this, wage demands made by black workers, and in particular by those in Cosatu-affiliated unions are seen to be the primary cause of several social ills. A common way for the debate to be cast is in terms of the impact on employment levels of unionised worker wage policies. Yet the example set by Cosatu in this matter could well be emulated by the relatively highly-paid, of whom there are still large numbers. Cosatu's policy (following Fosatu) has been one of insisting on the largest proportional increases for the poorest paid workers.⁴⁷ An attempt was made in the previous chapter to make sense of relative wage

(continued)

performance of unionised workers. To proceed thus is not to deny the importance of the problem of inflation - inflation is not treated here because it requires much more attention than can be given it. These issues are touched on in Meth (1991c).

46 See the NPI's *Productivity Focus* for every year from 1984 onwards.

47 There are many forces operating to reinforce and perpetuate the differential conditions of existence of the working class. For a variety of reasons, an excess supply of labour, especially of unskilled labour, does not readily drive down the wage, nor does it permit the substitution of 'cheap outsiders' for relatively expensive 'insiders'. For an exposition of the 'insider/outsider' thesis, see Lindbeck and Snower, 1988. Inter-industry differentials persist and sometimes grow because of differential productivities of capital that cannot be shifted magically from sector to sector as is done in some of the cruder versions of neo-classical economics. For a discussion of this, see Wachtel and Betsey, 1980.

There is, however, some evidence to suggest, as Peter Moll points out, that for a while

movements. Although not wholly satisfactory, the numbers do coincide roughly with the reality of the period as progressives have come to understand it.

From the mid-1970s onwards, the introduction of 'job evaluation' systems entailed identifying those groups whose wages were 'out of line' with what the (pseudo) scientific predictions of job evaluation theory said they should be. As a result, there was a reshuffling, both of the notions of the wages that attach to particular grades of work, and of workers between grades. The ideology of wages explicitly adopted by significant sections of capital, by trade unions and by the state in the 70s was one which called for the largest proportional wage increases to be awarded to the lowest-paid workers (Meth, 1985). To the extent that such wage increases were granted, they appear to have had the effect of causing the capitalist class to revise completely the view of black workers as 'cheap labour'. There thus existed for a while, a grudging consensus that black wages were too low. This quickly gave way to the now-popular belief that black wage demands are 'politically' inspired, and must be resisted unless matched by productivity improvements (Meth, 1985). The dominant ethos is now one which hints that wage increases for all workers be restricted to something near the average rate of growth of output for workers in general.⁴⁸

Despite the fact that conditions tended to favour a widening of the wage-gap between skilled workers and those at the bottom end of the (formal) job market where the bulk of Cosatu's membership is to be found, by and large, the disciplined and responsible stance adopted by Cosatu was maintained. There has been little or no public acknowledgement of this self-sacrifice - instead, organised workers, who are now said (pejoratively) to constitute an élite, have been accused of ignoring and indeed, exacerbating the plight of the very poorest (the unemployed, the under-employed and the non-organised workers). This is a sensitive matter, and one which is treated delicately in the official Cosatu history (Baskin, 1991). Commenting on successful efforts of "[S]trongly-unionised workers to increase their share of the economic cake", he observes that:

"Unfortunately, the other side of this achievement has been the growing gap between unionised and non-unionised sectors... Differentials have also grown within many sectors, and within many unions... This economic dualism between workers in different sectors contains dangers both for unions and the economy generally." (p254)

The extent to which organised labour may be held responsible for this 'dualism' remains an open question, and the debate on the effect of union activity on employment was apparently inconclusive, at least up until the time that the World Bank and the International Monetary

(continued)

"Presumably firms found it easier to promote their 'liberal' image by raising wages at the lower end of the spectrum than to raise wages of skilled workers." (1990, p89)

48 The conservative distributional implications of such a policy have been explored in Meth (1991b).

Fund (IMF) commenced intellectual operations in South Africa. In 1990, for example, Peter Moll stated that:

"...[the union's] impact should not be exaggerated. Unions and industrial courts cover only a small part of the economy, and this has led other economists to devise theories like 'efficiency wages' and 'implicit contracts' to explain the downward inflexibility of wages." (1990, p95)

Roux (1991, p111) was a little less uncertain - he claimed that:

"...factor price distortions, while not necessarily the cause of rising unemployment, have certainly made a contribution."

Regardless of the arithmetic of the question, to which we will come in a moment, it is clear that another brute fact of South African reality is the division of the economically active into those with access to modern- or formal-sector employment and those without. Apartheid regulation functioned until very recently (about 1985 or so) to keep informal-sector activity down to an abnormally low level for a country of South Africa's developing status. Speaking very roughly, white, Indian, a minority of coloured, and a tiny minority of African workers, occupy all of the skilled positions in the labour force. About one third of African, and a somewhat larger proportion of the coloured workers occupy either semi-skilled or unskilled formal-sector jobs. The remainder cannot realistically entertain any hope of obtaining access to formal-sector employment.

Under these circumstances, it has become increasingly fashionable to speak out against the wage increases garnered by unionised workers, because they diminish what little likelihood there is that those outside the formal sector will be able to obtain formal-sector employment. In the early days, few of those who advanced this argument attempted to present a fully-worked out position which quantified the effects of which they spoke. The set of beliefs which constitute the overall framework (the ideology) within which debates over wages are conducted has been a long time in the making. Although the concern of this study is with events up to about 1989, the fully-developed version of the ideology of wages (with econometric bells and whistles attached) only became visible from about 1992 onwards.

Manufacturing a gloomy consensus

Reference has been made above to the extent to which the analysis offered here draws on the paper I called "South Africa's Thatcherite Productivity Miracle: 1979-89" (Meth, 1991a).⁴⁹

49 End-year selection is often critical. The end-year chosen in the paper for discussion of the British economy (1979) was selected for political reasons - Thatcher, it will be recalled, came to power in that year. In the South African case, the year 1979 was selected because it is the last year for which a reliable estimate of manufacturing sector output was available (see Meth, 1991a) - the choice was thus dictated by empirical

That paper attempted to show that the campaign to encourage the compression of wage increases for the predominantly poorly-paid workers in the emerging unions into line with increases in output per worker⁵⁰ was congruent with the central tenets of the Thatcherite/Reaganite revolution which took hold in the world's least-successful major capitalist economies, Britain and the USA.⁵¹ The suggestion that the South African economy has enjoyed a Thatcherite productivity 'miracle' was made (half in jest) when an early version of the paper was presented to a seminar of the Durban branch of the Economic Society of South Africa (May 1991). As the work of writing up proceeded, it became clear that the suggestion was not as outrageous as first it seemed. This is so for three reasons. In the first place, the error in the national accounting estimates of the level of output in manufacturing changed the dismal productivity record into a fairly respectable one, given the desperate political conditions of the period. Secondly, part of the productivity improvement was secured at the cost of employment in the sector during the worst of the downturn - a typically Thatcherite feature that was echoed in large falls in the number of jobs elsewhere in the economy.⁵² Finally, the changes in the manufacturing sector were accompanied by a substantial redistribution of factor incomes away from the working class.

Stripped of its hyperbole, the *Monthly Review* jibe about the "...successful brainwashing of the public..." carries the message that one of the hallmarks of success of a propaganda campaign is getting the target audience to believe the propaganda. The NPI's commitment to changing

50 Not surprisingly, the NPI has never, to my knowledge, made any clear and unambiguous statement about what it considers to be the appropriate size of wage increase to be granted under inflationary conditions. Sensitivity to the important distributional implications of such a wage policy has presumably prevented those who would have wished to do so from recommending that wage increases be restricted to the rate of growth of real output per worker. The NPI has relied instead on issuing hundreds of statements linking inflation with wage push, leaving to others the task of drawing the necessary conclusions.

51 Without wanting to push the parallels too far, it was argued that the South African manufacturing sector displayed a growth pattern that bore some resemblance, albeit in less stark terms, to the Thatcherite productivity miracle (so-called) imposed on the British working class. It is not necessary to reproduce the British results here.

52 See Meth (1994). One of the clearest demonstrations of this is the record of job loss in what were formerly havens of secure employment. Simply by opening the CSS' statistical biennial at the right page one may see that in the government business enterprises and parastatals singled out for productivity improvement programmes, employment fell substantially. The number of workers employed by the (former) South African Transport Services (SATS) fell by 100 000 over the period 1982-89 (from a peak of 278 000 in 1982) (*South African Statistics 1990*, p7.26). Employment in electricity generation and distribution fell from 66 200 in 1985 to 52 400 in 1989 (p7.24). The number of workers in the basic metals industry, which includes Iscor, the former state-owned steel producer, declined from 124 600 in 1981 to 110 900 in 1989 (p7.20). Over the period 1982-89, the impact of this state-sponsored productivity drive on African workers was more serious in each case. White employment in SATS fell by 32,8 per cent and of African workers by 38,9 per cent. Employment of whites in electricity rose by 14,4 per cent while African employment fell by 16 per cent. African employment in basic metals fell by 11,8 per cent while white employment declined by 9,7 per cent.

The stance of the National Party toward job loss among state employees is interesting. Large numbers of jobs in SATS (formerly the employer of last resort for whites) were allowed to disappear, almost without comment. Other jobs for whites in the state bureaucracy have, however, been protected quite fiercely. Peter Moll (1990, p66) commented on the tenacity with which both the National Party and the Conservative Party would resist slimming down the bureaucracy, pointing to the clash to which this would lead in the new South Africa.

attitudes has been referred to above, as has the goal of instilling in the minds of 'all producers and consumers' the appropriate attitude to productivity improvement. Unless the campaign is based on rigorously impartial findings, this goal as articulated is but a polite and positive rendition of that of 'brainwashing the public'. The 'public' is, however, a crude, undifferentiated concept - one would expect some differences in the ease with which the message was accepted by different sections of the audience. Trained scholars, alive to the fact that South African statistics are dubious, and aware of the fact that the CSS has already misled the community (and the NPI) on a previous occasion, would (should?) bring to bear a certain scepticism, especially if they suspected special pleading. The wider public, generally lacking the tools of critical reflection in this sphere may be easier to convince, although paradoxically, a diffuse hostility to all statistics and the experts who promote them can sometimes serve as a shield against the tyranny of so-called science. Be that as it may, the softening-up exercise conducted by the NPI in the form of the repeated invocation of the conclusions drawn its simplistic analysis of incorrect data seems to have succeeded in convincing all and sundry (except diehards in the unions).

This helped to create an intellectual space in which the ethos of Thatcherism, an anti-union, anti-collectivist ideology whose policy prescriptions consist of handful of powerful measures - privatise, deregulate, do not redistribute,⁵³ and curb worker power by whatever means available - could flourish.⁵⁴ That climate is exemplified by the views of Chris Stals, governor of the Reserve Bank - a powerful figure, almost out of reach of any form of democratic control. Here he is, responding to critics who suggested that tight monetary policy was responsible for high unemployment:

"Reserve Bank Governor Chris Stals has isolated rising labour costs as the major economic problem South Africa faces.

"This is what is pushing our rate of inflation up, this is the malignant disease that erodes our competitiveness vis-a-vis the rest of the world, this is what is closing our marginal mines, this is where the origin lies of rising unemployment in SA," he said in Johannesburg yesterday.

He called on trade union leaders and management to work together to reverse "the alarming trends" of the labour market over the past 20 years.

Stals described the rates of increase in the labour costs per unit of production as "disturbing" and "alarming".

Unit labour costs rose by 17,2% and 16,3% in 1989 and 1990. The country's major problem was reflected in these statistics.

53 A separate paper in the Economic Trends Research Group series of Working Papers called "Income redistribution, productivity and trade union action in South Africa", examines the NPI's stance on redistribution. See Meth, 1991b.

54 The previous South African state, although it had to glance occasionally over the shoulder at the popular forces, came to subscribe more and more wholeheartedly to those basic tenets. Of course, it could not go 'all the way'. A rumoured 1992 decision not to deregulate the fuels industry because 40 000 or more petrol pump attendants stood to lose their livelihood, was consistent with the actions of a government being made increasingly aware of the fact that such overt commitment to the market had a high political price.

He chided economists who blamed tight monetary policy for rising unemployment, saying they should "rather address the real basic problem and analyse the reasons for the declining trend in productivity, for excessive increases in wages and salaries and for the rising unit labour cost."

Productivity per worker fell marginally in 1990 after increasing by only 0,8% in 1989 and rising by 2,3% in 1988. At the same time, the average real wage per worker increased at rates of 2,2%, 2,9% and 2% respectively in 1988, 1989 and 1990. (Greta Steyn, *Business Day*, July 24, 1991, p1)

Stals' figures for the later years may be correct, although one would not want to bet on it, but what is one to make of the references to the "...alarming trends..." of the labour market of the past 20 years? The reduction of the inflation problem to simple cost-push and the exaggeration of its impact on South Africa's competitive position is vintage NPI.⁵⁵ Sober analysis suggests that trends in the labour market are not anything like as alarming as Stals' claims suggest they are.

The rôle of some academics in this matter is a little perplexing: when it comes to a consideration of the labour market, economists seldom tire of reminding those within earshot of one of the few pieces of knowledge about which they feel sure - that the demand curve for labour slopes downwards to the right, ie, if the wage rises, the quantity of labour demanded decreases. Armed with this rudimentary insight, some of them venture into areas where prevailing levels of ignorance should daunt even the foolhardy. Instead, a readiness to be guided by simplistic theory into the making of pronouncements about 'appropriate' wage levels is buttressed by a willingness to use a rotten set of statistics to do so.⁵⁶

The NPI's unceasing efforts to promote the view that productivity growth in the South African economy is poor appear to have begun bearing fruit from some time in the early 1980s - from then onwards, one could detect evidence of the growth of a shared perception among commentators from business and the state that this was indeed the case.⁵⁷ Much easier to accept, of course, was the corollary of this view which has it that the rate of growth of wages, especially those paid to black workers was excessive. Increasingly, suggestions that the 'real' wage was too high began to appear. The manner in which this view was sometimes articulated by the NPI can only be described as crass. Here is an example from NPI chief executive Visser:

55 See Meth (1991c) for a discussion of the distorted presentation by the NPI of unit labour costs in an international context. The NPI makes no allowance at all for differential movements in exchange rates - it simply compares rises in costs in national currencies and infers falling competitiveness from the fact that South Africa's inflation rate is higher than those in other countries.

56 Instances of this are cited below. In each case, the academic concerned offers the policy prescription that the real wage should be lowered. In none of the cases is any attempt made to engage with the poor quality of the statistics on which this conclusion is based, nor is there any consideration of the politics of such suggestions.

57 The extent to which there is an awareness of a 'productivity problem' is the subject of some disagreement. See Meth, 1991c, Section 5.

"...you must choose the most cost-effective technology with a balance between labour and capital. If labour has outpriced itself as an input compared to capital, then labour must simply adjust its price." *Manpower Survey*, supplement to *Financial Mail*, September 16, 1983, p21.

Influenced by the NPI, the President's Council conducted two fairly extensive investigations, one of which deals directly with productivity (P.C. 1/1989),⁵⁸ the other indirectly (P.C. 1/1987). Both reproduce the same incorrect conclusions from the same incorrect data - the former in particular, containing large numbers of tables and graphs culled straight from NPI publications. The two reports have some pretensions to being scholarly, being written and presented in the style of an academic report. Of the two, the later report, the one dealing with productivity is the more relevant for this study. The repeated extolling of the virtues of free-market economies in it is tiresome, but presumably no more so than the obverse propaganda made by the left. Some passages are silly - the attempted description of the way in which black South Africans experience life as 'traditionally socialistic' being one of them. Lumping white racist authoritarianism with tribal structures into one and concluding that a lack of exposure to "...the so-called free economy..." will lead Africans to regard this 'socialism' as the norm and hence, to prefer it above free enterprise (P.C. 1/1989, para.5.193, pp201-202) betrays a world view which explains why some of the central recommendations are impossible to implement.

Anti-union sentiment was especially apparent in the earlier report (the Jacobsz Report). There was no shortage of businessmen willing to claim that rising militancy had the effect of speeding up the rate of mechanisation:

"It was repeatedly pointed out to the committee...how important a role... excessive demands and wage increases and strikes by Black workers - especially strikes instigated by trade unions - played and were still playing in the process of capital deepening."

Submissions to the committee contained bitter complaints about the:

"...unrealistic wage demands, stay-away actions and the amount of senior management's time that was taken up by the continual declaration of disputes which then had to be dealt with."

One witness stated that:

"An unexpected increase in strikes characterised the first few months of 1986. Trade unions are becoming more uncontrollable (*sic*) and leaders of trade unions and federations are making increasingly daring political demands. A new emotionality and aggressiveness is to be noted in labour relations. The Black unions are becoming an ever bigger platform in Black politics." (P.C. 1/1987, p52)

58 Large chunks of this document were written by an NPI economist, Mr Jan de Jager. He was formerly employed by the Federated Chamber of Industries, a major employer organisation, now a part of SACOB, the South African Chamber of Business. *pers. comm.*, Jan de Jager, March 1993.

Nor was there a shortage of witnesses articulating similar views in the later report. The report deserves a full-scale critique, something which unfortunately, it is unlikely to get. For the purposes of this study I wish to draw attention to just one aspect of what I would regard as a seriously flawed document, namely the recommendations aimed at incorporating the progressive unions into the productivity improvement campaign. The report is fully alive to the fact that its analysis is conducted in the midst of the political turmoil sweeping the country. It was conscious as well, of the fact that inevitably, the unions were in the thick of it, and what is more, without a coherent and clearcut position of their own either.⁵⁹ Despite this, the compilers of the report manage to gloss over the fact that debate did not turn on whether or not free enterprise was more productive than socialism, nor whether Africans really knew about the joys of capitalism. They miss completely the underlying political economy which informed the perceptions of many of the trade union activists - namely, the belief that capitalism is fundamentally riven by the conflict between labour and capital.⁶⁰ That being the case, any attempt to draw the Cosatu unions into the productivity drive was bound to fail.

Towards the end of a report in which large amounts of the standard NPI propaganda about imbalances between wage and productivity growth had been regurgitated, the compilers saw fit to make the following recommendation:

"The Committee is of the opinion that the labour union movement has not as yet been adequately involved in productivity activities in South Africa. This is demonstrated, *inter alia*, by the fact that the union movement is not comprehensively represented on the Productivity Advisory Council. It is **recommended** that the NPI embark on an intensive campaign to involve unions in the productivity movement in South Africa. The proposed general information programme suggested in paragraphs 6.11 and 6.12 should contain a special element aimed at labour unions. The activities of the Japan Productivity Centre, where a special Trade Union Productivity Council was established, could be used as an example." (P.C. 1/1989, para.6.37, p259) (Emphasis in original)

Paragraph 6.11 referred to the introduction of productivity education in the school curriculum - to be "...done without undue ideological bias...", in the words of the committee. Paragraph 6.12 referred to the rôle of the mass media, including the SABC, which had made a comprehensive submission containing a list of topics suitable for radio and television. This faith in the ability of propaganda to change attitudes is both amazing and pathetic. Its repeated failure should surely have caused questions to be raised about the analytical basis or premises

59 Paragraph 5.66 (p164) contains a discussion of the populist/workerist tension within the union movement, noting that it had been said that:

"Many labour unions... see the union mechanism as the only means through which they can show political muscle and as a result populist issues get precedence over workerist issues."

For an antidote to this somewhat superficial treatment of a complex issue, a reading of Baskin's history of Cosatu (especially Chapter 13) which discusses the deep divisions of the time (1991, pp212ff) is recommended.

60 See for example, Baskin's account (1991, pp212ff) of the differences in position between NUM (the National Union of Mineworkers) and NUMSA (the National Union of Metalworkers of South Africa).

on which it rested. The oddest aspect of it all is the inability of the authorities to recognise the one propaganda step that could possibly have succeeded, an acknowledgement that the NPI analysis was crass and over-simplified and widely taken as a mark of hostility. It seems not to have occurred to the authorities that to have acknowledged publicly that a more subtle, a more nuanced analysis was required, would at least have opened a space for discussion. There was, however, never any let-up in the stream of pronouncements along the lines quoted above - wage growth exceeds productivity growth, wage demands are political, unreasonable wage demands cause inflation, not to mention analyses that were economical with the truth, such as the distorted claims about the relationship between productivity growth and employment growth (Meth, 1994).

Other commentators, presumably mindful of the sensitivity of the subject, were more circumspect. The tendencies for the relative price of labour to rise too quickly were argued to have (and to have had in the past) important consequences for future development in South Africa. The need to solve the unemployment problem while simultaneously raising productivity levels, was said to pose severe and possibly insoluble problems. In this matter, a profoundly pessimistic analytical consensus among economists across the spectrum seems to have emerged. As far as the productivity leg of the dilemma is concerned, Gelb stated that:

"...there is a need to lower costs and improve productivity in the existing productive sectors, especially mining and manufacturing. This will require increased investment in new technologies, and/or lower wages at least in real terms. In other words, capital in these sectors needs to be strengthened, that is, profitability restored." (1988, p23)⁶¹

The same proposition may be found in the work of bourgeois economists where it is couched in the impeccable and non-emotive language of 'science': Holden's suggestion to tax wages in the resource sector of the economy to reduce the pressure on wages in the manufacturing sector is a case in point (1990, p272). A further example is this statement by McGrath (following a call by him for the dissolution of closed shops "... which still protect white workers ..."):

"Another related issue is whether attempts should now be made to lower the real wage of the urban labour force to levels more consistent with a labour surplus economy, as occurred in Brazil between 1965 and 1967.⁶² Lower real wages in the modern sector would have the advantage of increasing profits, and

61 The three authors cited here do not suggest alternative divisions of the benefits or burdens of production. Given that the spotlight in this debate has been on unionised black workers, this silence gives their work the character of a thinly-veiled attack against that group of workers, even though this may not have been the intention.

62 The use of this particular example to make the case is instructive. The wage reductions in question occurred after the military seized power in Brazil, forcibly restructuring Congress to construct a "one-and-a-half" party system, primarily at the expense of the PTB, the Brazilian Labour Party. (Cammack, 1991, p36). The Brazilian 'economic miracle' produced by what Cammack refers to as "savage capitalism", rested partly on the "...exclusion and repressive control of the majority of the population." (1991, p22). McGrath refrains from offering guidance on the ways in which a democratic state might set about 'lowering wage levels to make them more consistent with a labour surplus'.

perhaps the domestic propensity to save and invest, and would provide a stimulus towards less capital-intensive production methods and would increase the competitiveness of manufactured exports." (1990, p106)

If implemented, such a policy would entail a redistribution against workers, in favour of capital. These authors are silent on the problematic question of both the intra- and the inter-class struggles that would ensue were attempts made to put them into practice. Indeed, the amorphous manner in which the statements above are cast is typical of the conduct of academics when treading on dangerous ground.

The data on which these suggestions for a 'consideration' of a possible reduction in the 'real' wage rest have been shown to be somewhat shaky. Given the contentious and obviously political nature of the proposed interventions, those making them had a duty to be sure that what they recommended had a sound basis in fact. It is not obvious, however, that authors quoted here have considered critically the quality of the information on which they based their views.

Greater sensitivity was displayed by Peter Moll (1990) and by Natrass (1992), who, whilst stressing the impact of further wage increases on the excluded 40 per cent at the bottom of the income distribution, were long on the social democratic structures required to reduce the level of adversariality in South Africa's industrial relations system, and, in particular, on the need for highly-visible improvements in the social wage.

Until South Africa's recent re-entry into the comity of nations, the NPI acted as standard bearer in the debate over wages and productivity. Of late, however, the formidable resources disposed of by the World Bank and the IMF have been brought to bear on the question. What follows give a short account of the initial foray. There would appear to be some cause for alarm, insofar as these institutions follow a trajectory not dissimilar to that of the NPI in allowing relatively scholarly interventions to degenerate, in the popular media, into the same ideology-laden prescriptions. That these interventions are rooted in the same poor data-set, uncritically applied, is further cause for concern.

Mirroring Natrass' and Moll's delicacy, the first venture by the World Bank into this sensitive area (Fallon, 1992), displayed considerable tact (and ingenuity) in looking at what others might regard as 'unjustifiably' high wages, one of the more important causes of the substitution of machines for workers. Normally, it is easier to assert that an observed change of this sort has taken place than it is to demonstrate satisfactorily that it is has, and for the reason commonly given. To show conclusively that 'unjustifiably' high wages have contributed

to the unemployment problem, some way has to be found to establish what appropriate wage levels should be. In the absence of a determinate theory of wages, and the necessary empirical data, resort has to be had to a variety of what are commonly termed 'heroic' assumptions. The fact that neither of the ingredients necessary to make a proper investigation are available did not deter the World Bank - the rough lineaments of the method Fallon employed are discernible in the following passage:

"...if the ratio of capital to black workers had remained at its 1960 value, black employment in 1990 would have been 3.66 times its level in 1960 -- an increase of 266 percent. The estimated model predicts that, given observed change in relative factor prices and the change in the external terms of trade, black employment in 1990 would have only registered an increase of 78.3 percent. This compares reasonably closely with the actual increase of about 70 percent. The total impact of changing factor prices and other changes is then a reduction in the order of (266 - 78) or 188 percent of the 1960 level. If relative factor prices alone had changed, it is estimated that black employment in 1990 would have increased by 197 percent. The impact of the reduction in black employment due to changes in relative factor prices taken alone is then only (266 - 197) or 79 percent of the 1960 level. This estimate thus suggests that only (79/188) or 42 percent of the shortfall in black employment attributable to the change in the capital-to-black-labor ratio between 1960 and 1990 arose from changing relative factor prices. It seems that the other factors [as discussed above] account for the bulk of the shortfall." (Fallon, 1992, p27)

The tone of this piece is measured - there is nothing sensationalist about the estimates of the impact of changing relative wages. This is as it should be, for both the data on which these conclusions rest and the model used to generate them are not above suspicion.⁶³ Fallon attempts to minimise the effect - note well the use of the word 'only' in both estimates of it. Similar restraint is visible in a later piece by Fallon and Pereira de Silva (1994). Here it is pointed out that:

"...movements in the real black wage contributed to the slowdown in employment growth over the period 1970-85... In the absence of such changes, it is estimated that employment of blacks and real GDP would have been, respectively, about 14 percent and 3 percent higher in 1985. Although important, the historical

63 There are several weaknesses in the Fallon study, not the least of them the arbitrary division of the labour force into unskilled (black) and skilled (white). Another weakness is the failure to model labour demand dynamically. The purely static (mechanical) relationships posited cannot capture the complex realities of the South African labour market. This may not be too much of a problem if the limitations which such crudity imposes on users of the results are borne in mind. For more serious analysis, dynamic modelling is probably necessary. Even the most advanced models however, fall short of providing an adequate account. After conducting a survey of the literature on dynamic models that focus on "...different possible structures of hiring and firing costs, and their implications for the time path of employment...", Nickell (1986) gave some indication of the distance between them and the reality they seek to capture with the observation that:

"The combination of a flexible technology with multiple factors and a satisfactory treatment of expectations remains elusive but is clearly the next step on the agenda." (p520)

After further discussion of the problems caused by the unavailability of firm's data, he commented that existing models used procedures that assumed that firms followed:

"...a completely optimal strategy. If the econometrician requires a large computer to solve for the firm's optimal factor demand strategy in any period so, obviously, does the firm. Yet how many firms base their employment activities on such a complex activity? Probably rather few, if any. The question then remains as to whether this is a good "as if" story or whether we must find ways of mimicking firm's rules of thumb if we are ever to model their strategies with any accuracy." (p521)

The issue in South Africa is the question of how firms respond to changes in relative factor prices. The answer at this point has to be that apart from a few rough, aggregate estimates, we really cannot say for certain what has happened or why.

change in relative factor prices can only account for a minor part of declining employment growth." (1994, p15)

The tone changes somewhat in a World Bank paper which attempts to summarise the results of the many research efforts undertaken by the Bank from the time their staffers first reappeared in South Africa. Ahmad *et al* (1994) observe that:

"A World Bank study estimated that increased union activity raised African real wages in the formal sector by about 15 percent above what they otherwise would have been from 1979 to 1990. The evidence is clear that higher wages have led to lower demand for labour - although the magnitude of this effect remains in dispute. The World Bank study estimated that, in the absence of the 15 percent union-induced increase, formal African employment would have been 200,000-400,000 above the present level of about 5 000 000." (1994, p7)

In the hands of the media, it becomes considerably less balanced. Under the headline "IMF urges wage restraint on SA", an article in *Business Day* of 3 October 1994 reported that:

"The IMF has stated publicly for the first time that it sees unchecked wage increases as the single greatest threat to SA's economic recovery and its chances for competing internationally.

The wage message is delivered in the fund's just-released 1994 annual report, which contains a summary of its board's hitherto confidential assessment of the SA economy...

The IMF echoes the warning by a World Bank report in July - *Reducing Poverty in SA* - which said high wage demands had cost hundreds of thousands of jobs...⁶⁴

The *caveats* have gone and in the place of the disputed 200 000-400 000 we now have simply 'hundreds of thousands'. Not a direct misrepresentation, of course, but the very vagueness of the report invites the reader to draw the worst conclusions. Given the climate of reconciliation in South Africa and the attempts to build a social contract, another recommendation of "...the fund's directors..." referred to briefly in the article, one could imagine it being written in a very different way by someone less jaundiced than its author, Simon Barber. Instead of adopting a confrontational stance, and one which is almost certain to antagonise workers and their leadership, the article could have commenced by stressing the growing need to build the social contract, so as to win worker's acceptance of the need to moderate wage demands. This is what Nattrass (1992) does, and she does it, as mentioned above, with great tact.

64 Waiting in the wings is an even cruder estimate made by the IMF of future effects of the impact of real wage growth of 1¼ per cent over the projected period (1991-2000), or ½ of one percentage point more than a baseline growth rate. This, according to the IMF, will result in 1¼ million fewer jobs being created for "nonwhites" (1992, p17). Although this enterprise had about it a more-or-less scholarly air, the use of a simple Cobb-Douglas production function results in a very much higher estimate of the elasticity of substitution of capital for labour than the World Bank effort. Thus the projected IMF job loss for a nine-year period (1991-2000) is three to five times as large as the World Bank figure for the eleven-year period 1979-90. It will be interesting to see how the press handles this estimate if the IMF decides to relaunch it.

It is noteworthy that in this debate little consideration is given to the proposition that similar sacrifices be demanded of the highly-skilled.⁶⁵ Indeed, there seems to be general support for the proposition that this highly-mobile group must be cossetted because of the contribution its members can make to economic growth. Certain workers, whose relation to the apartheid state enabled them to gather economic rents, have come to be regarded as fair game, but the highly skilled are not amongst them. As for any suggestion that redistribution against the highly-skilled might be appropriate, in view of the fact that apartheid created the artificial skill shortages which enabled all possessors of skills to enjoy very high standards of living, I have never seen it suggested by any of those who talk about restricting wage growth.

The harsh truth though, is that at some point, rising real wages will diminish outsiders' chances of obtaining modern-sector employment, if they have not already done so. There are important questions to be faced about the distributional implications of different policies. Attempting to bury these in platitudes about 'wealth having to be created before it can be distributed' as the NPI has done on so many occasions in the past, is unacceptable (Meth, 1991b). The solution to the problem of the excluded ones (who are also under-represented politically) is something that should be found by delicate negotiation - it ill becomes the already over-privileged to suggest passing on the burden of securing the welfare of the worst-off in our society to the second worst-off group. Extreme care needs to be exercised when talking about appropriate wage levels, especially by those prone to expressing their sympathy for the worst off in our society in the form outlined above. In a sense, the very vagueness of the way in which this concern is expressed is an indication of an awareness of the sensitivity of the matter. The fact that people usually do not care to name the group of workers they think is overpaid speaks volumes.

Statistics without compromise

There is a long history of insensitivity on the part of the dominant groups in our society to the plight of the poor. Misguided social engineering, carried out by zealots riding roughshod over the intended beneficiaries is the hallmark of the past several decades. It would be ironic if a new consensus on the need to suppress unions emerges on the basis of a faulty understanding of the operation of the economy. The imprimatur of the World Bank and the IMF on the NPI's faulty analysis is an ominous sign of the gathering forces.

⁶⁵ A fragile social consensus of this type did exist for a short while in the 1970s. It probably helps to explain part of the remarkable decline in white earnings over the period. See Meth (1985). In the recent uproar over high parliamentary salaries, Trevor Manuel, the Minister of Trade and Industries called upon highly-paid managers to accept such a redistribution. His plea has not started a stampede.

If this does come to pass, the NPI will have much to answer for - in short, it has:

- (i) made uncritical use of suspect data to produce a relentlessly pessimistic, but at times, inaccurate picture of the performance of the South African economy, and,
- (ii) as part of the campaign to raise awareness about productivity in South Africa, selectively emphasised certain statistics and the conclusions based on those statistics in media releases so as to reinforce the aforesaid prejudice, and,
- (iii) insisted on using and defending a set of analyses that overshadowed alternative (and more plausible) explanations for South Africa's present predicament, despite having been informed of the existence of errors in the basic data used to perform the analyses.

The NPI, a quasi-government body, could not in the past develop the critical distance necessary to dissociate itself from the apartheid state, nor, as a body with overwhelming business representation on its advisory council, could it resist the temptation to lay heavy accent on the symptom which causes most distress for the business community, namely the struggle by black workers for what they regard as just recompense. Where poor productivity is revealed in the modern sector of the South African economy, it is likely, in many cases, merely to be one of the symptoms of a much deeper malaise. More than four decades of capitalist growth, sometimes fostered and sometimes hindered and distorted by the apartheid régime, have produced an illness whose economic and political dimensions are inextricably intertwined. Although one may be able to devise particular economic indices to measure the magnitude of the sickness, it is idle to ignore the fact that these are but partial indicators, and that any serious discussion of productivity is, sooner or later, going to enter a political terrain. It seems better to acknowledge this than to behave as if crises in the political economy were but deviations from harmony - temporary aberrations which could have been overcome, if not by rational discourse, then by good example, aided of course, by market forces. In other words, productivity statistics are saturated with political significance.

The steps necessary to avoid being seen as partisan in this matter are simple in the extreme - advocacy of any kind simply cannot be reconciled with the production of statistics. An institution such as the NPI whose primary function is the improvement of productivity simply cannot be entrusted with the responsibility of creating the yardsticks on which its own efforts are to be based. The Statistical Office of the Department of International Economic and Social Affairs of the United Nations has laid down comprehensive guidelines for the organisation of national statistical services. Spelling out the necessary conditions for official statistics and the agencies producing them to play their "wide-ranging role", this document states amongst other things that:

"A condition for the acceptance of statistical results is that the statistical office is, and appears to be, impartial and objective and beyond any professional reproach;" (UN, 1980, p3)

For neutrality and impartiality to be achieved, national statistical offices have to exercise extreme care in the interpretation of the statistics they produce. A balance has to be struck between the unacceptable practice of publishing statistics with no comment at all, and the advocacy of any particular policies based on the analysis of the information. Although the NPI is not a national statistical office in the sense envisaged in the UN document, it functions partly as one, being the sole producer of productivity statistics on a national basis. The obvious clash between its functions as statistics producer and as advocate of steps to improve productivity makes it difficult, if not impossible for the organisation to meet the UN's impartiality criterion.

Chapter 1-4

Research method, ideology and the choice of analytical framework

"Methodology and politics are both means for moving from one historical stage to another. The only difference is that the standard methodologies disregard the fact that history constantly produces new features." (Feyerabend, 1979, p18n)

Introduction

In the previous chapter, the empirical reality with which this study grapples, a significant under-estimation by the CSS of output levels in manufacturing, and the systematic use by the NPI of these fragile numbers to promote a particular view of the relationship between wage and productivity growth, have been presented. This chapter describes some of the methodological problems that have been encountered in the prosecution of the study and then takes a preliminary glance at the bodies of theory relevant to these two, very different research activities. First up for consideration is the theory pertaining to output valuation. Next is the Marxist political economy applied to some of the activities of the NPI.

Although not obligatory, it appears to be at least customary for a dissertation to commence with a statement of a research question, one which has been honed or refined by a literature survey of the theoretical areas from which the hypotheses to be tested are drawn. Often, these are expressed in the form of models which link the relevant variables in systematic ways.¹ A report on the findings, citing such evidence as is necessary to support, confirm or reject the

1 Developments in model-building have gone hand-in-hand with advances in the application of mathematics to the discipline. A number of eminent practitioners, however, now seem to believe that the discipline is suffering from 'excessive mathematisation'. Gerard Debreu, one of the leading proponents of mathematics in economics, observed that despite warnings of the price economics paid for its increasing use of mathematics, economic theory had been carried still further away by a:

"...seemingly irresistible current that can be explained only partly by the intellectual successes of its mathematization.

Essential to an attempt at fuller explanation," he continued "are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those values do not play a silent role; they may play a decisive role. The very choice of questions to which he tries to find answers is influenced by his mathematical background. Thus, the danger is ever present that the part of economics will become secondary, if not marginal, in that judgement." (1991, p5)

hypotheses typically follows, highlighting any contributions to knowledge that the research may have generated.²

If the impressionistic view formed by a reading of the literature in the particular fields with which this study is concerned is any guide, then empirical examinations account for the bulk of the total research output in the area. Most projects seem to owe their form either to the testing of hypotheses by creating new data, say by means of surveys, or, as is more common, to analyses or re-analyses of existing published and unpublished data. Pure theoretical investigations certainly are tackled, but in this field, apparently only in a minority of cases.

One might gain the impression in looking at some of the undertakings, that their conductors have been able to proceed without let or hindrance from research question to conclusion. This must be illusory - it seems much likelier that in many cases, the history of the process of research with its false starts, blind alleys, errors and misperceptions, has been smoothed so that the reader does not have to undergo the full rigours of the journey of discovery, but can turn instead to the results and their theoretical underpinnings with the minimum of discomfort. In short, the orderly appearance of the typical research report probably disguises a process which is fundamentally messy by nature.

Certainly, the somewhat idealised account offered above bears little or no resemblance to the history of this project, and there must be many others like it. The closest it comes to the standard theory/hypothesis/test/results format is in the now-truncated part of the study concerned with the institutional representation of productivity statistics, where evidence of a regularity (bias) was expected, sought, and found. As will be seen below, the prediction that evidence of bias of a certain kind would be found results from theoretical orientation.

Of the general character of the part of this study concerned with the accuracy of the manufacturing sector output estimates it can only be said that it commenced with the chance observation of an irregularity, and blundered forward from there. The task was rendered difficult both by obstructive officials and by the fact that the discovery was made in a field where theory appears to offer but scant guidance and where empirical work is relatively scarce. Under such conditions, it is hardly surprising that the investigation has been

2 This mirrors accurately the form of the conventional journal article. A witty account of the reasons for this may be found in an essay by Lawrence Boland called "Current Views on Economic Positivism" (1991). Apropos of the issue of the obligatory 'contribution to knowledge', a study commissioned by the American Economic Association into the education and training of PhD students in the USA concluded that "...the quality of Ph.D. dissertations was not viewed as particularly impressive." Only 21 percent of faculty respondents could agree that most or all of the dissertations made significant contributions to knowledge. (Hansen, 1990, pp443-444)

characterised by much stumbling about in the dark - groping for insights. Afterwards, looking back on the enterprise, one apprehends that the 'scientific method' as conventionally understood, at least in the early stages of the project, has played but a small rôle in isolating that which requires explanation.

For one steeped in a conventional view of what 'proper' research conduct should be, the knowledge that some large chunk of one's project has taken a somewhat permissive form can give rise to some insecurity - especially when the research method used to carry out the study has quite an important influence on the final shape of the report in which the results are presented. It was a source of some relief,³ therefore, to come across a published account of a similar experience. The research in question is into witchcraft, and the researcher, Carlo Ginzburg, retracing the route which led to his findings, had this to say about method:

"The great French sinologist, Marcel Granet, once said that *'la méthode, c'est la voie après qu'on l'a parcourue'*, method is the road after one has travelled it. The word 'method' in fact derives from Greek, even if the etymology proposed by Granet - meta-odos, after the road - is entirely imaginary. But Granet's jocular remark had a serious - indeed a polemical - content: in any scientific realm discourse on method has value only if it is a reflection a posteriori on a piece of concrete research, not when it presents itself (and that is by far the most frequent case) as a series of a priori prescriptions. I hope that the account I am about to give of how my research was born and developed may provide confirmation, minimal and negligible in itself, of Granet's ironical assertion.

To tell the story of the itinerary of a piece of research when it has already reached its conclusion (even if it is a case, by definition, of a provisional conclusion) always - as is obvious - carries with it a risk: that of teleology. In retrospect the uncertainties and mistakes disappear or else are transformed into a stair that leads straight to the goal: the historian knows from the beginning what he wants, seeks it and in the end finds it. But in real research things do not go like that at all. The life of a laboratory, as historians like the Frenchman Bruno Latour have described it in recent years using an anthropological model, is much more confused and untidy." (1993, pp75-76)⁴

The broad research project of which this dissertation is one component has its origins, as noted in Chapter 1-1, in a request from worker leadership in unions affiliated to Fosatu for an investigation into what was regarded as an ill-informed campaign conducted by the National Productivity Institute (NPI). The ostensible aim of the campaign was to improve productivity, but workers suspected that this was at least partly a smokescreen for a more sinister move aimed at limiting wage increases to improvements in productivity, as calculated using some poorly-defined measures.

3 In retrospect, there is no need for insecurity - the ability to appeal to Feyerabend's anarchistic theory of knowledge (1979) provides more than enough protection against the criticism of methodological authoritarians. Incidentally, Feyerabend's conclusions, if not his flamboyant style, are ably defended in McCloskey (1983, p509).

4 On reflection, it is clearly is not necessary to be alarmed by the untidy nature of research activities - as Denton has observed:
 "Search, learning, trial and error, flashes of brilliance, and so on are the essence of processes by which progress is made in science, both by individuals and by scientific communities as a whole..." (1988, p178).

Early on in the prosecution of the original research work, it was observed that 'officially correct' national accounting procedures had the unusual characteristic of converting the massive current-price income bonanza that flowed from the gold price rises of the late 1970s into a constant-price GDP decline (Meth, 1983).⁵ The uncovering of this oddity stimulated an interest in the problems of official statistics more broadly considered. This has resulted in several publications, including a lengthy report on the poor quality of the estimates of the numbers of economically active (and unemployed) Africans in South Africa (Meth, 1988). It also led to an interest in the quality of manufacturing sector output estimates.

The observation that standard national accounting practices led, on occasion, to some peculiar results, naturally encouraged an examination of those practices. Strenuous official denials of the fact that something was amiss (NPI, 1983; Swanepoel and Van Dyk, 1983) served only to convert an ordinary sceptic into a vigilant scrutiniser of certain statistical series. This paid dividends after 1990, when the publication by the CSS of an extensively revised set of manufacturing sector employment estimates raised the expectation that revised output estimates would soon be published (Meth, 1990b). The failure of the anticipated output revisions to materialise prompted the detailed investigation (Meth, 1991d; 1992; 1993a) that forms the substance of much of the inquiry into the quality of the estimates.

Overly-enthusiastic defence by the NPI of its workmanship, and an uncritical attitude towards a set of output statistics against which simple common sense should have provided some *caveat*, coupled with strident and unrealistic claims to neutrality, were more than sufficient to arouse suspicions of special pleading. Warnings from the classic texts on the abuse of statistics, like Darrell Huff's question "Who Says So?" (1954, p111) and Moroney's "Statistics Undesirable" (1962, pp1-3) provided the grounding and precedent for believing that all was not what it seemed to be.

Ginzburg's remarks about the dangers of teleology are apposite - I sought error in the manufacturing sector output estimates, knowing that inevitably some error would be present, and suspecting that it could be very large. In addition to that, I sought and found, and have reported on elsewhere (Meth, 1991a; 1991b), instances of what I would regard as a persistent slant in the reporting of the NPI. The latter 'prediction' is hardly significant enough to merit such a grand description as teleology - its interest lies in the argument over whether what may

5 Having uncovered the valuation problem in gold mining in South Africa in the Fosatu Challenge (Meth, 1983), I presented an early draft of a paper spelling out the phenomenon I call the Perverse Deflator Effect to a meeting of the Durban branch of the Economic Society of South Africa in May 1986. This was followed by a long paper that has not yet been published (Meth, 1991b). Kantor commented on the problem in 1983, Kantor and Abedian (1990) looked at it again, and of course through Moll (1992), the question obtained international coverage in the literature. None of these papers makes reference to the Perverse Deflator Effect.

appear to the casual observer to be a set of reasonable analytical statements about the performance of the economy are, in fact, evidence of a particular form of ideological bias.

Were it not for the fact that serendipity rather than prescience was at work, the former 'prediction' would have been a more promising candidate against which to bring a charge of teleology. To be sure, errors were expected, but never was it thought that these would exceed the level one would normally anticipate when regard is had to the difficulty of measuring output. That the CSS should have made mistakes of the magnitude revealed in this study is, from my point of view, pure good fortune - obviously, considered from the standpoint of the CSS, the event is somewhat less of a cause for celebration.

The rôle (and nature?) of theory

It is under conditions such as those related above that the question of what rôle theory has played in the study must be considered. In order to answer this question adequately, it is necessary to say a few words on 'theory' so that an ordered way of considering the different types of "theoretical work" in which academics engage may be constructed. The classification offered below was suggested by Leibenstein (1976), who noted that in the area, there is no "standardised vocabulary". First, he distinguishes "analytical work" from "theory proper", the latter having reference to:

"...a set of relations that are sufficiently specified so that some conceivably falsifiable conclusions can be reached. At least some of the conclusions resulting from the theory are, in principle, in a form that makes it possible for facts not in conformity with the theory to occur. Such a theory says something about the world of facts."

An analytical framework, by contrast, consists of:

"...a set of relationships that do not lead to specific conclusions about the world of events. In an analytical framework, the parameters are not sufficiently specified to lead to conceivable falsifiable conclusions." (1976, p17)

Often, it may not be possible to determine whether one has a theory or a framework. To illustrate the difference between the two Leibenstein suggests that (undergraduate?) economics text books provide frameworks rather than theories. Frameworks can be:

"...looked upon as toolboxes from which we can fashion theories to explain events, but they are not themselves such theories." (1976, p18)

A further distinction can be drawn between theory and model. Here, he notes, the problems are even greater, because there is no standardised usage. Used interchangeably at times,

models, he argues, appear at times not be distinguishable from analytical frameworks as defined above. Leibenstein proposes a concept of 'model' which would see it as "...a less rich form of theory." Viewed this way, a model:

"...allows for a smaller range of possibilities and considers a narrower range of phenomena than the theory, but it reaches some of the same *qualitative* conclusions. As a consequence, models are especially useful for didactic and illustrative purposes. Another version of a model would be to have the same set of relationships as the theory, but with the parameters restricted to a much greater degree to bring out some of the conclusions more clearly. Thus a theory may be said to have a large variety of models consistent with it." (1976, p18) (Emphasis in original)

Leibenstein's basic premise is that:

"...explanatory power is a basis for diagnosis, and that diagnosis should precede prescription. Policies cannot be determined once and for all in such a way as to set the system right forever after. As a consequence, there is a need for continuous diagnosis. Therefore, a theory that in *some way* (and to some degree) provides an organized set of diagnostic tools is desirable." (1976, pp21-22)

Frameworks are argued to have to be capable of providing moulds for different kind of models, and the models themselves contain "sample propositions". Sample propositions specify:

"...relationships that in themselves are not necessarily true. They are samples in the sense that they suggest the form the theory should take." (p22)

Arguing that "...sharp prediction employed as a rejection rule is an inappropriate criterion..." Leibenstein deploys the 'sample relation' concept as a 'soft prediction' tool. This retains some "...degree of prediction as a quality...", ie, it is "frequently" capable of providing useful bits of information, even though it "...does not pass the prediction test employed as a rejection rule." (p22). To illustrate, he draws on an example from the medical field - knowledge of the ability of aspirin to "cure" headaches (more precisely, to provide symptomatic relief). Like good economic theory, such knowledge provides useful information, and it is not rejected because some headaches are not 'cured' by aspirin. It "lacks precision but is nonetheless highly useful." Neither "headache", nor "frequently", nor indeed "cure" is precisely defined, yet as he points out, "...if this were all we knew about aspirins and headaches, it would certainly be useful." Moving from the general to the particular, we can test this 'sample relation' against specific individuals. The effect will vary between individuals, but the frequency with which the relation is confirmed (despite occasional failures) demonstrates the usefulness of the relation, and the fact "...that it contained some degree of predictability." (1976, p23)

Similar examples may be drawn from other aspects of our common-sense experience - in order to drive a motor car it is not necessary to know the precise relationship between the amount of

force required to be exerted on the accelerator pedal and the road speed which this produces. It is sufficient merely to know that increasing force on the accelerator increases velocity. The fact that the full relationship is probably too complex to specify with precision does not prevent anyone from driving. The knowledge is a useful sample relation of the type of which there are many in the field of economics. Monotonically increasing functions which cannot be specified with precision are a good example.

Applications of the approach spelled out above will be found in both parts of the study, but it is of particular importance in the first part. Thinking about theory in a systematic way (as opposed to thinking up theories) seems not to be something that economists do very often. Nor would it seem that it is generally necessary to do so. The departure of this study from what may be considered to be the norm in the use of 'theory' has, however, raised a number of questions to do with the nature of theoretical work. These are not easily answered without some clarity about the way in which the different 'levels' of theorising outlined above relate to each other. Although Leibenstein's formulation may not be wholly satisfactory (for sheer difficulty of attaining clarity in this matter), it does provide a starting point. Thus the schema above, which one could summarise as saying that models nestle within theories which in turn nestle within frameworks, will be used when an attempt is made to peer into the chasm between competing paradigms (analytical frameworks). For the most part, it is not necessary to be pedantic about the matter - the terms will be employed in the unself-conscious manner common within the discipline.

Returning now to the question of the rôle played by theory in the two parts of the study, it may be seen that in the part of it directly concerned with the accuracy of manufacturing statistics, this differed from that which it has played in framing the questions which prompted the investigations into the (mis)use of productivity statistics in the first instance. In the case of the former, the errors were exposed because of an interplay between my theoretical awareness and technical ignorance in response to a CSS announcement that an error had been made in the employment estimates. The theoretical part is the expectation of a monotonically increasing relationship between levels of employment and of output (at least as a first approximation) shared by all economists. An error in the estimation of one signalled the possibility (on mistaken grounds, as it turns out) of an error in the other. This led to the prediction that revised output estimates would soon follow. When none were forthcoming, the investigation commenced (Meth, 1991d). The technical aspect to this was that I possessed some (minimal) knowledge about the way in which output and employment statistics are produced. This knowledge was not comprehensive enough for me to be aware of the fact that the surveys that

measured output and employment respectively were entirely separate.⁶ In other words, there was no logical reason why, if the one was wrong, the other should be so as well. Ironically, therefore, the errors were uncovered because of my ignorance of the procedures by which official statistics are produced.

Reflecting on the relationship between theory and empirical work conducted at this fairly low level, one begins to understand that in circumstances such as those described above, one's training as an economist conditions one to respond in such a reflex manner that it almost seems as though theory played no part in the process. It cannot however, be argued with any conviction that the errors were simply 'social facts', lying around as it were, waiting to be uncovered with no aid from theory.⁷ Measurement without theory has a bad name, and for the most part, deservedly so. Having said all this, one is bound to muse on the possibility that someone better informed may have missed these errors, by virtue of their greater competence. The necessary conditions for the discoveries to be made, it would seem, was the appropriate combination of theoretical awareness, technical ignorance, and curiosity. Theoretical orientation (in the sense of commitment to a particular analytical framework - say Marxist or neoclassical) does not appear to have been of any consequence at that point in the proceedings.

It is one thing to use limited theoretical and technical knowledge to expose errors - it is another matter altogether to explain, if possible, why the errors have been made. Apart from access to the officials producing the statistics, theory is required for this purpose. Theory will thus not be wheeled on as an *ex-post facto* rationalisation of discoveries that took place largely without its conscious aid, but will instead be used in an attempt to penetrate the veil behind which the social process of estimating national output statistics takes place.

As far as the portion of the study concerned with the NPI's use of productivity statistics is concerned, a somewhat different process took place. It is not immediately obvious to someone who does not share my political viewpoint that the NPI's interpretation of the economy's performance is unwarranted - an uncritical acceptance of the output of the national accounting statisticians would indeed lead to the conclusion that much of what the NPI says is correct. Exposing the NPI's output as biased requires explication of the analytical framework one of whose reflex suspicions (hypotheses) it is that institutions such as the NPI are likely to engage in special pleading on behalf of the dominant groups in society.

6 As the inquiry proceeded, a phase of 'learning-by-doing' commenced, in which some of the practical techniques used to prepare national accounting output and income estimates were slowly mastered.

7 The problems of induction are so well known that they hardly need to be repeated here. O'Brien (1991, pp50ff) treats the matter very well. It is unlikely that many economists are unaware of the 'constructed' nature of economic 'facts'. They may not all be equally sensitive to the fact that "...government statistics..." rarely "...correspond to economic concepts..." (O'Brien, 1991, p57).

Two distinct and quite different analytical problems have therefore had to be confronted in this study. In the part concerned with statistics, the problem is not so much what theory to choose, for there is little scope for choice, but rather, how to proceed in practical terms when one or another of the well-known problems in the valuation of output and income is encountered. In the part concerned with the poverty of the productivity statistics and the NPI's bias, the truncating of the study has affected the content of the analytical material. Instead of attempting to develop and present a full-blown theory of institutional and individual behaviour in capitalist economies, the task has become one of skimming over the hypotheses drawn from Marxist political economy that prompted the examination of the NPI in the first instance, and sustained it throughout all the years when the workmanship of that institution was subjected to continuing scrutiny. The following chapter supplements this somewhat meagre offering by suggesting why the exclusive use of that particular approach might be appropriate.

Like the background question about the quality and use of productivity statistics that informs the larger project, the commitment to a Marxist approach has influenced the research in a number of ways. It goes without saying that in matters such as disputes over wages and productivity, Marxists would find themselves particularly at home - considering them to be an aspect of class struggle writ large. It is not immediately obvious that it is appropriate to refer to Marxist theory and national accounting practice in more or less the same breath. Yet a moments' digging is all that is necessary to uncover the fact that the national accounting systems in capitalist and (the now largely failed) socialist economies differ substantially. This should serve as a pointer to the fact that statistics (especially economic statistics) are social constructs, not natural phenomena. As such, the social forces which influence their make-up are *par excellence* the sort of phenomena with which Marxist analysts are wont to engage. There is, however, a more direct and important sense in which the political economy approach used to cast light on the activities of the NPI informs and dominates the technical analysis of the manufacturing sector statistics as well, and that is in its concern with value theory. The Marxist preoccupation with value theory as a vehicle for exposing class exploitation, is well-known.⁸ The inability of the CSS to estimate output levels correctly may be traced directly to one of the central failings in all theoretical approaches to economics, namely the absence of an adequate value theory.⁹

In short, the proposition that official statistical production is an inescapably political exercise can come as a surprise only to those who are unfamiliar with the history of national

8 Some of the problems of value theory are considered in Chapter 2-1.

9 Marxist value theory does not pretend to be able to solve many of the problems of the price/quantity domain - that is not its purpose. This being so, Marxist value theory is an inadequate tool for tackling the sort of problem posed in Part II of this study.

accounting. Lintott (1982) refers to the fact that the first national income estimates - William Petty's *Political Arithmetick* (1690) - were made within a context of mercantile capitalism in which:

"...the role of the State was limited to its monopoly of the means of violence, and in particular on waging war on other States and raising the taxes to pay for this. Not surprisingly therefore, 'how to pay for the war' was the starting point for these estimates no less than for modern national accounting." (1982, p49)

Although there are democratic forces which urge transparency and the untrammelled dissemination of information about the economy, the periodic crises which strike all economies often call forth attempts by anxious politicians to control information flows. Strong constitutional measures are required to protect chief statisticians against political manipulation.¹⁰ Extreme conditions in South Africa have produced numerous examples of the imposition of political control - the antics of the National Party government in preventing rational debate on sanctions are perhaps less widely known than they should be - the security precautions surrounding oil and military procurement are not. Nowadays, with the routine production of national accounts in accordance with the SNA (UN, 1968; 1993), interference in the production of statistics is less simple. It was not always thus - Lintott points out that in Britain, the government, having had national income estimates prepared by Inland Revenue as early as 1929, only published its own income estimates from 1941. The Inland Revenue estimates were kept confidential until 1977, on the strength of an official dictum that:

"...no figure should be published which could not be sustained as exactly correct in a court of law." (1982, p63)

Such a stance looks a little silly today, but the significance of the Inland Revenue figures seems to have lain less in their want of exactitude than in the fact that they were lower than other estimates made at the time. This was partly because of a lower estimate of the national wage bill - a finding whose ideological importance, at a time when debates over the distribution of income were no less heated than they are today, was not lost on the government of the day.

To end this discussion, and to illustrate the fact that theory is not 'innocent' (to use Althusser's term), let us drag a skeleton out of the statistics cupboard. This will show that the choice of analytical framework has an important influence on the conceptual tools developed.¹¹ Anyone with the most casual acquaintance with statistical analysis will have come across the term 'regression'. If they are curious, they may have wondered about its origins. Standard

10 Some of these tensions are explored at greater length in Chapter 2-1.

11 To anticipate the line of argument developed in the next chapter, however, the story related here also shows that these tools are often capable of being put to other, more general uses.

econometrics texts often provide a (partial) answer to this question. Gujerati, for example, explains that it was introduced by Francis Galton to describe the tendency for heights of children, both of short and of tall parents, to move or 'regress' towards the population average. Gujerati notes that for Galton, this was a "regression to mediocrity" (1988, pp13-14). The digression is short - barely 13 lines of text - the fascinating hint of what could have been in Galton's mind is left unexplored, and Gujerati moves swiftly to the quite different 'modern' interpretation of regression.

The full story is much more interesting (and sordid). Towards the end of the 19th century, some social scientists, alarmed at the potential damage to society that would result from unrestricted breeding amongst the 'residuum' in society - the chronic poor, the unemployed, the criminal, the mentally defective, the alcoholic - began developing theoretical tools which would enable them to place on a scientific footing, the understanding of this underclass. A passionate concern with racial purity had, as its central policy prescriptions, the means to prevent members of the residuum from breeding. The prescriptions which united biology, statistics and medicine emerged from a 'science' called eugenics, invented by Galton. His research in this field led to two key innovations, regression and correlation.

These were developed into systematic concepts by Pearson, who generalised the theory to take account of any number of variables. In 1911, Pearson founded the Department of Applied Statistics in University College, London - his chair having been established after a fund-raising drive as a chair of Eugenics. Pearson had many students who were later to become important statisticians, and he edited *Biometrika*, a leading journal of statistical theory for 35 years. According to one account:

"This work was motivated by a set of political and intellectual beliefs which he had held prior to becoming a statistician (and, indeed, which largely led to him becoming one)." (MacKenzie, 1979, p42)¹²

The important question is, of course, whether these discoveries would have been made independently of the motivations of these two theorists? It has been argued that Galton's concepts were a "...revolutionary breakthrough ...from the approach of [other workers] interested in the treatment of observational errors." Despite similar mathematical formalisms, the interpretations of errors were quite different - for them, statistical error was to be eliminated:

"For Galton, it was the source of racial progress... Galton, because he was a eugenicist, worked in a different framework of meanings and assumptions from his predecessors: he developed the theory of statistics in ways that would have seemed pointless to them." (MacKenzie, 1979, p44)

12 Eugenics also played an important part in the early work of R A Fisher, of analysis of variance fame. (MacKenzie, 1979, p42)

Of course, not all statisticians subscribed to these quaint beliefs - it is precisely the usefulness of the technique that enables Gujerati and others to gloss over its inauspicious beginnings with such ease. For some purposes, however, it is important to bear these origins in mind - the general lesson is that:

"...it is mistaken to see statistical theory as a field of knowledge developing simply by its own internal logic and giving rise to necessarily value-free techniques. Rather, statistical theory has evolved in historical interaction with conceptual change in other sciences, with the needs of production, and with theological, political and ideological developments. It is a social, historical and ideological product and not merely a collection of neutral techniques." (MacKenzie, 1979, p48)

The same applies, *a fortiori*, to economic theory. Different forms of economic crisis cause the spotlight to fall on different statistics - unemployment, capital flight, consumer price indices - but certain critical indicators like the wage/productivity relation are always sensitive. Even when sustained growth in capitalist economies enables competing claims to be met with relative ease, struggles over wages do not disappear. Under conditions of slow growth or decline, such as have been experienced in South Africa, claims by militant workers for an increasing share of the value added in production are bound to occasion vigorous debate.

Despite protestations that the contrary is true, the step for economists from the analytical to the prescriptive is often but a short one - and one which NPI economists have not hesitated to take. The empirical basis on which this has been done is slender - the theoretical foundation even slenderer. To challenge what has become the dominant view on the wage/productivity relation, a sharpening of the tools of critical analysis has been necessary. Coming to grips with the complexities of national accounting is one aspect of this process. As often happens when aspects of worker struggles find their way into the hands of 'experts' like lawyers, accountants or economists, matters become bogged down in the sheer technicalities of the enterprise.¹³ Thus, what started out as a relatively simple, albeit necessary detour through national accounting procedures on the road to a full engagement with the NPI, has come to take up

13 Trade unionists display a quite sensible impatience with the endless wrangling into which the surrender of arguments into the hands of experts degenerates. Not only does frequent recourse to experts (lawyers and economists in particular) prolong disputes, it is also disempowering, in that it takes matters out of worker's hands, handing them to technicians to haggle over. Expert assistance may be unavoidable in many cases, especially in matters involving litigation, but the preferred approach by unions in the Cosatu federation to issues such as those discussed in this study, is not to engage institutions like the NPI on the terrain staked out by that institution. Instead, they have concentrated on building organisational strength so that the resolution of matters like wage negotiations turn on an awareness of relative power. Ideology, is, however, recognised as important, and it is recognised as well that the endless assertions that productivity is poor need to be countered, if the legitimacy of wage and other claims is not to be compromised by sheer attrition (Gramsci's war of position). Cosatu's response to this has been to broaden its own research capacity, both in-house, through a research organisation called the National Labour and Development Institute (Naledi) and through association with sympathetic intellectuals in enterprises like the Industrial Strategy Project (ISP). NUMSA, the National Union of Metalworkers, has been in the forefront of attempts to understand the complex relations between productivity, wages and education and training. In this they have drawn quite heavily on international, particularly Australian experience.

most of the space in the study. The next section of this chapter is an introduction to the problems of output valuation.

Selecting an appropriate theoretical framework - (i) output valuation

Most economists will be aware, if only dimly, that there is a debate in economics about the 'index number problem'. It is the difficulties of index number construction, of one form or another, that bedevil CSS attempts to estimate manufacturing sector output levels. The history of the index number problem stretches back into the last century, but by the 1920s, many of the desired properties of indices had been explored and specified. The premier work is that of Irving Fisher (1922), but the decade saw a flurry of contributions from eminent economists keen to clarify the numerous puzzles that remained, especially in the construction of actual as opposed to theoretical measures. Not the least important reason for this concentration of intellectual energy on measurement and calculation was because of lively debate at the time around proposals about how "...to regulate the value of money by an official index number..." (Edgeworth, 1925, p387).¹⁴ Edgeworth's definition of an index "...a number adapted by its variations to indicate the increase or decrease of a magnitude not susceptible of accurate measurement..." (p379) captures the essence of it - the output of the manufacturing sector, for example, expressed as an index number, is an artifice - it refers to a 'bundle' of ever-changing commodities that have somehow to be added up. As early as 1928, Bowley had provided a comprehensive analysis of the problems to be encountered in index number construction (pp232-235). He also suggested that in arguments about index numbers of (industrial) production, one could not expect much guidance from theory (p231).¹⁵

Plus ça change, plus ces't...

By the time the first edition of the United Nations guide to the construction of national accounts, the *System of National Accounts* (SNA) came to be written (1953), many, if not most of the problems of valuation of output that trouble us today had been identified (but not solved!). A debate which touched on the central issues to be covered in the first part of the

14 With the rapid rise in union density in Britain after World War I, accurate measurement of the general price level became increasingly important. Grier notes that "...cost of living standards as a basis not for wages but for fluctuations in nominal wages are commonly enforced by Trade Union action." (1925, p532)

15 The earliest index of industrial output in Britain was prepared privately. Expressing dissatisfaction with this state of affairs, J W F Rowe, writing in 1927 on the difficulties of compiling such an index, stated that "...no one will be more pleased when this task is taken over by the State with its more ample resources." (p187). See Stolper and Doblin (1941) for a discussion of the 'new' Federal Reserve Board index of production in the USA, and a criticism of the 'old' index which it replaced.

study took place in the 1950s between Maddison (presumably the same Maddison who was still writing about productivity in the late 1980s), Pentland and Lerdaу. It begins with an article by Maddison which contained an examination of the contribution of productivity growth to rapid GNP growth in Canada in the period 1931-49. In it, the following statement appeared:

"There is an official index of productivity for the manufacturing sector, which has as its numerator a deflated index of the gross value of manufacturing output. This is not a suitable concept of output from the standpoint of the present analysis because the net value content of gross value can fluctuate widely from year to year."

At that point in the text there was a footnote which reads as follows:

"Cf. *Index Numbers of Industrial Production, Studies in Methods*, No. I (Statistical Office of the United Nations, New York, September 1950), p.9. It is suggested here that the most useful concept of productive activity for the purpose of an index of production is that which describes the industry's contribution to G.N.P. This is equivalent to the concept of value added used by the census of manufactures." (1952, pp589-590).

Maddison used the official index anyway, but backed up his results with independent estimates of his own which suggested that at least the orders of magnitude were similar. In 1954, Lerdaу examined productivity in the New Zealand economy, and took the opportunity to point out that:

"Maddison deflates GNP for different years by the cost of living index and the wholesale price index, divides the result by the civilian labour force, adjusts for changes in working hours and, in the form of index numbers, purports to show changes in *physical* productivity. This method would be roughly adequate for a closed economy, but introduces a bias from changes in *value* productivity by disregarding the "open ends", *i.e.*, changes in the terms of trade. The difference is conceptual, and corresponds to the Pigovian distinction of income produced and income available. It is apparent that, when the price of exports rises by more than the domestic price level, money GNP is not deflated sufficiently by the domestic price indices to give a constant real GNP, wherefore an improvement in the terms of trade, though unaccompanied by any real change in physical output per man hour will give the appearance of such a change on Maddison's index." (1954, pp184-185)

Weighing in with further criticism of Maddison's approach, Pentland commented that the small increases in productivity reported "...owe more to the nature of the statistics than to actual experience." Pentland then referred to the work of another contributor to the debate on productivity in Canada, stating that:

"Mr Sutton's computation of productivity in manufacturing from the net value of manufactured output, deflated by the wholesale price index of manufactured goods, is still more unsatisfactory. The wholesale price index of manufactured goods is essentially a gross concept, heavily affected as it is by changes in raw material prices. It is not, therefore, an appropriate deflator for a net value which should not be affected directly by raw material price changes." (1954, p400)

The intricacies of Maddison's response to these criticisms and Lerdaу's (1956) rejoinder to Maddison do not concern us here. What is of interest, however, is the reference by Maddison

to a "Revised Index of Industrial Production 1935-51", produced by the Dominion Bureau of Statistics (later Statistics Canada) which appeared after his original article was written. The form of the index used (for individual industries) was given as:

$$(\Sigma Q_1 P_0 - \Sigma q_1 p_0) / (\Sigma Q_0 P_0 - \Sigma q_0 p_0)$$

where QP refers to the gross value of output, and qp to the value of materials, electricity and fuel inputs. The subscripts 0 and 1 refer to a base year and any subsequent year. This is the standard Laspeyres double-deflated output index.¹⁶ In a footnote, Maddison observes that:

"This is the only official industrial production index, apart from that of Ireland, in which not only the weights but also the indicators for the individual industries are made up of net output indicators." (1955, p716n)

As will be seen below in this study, difficulties are still experienced in the estimation of the volume of output - deflated gross output (and possibly even worse, value of sales) is (incorrectly) used as a proxy for this, and the warning about changing net to gross output ratios in the South African economy has been ignored - at considerable cost. The manufacturing statistics are not accurate enough to permit double deflation, and, as I have shown elsewhere problems are still experienced in attempting to estimate the impact of changes in the terms of trade.¹⁷

Although output indices can be estimated in abstraction from productivity measures, the debate in recent times has tended increasingly to an inclusive view of input, output and productivity. Even so, theoretical questions relating to output and productivity measures can be separated. The brief survey in this chapter gives a flavour of the debate and a pointer to current thinking in the area of index number construction. The matter is treated at greater length in the relevant areas of Part II. On the question of the index number problem in manufacturing, the literature review gives some indication of how far common practice is from leading-edge thinking. This review should serve as a warning against the uncritical use of output indices and a reminder that certain problems are, and will remain insoluble.

The modest ambition of the part of the study concerned with manufacturing output estimates is to effect an improvement in the existing figures, sticking to the existing (Laspeyres) index

16 Laspeyres indices are defined in Footnote No. 10 of Chapter 2-2 (p206), and discussed at some length in Appendix 2-3 below. Double deflation is covered in Chapter 2-6.

17 What I call the Perverse Deflator Effect (PDE) is discussed in Appendix 1 below. The PDE is a problem that affects estimates of Real Gross National Income when changes in the terms of trade of more than a certain size occur. In the South African case, it is caused by rapid changes in the price of gold. The first public airing of this phenomenon was at a meeting of the Natal branch of the Economic Society of South Africa in May 1986. It was later written up into an unpublished paper (Meth, 1991b).

formulation. It will be noted in the study that the CSS apparently sets great store by the fact that, in accordance with accepted international practice, it produces Laspeyres output indices, presumably using Paasche price indices wherever possible. But of these two old warhorses, Hansen and Lucas state flatly that:

"...based on their rather implausible economic assumptions, it is no longer justifiable to use the Paasche or Laspeyres formulae, except possibly in situations where a subjective¹⁸ rather than an economic result is desired." (1984, p33)¹⁹

Advances in the understanding of the relations between indices of different types and their implicit or explicit economic properties have now reached the point where indices can be constructed that are robust over a range of plausible economic assumptions. As Caves *et al* point out:

"Rather than starting the selection process with a number of plausible index number formulas, one can specify an aggregator function²⁰ with desirable properties and derive the corresponding index number procedures." (1982a, p73)

Debates about the choice of index are more than mere hairsplitting, especially where long time series (like the South African national accounts) are concerned. In an empirical examination of Egyptian trade data, Hansen and Lucas show that indices of volumes and prices of imports and exports, as well as estimates of the terms of trade of the Sato-Vartia, Fisher and Törnqvist type differ by negligible amounts over the period 1885-1961. Laspeyres-Paasche by contrast had the volume indices 'incorrect' by nearly 90 percent and the price indices by about 55 percent and the terms of trade by 30 percent (1984, p33).

Despite the apparent discrediting of Paasche and Laspeyres indices, the latest version of the *System of National Accounts* (SNA) (UN, 1993) still makes extensive use of them, so it seems not inappropriate to pursue the modest goal spelled out above. In any event, the errors disclosed by the manufacturing sector study (Part II) would have defeated the other index formulae as well. The possible improvements that the use of indices such as the Törnqvist can deliver may be speculated upon, but the aim here is to attempt to correct misapprehensions in existing procedures, and to suggest ways in which these may be avoided in the future.

In order to achieve this, two areas require investigation - the first of them being the practical aspects of index number construction, and the second the complex and unresolved problem

18 For a discussion of what Hansen and Lucas mean by subjective, see p30.

19 The discussion here on the poverty of Laspeyres and Paasche indices is salutary - I have never come across any official or unofficial publication which provides a warning of this sort.

20 An aggregator function is identical to a production or utility surface. See Hansen and Lucas (1984, p30).

referred to in the historical survey above, namely the deflation of net output. Selection of a theoretical framework may thus be seen to be a tightly circumscribed affair. Most of the discussion relating to the problems of valuation dealt with is so dry and abstract that it is often difficult to recall that it relates to human productive activity. This might give rise to a temptation to regard the enterprise as a purely technical matter. It is necessary to bear in mind that this narrowing of outlook is merely a means to an end - as is the production of output estimates themselves. Fundamentally important questions about the purposes for which these figures are produced remain to be addressed.

Selecting an appropriate theoretical framework - (ii) productivity and class struggle

It may be recalled from Chapter 1-1 that the goal of developing an adequate analytical framework with which to analyse the NPI, placed alongside of all the other aims of the study, was recognised as being overly ambitious. The task of this section of the chapter is to present the beginnings of such a framework, pointing to where the major difficulties lie. In the previous chapter, the irreconcilability of the rôles of statistics production and advocacy was noted. Since so few people have been involved in the NPI's productivity improvement propaganda campaign, one could, of course, simply argue that the organisation attracted a handful of zealots whose enthusiasm for productivity improvement overwhelmed their commitment to sound economic research practice. This may be partly true, but its inadequacy as an explanation of the function of the NPI can be gauged by conducting a quick thought experiment. Imagine that instead of zealots, the NPI had hired several cautious academics who issued a constant stream of press statements about productivity similar to those that could be extracted from a study like this one. Interest of the media in repeated warnings about the fragility of some of the inferences drawn from South African statistics is unlikely to have been sustained - and the NPI has relied heavily on the media to discharge its primary duty. Given the NPI's mission to improve productivity and to arouse as much general interest in the topic as possible, it is clear that 'excessive' even-handedness and academic caution would not have been desirable characteristics in those responsible for creating and disseminating the NPI's publicity material. Theory is essential to an understanding of the constraints that make it likely that those located within an institution such as the NPI will behave in the appropriate manner, ie, that reported in the previous chapter.

A Marxist analytical framework is the chosen vehicle for this job, yet Marxist studies in South Africa do not provide a properly consolidated foundation on which to build.²¹ In a scathing critique of sundry efforts to develop a "neo-Marxist"²² analysis of the South African state, Yudelman (1984, p32) refers to their origins in the work of Poulantzas - work that according to Yudelman, is polluted by the proposition that the state always, even if only ultimately, protects the interests of the bourgeoisie. Poulantzas' theory, he says, is either "trivial or tautological". An earlier voicing of similar sentiments (before Poulantzas' time) had caused Schumpeter to observe that:

"...there is no policy short of exterminating the bourgeoisie that could not be held to serve some economic or extra-economic, short-run or long-run bourgeois interest, at least in the sense that it wards off still worse things." (Cited in Yudelman, 1984, p32)

Little of what had been written by Marxists up until the time Yudelman had completed his book escaped condemnation.²³ In defence of Marxist scholarship though, it should be pointed out both that the number of people active in this area was very small (never much more than dozen or so at any given moment), and that the theoretical resources they drew on represented only a fraction of those considered in the seminal review of Marxist theories of the state by Jessop (1983). The result is that the project of constructing a Marxist understanding of the

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- 21 The reason for this is not so much the absence of scholarly work originating on the left, but rather that the extreme difficulties of threading one's way through the analytical and political minefield of Marxist approaches to apartheid and capitalism. A hostile review by Yudelman (1977) of Frederick Johnstone's *Class, race and gold: A study of class relations and racial discrimination in South Africa* (1976), acknowledges the power of the left's "...incisive if overstated critique of the previous literature..." but points out that the book was a disappointment to those seeking "...alternative explanations of the complex mesh of industrialization, race and class." (1977, p205). For a further taste of these complexities, see Fine's (1990) review of Wolpe (1988). See also Fine and Davis (1990) for extended discussion of some of the arguments presented in Fine (1990). It is salutary that someone as eminent and experienced as Wolpe (one of the original contributors to the Marxist/liberal debate in South Africa in the late 1960s and early 70s), can go as far astray as Fine suggests he has. Time has certainly vindicated many of Fine's criticisms of Wolpe - see especially p103.
- 22 As a matter of taste, I dislike the use of the 'neo' as a prefix to the word 'Marxist'. In my view, it contributes nothing to an understanding of what it is that those of us who are committed to using what we understand to be a Marxist methodology actually do. If it is meant to suggest a need to update Marx's '19th century' views then it is unnecessary, because with the possible exception of cranks who may believe that all of the (contradictory) things that Marx said were correct, nobody would think otherwise. The term 'neoclassical', by contrast, despite the fact that it is used rather loosely, may possibly have acquired a fairly generally accepted meaning, namely, a form of analysis of the workings of the market, stripped, paradoxically, of the political economy in which its founding father, the classical economist Adam Smith, originally embedded it.
- 23 Before conventional economists succumb to the temptation to smile at this, two intellectual hurdles have to be surmounted. The first of these is a need for liberal scholars to provide a satisfactory account of the apartheid state. Yudelman reserves as much scorn for efforts in this regard as he does for the Marxists he criticises so energetically. In the second place, a moment's reflection on the (largely absent) theories of the state within their own brand of theorising is necessary (Shapiro and Taylor, 1990). When conventional economics finally acquired a theory of the state, it was of the fairly right-wing type - public choice theory - associated with Buchanan and Tullock (the Virginia School). The potentially useful insights of this approach, based on the problem of rent-seeking behaviour, are often vitiated by a reflex anti-statism whose lack of balance has attracted critical attention in a number of quarters, especially in more thoughtful texts on public finance such as that by Cullis and Jones (1992). The limits of Buchanan's contribution to an understanding of one of the most difficult questions in this field, the expansion of the public sector in the present century, are explored in Sandmo (1990, pp60ff).

apartheid state (and before that, the segregationist state) is far from complete. In short, there is much to be done on the theoretical front.²⁴

Some potentially profitable lines of inquiry have been opened up by Yudelman (1984) as well as by others like Greenberg (1980). In these works, the tendency to indulge in extreme abstract formulations into which empirical reality is forced, is disciplined instead by that reality. Both authors are sufficiently aware of the exploitative nature of capitalism, and critical enough of accounts of South African economic history which give undue weight to racism, to satisfy all but the most doctrinaire Marxist. Neither, unfortunately, address the issues with which this study is concerned. What follows is, of necessity, sketchy - it cannot do justice to the question.

Towards a (sort-of) Marxist analysis

In the early 1980s, as the buildup to the insurrectionary phase in the events which led to South Africa's recent democratic transition gained momentum, Fosatu, the strongly left-inclined Federation of South African Trade Unions, began the arduous task of creating a working class movement capable of challenging the combined might of the apartheid state and a capitalist class only weakly opposed to that régime (Foster, 1982; Baskin, 1991). As part of that struggle, intellectuals sympathetic to the movement were drawn increasingly into the rôle of producing analyses of developments in the economy that could be used in educational programmes for worker leadership.²⁵

It was in that climate, and in response to a request from regional Fosatu leadership, that I wrote a biting polemic attack on the NPI, accusing that organisation of, amongst other things, (ab)using poor statistics to further the dubious claim that worker productivity in South Africa was poor and that worker wage demands were unreasonable (Meth, 1983). Published as a Fosatu Occasional Paper, the title - "A Challenge from FOSATU on Productivity: to whom

24 Although Yudelman is dismissive of the stalwarts of pioneering South African Marxist writers like Davies, Kaplan, Morris and O'Meara (Yudelman, 1984, p47n) that does not mean that everything these writers have produced needs to be rejected. Davies' somewhat turgid *Capital, State and White Labour in South Africa 1900-1960*, (1979), for example, whatever one thinks of its theoretical basis, contains much of interest. Other contributions of note from Marxist economists are the 1981 and 1986 pieces by Saul and Gelb, and Gelb's essay in the book he edited on South Africa's economic crisis (1991). In the work which he did with Saul, they adopted a Gramscian approach to the notion of crisis. In his later work, Gelb has used a regulation theory approach, influenced by the work of the American regulation school (Bowles; Gordon and Weisskopf) as well as the French school (de Vroey).

25 Many of these were published in journals such as the *South African Labour Bulletin*, or in the more accessible *Work in Progress*. Political analyses tended to be generated internally. A vigorous left-wing representation within the union movement had to deal with some extremely sensitive questions. This took place in a climate of unrelentingly harsh repression at home and, in the early days, of some hostility abroad from exiled trade union leaders. See Foster (1982).

it may concern, your figures are wrong and so are your conclusions"²⁶ - was openly combative. In part, it had the desired effect,²⁷ sparking widespread discussion. Predictably, it also aroused a certain hostility. The *Financial Mail*, which gave the article generous coverage, quoted an 'economist' as saying that: "[T]he loose and vitriolic style in which he writes dismisses his paper as a serious academic endeavour" (April 29, 1983, p519). Given the ideological climate into which it was launched, the style probably did serve to alienate many potential readers and to distract attention from the serious points raised in the paper. It is also true, however, that the *Financial Mail*, which is not renowned for its espousal of left-wing causes, treated the charges made with the seriousness one would expect from an intelligent audience.

The 'vitriol' to which reference was made consisted of a set of statements made in language of the type routinely used by Marx in such works as *Capital, Volume I* to describe the relationship between workers and the capitalist class, as well as to express his boundless contempt for intellectuals who were either uncritical of capitalist social relations, or worse still, were active apologists for the capitalist class. Although it may have been somewhat intemperate, this form of address was certainly not unknown amongst Marxist analysts writing in the 1970s and early 80s. The point is not whether some people considered the language inflammatory (they undoubtedly did),²⁸ but rather, that the language signalled the fact that an explicitly Marxist analysis was being used to generate certain findings.

Two influences were at work - one of them the classical Marxist critique of the capitalist mode of production, and the other, the Marxist literature on theories of the capitalist state. The latter is huge²⁹ and growing.³⁰ Three seminal pieces, by Althusser (1971), Therborn (1980) and Cohen (1983) influenced my thinking at the time (and, to an important extent, continue to do so today). Althusser's essay "Ideology and Ideological State Apparatuses" (1971) introduced

26 As noted in Chapter 1-1, it is referred to in this study as the 'Fosatu Challenge'.

27 Workers in Cosatu affiliates have refused almost consistently to take part in 'productivity bargaining'. Of the very few exceptions to this, the most prominent example, the agreement between Gengold and the National Union of Mineworkers (NUM) in 1991, soon collapsed (Nattrass, 1992, p31). Part of worker hostility appears to have been based on a profound distrust of management-produced figures. Current attempts to construct a social democratic compact may see greater success in this field, but much remains to be done.

28 I recall thinking at the time that the piece would probably attract the unwelcome attention of the security police, whose 'labour relations experts' were actively engaged (and remained so until relatively recently) in making life difficult (literally, in many cases) for the emerging union movement, all too often at the behest of members of the capitalist class.

29 The review by Jessop (1983) gives some indication of this, but despite its size, hardly even touches on 'Regulation' theory. The French school (Aglietta, 1979; de Vroey, 1984) had been active since the early 1970s. The US school - chiefly the works of Weisskopf, Bowles and Gordon (1983; 1986) were not far behind. The regulation school has spawned a host of critics, notably a classical Marxist response from writers such as Simon Clarke (1988).

30 See, for example, the chapter on the capitalist state in Pitelis (1993).

the (problematic) concept of the Ideological State Apparatus (ISA). This tentative work, as evidenced by its subtitle - "Notes towards an Investigation" - approached the problem of the state in classical Marxist terms, ie, using the base/superstructure metaphor. Therborn's *What does the ruling class do when it rules?* (1978), a substantial account of state power and state apparatuses, served as a counter to the excesses of the appealing, but somewhat under-developed guide offered in Althusser (1971). Cohen's essay on the "Forces and Relations of Production" (1983) tackled the awkward question of functional explanations in the sciences. Within the corpus of Marxist analytical work there are many examples of what Cohen calls "...sloppy functional explanatory theorizing..." (1983, p120). In essence, this amounts to a claim that every action undertaken by the state is functional to the reproduction of capitalist relations of production. As Cohen observes:

"...those who propound the general claim about the state rarely trouble to say what sort of evidence would falsify or weaken it, and therefore every action of the state is treated as confirmatory, since there is always some way, legitimate or spurious, in which the action can be made to look functional." (1983, p120)

Claims like this (with Schumpeter's contempt resonating firmly around them) may be traced to fundamentalist interpretations of the admittedly programmatic methodological pointers given in Marx's "Preface to *A Critique of Political Economy*" (McLellan, 1997, p388-391). These rest in turn partly on the translation from the German of the crucial sentence:

"The mode of production of material life conditions the social, political and intellectual life processes in general." (McLellan, 1997, p389).

Rader (1979, p15) notes that the verb *bedingt* in the original is sometimes translated as 'determines' rather than 'conditions', and the last word in the sentence, *überhaupt*, rather than being rendered as 'in general', can be translated as 'altogether'. Such interpretations form the basis of a reductionist or reductive base/superstructure metaphor in which:

"...the economic system is the base that causally determines the political and economic superstructure..." (Rader, 1979, p14)

The standard response is to acknowledge the relative autonomy of the elements of the superstructure. Having done so, however, the problem of what meaning to give to this notion must be confronted. This is no simple matter, and there is but scant guidance available from classical Marxism.³¹ Althusser's route out of this difficulty is to point out that notwithstanding the virtues of the base/superstructure metaphor, given:

31 Rader's reading of Marx elicits an 'organic totality' model which avoids the weaknesses of the base/superstructure model ("...its tendency towards reductionism") whilst retaining its strengths ("...its emphasis on the historical importance of the mode of production") (1979, p56). Transforming this into a theory of the state is still no simple matter.

"...the relative autonomy of the superstructure and the reciprocal action of the superstructure on the base..."

there is a need to move beyond Marx' spatial metaphor precisely because of the fact that:

"...it is metaphorical: i.e. it remains *descriptive*." (Althusser, 1971, p136)³² (Emphasis in original)

Taking as his starting point Marx's observation that the ultimate condition of production of a social formation is the need to reproduce the conditions of production at the same time as it produces, Althusser's sets out to analyse the law, the state and ideology. Moving swiftly through the classical Marxist propositions about the state as a repressive apparatus, and the distinction between state power and state apparatus, Althusser arrives at what needs (in his view) to be added to the theory, namely:

"...another reality which is clearly on the side of the (repressive) State apparatus, but must not be confused with it. [He calls] this reality by its concept: *the ideological State apparatuses*." (Althusser, 1971, p142) (Emphasis in original)

He proposed (with suitable reservations) a list of ideological state apparatuses (ISAs) which includes most of the institutions of civil society. Briefly, they are:

- the religious ISA
- the educational ISA
- the family ISA (this obviously has other functions as well)
- the legal ISA (also part of the repressive state apparatus)
- the political ISA (the political system including parties)
- the trade-union ISA
- the communications ISA (press, radio, television etc)
- the cultural ISA (literature, arts, sports) (1971, p143)

The attractiveness of such a proposition to one bent on seeing the ubiquitous hand of capital in every facet of existence is immediately obvious. The question of what, precisely, ideology is, we leave dangling for the moment - Althusser's treatment of this phenomenon is not satisfactory. Turning to the question of the salience of ISAs, he argues that in different modes of production, different ISAs are dominant. Thus in the mode of social organisation characterised by serfdom, he singles out the church (1971, pp151ff). Under the capitalist mode

32 Marxist theory written in English is often not very easy to understand. When it is translated from another language, in this case, French, it can become much more difficult to comprehend. The full passage from which the argument above is drawn illustrates this:

"...the great theoretical advantage of the Marxist topography, i.e. of the spatial metaphor of the edifice (base and superstructure) is simultaneously that it reveals that questions of determination (or of index of effectivity) are crucial, that it reveals that it is the base which in the last instance determines the whole edifice; and that as a consequence, it obliges us to pose the theoretical problem of the types of 'derivatory' effectivity peculiar to the superstructure, i.e. it obliges us to think what the Marxist tradition calls conjointly the relative autonomy of the superstructure and the reciprocal action of the superstructure on the base."

of production, it is the educational apparatus. In support of this proposition he offers a number of arguments about the way in which education prepares citizens for their rôles, most especially those they will fill in the workplace (pp155-156). Not much criticism (or defence) of Althusser's argument will be offered here. That does not mean that Althusser has not attracted criticism. Therborn, for example, states that:

"For reasons which remain unconvincing, Althusser talks of ideological *state* apparatuses, thereby obliterating the peculiar apartness of the state from the rest of society, which has always been regarded as a distinctive feature of the state in the theory of historical materialism. It is precisely because of the cleavage of society into a separate state body and other social institutions that the state is bound up with the division of society into classes." (1978, p172n) (Emphasis in original)

Technically, Therborn is probably correct.³³ Even so, Althusser's suggestion remains seductive. When the concept is applied to quasi-state institutions such as the NPI (and, to a lesser extent, the SARB), Therborn's objections fall away.³⁴ Since, despite their nominal independence, both are part of the state apparatus, and certainly not part of the (directly) repressive state apparatus, some way of dealing with them conceptually is required. Althusser's approach - that one of their important functions is to disseminate the ideologies necessary for the reproduction of capitalist relations of production - provides it. Similarly, one could dispute the wisdom of including the family among the ISAs, but one can scarcely quibble about the educational system.

Direct evidence of attempts by the state to expand the school curriculum in South Africa to include productivity awareness training is available. It is written in language which no doubt would have pleased Althusser. The annual report of the National Manpower Commission for the year 1984 states that:

"In order to ensure a productivity-conscious workforce the broad concept of "productivity", with all its implications should be impressed upon children at school level. The various Ministers of Education are indeed mindful of the desirability of such a productivity awareness campaign, and against this background the Committee of Heads of Education has referred the matter to the various education departments.

33 Althusser's 'unconvincing' reasons turn on the proposition, noted by Gramsci, that the distinction between public and private is internal to bourgeois law, and that the state, which is the state of the ruling class, is neither public nor private (1971, p144).

34 Althusser's extension of the state to include so many of the institutions that one would normally regard as part of civil society is problematic. It may be useful in this regard to consider the quite tight distinction drawn by Yudelman between the state and the government. For the purposes of analysis, it is useful to separate these two conceptually, even though, as he points out, it is not easy to pin down the "...notoriously treacherous..." concept of the state. The state he defines as encompassing:

"...the civil service, judiciary, police, army: the institutions that make and enforce public policy, symbolically and actually. It is the "continuous administrative, legal, bureaucratic, and coercive systems" that seek to structure relations between government and society, as well as relations within a society. The state is a complex of mechanisms of domination and control, with the exclusive legal monopoly on the use of force and a territorial basis."

Government, by contrast, is merely the executive branch of the state:

"...defined narrowly to mean the executive heads of the ruling party in Parliament." (1984, p17)

However, the NPI was also asked to make a submission in this regard to the Committee of Heads of Education, and this matter is now receiving the attention of the Productivity Advisory Council." (RP 46/1985, pp198-199)

The annual report for the previous year, which carried a similar story, suggested that awareness be fostered "*inter alia*" by means of "...competitions with prizes, film shows, exhibits and the use of slogans..." (RP 41/1984, p279). The powers that be demurred - the 1984 Report noted that:

"It would appear that further efforts to instill the idea of productivity into scholars through the extension of school syllabuses will or could be hampered by the fact that the syllabuses are, it is claimed, already overloaded." (RP 46/1985, p199).³⁵

If the use of this Althusserian insight is to be branded as instrumentalism (or functionalism), it is certainly matched by the NPI's equally crude attempts to submerge real differences between workers and capital in the comfort of notions of a convergence popularised by the likes of Daniel Bell's *The End of Ideology* (1960).³⁶

One building block, the notion of the NPI as an ideological state apparatus, is in place - the task now is to sketch an analytical framework capable of showing why a state institution such as the NPI reproduces a specifically capitalist ideology, given that all are agreed on the relative autonomy of the state.³⁷ Dealing with this problem could take up as much space as that required by Therborn to begin addressing the question *What does the ruling class do..?* Clearly, such extravagances cannot be contemplated here. What follows must, of necessity, sacrifice rigour in the name of brevity. The simplest approach is to sidestep the problem by adopting a pragmatic stance similar to that taken by Yudelman (1984). Still rooted in a Marxist understanding of the economy, this work manages to avoid the scholasticism into which it is all too easy to slip. It does so by positing a symbiotic relation between capital and the state around two major imperatives of the state, those of accumulation and legitimation.³⁸ Failure to

35 In the 1980s, the NPI attempted to encourage education on productivity in universities. This enterprise has enjoyed at least some success. The NPI Annual Report for 1990/91 observes that:

"Further activities of the [Special Projects] department included interaction with the Pretoria and Rand Afrikaans Universities, where the NPI assisted with the development of courses, and with UNISA, in the structuring of quality and productivity modules for its School of Business Leadership." (NPI, AR 1990/91, p10)

The suggestion to introduce productivity training into the school curriculum resurfaced in the examination by the President's Council into the productivity problem. See P.C. 1/1989, paras.5.136-140, pp184-185. The report modelled its proposals on what was being done in West Germany.

36 In many respects, this is a sociological fleshing-out of the non-conflict theory that characterises the simplest and most popular varieties of bourgeois economics, with its 'factors of production' striving through the good offices of the market to attain points of Pareto optimality.

37 More precisely, since institutions cannot act, we are concerned with the problem of why agents within an institution act in a manner that tends to reproduce bourgeois ideology.

38 One of the authors from whose work these concepts derive, O'Connor (1973), has the distinction of being one of the very few Marxists whose work is given any consideration in the standard texts on public sector economics, the equivalent in conventional economics of theories of the state. See, for example, Cullis and Jones (1992, p386).

resolve the contradictions and tensions between these imperatives is manifest in periodic crises. Unlike some of the abstract exercises of which he is so critical, Yudelman's analytical approach is put to work on South Africa's economic history in a particularly fruitful way (Yudelman, 1984).

For ease of exposition, the major constituents of the approach as outlined by an analyst of similar persuasion, Claus Offe, are given below. This passage has been selected in part because its clumsy formulation of the relative autonomy question³⁹ forces a confrontation with this thorny issue. Although the passage quoted below suggests that there is some room for the pursuit of non-shared goals, ie, it acknowledges the relative autonomy of the state, it circumscribes that autonomy so narrowly that state and capital are almost indistinguishable. The passage reads as follows:

"Considered at the most abstract-general level, the concept of the capitalist state describes an institutional form of political power which is guided by the following four functional conditions:

1 *Private Production* Political power is prohibited from organizing material production according to its own 'political' criteria; property, whether in labour power or capital, is *private*. Hence, it is not political power, but private decisions that determine the concrete use of the means of production.

2 *Taxation constraints* Political power depends indirectly - through the mechanisms of the taxation system - on the volume of private accumulation. Those who occupy positions of power in a capitalist state are in fact powerless *unless* the volume of the accumulation process allows them to derive (through taxation) the material resources necessary to promote *any* political ends.

3 *Accumulation* Since state power depends on a process of accumulation which is beyond its power to organize, every occupant of state power is basically interested in promoting those political conditions most conducive to private accumulation. This interest does not result from an alliance of a particular government with particular classes or social strata also interested in accumulation; nor does it necessarily result from privileged access of the members of the capitalist class to centres of state decision-making, a privilege which in turn makes it possible for that class to 'put pressure' on the incumbents of state power to pursue their class interest. Rather the *institutional self-interest* of the state in accumulation is conditioned by the fact that the state is denied the power to control the flow of those resources which are nevertheless indispensable for the exercise of state power. Although the agents of accumulation are not primarily interested in 'using' the power of the state, state actors must be interested - for the sake of their own power - in guaranteeing and safeguarding a 'healthy' accumulation process.

4 *Democratic legitimation*⁴⁰ In parliamentary-democratic political regimes, any political group or party can win control over institutional state power only to the extent that it wins sufficient electoral support in

39 This is so clumsy as almost to make its author vulnerable to the same charge levelled by Yudelman above against some followers of Poulantzas.

40 The approaches of Yudelman, Offe and O'Connor must answer Therborn's criticism that: "...the modalities of class rule cannot be pressed into either the straitjacket of force-and-consent or into that of the structural dynamics of the economy (under capitalism, accumulation) combined with political legitimation. The concepts of consent and legitimation do not embrace the complex functioning of ideology." (Therborn, 1978, p242). Other than the cursory treatment which it receives below, this issue will not be confronted. As a crude working hypothesis, subject to the need for refinement along the lines suggested by Therborn's critique, the imperatives of accumulation and legitimation, and the symbiotic relationship between capital and the state in securing them will be accepted.

general elections. This mechanism plays a key rôle in disguising the fact that the material resources of state power, and the ways in which these are used, primarily depend upon the revenues derived from the accumulation process, and not upon the voting preferences of the general electorate. In other words, there is a dual determination of the political power of the capitalist state: the institutional *form* of this state is determined through the rules of democratic and representative government, while the material *content* of state power is conditioned by the continuous requirements of the accumulation process." (Offe, 1984, pp120-121) (Emphasis in original)

Issue will be taken below with the claim that "...every occupant of state power is basically interested in promoting those political conditions most conducive to private accumulation...", but before doing so, a word on certain aspects of the ideologies promoted by the state is necessary. Therborn observes that:

"The state, particularly its commanding personnel, must *represent*, that is to say, promote and defend the ruling class and its mode of exploitation or supremacy. At the same time, the state must *mediate* the exploitation or domination of the ruling class over other classes or strata. (1978, p181) (Emphasis in original)

In bourgeois states, be they democracies or not, the capitalist class is almost never directly represented in the state. It is necessary therefore to explain how the interests of this class come to be articulated. Fascinating though that topic is, it is not possible to do much more than skate over the surface of the question. A full chapter each is devoted by Therborn to explaining and furnishing examples of what is meant by (formats of) representation and (processes of) mediation. Such luxuries cannot be afforded here - there is space only for the few illustrations required to hint at a characteristic of each that is important for the purposes of this study. Mediation between "ruling and ruled classes" by the state is typically in universalistic terms. This is exemplified by maxims such as "Equality before the law" (Therborn, 1978, p170). To demonstrate the class nature of bourgeois justice (and democracy) and the systematic bias which this implies, Therborn points out, for example, that:

"During a strike, it is only one side which finds it necessary to 'disturb law and order' by mass meetings and picket lines." (1978, p170).

As to the general principle of representation, Therborn note that:

"Given the role of national unification and independence in the bourgeois revolution and system of power, the leading political spokesmen (*sic*) of the bourgeoisie have to put themselves forward as representatives of the nation." (1978, p185)

Without entering any discussion of the way in which leadership is attained, it can be stated that as a rule, the reproduction of bourgeois domination will be accompanied by frequent invocation of the 'national interest'. This claim is not necessarily entirely hollow - as Rueschemeyer and Evans observe:

"Ideologically, the state's tendency to present itself as the guardian of universalistic interests is likely to give state managers a distinctive affinity for ideological formulations that can be phrased in universalistic terms, an affinity that will in turn shape more concrete preferences. Although certain bureaucratic elites may be more exposed to short-run profit considerations (i.e., executives of state enterprises) or subject to cooptation in a way that precludes the development of an outlook distinctive from that of their constituencies, the potential for a distinctive outlook among state managers remains an important aspect of state structure, both because it may provide policies different from and occasionally superior to those espoused by private elites and because a distinctive outlook is critical to the state's ability to realize, even partially, its role as a corporate actor." (1985, p53)

These authors note, however, that the state's claim "...to being the "guardian of universal interests" is intrinsically problematic..." (p69). This is because the inherently contradictory nature of interventions within the economy and in civil society more broadly, excite the criticism of those not favoured by the results of such interventions.

Returning now to the offending statement - "every occupant of state power is basically interested in promoting those political conditions most conducive to private accumulation" - which appears in the passage cited above from Offe (1984), it is clear that anyone making such a claim about the servants of the apartheid state would be judged ill-informed, to say the least. A closer reading, however, reveals the importance of Offe's qualifying statement about the level at which the analysis is applicable - the most abstract-general level. Moving to the level of actual social formations, it is clear that further exploration of the limits of autonomy is necessary. This may be found in works such as that by Rueschemeyer and Evans (1985). These authors, whilst stressing the tightness of the constraints ("...autonomy remains very relative..." (1985, p62)), provide the beginnings of a systematic analysis of conditions leading to greater autonomy of the state (1985, pp63-65). These will not be explored at any length here - suffice it to say though that three of them apply with varying force in the South African case. They are:

- i Divisions within the ruling class
- ii Increased pressure from subordinate classes
- iii Ethnic solidarity

Over time, divisions within the capitalist class in South Africa between the different fractions of capital (mining, manufacturing, agricultural and commercial) diminished, pressure from the subordinate classes increased beyond the point where mere repression was appropriate, and ethnic solidarity within the group that constituted the bulk of state functionaries began to dissolve. As Yudelman points out, the open alliance of state and business dates from the so-called Carlton conference of 1979 (1984, p6). From then on, the painful process of shrugging off the apartheid albatross commenced in earnest, and with it, the abandoning of any serious claims of apartheid autonomy. Hiccoughs there were aplenty, but despite 'state capitalist'

deviations like Armscor, Mossgas and the two Sasols, the creation of alternative structures of control (the Joint Management Councils), and a flirtation with the dictatorial state forms prevalent in some of the East Asian NICs, both capital and the state moved inexorably towards 'free enterprise' and with it, to the concentration of increasing power in the hands of the capitalist class. That power was scarcely visible during the depths of the state of emergency in the late 1980s when the more conservative fractions of business, who cautioned a gradualist approach to deracialisation and democratisation, were in the ascendant (Morris and Padayachee, 1988). Neither the siege economy, nor authoritarian capitalism (*à la* East Asian NICs) was, however, ever a viable option, and within a much shorter space of time than almost anyone had predicted, the apartheid régime surrendered, almost secure in the knowledge that South Africa had been made safe for capitalism.

The transition to democracy saw many analysts of the capitalist class taken in by the populist rhetoric of the ANC, and the even more ominous-sounding socialist rhetoric of the union movement. Nearly every utterance of the Government of National Unity now discloses, however, its firm resolve to show that:

"...every occupant of state power is basically interested in promoting those political conditions most conducive to private accumulation..."⁴¹

In other words, historical digressions like apartheid which endure for decades are possible, and they can end, as Marx suggested in a somewhat different context, in the common ruin of the contending forces. If, however, holders of state power entertain serious notions of fostering private accumulation through the market on a long-term basis, accommodation with capital (usually on capital's terms) is ultimately unavoidable.⁴² It is in this sense that Yudelman's notion of symbiosis is appropriate.

41 One of the more interesting and predictable tensions in the ANC in South Africa today is that between a radical membership, eager for change, and a conservative leadership grouping, acutely aware of the way in which the scope for change has been narrowed by the combined forces of local and international capital. Some of these tensions were reflected in the drawing up and the launching of the MERG policy framework (MERG, 1993), a document that is well to the left of leadership on almost every issue.

42 It is important to note that the argument presented above is not simply a retreaded version of the old liberal claim about the economy and the polity being in profound contradiction, with the rationality of the former bound to triumph, sooner or later, over the irrationality of the latter. The relative autonomy of the state - its ability to act against the interests of certain fractions of capital, or indeed against capital in general - is important, and must be recognised. Marxist political economy is concerned to understand the forces which shape the state at particular junctures, not to pass judgement on the rationality of this or that capitalist state. It is also concerned with the limits to state autonomy at various points.

The problem of ideology

So far, discussion of what exactly is meant by ideology has been avoided. Some attempt must now be made to set this to rights, if only by showing how complex the problem is. As has been the case at other points in this part of the study, the relevant literature is huge and contentious. Surveying it is out of the question. An attempt will be made to provide a few pointers by referring to selected passages from some of the better-known Marxist works in the field. To begin with, the general conception of the relationship between science and ideology, from a Marxist perspective, is spelled out in the following quote from Larrain's compendious work on the topic.

"First of all, ideology can be seen as the opposite or antithesis of science, and the distortions of ideology as mere cognitive errors. The relationship between science and ideology is the opposition between truth and error. In so far as true knowledge appears the only means to surpass error, science appears as the way to overcome ideology. Ideology and science assume opposite characteristics which are irreducible to one another. Science involves a kind of cognition entirely different from ideological cognition.

This way of understanding the relationship is typical of certain traditions of positivist origin...

Second, ideology can be interpreted as different from science, though not its antithesis. Despite their differences, science cannot possibly defeat ideology, as ideology is rooted in social contradiction. Ideology is not simply a cognitive error which can be overcome by a more adequate cognition. Nor does science exhaust the concept of truth. There are errors which are not ideological, and there are truths which may be found beyond the actual cognition of society as it is. The specificity of ideological error is the fact that it conceals contradictions. The only truth which may defeat this particular error is the practical solution of these contradictions. Ideology cannot be dispelled by simple theoretical means because its roots are beyond the boundaries of mere ideological mistakes. This position, I think, can be attributed to Marx." (Larrain, 1979, p173)

The argument that science cannot dispel ideology is of considerable significance for this study. The engagement with the NPI⁴³ has been a tussle about ideology, one which could not be won simply by showing existing figures to be in error. As will be demonstrated in the next chapter, it required a major change in the balance of class forces to impress upon the NPI the need for compromise. So much for the relation between science and ideology. Larrain's work addresses three other basic questions, each of which has relevance for this study, but none of which can be considered at any length. The first of these is whether ideology is to be thought of negatively as false consciousness, or whether ideologies (note the plural) may be seen (positively) as world views that express the attitudes, opinions and theories that defend and promote the interests of a particular class. If the former, then "...the cognitive value of the ideas affected by ideology is called in question." If the latter, then "...the cognitive value of ideological ideas... is set aside as a different problem." (1979, pp13-14)

43 The explanations for the difficulties experienced in dealing with the CSS and the SARB probably lies elsewhere, possibly in an extreme sensitivity to criticism. This almost universal characteristic of bureaucrats has no doubt been exacerbated by the tense relations between the authorities and the liberal universities.

The next question is concerned with the problem of whether ideology is a subjective, psychological phenomenon or whether it depends on objective factors. If the latter, then:

"...ideology appears as a deception induced by reality itself: it is not the subject that distorts reality but reality itself which deceives the subject."

If it were the former, however:

"...ideology [would be] conceived of as a deformation of consciousness, which somehow is unable to grasp reality as it is."

Finally, he tackles the problem of:

"...whether ideology should be considered as a particular kind of phenomenon within the vast range of superstructural phenomena, or whether ideology is equivalent to and co-extensive with the whole cultural sphere usually called the 'ideological superstructure'. The first of these alternatives relies upon a restrictive concept of ideology since not all cultural objects would be 'ideological'. Conversely, the second identifies ideology with an objective level which includes all of social consciousness." (Larrain, 1979, p14)

At various points, Marxists of one hue or another have advanced each of the positions spelled out above. Larrain's project, a delicate one, is to thread his way between them. Thus the attempt by Althusser (1971) to escape the subjectivism (and idealism) of false consciousness by introducing structuralism into the analysis (Larrain, 1979, pp154ff) runs up against barriers of its own making. Althusser's structuralist extremism turns ideology into "...an objectified functional requirement of all societies..." and hence into another form of idealism (1979, p164).

Delving further into the problem takes us unto into the realms of psychoanalytics, social psychology (social identity theory), and linguistics - all of them far removed from even the most generous definition of political economy. Once more, therefore, the imperative to present a working definition of the concept, stripped of any finesse, and lacking the rigour one would normally wish to see, overcomes the arguments against doing so. To that end, the following working definition of ideology and a brief discussion of the way it functions, drawn from Therborn (1978), is offered:

"Ideology functions by moulding personality: it *subjects* the amorphous libido of new-born human animals to a specific social order and *qualifies* them for the differential roles they will play in society. In this process of subject-qualification, all ideology, both revolutionary and conservative, proletarian as well as feudal or bourgeois, interpellates individuals in three basic ways.

1. Ideological formation tells individuals *what exists*, who they are, how the world is, how they are related to that world. In this manner, people are allocated different kinds and amounts of identity, trust and everyday knowledge. The visibility of modes of life, the actual relationship of performance to reward, the existence, extent and character of exploitation and power are all structured in class-specific modes of ideological formation.

2. Ideology tells *what is possible*, providing varying types and quantities of self-confidence and ambition, and different levels of aspiration.

3. Ideology tells *what is right* and wrong, good and bad, thereby determining not only conceptions of legitimacy of power, but also work-ethics, notions of leisure, and views of interpersonal relationships from comradeship to sexual love.

All ideology contains these three modes of interpellation, but one or the other may receive greater emphasis in a given ideological discourse, or play a more important role in the process of social reproduction." (Therborn, 1978, pp172-173) (Emphasis in original)

Comprehensive though this account is, there is one important omission, namely a consideration of the way in which individual subjects respond to their ideological setting. Implicitly, in the discussion of ideology above:

"...it appears as if the dialectical process in society, which is based upon contradictions, leaves man (*sic*) 'untouched as a unity, in conflict with others but never with himself'. [What is proposed] on the contrary, [is the] existence of a subject which, like society, is 'in process', a subject whose identity is in crisis." (Larrain, 1979, p168)

Proposing such a subject is one thing, understanding the processes is another matter altogether. Still, one obtains an indication from this of where to begin looking. Very crudely, individuals are located within particular objective structures within society. Those structures cannot determine their response to the world, but they certainly condition it. The dialectic of capitalist development with all of its contradictory moments, at times places individuals in situations where the consequences of alternative courses of action are made clear. The ability to act in a manner not apparently consistent with class interests may thus be understood as the working out of individual responses to internal and external contradictions.

Individuals, institutions, psychology and economic theory

"Economics and psychology have in common that they try to describe, to explain and possibly to predict economic behaviour at the individual or the aggregate level. Economics is mainly concerned with the prediction of economic behaviour at the aggregate level, and with direct relations between input and effect, for example between prices and purchases. However, this does not mean that economists should not be interested in psychological variables, for example perception and evaluation, that intervene between input and effect." (van Raaij, 1991, p798)

Inevitably, since it is individuals rather than organisations that produce statistics and interpretations or analyses of them, this study makes frequent references to the professional

activities of certain individuals who work, or have in the past worked for the NPI. The concern is with the way in which productivity statistics generated by the NPI have been interpreted by these individuals. The activities in question, which have been argued to reproduce a particular view of the way in which the economy has performed, and of what should be done to improve this, take the form both of public pronouncements and of private communications. Almost unavoidably, questions arise about the extent to which the NPI's economists can reasonably be expected to have been sensitive to the quality of the data with which they have worked. Addressing this question further increases the concentration on the activities of the few individuals concerned. This could result in the study giving the impression of its having an undue interest in the activities of four or five individuals.

The theoretical excursus undertaken above has led from the imperatives of accumulation and legitimation to a consideration of the institutions (ideological superstructures, state apparatuses) of a social formation which function to reproduce class rule.⁴⁴ Since the express intention of the NPI's communications is to persuade, and since much of the campaign to improve productivity has been conducted through the media, the information required to evaluate the quality of the output of the institution is a matter of public record. The final object of inquiry is not, however, the essentially private process of the production of statistics, carried out more or less competently in accordance with a standardised set of rules. Rather, it is a public activity in which statistics are used to advance particular analytical (political) positions, but for private reasons. Choosing or developing analytical tools to use in examining certain behaviours of individuals (social agents) within particular institutional settings is thus the last step in the theoretical project. To say that this is a complicated and sensitive project is to understate, fairly considerably, the difficulty of the task. In short, it cannot be performed here, because to do so requires a movement away from economics, no matter how broadly conceived, into that part of psychology concerned with social identity.

Lest it be thought that the study of individual contributions to the development of institutions and the propagation of particular ideologies is the exclusive province of biographers (or psychologists), it should be pointed out that there exists at least one such enterprise (there are probably many more) conducted by someone who shows all the hallmarks of a thorough grasp of economics from a left-wing point of view. This is the piece by Radhika Desai called "Second-Hand Dealers in Ideas: Think-Tanks and Thatcherite Hegemony" (1994). Unlike my engagement with the individuals in the NPI (and the CSS), Desai has not been directly

44 The proposition that certain institutions function in this manner should not be understood to be a claim (along functionalist lines) that they exist (and come into existence) because they are functional to the reproduction of capitalist relations of production.

involved with the subjects of her study. Her cast of characters, though, is similarly small - she is concerned with the handful of (right-wing) activists whose endeavours over the years prepared the soil in which the Thatcherite ideology was to take root in Britain. The two institutions with which she deals are the Institute of Economic Affairs (IEA) and the Centre for Policy Studies (CPS). The origins of the former she traces directly to the apprehensions of its founder, Anthony Fisher, about the directions of British society in the 1950s. Inspired by a condensed version of von Hayek's *The Road to Serfdom* that appeared in the *Readers Digest*, Fisher handpicked Ralph Harris, a lecturer at St. Andrews and Arthur Seldon, a journalist, to build the IEA. Both, she notes, were energetic and committed to free-market ideas. In her words:

"Like many of the main actors in the think-tanks, Harris and Seldon displayed the characteristics of sectarians - a zeal in their mission, stubbornness and ability to deflect criticism and a profound faith - in a competitive, free-market economy based on individualism and enterprise." (1994, p45).

This is not the place to review her study - suffice it to say that she not only provides a theoretical framework in which to comprehend the activities of these 'organic intellectuals' of the ruling class (she makes extensive use of Gramsci's ideas), she also does not shy away from the somewhat sensitive fact that ultimately it is individuals who act in society - not institutions. Dealing with this awkward reality is something with which economists have never been very comfortable. Most Marxists have also shied away from the problem - even at the same time as they recognise that:

"Classes are not actors in the same sense as individuals, groups or organizations are, decision-making actors bringing about events or 'monuments', such as programmes, codes, etc. A class can never make a decision as a class. But nor is class agency, in the Marxist sense, a series of isolated actions, the indirect effects of which are gauged by the analyst in the form of a statistical measure of some sort, such as rates of economic growth, social mobility or electoral participation and the distribution of income or of votes. Class is a third kind of agency, of tendentially acting forces defined by their economic location, acting collectively, to an ever-varying but (virtually) never complete degree.

There is a Marxist tradition which has tended to treat class as an agency in the first sense, but this has meant little more than a metaphor or device for summing up some social process, referring to the bourgeoisie or the working class thinking or doing this or that. In political sociology, on the other hand, class agency is usually conceived of in the second meaning, as an analytical construct for the ordering of individual acts, most often voting. But the intellectual, and political challenge to social scientific Marxism will be to elucidate and elaborate class agency as a specific, third kind of agency.

Classes act through the actions of individuals, groups and organization[s]. The operation of class agency may be seen in a commonality of concerns, in a parallelity of strivings, a similarity of the forms of actions and in interrelationships of mutual reinforcement between the actions of members of the same class. To what extent this commonality, etc., becomes conscious and manifested in processes of collective decision-making with a specific outcome is a set of empirical questions about class formation and class history." (Therborn, 1983, p190)

Clearly, a broad formulation such as this maps out a direction which class analysis should take, but does so in such a general fashion that it does not assist greatly with the concerns of this study. There are two possible avenues to explore which may direct our energy more fruitfully - the development of micro-foundations for Marxist macro-theory along lines suggested by Wright *et al* (1992, pp120ff), or alternatively through the examination of the function of intellectuals in Gramscian terms as suggested by Desai (1994, pp38ff). Fascinating though these possibilities may be, they cannot be pursued here - that is a task for the future.

Paradoxically, despite the accent laid by bourgeois economics on the significance of individual action,⁴⁵ that body of theory is no more helpful when it comes to understanding the kind of actions reported in this study. Consider, for example, Darrell Huff's quite harsh judgement (quoted at the beginning of Chapter 1-2) that:

"As long as the errors remain one-sided, it is not easy to attribute them to bungling or accident..."

Teasing out the implications of a statement like that leads one rapidly into difficult terrain. Huff would seem here to foreclose on mere incompetence. That being so, where does one take the analysis? Clearly, if the matter is to be pursued, one takes it into even more difficult terrain. It would seem therefore that there are certain directions in which it would be inadvisable to push this study. For example, insofar as actions have consequences, one could argue that individuals, whether acting in their own capacity or as representatives of organisations, are, or should be, held responsible for the public statements that they make.⁴⁶ Since, however, no reasonable method exists for determining, with any precision, the impact of statements about the economy, and since furthermore, one cannot discover whether or not the statements at issue are made in good faith, one cannot reasonably attribute any kind of blame for any ill consequences to those who have made them. In short, one has to restrict one's interest in the activities of the servants of an organisation such as the NPI solely to their rôles as bearers of social relations, not as individuals *qua* individuals. No matter how curious we may be to know the true reasons why people say (and apparently believe) some of the things that they do, we, as economists, save ourselves a great deal of bother if we content ourselves with examining the content of what is said and the context in which it is said, rather

45 Leibenstein (1979, p497) states simply that "Microeconomics has avoided the study of individuals".

46 The importance of the spoken word is most evident in the case of prominent politicians - financial markets, for example, hang on every utterance of ministers of finance or chancellors of exchequers, especially in times of economic crisis.

than with speculation about possible motives.⁴⁷ The problem is hardly of recent vintage - the French deputy for Nemours, Pierre S du Pont,⁴⁸ said in 1790 that:

"...it is a disagreeable custom to which one is too easily led by the harshness of the discussions, to assume evil intentions. It is necessary to be gracious as to intentions; one should believe them good, and apparently they are; but we do not have to be gracious at all to inconsistent logic or to absurd reasoning. Bad logicians have committed more involuntary crimes than bad men have done intentionally." (cited in Friedman, 1977, p471)

Given the limits of the analytical apparatus deployed in this study, counsel like this ought not to be ignored. In attempting to understand the actions of individual social agents within an institution such as the NPI, it would seem sensible not to make individual motives the subject of speculation. Thus although some seemingly callous utterances are quoted in the study in support of the general argument on the nature of the NPI as an ideological state apparatus, it cannot be stressed strongly enough that no motives are imputed to any individual. In subjecting the logic of the NPI (and those who share the viewpoints it promotes) to critical scrutiny, there is no suggestion that individuals who subscribe to the 'NPI line' act with 'evil intentions'. In terms of the approach adopted, in certain crucial matters, social agents behaving as though they subscribe to the 'NPI line' may be argued to be 'bad logicians', and as such, some of their actions may be construed as 'involuntary crimes' against the working class.

Social engineering (of which the NPI's national campaign to improve productivity is an example) is always problematic; when to the effects of bad logic one adds the operation of the law of unintended consequences, the scope for mischief is great indeed. It results chiefly from the fact that so-called experts are often granted important rôles in the formulation and

47 Although economists do not hesitate to discuss causality when it runs from economic phenomena to (abstract) individual behaviour, they are not so keen to do the reverse, if for no other reason than perhaps to avoid the charge of employing *argumentum ad hominem*. Short of conducting detailed cross-examinations of those who utter (economic) statements of interest or consequence, causality in this direction must remain poorly understood. Knowledge about motives is generally inaccessible, and economists therefore are more or less obliged to settle for relatively crude statements about aggregate behaviour. In general, they have been reluctant to stray beyond limits established by convention in the use of what clearly are a set of quite sweeping psychological postulates. Simple notions such as 'taste', 'utility maximisation', 'self-interest' or, more daringly, 'conspicuous consumption', have served the profession well. This notwithstanding the efforts of critics of the foundations of conventional (neo-classical or subjective preference) theory, like Foley (1975) who has argued that concepts such as 'taste', are often defended merely by private affirmation of belief on the part of the economists who use them. As Akerlof and Dickens observed:

"...economists have built an entire profession on a single powerful theory of human behavior based on a few simple assumptions." (1982, p307)

The work of anthropologists, sociologists, and political scientists has, for the most part, been ignored. Necessary though such surrender to prudence or prejudice may have been thought to have been, it has not been without price - an understanding of individual motives could make a significant contribution to an understanding of the very foundations of the discipline. A partial explanation of the poverty of the psychology deployed by economics is the fact that "...[P]sychological research during the nineteenth century was not yet sufficiently well developed to provide a base for microeconomics." (van Raaij, 1991, p801). With the rapid growth of the discipline of economic psychology, there are, however, some signs that this is changing.

48 The name is so unusual, that this person must be related to the founder of the giant American chemical manufacturing corporation, E I du Pont de Nemours.

implementation of policy without adequate mechanisms to control them being present. Even representative democracies sometimes allow this. The danger inherent in this is that the 'experts' may then be in a position to indulge any propensity they may have for ignoring evidence that does not coincide with their (often pre-conceived?) notions of the truth of the problem being confronted.⁴⁹ Obviously, policy formation and implementation involves trade-offs - if welfare economics has any contribution to make it must surely be in helping to identify winners and losers. There is, however, no guarantee that adequate analysis will place, and even less likelihood that losers will be compensated. In democratic societies, the influence of the overbearing expert may well be countered if the institutions of civil society are sufficiently cohesive and powerful.⁵⁰ In undemocratic countries like the old South Africa, the danger of (well-intentioned) experts influencing policy in ways that could inflict damage on individuals and the social fabric was much greater. When the social safety net is limited or maybe even non-existent, the consequences can be serious indeed.

A working hypothesis (and some of the evidence in support of it)

Guided by the analytical arguments offered above and a working knowledge of the performance of the economy, a basic hypothesis (with Marxist contours) can now be developed. In keeping with the somewhat idiosyncratic way in which this study is structured, this 'hypothesis' comes after most of the evidence required to at least establish that it is worthy of further investigation. In a sense, this is appropriate - my view of the institutions of capitalist social formations was certainly not fully developed when first I confronted the NPI (and the

49 As stated above, it is not the intention of this study to delve into the psychological aspects of 'economic' rationality, but it is worthwhile noting that there exists a substantial literature on many topics that ought to be of considerable interest to economists. For example, once one moves away from the somewhat sterile public-choice theoretic explanations of self-interested behaviour in political actors - of which Buchanan's (1987) celebration of the centrality of the methodological individualism, *homo economicus* and politics as exchange is the most prominent, one finds a treasure trove of explorations. An interesting and highly controversial starting point for the examination of the question of prejudice could be an article by Fox (1992). This author rejects a model of (perfectly?) rational human behaviour in favour of a view that rationality consists in a series of pragmatic prejudgements of reality that have stood the test of natural selection. This leads Fox to a reconstruction of the idea of prejudice from a negative to a mildly positive attribute and hence to the conclusion that prejudice is not a form of thought but that thought is a form of prejudice. Not unsurprisingly, the work aroused a storm, the content of which does not concern us here. Suffice it to say, that given time, it should be possible to uncover explanations of the activities of social agents that are more intellectually satisfying than those currently on offer in the economics profession.

50 Evaluating the politics of 'cost-of-production' theorists (broadly speaking, Keynesians and Ricardians of all hues) Cole, Cameron and Edwards (1991, p170) refer somewhat gloomily to the paradox by which "...fascination with complex technology, distrust of the market and genuine concern for the deprived gives rise to political forms which produce active, large-scale intervention to reduce inequality but on the terms of authoritative experts, not of the mass of the population. Cost-of-production theory seems thus to be, in the final instance, a managerial ideology, capable of delivering to a deprived minority of us [or majority in the South African case] what a well-educated minority feels they need. At one extreme, it is muddled, benign and well-meaning, if ineffective, at the other it is arrogant, manipulative and authoritarian if, in some narrow sense, efficient."

CSS and SARB!). It is still a long way from being so now, and the admixture below of evidence and proposition reflects the 'work-in-progress' status of this part of the project. Maybe it would be more accurate to describe what follows as a group of propositions - some empirical, some theoretical - about the general climate in which the NPI was constrained to function.

The first step is to argue that the impetus towards the creation of an institution such as the NPI arises out of the accumulation and legitimation imperatives of the state,⁵¹ with the former being the more important. A perception (considerably exaggerated, as has been demonstrated in Chapter 1-2) that productivity growth was faltering, and the tapering off of the growth phase based on import substituting industrialisation (ISI) created conditions which necessitated a change in state strategy.⁵² A new balance between the (changing) requirements for sustained accumulation and the need to satisfy an increasingly militant black workforce was called for. Most prominent among the black demands were the right to organise and the right to a living wage. At the same time, however, a vociferous white minority, threatened by changes in the political economy, had to be placated.

In the debate over the wage/productivity nexus social agents in the organs or institutions of the state are likely to come into confrontation with the working class over the latter's claims on the total social product. Conservatism on the part of the NPI over the question of wage increases

51 To talk of a ruling class and its interests is not to conflate a state (like the apartheid state) with its own projects, priorities and constituencies, with the capitalist class, itself a fragmented and contradictory grouping, in the manner that some mechanical (vulgar) Marxist analysis of the early 1960s did. The straw men who equated apartheid with capitalism, partly conjured up and disposed of by Lipton (1985), were merely the opposites of the equally vulgar *homo economicus* posited by liberals in South Africa as the natural (economic) enemy of an irrational politics. Political economy is much more complex than that, and so too, are theories of the state. By the same token so too must be one's approach to understanding ideological state apparatuses.

52 It should not be imagined that because the state and certain advanced fractions of capital recognised the need for a sea-change that such change could necessarily be brought about. The new South African state is still wrestling with the problem. There is an energetic debate on this general issue within Marxist theory, the details of which need not detain us here, between 'regulationists' and classical Marxists, some of whom see regulation theory as an escape route into post-Marxism, or even worse, into post-modernism. One of the accusations levelled by classical Marxists against regulationists is that the latter are guilty of functionalism - a charge that is hotly denied. Having examined the long cycles of boom and bust to which capitalism is prone, here is Lipietz (1984), a well-known member of the French school, defending regulation theory against such a charge:

"We have just referred to the precariousness of the successful instances of capitalism, the scale of the contradictions which have to be resolved before it can reproduce itself, the necessity of finding a system of accumulation and of assembling an appropriate mode of regulation. In short, what we have said about the much more improbable than likely existence of capitalism and its concrete instances must not lead us to think that 'when it works, it must have been designed for that purpose', or that this mode of regulation *was meant* to encourage this regime of accumulation, for example like saying that Social Security was invented for the purpose of keeping mass-production going smoothly. One merely ought to say that a regime of accumulation and certain modes of regulation have become stable together because they have allowed social relations to reproduce themselves without crisis for a while. At best, one can apply some kind of *posteriori* functionalism at a metaphorical level so to speak, such [as] 'it all happens as if' ... as if, for instance, the underdevelopment of the periphery had helped metropolitan capital to prosper." (Lipietz, 1984, pp86-87) (Emphasis in original)

unrequited by productivity improvements is inevitable because of the accumulation imperative of the state. Insofar, however, as the working class disposes of significant power, one can expect that these confrontations will not necessarily be head-on, but may take the form of skirmishes around general and sometimes quite subtle points of principle relating to 'economic reality'. One would also expect the dominant class and its organs to attempt to separate 'economics' from 'politics'. Various tactics are deployed to achieve this end. One of these is the attempt to present the particular interests of the dominant groups as the common or national interest. Another involves attempts to neutralise working class demands by enticing leading elements of militant worker organisations into corporatist institutions (ie, into institutions which manufacture consensus) in subaltern rôles so that militancy can be damped.⁵³

In other words, the organs of state and/or ideological state apparatuses in a capitalist economy attempt to block worker claims for an increasing share of the total social product, sometimes confrontationally, sometimes by trying to undermine the legitimacy of the claims, usually by seeking to represent 'reality' in a particular way, and sometimes, when the balance of class forces is roughly equal, by facilitating attempts to hammer out positions that can be shared by labour and capital. Struggles can be expected to take on a particular urgency when a serious challenge to the hegemony of the dominant classes is mounted. Increasingly, that challenge in South Africa came from a resurgent worker movement - highly politicised, articulate, and unwilling moreover to tolerate attempts to have foisted upon its members interpretations of the reality of South Africa's political economy constructed by the ruling groups.

Of very few modern societies can it be said that a single view, that of the ruling groups, dominates all discourses for any length of time. This is probably true even under quite authoritarian regimes. Opposing views are constantly being advanced, with greater or lesser success, depending on the political conjuncture and the balance of class forces. Attempts to comprehend the processes by which the dominant groups seek to legitimate the structures of society require that the perceptions of reality and the means by which these perceptions are created, sustained and challenged be understood. It would be silly to suggest that in the Western capitalist democracies the dominant or hegemonic views are propagated consciously by a mechanistic process in which all or most ideas, especially new ideas, are extruded

53 To be critical of institutions that manufacture consent by negotiation is not to suggest that adversarial relations are preferred, ie, that adversariality *per se* has social value. Class conflict is endemic in capitalist economies - clearly an undesirable state of affairs. In a conflict situation in which capital and labour are relatively evenly matched, perpetual antagonism is obviously possible, but agreement over issues of common interest, if it can be reached without too great a compromise of principle, is preferable. See the discussion in Meth (1993c) on the way in which such agreement has been negotiated in Canada by the Canadian Labour Market and Productivity Centre - a joint worker and management body. The National Economic Development and Labour Council (NEDLAC), successor of the National Economic Forum (NEF), has the potential to perform a similar function in South Africa.

through filters (theoretical frameworks) designed and controlled by the ruling groups. And yet something akin to what Marcuse (1970) termed 'repressive tolerance' operates as far as the popular media are concerned. In non-authoritarian states, the commitment of leading intellectuals (including media analysts) to the dominant view is *sine qua non* for the continued reproduction of the hegemonic ideology.⁵⁴ One of the most important arenas in which struggles over legitimacy are conducted is within what may be described as 'media-space'. It is here that battles over 'hearts and minds' are fought.⁵⁵ Thus in a capitalist social formation, one would expect the media to reproduce an economic ideology consonant with the aims of capitalist production and accumulation.

In South Africa, there was never, for example, any serious debate about the merits of sanctions. In part, this was because the state forbade the publication of material supportive of sanctions. The media, however, never short of creative ways to articulate their disapproval of other government actions where comment was declared taboo, was vociferous in its opposition to sanctions. The productivity debate has about it a similar air. Critiques of the NPI outpourings have been granted some space, but in general, the coverage has taken the suitably uncritical (when not biased) form that one would have expected.

The evidence one would search for to substantiate this hypothesis is of two main kinds - both to be found within the media-space referred to above. The first type of evidence would be a systematic representation of the wage/productivity relation and all of the associated variables in a form that suits the goals of the dominant classes or groups in society. If these representations are to be taken seriously one would expect them to have an authoritative appearance or character - academic output, suitably reworked for popular consumption would seem to be ideal for this purpose.⁵⁶ The second type of evidence is less easy to define, but like an

54 Success in this enterprise is never guaranteed - hegemony has continually to be reasserted, because class struggle threatens existing mechanisms of social control. A failure to discard old control mechanisms when they no longer function can result in chaos, with order not being restored until suitable new institutions emerge. In practice, change is usually gradual or evolutionary rather than convulsive, but evolution is not inevitable.

55 One of the earliest reported attempts to mobilise support on a large-scale in South Africa was the publicity campaign run by the Transvaal Chamber of Mines in the run-up to the strike of 1922. Interestingly, once one gets behind the racial rhetoric, it becomes clear that the core dispute in the strike was the question of the relationship between wages and productivity. See Yudelman (1984, pp138ff).

56 Richard Hyman's "small book on a large subject" *Strikes*, contains an excellent chapter on the nature of perceptions of strikes and the ways in which these are formed. Public hostility towards this "deviant" behaviour is in no small measure due to the activities of the mass media. As Hyman observes, these:

"...do not purport, for the most part, to provide a detailed and systematic analysis of the social phenomena which they report. This, it would be argued, is not the function of a television newsreel or a mass-circulation newspaper. One consequence of this is that what is socially distributed as 'news' is typically characterised by superficiality, trivialisation and sensationalism." (1981, p146)

Productivity performance reports prepared for public consumption suffer a similar fate - they are reworked to strip away of all the *caveats* an academic would deem vital, explanations are reduced to the absolutely simplistic, and an air of spurious authority is created by the use of results given to two or more decimal places - results which any serious scholar would know are impossible to obtain.

elephant, one recognises it when it is encountered. It consists in a diffusely conservative set of claims about the workings of an economy made by economic right-wingers. These are as distinctive as those made by the left-wing counterpart of this group, except that they are inverted. Thus, for example, where the right sees potential convergence, the left discovers endemic conflict - where the right demands the removal of government 'interference', the left insists on the benefits of government 'intervention'.

In Chapter 1-2 the evidence offered of the first kind, although it may not be sufficient to support a charge of systematic misrepresentation of the 'truth' of South African worker performance, does at least point to a use of statistics so uncritical as to border on the negligent. The NPI's unwillingness to look carefully at output measures, the careless use of indicators such as GDP per capita, or even worse, GDP per economically active person, can most charitably be described as sloppiness. Space constraints prevent an adequate consideration of evidence of the second type, but I have addressed some of the issues elsewhere. One of the published works (Meth, 1991b) tackles a range of issues such as the NPI's stance on income redistribution, and the unwillingness of the organisation to acknowledge the intensely political nature of the productivity improvement campaign in South Africa. It also tackles the ideology of convergence underlying the NPI's campaign. A lengthy conference paper attempted to unpick the strands of the ideological climate within which the debate over African workers' wages has been conducted (Meth, 1985).

The analysis of the press clippings on the topic, whilst not conclusive, adds further weight to the claims about the predicted behaviour of individuals within ideological state apparatuses. The general tone of the articles, some sense of which has been conveyed in previous chapters, may be regarded as a fairly accurate social barometer. The tilt in the direction of capital is exactly what one would have expected to find.⁵⁷ Another important piece of evidence favouring the argument advanced here about the nature of the NPI and its activities was the grudging acknowledgement drawn from an NPI official at a point in the institutions' history when it redoubled its efforts to win legitimacy by persuading Cosatu (the Congress of South African Trade Unions) to participate in the affairs of the NPI.

Institutional goals, either as articulated at the level of the dominant class in general or by individuals who dominate the power structures change over time in response to changes within the broader political economy. Social actors within institutions, especially, but not exclusively

57 The contributions of the press and the electronic media (radio and television), institutions which themselves must be described as ideological state apparatuses, to the NPI's productivity campaign are worthy of a separate study.

ideological state apparatuses (ISAs) such as the NPI, have before them a menu of possible actions which they can undertake. Although individual behaviour is not determined in any mechanical sense by institutional reality, the goals and structures of the various institutions place strong limits on what any individual may or may not do. Systems of positive and negative sanctions serve alternately to reward or discipline individual agents. As an extreme example, no servant of the NPI (or any other ISA) could advocate a planned socialist economy. One cannot deny the importance of human agency, but by concentrating the analysis on the structural limits within which the adherents to what may be termed the 'NPI line' operate, one begins to detect where those boundaries are. The repeated references made to the increasingly Thatcherite milieu within which the NPI's productivity improvement campaign has been conducted represent part of the attempt to identify some of these boundaries.

Chapter 1-5

The significance of theoretical commitment

On science...

"A profession which allows alternative theories for the same phenomena (especially those with different policy implications) to coexist for substantial periods of time without serious attempts to discriminate between them is likely to lose its credibility, and is indeed non-scientific." (Mizon, 1991, p576)

Introduction

Sufficiently few economists dig into the question of the way in which statistics are used as ammunition by particular social groupings to justify concluding that a measure of complacency is abroad among the profession.¹ The same may be true of statisticians, but their location at one remove from the policy coalface, as it were, makes such a sin less besetting than it is in a profession where application is of such political significance. In any case, as the references to Huff's and Moroney's classic works demonstrate, statisticians have long been conscious of the ways in which statistics are abused, and have striven energetically to *cave* both *emptor* and *vendor*. The validity of the warnings they have issued is scarcely diminished by the absence of a social theory with which to understand the abuses to which they draw attention - that was not the purpose of their respective books.

Neglect of difficult-to-detect errors in statistics is one thing - the apparently uncritical treatment of questions which, one imagines, would impel any economist, especially any

1 There have been several warnings of the dangers of drawing incorrect conclusions from weak statistics. This applies to contemporary debates as well as the historical record. Economic historians have to deal with these problems as a matter of course - although there is probably a limit to what can be milked out of the existing numbers, ingenious reinterpretations continue to appear. Michael Darby, to whom reference was made in Chapter 1-1 for his work on productivity statistics, is an academic with a cheerfully iconoclastic style, if the titles and content of his work are anything to go by - his "Three-and-a-Half Million US Employees Have Been Misled: Or an Explanation of Unemployment, 1934-1941," (1976) is not only thorough, it also makes good reading. On contemporary material, Dudley Seers' (1975; 1976) and Polly Hill's (1986) cautionary tales about national accounting statistics as development indicators are well known. These have been followed in recent times by the work of Griliches (1985) and Deaton (1993), both of whom are considered further below. Even so, few economists appear to have concerned themselves with the precise manner in which statistics are abused.

Marxist economist, willy-nilly into a consideration of the ideological foundations of economic theory, is another matter altogether. That is the issue addressed in this chapter. It could be phrased as follows - what is it about the theoretical commitment to one or another world view which predisposes social scientists in general, and economists in particular, to adopt positions which so often advance particular political stances, whilst (sometimes) denying that they do so?

Although at times the effort of dragging oneself through a large literature is tiresome, one of the spinoffs from the exercise is the opportunity it provides for confrontation with one's prejudices. This is not to say that the experience is everyone's road to Damascus. There is a sense in which prior commitment to a particular theoretical framework proves so tenacious that for many (most?) people, a brush with alternative ideologies has no noticeable effect (they may indeed become more reactionary). In its early stages, my theoretical understanding of the NPI, incompletely developed as it still is, had something of that character.

It will be recalled that towards the end of the previous chapter, a fairly detailed working hypothesis was drawn from the broad body of work called Marxist theory. In the first draft of this work, a lengthy apologia was written to justify the use of an exclusively Marxist approach. It seemed at the time to constitute an adequate defence of the stance adopted. On reflection, and with further reading, an uncomfortable possibility began increasingly to assert itself - namely that similar hypotheses to the one proposed in Chapter 1-4 could, in principle, be generated using conventional economic theory. 'Marxist' the working hypothesis outlined in the previous may well be, but of the two different types of evidence needed to confirm or reject it there seems to be no reason why at least some, if not all, of the evidence of the first type, misrepresentation of the wage/productivity relation, could not be solicited by a conventional economist posing a simple question, to wit, has the NPI been scrupulous and judicious in its use of statistical information?

Content analysis of publications and media pronouncements is somewhat more complex - a Marxist would, as a matter of course, search in the reports for attacks (thinly-veiled or otherwise) on workers, especially around the wage question - it is unlikely that economists of other persuasions would do likewise. Sooner or later, however, a bourgeois economist of critical bent conducting a search through NPI publications and through newspaper clippings on productivity would begin to see a pattern emerging - frequent references to high wage growth coupled with low productivity growth, the inevitable association with inflation (cost push), rising unit labour costs and the concomitant impact on international competitiveness, a causal link between unreasonable wage demands and rising capital intensity, apparently inexplicably

low capital productivity, the unwillingness of the 'black' unions to cease making political demands, etc, etc. Politically, bourgeois economists may well sympathise with the underlying antipathy towards aggressive trade union federations like Cosatu, but any of them whose intellectual integrity is intact could surely be engaged in a debate on the relative fragility or robustness of the revealed content of what the media, following the NPI, has said. On the question of the evidence of bias, not to say misrepresentation in the use of statistics, it seems likely that economists of all political persuasions could be brought to agree. Consensus on the slant of the campaign as a whole might not be so easy to secure, but a frank confrontation with the limitations of bourgeois theory in the fields of wages and income distribution, not to mention the confusion of the growth accounting literature, should soon elicit concurrence with the view that careful academic analysis offers but scant support for many of the simplistic statements churned out for popular consumption by officials of the NPI.²

Awareness of some degree of agreement, if not of actual convergence, began to dawn as a result of a reading of attempts by Erik Olin Wright, a highly-respected Marxist theoretician, to 'reconstruct Marxism'. Wright's opening gambit is innocent enough, a straightforward restatement of the continuing value of Marxist theory, of its continued vitality as a critical analysis of the capitalist mode of production, despite the confusions and doubts brought about by the collapse of the Communist experiment in Eastern Europe and Russia. It is all too easy to lose sight of the fact that Marxist social theory and Communism are not the same thing, a conflation which Wright tackles thus:

"In both the popular press and the scholarly media we hear a lot about the crisis of Marxism, even of its death. Frequently the collapse of regimes ruled by Communist parties is equated with the collapse of Marxism as social theory. However, while there is unquestionably a historical linkage between Marxism and capital-c Communism, they are not interchangeable. Marxism is a tradition of social theory, albeit a social theory that has been deeply embedded in efforts to change the world. What is more, it is a tradition of social theory within which it is possible to do social science - that is, identify real causal mechanisms and understand their consequences. Capital-c Communism, on the other hand, is a particular form of social organization, characterized by the eradication or marginalization of private ownership of productive resources and high levels of centralization of political and economic power under the control of relatively authoritarian political apparatuses, the party and the state. Such parties and states used Marxism as a legitimating ideology, but neither the collapse of those regimes, nor their failure to live up to the normative ideals of Marxism are, in and of themselves, proofs of the bankruptcy of Marxism as a tradition of social-scientific practice.

Indeed, there is a great irony in the claim that the demise of Communist regimes based on command economies implies the demise of Marxism. The core ideas of classical Marxism as developed in the late nineteenth century would lead one to predict that attempts at revolutionary ruptures with capitalism in backward, nonindustrialized countries would ultimately fail to accomplish their positive objectives. Orthodox historical materialism insisted that socialism only becomes possible when capitalism has

2 Yet another of the areas in which the NPI has played fast and loose with the 'truth' is in the highly contentious field of the impact of productivity growth on employment. Conflicting NPI positions on this question are examined in Meth (1994).

exhausted its capacity for development of the forces of production - when it is a fetter on the future development of society's productive capacity. All Marxists, including Lenin, believed this prior to the Russian Revolution. The anomaly from the point of view of classical Marxism, therefore, is not that state bureaucratic command economies have failed and are in a process of transition to capitalism, but that they survived for as long as they did.³ This reflects a basic silence in classical Marxism: it contains no temporal scale of its predictions. But the important point in the immediate context is that the collapse of Communist states is not a refutation of Marxism; it is at most a refutation of Leninist voluntarism, of the belief that by revolutionary will and organizational commitment it is possible to build socialism on inadequate material foundations." (1993, pp15-16)

Amongst adherents to the Marxist tradition there is no shortage of claimants willing to supply answers to the question of what Marx really meant, and of what a Marxist methodology should be.⁴ This can be read unsympathetically, as evidence of confusion, or perhaps less so, as a sign of a willingness to explore what could, in any event, never be a settled question. This fluidity does not mean the abandonment of everything that in the past gave Marxism its distinctive character. In asking critical questions about what remains, however, the discomfort level rises somewhat, particularly when one is forced to re-examine one's position on what in the past would have been dismissed as bourgeois ideology in motion. To take but one example, Wright's discussion on the merits and demerits of methodological individualism, a stance and approach which in the past would have aroused contempt if not overt hostility, suggests that it is worthy of a more sympathetic hearing than it has had from past generations of Marxist scholars.

An elaborate *mea culpa* is not called for - the point being made is simply that some of the old ways will not do. To say that Marxist theory needs to rethink certain of its categories is not to lapse into a passive acceptance of the convergence theory so often articulated by the NPI⁵ -

3 Karl Kautsky, one of the most prominent Marxist theoreticians of the early part of this century, stood more or less alone in his rejection, from a Marxist perspective, of the possibility that the Russian revolution, based as it was on "... terroristic expropriation and super-exploitation of the peasants" did anything other than fly in the face of "... reason, democratic values, and most important of all, Marxism." For Kautsky:

"The Soviet Communists, instead of recognizing that conditions were not ripe in Russia for a rapid transition to socialism and seeking to adapt their policies to the real level of development in their country, were simply rushing headlong into a world of catastrophic fantasy. This was indeed, in Gramsci's words, the 'revolution against *Das Kapital*'." (Wollen, 1993, p87)

Kautsky was openly critical of the path taken by the Bolsheviks from the very start, but for reasons that do not concern us here, was marginalised both by renegades within the Communist movement like Trotsky and Luxemburg, as well as by the group later described as 'Western' Marxists, amongst them Karl Korsch (Wollen, 1993, pp86-87).

4 At very least, Marxists should share a commitment to a materialist approach to understanding social reality, they should embrace the concept of class, even as they acknowledge difficulties in operationalising it, and they should also pay more than mere lip-service to the importance of history. Above all, they should be critical - not merely sceptical - but critical in the sense that no statement, no concept, no statistic even, can ever be taken at face value - no single piece of knowledge can be appropriated until such time as it has been screened through a conceptual grid to establish its adequacy.

5 Typically in statements of the following form:

"Employers and employees always strive towards a common objective, i.e. to achieve a higher standard of living...In South Africa however, the emphasis in labour relations focuses on re-distribution of income rather than on the generation of additional income. This leads to conflict and financial losses for both parties. Such confrontational labour relations lead to the re-distribution of poverty instead of the re-distribution of wealth." (du Plooy, 1988, p14).

Crudities of this kind drew on the tradition referred to in the previous chapter, one which proposed the convergence of industrial societies towards similar ends. This was the 'convergence theory' of the NPI.

there remains a genuine problem, capitalist domination and exploitation, which can only be grasped by Marxist theory.⁶ Bourgeois economics may well be capable of generating similar hypotheses to those used in this study (and *vice-versa*), but the fact is that for the most part it has not, and, generally speaking, and looking more broadly at the things with which Marxists concern themselves, it does not. The question then is - why not? One answer may be that bourgeois economists consider the interests of Marxists either trivial or misplaced. Although this should be taken seriously, it cannot be a complete explanation. This chapter seeks to explore the problem of what it is about the social scientific enterprise that makes it possible for the holders of competing views to avoid the criticisms of the 'other side'. In particular, it seeks to understand why is it that the phenomena researched and reported on here have hitherto been what Wright *et al* would describe as a 'non-event' (1992, p147).

Competing paradigms: the importance of primary allegiances

The research in the part of the study concerned with errors in the manufacturing statistics owes its existence to the discovery of an irregularity, that in the part concerned with the NPI's analysis of productivity statistics is due to what has been argued to be the existence of a predictable and regular phenomenon, namely, the slanting of economic analysis to reflect a particular viewpoint. Evidence of this bias comes readily into focus when viewed through one kind of theoretical lens (Marxist political economy). Since other theoretical frameworks have not generated research agendas concentrating on the same phenomena, the exclusive use of the approach would seem to be justified. It would be a relief to be able to conduct the research without having to consider every other theory that may possibly be applied to the phenomenon. How reasonable is such an aspiration?

If 'theory' is taken to mean the broad framework or orientation within which a social scientist, or more narrowly, an economist works, then, following Cole, Cameron and Edwards (1991),

(continued)

them, a new, and far more confident set of assertions began to be made - namely that the combination of liberal democracy and capitalism marked the end of historical evolution. The most powerful work in this vein is Fukuyama's *The End of History and the Last Man* (1992). But Fukuyama's work is deeply flawed - to name but one problem identified by McCarney (1993), Fukuyama has no theoretical response to the distinct possibility that "...the end of history thesis will stand as long as authoritarian is substituted for democratic capitalism as the final form of human society." (p47) A reading of McCarney's thoughtful critique is a useful antidote to the hype with which *The End of History* was greeted.

6 The ultimate demise of the previously 'already existing socialist states', an event whose rapidity surprised everyone, added momentum to the major re-evaluation of Marxist thought already underway before the collapse - a re-evaluation necessitated in part by the resurgent conservatism in most of the advanced capitalist economies. Out of this has come an impressive flood of thought, of which Erik Olin Wright's work is a good example. For a solid defence of the good that there is within the classical Marxist approach, and an energetic attack on ex-Marxists attempting to rationalise their abandonment of a previous commitment to Marxism, see the review by Geras (1987) of a book by Laclau and Mouffe, their response to him (1987) and his reply (1988).

three major frameworks may be distinguished - the subjective preference approach - this includes all of theories loosely described as neo-classical; the cost-of-production approach - this includes Keynesians, post-Keynesians, neo-Ricardians and the like, and finally, the Marxists.⁷ Obviously, within each framework there are many variants, but the distinguishing characteristic of each is a set of core propositions about the way the world operates.⁸ Equally obviously, the categories are not watertight - there is sufficient eclecticism and borrowing between frameworks to make all manner of combinations of views possible.

In general, it seems that most researchers conduct their work within the theoretical framework to which they owe primary allegiance - usually without reference to the competing frameworks, or paradigms, as they are sometimes called. Only in cases where competing views from different paradigms are expressly compared⁹ does one find details of the alternatives being spelled out - such enterprises are the exception rather than the rule. Few researchers, it would seem, conduct projects in such a way that all or most of the possible approaches are taken into consideration.

Those among us who see themselves as more practical (and less 'ideological') may argue that a test of the validity of the decision to use a particular theoretical framework would be the question of whether or not it has allowed for additions to the stock of knowledge, and whether or not more knowledge could have been generated if some other body of theoretical work had been adopted. Without conducting an extensive comparative exercise, it is not possible to perform the second of these tests. Clearly, unless competing theories are all granted roughly equal space, this latter question cannot even be formulated, let alone answered. If such an exercise were to be contemplated, however, a further problem would need to be confronted, namely that different theoretical approaches do not easily permit the same questions to be asked of social reality.¹⁰ This characteristic of 'actually existing theory' militates against the unproblematic adoption of an eclectic or agnostic approach.

7 A similar view is advanced by Coats (1991, p121) in an article on economics as a profession. He notes that there are:

"...not one but three distinct professional economic ideologies or world views - the conservative, liberal and radical versions - each with its own ideological assumptions, theories and policy preferences..."

8 Neoclassical theory is characterised by a methodological individualism verging, in its more extreme versions, on atomism (a stance that denies that relations are ever genuinely explanatory) and by domain assumptions like the maximisation hypotheses. Central to Marxist theory is the labour theory of value, and a commitment to the salience of class struggle. As noted above, the phrase 'methodological individualism' may be used as a term of abuse, but it is a complex concept which cannot be allowed to be monopolised by neoclassical economics (Wright *et al*, 1992, Ch. 6).

9 One example is Glen Cain's (neo-classical) assessment (1980) of segmented labour market theory. Another, more prominent case is the 'Two Cambridges' debate over the theory of capital - see Harcourt, 1972.

10 It is worthwhile recalling at this point, the review by Frantz (1992) of Leibenstein's X-efficiency postulate, and his remarks about the invisibility of the effect in certain theoretical models, which, being hostile to the notion, cause it to disappear by virtue of the assumptions used to build their models. See Footnote No. 11 in Chapter 1-2 (p17).

Clashes between paradigms are fairly common, and it seems difficult, if not impossible, to resolve what is sometimes referred to as the 'competing paradigms problem' in the social sciences. This has the uncomfortable implication that, strictly speaking, one's choice of overarching theoretical framework needs to be justified with every new piece of research published. In practice, such a tiresome and onerous duty is pragmatically neglected. Unless a confrontation with a competing model is contemplated, researchers working within a particular world view tend simply to use the core propositions appropriate to that view without further ado.

Without at this point wishing to enter the debate about why certain theoretical models dominate discourses at particular historical junctures, one can say with little fear of contradiction that the view broadly known as the neo-classical is dominant in economics. The merits of this may be, and indeed are questioned,¹¹ but it is undoubtedly the case that the schools of economic thought that trace their lineage back to the classicals are somewhat marginalised - post-Keynesians, neo-Ricardians and Marxists relate to the mainstream of the discipline much as reflexologists do the medical profession. Doubtless those of neo-classical persuasion would want to claim that this demonstrates the superiority of their own theoretical apparatus. Dissenters are more likely to view it as a reflection of the prevailing political climate, with worker organisational strength threatened everywhere, the socialist experiment in ruins and social democracy in retreat.

Even under the spacious rubric of neo-classical economics, the particular position taken would have to be defended. What could remain unspoken, however, is the set of assumptions undergirding the approach, broadly conceived. Given the dominance of 'conventional' theory, it seems unlikely that an analysis located within one of the dissenting theories, destined for dissemination outside of the circle of like-minded economists, could pass muster without a good defence of the decision to use the approach. Unfortunately, the 'competing paradigms' problem - which can be neither avoided nor easily settled - affects the proponents of the mainstream and dissenting views differentially. In short, mainstream economists simply ignore the problem.

11 The case against neoclassical (conventional or orthodox) economics (like that against Marxist economics) has been put innumerable times, and over a long period. Choosing the appropriate argument to bring to bear on the matter is one of the rewards the profession - much as somebody once described academic conferences as the 'leisure of the theoried classes'. What opponent of orthodox theory could fail to delight in a statement like this one by Clower?:

"...our most powerful contemporary tools of theoretical analysis appear to be applicable only to economic systems that are devoid of just those institutional features that seem to lie at the heart of our most pressing practical problems." (1988, p96)

An analysis which captures this superbly may be found in a review, from a Cambridge (UK) perspective, of the papers that emerged from a conference celebrating 50 years of economic measurement (Berndt and Triplett (eds), 1990) - almost all of them written from within an exclusively conventional, or neo-classical stance. The review, by Harcourt and Kidson (1993), is full of praise for the virtuoso performances of many of the participants, but points keenly to the exclusion from consideration of anything other than conventional economics and the statistical concepts it generates. In their words:

"A feature of virtually all of the chapters in the volume is the complacency of mainstream economists. The large body of economists working outside, or not solely within, the neoclassical paradigm either gets no mention at all, or are summarily and contemptuously dismissed. Thus those economists sceptical of the aggregation of capital goods, or labor economists working within an institutionalist framework, are largely ignored." (Harcourt and Kidson, 1993, pp437-438).

If the purpose of writing this section were to respond (defensively) to the claims of the dominant school in economics, then one may as well spare oneself the effort - the debate between the competing paradigms has raged (sometimes quietly) for over a century - to use McCloskey's unhappy phrase, it undoubtedly will drone on for a while more. In the long citation from the Harcourt and Kidson piece which follows, the reason for hammering away at the (several) monolithic structures of neo-classical economics is made clear. They observe that:

"The point we wish to make is that [the] surveys [in Berndt and Triplett] are workmanlike efforts *if* initially it is accepted that the main processes in economic life are captured in a basically Fisherian world of intertemporal consumption, saving and investment behaviour (or even a simple extension of J. B. Clark's theoretical vision to applied work) so that all of the statistical data which is used may be taken as reliable quantitative expressions of the concepts of this theoretical approach. Solow put this very well in his exchange with Anwar Shaikh concerning the humbug production function: "It merely shows how one goes about interpreting given time series if one starts by *assuming* that they were generated from a production function and that the competitive marginal-product relations apply" (italics in original). *If* this is accepted, it is possible to assess how good the statistical methods are for getting reliable measures of the orders of magnitude associated with the variables and the parameters of the assumed interrelationships between them over time and place. What is missing is any discussion of whether there are ways of testing whether the statistics thrown up in the data may have been generated by entirely different economic processes to those modelled. (Also, the theoretical approach taken imposes conceptual meanings on the variables in the theoretical relationships. This, in turn, raises the question of whether the available statistics are suitable for matching, even at several removes, the theoretical concepts. Applied economists, even those in this volume, often forget to ask if the conceptual categories which define the measurement of statistics are the appropriate ones for modelling the underlying economic processes.) That this should be so after 20 years of the Western World lurching from one crisis to another (having been preceded by the Golden Age of Capitalism which on the face of it, was the outcome of the view of "economic processes" least likely to be considered or accepted by the authors of these surveys) is an index of the extraordinary hegemony that characterises the mainstream practitioners in the U.S.-and, no doubt, elsewhere as well." (p438) (Emphasis in original)

It would be overly generous to characterise the interventions of the NPI as being in any way theoretically sophisticated. Yet mere incompetence is of little relevance to the success or otherwise of a propaganda campaign - as little, one suspects, as does being in the right serve to

guarantee that one will prevail. The implicit commitment within the NPI analyses to standard neoclassical propositions about the nature of economic process finds deep resonance with more skilled practitioners. That the repeated findings of the NPI and the, at times, almost unbelievably poor statistical base on which they have been constructed have never been challenged in a sustained manner by anyone else (the 'non-event' referred to above) is important. It may not be proof of agreement - acquiescence may merely reflect an ordering of priorities of busy economic researchers. To me it seems likely, however, that the low priority accorded a critical analysis of the outpourings of the NPI is at least partly explained by the dominance of the neo-classical paradigm (in 'theoried' circles) in South Africa. This world view does little to discourage the acceptance of findings not uncongenial to the ideological commitments of its proponents.¹² Alternative perspectives may not feature despite the analytical richness they offer. As Harcourt and Kidson observe:

"The Cambridge approach to applied economics...stresses the limitations of much of orthodox neoclassical theory, however elegant, in explaining economic phenomena in the real world. Instead, it emphasises the importance of relevance in economics, incorporating the lessons of history, the institutional context and prevailing social and political conditions. Theory and measurement are thus mutually dependent as robust empirical analysis is dependent on relevant theory, which in turn depends on reliable observations. Cambridge advances in theoretical and applied economics have, therefore, gone hand-in-hand. Furthermore, techniques have never been allowed to obscure the analysis - the medium is not the message." (1993, p437)

"The neoclassical approach...provides clarity and internal consistency. An alternative Cambridge approach, sceptical of the ability of markets to clear, would more readily accept that individual and collective actions are affected by institutions and political and social forces. The resulting picture of the world that emerges may be less defined, but also perhaps less distorted." (p446)¹³

Of course, as anyone who delved into the murky debate on value theory¹⁴ Marxist theory goes even further in this direction. There is a pressing need, although many economists may not acknowledge it, to choose between competing frameworks.

12 A carefully-documented analysis of a similar failure on the part of mainstream economists to challenge crude popular misrepresentations of British worker productivity may be found in Nichols (1986).

13 For a display of complacency (rooted in ignorance?) see the article by Bleaney and Stewart (1991) that celebrates the fundamental unity of the discipline based on its shared "common ground". The passage that follows provides a sample:

"The eulogy of markets in which economics indulges [therefore] often appears one-sided and ideological, and it is the emotional reaction against this which underpins self-styled 'radical' schools such as the post-Keynesians. It is by no means clear that these unorthodox schools actually reject [these] propositions about the efficiency of markets, or that their positive theorizing is based on radically different premises, but they certainly wish to place emphasis on other issues."

At that point in the text there is a note which reads as follows:

"It is difficult to think of an experiment that would test the true attitudes of radical economists. If all orthodox economists were wiped off the face of the earth, it is quite likely that post-Keynesians would find themselves filling the vacuum that was left as exponents of the virtues of markets. Moreover, radical schools of thought often attract people who are critical of mainstream economics for different reasons, such as their dislike of the extreme assumptions involved in mathematical models." (Bleaney and Stewart, 1991, pp729-741)

14 References to some of the seminal works in this field, and to recent contributions to the debate are given below.

Competing paradigms: Free to choose?

Suppose for the moment that all of this were not an issue - that the question of choosing a theoretical framework was unimpeded by such considerations. Choice presents itself both as an opportunity and as a compulsion - this is perhaps best summed up in the existentialist aphorism about freedom being a situation in which one is forced to make choices. When it comes to the choice between competing theoretical models, the process suggested above of consulting all relevant theories to see which best fits the 'facts' gathered could be claimed to be a way of reducing compulsion to tolerable levels. That may be what is intended by a literature review, but if so, that is certainly not what has been done in this study. One wonders indeed whether it is ever done in a manner that would satisfy a stickler in these matters? In most work, competing theories may be referred to in passing, but seldom is any attempt made either to present or to defend them in a way which would satisfy supporters of those views.

To the extent that I have succeeded in revealing bias in the workmanship of the NPI, I would argue that I have been able to do so because I approached the problem from a Marxist standpoint. Using a Marxist framework rather (more) than any other, does not imply that Marxist theory claims any sort of monopoly on the truth of social reality, whatever that is understood to mean. The approach has been used primarily because, apart from my commitment to it, it has furnished the organising framework (theory and hypothesis) which makes the relevant phenomena both visible and comprehensible.¹⁵ This single-mindedness may be viewed as a problem, but I would argue that it is one which plagues the social sciences more generally, namely, the difficulty of justifying the use of a body of theory (Marxist or any other) without getting bogged down in a never-ending wrangle about the relative merits of all relevant theoretical approaches.¹⁶ That this problem is not generally acknowledged to exist does not render it any less a problem.

Part of the wrangle would take place over topics for which Marxists argue that conventional theory cannot provide satisfactory answers (income distribution and the wage/productivity relation), or in areas where conventional theory has not distinguished itself (institutions, power

15 Nailing one's theoretical colours to the mast inevitably exposes one's politics - it is with great difficulty that one attempts to imagine a Thatcherite disapproving of the market, or a Marxist not supporting a call for socialism (a political arrangement whose possible forms are currently the object of heated debate). This carries with it the risk that the work in question will be rejected merely because it belongs in this or that theoretical camp. This is unfortunate, if only because it is bad manners not to hear the other side.

16 One can only speculate on the question of whether a strongly committed 'free-marketeering' economist tackling the research questions posed in this study would have included Marxist or even post-Keynesian theory in their review.

and ideology).¹⁷ To establish this, however, one would have to undertake a review of all of the appropriate literature - and then engage in the unsatisfactory business of arguing that the evidence available, some of it quite slender, best fits this or that theory. As so many authors have noted, the instruments available to economists to establish the superiority of one proposition over another are quite coarse. On this head, Solow observes that:

"A modern economy is a very complicated system. Since we cannot conduct controlled experiments on its smaller parts, or even observe them in isolation, the classical hard-science devices for discriminating between competing hypotheses are closed to us. The main alternative device is the statistical analysis of historical time-series. But then another difficulty arises. The competing hypotheses are themselves complex and subtle. We know before we start that all of them, or at least many of them, are capable of fitting the data in a gross way.¹⁸ Then, in order to make more refined distinctions, we need *long* time series observed under *stationary* conditions." (1985, p328)

Unfortunately that is precisely what a dynamic economy does not provide. Even if the data were available, the strong likelihood is that their quality would be suspect. As Denton puts it:

"Definitional problems aside, [the] measurement processes are unavoidably imprecise, and sometimes downright horrible." (1988, p168)

Even where the data are relatively good, and (or especially?) where sophisticated econometric techniques are applied to the data, attempts to test hypotheses are inconclusive or suspect in some large number of cases.¹⁹ The abuse of econometric tools is said by some to be widespread - on the basis of a survey that he conducted, McCloskey (1985) claimed that:

17 This is not to suggest that Marxist attempts to develop a theory of power in the marketplace have been successful - Bowles and Gintis state plainly that:

"... the left lacks a compelling account of the exercise of power in the economy, never having convincingly responded to the proposition that in a system of voluntary contractual exchanges no agent has power over any other simply because a buyer or seller can walk away from any transaction with impunity." (1990, p165)

The important thing though is that Marxists are aware of the importance of power in economic analysis - the work by Bowles and Gintis is an attempt to fill the gap.

18 As an instance of this, consider the phenomenon of the productivity slowdown that allegedly affected the US economy from the early 1970s onwards. There is general agreement that a slowdown did indeed occur and the topic has been heavily researched. Yet Michael Darby claims that:

"...the productivity panic [as he calls it] is based on statistical myopia, and that a careful analysis within the perspective of the entire twentieth century discloses no substantial variation in what is variously described as growth in total factor productivity or technical progress." (1984, p301)

Even in countries such as the US where statistics are comparatively good, interpretations are contested. The crude measuring instruments available require massive swings one way or another before everybody is convinced, and even then, causes remain in dispute.

19 For an overview of the taming of the ambitions of the two arch-high priests of 'positive economics' - Lipsey and Friedman - see the article by Evans (1991). This piece of work charts the decline in the hopes originally entertained for 'positive economics' on the basis of the growing sophistication of econometrics. From being viewed as a tool which it was thought would make falsification or refutation of hypotheses possible, years of manifest failure to do so saw the "vision of resolving disputes" being replaced by an "emphasis on verification and forecasting". In practice, econometricians soon learned to be content if evidence was consistent with hypothesis, and if forecasts were not "widely out of line with actual outcomes." (p51)

"Roughly three-quarters of the contributors to the *American Economic Review* misuse the test of significance. They use it to persuade themselves that a variable is important..." (p201) by allowing "...statistical significance to do the work of substantive significance..." (p204).

Settling disagreements in economics (and the social sciences more broadly) is a painful and protracted business.²⁰ The difficulty, not to mention the expense, of creating new economic data, and almost universal access to high-powered computers has encouraged instead the proliferation of what is known as 'data-mining'. This is the term used by Denton (1988) to describe 'unreported specification search'. He notes that "...specification search" over alternative models using the same (or at least not independent) data sets...has long been known to distort standard hypothesis tests" (p176). Difficulties can arise even if the search is reported by the author, but when it is not, the problem, although not apparent, may be much worse.

A trenchant criticism of the way in which the well-known problems of hypothesis testing can be suppressed by prior beliefs (in the case considered, in favour of monetarism and against Keynesianism) is given in Cooley and LeRoy (1981). The ability to do so using apparently respectable econometric tools is traced to its roots in the difficulties faced by those branches of science in which controlled experiment is not possible (p825).²¹

Policymakers make economic policy, with or without 'good' economic advice. Apart from the ideological bias visible in the previous example, the fact is that there are often times when several theories are compatible with the available data. This issue was tackled recently by Becker *et al*, who observe that:

"An important problem in macroeconomic policy design that has hitherto received little attention is the uncertainty of model structure. By that we mean that more than one model fits the data reasonably well. However, each model is based on a different school of thought and thus has a different structure. Hence our analysis is based on non-nested models, when statistical inference cannot reject one model in favour of another." (1986, p425)

20 Challenging findings generated using a particular theoretical approach is usually a tiresome and lengthy, if occasionally necessary affair. Anyone wishing to challenge those presented here can do so by the usual methods either of deploying an alternative model and attempting to demonstrate the greater validity of their own interpretation, or by showing that the chosen approach used has been used incorrectly.

21 When adherents of two opposing theoretical (ideological) positions battle it out between the covers of reputable journals such as the *American Economic Review*, false reports of the verification of the hypotheses of one side give that side an unjustified advantage. Yet there are in truth few compelling arguments that could persuade an audience to choose one in preference to the other. As Eichner and Kregel argue:

"...the quite different purposes that post-Keynesian and neoclassical theory are meant to serve makes even more problematical the already difficult task of determining which of these two models is the better one for analyzing a modern, market-oriented economy. As Kuhn's work brings out, it is difficult to choose between alternative paradigms - especially when the newer one is still in an inchoate state - even if there is agreement that the purpose of a theory is to explain the empirically observable world. When there are two alternative paradigms, each designed to serve a quite different purpose, the task of choosing between them is further complicated." (1975, p1310)

Using an optimisation algorithm, Becker *et al* test two different models for the UK, HM Treasury and the NIESR, for the impact of using one model when the other turns out to be closer to the real world. Optimal policy based on a single (incorrect) model turns out to have worse effects than even a 'do-nothing' policy. When a 'combined optimal policy' drawing on both models is developed, substantial gains relative to the single model result, and losses associated with the combined model were small compared with what a single-model policy would have had if it were the correct one (1986, p446). Convergence theorists might wish to interpret this result as evidence that bridging the 'competing paradigms' gap is possible. Before that conclusion can legitimately be reached, however, there is a long road to travel. The artifices under consideration in this experiment were macro-models in which the political variables could be relatively painlessly moulded to produce an eclectic solution. It is a long way from laboratory experiments like this to the rabid anti-workerism of the likes of Thatcher. Some indication of the distance may be gained by calculating the effort required to persuade someone of her ideological stance of the virtues of Keynesian economics, or better still, of Marxist economics.

Compounding the problem even further is the fact that in many instances, what is sought (evidence of a particular economic ideology) cannot be revealed solely by statistical analysis - much of the evidence is textual in nature. That is certainly the case in this study, where some of the work has had to be carried out in the ill-charted territory between text and context. To be sure, the texts are based on economic analyses, but the particular twist given them is the ideology in operation. All in all, it has been easier, at least in the formative stages of the study, to eschew conventional theory, announce a limited aim, provide a brief review of the necessary theory (Marxist), and then offer (limited) evidence in support of the claims made.²²

In defence of this approach and to avoid being obliged to engage in a fruitless search for an answer to the frustrating questions posed by the absence of a satisfactory means of choosing between competing theories, it is useful to stand back from the problem, and by tracing it to its roots in the metaphysic underlying all theoretical positions, show that since no theory can lay any *a priori* claim to superiority, any of them can be used without reference to the other. To say this is not to deny that there is some, maybe much, shared knowledge. To start with, this will be ignored in favour of a concentration on the different findings that different perspectives make possible. The question of shared knowledge will be returned to towards the end of the chapter.

²² It is unfortunate that the truncated version of the NPI story presented in Chapters 1-2 and 1-3 has to stand on the somewhat slender merits of the space-constrained evidence presented.

Competing paradigms: Do economists disagree?

There are those (and I am one of them) who would argue there is fundamental disagreement between economists. The different schools of thought certainly appear to be characterised by distinctive core propositions, which sometimes take the form of 'domain assumptions'. Yet if Milton Friedman were correct about the relative unimportance of assumptions, given their necessarily abstract and hence unrealistic character, the distinctiveness of different theories would lie not in the character of the assumptions but in the (differential) ability of theories to predict.

What is important about economic theories, however, is probably not the limited and technical way in which they deal with specific phenomena (by a rather nice coincidence, Marx and Friedman both hold to a quantity theory of money, separated only by the century in which they were 'discovered') but rather by the way in which the component theories (the firm, the consumer) are assembled to form a more-or-less integrated world view. It is within the totality of such a world view that recognisable 'positions' emerge and in which policy is formulated.²³ On this basis, it may be argued that there do indeed exist significant differences between the different schools of economic thought - the gulf between Friedman and Marx is unbridgeable.²⁴

Radicals both to the right (Austrians, libertarians) and to the left (Marxists and fellow-travellers) of the approach conventionally described as neoclassical reject large parts of it.²⁵ These differences deserve to be treated seriously - a clear example of a work that does so is the book by Cole, Cameron and Edwards (1991) cited above. The lines of theoretical cleavage suggested immediately above are not exactly those identified by these writers, but that is not of consequence. What is important is the notion of incommensurability of world views with

23 For a different route into this debate, see Mirowski (1988; 1989). The latter work is an introduction to the confrontation between himself and the work of a 'Whig' scholar who is trying to wish away the substantive differences between classical and neo-classical economics. The 1988 article traces the lineage of neoclassical economics back to the 19th century physics whose methodology it apes.

24 This does not mean that Marxists may not, on occasion, use theory in a similar way to what Friedman has - as Boland (1991) argues:

"...for Friedman the only relevant positive data will be successful predictions which assure the usefulness of a model or theory. There is nothing in Friedman's [well-known 1953] methodological essay that would prevent his form of instrumentalism from being used by post-Keynesians or even Marxists." (p94)

25 Amongst the less tolerant and less well-informed of those holding the 'centrist' views, a tendency exists to dismiss competing theoretical approaches as 'extremist' - pejorative terms, such as 'loony left' (or right) being used sometimes to describe these opponents. Such behaviour is by no means the prerogative of the undisciplined at the centre - one finds instances of similar bad conduct amongst the less-disciplined adherents of almost any world view.

which Cole, Cameron and Edwards flirt,²⁶ and their discussion of the problem of the relativism which arises when choices have to be made between competing theories. Each of the schools of thought they identify can lay claim to the allegiance of its proponents because it meets a set of (minimum) criteria that testify to its scientific nature, and because it provides, as well, reasonable political guidance. In each case, once the mainly unchallengeable core propositions or domain assumptions of the differing schools are accepted, a coherent logical structure can be built that will defy most attempts at falsification. Even the briefest acquaintance with the rudiments of scientific methodology will reveal that in the social sciences at least, once a theoretical framework has been hammered into a clutch of internally consistent propositions and can cope with the real world without the need to invoke too many supplementary or *ad hoc* hypotheses to explain irregularities, it is almost impossible to dislodge from the body of 'science' (Cole, Cameron and Edwards, 1991, p13).

Attitudes towards this in the profession are sometimes ambivalent. Some appear to want to claim that genuine and fundamental disagreements among economists do not exist, or alternatively, that conventional economics is simply so superior to any of its competitors that little debate on the matter is called for.²⁷ A recent survey in the US reported that for "...all important or even relevant issues there is much consensus among economists" (Alston *et al*, 1992, p209).²⁸ The method employed to determine this, a questionnaire covering 40 issues of general interest, had little power to reveal differences in core propositions or domain

26 It used to be popular, at least among Marxists, to argue that 'incommensurability' makes conversations between competing paradigms (or world views) almost impossible. This may be an overstatement of the case, but if meaningful conversations (comparisons) are to take place, familiarisation with what Feyerabend calls the "observation languages" is vital:

"...we must make sure," he argues, "that the comparison is *fair*. That is, we must not criticize an idiom that is supposed to function as an observation language because it is not yet well known and is, therefore, less strongly connected with our sensory reactions and less plausible than is another, more 'common' idiom..."

...a comparative judgement of observation languages, e.g. materialistic observation languages, phenomenalist observation languages, objective-idealistic languages, theological observation languages, etc., can only start *when all of them are spoken equally fluently.*" (1979, p80) (Emphasis in original)

This does not mean that deep differences will not still exist, for not all theories can be correct. It may mean only that the grounds of disagreement have been made clear. Within this context, evidence, and arguments over what constitutes adequate evidence, assume a rôle of great importance. Incommensurability thus refers to the problem of communication before this ground clearing exercise has taken place - it does not mean that debate about competing (and between) 'paradigms', as they are sometimes (misleadingly) called (Elguea, 1985), is (nearly) impossible. Malone (1993) argues that if Kuhn is correctly understood to be claiming this, then he is incorrect. In the field of labour economics, for example, Rebitzer (1991) shows convincingly how conventional theories have learned from radical theories - so thoroughly are some of the insights absorbed that they have now apparently lost touch with their origins. An interchange on the possibility of convergence on certain economic questions when viewed from Marxist and neoclassical perspectives took place recently in the pages of the *Cambridge Economic Journal* between Young (1992) and Green (1992).

27 Not all commentators are of this view - Phelps Brown (1972) expressed dismay at the smallness of the contribution of economics to the most pressing problems of the times. He was also deeply critical of the premature mathematisation of the discipline.

28 There was some suggestion of ossification of views in the survey's findings in that in 16 of the 40 issues covered, the less recent the vintage of the highest degree held by the economist in question, and the less actively engaged they were in research, the more likely they were to tend towards conservatism.

assumptions. Fundamental questions such as the underlying social model adhered to - conflict or consensus - were not addressed.

A tendency towards homogeneity or coalescence around a received view in the US was remarked on by Frey and Eichenberger (1992, p216) who report widely different views on a range of issues as between US economists on the one hand, and Europeans on the other. This is (partly) explained in terms of the different markets in which the two groups work - the large and relatively uniform US environment encourages theoretical specialisation, especially in the dominant neoclassical paradigm, at an abstract, non-institutionally-specific level, while the more fragmented and differentiated European market stimulates theoretical broadness and institutional specialisation. A study of the views of British economists (Ricketts and Shoesmith, 1992) found important inter-country differences in responses to the questions common to studies in other countries (p211). An attempt to examine levels of agreement with normative as opposed to positive questions tended to confirm the US finding that there were significant differences. In its examination of the rôles of 'science' and values (attitudes and empirical findings) in policy judgements Ricketts and Shoesmith reported a great deal of variability in attitudes to policy propositions which seemed to imply that these might well "...determine beliefs about empirical relationships rather than the other way round" (p214). Another interesting aspect of the British findings was the greater sympathy with "...income-redistribution objectives...", and the greater willingness to "...countenance government intervention to achieve them..." than in the US (p212).

Although there is reference in the studies cited above to some similarity with the findings of earlier surveys, one discovers, upon scanning the literature, evidence of an apparent and repeated need on the part of eminent economists to reassure practitioners that perceived divisions in the profession, particularly insofar as these perceptions are held by non-economists, are not fatal. This need seems to express itself with most clarity at times of economic crisis. To illustrate this, consider the following example drawn from the mid-1970s, a period in which inflation threatened to undermine the central institutions of capitalism. In his presidential address to the American Economic Association, Kenneth Arrow claimed that "...[n]o really cohesive alternative which aspires to the same level of completeness [as the much-abused neoclassical theory] exists..." Arrow went further - identifying problems such as the rapidly rising relative cost of medical services in the US after 1967 as being matters which he could not imagine how a Marxian or neo-Ricardian would even approach, adding that he thought they might dismiss the question as unimportant. Arrow's primary criticisms of neoclassical theory, as it stood then, were rooted in a "...need for a better understanding of the economics of uncertainty..." For him, the "...lack of economic knowledge..." could, in good

part, be pinned on the "...difficulty in modelling the ignorance of the economic agent (1974, p1)." The following year, Walter Heller assured his audience that:

"What we know is that beneath the visible tip of disagreement and rivalry lies no huge iceberg of divisiveness." (1975, p4)

The year after that, the address by Gordon to the same learned society concerned itself with two topics - rigour and relevance. By way of introduction to a discussion on the first of these he observed that:

"Many if not most of today's economic theorists seem to agree with [an] ordering of priorities [in which the question of whether or not the real world matters depends on its] relevance to current economic theory." (1976, p2)

Providing several examples to back up his claim, he noted that it was not surprising (at the time) that younger economists turned to Marxist radicalism to address problems like those of the inequality of personal income distribution, which, according to one eminent conventional practitioner would "...eventually receive much theoretical attention..." (p4). On the question of relevance, Gordon, noting that neoclassical economics lacks a "...dynamic, politico-economic, and institutionally oriented model...",²⁹ called on "orthodox" economists to "...borrow what seems appropriate from Marx and his followers as well as from others..." (p11) Gordon considered such adornments to be crucial for answering the large questions like "...what is the future of capitalism and of the kind of market economy to which we are accustomed and which is changing before our eyes?" (1976, p11) In ending his address he asked only that economists subscribe to the credo "relevance with as much rigor as possible," and not "rigor regardless of relevance." (p12)

As is well known, for at least a while, economists supportive of the neo-liberal ideologies known as Thatcherism and Reaganism assumed a position of dominance within the conventional school, displacing for a while other less extreme ('wet') varieties of it. But theoretical dominance in the absence of political muscle is trivial (it usually remains locked up in universities or think-tanks) - the dominance of a right-wing intellectual tradition coincided with the sharp turn to the right as some of the major capitalist countries embraced aspects of the policies advocated for decades by intellectuals (Desai, 1994).

29 The absence of an adequate treatment of institutions in conventional theory is hardly novel. Hick's introduction to his *Value and Capital* (1953), the first edition of which was published in 1939, describes the work as one of "Theoretical Economics", ie, it was done without reference to "institutional controls." Acknowledging the importance of institutions, Hicks claimed that the survey of them was best done by other methods, the responsibility for which fell upon the shoulders of economic historians. But, he noted: "It is only when both tasks are accomplished that economics begins to near the end of its journey." (p7)

Some economists appear to have been less affected than others by the bitter disagreements to which this gave rise. Richard Stone's presidential address to the Royal Economic Society in 1980 suggested the grouping of economists into three types: the speculative, the active and the inquisitive, and had all three types making contributions, "...especially where the lines of communication are kept open." (1980, p723) Stone's optimistic conclusion was that:

"The world may be going to the dogs, but economics is certainly not." (p733)

Phyllis Deane's address to the same society a couple of years later was much less sanguine. Focussing on the distinction between pure and applied economics, and the way in which practitioners of the former attempted to dominate the latter, she drew attention towards the end of her address to the bitter dissension that re-emerged among theorists with the ending of the long post-war boom, commenting that:

"This background noise of dissent dividing leaders of the discipline on fundamental issues does not constitute a comfortable working environment for the would-be *normal* practitioners and there is no lack of external as well as internal pressures to encourage the search for a new consensus. The difference today, however, as compared with a hundred years ago, or even fifty years ago, is that there is now a plethora of 'new theoretical ways of looking at things' and it is not going to be as easy in the 1980s as it was in the 1880s to pretend that there is a hard core of cumulative economic knowledge to which - if it is defined carefully enough - the majority of economists could confidently subscribe. Econometricians are stuffing suitably trimmed empirical data into the new economic boxes almost as fast as the theoretical innovations appear in print, though it is not obvious that the recent explosion of quantitative research in mathematical models has improved the explanatory or predictive power of the economist's 'engine of thought'.

In the end, however, it is the external pressures for consensus that are likely to be most potent. For economics is more vulnerable than most other sciences to the immediate needs of social policy. ...It is here, in its implications for policy prescriptions that dissension among economists is most disturbing." (1983, p11) (Emphasis in original)

To end this brief foray into the perceptions of respected, or at least, well-known figures in the profession, three pieces of advice from Alice Rivlin,³⁰ concerned about self-deprecation within the profession, and anxious to improve the contribution that economists can make to the process of policy formation:

- i. Economists should put much more emphasis on their areas of agreement.
- ii. Economists should devote more serious attention to the basic economic literacy of the public, the media, and the political community.

30 Rivlin has attracted energetic criticism from the left. In an analysis of the impact which Ross Perot ("an Orwellian voice with a just-plain-folks East Texas drawl") had on the shape of Democratic politics in the US after Clinton had won the electoral race against George Bush, Davis observes that:

"Not surprisingly, the Clinton cabinet is top-heavy with deficit-hawks and admirers of Reagan's New Federalism. In particular, the combination of Leon Pannetti ('time to make sacrifices...cut, not raise public spending' etc.) and Alice Rivlin in the Office of Management and Budget is the moral equivalent of having Perot himself in the cabinet.' (Davis, 1993, p21)

- iii. Economists need to be more careful to sort out, for ourselves (*sic*) and others, what we really know from our ideological biases.³¹ (Rivlin, 1987, pp8-9)

The reflex response of a Marxist to all of this would be to want to conduct an analysis of both the reported disagreements within the discipline, especially the disagreement within the discipline as to whether or not there was serious disagreement, in a materialist and historical framework.³² The different positions taken by the commentators cited above can be traced not only to general conditions in the economy but also to a deeper set of arguments about the nature of social scientific theorising. It is not the intention here, however, to become overly involved in debates that are the substance of the philosophy of science - the limited aim is to argue that underlying every (social scientific) theoretical position is a set of core propositions about the way in which the world is thought to operate,³³ some of which take the form of pre-scientific judgements.³⁴ In passing, it is worthwhile taking note of the debate about the vicious circularity that arises when a first-order discourse, such as economics, attempts to supply its own truth criteria. The traditional escape route from this, an appeal to a higher order discourse such as philosophy to provide the wherewithal for concept-formation and concept-validation (the rôle that philosophy has traditionally arrogated for itself), leads to an infinite regress as the second order discourse calls on a third order discourse to validate its procedures (Gunn, 1991, p194).

Competing paradigms: In search of a metaphysic

It is not difficult to demonstrate that many of the domain assumptions underlying competing views are metaphysical in nature. Paradoxically, the fact that this is so creates space for all

31 McCloskey (1983), acknowledging that economists disagree, states that the extent of their "disagreements is exaggerated" (p493), and argues that "...a good part of the disagreement is over evidence that is not brought openly into the discussion, though it is used." (p494)

32 Sometime, someone may write an interesting history of the profession simply by examining presidential addresses to learned societies.

33 If one wished to, one could claim, on the basis of an argument used by Marx himself in *The German Ideology* to reject criticisms of the 'premisses' on which his theorising was allegedly based, that Marxist theory is exempt from this need to base itself on prior assumptions. Marx argues that the materialist methodology relies only on the self-evident truth of its starting point, namely that in order to exist, human beings must produce and that to do so, they enter into social relations with others. All else follows from this. Clearly, such a point of departure is very different from bourgeois economics with its utility maximising individuals. For the purposes of the present discussion, the softer argument advanced above can be allowed to stand.

34 Identifying the pre-scientific judgements that underlie assumptions is not always easy. What is certain, however, is that everyone has made these judgements in one form or another. They translate into personal commitments that have an important bearing on research. Whilst it is not dishonest to write in such a way that prior commitment becomes invisible, it must be conceded that the style in which 'academic' work is presented often serves to foster the myth that a value-free science is possible. Since objectivity, though desirable, is impossible to achieve, it seems preferable, in my opinion, to inform the reader of the slant of the analysis from the outset.

approaches to compete on equal terms. One (somewhat unlikely) route into this debate is via the disagreements sparked by Friedman's instrumental³⁵ methodology. Boland claims that it has withstood the test of time (Boland, 1979), and that it is futile to criticise the central hypotheses of neoclassical economics (Boland, 1981), primarily because notions such as maximisation are metaphysical in nature and hence beyond criticism. This presents both a problem and an opportunity - Boland states that:

"Fifty years ago metaphysics was considered a dirty word but today most people realize that every explanation has its metaphysics. Every model or theory is merely another attempted test of the "robustness" of a given metaphysics." (1981, p1035)

Perhaps not unsurprisingly, Boland's work generated energetic responses (Hirsch and De Marchi, 1984; Hoover, 1984). These were brushed aside with the same panache as all other criticisms of Friedman (Boland, 1984; Frazer, 1984). The point is not whether Boland and Frazer are correct about Friedman's use of an instrumental approach for short-term policy-oriented research, nor whether Friedman and Popper agree on instrumentalism - it is rather that once it has been acknowledged that underlying every theory is a metaphysic, it must also be acknowledged that all theories start off on an equal basis. The choice between them has to be made on other grounds. Unlike Friedman, Popper may well have been striving, as Hirsch and De Marchi observe, after "true knowledge" (1984, p784) instead of mere "useable theory" (1984, p785), but even Popper faced the same problem as the rest of us - a criterion for establishing what is true. In practice, for Popper (as for the rest of us) Hirsch and De Marchi argue that this meant that:

"By severe testing of [the vulnerable parts of a conjecture] - testing those **strong claims** for which matters could most easily be otherwise - and failing to reject them we pare down the falsity content and add to the tentative truth content." (p784) (Emphasis added)

For many, perhaps most economists, empirical testing of the predictions of Marxist theory consists in the demonstration of the falsity of the immiseration thesis, or, if they are better informed, in some acquaintance with the literature on the rising organic composition of capital and the tendency of the rate of profit to fall. It is not intended to provide a list of 'successful' Marxist research projects - I will merely point out that the core propositions of classical Marxist theory, those around the labour theory of value, are still under debate.³⁶ Thirty years

35 For an analysis of the instrumentalism of the type advocated by Friedman, see Lawson (1989, p61). Reference is made in this piece to the criteria conventionally deployed (simplicity, parsimony) to choose between models when more than one of them 'accommodate' the empirical phenomena under examination.

36 Seminal works in this debate appear in the collections edited by Steedman (1982) and by Mandel and Freeman (1984). The continuing relevance of the debate for Marxists may readily be seen by turning to a journal such as *Capital and Class* - see for example, the recent piece by Saad-Filho (1993), or those by Rosenthal (1993) and Kliman (1993). Another source is the *Cambridge Journal of Economics*. To cite but two representative pieces - a recent article by Cartelier (1991) attempted to reformulate Marx's theory of value, whilst an earlier work by Szumski (1989) provided a comprehensive and innovative (but possibly non-Marxist) solution to the transformation problem - a perennial issue within the Marxist world view. Like most provocative theoretical work, this elicited a response in due course. It came from Duménil and Lévy (1991). The authors of *The Wealth of Nations* (1991) argued that the transformation problem is a red herring, and that the correct approach is to use the concept of 'socially necessary labour time' to determine the value of commodities. This is a highly controversial position, and one that has been widely criticised. For a detailed discussion of the transformation problem, see the book by Steedman (1982).

ago, Joan Robinson criticised this core, saying that:

"...the whole argument appears to be metaphysical: it provides a typical example of the way metaphysical ideas operate. Logically it is a mere rigmarole of words, but for Marx it was a flood of illumination and for latter-day Marxists, a source of inspiration" (1962, p39).

She did however, show how the metaphysical statements could be used to generate hypotheses (not hypotheses that anyone would care to test, she argued), and she noted that on the scientific plane, the Marxist story offered "...the basis for an approach to the analysis of capitalism." Applying Boland's reasoning, any metaphysic, Marx's included, is useful if robust models or theories can be devised using it as a starting point. We cannot dispense with any body of analysis that meets the minimum criteria necessary for it to be described as scientific using the conventional tools of logic. There is also no satisfactory method for choosing between them apart from their alleged superiority as explanatory tools - precisely the unhappy conclusion reported above.

Enough and more has been said to make it clear that a prescriptive approach to the choice of theoretical framework is neither defensible nor desirable. The choice of theoretical framework cannot be squeezed into a procedural straitjacket called 'scientific method'. A halt to these proceedings can be called by declaring that in the absence of adequate rules or truth criteria, all analytical frameworks and their attendant theories which comply with certain minimum standards meet as equals. An initial separation between them can be made on political grounds using an argument advanced by Wolff and Resnick (1988). They insist that:

"Different social theories matter enormously [but] not in terms of the futile and fetishistic game of asking which one is closer to some absolute truth."

That being so, they argue:

"...we cannot remain indifferent before [the] onslaught of different theorizings, each offering its own knowledge, truth claim, standards of proof and distinctive social consequences. We choose among them not on the basis of a discovered essence, "the truth," but rather because of the different consequences each produces in and on our lives. Because those different social consequences matter urgently to us, so, too, must the theories linked to them.

We find some theories horrific, other magnificent, and still other at various points in between. We form close theoretical alliances with some theorists, while we fear or ignore others. We do this through our theory's assessment of the conjunctural connections between all theories and the rest of the society in which we live. We are not lost in a relativist limbo, but are rather partisans of some theories as against others." (1988, pp61-62)³⁷

37 Arguments of this type do not and cannot satisfy everybody - Hodgson (1982, pp82-85) rejected an attempt by Wright (1982, pp59ff) to employ a similar line of reasoning in defence of the continued use of Marx's labour theory of value.

Similar sentiments are voiced in Cole, Cameron and Edwards (1991). Their stance is that since:

"...each theoretical perspective implicitly serves a sectional interest, all theories are correct *in so far as they further such interests*." (p13) (Emphasis in original)

Mere partisanship in favour of one theory rather than another is, however, clearly not sufficient. Wolff and Resnick may not be adrift in a sea of relativism, as they claim, but there has to be more to choosing between analytical frameworks. In a recent article which attacked the circularity inherent in the 'conventionalist' defence of neoclassical theory (the implied votes of the academic community), O'Brien (1991) argued that:

"Above all, testing is in natural science, and should be in economics, essential in cases of theory conflict. That a particular confrontation between theory and evidence may not be admitted as decisive by everyone is hardly surprising, given both human nature and methodological tenacity, but that such a refusal should be claimed as destructive of the need for such confrontation is really surprising. The precise form of confrontation with the data is a separate issue. All procedures, especially in economics where there is no scientific control, clearly face difficulties. But to claim that because it faces difficulties we give up on it seems to be a form of defeatism leading ultimately only to irrationalism." (O'Brien, 1991, p59)

When all is said and done, he continues, "...any subject with claims to truth needs empirical discipline" (O'Brien, 1991, p60). Following McCloskey (1983) it could be argued that the demands of scholarship require only that one should make (and defend) plausible statements. As economists, we are forced to choose a basis on which to make these statements in situations in which the data are rarely good enough - this is an unfortunate but irresistible fact of social inquiry.³⁸ So too is the fact that in situations where causality is not simple, ie, when events have multiple determinants (are over-determined), one can never capture that reality adequately. That being the case, we are obliged to exercise judgement. In my judgement, the problem of the ideology of the NPI is best tackled using a Marxist approach. Although this judgement rests in part on the partisanship outlined above,³⁹ the use of a framework which sees conflict in the socio-economic system as endemic has generated fruitful hypotheses for which at least some supporting evidence has been adduced. Not only that, the initial Marxist hypothesis (Meth, 1983) has opened up a space in which research into an apparently unrelated field - national accounting practice - has yielded valuable and hitherto unknown facts.

Having argued for an acknowledgement of the explicitly political nature of the choice of analytical framework, it must be admitted (a) that not all economists appear to be aware of the fact that alternative frameworks exist, and (b) that theory does not reveal its political colours

38 It is a sobering thought that statements made by economists (plausible or otherwise), often and unavoidably on the slenderest of grounds, can have consequences well beyond the imaginings of their originators.

39 It also rests partly on a conviction that the approach adopted - a 'realist' Marxism in which a search is conducted for 'tendencies' and 'generative structures' (Lawson, 1989) - is more fruitful than any other.

with equal ease in every field. The aforementioned ignorance results from the dominance of the current viewpoint and the way in which this is maintained, ie, in such a manner as to all but prevent other voices from being heard. The deeper the politics is hidden, eg, in a debate over whether a Cobb-Douglas or a constant elasticity of substitution (CES) production function is appropriate in a particular setting, the greater the likelihood that the conventional wisdom of the time will go unchallenged.

To illustrate the first of these points, two examples below drawn from the South African literature on labour markets, show quite clearly how theory is used, apparently unconsciously, in a way that provides support to the dominant classes in the economy. As far as the second point is concerned, one would have thought that given the long history of strife in the labour market, the politics of labour markets (and hence of wage theories) would lie well towards the highly-visible end of the spectrum. Yet the conventional view, the so-called marginal productivity doctrine, seems capable of submerging the politics of the workplace almost entirely. Notwithstanding the fact that even the economics are suspect - Thurow's critique about the problems of attempting to operationalise this 'theory' was made over a decade ago (Thurow, 1980) - one still needs to be careful not to underestimate the powerful hold that the theory exercises on the minds of those who wield it.⁴⁰

At a very early stage in the debate on the conditions of African workers, Owen Horwood, sometime Head of the Department of Economics in the University of Natal, before going on to become Principal of that institution and then Minister of Finance, made a contribution to a special issue of the *South African Journal Economics* on the minimum wage. In keeping with liberal sentiment of the time, he wrote that:

"The need to raise the wages of many African workers on grounds of sheer humanity is not disputed...[but]...low wages appear more likely to be associated with low productivity, however little that may be the fault of the worker".

Presumably unaware at the time of the criticisms of marginal productivity theory that have now become commonplace, he also observed that:

"However powerful the mines, agriculture and secondary industry may be as buyers of African labour, no reliable evidence has been made available to show that they are 'exploiting' their workers in the sense of paying them less than their marginal revenue product". (Horwood, 1962, p129)

40 In some of the more modern introductory economics texts one finds attempts to defend the marginal productivity doctrine. One example is Sloman (1994, p329). Some of the reminders he offers are timely, pointing out, for example, that a profit-maximising employer is equating MC_L and MRP_L whether they realise it or not. Sloman's treatment of the Thurow-type objections is, however, inadequate. The theory remains impossible to operationalise.

If Thurow's criticisms are justified, then the slipperiness of the concept of marginal productivity is such that no measurement could be devised which would test the proposition. Exploitation in the scientific sense thus remains invisible even though any dispassionate observer must have concluded that the conditions of existence of many of these workers were barely human. The usefulness to the capitalist class of a finding that workers were not being 'exploited' is obvious.

Horwood's effort, although it may raise an eyebrow today, at least had the merit of not importing racism into the argument as well. In this, it differs substantially from the other case to be considered, a paper by the former chairman of the Wage Board. This was also published in the *South African Journal Economics*. The article, by Professor W F J Steenkamp,⁴¹ is a mixture of crude conventional theory, tinged by racism. Two paragraphs from the article suffice to make this clear:

"White labour, as also coloured and Indian labour, cannot always be fully equated with black labour. In the textile and clothing industries, for example, efficient employers will tell one that even after extensive training, the black man tends, on average, to attain only about 75 to 80 per cent of the efficiency of members of the other races. This sort of difference is particularly noticeable in activities such as farming and other types of field work that demand initiative, originality or a zest for work. One could therefore conclude that the labour market is operating efficiently in that it equates wage rate with marginal revenue product".⁴²

Theory, also, fails to take account of the complications that arise when much unemployment and underemployment are found in the ranks of one race (in South Africa the blacks), and full or overfull employment in those of another race (in South Africa the whites). The black man is on that account prepared to perform semiskilled and skilled work at lower wages than those the white man has come to expect. Moreover, he is satisfied with those wages, because they are higher, often much higher, than the pay which members of his race receive for unskilled work. Although one must not underestimate the importance of the 'demonstration effect', it remains true, by and large, that the needs of the black man are generally less extensive than those of the white man". (Steenkamp, 1983, pp64-65)

One would not wish to judge conventional economics by the use made of it by bigots - this passage is quoted simply to expose the politics implicit in marginal productivity theory at its

41 Steenkamp, long-time Chairman of the Wage Board, was at one time paid a higher salary than the Minister of Labour, an indication of the importance of the post he held.

42 This influence of the marginal productivity doctrine is also evident in a report on the (un)desirability of a minimum wage carried out by National Manpower Commission in the early 1980s, partly in response to the growing pressure of the Fosatu Living Wage campaign. At one point this document contains the statement that:

"The MW [minimum wage] determination must not exceed the marginal product of the workers". (RP94/1982, para 7.2.1(b))

Discussing the question of the relationship between wage and marginal product, the report observes in paragraph 3.3.2 that:

"In practice there are many factors that may result in workers not being paid according to their marginal productivity. One such factor is the state of the competition in specific sectors, occupations or areas. The introduction of an MW (*sic*) may also result in certain workers being paid more than their marginal productivity and the converse. This situation obtains in most developing countries".

crudest.⁴³ A competent economist would not accept the idle chatter of 'efficient employers' as evidence without attempting to measure output, nor would they make any assertions about 'needs'. Of course, the fairly unusual combination of capitalism and 'legalised' racial ideology in South Africa tended to throw up extremes, but conventional theory has caused selective blindness in other, possibly equally highly-charged matters.⁴⁴

Competing paradigms: Closing the gap

Defence of a particular world view does not depend on taking obviously inflexible positions (methodological tenacity). The mechanisms can be more subtle. Some insight into their operation is suggested by Leibenstein (1976). Taking the notion that theories possess a *hard core* which is surrounded by a *protective belt*, he argues that the latter:

"...allows for a range of reinterpretation of what [he has] termed "sample relations" in the face of criticism so that the criticism is disarmed..."

43 Work into the operation of the South African labour market has been done recently by the World Bank and the IMF (Fallon (1994), and Lachman and Bercuson, (1992), respectively). Of the two, the IMF effort is the less sophisticated, embodying as it does, propositions such as the following:

"...the amount of income going to white labor can be written as the sum of white skilled and unskilled earnings at their "fair market" wage rate (the wage rate that is equal to their marginal product) adjusted by a markup representing the economic rent associated with apartheid..." (Lachman and Bercuson, 1992, p9).

The IMF paper is an example, albeit unconscious or reflex, of the use of the protective belt around the hard core of conventional theory to reinterpret the sample relation linking wages to productivity so that criticism of the concept can be disarmed.

The World Bank effort, by contrast, uses an efficiency wage approach in which an attempt is made to estimate the impact on the wage of several forces acting to raise or lower it. Direct estimation being impossible, as usual, resort has to be had to a number of proxies (Fallon, 1992, pp40-44). They are clearly spelled out and simple to take issue with if one were to choose to do so.

As shown in Chapter 1-3 (p83), Fallon's attempt to estimate the impact of changing factor prices on African employment levels does not require as baseline, a counterfactual estimate of the number of jobs that could have been created if a competitive labour market (in which wage is determined by marginal product) had existed. He merely estimated the number of jobs that would have been created if the ratio of capital to black workers had been maintained at its 1960 value.

44 Ironically, as Shapiro and Taylor (1990, p865) point out:

"...early development economists did not perceive [the] problems..."

entailed in proposing the state as primary agent of development in the face of pervasive market failures because:

"...they accepted the traditional separation of the economic and political spheres. Basically, they did not contemplate a theory of the state."

One result was that when neo-classical theorists finally got around to developing a theory of the state, an activity almost exclusively dominated by the public choice school, it displayed a marked bias in favour of measures to prevent special interest groups taking over the state. So strong is this that the more extreme proponents like Deepak Lal can be found providing intellectual support for authoritarian regimes. As Shapiro and Taylor observe, Lal's judgement (cited in Shapiro and Taylor, 1990, p867) that in the developing country context:

"A courageous, ruthless, and perhaps undemocratic government is required to ride roughshod over... newly-created special interest groups,"

has become famous. The public choice framework does not automatically and inevitably propel analysts into an antipathy to the state, but as Cullis and Jones (1992, pxi) observe, the school, even though an attractive one, does have an "...almost blanket anti-public sector..." position - one which they seek to counterbalance by attempting to set the public choice critique more firmly "...against the failings of the market".

The technique can be used, for example, whenever the simple maximising hypothesis runs into difficulties. Say firms are shown not to maximise profits, then theory may be recast "...so that firms are now presumed to maximize a utility function in which profits enter as one of the arguments..." (1976, p27). As Leibenstein notes:

"The prediction test has frequently been used as part of the protective belt of existing microtheory. If assumptions are immune from criticism, then one broad area of possible counter examples - those that deal with behavioral hypotheses - are ruled out of court so to speak and therefore cannot be brought to bear in the critical evaluation of the theory." (1976, p27)

Reworking theory "...so that ...faulty prediction no longer emerges..." is a frequently-used device seemingly encouraged by the prediction test. As an instance of the rationalisations made when "...implied consequences of the theory differ from known observations..." Leibenstein cites the refinement of a crude maximand such as wage income to income plus leisure (1976, p27). Another example is to be found amongst those who, like some academics, derive a great deal of job satisfaction from employment which pays well below the market rate for those with equivalent qualifications - academics are argued to be maximising the total of monetary and 'psychic' income. This rescues the maximisation hypothesis, but only at the cost of trivialising it.⁴⁵ Desperate attempts to rescue a body of theory suggest that something more than mere scientific inquiry is at stake.

'Theory', it was stated above, 'does not reveal its political colours with equal ease in every field'. That may well be so, but since, according to the line of reasoning adopted here, theories are usually embedded within a broader analytical framework which almost certainly does have a readily ascertainable politics, particular theories are likely to be stamped with the same politics. There is nothing inevitable about this, and the details of the politics (often subtle) can make a substantial difference to the practicability of what is proposed (implicitly or explicitly). Take, for example, the debate reproduced above about the real wage. The participants agree that the real wage (not defined) should either be reduced or, at best, should not be allowed to grow at more than some rate warranted by productivity growth. The NPI rumbles darkly about the evils of nominal wage growth exceeding productivity growth but avoids being drawn into the specifics of the debate. The economists cited (Gelb, 1988; Holden, 1990; McGrath, 1990) suggest reductions in the real wage, but stay far away from the messy politics of such proposals. Nattrass (1992), by contrast, acutely aware of the fact that militant trade unionists would resist strenuously such a call, goes a lot further and discusses both the social contract necessary to make such a policy feasible, and the increases in the

45 Clearly, excessive use of such rationalisations will ultimately discredit a body of theory. Hodgson (1982, p99) levels this charge against classical Marxist theory, which he likens to the Ptolemaic cosmology. The Sraffian view with which he seeks to supplant it is likened to the heliocentric view of the universe. The Kuhnian influence on Hodgson is obvious in his opening quotation (p75).

social wage required to make it palatable. With the possible exception of Gelb, it is unlikely that the analytical frameworks deployed by the participants in this debate differ greatly. That being so, there is an important lesson to be learned from this interchange, namely that political awareness matters.

Traditionally, conventional economics has distanced itself from radical political economy by its reluctance to admit the importance of institutions and the dynamism of social process in determining outcomes. A harsh judgement of the end-result of such a stance may be found in Douglass North's Nobel lecture. He claims that:

"Neoclassical theory is simply an inappropriate tool to analyze and prescribe policies that will induce development. It is concerned with the operation of markets, not with how markets develop."

North concludes that the framework contains:

"...two erroneous assumptions: (i) that institutions do not matter and (ii) that time does not matter." (1994, p359)

Reading further in the North piece, it is clear that the steps necessary to allow neoclassical theory to obtain a proper grasp of its subject matter entail an abandonment of the elegance of general equilibrium models and other such toys and a descent into the messy world of real human beings who function in a real world. North talks of the virtues of adopting an "institutional/cognitive approach" and observes that:

"There is nothing automatic about the evolving of conditions that will permit low-cost transacting in the impersonal markets that are essential to productive economies." (1994, p365)

In other words, we have come more or less full circle to the Harcourt and Kidson (1993) position outlined above. Not everyone would agree with North's judgement, but conventional economists are going to find it hard to deny the force of his arguments. Speculating on the likely reasons why time and institutions are so conspicuous by their absence in conventional economics, it seems likely that the sheer difficulty of theorising these two unruly variables accounts for their absence. Conventional economics has the virtue of obliging one to search for the 'strictly economic' in a particular social interaction, but when that notion is reified into the form of intellectual practice one sees so often, the outcome is apologetics, not science.

Once it has been acknowledged that the core propositions of analytical frameworks and the assumptions used to construct them matter, it becomes easier to see why economists disagree. If methodological distinctiveness is going to be defended on the basis of the inviolability of the key assumptions, then few meaningful exchanges with others are possible. If, by contrast,

analysts are prepared to relax the often rigid formalism inherent in the ways in which they look at the world, fruitful conversations with economists of other persuasions can take place. Thus although it is difficult to devise adequate criteria for choosing between rival theoretical models, the possibility of reconciling competing paradigms, at least over discrete questions, need not be dismissed.⁴⁶ To admit of the possibility of *rapprochement* is not to endorse woolly-headed eclecticism, nor is it to accept the kind of convergence theory punted by non-conflict theorists⁴⁷ - after all has been said and done, real political differences remain.

Many of the questions posed in this study could, in principle, and as noted in Chapter 1, have emerged from the analytical frameworks that compete with Marxist theory. This is not to say that all of the questions could have been asked. The fact is, however, that not even the most obvious questions have been posed. This has limited debates over productivity in South Africa. Even some left-wing economists have built in to their analyses, the view of the productivity performance of the economy according to the NPI. Speculating on the reasons for this, one might find that the sheer pressure of other, more engaging research topics siphons resources away from the uninviting tasks of poring endlessly over national accounting statistics, looking for inconsistencies, or trawling through newspaper clippings, searching for evidence of 'fragile inferencing'. The critical variable, however, was probably my commitment to a particular politics.

Unpacking this, one is obliged to ask whether there is any substance to Marxist claims of methodological distinctiveness. A careful reading of the work of Wright *et al* (1992), obliges one to concede that there may not be as much of it as has previously been held to be the case. That does not mean conceding the field to the competing approaches. What it entails instead is a recognition that the substantive hypotheses of Marxist theory differ because it does not automatically foreclose on certain avenues of investigation as Frantz (1992) suggested critics of Leibenstein have done. In other words, *pace* Friedman, assumptions, although unrealistic, are often decisive in shaping answers to research questions.⁴⁸

46 Often, the difference between competing explanations lies in the fact that the apparently similar research questions (the objects of explanation) they address are not the same (Wright *et al*, 1992, p134). McCloskey's observation that: "...a good part of the disagreement [between economists] is over evidence that is not brought openly into the discussion, though it is used..." may also be of consequence. The practice, if it is widespread, could serve to widen the apparent gap between competing analytical frameworks.

47 One of my interchanges with the NPI involved a wrangle over the question of whether or not it was appropriate to infer convergence between Marxist theory and neo-classical economics because Lenin and Milton's Friedman had both expressed a concern with productivity. See *Productivity Focus 1989*, p7; *Productivity Focus 1990*, p7; du Plooy (1988, p88); Meth (1991a).

48 It seems unlikely that there will be all that many conventional economists who feel a need to adopt what one might dub a 'hard' Friedmanite position on the importance of assumptions. The importance of assumptions is considered in O'Brien (1991, pp51ff). As regards the question of their 'unreality', O'Brien uses an analogy which likens assumptions to maps - accurate in outline while not reflecting every detail of the terrain (1991, p52). A second property of assumptions, the fact that they embody previous work that has stood up to theoretical criticism (and in the natural sciences, replication), tends to be treated with less than the significance that it deserves, because of the near impossibility of replication in economics (1991, p52).

Political commitment lurks in the core propositions and the protective belt of analytical frameworks and the theory resting within them. As long as there is a tenacious attachment to the core and its guardian belt, talking across the paradigmatic chasm will be difficult, if not impossible. Prior political commitment (on all of our parts) has the effect of habituating us to certain forms of thought and to certain ways of working which have genuine and serious implications for the way in which we do 'economics'. Along the slippery continuum which links our theoretical universes, there is little purchase to be had on any facts - either about class struggle, or about individuals maximising their utility. That being so, the more information we can make available about our pre-scientific judgements and the assumptions about the world that we deploy, the better. When discussion over the validity of these can take place, some narrowing of the paradigmatic chasm is possible - in other words, scope for the reduction of the size of the set containing 'non-events' becomes visible.

So much for the content of the work - form, however, is also important. The final section of this chapter takes a brief look at the problem of form, and locates what I would describe as a conditional victory over the NPI within context of the earlier discussion on the nature of struggles over ideology. In particular, the concern is with the difficulty of dislodging ideological belief with mere science.

Form vs content in the diffusion of ideology

One effect of the NPI campaign to improve productivity in South Africa (a noble aim) has been an undermining of the legitimacy of the struggle by unionised workers for a living wage (an ignoble, even if unintended consequence).⁴⁹ Misrepresentation of the wage/productivity relation has heightened resistance to claims for fair and decent treatment in the workplace, and has almost certainly contributed to the formation of perceptions which Welcher (1991) finds to be associated with the increasing capital intensity of production. The question is, how should the critic of the NPI's polemic respond?

The media, most especially the press, both popular and financial, has reacted warmly to the NPI, granting acres of coverage to its campaign, touting endlessly the message that productivity performance is poor. As far as I can determine, apart from the work done by

⁴⁹ To accept the validity of this conclusion, one needs to accept that the evidence offered in Chapter 1-2 is adequate, or at least that it has the makings of a good case.

Moll,⁵⁰ no critical evaluation of the NPI has been undertaken by anyone other than myself. The research which formed the basis of this study was done only because comrades in the worker movement expressed the view that the burden of their oppression was likely to be increased as a result of the imposition on them of policy allegedly substantiated by the apparently 'scientific research' work of the NPI. They needed to be able to respond. My couching of the initial response in uncouth terms resulted in its being rejected in official and business circles. Apart from any comment that this may make on my approach to the discipline at the time, it raises questions about what the most effective way is to respond to a public relations onslaught, as well as questions about who decides what is relevant and appropriate, and why.

The one thing which was not forthcoming in the early days of this contest was a 'translation' of the Fosatu Challenge into academic language. This may have been a miscalculation on my part, but the subsequent history of the project (its retreat into the more-or-less polite language of 'science') is not such as to permit the drawing of easy conclusions on this head. It seems likely that some of the conjunctural aspects of the relationship between academic work and the class struggles in which that work is launched are the most important determinants of the 'success' (influence) of any particular academic enterprise.⁵¹

To be classed as 'academic', a piece of work must survive the rigours of peer evaluation. Without this, it may be interesting, but it cannot be said to be 'academic', as that term is conventionally understood. The most obvious candidates for inclusion under the heading of 'academic output' - ranked in probable order of professional respectability - are articles published in learned (refereed) journals, books based on 'serious' research, dissertations, research reports and monographs. The pre-eminence of the first of these is referred to by Klammer and McCloskey (1988), who comment that:

"Presumably economic knowledge is manufactured in the 10,000 or so articles catalogued annually in the *Index of Economics Articles*. Economics is a field of articles rather than one of books, although less so than physics, its model beloved from afar. Most of the articles present a fact or two, advise the prince, suggest an easy tool. Probably most of the articles, as is said to be the case in the physical and biological sciences, are wrong or irrelevant. Only a few have influence." (p7)⁵²

50 He makes brief references to statistical problems in his PhD dissertation (1990), but has engaged in extended correspondence with the NPI on the quality of their output (*pers. comm.* Ms K Liebenberg, March 1994).

51 The piece by Desai (1994) on the international success of the right-wing intellectual project is an excellent demonstration of this. From small beginnings in the 1940s, libertarians struggled for decades in a, for them, harsh climate, before the tide turned against social democracy.

52 O'Brien (1991, p55) observes that:
"Scientific activity involves selection of those hypotheses that appear in the journals that will actually stand replication - perhaps as little as 10 per cent of the whole."

One could caricature journals and their referees by suggesting that it would seem that although relevance and/or 'correctness' may be held by some to be desirable characteristics of academic output, their absence is not too serious a handicap. This is fun, but it misses the point that all knowledge is conditional - limited by what can be known at any particular time. Thus academic output must meet the standards of the day, but as is possibly not as well known as it ought to be, both these standards and the perceptions of what is true change over time.

Peer evaluation, a process that extends well beyond the process of refereeing - witness the reviews, responses, rejoinders, replies, ripostes and rebuttals published in the literature, for years (sometimes decades or centuries even) after the appearance of the original - cannot alone produce 'quality'. Time, aided and abetted by political process, will consign to obscurity the true but trivial or merely topical, be it never so highly regarded in its own day. This fact alone should teach humility - the arbiters of taste in academic matters are far from being infallible. This becomes clear as soon as one attempts to grapple with the awkward notion of 'quality'. A piece by Liebowitz and Palmer (1984) attempted to assess the relative impact of economics journals, but these authors, unlike some of their predecessors, gave the notion of 'quality' a wide berth. They note that:

"Since "quality" is somewhat arbitrary and subjective and since it is possible that some of the best quality work exists in a filing cabinet or in a very low-circulation journal, we are reluctant to use this term."
(p78n)

Politics can also bury the deserving, especially when it promotes unpopular viewpoints. Victory in the methodological struggle in economics, won handsomely by what Cole, Cameron and Edwards (1991) describe as the subjective preference school, has ensured, for example, that few articles published in left-wing journals find their way into the citations catalogues that are one of the marks of arrival in professional circles. Although there are some Marxist scholars (and journals) in the US, they form part of a tiny minority, and are unlikely to make much headway against the dominant view. The paper by Frey and Eichenberger (1992) referred to above, hints at some of the possible reasons for differences in views between US and European economists - similar considerations apply to publications and to the creation of ranking devices such as the *Social Science Citation Index* for the recognition of eminence. A mere 25 percent of European, as opposed to 72 percent US and Canadian economists, find their way into the list. This is primarily because of the aforesaid dominance of the received (neoclassical) view.

If one defined academic in such a way as to include some metric of the ability of the work in question to influence broader debates, then the question of what should qualify as 'academic

output' (and why) would be less simple to answer than would appear to be the case in the standard process of peer evaluation described above.⁵³ In the ensuing debate, the question of what the desired end-result of 'academic' activity is, would also inevitably be raised. The 'Fosatu Challenge' can be used to illustrate the point. Written with the explicit purpose of advancing a political position favourable to workers, it was published by a trade union federation, whose successor, Cosatu, is still viewed by many as the embodiment of revolutionary socialist struggle. The accuracy of that perception need not be debated here - suffice it to say that few members of the academic community would disagree with the view that whatever else its merits, in 1983, the then emergent union movement could not possibly have mustered the impartiality required to referee an economic analysis written by one of its sympathisers. Since it was not published where what would normally be considered to be the author's 'authentic' peers, the academic community, would be likely to stumble across it, the piece did not receive the required scrutiny. Being published in the wrong place (a trade union occasional paper series), coupled with the failure to obtain peer approval is more than sufficient reason for dismissing the piece as 'not academic'.⁵⁴

Outside of academe, however, there exists (on the left and elsewhere), a venerable tradition of 'pamphleteering' into which the 'Fosatu Challenge' fits. This 'activist' work takes seriously Marx's dictum that "The philosophers have only interpreted the world, in various ways; the point is to change it" (*Theses on Feuerbach, No. XI*). One major difference between 'activist' and so-called 'academic' work is that the former is overtly rather than covertly political - it is, or seeks to be an input into mass or nascent mass organisations looking to influence policy directly.⁵⁵ Even if not explicitly 'academic' as gauged by the criteria discussed above, this

53 That much of the academic work to survive its way through peer evaluation into the journals is either irrelevant or incorrect, or both, cannot be taken as a license for proposing that adherence to the rules of good scientific conduct be abandoned. A populist, rabble-rousing tract may enjoy great acclaim and exert wide influence and yet be entirely devoid of any validity. It is not suggested that mere influence or the special pleading that advances sectional interests become the arbiter of the 'truth' - rather, it is recommended that the process by which particular intellectual products are incorporated into class struggle be examined more narrowly.

54 Ironically, judged by these criteria, much of the NPI's workmanship would never have survived the ordeal of peer evaluation. The acknowledgement of this by the NPI in their letter seeking a rapprochement with Cosatu (NPI, (June 1992) - cited above in Chapter 1-3, p70) contains as clear an acknowledgement as one is ever likely to obtain from the NPI of a failure to exercise due care when using poor data, and also of wrongful apportionment of the 'blame' for poor productivity performance.

55 Given this, one would expect all but the most abstruse Marxist research to be of an explicitly interventionist character. Marxists call this 'praxis'. It is defined by Cole, Cameron and Edwards (1991) as follows:

"... the nature of intellectual activity is to generalise and abstract from experience, to seek the unifying principle which explains the complex chaos of the concrete reality perceived by the senses in terms of logical relationships which form an intelligible abstract reality in the mind. The nature of political activity is sensitively to develop that abstract reality in the mind as a guide to action in new situations, in which the chaotic appearance has been diminished by the intellect. The desirable combination of these intellectual and political activities into a single process, we can call praxis. The intellectual aspect without the political we could call contemplation, which contains the danger of concepts in the mind becoming fixed and unquestioned categories defining concrete reality. The political without the intellectual we could call pragmatism, which contains the danger of action becoming arbitrary and confused because it is derived from chaos. The test of good praxis is the ability to win through against strong, inevitable, and usually violent, opposition and learn from experience." (p222)

work can readily act as catalyst to further research. Pamphlets, often written under conditions of severe social stress, combine polemic and analysis in varying measures. In left-wing circles, and elsewhere as well, the more memorable of them constitute the starting points of some of the most important debates. The origins of the 'underdevelopment' or 'dependency' school of theorists, for example, have been traced to Lenin's pamphlet "Imperialism: The Highest Stage of Capitalism". According to Warren (1980), although Lenin's results were "...logically and analytically lamentable..." the pamphlet "...did score Marxism's glittering propaganda success of the twentieth century" (p49). Not surprisingly, Warren's arguments are hotly contested, and the debate continues.

It is not suggested that the 'Fosatu Challenge' is even remotely comparable in importance to any work of Lenin's - the point is simply that the challenge stood within the classical Marxist tradition - using analysis and polemic to rebut a particular set of views about the validity of wage demands made by a certain group of workers. The responses it elicited were both 'academic'⁵⁶ and 'ideological', and it functioned therefore in exactly the same way as an 'academic' paper. It may well be that a less polemical stance would have elicited a more considered response, but that is not a foregone conclusion - when there is much at stake, opponents do not readily cede points.

Social science is, by its very nature, an enterprise that aims to persuade by the process of offering evidence and argument (McCloskey, 1983; 1986), but when there is much at stake, interest groups wedded to the received wisdom, especially if it is the dominant view, are unlikely to abandon that view merely because someone has shown it to be of dubious validity. Commitment to a particular theoretical standpoint, implies, as Feyerabend (1979, p25) has so convincingly argued, more than just a set of 'mental software' used to asked questions about the world. It normally also implies an unwillingness to be swayed by mere reason from the beliefs of one's choosing, especially when those beliefs are connected to one's material well-being. This is one of the reasons why debates in the social sciences 'drone on for centuries' (McCloskey, 1983, p515). It is also one of the reasons why academic output is treated with contempt in some quarters.

As matters presently stand, changing circumstances have brought with them a change in fortune for the participants in the struggles recounted in these pages. In the past, officials of the NPI, in an attempt to establish credibility amongst critics, proclaimed a willingness to listen to any and all criticism, but most particularly, to what they were pleased to call 'constructive criticism'. All too often, such sentiments are mere window dressing used to

56 It is cited (critically) for example, in Terence Molls' *Review of Income and Wealth* paper (Moll, 1992).

disguise a commitment to precisely the opposite of such a willingness. In Part II of this study, some attempt is made to show how a commitment of this sort on the part of the CSS worked its way out in practice. Until relatively recent times, the NPI has responded in a not dissimilar fashion.⁵⁷

Of late, however, the NPI has become even more anxious about securing co-operation from the major trade union federations, most notably Cosatu. The changing configuration of political power has been a major factor in this, but the way in which the change in attitude actually occurred is instructive. All government and quasi-government bodies have had to accommodate themselves to the reality of the new South Africa. For some far-thinking individuals, this process started years ago - others have been less imaginative. The NPI has done some major internal restructuring, but has yet to place on the table any proposals for restructuring its advisory and controlling structures. There has apparently been a shift in attitude of the management structure of the NPI to the point where the criticisms of the institution and of the way it makes use of the data it uses and produces are now publicly acknowledged to be of substance, but this shift seems to have come about largely because of the initiative and perceptiveness of a single individual.

Ms K Liebenberg, compiler of the NPI's flagship publications - *Productivity Statistics* and *Productivity Focus*, having read a number of published and unpublished works of mine, and having solicited and absorbed a series of critical but constructive suggestions from Terence Moll, has agreed that there is a case to answer and has set about doing so. Thus *Productivity Statistics 1994* contains a reference to the revised output estimates presented in this study, along with a comment noting that:

"...[Meth's] objections have not been in vain, as the CSS has revised its PVMP [physical volume of manufacturing output] series and published a new set of estimates with 1990 as base year that comes close to Meth's original calculations." (p116).⁵⁸

The most interesting aspect of the relationship it has been possible to strike up with the NPI through Ms Liebenberg (and a few of her less blinkered colleagues) is a new and refreshing lack of concern with the politics and polemic of her critics.⁵⁹ This goes beyond a cynical

57 Several invitations from the NPI to discuss the problems I had with their output followed the publication of the Fosatu Challenge. Taking up one of these in May 1983 provided an opportunity to understand what they saw as constructive criticism. In essence, this consisted in a plea to see the world in their way. This emerged after a lengthy presentation, at a primitive level, of the NPI's position on productivity as an economic concept.

58 The same publication also notes that the method proposed in Moll (1990) for estimating the output of the gold mining sector has been used because it provides a "better productivity indicator" (p116).

59 One gets a sense at this point of why it is that a theory of individual behaviour within institutions is important. Of course, as pointed out, this can never be a deterministic. What one is striving for is an understanding of the processes by which individuals located in key positions at particular historical conjunctures are able to shape the course of events in certain ways. Many things enter into these changes, the force of argument is important, but so too is the balance of class forces.

calculation of latching onto people who, it may be believed, will have some influence with those now in power. Although such motives cannot be discounted, the manifest willingness to incorporate any and all revisions providing only that there is sound academic argument to support them, is most encouraging.⁶⁰ Unfortunately, a break in her service has meant that the momentum established in *Productivity Statistics 1994* was lost for a while. *Productivity Focus 1994*, published about six months after *Productivity Statistics 1994*, makes no mention of the revised CSS output estimates - it simply uses the old (incorrect) 1980-based CSS output figures to churn out the same conclusions as have been produced by the NPI in the past. This lapse points to the dangers of relying on isolated individuals to produce documents that are of some national significance. During Ms Liebenberg's absence from the NPI (January to May 1994) there was nobody else in the organisation who possessed the skills required to produce *Productivity Focus 1994*, so the work was sub-contracted to her to do privately.⁶¹ The unimaginative content is a reflection of a job hurriedly performed, and acknowledged to be so.⁶² The NPI's subscribers, who pay a substantial fee, will presumably not be too dissatisfied with *Productivity Focus 1994*, given the general tenor of its findings. Economists who make uncritical use of its contents will only serve to perpetuate the tradition of prejudice the NPI has helped to foster.

Whether the influence of this individual, and a few others within the NPI of similarly open mind, will be sufficient to transform the organisation is very much an open question. There are, in any event, other forces poised to bring about major changes from the top down. Cosatu, always likely to have been an important contributor to the debate on the future of the NPI in the new South Africa, has never taken a public stand on the position the Congress takes towards the NPI. Informal discussions with a variety of officials, past and present,⁶³ revealed a rough consensus that favoured absorbing those parts of the NPI that could be identified as performing a public service into an appropriate organ of the state, an expanded National

60 For many years now, the NPI, being aware of worker sensitivity to job loss, has trotted out a hackneyed argument about the positive association between productivity growth and employment growth - see, for example, *Productivity Focus 1994*, p4. While this is probably true at some aggregate level in the medium- to long-term, it ignores completely the often severe dislocations that occur in the short-run (say, up to two years or so). For countries with inadequate social security systems the impact on workers can be devastating. In less-jaundiced quarters (UNIDO, 1986, p100; BLS, 1988, p44) this is acknowledged to be a problem. Ms Liebenberg has agreed to accept a paper on this topic (Meth, 1994) at the NPI's annual Economics and Business Management conference to be held at the end of this year and to publish the results in *Productivity Focus 1995*.

It is important to note that this willingness to consider critical input does not amount to a surrender of ideological commitment on either side - in Feyerabend's terms, we have become fluent in each other's languages and can now disagree constructively over substantive matters of economic concern, rather than simply quarreling over politics and ideology.

61 *pers. comm.* Ms K Liebenberg, April 1994.

62 *pers. comm.* Ms K Liebenberg, August 1994.

63 These included Geoff Schreiner, formerly of NUMSA; Lisa Seftel of Cosatu Head Office; Alec Erwin, formerly of NUMSA; Ebrahim Patel of SACTWU; and Jayendra Naidoo, formerly of Cosatu Head Office.

Manpower Commission (NMC) being one of the possible vehicles, and hiving off the consulting parts of the NPI into the marketplace. In October 1993, a workshop hosted by the NMC met to discuss "Policy options to improve union-management co-operation with a view to advancing productivity as a national goal". This workshop was convened at the instance of the then Minister of Manpower, under whom the NPI fell at the time. A confidential report to the new Minister of Manpower, Tito Mboweni, makes a series of suggestions on ways to improve productivity, but according to one of the compilers of the report, no recommendations on the future of the NPI have been made.⁶⁴

In June 1994, the NPI, along with the CSS and the Central Economic Advisory Service (CEAS) were relocated so as to fall under the Minister without Portfolio (the Minister responsible for the Reconstruction and Development Programme), Jay Naidoo.⁶⁵ NPI officials, not surprisingly, are somewhat concerned about the future of the institution.⁶⁶ Whatever the outcome of current tussles over the form and proper location of an NPI, the institution that emerges will differ quite considerably from the one that presently exists. The commitment of the Government of National Unity to openness, transparency and a more even balance in public institutions ought to be sufficient to ensure this.

Given what has been said above, it should be clear now why a theory that could begin to grapple with individual action within institutions is required. It should also be clear why an understanding of the relationship between ideology and science (discussed in Chapter 1-4) is so important. Recognising the relative impotence of theory in the face of ideology, be the theory never so penetrating, is of the utmost importance. In a sense, the process that began with the Fosatu Challenge has ended with the NPI being obliged to make a number of concessions, and apparently to an unreconstructed Marxist at that. Although the persuasiveness of the arguments I have offered may have played some part in achieving this 'victory', it is impossible not to be aware of the overwhelming importance of the change in political milieu in bringing a measure of sense to the proceedings of the NPI. By the same token, it is not difficult to imagine a scenario in which, if the African National Congress (ANC) had not won the elections, conservative tendencies, reinforced by a much more market-oriented regime than that governing at present, could have stifled debate in favour of an all-out attack on what even now are construed as the excesses of Cosatu. Under such conditions, 'science' has but scant hope.

64 *pers. comm.* Ms Avril Joffe, August 1994.

65 See Government Gazette No. 15792, Vol. 348, 3 June 1994, Pretoria.

66 *pers. comm.* Ms Karin Liebenberg, August 1994.

PART II

PRODUCING RELIABLE MANUFACTURING OUTPUT ESTIMATES: A PROBLEM IN NATIONAL ACCOUNTING

"The essence of national accounting is *integration*: bringing together data from many different sources and arranging them so that they present a coherent picture of the whole economy. This requires persistence in tracking down possible data sources, and ingenuity and imagination in using them. National accounting, to a greater degree than other branches of statistics, is an art, not a science."¹

Chapter 2-1

Statistics, institutions and the failure of value theory

Introduction

To refresh the memory as to what it is that needs to be explained, it may be as well to refer once more to Figure 1 in Chapter 1-2. In that chart, five sets of output estimates are presented. Three of these are the work of the CSS. The oldest of the CSS estimates is the set labelled 'Unbenchmarked'. These were followed by the 'Benchmarked' set, and later, by the 'Reconstructed' series. My original argument was that the 'true' values of output lay within the region demarcated by the two series I have called 'Exuberant' and 'Conservative'. More detailed consideration leads me to believe that the trend line plotting 'true values' will pass somewhere near the square markers in the chart. The goal of this part of the study is to present a critique of the CSS figures and to defend my own estimates.

Two distinct and almost entirely separate bodies of literature have been examined in the course of conducting this study. The material in Part I is deeply political - it is almost with a sense of relief that one turns to the more technical business of looking behind the CSS' attempts to estimate output levels. This activity is not wholly without political overtones, but within the constraint referred to in Chapter 1-4 on the nature of statistics as social products, the CSS, in producing the estimates, primarily the national accounting and other statistics, appears to have

¹ This quotation (including emphasis) from a United Nations handbook on national accounting appears in the latest version of the national accounts published by the South African Reserve Bank (1991, p5). There is a particular irony as regards its applicability to attempts by the authorities to estimate the value of manufacturing sector output - if ever a demonstration of the failure to adhere to the tenets of a creed were required, the story that follows provides it.

behaved about as 'neutrally'² as servants of the apartheid régime could have been expected to have behaved. It is this relatively neutral material with which we will now engage.

Structure

Part II of the study consists of eight chapters and seven appendices. Apart from this introductory section, **Chapter 2-1** is given over to a number of questions of concern to academics and other users of statistics who are bothered by the errors which are an inevitable part of the process of statistics-gathering. It commences with an examination of the over-arching theoretical reason for the inability to measure economic variables involving price and quantity (volume) 'correctly' - the absence of a credible value theory. This is followed by a brief note on the concept of errors, after which a short history of the debate of which this work is a part is presented. The chapter includes, as well, a discussion on the question of the sensitivity of officialdom to criticism, highlighting some of the difficulties involved. Reference is made to recent developments in the scrutiny of the institutions responsible for the production and distribution of statistics in South Africa. There is a brief look at the relationships between economists and policymakers, following which certain questions about the importance of errors to users of statistics - a little explored area - are given some consideration.

In **Chapter 2-2**, the mechanics of the Euler Consistency Test (ECT), a simple procedure designed to detect errors in the national accounts, are explained. An attempt is made to provide the minimal theoretical justification necessary for the test to be considered respectable. Attention is drawn to the fact that the national accounting correction for terms of trade effects obscures the problems ECT is designed to detect. Even so, it will be argued, the ECT was still capable of giving a strong hint that the national accounts of South Africa had been thrown into turmoil by events like the very rapid gold price rises at the end of the 1970s.

In the first part of **Chapter 2-3**, the extent of error in the unbenchmarked CSS industry (3-digit SIC or major-group) estimates revealed by the benchmarking to the results of the 1985 Manufacturing Census is discussed. The inaccuracies are presented as a counter to the CSS

2 Moll's (1984) view of the CSS is that they are honest technocrats. Of course, this does not mean that CSS officials are either able, or indeed even willing to challenge the government of the day. By and large, bureaucrats and civil servants everywhere are notorious for their slavish adherence to the rules laid down by their employer. It would be unreasonable to expect South Africans to differ in this regard. Thus when officials of the CSS were required to produce statistics which furthered the aims of the apartheid régime, they did so. The most obvious example is the compulsory racial categorisation of persons in accordance with the dictates of the state. One of the low points in the annals of the CSS is the publication of a set of estimates of the numbers of African people, by 'nation' (language), still to be 'relocated' (usually forcibly) because they happened to be situated in the wrong place, according to the schema of grand apartheid. This appeared as one of the results of the 1980 population census. See Report No. 02-80-04, Table A, ppix-x.

claim that continuing production of these indices was justified by the demand for them. The chapter also dissects an error I made in my first attempt to benchmark the output estimates. Flowing from this is a consideration of the residual differences between three approaches to the estimation of output volumes - two of them routinely used by the CSS. The proposition is advanced that the sum of the individually deflated components of gross output in manufacturing should not differ significantly from the value of total gross output deflated by the aggregate Production Price Index (PPI), and that neither should deviate from the Physical Volume of Manufacturing Production (PVMP) where attempts are made to measure this.

Chapter 2-4 looks at the issue of convention and compromise in national accounting. In order to establish and test the validity of the proposition offered above, it is necessary to examine the properties of the formulae used to construct output and price indices. It is demonstrated that for most practical purposes, it does not matter whether the deflators (price indices) used are of the Laspeyres or the Paasche form. This is done by reference to the United Nations guide to national accounting practice - the System of National of National Accounting (SNA) (UN, 1968; 1992, and 1993).³

Chapter 2-5 contains a practical demonstration of the arguments advanced in the previous chapters. This is done by considering the simulated growth experiences in a hypothetical economy made to undergo moderate and major change. These simulations appear in **Appendix 2-4**. An examination of the South African data yields the first evidence of major structural change in the industry 'Other Chemical Products'. This change, a massive increase in the ratio of net to gross output, is considered within the context of conventional national accounting practice.

Chapter 2-6 commences the analysis of the problems caused by changes in the ratio of net to gross output. A simulation exercise, the details of which are given in **Appendix 2-5**, uses the double deflation technique to suggest that output in South African manufacturing has been seriously underestimated. It does so by using hypothetical figures that approximate the more important features of the rapid change in the industry 'Other Chemicals'.

What used to be Section 5 in the earlier versions of the monograph on which this part of the study is based has been consolidated into **Appendix 2-6**. This appendix now contains all of the text and the calculations necessary to perform an 'unofficial' or 'do-it-yourself' (DIY)

³ An extensively revised edition of the SNA was published in 1993. Apart from the fact that the problems addressed in this study are not solved by the latest SNA (UN, 1993), virtually all of the errors considered in the study were made in the period when the 1968 SNA was in force. The reference to a 1992 UN document is to the drafts of the latest SNA.

benchmarking of the 1982, 1985 and the recently published 1988 Manufacturing Census results. The purpose of the section in the original version of the work was to show that an inability to replicate⁴ the CSS results pointed to a major error in those results. Since that error⁵ has now been acknowledged by the CSS, the reason for presenting the argument and all of its calculations has changed. It now forms part of the history of the badgering exercise that was necessary to get the CSS to change the way in which the estimates are prepared. By spelling out the details of the abortive attempts at replication of the CSS figures, it becomes possible to see why the suspicion that something was wrong with these figures hardened into firm disbelief. In short, estimates of the 1982, 1985 and 1988 output figures show that the choice of deflator (price index) determines whether or not the results pass an internal consistency test which requires that the sum of a set of individual industry output estimates, each deflated by its own price index, to be equal (approximately) to the value of total gross output deflated by the aggregate price index. Failure to pass this test is an almost certain indicator of error somewhere.

In an extension of the process begun in Chapter 2-5, **Chapter 2-7** scrutinises the literature on the calculation of Real Net Output, and then offers a set of estimates of this will-o-the-wisp. The results suggest that the 'true' value of real (constant price) output differs very significantly from the official figures. Calculations and basic data for these results are to be found in **Appendix 2-7**. **Chapter 2-8** summarises the major findings of the study and offers a series of policy recommendations.

Appendix 2-1, as noted in Chapter 1-1, draws attention to an apparently unreported phenomenon, namely the negating on occasion, by means of what I have called the Perverse Deflator Effect (PDE), of the adjustments made to the national accounts to accommodate changes in the terms of trade. **Appendix 2-2** reproduces a few items of correspondence between myself and the CSS and the Statistics Council.⁶ In Part II of the study, frequent reference is made to the United Nations guide to the construction of national accounts, the *System of National Accounts* (SNA), both the edition which held sway throughout the period covered by this study (UN, 1968) and its recent successor (UN, 1993). To avoid summarising a large chunk of this not very accessible document, **Appendix 2-3** presents a few of the relevant pages.

4 For a detailed examination of the difficulties of replication in large projects, see Dewald *et al* (1986).

5 As predicted by the Euler Consistency Test developed and applied in Meth (1992), output levels were found to be higher in 1985 and 1988 than was previously conceded by the CSS.

6 The Statistics Council is a statutory body set up to provide users with a forum in which to review certain of the activities of the Central Statistical Services (CSS). In particular, the Council can investigate complaints about the reliability of any statistical series.

On value theory in general and the valuation of output in particular

Economics may be viewed as an attempt to recreate or reproduce in thought, certain aspects of processes defined as 'economic', that take place in the real world.⁷ Economic processes take place whether or not 'economics' is practised. Obviously, however, it is possible to intervene in the economy in ways which either improve or worsen economic performance - with or without such guidance as may be obtained from economics. Apart from sheer intellectual curiosity therefore, one of the most important reasons for 'doing economics' is to generate knowledge which can be used to influence the way in which such interventions are made. In the course of trying to recreate or reproduce an approximation in thought of parts of the economic world, a number of barriers - theoretical, conceptual and practical - are encountered. Some of the more fundamental of these have to do with the valuation of output.

There is little novelty in the hypothesis that estimates of output (official and otherwise) will contain errors. If measurement is taken to mean comparison of the object or magnitude of interest with some socially constructed or estimated standard, then to suggest that measurement without error in any field is possible is to display ignorance of what is meant by the process. Moving from the somewhat abstract proposition that measurement without error is not possible, to the peculiarities of the errors found in the different branches of scientific inquiry, one will encounter many different types of measurement problems. Some of these will undoubtedly have attracted the attention of researchers anxious to reduce the potentially harmful effects of errors on their work. In the case of the socio-economic data to be considered in this part of the study - primarily the manufacturing sector output estimates - much remains to be done.

Although there is no shortage of work on sampling errors in the statistical literature, when it comes to theoretical work on measurement errors in national accounting estimates there is precious little available. Empirical work on the problem is rare, and it is not difficult to understand why this should be so.⁸ Although some of the errors to be examined in this study

7 Extreme adherents of the Chicago school would probably deny that such an activity is relevant to their concerns - the instrumental approach of Friedman discussed in Chapter 1-5 is not concerned with explanation - it is an instrument for predicting. Marxists, on the other hand, would claim that there is a dialectical relationship between theory and the reality it seeks to represent. A simple example - that of a policymaker acting under the influence of theory, and changing the world in perhaps unintended ways - serves to illustrate this view.

8 A piece by Hansen and Lucas (1984) (referred to in Chapter 2-4, p251) is one of the few articles I found which exposes the theoretical weaknesses of particular measures (Laspeyres and Paasche price and volume indices), and then shows as well, the extent to which users are likely to be misled by employing them. Some work has been done on the impact of the frequent revisions to national accounting magnitudes. Rees (1975), working with the South African national income data, noted that the data problem is "... one of the most frustrating in econometric work ..." He looked at the question of the selection among series of different vintages, and concluded rather optimistically that the most recent estimate was presumably the most accurate. This was challenged by McDonald (1976), who while suggesting that the impact of revisions ought not to be exaggerated, was at pains to show that the 'latest' estimates need not necessarily be best. As evidence, he

result from incompetence, the really intractable problems are located precisely in those regions where theory cannot provide determinate answers to the questions raised. One of the most refractory of these is the problem of value theory - in short, what is the 'value' of the commodities (goods and services) produced? Inflation alone ensures that value is not to be measured by price (in itself, an often far from easily measurable entity) nor by quantity (volume). Not even a combination of the two can capture adequately the everchanging 'essence' of most commodities and services. Yet, in general, relatively crude estimates of price and less frequently, of volume, are all that are available - that being so, the difficulties consist in the selection of appropriate combinations of these two to represent 'value'.

The value theory vacuum

Conventional introductory texts usually define economics in such a way as to focus attention on the notion of choice by individuals among an almost infinite variety of wants, constrained by limited or scarce resources. Thus conceived, the central problem becomes one of the optimal allocation of resources for the maximisation of utility. Owners of the different 'factors of production' are rewarded for the disutility of exercising their 'talents' by means of income which gives command over the commodities that render 'utility'. In other words, some sacrifice of the consumption of leisure is necessary if other commodities are to be consumed. Value problems are resolved in the market, where the meeting of consumer taste (utility) with producer costs (disutility) results in an equilibrium price. In this world, price theory and value theory can be treated as one.

As far as the theoretical representation of this world is concerned, once the parameters within which the maximisation process occurs are specified, the mechanics of solving the (resource) allocation problem reduces to a set of mathematical processes of greater or lesser complexity, of which linear programming is an example. The implicit or explicit assumption of an absence, or at least, of the relative insignificance of conflict, as well as the almost total absence of any consideration of power, reduces the question of distribution to a technical matter of factor endowments and relative factor productivities.⁹ Problems caused by the non-existence of a stable, let alone invariant measure of value as a yardstick against which to gauge changes in production and consumption are relegated to the status of 'practical' matters to be resolved by the use of appropriate conventions.¹⁰

(continued)

to intermediate estimates (p198). He emphasised the fact that data revisions merely reveal known errors of measurement - unknown errors could well remain even after adjustments have been made.

9 This approach has come under increasing attack in the work of the school of Radical Political Economists in the United States of America - see, for example, Bowles and Gintis (1990) or Bowles (1992).

10 In a sense, most of this study is about the difficulties encountered when these conventions fail. In Chapter 2-7 reference is made to the striking similarities between the problems experienced by conventional and

Most Marxist economists, by contrast, a heterogeneous group laying claim to the mantle of Classical Political Economy, display a keen interest in 'value', as distinct from price. Being concerned with the question of why it is that commodities have value, and of how that value is to be measured, they tend to treat short-run price changes with relative indifference, seeking to understand broader social questions in terms of an analysis of the value categories that are argued to underlie long-run price changes. Whereas conventional economists posit a fundamental equality (either actual or potential) between the participants in capitalist socio-economic relations, Marxists attempt to use value theory to rank the owners of 'factors of production' into a hierarchy of power relations in the market. In this form, value theory becomes a tool for exposing the hidden conflicts and contradictions within capitalism. As far as value analysis of changing patterns of production and consumption in Marxist terms is concerned, once one moves away from a simple static model of the economy, the value-price transformation problem is soluble only with great difficulty (Desai, 1974; Junankar, 1982). The net result is that the value question, as it applies to actual economic performance, is all but ignored by conventional economists, and is a subject of such extreme complexity in Marxist economics that the linking of theoretical with empirical work is always problematic.¹¹

Occupying the middle ground between Marxist and conventional analysis lies a group ranging from neo-Keynesians to neo-Ricardians. As was noted in Chapter 1-5, Joan Robinson, politically a radical liberal with much sympathy for the socialist cause, described Marx's theory of value as metaphysical - most neo-Ricardians, exemplified possibly by Ian Steedman,¹² reject it as simply redundant. Both these groups view the distributional struggles taking place in capitalist society as being of importance, but in neither case is 'value' analysis seen as useful in understanding them. It is within the vacuum (or maelstrom) created by the over-simplification of the right, daunting complexity on the left and the indifference of the centre, that the problem to be addressed in this part of the study must be considered.

The question of what constitutes value and how it may be measured clearly subsumes that of scarcity, for the simple reason that once one moves away from absolutely scarce goods such as gold and life itself, and obviously abundant ones such as air (but not clean air!), there is no way of measuring relative scarcity other than by means of some value theory.¹³ Clearly that which has 'value' must be socially desirable, ie, it must be chosen, but the insights which theories of choice can give us into an understanding of value are so limited as scarcely to

11 The US school of Radical Political Economists criticises most Marxist theory for its failure to develop a microeconomic foundation that differs substantially from that found in conventional neo-classical (Walrasian) economics. See Bowles and Gintis (1990, p166).

12 See Steedman, 1982, and Mandel and Freeman, 1984.

13 Cole, Cameron and Edwards (1991) argue that this is the central problem of economics (pp4ff).

justify the effort of reproducing them. When these theories are based on individualist perceptions they are invariably positivist in the crudest possible sense (like 'revealed preference'), or they lapse into a residual psychologism, taking refuge in notions such as 'taste' (Foley, 1975). A similar fate awaits conventional theories which attempt to comprehend 'choice' on some sort of collective basis - in order to understand these things it is necessary to move out of economics into related disciplines like sociology and social psychology.

The theorising of conventional economics, despite the fact that it rests on a few simple propositions about the operation of markets, is available in all grades of sophistication to cope with the enormous complexity of the real world. It does this with varying degrees of success. As a set of operational procedures it has its uses, but as a tool for valuation it is somewhat less than satisfactory. An adequate theory of value would allow one to estimate, with some precision, the welfare effects of economic processes. In short, it would enable us to measure accurately what is happening in the real world.¹⁴ If it were the case that price were synonymous with value, a great many problems would be avoided. The value of all output could then be obtained merely by summing all final market transactions and by summing the total of non-marketed output transactions using shadow prices. If, in order to be regarded as performing adequately, a value theory must be able to record faithfully welfare changes in the real world, then clearly, the process described immediately above does not qualify. Continuing inflation, as noted above, let alone changing structures of production and relative prices, ensures that this is so.

There are several other obstacles standing in the path of a simple summation of 'values' to produce a measure of national well-being. Some of the more important of these relate to the statistical difficulties of aggregating the hundreds of thousands or millions of different commodities and services produced in the modern economy whose value is not measured simply by their price.¹⁵ Some of the puzzles to be considered in this part of the study present themselves as a set of inconsistencies that become visible when rebasing or reweighting of national accounts take place. These are caused by changing price and quantity relativities in the Laspeyres indices used to measure output levels. Of the many ways in which index numbers can fail, this is perhaps the commonest (David, 1962; UN, 1993).

Ironically, few of these problems affect gold, the commodity considered in Appendix 2-1 of this study. Both volume and price measures for gold are very good. Not only that, the quality

14 Marxists, of course, use a theory of value for a somewhat different purpose, namely, to reveal the exploitation on which the capitalist mode of production rests.

15 Technological and quality changes make it difficult to compare the 'value' of this years' production with that of last year, or worse still, of that five years ago.

does not change. Even so, there are formidable problems in the valuation of the contribution of gold to the South African economy. The phenomenon that I call the Perverse Deflator Effect (PDE) undermines the corrections made to the national accounts to compensate for changes in the terms of trade (the ratio of the price index of exports to that of imports) caused by large and rapid changes in the gold price. The disjunction to which this gives rise varies in intensity from the trivial to the massive, such as that which occurred at the rebasing of the South African national accounts to the year 1980.

Convention to the rescue

The United Nations-sponsored System of National Accounts (SNA) (UN, 1968; 1993), a concerted attempt to resolve some of the more pressing problems of measurement, valuation and the aggregation of output, emerged after the Second World War in the context of a general commitment to growth and development.¹⁶ National accounts prepared to the uniform procedures contained in the SNA provide a yardstick by which any market economy can measure its performance - both internally, and in relation to the performance of other economies. In the SNA, a set of agreed conventions are used to resolve those problems which theory cannot. High on the list are the problems caused by changes in price relativities. To make any sensible estimate of changes in welfare, it is necessary to eliminate extraneous 'noise' such as that deriving from price changes.

Precise welfare concerns have to be specified in advance before the impact of a price change can be measured. The trivial example of a price increase by a local monopolist who may increase his or her welfare at the expense of everyone else, points to the possible existence of more serious problems where markets are not competitive. Crossing international frontiers, it may well be that price changes in goods, the demand for which is inelastic, can produce substantial benefits for the supplying country, at the expense of consumers in all the other countries to which the particular good is exported. The unit of analysis used in attempting to measure total output (and hence, welfare) and the ways in which it has changed, is important. For the purposes of national accounting the most obvious contender as unit is the nation state, although regional analysis should not be neglected, especially if large scale redistributions are occurring. Given the nation state as this unit, price changes in certain commodities, eg, gold

16 The brief reference here to the origins of national accounting glosses over a complex history, and places the best possible interpretation of the aims of the governments that were instrumental in formulating the principles which came to be embodied in the SNA. For a critical analysis of the development of national accounting, see Lintott (1982). For a less contentious view, see UN (1993, ppxxxviiff).

and oil, have, on occasion, had a series of effects that can scarcely be considered desirable in an index that purports to measure welfare.

Amongst the necessary characteristics of a measuring device are that the results it yields be relatively unaffected by external conditions, and/or that it be reasonably easy to calibrate when conditions do change. The SNA, as a set of procedures for measuring output, does this with indifferent success when confronted by severe price instability in major components of the total output.¹⁷ Where an instrument is known to produce errors, it is essential that the likely magnitude of those errors be made known, especially where they could affect significantly the work of those using them.¹⁸ This is fairly standard practice in the natural sciences. Unfortunately, the habit has yet to find general acceptance in the social sciences.

These weaknesses - the occasional impossibility of valuing output adequately, the failure of the authorities in South Africa to warn users that such is the case, and the unwillingness of those authorities to acknowledge error generously when it is pointed out - and, more positively, the attempt to develop tools that can aid in the process of detecting errors, are the subject of this part of the study.

On the nature of errors

Given that much of the study is concerned with errors, both of the avoidable and the non-avoidable type, it is as well to be clear about the types of errors to be encountered, and the context in which they occur. The nature of the production process of official (and presumably most other) statistics offers some indication of the types of errors likely to be made, as well as a guide to what the contents and scope of a rigorous examination of the quality of official statistics should be. This is neatly encapsulated in the longish quote below:

"Official statistics are produced by a conjunction of 'conceptual' and 'technical' instruments. It follows that the evaluation of such materials for scientific purposes must never be restricted to a concern with more or less deliberate misrepresentation or with the identification of technical errors and inadequacies. These problems are undoubtedly important but, ...they are not the only sources of a lack of objectivity on the part of statistical materials. In addition to their consideration, the rational evaluation and utilisation of statistics for scientific purposes must take account of the conceptual means of their production, that is, of the system of categories with the instructions and elaborations in which they are specified. This means that it must depend on the theoretical interests of the science concerned, since it is these that determine what is

17 National accounts produced according to the SNA rules are plagued by a series of weaknesses that have been explored at length elsewhere. These will not be considered here, but see for example Seers (1975; 1976).

18 It is for this reason that the CSO in Britain attaches an estimate of reliability to the national accounting figures. See Maurice (1968, pp40ff).

required of the statistics if they are to be of use. Clearly, then, there is little of any value to be said concerning the generality of official statistics or even, apart from purely technical questions, concerning the utility, in general, of any given set of statistics. The usefulness of such statistics is a function of the theoretical problematic in which they are to be used and on the use to which they are to be put within it." (Hindess, 1973, p45)

Taking issue both with positivists who treat error merely as the corrigible, if inevitable results of the use of standard statistical techniques, and anti-positivists who locate error (incorrigibly) in the "...necessary structures of human experience", Hindess argues that a "rational evaluation" (without reference, that is, to "...the 'subjective experience' of enumerators or officials") of social statistics is possible (1973, pp28-29). This study, with its focus on a single series - the manufacturing sector output estimates - is an attempt to perform a rational (non-subjective) analysis on that series.

Other workers in the field of errors in economic statistics have arrived at similar conclusions to those reached by Hindess - Griliches, for example, has examined the encounters between econometricians and the data, and declares them to be both "...frustrating and ultimately unsatisfactory." This is because too much is demanded from the data, with the result that the answers given are disappointing. Answers are also of indifferent quality because the data are "incomplete and imperfect." He isolates three causes of current difficulties, namely:

"...1) the theory (model) is incorrect; 2) the units are wrong, either at too high a level of aggregation or with no way of allowing for heterogeneity of responses; and 3) the data are inaccurate on their own terms, incorrect relative to what they purport to measure. The average applied study," he notes, "has to struggle with all three possibilities." (Griliches, 1985, pp197-198)

As an example of a spectacularly large error (a horror story, as he terms it), Griliches cites estimates of the 1982 US trade deficit with Canada - either \$bn12,8 or \$bn7,9 depending on whether the US or Canadian figures are used (p199). To place the matter in perspective though, he also draws attention to an ambivalence towards the imperfections which make the jobs of economists "difficult and often impossible", namely that:

"...these imperfections give us our legitimacy in the first place. If the data were perfect, collected from well-designed randomized experiments, there would be hardly room (*sic*) for a separate field of econometrics. Given that it is the "badness" of the data that provides us with our living, perhaps it is not all that surprising that we have shown little interest in improving it, in getting involved in the grubby task of designing and collecting original data sets of our own. Most of our work is on "found" data, data that have been collected by somebody else, often for quite different purposes." (Griliches, 1985, p196)

The problems analysed in this study result from all three of the causes isolated above by Griliches. Much of the space in the study is given over to a discussion of the imperfections resulting from the type of error referred to in third point made above, namely that "...3) the data are inaccurate on their own terms, incorrect relative to what they purport to measure...".

[In]correct theory is, however, also important, as is the problem of aggregation (in a variety of ways!). The errors uncovered by this study result not only from incompetence, but also from the fact that no 'correct' solutions exist to the problems confronting the national accounting statisticians when they set out to measure the variables¹⁹ at issue in this study. It would be unreasonable for users to demand that these problems, most of which have been known about for decades, be solved, simply and unambiguously - but it is equally unreasonable for the CSS not to advise users that the estimates concerned are beset by problems (usually of unknown, but sometimes of significant magnitude). It is even more unreasonable for the CSS to take refuge, when criticised, behind international conventions on the production of statistics, as though these could ward off the injurious effects of shocks to the system of data collection and manipulation.²⁰

Chasing a moving target - some background to the study

Sometimes the door can be closed firmly on an enterprise and one can walk away from it knowing that all the finality imaginable or possible had been attained. Often, the resolution will be unsatisfactory, but one senses that nothing more can be done to change the outcome in any significant way. Such conclusiveness is rarely experienced in the social sciences. At worst, the controversies that arise may simply be incapable of being settled. An unwillingness to acknowledge this leads, as Donald McCloskey observes, to debates that drone on for centuries.²¹ Situations do arise, however, where it is worthwhile carrying on banging one's head against what may seem to others to be a stone wall. This does not mean that there may not come a point at which the persistence so necessary to the solution of a complex problem degenerates into obstinacy and mere willfulness. Good judgement consists in knowing when to

19 Economic variables are peculiar animals - in a highly jaundiced view on the possibility of progress in economics, Worswick drew attention to the characteristic of an economic variable whereby it may be:

"...determined, not only by the other variables normally expected to influence it, but by a unique history as well, in a manner which does not occur, with anything like the same force, in natural science." (1972, p85)

Adding to the problems is a peculiarity of economics as a body of scientific practice upon which Griliches has focussed attention. He notes that:

"...often we know little about what is really going on (for example, when we estimate a production function for the cement industry without ever having been inside a cement plant).

In this we differ quite a bit from other sciences (including observational ones rather than experimental) such as archeology, astrophysics, biology, or even psychology where the "facts" tend to be recorded by the professionals themselves, or by others who have been trained and are supervised by those who will be doing the final analysis. Economic data tend to be collected (or often more correctly "reported") by firms and persons who are not professional observers and who do not have any stake in the correctness and precision of the observations they report." (Griliches, 1985, pp196-197)

20 Instances of attempts by the CSS to do this appear below at several points in the study.

21 This comes from a passage which reads as follows:

"...scientific debates in economics are long-lasting and ill-tempered...bitter controversies...[that] drone on from one century to the next...mathematical and statistical tools that gave promise...of ending economic dispute have not succeeded because too much has been asked of them." (McCloskey, 1983, p514)

stop - it consists, in other words, in recognising when one is trapped in a debate that is not capable of being resolved. In the prosecution of this particular study, it has often been felt that that point had been passed. The publication by the CSS on 12 November 1993 of a set of significantly (upwardly) revised output estimates makes it clear that it was worthwhile persisting.

What follows is a brief account of the steps taken in the attempt to reveal the existence of major errors in the manufacturing sector output estimates. In brief, two monographs on the issue have been written. A draft version of the first was submitted to the Statistics Council in 1991.²² Revised and updated, it was then published in 1992 under the title 'Data Problems in the Manufacturing Sector' (Meth, 1992). The second monograph was called 'More Data Problems in the Manufacturing Sector'. It will be referred to as 'More Problems...' to distinguish it from the 1992 monograph, which will, on occasion, be referred to as 'Data Problems...'. 'More Problems...' was submitted in first draft form to the Statistics Council in March 1993. An unsatisfactory response by the Council to this submission led to a second draft. This was nearing completion in November 1993, when the CSS published the second revised set of output estimates shown in Figure 1 above. In so doing, the CSS conceded, although not in so many words, that the criticisms made of their workmanship during this lengthy affair were, in large measure, justified.

The task undertaken in the first and second draft versions of 'More Problems...' was essentially that of dismissing the CSS' attempted defence of the first revised set of estimates in Figure 1 above, and of putting forward a set of alternative estimates. This part of the study incorporates most of the work done on a final draft of 'More Problems...' now being prepared for publication (Meth, 1994). The work on the monograph was considered worthwhile undertaking, even though, as noted above, the CSS has implicitly acknowledged that the criticisms made of their workmanship were correct. Apart from a need to place on record the difficulties experienced in persuading a state institution to act on reasonable criticism of what is probably one of the more significant economic indicators, the study offers suggestions for a consistency test that can be used to check the output estimates. It offers as well, a defence of the high estimates in Figure 1.

In the draft monograph called 'Manufacturing sector data problems' (Meth, 1991a) submitted to the Statistics Council it was argued that there were substantial errors in the official output

22 The Statistics Council is a statutory body set up to provide users with a forum in which to review certain of the activities of the Central Statistical Services (CSS). In particular, the Council can investigate complaints about the reliability of any statistical series.

estimates. The response from the Council, in which the CSS acknowledged that the output series contained errors, offering, as explanation, the fact that benchmarking had not yet been carried out (Meth, 1992, pp66-67), was not particularly satisfactory. Nonetheless, some of the criticisms made were useful, and these, along with the much more substantial comments made by a referee and by others who read the draft, were incorporated into a substantially revised version of the monograph that was published under the title 'Data Problems in the Manufacturing Sector' (Meth, 1992). As the monograph was being printed, the CSS published a set of benchmarked results. An addendum to it (literally - a 'stop press'), hurriedly tacked on, found fault with the newly-published official output figures as well. Specifically, it was argued that a set of results in which the value of the sums of the outputs of individual industries, each deflated by their respective Production Price Indices (PPIs) is significantly greater than the sum of total output in current prices deflated by the aggregate PPI for manufacturing, cannot be correct (Meth, 1992, pp74-76). The present study shows conclusively that the errors in the estimates are much larger than was acknowledged by the authorities in the first benchmarking of the estimates, and larger even than the most recently conceded errors.

After the submission of the first draft version of 'More Problems...' to the Statistics Council, the Council resolved at its meeting on 25 September 1992 that an *ad hoc* sub-committee, consisting of senior officials from the CSS and the SARB be formed to consider problems with the manufacturing sector estimates. The committee has completed its deliberations, reports have been dispatched to the responsible Minister, and a letter (from the Statistics Council dated 1 October 1993) conveying the sub-committee's findings has been received. The letter is reproduced as part of Appendix 2-2 in Volume II of the dissertation.²³ It provides little evidence that the committee has been able to extract from the CSS an adequate explanation for the errors revealed in this study.

Since new or revised statistics constantly make their appearance, often without much warning, an enterprise such as this, which seems by its very nature to be a protracted affair, is sometimes in danger of being overtaken by events. That was certainly the fate of the forerunner of this piece (Meth, 1992). The publication of new and revised information has affected the present work as well. The first draft version of 'More Problems...', submitted to the Statistics Council on 3 March 1993, which is listed in the bibliography as Meth (1993b) contained criticisms that referred mainly to the rebased estimates of the Physical Volume of

23 There was talk that a suitably qualified academic would also serve on the sub-committee, but this appears not to have happened. The sub-committee consisted of Dr A P T du Toit and Mr John Lynch of the CSS, and Dr Meijer and Mr J Prinsloo of the SARB.

Manufacturing Production (PVMP), the 1985 Manufacturing Census, and to an unpublished set of revisions, some of them quite large, to the estimates of gross output in certain industries.

The long-awaited 1988 Manufacturing Census results were finally published in summary form at the end of June 1993 (SNR P3001, 28 June 1993), and that made possible a simplification of the analysis conducted in the first draft version of the work. The summary census results contain revised estimates of net output and further revisions to many of the gross output estimates, although the latter are not as large as the unpublished revisions on which the first stage of the analysis was based.

No revised estimates of net output were available when the first draft version of 'More Problems...' was being written, so it was necessary to make certain assumptions about the ratios of net to gross output. Because one of the industries (major groups) involved, 'Other Chemicals', makes such a large contribution to total output, the effects of making differing assumptions about this ratio were non-trivial. The ratio of net to gross output in the industry jumped from 0,32 in 1979 to 0,51 in 1985, and the contribution of the industry to total net output (in current prices) grew from 8,6 percent in 1979 to 15,5 percent in 1985. This, it would appear, is attributable to the giant Sasol II and III plants which came on stream in the early 1980s.

Changes of this magnitude in the ratio of net to gross output have, as will be seen below, a profound impact on the estimates of the PVMP. When the calculations for the first draft version of 'More Problems...' were made, I thought that the reported change in value added may simply have been an error. To accommodate this, pairs of estimates were made, with net to gross output ratios ranging from 0,32 to 0,51. It seems, however, that the published ratios are not incorrect. Discussions with the CSS²⁴ on this matter suggest that the difference is probably due to the very high value added to the cheap raw material, coal, used as an input by Sasol.

In addition to the changes resulting from the availability of the revised manufacturing census results, one other key piece of information became available. Noting that CSS estimates of value of output in constant prices obtained variously by summing the individually deflated industry gross outputs and the deflated value of total output differed quite considerably, I had attempted to obtain an explanation from the CSS. This had no effect until a direct request to the Head of the CSS yielded part of the desired information.²⁵ The CSS response was not

24 Telephone conversation with Mr Roelf van Tonder, 30 June 1993.

25 These differences are discussed in Chapter 2-3 and illustrated in Table 2-3.4. The request was made in a letter to the CSS dated 31 July 1993. A reply by telephone was provided on 2 August 1993.

wholly satisfactory, but it did resolve one mystery which no amount of simulation and calculation in the earlier version of this work could eliminate. The CSS could have supplied this information in March 1992 when a request was made, via the Statistics Council, for an explanation of the discrepancies then visible. Instead of furnishing the correct details of what procedures had been followed, the CSS supplied a vague three-part explanation, some of which was wrong and the remainder of which may only be applicable to some small part of the observed error.

This failure to provide the correct information forced me to undertake a major detour through all of the relevant published statistics, the mechanics of index production and the United Nations publications on national accounting techniques simply to demonstrate that the explanation provided was largely false. One can speculate on the reasons why, in this instance, the CSS provided such poor service, but it has made one thing clear, and that is that in its present form, the Statistics Council is incapable of meeting users' needs in matters of this sort. A few well-placed questions from the Statistics Council, put with determination and pursued until satisfactory responses were forthcoming, would have resolved the problem immediately. The fact that this did not happen is evidence of a lack of capacity which must be addressed if the Council is to function as anything more than a rubber-stamp for the activities of the CSS.

One of the final stages in this saga was the publication by the authorities of the second set of revised PVMPs in November 1993. These figures make it immediately obvious why the replication exercise attempted in Appendix 2-6 could not succeed. The upwardly revised figures, validating as they do the criticisms made of the earlier estimates, pose questions for the form of the study. Chasing a moving target through private correspondence, as I was, it was always likely, as noted above, that the research would be overtaken by events. After all, the provision of a critique, if it is correct, enables the party criticised to emend that which has been found wanting, and the act of doing so renders the criticism at least partly superfluous.

It has been decided to leave the abortive attempted replication of the CSS figures in more or less the form it had reached in early November last year, and this for two reasons. In the first place, it provides an excellent example of the kind of difficulties the statistics user is likely to encounter in the official statistics. In the second place, much of the basic data used in Appendix 2-6 is required to construct the alternate net output-based volume estimates which I argue are closer to the 'true' values of output in the manufacturing sector.

Relations between the statistical establishment and users

In most countries, the state is the major collector and publisher of statistics. The reasons for this are so obvious that they scarcely need to be spelled out here. For equally obvious reasons, it should not be necessary to point out that very few records exist of what goes on inside these government agencies²⁶ - autobiographies of official statisticians seem unlikely to capture the public imagination. Relations between producers of official statistics and users, as well as those between the producers and the governments who are their employers are far from unproblematic. Yet all that outsiders can ever obtain by way of an understanding of the inner workings of institutions like the CSS and the NPI and of the true reasons, from their side, for the tensions in these relationships, is such filtered and pasteurised information as official spokespersons are pleased to dispense. In societies like South Africa, Britain and many others, where bureaucracies are tightly closed and hypersensitive to criticism, all that is available is the usually not very informative public record.

Users of statistics differ in their access to power - when the temptation to use statistics, by whatever means, to advance particular causes, cannot be contained, the opportunities for mischief are manifold. Those with a vested interest of one sort or another can often profit by the publication of incorrect or misleading statistics - history is replete with examples of interference by politicians in the autonomy of official statistical agencies - invariably because the figures produced do not please the politicians. In recent times, one of the more notorious instances of this is the Thatcher government's 'fiddling' with the unemployment estimates.²⁷

Attempts by governments to bully statistical agencies into producing the 'correct' information invariably succeed when the agency in question has insufficient 'distance' from the government. I explored this issue in "Data Problems..." (Meth, 1992), citing from one of a number of articles charting the decline of the British Central Statistical Office (CSO) that appeared in *The Economist*. Even after allowing for journalistic excess, the tale told is not a happy one. In the article in question it was stated that:

"...the poor quality of Britain's economic statistics makes steering the economy like driving a car with steamed-up windows, wonky dials and no lights." (September 7th 1991, p44)

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- 26 All the more reason then to celebrate the single article I have come across which gives an insider perspective. It was written by an anonymous 'Government Statisticians' Collective' (1979) in Britain, and, for those interested in such esoteric matters, makes fascinating reading. Further reference will be made to it below.
- 27 This notorious episode is well documented. A reference is given overleaf to one of the scholarly works which discusses to it.

This had certainly not been the case a scant decade or so ago; as *The Economist* noted, "...Britain once boasted the best economic statistics in the world..." The official guide to the British national accounts gave estimates of the reliability of all the published data (Maurice, 1968), but years of savage spending cuts and interference by the Thatcher government reduced the CSO to a point where it could be claimed that:

"In Britain GDP stands more aptly for grossly deceptive product, and RPI for ropey price indicator. The 30 changes in its [the CSO's] definition of unemployment since 1979 smack of something worse than indecision." (September 7th 1991, p44)

In a lengthier article in the same issue of *The Economist* the periodical's 'panel of statisticians' isolated as the most important quality of a good government statistical agency "...integrity and objectivity..." The countries with the best statistics, Canada, Australia, Sweden and Holland, all had centralised systems in which "...numbers are collected by a single agency whose independence (except in Sweden) is guaranteed by law." Countries which have decentralised systems, with statisticians "bedded out" in different departments, reporting directly to their ministers, may allow politicians to become more involved in the compilation of statistics than is desirable. Because Britain's statistical service is amongst the least independent, "...the figures often taste of fudge..." (p84).

The '30 changes' have become an important part of the history of the abuse of statistics. Harcourt and Kidson (1993) note that all but one of these changes "...reduced the jobless total" (p445n) - a congenial result for a régime under assault because its policies were raising unemployment levels to almost unprecedented heights. In support of the claim of continued political interference in the production of these statistics they cite an acknowledgement in the autobiography of a former government minister responsible for employment that "...his job seemed to be not to address the unemployment problem but to devise ways of reducing the figures." (p445)

It is unusual in a democracy, for a statistics-producing institution of integrity to produce, on demand, statistics that advance sectional interests. Unfortunately, it is, however, not unheard of. Another example of Thatcherite meddling may be found in the statistics on health care and education in Britain. Citing the official guide to the national accounts, Harcourt and Kitson state that:

"For education and health services, "the indicators used for the public sector elements cover employment and capital consumption... In the absence of suitable alternatives, the private sector elements are also covered by arbitrary series or employment indicators, *with arbitrary adjustments for changes in output per head*" (CSO, 1985, p. 44, italics added [by Harcourt and Kitson]). So an arbitrary productivity term is added to the private sector element, but not the public sector. There can be no justification for such a

biased approach; data should be treated in a consistent manner and not according to dogma." (1993, p441n).²⁸

A more common form of intervention is for the state to attempt to curtail the activities of the official statistics-producing institutions. There have been two prominent examples of this in recent years, both in countries with radically conservative régimes. In Britain, the so-called Rayner doctrine sought to limit statistical production by government.²⁹ In line with the general policy thrust aimed at reducing public provision, the White Paper (1981) based on the Rayner Report (1980) argued that:

"Particular attention needs to be paid to all information collected or costs incurred primarily to meet demands outside central government. There will be exceptions (for example due to confidentiality of data sources), but in general there is no more reason for Government to act as universal provider in the statistical field than in any other.

Information should not be collected primarily for publication. It should be collected primarily because government needs it for its own business." (Cited in McClellan, 1993, p5)

McClellan (Head of the CSO at the time, and appropriately circumspect in his choice of language) notes that the combined Rayner and deregulation influences (reducing the load of government demands on business) had the desired effect of reducing the number of forms filled in (1993, p5). He also observes, however, that the restricted view of official statistics articulated by Rayner (and confirmed by a further study conducted in 1988):

"...could only have a limited life. Indeed, one could argue that such an interpretation was never fully implemented." (1992, p6)

After users had "...expressed their concerns vigorously..." the responsible Minister backed off somewhat and acknowledged, albeit indirectly, the importance of the needs of non-government users (McClellan, 1992, p6).³⁰ Prominent among the critics was the Royal Statistical Society which rejected the Rayner doctrine, arguing that it was "...not the generally accepted norm

28 The CSO document referred to is the *United Kingdom National Accounts: Sources and Methods* - presumably the update of the Maurice *et al* (1968) and Copeman (1981).

29 In fairness to Rayner, it should be pointed out that a not dissimilar philosophy was articulated in guides to the government statistics compiled while the Labour Party was still in power. The difference is that the value of statistics to business, in particular, was recognised. It was acknowledged that parts of the statistical system were shaped with business interests explicitly in mind. Academics and other users were assigned the lowest priority (Government Statisticians' Collective, 1979, p130). The Government Statistical Services in Britain, which were co-ordinated through the Central Statistical Office (CSO), enjoyed a period of high prestige because of the perceived importance of statistics in the race to restore Britain's competitiveness. Claus Moser, head of the CSO in Britain during this period, enjoyed the confidence of prime ministers Wilson and Heath. He presided over the institution during a period when it doubled in size every seven or eight years. The fiscal crisis of the late 1970s put paid to any further growth, and ushered in the Thatcherite austerity programme which led to the decline lamented in the pages of *The Economist*.

30 In short, the Rayner view was rejected by users through the various organs that represent them (Moore, 1991; 1992, McClellan, 1993), and the British state, which had overseen substantial cutbacks in the production of statistics, softened its stance (CSO, 1991; Smith, 1992, McClellan, 1993).

internationally" (Moore, 1992, p6). Moore, a past president of the Society noted by contrast that:

"...the Crisp report (1974) in Australia stressed that in seeking to satisfy 'the whole world spectrum of government, business and general community statistical needs' the statistical service should treat all users equally. The view that government users should have priority was emphatically rejected, the responsible Minister supporting the Crisp committee's reasoning and its conclusions." (1992, p6)

Whilst it is not unreasonable for the state to look critically to its own data requirements, an inescapable effect of the Rayner dictum, rigorously applied, would have been to have restricted the information available to the broader community of users outside of central government. This is true even if were not the intention of the state to deprive users of information.

In the 1991 edition of the *Journal of the Royal Statistical Society*, a joint editorial written by five experts in the field including the Chief Statisticians of Canada and Australia - the countries reputed to have the best statistical services in the world - contained a contribution from one Stephen Fienberg. His criticism of the US administration was stronger than the corresponding criticisms his colleagues made of the state of affairs in England. Citing research work in support of his claims, Fienberg argued that:

"For the past decade in the USA, we have witnessed an attempt by the executive branch to redefine the constituency for government statistical data. The Office of Management and Budget, through official circulars and statements before congressional committees, has repeatedly endorsed a minimalist role to the effect that statistical agencies should not produce information products and services that are not essential to the proper performance of federal agency functions... The use of government statistical data to monitor and assess government policies is a time-honoured American tradition, and this attempt to limit the types and extent of data available to the public has been fiercely opposed, not so much by statisticians, but by data users in the private sector, in state and local governments and in universities, who are without the extensive resources required to mount national data collection programmes." (1991, p11)

Fienberg seeks to establish the right of statisticians, rather than politicians, to decide on the way in which "...statistical information is collected, produced, analysed, interpreted and reported..." (1991, p12). In support of his plea, he recounts the manner in which the sensitive decision not to adjust for differential undercount in the 1990 census estimates of the black and white populations was taken, not by the Director of the Bureau of Census, but rather by administration officials in the Department of Commerce.

Regardless of whether intention can be demonstrated or not, it is undoubtedly the case that under the guise of taking power away from government and handing it back to individuals, the Thatcher and Reagan régimes curtailed liberties by restricting the flow of information.

In the light of these examples from so-called 'democracies', it is obvious that the question of control over information flows is a matter of some concern. What then, of South Africa's manufacturing statistics? South Africa was not a democracy during the period under consideration in this study, but it had some of the institutions and trappings of democracies. Although the civil service followed slavishly the dictates of the apartheid régime, those parts of it concerned with the technical aspects of government, for example, the CSS, seem to have subscribed to the rules of impartiality commonly believed to be the hallmark of good civil servants everywhere. Of course, this notion of the civil servant is more than a little suspect, and in the South African case, very few civil servants ever questioned the ethos of apartheid (at least, not in public), with its concentration of resources on the 'haves'. Nonetheless, the CSS seems to stand above partisan appeal, and seems, for the most part, honest. That being so, one would surmise that the errors one finds in the manufacturing statistics are the result either of one form or other of incompetence, or of an inability to solve the difficult conceptual problems involved, rather than deliberate attempts to mislead.³¹

Like most clichés, the one that sees manufacturing as the 'engine of growth' in an economy contains more than a grain of truth. That being so, one might suspect that a seriously deficient record of the progress of the sector would be cause for some concern. Investment, to name but one important activity, one in which expectations, 'climates' and a fickle confidence play such important rôles, may well have been affected by persistent misreporting. It is, however, no easy task to determine how much of an impact the substantial under-estimation of the aggregate performance of the South African manufacturing sector over the period since 1979 has had on any economic variable.

By contrast, it appears to be a relatively simple matter to demonstrate that 'managers' have been persuaded that the relationship between wage and productivity growth of above-all unionised workers - chiefly in the Cosatu affiliates - has long been unhealthy. This, it has been argued by Welcher (1991), has been one of the major determinants of rapidly rising capital/labour ratios in South Africa. Firms mechanised, it is claimed, to be able to pay the allegedly excessive claims made chiefly by (black) workers. To the extent that this perception is founded upon incorrect information, damage may have been done to the economy.

There are several causes of errors in the statistics published by the CSS. Sampling bias has already been mentioned, but of considerable importance is the fact that the CSS is so poorly

31 One should, of course, add to this that the suspicion and hostility with which the CSS has treated critics of whom it has disapproved, the unwillingness to admit error (demonstrated handsomely in this study), and the reflex recourse to the secrecy provisions of the Statistics Act, combine to make the CSS somewhat less open than it might otherwise have been.

staffed that difficulties raised by unsolved problem areas such as those canvassed in this study may not even come to the attention of those responsible. A reflex response of the CSS to criticism is the retort that the statistics they publish are gathered and produced according to international standard practice. It has been my experience that when exceptions to the rules have arisen, the CSS has been unable to respond creatively. In the particular case with which we are concerned here, the CSS could not spare suitable personnel to carry out a proper investigation. This is in stark contrast to a body like Statistics Canada, where several of the approximately 200 specialists in statistical methodology could be deployed to tackle such issues.

These staff shortages give rise to incompetence, particularly where time series data are concerned. What has happened in the past is that an expert team will be assembled to deal with a particular survey. Once the design phase has been completed and the questionnaires have been administered a few times, the team (often containing outside experts) will be dispersed, and the task of running the survey will fall to a junior staff member. Error creeps in, and in the absence of an experienced analyst to keep the series under control, garbage is easily generated. This happened in the case of the politically-sensitive Current Population Survey figures, which measured, amongst other things, unemployment levels among African workers. It became obvious from about 1987 onwards that the results were not credible (Meth, 1988), but despite widespread criticism, the CSS suspended the survey and ceased publication of the figures only in August 1990 - long after most analysts had ceased to have any faith in them.

It is probably fair to claim that the CSS has neglected the gathering of certain statistics that are vital to the upliftment of the less-developed peoples in this country in favour of easily-collected, but less relevant material. It does not seem reasonable to suggest, however, that the catalogue of errors to be opened below, and the reflex defensiveness that has surfaced each time an error was pointed out, results from anything other than the circumstances outlined above. As far as manufacturing sector output estimates are concerned, it does seem as though the CSS cannot be accused of anything more than incompetence.

The same cannot be said of the productivity statistics pumped out by the NPI, at least, not until very recent times. As was pointed out in Chapter 1-1, the NPI, by virtue of the fact that it combines statistical production with advocacy of a particularly activist kind, renders itself suspect by the very nature of its activities. Not only has the NPI not been sufficiently critical of the CSS output estimates - on at least one occasion, when it has had the poverty of those estimates forcibly laid before it, the organisation has claimed that the errors did not alter the conclusions previously drawn (NPI, 1983). This was the NPI's response to the observation in

the Fosatu Challenge that the actual growth rate in manufacturing between 1970 and 1979 was not 2,6 per cent *per annum* as the CSS 1970-based accounts intimated, but rather 5,0 per cent as indicated by the 1975-based accounts (Meth, 1992, p15). To be so blinded by prejudice as not to recognise that a doubling of an output growth rate turns a dismal productivity growth rate of about one per cent *per annum* into a not too disappointing 2¼ per cent (roughly that sustained in the US economy over the long-run) is, if not spectacular, then at least solid, is the natural outcome of an over-zealous commitment to the entirely laudable goal of raising productivity.³²

These two quite different set of circumstances give some indication of the general contexts in which criticism of the output of the various official institutions has taken place.

Using the NEF to effect change

Developing comfortable relationships with bureaucrats of whom one is critical is not a simple task anywhere - in South Africa, where relations between the liberal universities and the state have, for the most part, been quite strained, the task requires some sensitivity - not all of us are equally diplomatic. Relations between critical users of statistics and the producers of those statistics are, however, changing.

When the first signs of the likelihood of a large error in the manufacturing sector output estimates presented themselves about three years ago, the question arose as to how these errors should be taken up with the authorities. In the past, I have adopted a confrontational stance, most particularly in the FOSATU Challenge (Meth, 1983), and somewhat less so in Meth (1988). Not only has this failed to make much impression on the authorities - colleagues have been critical of the approach, in part because of the negative impact (real or imagined) that it has had on their relations with the CSS, and to a lesser extent, with the South African Reserve Bank (SARB). Some of them insisted that the only way to enlist the co-operation of the powers-that-be in sorting out problems of the kind tackled in this study was one of patient and polite interrogation. A serious attempt has been made to take this advice - almost every query has been referred to Pretoria for comment. Between May 1990 and about October or November 1993, numerous communications between myself and the CSS took place, many of

32 Barry Goldwater, renowned extreme right-wing US politician, once said that excessive zeal (or words to that effect) in the pursuit of virtue was no vice. One of the contradictions of commitment to causes is precisely the ease with which a sense of balance is lost, particularly when members of a group espousing a cause perceive themselves to be embattled. Enthusiasm is a necessary element of any campaign, but when taken to extremes it crosses the fine dividing line into the unreasonable.

them in writing, some in the form of telephone conversations and others during visits to the CSS head office in Pretoria. Apart from the odd hiccup in the relations with the SARB,³³ dealings have been cordial.

It may well be that my colleagues are correct, but until very recently, it was not clear that (constructive) criticisms made of the manufacturing sector output estimates were treated with anything more than reflex defensiveness. The Head of the CSS has politely but firmly rejected them, each and every one, claiming at one point that the method used by the CSS to create the manufacturing sector output estimates is regarded as "...reliable and conformable to international practice..." Despite the massive errors in individual industry estimates to be disclosed in Table 2-3.1 (Chapter 2-3),³⁴ the Head also suggested that users hold the CSS material in high regard (letter from CSS, 10 August 1992). My response to that was to point out that:

"To the best of my knowledge, no user but myself has ever subjected the manufacturing sector statistics to such detailed scrutiny. That being so, the claimed state of 'satisfaction' in which the users are reportedly to be found is probably more accurately described as the bliss produced by ignorance." (Letter to the CSS, 18 August 1992)

The note of exasperation that crept into the correspondence as it dragged on fruitlessly was born out of the frustration of trying in vain to wring from the CSS a detailed explanation of what exactly had been done, and why. It came as well from trying to extract from that institution an acknowledgement of the fact that the attempt to adhere to what are plainly impossibly rigorous interpretations of conventions³⁵ that are in themselves quite pragmatic, is not a virtue. The rectitude with which obviously inappropriate practices were defended was taken as indication of inflexibility, and was felt to bode ill for an institution that should play an important rôle in the transition to democracy.³⁶

33 The SARB response (reproduced as Appendix 2 in Meth, 1992), to the draft version of the monograph (Meth, 1991a) can best be described as unfortunate. A suitably rude response was dispatched to the authors of the offending document. A senior official of the CSS has stated in private conversation that the SARB response was ill-considered, and should not have become part of the official record.

34 A senior CSS official has informed me that it was only when these errors were pointed out to him that he started taking seriously the question of CSS procedures. (*pers. comm.* November 1992)

35 Yet another instance of a claim by the CSS to be following UN recommended procedures can be seen on page 2 of the letter dated 1 October 1993 (reproduced in Appendix 2-2). This alleged recommendation is used to justify to the Minister the switch from 'direct' to 'indirect' index estimation. If a claim of this type is to be taken seriously, detailed references to the precise passage in a UN document which supports the proposed change must be provided.

36 An indication of the extent to which progressive forces in South Africa (the then government-in-waiting) distrust the CSS is given by the fact that the task of creating a Poverty Profile has been entrusted by Cosatu and the ANC to the World Bank, an organisation not exactly noted for the esteem in which it was held by these bodies. To create the Profile, a massive survey, not unlike the CSS' Current Population Survey, had to be conducted. Instead of being able to draw on the structures and expertise of the CSS, the South African team responsible for carrying out the work on behalf of the Bank started from scratch - the CSS not being considered to be a suitable partner in this venture.

On the positive side, two things stood out. The first of these was that certain individuals in the CSS were very helpful. There were and are, of course, limits to what they could do, but within these they have provided many useful snippets. The second, and probably more important, was the growing perception, in my mind at least, that despite its poor showing in this affair, the Statistics Council has the potential to be transformed into a body that can reflect the needs and views of users effectively. Increased powers, a substantial widening of the purview or area of competence of the Council, and improved representation will be required.³⁷ Once these are granted, a number of institutions which at present are accountable only to some remote and powerful individual such as a minister of state, can be made more responsive to users' needs.³⁸

If the Statistics Council had been constituted along the lines suggested here, much effort could have been spared. At the first sign of possible inaccuracies in the manufacturing sector output estimates (the detection of a downward bias in the manufacturing sector employment estimates some time around August 1989), a suitably-qualified expert could have been employed to ask all of the questions that have been posed in 'More Problems...' and its companion (Meth, 1992).³⁹ Instead of being able to frustrate the inquiry, especially by being permitted to conduct an in-house investigation into the problem, the CSS would have been obliged to co-operate. That is the way things must be arranged in the future.

In the last days of the existence of the apartheid régime, there were some encouraging signs that the obstructiveness of the past would yield to a new spirit of co-operation, as all the participants in the debate became convinced of the fact that at heart, each of them was concerned to nurture the statistics-creating institutions and to foster the integrity of the statistical base. In that new climate, a major inquiry into the production and distribution of statistics in South Africa was mooted, enlisting along the way, the support of institutions which, it was imagined, would have been profoundly hostile to such an enterprise.⁴⁰

37 The question of the appropriate form of bodies such as the Statistics Council was examined at a recent meeting of the Royal Statistical Society. See Moore, 1992. See also the document known as the Crisp Report (Australian Government Publishing Service, 1974) - the review on the basis of which the Australian Bureau of Statistics was reorganised.

38 A prime example is the statistics-producing division of the South African Reserve Bank (SARB). It cannot be held to account by any institution in civil society, and yet it has enormous influence on policy formation. Its governor is well-known for his vociferous criticism of wage demands made by Cosatu affiliates. The statistical basis for his views are the poor statistics produced jointly and severally by the CSS, the SARB and the NPI. On this score, see Meth, 1991b and 1991c.

39 The ambivalence to which Griliches (1985) so gently draws attention, namely that "imperfections give us our legitimacy in the first place" makes its presence felt at this point. Were it not for the errors present in the statistics, I would have been denied the pleasure of what has been quite an exhilarating search, even though the work has been tedious at times.

40 Meetings with senior CSS and SARB officials in Pretoria in February 1993 following the tabling of the Cosatu document on statistics at the NEF (Cosatu, 1993) made it clear that such an inquiry would not be opposed, indeed would even be welcomed in most quarters.

The institution that made this possible was the National Economic Forum (NEF) - the tripartite body where business, the state and worker representatives (chiefly Cosatu) met to address economic issues and to seek consensus on their resolution. A written request was made to the Statistics Council to initiate such an inquiry (letter dated 15 February 1993). In addition to this, I wrote a document for Cosatu (Cosatu, 1993) outlining some of the major problems in South African statistics. This document, in which an appeal was made for the support of the NEF in ensuring that the inquiry did take place, and that the results were not pigeonholed, was tabled at a meeting of the Long-term Working Group of the NEF in February 1993. The government responded in June 1993 (SAG, 1993), acknowledging the need for a review of certain critical statistics for economic reconstruction and the need to look carefully at the Statistics Council. In the debate which ensued, it was agreed that under the circumstances, the NEF was the most appropriate body to conduct the inquiry, and a six- or nine-person committee, two or three representatives each from business, the state and labour was convened in August 1993. The first draft of Part II of this study was presented to the committee for consideration - in the absence of local expertise in the field it was agreed in principle by this committee that the document be submitted to an acknowledged international expert in the field for evaluation.

Delays were inevitable as the country geared up for its first ever democratic election. Since the election of the Government of National Unity progress has been slow as new officials wrestle with the mammoth task of undoing the worst ravages of apartheid. Certain important developments have, however, taken place. As noted in Chapter 1-5, with effect from June 1994, ministerial responsibility for the CSS has been assigned to the Minister without Portfolio (the Minister responsible for the Reconstruction and Development Programme). Officials within that ministry have created a task force to consider various aspects of the problems with statistics in South Africa. The task force met in October 1994 to consider proposed new legislation governing the Statistics Council. The NEF has been absorbed into the National Economic Development and Labour Council (NEDLC), a new statutory body housed within the Department of Manpower. A former member of the NEF committee on statistics, now active in the NEDLC, deeply concerned about the lack of progress in addressing the serious problems that beset statistics users and producers in South Africa, will be communicating this concern to the responsible ministers.⁴¹

41 *pers. comm.* Terence Moll, December 1994.

So what if the figures are wrong?

In the course of conducting the research which has led to the publication of this report and its companion (Meth, 1992), a great deal of energy has been expended to show that a single series, the manufacturing sector output estimates, is incorrect. It is important to stand back from all of this and to pose the question - so what if the figures are wrong? To answer this, one needs to have some idea on the manner in which these particular statistics inform the decisions of the various social actors who have recourse to them. Demonstrating that such and such a group demand that information of a particular kind be produced is one thing - it is another matter altogether to show that that information is used in a way that is congruent with what the producers of the information either intend or even imagine.

The processes by which economic information of the sort discussed in this study is translated into action are opaque - not too many studies deal with such questions directly. Eisner, in an article with the title "Divergences of Measurement and Theory and Some Implications for Economic Policy" (1989) looks at ways in which incorrect measurement has caused economists in the USA to draw false conclusions and to give incorrect advice, some of which has been taken.⁴² An interesting and quite polemical piece by Worswick chronicles the saga of the Phillips curve, and the policy uses to which this dubious but "...daring simplification of what is generally thought to be a highly complex problem" was put. Posing the question of whose fault it was that the "...initial results were given such a build up?", he lays the blame in the first instance at the feet of the "...those "communicators" who seized upon the article by Phillips and upon subsequent studies and dressed them up for public presentation." (1972; p83)⁴³

One of the more direct instances of incorrect figures influencing policy in a significant way is recounted in Harcourt and Kidson (1993). They note that:

"In the mid 1970s initial estimates of the British fiscal deficit for 1976-77 were put at £11.2 billion. The perception of a major fiscal crisis led to an arrangement of an IMF loan, controls on public expenditure and the introduction of monetary targeting. For many observers the disastrous British monetarist experiment started here and not with the election of Mrs Thatcher in 1979. As for the fiscal deficit of 1976-77, it turned out to be a far more manageable £8.8 billion, more than 20 percent below the initial estimate; with more reliable initial figures, 15 years of economic mismanagement may have been avoided." (p445)

42 The Eisner article examines the major weaknesses resulting from fact that economists and econometricians have lost essential communication with compilers and synthesizers of their data (1989, p2).

43 This particular article is one of very few that I have found which refers to the connections between academia and the media. The NPI is argued to have used 'communicators' in much the same way as that to which Worswick took such strong exception.

Questions about the influence of statistical measures, and the errors they invariably contain are a subset of the more general question of the rôle of the economist in policy formulation. A fair amount of work has been done on the way in which economists, working as economic advisers, function in relation to policymakers. Work has also been done on the way in which research findings by independent researchers and 'think tanks' find their way into the realm of policy formulation (Desai, 1994). Certain outstanding individual researchers like Milton Friedman have been influential in using statistical analyses to promote policy change. As one might expect, the process is not without controversy. The 1974 Nobel Memorial Lecture delivered by von Hayek contained a stinging indictment of economists who lead policymakers astray by, for example, persuading them to accept a false theory because evidence exists which:

"...is more "scientific", than for a valid explanation, which is rejected because there is no sufficient quantitative evidence for it" (1989, p3).⁴⁴

An example of an attempt to construct a theoretical model for understanding the market for economic ideas (and it smacks a little of the intellectual imperialism that is characteristic of many economists), was a book on the changing perceptions of economic policy by Cairncross. A reviewer of the work noted that it:

"...begins by asking how the ideas of economists become accepted in policy and practice. [Cairncross] poses a market for economic ideas where consumers and producers meet to satisfy perceived needs and examines how this market has altered since 1911. The size, scale and complexity of this market has expanded massively throughout the past seventy years and the influence of economists has fluctuated. How economic ideas do or do not get accepted, and then put into practice is an interesting and difficult question and Sir Alec's model seems a useful one for further development. More attention must be paid to broader political economy factors in understanding these issues and these may be accommodated in this general framework."(p411)

Even under competitive conditions, the identification of demand and supply functions is no simple matter - when it is ideas that are on offer, a 'product' unlikely to create any obvious price signals, an already difficult empirical task becomes more difficult still. The notion of a market for policy ideas in which "the economist's view of the world" (amongst others) is peddled, is present in a substantial review article by Nelson (1987), which, unfortunately, is restricted in its coverage mainly to the USA. This article examines the changing relations between experts and politicians in a historical context, pointing to both successes and failures of 'policy entrepreneurs'. Instances where economists have succeeded (and failed) are identified, but one of the more interesting aspects of the piece is the exposure of the relative weakness of claims to neutrality of the profession. It is often asserted by economists that

44 This was firmly rejected by Heller, who likened the blaming of Keynesian macroeconomics for inflation to saying that trees are the cause of forest fires (1975, p16).

"...ideology and values [belong] to the irrational part of the world..." Nelson, however, cites one view of the rôle of economic advisers in policy formation which argues that:

"The confidence of the economist's policy recommendations is essentially ideological: it rests on their commitment to the competitive market as an ideal, and the consequent belief that any step in the direction of the ideal is desirable.

He functions primarily as a propagandist of values, not as a technician supplying data for the pre-existing preferences of the policymakers. Some of his propaganda is directed at those participants in political decision-making to whom the advisers are directly responsible, aimed at shaping their values in the direction of the adviser's own. Much of it is directed through his political superiors to other participants in the political process - including the general public - and the adviser becomes, in fact, a supplier of arguments and briefs which seek to gain wider acceptance for the economist's political values." (1987, p59)

A welter of other research which has examined "...the impact of economic and other social science research on government decisions..." is cited, but although the researchers concerned seemed to believe that social science offered much, they:

"...still tended to reach pessimistic conclusions concerning the policy impact of technical calculations resulting from well-defined "problem-solving" efforts." (p59)⁴⁵

A brief search failed to disclose any similar work in South Africa - that being so, what follows, must, of necessity, be mainly speculative. Possibly the simplest way to tackle the question of the impact the incorrect statistics under consideration might have had is to identify the potential users of the manufacturing sector data, and then to consider the ways in which they might behave. Three broad groupings of users may be distinguished - decision-makers in firms constitute the first group, policy-makers (in government and other bodies such as trades unions, business associations) make up another, and the academic community, a third. In practice, the analyses produced by these groups, particularly the academic community, will interact to a certain extent to influence the general economic climate.

Without going into detail in what is an extremely complex issue, it is likely that certain economic data influence both short-term or tactical decisions and long-term or strategic choices. In principle, the importance of any particular error may be gauged by the extent to which it distorts decision making. Since information is always partial and imperfect, and since the actual process of economic decision making is, in any event, an intricate and mostly non-transparent process, it is difficult, if not impossible, to estimate the impact of added imperfections. Suffice it to say that if the errors are large enough, they can influence the analyses of each of the three groups identified. The extent to which results either confound or confirm expectations can also enter into the calculus. Manufacturing sector output estimates

45 Some work has been done on the reasons why policymakers within the state so often ignore economic advisers and policy analysts (Klitgaard, 1991, pp217ff; Peacock, 1992).

may do this to a somewhat lesser degree than sensitive measures like trade figures, unemployment totals or inflation estimates. There are times, however, when politicians cling desperately to results suggesting a fractional increase in manufacturing output or employment levels. This is especially true when the proverbial 'light at the end of a tunnel' is being sought by policy-makers under fire for causing or allowing austerity measures to inflict intolerable social suffering. Whether or how such news actually influences economic actors is difficult to determine.

Without being able to support the proposition fully, I would like to suggest that manufacturing sector data have, up until now, been relatively unimportant in policy formation processes, with one notable exception, to be discussed below. The first group of users singled out above, the business sector,⁴⁶ is alleged by the CSS to set great store by the data, presumably as a form of commercial intelligence. If that is so, then the results of this study suggest that they have been most sadly misled (not deliberately, of course, but the end-result is the same). Some of the manufacturing sector data to be discussed below are so massively inaccurate that if they were the sole source of information on which decisions by this grouping were based, then serious errors must automatically have ensued.

It seems improbable that the business community would so easily be taken in - important decisions, especially about production levels and investments, are unlikely to be based on single sources. The greater likelihood is that the incorrect information produced by the CSS has been balanced against intelligence garnered from other sources. There is evidence, admittedly mainly anecdotal, that many in business treat CSS output with contempt, at least in part because of the haphazard way in which they themselves complete, or cause their underlings to complete, the often admittedly elaborate survey questionnaires distributed by the CSS. If this is so, then the statistics have even less influence than may be imagined at present. For the meanwhile, the truth of this proposition and of the question of what has actually happened among statistics users in South Africa's recent past must remain part of a large research agenda. I would venture the opinion that at worst, the monthly output estimates produced by the CSS may have confused some business decision-makers by leading them to believe, falsely, that they were falling behind their competitors. Alternatively, when the figures understated production, they may have bred a generalised complacency, as each producer believed its market share to be increasing. If firm behaviour is of the satisficing type,

46 Business confidence is an important determinant of investment, so too the extent that output estimates function as a measure of economic activity, they may influence the overall climate within which investment and expansion decisions are taken. Certainly, measures such as the PPI have an important announcement effect. This has been investigated by Chang and Rhee (1986).

this may have affected motivation, but if profit maximising is the norm, then it is unlikely to have been of consequence.

Policy interventions on a wide front are undertaken by the state, and it is not inconceivable that some of these have been influenced by the errors disclosed in this study. Once again, though, it seems unlikely that these have been of much consequence.⁴⁷ As far as industrial policy is concerned, apart from the standard macro-economic tools, the most important state interventions could be argued to be tariff policy, regional development and strategic investments. Although there is now considerable pressure for change, the first of these is notorious for its *ad hoc* character - responding more to the demands of rent-seeking producers and to those under threat (real or imagined) from dumping, than to any systematically executed plan based on economic indicators of the type to be discussed below. As for the regional industrialisation policy, the growth of this political stepchild, born of a desperate desire by the Nationalist government to prop up corrupt bantustan leaders, was ultimately only curtailed by the slow-dawning recognition that profligacy on the required scale simply could not be sustained. Strategic investment does not warrant any comment.

Precisely how much influence academics in South Africa have had on policy formation in the past is not easy to measure. The stream of criticism of the failed regional policy may have had some effect, as may the equally persistent alarms about rising capital intensity. Whether or not their contribution in the past was important, there are two reasons why accurate information is important for this grouping. The first is the relatively detached activity of writing an accurate economic history.⁴⁸ The detachment is, however, only relative - at times, history becomes hotly-contested terrain. This history could be of some consequence in helping to form the policies of the state that has replaced the apartheid régime. This state is likely to be interventionist, but not in the irrational manner (one hopes) of its predecessor. In this context, academic analysis and good data will probably become much more important.⁴⁹ A rash of research endeavours, recently undertaken in such varied institutions as the World Bank and the Economic Trends group's Industrial Strategy Project all made use of the existing

47 The letter to me from the Statistics Council dated 1 October 1993 reporting on the deliberations of the sub-committee set up to examine problems in the quality of the manufacturing sector output estimates, reproduces from a document prepared by the CSS a comment to the effect that:

"...although fairly significant differences [do] occur between the results of the two ways tested for estimating real value added in manufacturing, the order of magnitude of these differences, historically at least, has been sufficiently minor to suggest that no different economic-policy decisions would have resulted from the availability of one set of estimates rather than the other." (p2)

48 See Solow (1985) for a discussion on a desirable relationship between economics and economic history - note in particular, his comments on "rough" quantitative judgements (p329).

49 The problematic relationship which econometricians have with the data on which they operate is discussed in Griliches (1985). Attention is drawn in this article to the very slender connection between the practitioners of this art and the mainly government bodies responsible for the collection of data.

incorrect output. It is highly likely that the results of these activities have been affected by the errors present in the published data.

The one area where the errors have undoubtedly influenced the course of events is in the debate over labour productivity. That assertion need not be backed up here - I have described at length in Part I of this study the way in which the NPI has used the poor CSS data to underpin the national campaign to improve productivity. The merits of the campaign, and of the attempt by the NPI at passing off its activities as politically neutral are addressed there. One outcome of the campaign was a generalised hostility on the part of state and capital alike to worker organisations such as Cosatu, which in the past have extracted what were described as 'political wage increases'.⁵⁰ Timely publication of better estimates of output would have taken some of the sting out of this campaign. Even if the truth about manufacturing sector output levels does not aid at all in policy formation or business decision making, it is worthwhile pursuing so that the historic wrong done to worker organisations can be set to rights. The debate about the relationship between wages and productivity is unlikely to become non-contentious in the future, so there is a strong imperative to keep this sensitive series in good condition.

50 The best evidence that such perceptions exist is the study by Welcher (1990) into the causes of increasing capital intensity - trade union activism features importantly. It is interesting that the Welcher study elicited 'perceptions' only - it did not enquire as to whether or not these accorded with reality.

Chapter 2-2

The Euler Consistency Test

Introduction

The aim of this chapter is to describe the development of a simple tool or technique for exposing certain types of errors in the national accounting statistics. The tool, which I call the Euler Consistency Test (ECT), is then applied to the data for the mining and manufacturing sectors at critical junctures in their respective histories. The ECT can be used to indicate points at which constant price output estimates in the national accounts begin to drift from their 'true' or expected values. The test consists of two distinct operations. In the first of these, the relevant data are interrogated to see whether they contain differences that may constitute evidence of the possible presence of one or the other of two types of error to be identified below. When such differences are detected, a second, and more controversial operation, the assessment of whether or not these differences are 'reasonable', must be attempted. It is difficult to specify in advance, the differences one would regard as 'normal' or 'abnormal' in an economy, not least because of the extreme difficulties of defining adequately the notion of 'normality'. Judgement, based sometimes on little more than the feel of the data, has to be exercised.

Developing the test

Development of the ECT takes place in three stages. In the first stage, theoretical reasons are advanced for the proposition that under conditions of relatively stability in an economy, ie, conditions marked by an absence of severe supply or demand shocks, even when there is fairly rapid inflation, most aggregate market-related domestic price indices (deflators) in the economy will grow at roughly the same rate. Making certain restrictive assumptions which yield a stripped-down model of an economy, the deflators are shown to be identical. Relaxing these assumptions to bring the economy closer to the real world yields the prediction that the aggregate market-related domestic price indices will grow at similar, but not identical rates. Evidence in support of this hypothesis is then offered. Some of it is from Britain. This is presented for the period 1960-81, an era which at times saw inflation rates approaching 25 per cent. Similar evidence for South Africa for the period that is critical to the issues under consideration here is also offered.

Following this, a brief digression on the impact of rapid price changes in the gold mining industry in South Africa is given. The aim of this is to show how the inclusion of the results for an important industry experiencing rapid price changes can make it appear as though structural transformation of not inconsequential proportions is taking place in the economy more generally.

The second stage of the development of the ECT commences with an examination of the properties of the truism that in a closed economy, the real value of the income measure of GDP - GDP(I) - is equal to the sum of the real value of remuneration plus the real value of gross operating surplus. Since in a base year, the real and current price values of these three magnitudes are equal, and since, in principle, any year may be nominated as base year, the ratios of any of these magnitudes to any other of them should be the same in real or current price terms. In other words, income shares in any particular year must be the same whether measured in current or in real terms.

Relaxing the closed economy assumption introduces net factor payments from abroad into gross surplus, with unpredictable results. Moving still further into the real world entails acknowledging that finding deflators for each element of the truism is problematic. Real values of GDP(I) cannot be obtained because appropriate deflators for gross operating surplus do not exist. Estimating the real value of remuneration is, as the discussion in Chapter 1-2 on the appropriate deflator for this magnitude - the PPI (to yield the product wage), or the CPI (to yield the consumption wage) - demonstrated, only marginally less fraught.

To circumvent this trap, the sample relation revealed in Stage 1 is called upon. Pragmatically, since market-related domestic price indices change at roughly the same rate over time, the use of any of them in an attempt to estimate real remuneration, real gross surplus or real GDP(I) should not do too much violence to the truth of the matter. Applying this reasoning to the data, the 'Shares' version of the test is performed (tentatively) on the mining sector results for the period 1972-80, and, a little more confidently, on the manufacturing sector results for the period 1979-89. Large residuals emerge in both cases.

In Stage 3, development of the 'Deflators' version of the test is undertaken. Derived from the 'Shares' version of the test, and formally its equal under certain restrictive assumptions, the 'Deflators' version of the ECT has the advantage of enabling one to identify, to within about four or five years, the point at which an output series become unreliable. This may not sound like a very precise instrument, but when it is considered that at present, no consistency tests of this type are performed at all, it is clear that the 'Deflators' version of the test represents a

considerable advance. Before turning to the development of the ECT, we will take a brief look at the background to it.

Background to the test

In order to make clear the significance of what is at issue here, it will be useful to draw attention to the distinction between what might be termed 'economic reality', and the theoretical and statistical constructs which economists use to attempt to reproduce in thought, (some of) the economic interactions that take place in the 'real' world. Conventional national accounting, one of the more important of these 'thought constructs', uses a broadly Keynesian theoretical framework as a means of directing the collection of data in particular categories. The 'materials balance' approach to national accounting, used in the former socialist economies, represented a different theoretical stance - it asked different questions of the accounts, and focussed on different aspects of 'economic reality'. The differences between these approaches are of no concern here,¹ suffice it to say that national accounting is a process that seeks to ascertain certain 'dimensions' of an everchanging 'economic reality', based upon some or other theoretical framework. In the capitalist economies, this is done, with varying, and sometimes limited, success, using a set of conventions codified by the United Nations into the document known as the *System of National Accounts* (SNA).

Individual member countries of the United Nations are allowed some leeway in the application of the SNA rules - rules that are revised from time to time. In the absence of theoretical resolution of the conceptual problems involved, many of these rules rest upon convention. The ability of these conventions to cope with 'reality' varies, depending not only on the severity of the shocks to which economies are frequently subject, but also on the pace at which the structural transformations that characterise economic growth and development take place. Large, rapid price changes in certain commodities, eg, gold or oil, which contribute a substantial proportion to total output in some economies, are known to distort the workings of these internationally agreed rules.² So too, do the transformations caused by growth or decline. The wisdom of national accounting statisticians (and any users) who place too much faith in what are quite fragile tools is questionable.

1 For an examination of the differences between the SNA and the Material Product System (MPS), see Chapter 6 of Lintott (1982). The SNA is in the process of being introduced in the Commonwealth of Independent States - see Ivanov *et al* (1993).

2 The compilers of the SNA have long been aware of these problems. No guidance, however, is offered in the SNA to users confronted by spells of severe price instability.

'Economic reality' is not directly altered by the fact that it cannot be measured adequately by national accounting statisticians. To the extent that the statistics produced by official statisticians (and others) affect people's perceptions of that 'reality', their responses may be influenced in one way or another, but the connection between these responses and 'reality' is likely to be tenuous, a problem that has not received enough attention in the literature. If one can improve the quality of the information on which economic agents act, or at least make them aware of the inherent fragility of the information to hand, one is taking a necessary (but not sufficient) step towards improving the quality of those actions. The Euler Consistency Test (ECT) looks as though it can contribute in a modest way to the goal of improving the national statistics - one of the more important sources of information on the economy.

In Appendix 2-1, a phenomenon which I call the Perverse Deflator Effect (PDE) is described, and the conditions under which it can occur are spelled out. This effect came to my attention as a result of further research into some of the questions raised by the paper I have called the Fosatu Challenge (Meth, 1983). In that paper, the deflation process that converts the income windfall consequent upon a massive increase in the gold price into a fall in GDP was criticised. An attempt to show that this pointed to an error in the national accounting procedures, made use of a primitive version of the Euler Consistency Test (ECT).³ Changes in the gold price, the primary cause, in the South African case, of the Perverse Deflator Effect give rise to changes in the terms of trade, for which adjustments in the national accounting estimates are made. Because of this, the ECT is probably less reliable when applied to the mining sector data than it is in sectors where the terms of trade effects are less prominent, as they are, for example, in manufacturing.

When I used the Euler Consistency Test (ECT) in its embryonic form (Meth, 1983), the NPI, prompted by Swanepoel and van Dyk (1983) dismissed the approach out of hand (NPI, 1983). That response was ill-considered - the ECT, if not theoretically watertight, will be shown to possess the admirable quality, when applied to the manufacturing sector output estimates, of having worked well on two separate occasions. The first of these is a retrospective test. If the ECT had been applied at any time after 1975 and before revised and rebased figures for the period 1970-79 were published by the CSS (SNR P12.1, 4 June 1982), it would have exposed a substantial error in those figures, drawing only on information available at the time. This is demonstrated below in Table 2-2.6.

On the second and more recent occasion, the presence of further large errors in the manufacturing sector output estimates for the period 1981-88 was successfully predicted

3 A reasonably complete version of the ECT was given in Meth (1993a).

(Meth, 1992; 1993a and 1993b). After a lengthy delay, the errors made by the CSS were finally acknowledged in the letter to me containing the report of the findings of the sub-committee of the Statistics Council referred to in Chapter 2-1. That letter makes reference to an investigation conducted at the request of Dr Treurnicht du Toit by the staff of the CSS. It examined different ways of estimating the real value of manufacturing sector output. The letter offers the following summary view of the findings of that investigation:

"The principal finding in the report (Document A) was that, although fairly significant differences to (*sic*) occur between the results of the two ways tested for estimating real value added in manufacturing, the order of magnitude of these differences, historically at least, has been sufficiently minor to suggest that no different economic-policy decisions would have resulted from the availability of one set of estimates rather than the other." (Appendix 2-2, letter dated 1 October 1993)

The publication, in November 1993, of the substantially revised estimates was the way in which the errors were made known to the wider statistics-using public. No reference to the investigation, nor to the conclusion that the errors were sufficiently minor as not to trouble policymakers was made.⁴ Implicit in this somewhat surprising claim is the conclusion that so unimportant is the accuracy of the manufacturing sector output estimates that it makes little difference whether they record a growth rate of one half of one per cent *per annum* or two per cent *per annum* over a six-year period - an interesting revelation. Be all this as it may, the fact of the matter is that the ECT detected the errors. As long as it is remembered that the test is a crude device whose only function is to expose 'fairly significant' errors, it can effectively be used to trawl for them in the national accounts.

The test is suggested by the theorem derived by Wicksteed from Euler, a theorem that is discussed and dissected in an article by Joan Robinson published in the *Economic Journal* of 1934. In the article Robinson refers to the "...history of the famous "adding-up problem"...first canvassed in 1890...and still the subject of lively controversy" (Robinson, 1934, p398 and p398n).⁵ The theorem states that the total proceeds from production in any period t , when distributed amongst the (four?)⁶ factors of production, are wholly exhausted, ie, there is no positive residual, nor do any factors go unrewarded. Since it is obvious, as Robinson points out, that "...the total product is distributed among the factors of production..." (p398), her concern, and that of the other participants in the debate was with

4 See SNR P3041.3 of 12 November 1993.

5 Life is breathed into this apparently dry and dusty debate by the lively language and engaging manner of economists writing in the early part of the century - professional style had not yet degenerated into the arid scientificity that typifies much of today's work. At one point in the article, Robinson states that:
"The peevish egoism of Walras contrasts unfavourably with the modesty and single-mindedness of Wicksteed." (p401n)

6 Note that in the national accounts, consistent time series data are available for only two factors of production, labour and capital. The returns to the latter are aggregated, and include rent, interest and profits.

the relationship of actual rewards to the (hypothetical) marginal products of the co-operating factors.⁷ It is not necessary here to go into the intricacies of marginal productivity theory - it is sufficient that output is produced, is sold and the proceeds of the sale of that output are distributed. The first version of the Euler Consistency Test (the 'shares' version) makes use of this property of an economy to show that when national accounting conventions fail, for whatever reason, hypothetical residuals, either positive or negative, emerge. The other version of the test registers deviations of the Consumer Price Index from the implicit GDP deflator. It will be shown that the two approaches are formally identical.

Stage 1 - The relative equality of deflators (price indices)

Consider, for the moment, a simple closed economy in dynamic equilibrium such as that envisaged by, for example, the Harrod-Domar or the Solow growth models. Under these conditions, capital/output ratios, capital/labour ratios, consumption, investment and savings, and all other relevant ratios will be at their warranted or appropriate levels. If changes in consumer tastes and technological progress are also assumed away, there will be no intersectoral resource transfers. Assume further that this imaginary or ideal-type economy is plagued by inflation (at a fixed rate), but that it is fully anticipated and that steady-state growth is unaffected by it. The inflation may, for the present purposes, be assumed to arise from causes outside of the model - possibly a permissive government which prints too much money. Under these conditions, quantity relationships remain steady over time, and price relationships change at a fixed and predictable rate. The Consumer Price Index (CPI), the Production Price Index (PPI), the GDP deflator and any other deflator one cares to name would thus all be perfect predictors of the implicit GDP deflator in the any sector. In other words, any of them could be substituted for the other, and the same output trends would still be measured.

This hypothetical economy is doubly removed from 'reality' in that it takes the already highly abstract concepts used to represent a 'real' economy and removes them still further from 'reality' by assuming away the most important effects of everyday economic activity, eg, changes in tastes and technology. Its usefulness lies in the fact that it constitutes the most basic

7 Attempts to cast the theorem in marginal product terms run into difficulties unless certain quite restrictive assumptions are made about competition and about economies of scale. Robinson picks her way nimbly through the mire, but simple-minded conventional economics displays a certain reluctance to admit of economies or diseconomies of scale in production. If there are economies of scale, then marginal product exceeds average product, and the total value of the claims on total product exceed the value of that product. If, on the other hand, diseconomies of scale arise, then average product exceeds marginal product, and there is a residual which is undistributed. Solow (1991, p394) describes recent developments in neoclassical growth theory which make increasing returns to scale at the macroeconomic level compatible with constant returns at the microeconomic level.

form of a model in which the national accounting identities would always reflect the exact 'truth' of what was happening in the economy.

Rather obviously, real world economic processes do not obey the strict assumptions used to construct the model. Each movement away from these idealised conditions may be seen to impose strain on the model and the conclusions drawn about the relationships between the variables. It is thus to be expected that differing relative growth rates of output in different sectors and different growth rates of the corresponding prices will occur. The test works on the assumption that these changes will, in general, be marginal rather than quantum leaps. It will be argued below that a sudden and large divergence between any pair of price indices or aggregate deflators⁸ is evidence either of error and/or of major structural change. One thus relaxes the assumptions of the model by releasing it into the real world. Economic growth or stagnation causes relative price and quantity indices to drift apart, but all indices are formally reset to zero when rebasing takes place every five years or so. This is tantamount to recalibrating the model.

Some evidence in support of the proposition that certain price indices or deflators move roughly in line with each other (and that where they do not, plausible explanations are to hand) is given in Table 2-2.1 below. This table shows how various indices and deflators in the UK and in South Africa behaved over two critical periods. The UK case includes the phase of galloping inflation of the 1970s, and the South African data cover the period of particular interest as far as the manufacturing sector results are concerned. Scanning these results one may say with some confidence that aggregate indices or deflators for market-disciplined activities move very much in step with one another. Growth rates of what could be regarded as the three major market-related aggregate deflators or indices, Consumer's expenditure; Total domestic expenditure, and Total final expenditure all lie within narrow bands. In the earlier period in the UK the range was 3,9 to 4,0 per cent; for the later period it was 11,5 to 12,0 per cent, and for South Africa the range was 12,7 to 13,3 per cent.

8 Thus far, the terms 'price indices' and 'deflators' have been used loosely - as though they were interchangeable. To be more precise, a price index, such as the CPI or PPI is normally measured by regular surveys. By contrast, many of the deflators, eg, the implicit (or implied) GDP deflator (IGDPD), are derived by dividing current price estimates by their constant price counterparts. Although errors of measurement occur in the former, it is the latter, the deflators, that are most frequently at fault in this study. The IGDPD of particular interest in this chapter - that for the manufacturing sector - is obtained by dividing the value of GDP (net output) in manufacturing in current prices by the corresponding estimate of the GDP in constant prices. In national accounting practice, the contribution to GDP of each sector in constant prices is built up from estimates of the various contributions at the 3-, 4-, or 5-digit SIC level. Sectoral (major division) contributions are then summed to estimate total GDP. The overall GDP deflator is thus a synthetic index - it should equal the weighted average of every actual or implicit deflator used to derive the value of the GDP in constant prices.

Importantly for the ECT, the growth rate of the Implicit GDP Deflator (IGDPD) in manufacturing in the UK fell within the narrow range set by the major deflators and indices. This was not so in the case of the IGDPD for South African manufacturing - a fact of considerable significance in what follows.

Table 2-2.1 Growth rates of index numbers and implicit deflators in the UK, (1960-70 and 1970-81) and South Africa (1979-89)

	UK		South Africa 1979-89
	1960-70	1970-81	
Consumers' expenditure	3,9	11,5	12,7
General government final consumption	5,4	13,3	14,1
Gross domestic fixed capital formation	3,3	12,4	12,9
Total domestic expenditure	4,0	12,0	13,3
Exports of goods and services	3,2	11,6	11,5
Total final expenditure	3,9	11,9	13,0
Imports of goods and services	2,9	11,4	12,4
Implicit GDP deflator - manufacturing	-	11,7	14,1

Source: UK - estimated from the implied index numbers in the *Blue Book, 1982 Edition*, Table 2.6. The Implicit GDP Deflator is estimated from Tables 1.9 and 2.3.
 South Africa - estimated from SARB *Quarterly Bulletin*, December 1987, pS-86, and December 1990, pS-85.
 The Implicit GDP Deflator for South African manufacturing is estimated from pS-83 in the December 1987 *Quarterly Bulletin* and pS-84 in the December 1990 *Quarterly Bulletin*.

Notes: Growth rates are per cent *per annum* compound.
 The growth rate for the Implicit GDP Deflator in manufacturing for the UK is for the period 1971-81. Current price data for the earlier period were not available.

In both sub-periods in the case of the UK results, the anomalies or departures from an average for market-disciplined activities are consumption expenditure by general government and imports.⁹ In neither case do market processes within the country have direct impact on the two sets of expenditures. Without direct market discipline, government inflation can easily exceed the general level of price increases, and probably for quite lengthy periods. Unless foreign exchange movements are fully compensatory (and in the long-run one would expect them to be) domestic and foreign rates of price change can differ quite considerably.

Similar conclusions apply in the South African case as well - government inflation is higher than domestic inflation on average. The collapse of the rand in 1985 ensured that import price increases in rands almost kept pace with domestic inflation, despite rapidly declining levels of inflation in South Africa's major trading partners. The relatively lower rate of growth of export prices is probably an artifice resulting from the fall in the relative price of gold - a

⁹ Further confirmation of this hypothesis is suggested by the UK results in Table 2-2.6. In Column 2 of that table, after 1964, the implicit GDP deflator in the manufacturing sector differs from the Retail Price Index by an average of less than three per cent in absolute terms.

major contributor to export earnings. The core items of marketed domestic expenditure all show roughly similar rates of price increase.

Others have commented on this regularity in the past - Mohr *et al* point out for example, that:

"...between 1980 and 1986 the CPI, GDE deflator, GNP deflator and PCE [private consumption expenditure] deflator increased at average annual rates of 14,8%, 14,3%, 14,2% and 14,5% respectively."

Significantly, they note that:

"The only index which has to be avoided is the GDP deflator." (1991, p46)

The first stage of the proceedings is more or less complete - it may be argued that there is a modest but respectable theoretical rationale for proceeding with the calculations. At this point, it is also appropriate to refer back to the discussion in Chapter 1-4 of Leibenstein's (1976) notion of "sample propositions" - propositions which specify:

"...relationships that in themselves are not necessarily true. They are samples in the sense that they suggest the form the theory should take." (1976, p22)

Leibenstein's argument that "...sharp prediction employed as a rejection rule is an inappropriate criterion..." and his deployment instead of the 'sample relation' concept as a 'soft prediction' tool which retains some "...degree of prediction as a quality..." provides an additional theoretical defence of the use of the Euler Consistency Test. The ECT, it will be argued, is "frequently" capable of providing useful bits of information, even though it "...does not pass the prediction test employed as a rejection rule." (1976, p22). It is claimed that the relation which the ECT sets out to examine will frequently be confirmed. Occasionally, the ECT will fail. The frequency with which it is confirmed (despite occasional failures) demonstrates the usefulness of the relation, and the fact "...that it contained some degree of predictability." (1976, p23). In short, it is expected that the relevant deflators and indicators will move 'roughly' in step - and it is expected furthermore that when they get out of line, either an error has been made in the national accounting statistics, or some major structural change with which the accounts cannot cope has taken place, or some combination of these two has occurred.

This proposition will now be considered in somewhat greater detail by examining an aspect of the impact of rapid changes in the gold price on the national accounts between 1970 and 1985.

Real perturbations vs statistical artifacts

National accounts still rely heavily on Laspeyres¹⁰ indices despite the fact that their use elsewhere has, on occasion, proved to be decidedly sub-optimal. To all of the other hazards to which the output estimates are prone must therefore be added the possibility of further errors resulting from incorrect theoretical formulation. In this study, it will be assumed that for most purposes, Laspeyres indices are adequate, at least in the medium term. This is tantamount to expressing the hope that these crude approximations of a complex and ever-changing reality will yield a tolerably accurate report in which, under 'normal' circumstances, a reasonably consistent set of relationships will exist between most, if not all, deflators.

Sudden or violent structural change in an economy constitutes a breach of these 'normal' conditions and is likely to disturb fragile national accounting conventions. A pattern of changes in the sectoral weights (price and quantity relativities) such as that shown in Table 2-2.2 below - especially for the gold mining industry, where the weight of the industry (proportional contribution to total output) jumps from 5,5 per cent to 16 per cent in a decade, only to fall back to ten per cent five years later - does not meet this criterion of an absence of 'sudden and violent' change. The predicted stable relationships are therefore unlikely to exist.

Table 2-2.2 Sectoral Weights for Various Base Years

Sector	1970	1975	1980	1985
Agriculture, forestry and fishing	8,11	8,24	6,96	5,85
Mining and quarrying	10,06	12,31	21,96	15,29
Gold mining	(5,51)	(8,32)	(16,05)	(10,20)
Manufacturing	23,31	23,18	21,74	22,65
Electricity, gas and water	2,56	2,36	3,32	4,25
Construction (contractors)	4,23	5,27	3,59	3,67
Commerce, catering and accommodation services	14,93	14,19	11,74	11,77
Transport, storage and communication	9,62	9,27	8,70	8,78
Finance, insurance, real estate and business services	14,45	12,64	10,96	13,85
Community, social and personal services	1,90	1,50	1,41	1,69
Less imputed financial services	(-1,74)	(-1,49)	(-1,83)	(-2,81)
General government	9,44	9,66	9,29	12,51
Other producers	3,13	2,87	2,15	2,50
Total	100,00	100,00	100,00	100,00

Sources: 1970 and 1975, SNR P12.1, 4 June 1982; 1980, SNR P12.1, 21 January 1986; 1985, SNR P0441, 27 February 1989.

10 The Laspeyres volume index is a base-weighted index, ie, it takes the form $\frac{\sum Q_n P_o}{\sum Q_o P_o}$. Laspeyres price indices are written $\frac{\sum Q_o P_n}{\sum Q_o P_o}$. Current-weighted, or Paasche volume indices are of the form $\frac{\sum Q_n P_n}{\sum Q_n P_o}$, whereas Paasche price indices are expressed as $\frac{\sum Q_n P_n}{\sum Q_n P_o}$. Further discussion of the properties of these indices may be found in Appendix 2-3, 'Selected Pages from the SNA'.

When the destabilising effect of gold price fluctuations is removed, as is done in Table 2-2.3 below, a slow and steady structural transformation of the remaining sectors of the economy may be observed.

Table 2-2.3 Proportional Contributions to GDP excluding Mining (current prices), Various Base Years

Sector	1970	1975	1980	1985
Agriculture, forestry and fishing	9,0	9,4	8,9	6,4
Manufacturing	25,8	26,4	27,9	26,3
Electricity, gas and water	2,8	2,7	4,3	4,8
Construction	4,7	6,0	4,6	4,2
Commerce, catering and accommodation services	16,8	16,2	15,0	13,5
Transport, storage and communication	10,7	10,6	11,2	10,2
Finance, insurance, real estate and business services	16,0	14,4	14,0	17,8
Community, social and personal services	2,1	1,7	1,8	2,1
Less imputed financial services	1,9	1,7	2,3	3,2
General government	10,5	11,0	11,9	15,1
Other producers	3,6	3,4	2,8	2,8
Total	100,0	100,0	100,0	100,0

Source: Estimated from the data in *South African Statistics 1988*, p21.9

A glance at the changes from the base year 1970 to 1975, or from 1975 to 1980 confirms the varying fortunes of the different sectors. Agriculture declines in relative importance, manufacturing fluctuates around the 26 per cent mark, electricity, gas and water nearly doubles its (relatively small) contribution, whereas construction, after a rapid jump between 1970-75 slumps somewhat by 1985. Commerce declines quite significantly - the only sector to grow is general government, a fact of South Africa's economic performance that has often been decried. It is instructive to compare the magnitudes of the changes reported in Table 2-2.3 with those in Table 2-2.2. In manufacturing, for example, the peak contribution when gold is removed occurs in 1980 - when gold is included the peak is in 1970. Finance and real estate fall in relative importance in the inclusive figures, but rise between 1970 and 1985 when gold is taken out.

Such is the dynamism of capitalist development that even in an economy experiencing relatively steady growth (economic transformation), the price and quantity (and value) relativities that obtained in an economy (or sector) at the beginning of the period demarcated by any particular base year are extremely unlikely to hold at the end of it. In the South African case, once gold mining has been removed from the accounts, however, the Laspeyres (base-weighted volume) indices that are used to estimate the value of output in constant prices

probably do not do too bad a job.¹¹ Inevitably there will be some deviation from 'reality' as measured, say, by the constant price estimates, but this is assumed to be within the bounds of the tolerable.¹²

When structural change, at a measured pace, does take place, one may expect this to be reflected in a set of slowly changing, but still relatively stable differentials between the deflators for the different sectors of the economy. This proposition has two implications - firstly, that under certain conditions, there will exist stable relations between deflators and indices which permit them to be substituted for each other (or used to backcheck each other). This has already been demonstrated in Table 2-2.1 above. The second is that the national accounting conventions are not capable of dealing adequately with changes of more than a certain magnitude. When these occur, the national accounts can be expected to break down in various ways. There is no precise way of determining what these magnitudes are.

The Euler Consistency Test (ECT) can function as an 'early warning system' that detects two different types of error in the national accounts. These flow from the conditions discussed above. If an economy, or a significant sector of an economy, is experiencing relatively stable growth conditions, ie, the structure of prices and quantities is not undergoing traumatic change, then a sudden breakdown of stable relations between deflators is probably evidence of substantial error in the national accounts. If, on the other hand, sudden and violent change

11 One of the more common conventions upon which national accounting statisticians rely is the use of Laspeyres approach to the estimation of volume indices. In the resulting indices, price and quantity (volume) relativities or 'weights' obtained from the current price estimates in the base year are valid throughout the currency of that base year.

The real world, however, is not bound by the assumptions which national accounting statisticians use to simplify it, and in reality, actual price and quantity relativities change or drift away from those observed in a base year. Obviously, for national accounting purposes, the price and quantity relativities established in a base year hold for the entire duration of the currency of any particular base year (usually for about five years). A significant movement away from these weights in the real world, however, especially if it comes soon after the base year has been changed gives rise to the kinds of problem discussed here - problems with which the accounts are ill-equipped to deal.

12 The use of terms such as 'reality', 'true' value, 'tolerable', etc, opens wide one of the statistical profession's Pandora's Boxes. The question of what is 'tolerable' is not easy to answer in a manner that is not tautological. Pragmatically, one could argue that national accounting statisticians and the policymakers who utilise the information they produce become accustomed to tolerating statistics which they imagine or believe or estimate to be accurate (reliable?) to within, say, for example, the ranges specified in Maurice, 1968, pp39-42. When rapid inflation and/or rapid structural change pushes errors beyond these limits, the time to recalibrate the statistics is approaching.

It used to be the case that national accounting statistics were rebased every ten years or so. That period is now five years. The change may be interpreted as an implicit acknowledgement of the fact that the longer period led to 'intolerable' inaccuracies. Rather obviously, this international convention will not suit the circumstances of each economy. Errors are likely to vary quite significantly from country to country with consequent damage to reliability in those countries posting greater errors when rebasing does take place.

Some of the more affluent countries lay down performance criteria specifying acceptable levels of error as evidenced, for instance, by subsequent revisions to estimates. A case in point is the UK - one set of targets is spelled out in the CSO *Agency Framework Document* (1991, pp26-29). A variety of standards may be seen to hold, ranging from a maximum absolute average figure, to a stipulation that, say, no more than one of four or five revisions may exceed a certain percentage. The penalties for failing to meet these targets is not specified.

does take place, as occurred for example during the period 1972-80 when the gold price jumped from \$35 to \$800 an ounce, the ECT can be used to show where the national accounts cease being able to capture 'reality' adequately. In both instances, the ECT will pick up an unexplained deviation of certain indices from their 'normal' or expected pattern. In the first case, the error is statistical, and in the second, the error arises from changes in the 'real' economy. Detailed investigation is required to discover which of the two has occurred. In practice, of course, the national accounts could well be affected by combinations of these two types of error - distinguishing between them is unlikely to be a simple task.

Stage 2 The 'Shares' version of the ECT

The first of the two methods of performing the Euler Consistency Test compares the shares (proportions) of total output in constant and current price (nominal) terms going either to capital or labour. It will be argued that in any particular year, the two estimates of the share going, say, to labour, yielded respectively by the current price and the constant price data should be roughly the same. A significant difference between them can only result from some divergence of the relevant price indices from their warranted values. This, it was argued above, '...is evidence either of error and/or of major structural change' not accurately captured by the national accounts.

The assumption that the economy is closed (there are no exports or imports) will be maintained, and the economy is also assumed for the moment to consist of a single sector only. Assuming that there are no exports of goods means that changes in the terms of trade, with their impact on consumption possibilities, do not have to be considered. If there are no imports of goods, then, following the reasoning adopted by (Lawrence and Slaughter, 1993), one measure of real compensation (the product wage) may be obtained by deflating the current price estimates of remuneration by the Production Price Index (PPI). If no factor services are imported or exported, then the problems caused by differential rates of inflation between countries do not have to be taken into account. The relevant variables (in any year t) are as follows:

Y = GDP (at factor cost or incomes) in nominal price terms
 y = GDP (at factor cost or incomes) in constant price (real) terms
 P_y = implicit GDP deflator

W = remuneration in nominal (current price) terms
 w = remuneration in constant (real) price terms
 P_w = wage deflator

S = gross operating surplus in nominal (current price) terms
 s = gross operating surplus in constant price (real) terms
 P_s = gross operating surplus deflator

In terms of Euler's theorem, in the base year o :

$$Y_o = W_o + S_o \dots\dots\dots (1)$$

and, by definition:

$$y_o = w_o + s_o \dots\dots\dots (2)$$

Also by Euler's theorem, in year t :

$$Y_t = W_t + S_t \dots\dots\dots (3)$$

and, by definition:

$$y_t = w_t + s_t \dots\dots\dots (4)$$

ie:

$$Y_t / P_{yt} = W_t / P_{wt} + S_t / P_{st} \dots\dots\dots (5)$$

The Euler Consistency Test is based on the (unstated?)¹³ property of the national accounts that in any year t , in order for consistency to be maintained, the following condition must hold:

$$W_t / Y_t = w_t / y_t \dots\dots\dots (6)$$

ie:

$$S_t / Y_t = s_t / y_t \dots\dots\dots (7)$$

This follows from the fact that although in practice the years ending in 5 or 0 are conventionally nominated as base years, in principle, any year may be nominated as base year, and **that in any base year**, the two following relationships must be true by definition:

13 It is known that in practice, this condition is breached because of the changes in price and quantity (volume) relativities that inevitably occur in a dynamic economy. This characteristic of the real world forces compromises on national accounting statisticians - it usually entails the loss of additivity in the accounts. See UN, 1993, pp385-387.

$$W_o / Y_o = w_o / y_o \dots\dots\dots (8)$$

and:

$$S_o / Y_o = s_o / y_o \dots\dots\dots (9)$$

Under the conditions assumed above, it can be shown that (6) cannot be true unless $P_{yt} = P_{st}$, nor (7) unless $P_{yt} = P_{wt}$. In other words, for the ratios of the share of remuneration (or surplus) in total (net) product in current and constant prices to be equal, all three price indices must be equal. That this is so, is demonstrated as follows:¹⁴

$$\begin{aligned} s / y &= (S / P_s) / (Y / P_y) \\ &= (S / Y)(P_y / P_s) \end{aligned}$$

From expression (5):

$$Y = (P_y / P_w)W + (P_y / P_s)S$$

Therefore:

$$\begin{aligned} P_y / P_s &= (1 / S)[Y - (P_y / P_w)W] \\ &\neq 1 \text{ unless } P_y = P_w \end{aligned}$$

Therefore:

$$s / y \neq S / Y \text{ unless: } P_y = P_w \dots\dots\dots (10)$$

Note that if $P_y = P_w$, then expression (5) may be rewritten as:

$$Y / P_w = W / P_w + S / P_s$$

ie:

$$Y = W + S(P_w / P_s)$$

But:

$$Y = W + S$$

Therefore:

$$P_w / P_s = 1$$

ie:

$$P_w = P_s \dots\dots\dots (11)$$

In other words, the only condition under which the ratios of either surplus or remuneration to total (net) product in current and constant prices are equal is when the price indices are equal.

14 I am indebted to my colleague Julian Hofmeyr for pointing this out, for writing down the proposition which follows in algebraic terms, and for finding the gaps in the logic of the test in my first attempt to spell it out in full. Naturally, any errors that remain in this second writing of it are my responsibility.

Any deviation from this will cause the relative shares expressed in these two ways to differ.¹⁵ The apparently trivial findings of this exercise (trivial, in the sense that they follow logically from the assumptions used to set up the model) become meaningful once these assumptions are relaxed. The movement to the 'real' world - the world of actual economic processes and of attempts to measure them using the conventions of national accounting (conventions which cannot themselves avoid causing errors because of the sheer difficulty of measuring certain kinds of economic activity) - introduces forces which can and do cause the relations specified in (6) and (7) above to break down. It is worth recalling at this point that the argument and evidence offered in Stage 1 above suggests that dispersion of the relevant aggregate price indices and deflators should not be excessive.

It is a simple enough matter to go to the national accounts and show that in different years, ratios of remuneration (or surplus) to total (net) product in current prices differ. Over time, the dynamics underlying economic growth (or stagnation) ensure that this occurs frequently - struggles over the distribution of the total social product are an important cause of these changes. Suppose for a moment that a redistribution between two base years is revealed by the current price data. If we accept the (implicit) national accounting convention that current-price values in base years represent the 'real' values in that year, then at the change-over point from one set of base year price and quantity relativities to the next, it will become obvious that the ratio of remuneration (or surplus) to total (net) product in real terms has changed as well. The change in real terms will be visible **only** at this point - as will be shown below, the ratio of remuneration (or surplus) to total (net) product in real terms cannot be calculated except in base years. Now, if the ratio of remuneration to total net product (GDP), W_o / Y_o in year o does not equal the ratio W_t / Y_t in the year t , where the latter year is the succeeding base year, then the ratios w_o / y_o and w_t / y_t cannot be equal. Obviously, the same conclusion must hold if surplus is considered rather than remuneration. What the shares version of the ECT does (allowing itself some licence in the choice of base years) is to estimate and compare W_o / Y_o ; W_t / Y_t , and w_t / y_t . The expectation is that W_o / Y_o and W_t / Y_t will differ, but that if w_t / y_t has been correctly estimated, then W_t / Y_t and w_t / y_t will be approximately equal.

If it were possible to estimate theoretically-valid real-world price indices corresponding to P_y and P_w , (or P_s) one could track the changes in the 'real' shares of net output going to the two factors of production, capital and labour, over time. Economists are unable to specify exactly what form these deflators should take, and national accounting statisticians are consequently

15 Under the conditions assumed above for this economy, a discrepancy between the estimates of the relative shares in constant (real) and current price terms, or between the relevant deflators can only occur if an error is made in the preparation of the 'national accounts'. Recall here that because any year is in principle capable of being nominated a base year, the identity between price indices must hold in every year.

unable to produce them. It is this which accounts for the fact that real shares can only be estimated with confidence in base years. Some of the reasons why the relevant deflators are not available will be discussed shortly. The fact that they are not has created the space in which the ECT can operate. If both real and current price estimates of the ratio of remuneration (or surplus) to total (net) product could be made, any discontinuity at the change-over in base years would be detected immediately, thus prompting national accounting statisticians to look more carefully at their workmanship.

Progress out of the present impasse requires that valid proxies for P_y , and P_w (or P_s) be found, a search to which we now direct our attention.

A digression on deflators

Conceptually, the national accounting magnitude equivalent to Y is the current price measure of GDP at factor cost (factor incomes) (Y), ie, GDP(I) - GDP estimated by the income method.¹⁶ Theory cannot provide a simple answer to the problems of selection of appropriate deflators to apply to the various current price estimates, and so neither y_t nor its components w_t (and s_t) - the terms in the right-hand side of expression (4) - can be estimated rigorously.¹⁷ Their current price counterparts, Y_t , W_t , and S_t are freely available in some countries. The reasons why estimates are not made of the 'real' magnitudes differ. When the closed economy assumption is dropped, the likelihood arises that the S component of Y will be partially non-domestically generated. Also, as noted above, the falling away of the closed-economy assumption alters the real value of consumption possibilities of a given W .

Notwithstanding the difficulties involved, one may see, for example, that in the standard form of the national accounts used by the British, results are presented for the three forms in which GDP may be estimated - GDP(E); GDP(I), and GDP(P). Current price estimates using the different measurement approaches (expenditure, income and output) may be seen to differ. So too do the constant price estimates. Because of the differences in the constant price estimates, another measure - GDP(A), the average of these three, is commonly used as an index of economic performance.¹⁸ Of the three sets of constant price GDP estimates produced by the

16 The current price magnitudes GDP(P) (GDP estimated using the production approach) and GDP(I) are identical - ie, the production and income approaches to the estimation of GDP both measure value added at factor cost (Mohr *et al*, 1991, p39). See the discussion in Chapter 2-7 on the meaning of the concept of value added, and in particular on the relationship between the concepts of factor costs and factor income.

17 A more extensive discussion of the problems involved in preparing (rigorous) estimates of GDP(I) is given Chapter 2-7 under the heading 'A digression on real product vs real income'.

18 In the early days of national accounting, these differences were sometimes quite substantial. For example, with 1975 set equal to 100, the 1960 estimate of GDP(E) was 67,8 whereas GDP(P) was 71,8. The differences appear to diminish after about 1975. See *Blue Book, 1982 Edition*, Table 1.11.

British, only GDP(E) and GDP(P) come close to being rigorously estimated. The constant price GDP(I) series is cobbled together using a technique described in this longish extract from the early guide to the British national accounts:

"Direct estimates of gross domestic product at constant prices cannot be made from income data. Although wages and salaries may be deflated by an index of wage and salary rates, this provides a measure of only part of net output which does not take full account of changes in labour productivity. The only satisfactory revaluation of profit incomes is obtained by taking the difference between deflated gross output and deflated inputs including labour costs, which is not distinct from the output estimate. The various types of factor income might be deflated by changes in the prices of the goods and services on which the income is spent; but not all income is immediately spent, and until income is spent it cannot be identified with actual goods and services. However, since by definition total factor income equals total expenditure on the gross domestic product at current prices, the price index derived by dividing the expenditure-based estimate at current prices by the corresponding estimate at constant prices can be treated as a currently weighted price index applicable to total factor income. The estimate of gross domestic product at constant prices obtained in this way differs from the expenditure estimate only by the deflated value of the residual error (the difference between the income and expenditure estimates of gross domestic product at current prices). It provides no information on changes in the deflated values of separate components of factor income, but it does provide an alternative estimate of gross domestic product at constant prices which may be compared with the results of the direct measures of expenditure and output." (Maurice, 1968, p43)

From this passage it is clear that apart from the residual error, which can be quite substantial, the GDP(E) deflator can serve as a deflator for GDP(I).¹⁹ Digging a little deeper, one finds repeated in a later guide to the British national accounts a statement about the absence of a suitable deflator for profits. The guide notes that:

"For this reason no regular estimates are published, for example, of changes in company profits at constant prices..."

It does, however, continue with the statement that:

"...users may sometimes use a selected price index such as the implied GDP deflator to gain a broad impression of the way in which company profits have moved in 'real' terms." (Copeman, 1981, p3-5)

What begins to emerge, therefore, is evidence of a pragmatic or permissive climate in which the use of rough and ready measures for particular, limited purposes is treated as acceptable. The risk of error involved would not appear to be great - referring back to the findings reported in Table 2-2.1, it will be recalled that the major market-related indices and deflators grew at roughly similar rates. Any of these deflators, it would seem, may be used to deflate S_t with roughly similar outcomes. A sensitivity test performed on the results to measure the impact of using one rather than any of the other deflators available would soon settle the matter of whether or not they are good enough substitutes.

19 One would not expect the GDP(E) deflator to differ too markedly from the CPI. In the South African case, however, the rapid increases in the price of gold in the late 70s and the decline in the early 80s both distort the expenditures deflator. See Meth (1991b).

If the question of an appropriate deflator for W_t can be settled, then the deflated value of W_t (w_t) so derived may be added to that of the deflated value of S_t (s_t) to provide an alternative means of estimating y_t . There should be little difference between the values of y_t obtained in this way and official estimates of GDP(I), where such estimates are made. With the discussion of the 'consumption' and 'product' wage conducted in Chapter 1-2 in mind, and mindful, as well, of the fact that it is necessary to relax the closed-economy assumptions in order to perform the ECT on 'live' data, the problem of selecting an appropriate deflator for W_t will be resolved by using the CPI, the index conventionally employed to estimate what is referred to (somewhat misleadingly) as the 'real' wage (the value of remuneration in constant prices).

In the South African case, no estimates of GDP(I) are made, thus adding a further complication to the exercise. This problem, and certain others which arise in connection with the application of the test will now be considered in the course of performing the ECT on the mining and manufacturing sector data.

The 'Shares' version of the ECT - (i) mining

It may be useful at this point to recall that the ECT was developed to test the validity of constant price output estimates. In a closed economy, one would expect the divergences between GDP(P) and GDP(I) to be small, possibly negligibly so. Once external trade is admitted into the calculus, changes in the terms of trade can prise the two apart. This is discussed at length in Appendix 2-1, so it need not be pursued here.²⁰ Suffice it to say that the accepted official processes of adjustment of the national accounts to accommodate changes in the terms of trade pay no heed to the valuation problems which can arise in respect of GDP(P) and GDP(I).

In this first application of the ECT, the test as executed, records the difference between W_o / Y_o and W_t / Y_t (remuneration as a proportion of GDP(P) in current prices in periods o and t respectively), and then considers the respective values of W_t / Y_t and w_t / y_t ('real' remuneration as a proportion of 'real' GDP(P)). This is not ideal - we should be working with GDP(I) - but if the differences between W_t / Y_t and w_t / y_t are large enough, they may be taken as indications of the possibility of error. The question then becomes - how large is large

20 When the ratio of export to import prices undergoes change, economic welfare among the trading partners is affected. Our concern here is with the impact on the domestic country. A number of adjustments to compensate for this have been devised. Appendix 2-1 examines these measures and shows that under certain conditions, none of them is adequate to cope with the strains imposed by sudden and rapid price changes in important export commodities like gold, in the South African case, or oil, in the case of those nations lucky enough to be endowed with it.

enough? In the mining sector, 'large' may have to be very large, precisely because of the terms of trade effect. Mohr *et al* point out that the relationship between the CPI and the implicit GDP deflator, the core of the ECT, becomes unstable when substantial terms of trade effects occur (1991, pp42 and 44).²¹ Interpreting the test results in the case of the mining sector becomes problematic, because the terms of trade adjustments to the national accounts may, in fact, compensate for apparently large discrepancies between GDP(P) and GDP(I) (if the latter could be estimated).

Leaving aside these difficulties (and they do not exhaust the list of problems to be faced), let us see, if for nothing other than curiosity's sake, how the test behaves. In Table 2-2.4 the results of applying it to the 'live' data for the mining sector from *South African Statistics 1990* for the period 1972-80 are presented. According to the 'shares' version of the ECT, about 38 per cent of net output, measured in current price terms, went to the factor of production labour, in the form of remuneration in the year 1972 (ie, $W_0 / Y_0 = 37,9$ per cent). The gold price rose rapidly, and by 1980, the current price share of remuneration in total value added (W_t / Y_t) had fallen to about 23 per cent. Deflating 1980 remuneration to 1972 constant prices gives a value for w_t of R611,8 million. Expressing this as a proportion of y_t (also in 1972 prices) suggests that w_t / y_t rose from its 1972 value of 37,9 per cent to nearly 43 per cent.²²

These results contradict the proposition made above that since any year could, in principle, be nominated a base year, the proportional division of output between the factors of production must be the same when expressed in both current and constant prices. They appear also to be an offence against common sense. In addition, they give an indication of the burden thrust upon the terms of trade adjustment mechanism - somehow, that device must convert the (artificial) decline in the apparent share going to capital (62,1 per cent in 1972 and 57,1 per cent in 1980) into the windfall that capital really enjoyed, ie, 77,4 per cent of a substantially larger amount of 'value added'.²³

In interpreting these results, caution needs to be exercised, and that on a number of grounds. Foremost of these is the fact that GDP(P) has been elided gently into GDP(I) - an unacceptable step. At the time the Fosatu Challenge was written, I was unaware of the existence of the

21 This echoes the criticism referred to in Chapter 1-4 (p101) above, made on precisely the same grounds by Lerdau in 1954 of Maddison's use of the CPI and PPI to deflate nominal GNP.

22 Obviously, the shares going to capital emerge as residuals from the calculations above. They are, however, of no immediate interest.

23 If the relationship spelled out in Stage 1 above between different deflators had held true, ie, the various deflators had all grown at roughly the same rate, then using the deflators in Table 2-2.4, one can estimate that the value of Y_t in 1980 corresponding to a Y_0 of R1 513 million in 1972 would have been about R6 900 million.

compensating factor applied to national accounts (income) estimates in the form of an adjustment for changes in the terms of trade. Given the importance of the terms of trade effect in the case of gold, the question of the applicability of the ECT to the mining results will not be pressed here. Thus, although the difference looks as though it may be significant, this will not be insisted upon. Taking advantage of the results of the analysis that follows, it is, however, worthwhile pointing out that the uncovering of the workings of the Perverse Deflator Effect (PDE) over the same period suggests that the application of the ECT to the mining results is not wholly inappropriate.

Table 2-2.4 The Euler Consistency Test (Shares version) on mining sector results (1972 and 1980)

For the year 1972		
Y_o	R1 513 m	(p21.9)
W_o	R573 m	(p21.11)
y_o (1985 prices)	R18 359 m	(p21.6)
P_{wo} (1985 prices)	21,2	(p8.20)
For the year 1980		
Y_t	R12 742 m	(p21.9)
W_t	R2 886 m	(p21.11)
y_t (1985 prices)	R17 321 m	(p21.6)
P_{wt} (1985 prices)	100,0	(p8.20)
Convert y_t to 1972 prices	R1 427,5 m	(17321x1513/18359)
Convert P_{wo} to 1972 prices	100	
Convert P_{wt} to 1972 prices	471,7	(100/21,2x100)
Estimate w_t	R611,8 m	(2886/471,7x100)
W_o / Y_o (%) (= w_o / y_o)	37,9	(573/1513x100)
W_t / Y_t (%)	22,6	(2886/12742x100)
w_t / y_t (%)	42,9	(611,8/1427,5x100)

Source: Basic data are drawn from *South African Statistics 1990*. Page numbers are given in the table.

There is a certain sleight of hand in the way the test has been applied above. All of the argument so far has been cast in terms of economy-wide relations, yet the first application of the test has been to a sub-sector of the economy. Here, it must be conceded that it is not known whether or not it is valid to apply the test to separate sectors of the economy (major divisions), or worse still, to single industries, albeit large ones like gold mining, in isolation from one another. Given the density of the interconnections between the different sectors of the economy it might be argued that it is necessary to devise a theoretical approach to the

problem based, say, on general equilibrium theory. If such a call were justified, then it could render the results of the about-to-be-attempted application of the ECT to the manufacturing sector invalid.

The 'Shares' version of the ECT - (iii) manufacturing

Such a drastic position as that outlined immediately above smacks of overkill - the results presented in Table 2-2.1 suggest that if the modest aims of the ECT are kept in mind the problem (if it exists) may simply be ignored. The results of an ECT would, in any event, be of little consequence if they could not be corroborated by further research. It may be recalled that the Implicit GDP Deflator (IGDPD) in manufacturing in the UK grew at almost the same rate as the other market-disciplined aggregate indices - a fact which suggests that looking at the various sectors of the economy in isolation is not necessarily inappropriate. It will be recalled as well that in the South African case, the IGDPD grew faster than the other market-disciplined aggregate indices - precisely the result one would expect if one is arguing that output levels have been understated.

There may have been grounds for ignoring or dismissing as of no consequence the discrepancy revealed by the application of the ECT to the mining data, were it not for the fact that corroborating evidence of error is available from a separate source - this is the subject of Appendix 2-1. Similar considerations apply to the manufacturing sector - the ECT, in what some might regard as the primitive form presented here, has successfully detected errors which the CSS has been obliged to acknowledge. Even if this acknowledgement had not been forthcoming, the attempts in subsequent chapters of this study to replicate the CSS figures reveal quite conclusively that the CSS figures were in error. Given this, and given the fact that manufacturing sector output, although subject to terms of trade effects, is unlikely to be as seriously affected by them as is mining, there can be little serious objection to the application of the ECT to the manufacturing sector results. That being so, the same technique can be applied with greater confidence.

In Table 2-2.5 the test may be seen in action on the results for the sector for the period 1979-89. W_0 and Y_0 are, of course, obtainable directly from the national accounts. Once again, the deflator applied to $W_1 (P_{wt})$ is the CPI. GDP(P) is also pressed into service again (as y_1) in place of GDP(I), but without the impediment of being at the mercy of changes in the terms of trade.

Table 2-2.5 The Euler Test (Shares version) on manufacturing sector results (1979 and 1989)

For the 'base' year 1979: ²⁴			
Y_o	R9 767 m	(p21.9)	
W_o	R6 048 m	(p21.11)	
y_o (1985 prices)	R25 302 m	(p21.6)	
P_{wo} (1985 prices)	45,6	(p8.20)	42,8
For the year 1989:			
Y_t	R50 896 m	(p21.9)	
W_t	R28 024 m	(p21.11)	
y_t (1985 prices)	R28 742 m	(p21.6)	
P_{wt} (1985 prices)	178,2	(p8.21)	178,4
Convert y_t to 1979 prices	R11 095 m	(9767x28742)/25302	
Convert P_{wo} to 1979 prices	100		
Convert P_{wt} to 1979 prices	390,8	(178,2/45,6x100)	416,8
Estimate w_t (W_t / P_w)	R7 171 m	(28024/390,8x100)	R6 723 m
W_o / Y_o (%) (= w_o / y_o)	61,9	(6048/9767x100)	
W_t / Y_t (%)	55,1	(28024/50896x100)	
w_t / y_t (%)	64,6	(7171/11095x100)	60,6

Source: Basic data are drawn from *South African Statistics 1990*. Page numbers are given in the table.

According to the 'shares' version of the ECT, about 62 per cent of net output, measured in current price terms, went to the factor of production labour, in the form of remuneration in the year 1979 ($W_o / Y_o = 61,9$ per cent). By 1989, this had fallen to about 55 per cent in current price terms ($W_t / Y_t = 55,1$ per cent). The constant price estimates for the same year suggest, however, that labour received about 65 per cent ($w_t / w_t = 64,6$). Once again, the result contradicts the proposition made above that since any year could, in principle, serve as base year, the proportional division of output between the factors of production must be the same when expressed in both current and constant prices.

Casting these results in the forms of the positive or negative residuals suggested by the original debate around Euler's Theorem, we could say that the claims on total product suggested by the results above imply a negative residual of nearly ten per cent. Labour's 'true' (current price) share in 1989 was 55,1 per cent, but the share implied by the deflation process carried out above was 64,6 per cent.

24 It is important to note that calendar year data (ie, from 1 January to 31 December of any particular year) are used here. Manufacturing census data are for 'financial' years - (arbitrarily) from 1 July in one year until 30 June in the next. Deflating financial year output estimates by calendar year PPIs, an easy error for the inexperienced to make, is obviously not permitted. It was through making an error of this type that I finally stumbled onto one of the reasons why the official output estimates were incorrect.

Sensitivity analysis was mooted, in the digression above on deflators, as one means of checking that the ECT was not producing misleading results. Looking back at the growth rates of the various indices considered in Table 2-2.1 it may be seen that of the aggregate market-disciplined (market-related) domestic deflators, that for Consumer's Expenditure (very roughly, the CPI) grew slowest of all (12,7 per cent *per annum*) and that for Total Domestic Expenditure most rapidly (13,3 per cent *per annum*). Since a faster-growing deflator will result in a smaller value of w_t and hence a smaller value for the ratio w_t / y_t , one should use the latter as deflator in the performance of the sensitivity analysis. The final column of Table 2-2.5 contains the set of results which follow from the use of the Total Domestic Expenditure deflator instead of that for Consumer's Expenditure. Thus P_{w_0} in constant 1985 prices becomes 42,8, and P_{w_t} increases marginally to 178,2. In 1979 prices, P_{w_t} becomes 416,8. This reduces the value of w_t to R6 723 million. This means that w_t / y_t is only 60,6 per cent instead of the 64,6 per cent obtained using the Consumer's Expenditure deflator. The question was posed above - what divergence between W_t / Y_t and w_t / y_t is unacceptably large? To my way of thinking, a divergence as large as five per cent requires explanation.

Stage 3 The 'Deflators' version of the ECT

The second version of the ECT is called the 'deflators' version. This version of the ECT is performed by estimating the differences between the CPI (the deflator used to estimate the real value of remuneration w_t in the 'shares' version of the ECT) and the implicit GDP deflator (IGDPD) for the specific sector under consideration over a reasonably lengthy period of time.²⁵ Significant deviations of one from the other will be taken as evidence of error. Drawing once more on the findings presented in Stage 1, and on the argument in Stage 2, we advance the hypothesis that the relevant deflators and indices will track each other closely if output levels are correctly estimated. Where this does not happen, 'errors' may be argued to have resulted either from an event in the 'real' economy with which standard national accounting procedures are unable to deal, or from an error in the national accounting estimates, or from some combination of the two. Errors disclosed here and in succeeding chapters are probably of this latter type.

The two versions of the ECT yield exactly the same results. That they should do so is, at first

25 I am indebted to Terence Moll for suggesting that this test be carried out using time series data, instead of merely taking values for two end-years, as I did in the Fosatu Challenge.

sight, not intuitively obvious.²⁶ The 'deflators' version of the test expresses the difference between the CPI and the IGDPD as a percentage of the CPI ie, $(CPI - Y / y)/CPI$. If one wished to express the errors detected by the 'shares' version of the test in percentage terms, then one would take the difference between the share of output going to labour in current and constant prices and divide this by labour's share of output in current prices. It is a simple matter to show that the following identity holds:

$$(CPI - Y / y)/CPI \equiv (W / Y - w / y)/(W / Y) \dots\dots\dots(12)$$

In this expression, the left-hand side represents the 'deflators', and right-hand side the 'shares' method of performing the ECT. The identity may be checked for validity by transforming the right hand side:

$$\begin{aligned} (W / Y - w / y)/(W / Y) &= (W / Y - w / y)(Y / W) \\ &= (1 - (w / W)(Y / y)) \dots\dots\dots(13) \end{aligned}$$

But $w = W / CPI$

Therefore (13) equals:

$$\begin{aligned} &(1 - (Y / y)(1 / CPI)) \\ &= (CPI - (Y / y)) / CPI \end{aligned}$$

The 'deflators' version is thus formally identical to the 'shares' version. The former is, however, to be preferred on the grounds that fewer data are required to conduct it. In the expression $CPI - Y / y)/CPI$, the fraction Y / y yields the Implicit GDP Deflator (IGDPD). Measured values for the variables Y and y , rather obviously, are the same as in the 'shares' version of the ECT, ie, they are current and constant price estimates of GDP(P) rather than GDP(I), for which there are no official estimates. What remains to be considered is the question of whether or not it is reasonable to expect the differences between the CPI and the

26 One reason why it is not intuitively obvious, and immediately so, that the two methods should produce the same result must be the expectation that in the real world, P_y , P_w and P_s will differ, one from the other. It is trivially obvious however, that if, in the spirit of the somewhat unrealistic assumptions used to develop the ECT, the IGDPD for the sector as a whole P_y (the left-hand side of expression (12)) were set equal to the CPI, and the deflator for W , P_w is also made equal to the CPI, then in terms of the earlier argument, P_s must also equal the CPI.

IGDPD to be minimal. In response, one can only claim once again that the results presented in Table 2-2.1 do little to discourage the expectation that the differences should indeed be minimal if output levels have been correctly estimated.

The 'Deflators' version of the ECT in operation

The investigation into the manufacturing sector output estimates conducted in this study was sparked, it was noted above in Chapter 1-2, by the publication of a revised set of employment figures. The manufacturing census results from which these were drawn suggested that the regular monthly surveys of employment in manufacturing had under-estimated employment by about 8 per cent in 1985 (Meth, 1992, p11). Although it was not inevitable that the output estimates would be incorrect as well, the revised employment estimates did at least hint at the possibility of error in the output estimates.

Under the circumstances, the application of the ECT to the existing figures was the obvious and logical thing to do. The results below are reproduced from 'Data Problems...' (Meth, 1992). As recorded in that work, the ECT apparently did little at the time to persuade the authorities that there were indeed errors in the estimates - the acknowledgement of error came very much later, and with no reference to this test.

Table 2-2.6 below shows the ECT in operation on three separate data sets. In addition to this, it would seem sensible to look as well at the performance of a related indicator, the Production Price Index (PPI). One could argue that, in principle, the PPI should be roughly equivalent in value to the sectoral GDP deflator. Although one would expect movements in the CPI to be related to movements in the IGDPD, the fact that the connection between these indices is mediated by a greater number of exchanges than is the case with the IGDPD and the PPI, might incline one to predict greater variability of the CPI from the IGDPD than of the PPI from the IGDPD. As may be seen below, this turns out not to be the case. The values in the four columns of this table show the percentage by which the IGDPD exceeds the CPI (negative values) or *vice-versa* (positive values). Obviously, when the IGDPD is greater than the CPI, the deflated values of output in manufacturing obtained using the CPI will be higher. Similarly, when the CPI exceeds the IGDPD, the use of the CPI as deflator will cause output estimates to be lower. Strongly negative estimates of the deviation therefore point to the possibility of significant under-estimates of the level of output.

Table 2-2.6 Estimated Deviations (%) of IGDPD from CPI and PPI in Manufacturing (South Africa: 1970 and 1980 prices) and Deviation of IGDPD from CPI (United Kingdom: 1975 prices)

Year	SA 1970	UK 1975	SA 1980	SA PPI
1960	-3,0	-9,3	2,6	-7,0
1961	-5,1	-8,8	0,7	-9,6
1962	-3,5	-5,6	2,2	-9,3
1963	-2,7	-4,8	2,9	-8,4
1964	2,0	-2,6	7,4	-3,8
1965	1,4	-2,2	-8,5	-22,6
1966	3,8	-0,6	-4,9	-19,0
1967	5,3	0,6	-1,9	-16,2
1968	2,3	5,6	-3,2	-17,9
1969	1,2	5,9	-4,1	-18,9
1970	0,0	4,2	-3,3	-19,9
1971	1,2	2,9	2,4	-14,0
1972	0,7	-1,3	3,3	-13,4
1973	-5,0	0,8	-1,2	-15,6
1974	-6,1	5,4	-1,8	-9,7
1975	-12,2	0,0	-5,8	-10,9
1976	-20,6	3,2	-6,0	-8,9
1977	-22,4	-0,2	-0,4	-1,4
1978	-14,6	-5,2	4,4	2,9
1979	-16,8	-4,1	5,3	4,1
1980	-	-4,6	0,0	0,0
1981	-	-0,8	-7,2	-8,8
1982	-	0,3	-6,3	-8,5
1983	-	2,0	-15,3	-19,6
1984	-	3,8	-14,4	-22,5
1985	-	1,6	-13,8	-22,8
1986	-	-2,2	-11,3	-19,8
1987	-	-	-8,9	-18,6
	1	2	3	4

Sources:

1. South African data:

Output in current prices: 1960-85, SAS 1986, p21.9; 1981-87, SAS 1988, p21.9.

Output in constant 1970 prices: 1960-79, SAS 1980, p21.6.

Output in constant 1980 prices: 1960-83, SAS 1986, p21.6; 1984-87, SNR P0441, 17 May 1988.

CPI: 1960-85, SALS 1987, Table 4.3.1; 1986-87, converted values from SAS 1988, p8.21.

PPI: 1960-70, SAS 1976, p8.6. There is no estimate for 1960, so it has been assumed to be equal to the value of the Wholesale Price Index on p8.4 of the same publication. 1970-85, SAS 1986, p8.6; 1986-87, SAS 1988, p8.6. SAS here refers to *South African Statistics*, and SALS to *South African Labour Statistics*.

2. British data:

Output in current prices: *Yearbook of National Accounts Statistics 1970. Volume 1, Individual Country Data*, New York: United Nations, 1972, p1260. *Yearbook of National Accounts Statistics 1978. Volume 1, Individual Country Data*, New York: United Nations, 1978, p1330. *National Accounts Statistics: Main Aggregates and Detailed Tables, 1986. Part II*. New York: United Nations, 1989, p1508.

Index of output in constant prices: *International Financial Statistics, Supplement on Output Statistics, Supplement Series No. 8 1984*, Washington: IMF, pp158 and 159; and *Blue Book 1990*, Table 2.4.

CPI: *International Financial Statistics Yearbook 1990*, Washington: IMF, pp726-727.

The Column 1 results show what would have been uncovered if the ECT had been applied to the 1970-based output estimates for South Africa. The increasing inaccuracy suggested by the growing deviation of the IGDPI from the CPI from about 1973 onwards was confirmed when the rebased and revised 1975-based estimates were published. It is necessary to recall that a deviation revealed by the ECT does not explain anything - it merely points to the possibility of error. In the case of the post-1972 estimates, it is known that the error lay in the estimates of the Physical Volume of Manufacturing Production (PVMP).²⁷ Since the error had already set in as early as 1968 or 1969, the test obviously does not serve as a perfect 'early warning system' - it is, however, a very considerable advance on the apparently total absence of any other indicator of the fact that the surveys were being affected by bias.

In Column 2 of the table, the 1975-constant price data for the United Kingdom are subjected to the ECT.²⁸ If it is assumed, not unreasonably, it would seem, that despite reports of deterioration in the quality of UK national accounting data, that these data are nevertheless superior to the South African data, then a satisfactory performance by the UK CPI as a deflator for use on manufacturing sector output estimates would provide some support for the proposal that the process be applied to the South African data. The UK results are quite encouraging. There are problems with some of the estimates in the early years, but these probably result, as might the similar problems in the South African figures, from difficulties involved in preparing national accounts before the revised United Nations *System of National Accounts* (SNA) appeared in 1968.²⁹ For this reason, it seems appropriate to consider only the

27 Two methods of estimating output levels exist - one of them attempts to measure volumes (or numbers) of individual commodities produced. In a given period, an integrated petro-chemical plant may, for example, produce 300 tonnes of multi-grade lubricating oil (packaged in a changing mix of containers ranging in capacity from 500 ml to 200 l), 600 tonnes of 93 octane petroleum, 400 tonnes of 97, and 200 tonnes of bunker oil; 1000 tonnes of ethylene, some of which is sold as a feedstock and some of which, after further processing, goes into the manufacture, on the site, of 100 000 ball-pen caps, 600 000 milk sachets and 450 000 toothpaste tubes. Using one of a number of techniques, these separate outputs, the absolute and relative quantities of which may change significantly over time, can be aggregated into an index of output. The result is a Physical Volume of Production (PVP) index - and, in the case of the manufacturing sector, the Physical Volume of Manufacturing Production (PVMP) index. Such indices are described as 'direct' measures.

The other method generates 'indirect' indices by deflating the value of output (roughly, total revenue, or gross output - ie, price per unit of output multiplied by number of units sold), by a suitable price index. Because of the difficulties of obtaining the information necessary to produce direct indices, extensive use is made of indirect indices. As long as the ratio of net output (value added) to gross output, or, what amounts to the same thing, of intermediate inputs to gross output does not vary too much, indices of this type are believed to be reasonably reliable.

Benchmarking, a process first referred to in Footnote 17 of Chapter 1-1, uses indirect volume indices obtained by deflating manufacturing census values of gross output by Production Price Indices (PPIs), as reference points through which the trend lines plotting the growth of the Physical Volume of Manufacturing Production (PVMP) are forced. Benchmarking is discussed at greater length in Chapter 2-3.

28 Data for the UK were selected on the basis of availability. It would have been interesting to compare differences in the relative rates of change of the implicit deflators, but this was not possible.

29 For the UK, the United Nations *Yearbook of National Accounts Statistics 1970* gives "Former SNA" and "Present SNA" estimates for the years 1968 and 1969, which differ very considerably, one from the other. See *Volume 1: Individual country data*.

post-1968 estimates. When this is done, the mean absolute deviation of the CPI from the IGDPD is a mere 2,7 per cent. In 18 observations, the deviation exceeds 5 per cent on only three occasions, and 4 per cent on a further three.

In Column 3 of the table, the CPI/IGDPD comparison is performed for the 1980-constant price South African data. Ideally, one would have wanted to apply the test to the 1985-based estimates, but since these do not extend back beyond 1981, and since it is known that the CSS has simply used the 1980-based PVMP in its 1985-based figures, the use of the 1980-based estimates is justified.³⁰ The problems that arise as a result of the unexplained change in the 1964/65 estimates may be ignored because it is known that there is something wrong with the estimates for these years. For the 20 observations between 1960 and 1979,³¹ there are only two occasions on which the deviation exceeds 6 per cent (1964 and 1965).³² The mean absolute deviation for the 20 observations is 3,6 per cent. A series such as this is probably not accurate enough to use in a medium-term analysis of, say, output or productivity growth, but it is certainly accurate enough to give an indication of growth trends in the long term.

It may be unwise to attach too great a significance to any estimate made in the days when national accounting in South Africa was still in its infancy (not that it has improved much in its dotage!), but if one estimates the mean absolute deviation of the CPI from the IGDPD in the period 1969-79, one finds that it drops only marginally to about 3,5 per cent. Matters are obviously different in the period after 1981, and most especially so between 1983-87, precisely the period when the errors in the estimates of the value of the PVMP occurred. The observed deviations tell the same story as those in Column 1 - there is an error somewhere that needs to be investigated! The only reasonable explanation for the very high errors recorded in those years is that the IGDPD is much too large - a state of affairs brought about by the fact that the corresponding PVMPs are too small.³³

30 When the CSS rebased the manufacturing sector output estimates from the year 1980 to the year 1985, they made no changes to the estimates of output for the years prior to 1985. The changes for the years 1986-89 were minor. See Table 8 in Meth, 1992, p32.

31 Obviously, there will be no deviation in a base year, because the current and constant price values are equal by definition.

32 For the 20 observations covering the period 1960-79, the absolute error exceeded four per cent on nine occasions - eight per cent once (1965: -8,5 per cent); seven per cent once (1964: 7,4 per cent); it was equal to six per cent in one year (1976: -6,0 per cent); it exceeded five per cent twice (1975: -5,8 per cent; 1979: 5,3 per cent), and four per cent on three occasions (1966: -4,9 per cent; 1969: -4,1 per cent; 1978: 4,4 per cent).

33 Aggregate output estimates in constant price terms for the manufacturing sector used to be constructed from the weighted sums of the PVMPs of all of the 3-, 4-, or 5-digit PVMPs of the industries that made up the sector. The most common technique for weighting assigned a weight to each industry equal to its proportional contribution to total net output in the base year. A sectoral IGDPD for manufacturing is estimated by dividing the current price value of GDP by its constant price counterpart. Rather obviously, if observed PVMPs under-estimate the actual level of production, the IGDPDs will be too high, ie, larger than the 'true' deflators of the corresponding indirect measures should be. PVMPs are no longer estimated for all industries in manufacturing. Instead, a Monthly Sales Inquiry is used to estimate indirect output indices. This is discussed in Chapter 2-4.

For the rest, there appears to be a tendency (more systematic in the later than the earlier half of the period) for the implicit GDP deflator to be larger than the CPI during downturns, and *vice-versa* during upturns, though possibly not by as much. If this were the case it would imply that estimates of GDP(I) obtained by using the CPI as deflator would overstate the GDP(P) during downturns, and understate it during upturns. The long and short of the matter is that if, based on historical experience, one were to have used the CPI as deflator of net output in manufacturing in the period 1960-79, one could have said that there was a 90 per cent probability that the error involved in so doing would have been 6 per cent or less in any estimate, and a 75 per cent probability that the error would have been less than 5 per cent.³⁴

A comparison between the Column 1 and the Column 3 figures in Table 2-2.6 is instructive. The significant results are those for the period from 1975 onwards. Although the 1980-based figures in Column 3 do exceed somewhat the mean absolute deviation of 3,6 per cent for the 20 observations, this excess pales into insignificance by the side of the deviations shown in Column 1 for the years 1975-79. In the case of the 1970-based figures, it is known with certainty that as far as the constant price estimates are concerned, the surveys devised to measure the PVMPs from which the estimates were computed developed a bias that significantly understated actual production levels. This shows quite conclusively that this bias cannot escape the Euler Consistency Test, even if it takes a while to pick up the error.

Similar mistakes in the output estimates in constant prices can now be said, with some confidence, to have been made at least twice. An interesting feature of these errors is that in the case of the 1970-based figures, output in current prices was over-estimated, whilst in the case of the 1980-based (and 1985-based) figures, it was under-estimated.³⁵ These divergences open up another fascinating question about the national accounts, namely, what are the mechanisms by which revisions are made? Unfortunately, this topic lies well outside of the scope of the present inquiry, so it must, with some reluctance, be ignored.

34 It was noted at this point in the original published version of the test in operation that:
 "The conclusions flowing from this test are corroborated by other circumstantial evidence - especially the deflated values of gross output estimated in Table 10, and to a lesser extent, the value of sales estimates in Table 9.

The relative ease with which employment, as opposed to output, can be measured has already been referred to. Given this, and given that the employment estimates are known to have been severely affected by bias, the possibility must exist that bias could creep into a survey designed to measure output with as much, if not more ease, than it could (and has) into one designed to measure employment levels, even if these surveys are conducted entirely independently. This further strengthens the case for the application of the ECT." (Meth, 1992, p53)

35 This may be confirmed by comparing the results in Columns 1 and 2 of Table 7 in Meth (1992).

In Column 4 of the table, the PPI³⁶ and IGDPD results are compared. The CPI and IGDPD comparisons yield pleasing results - the same exercise performed using the PPI does not. This is somewhat surprising in that it suggests that the PPI is not very closely related to the IGDPD. It is by no means obvious why the substantial differences revealed in Column 4 of the table should exist. The results do, however, display a rather distinctive pattern. If one could ignore the period 1965-73, when the difference leaps to around 20 per cent, one could argue that although the series shows a consistently higher absolute deviation in most years than does the CPI from the IGDPD, it also suddenly more than doubles in 1983, exactly as the latter does - corroborating thereby the claim that the IGDPD is incorrect. But the 1965-73 deviations are troublesome, and there is nothing in the published information that provides a clue as to where to begin looking for an explanation. This is thus the sort of question that can only be answered by the CSS.

With the exception of the last set of results (those using the PPI figures), the available evidence provides strong support for the use of the ECT.

A concluding note on official ignorance

In the Fosatu Challenge, the earliest version of the ECT was used in an attempt to show that mining output in the South African national accounts was, at times, incorrectly valued (Meth, 1983). Essentially, what the results of the test hinted at was the appearance in South Africa of a phenomenon that has been widely reported in the international literature, namely, that small open economies which are highly dependent for their export revenues on a single commodity such as gold or oil suffer extreme difficulties in compiling accounts when the price of that commodity fluctuates significantly. That version of the test deflated the nominal values of the wage bill and of gross operating surplus (the two components of total value added) by the CPI - arguing that the sum of these two items should equal (approximately) output as measured in constant price terms.

The SARB, and, following them, the NPI rejected this test, arguing that it displayed an ignorance of national accounting procedures (Swanepoel and van Dyk, 1983; NPI, 1983). That, of course, was true - my error was to suggest that the estimate of GDP(I) produced in this way could be used as a measure of GDP in general. The error of the SARB and NPI was a

36 The PPI is estimated for three distinct sets of commodities - 'Commodities for South African consumption', 'Output of Total South African industry groups - Total output', and 'Output of South African industries for South African consumption'. It seems most appropriate to use the second of these, because the first includes imports and the third excludes exports.

failure to point out to users that terms of trade changes can have a devastating effect on national accounting procedures - a fact which was to be more than amply demonstrated when the national accounts were rebased to the year 1980. As Moll observes, the use of 1980 as a base year was extremely unfortunate, a fact of which the SARB was aware (1992, p194). Accepting then that it is inappropriate to conflate 'real income' in the mining sector with 'real product' - a "curious procedure", as Moll (1992, p192) has pointed out - it is nevertheless legitimate to use the method suggested by the guide to the British national accounts to obtain a crude approximation to GDP(I).

The only difference between what the guide to the British national accounts sanctions and what was done in the Fosatu Challenge is that the deflation procedure suggested in the guide is presumably intended to be applied to the economy as a whole whereas the Fosatu Challenge applied it to one sector only. In the Fosatu paper the specific concern was with mining output estimates. After some considerable effort had been expended on an attempt to derive composite indices based on assumptions about the way in which the income accruing to the different factors of production might be spent, it was suggested that the resulting deflators were probably not very different from the Consumer Price Index (CPI) (Meth, 1983, p8). This suggestion and its application to the mining and construction sectors elicited the comments about the 'ignorance of national accounting procedures displayed in the Fosatu Challenge'. In its response, the NPI claimed that:

"According to this logic it will be equally acceptable to deflate the value of motor vehicle sales at current prices by the Consumer Price Index for footwear to obtain real motor vehicle sales." (NPI, 1983, p7)

Such a statement displays a disturbing ignorance of the disagreements that exist about the selection of appropriate deflators for the GDP(I) as a whole, and for its parts (Copeman, 1981, p3-5). It also displays an ignorance of the phenomenon reported on above in Stage 1 of the development of the ECT - namely the relatively close correspondence of aggregate market-related domestic deflators. This is understandable as far as the NPI is concerned, but less so in the case of the SARB. Not long after the interchange reported above took place, the premier journal in the field of national accounting, *The Review of Income and Wealth*, published an article that put forward a cogent case for the use of the CPI as a deflator for measuring the purchasing power of money in the economy as a whole (Lützel, 1985). After examining the strengths and weaknesses of a variety of deflators, Lützel states that:

"...the question arises which price index should be used as an indicator for the general development of the purchasing power of money for the purpose of inflation accounting. It is not possible to give a definite answer to that problem: it depends on the respective object of analysis. Quite a lot speaks in favour of the consumer price index." (1985, p220)

Both the NPI and the SARB seem to have been unaware, for example, that at the time of publication of the Fosatu Challenge, the CSS used the CPI as a GDP deflator for the output of the bantustans economies.³⁷ Criticism of the original ECT on the grounds that it made illegitimate use of the CPI has been shown above to have been misplaced - the test has proved its worth.

37 See for example CSS Report No. 09-17-01.

Chapter 2-3

Ignorance is bliss (of errors in the unbenchmarked estimates)

Introduction

Benchmarking a series can be expected to reveal errors. That certainly occurred when the unbenchmarked CSS industry (3-digit SIC or major-group) estimates were benchmarked to the results of the 1985 Manufacturing Census. This chapter considers first the extent of the errors revealed by this process. So large are the inaccuracies revealed to be, as to render invalid the claim by the CSS that the continued production of these indices was justified by the demand for them.

Conventionally, the information available on manufacturing sector output is of two types - the first a set of estimates of gross output and net output in current prices for each 'industry' (3-, 4-, or 5-digit SIC major groups or sub-groups) that goes to make up the sector. Summed, these yield total gross or net output for the sector. Production Price Indices (PPIs) are available for many, but usually not all of the industries. These are also aggregated up into a weighted price index for the sector as a whole. Deflating the estimates of gross output for any industry by the relevant price index yields an (indirect) estimate of the volume of output. Total output of the sector is equal to the weighted sum of these individual indirect output estimates. Alternatively, total gross output in current prices may be deflated by the aggregate PPI to yield an (approximate) indirect estimate of real output. The other type of output estimate is a direct measure of the volume of output in any industry. This is called the Physical Volume of Manufacturing Production (PVMP). It too, is aggregated up into a composite (weighted) index which measures total output.¹

It is argued that when an output estimate is aggregated up from a number of sub-estimates, one of the tests it should pass, if it is to be considered 'consistent', is that the sum of the individually deflated components of gross output should not differ significantly from the value of total gross output deflated by the aggregate PPI. Neither, obviously, should the resulting index of output deviate significantly from the Physical Volume of Manufacturing Production

¹ Recall here that Footnote 34 in the previous chapter discusses direct and indirect indices in somewhat greater detail, and that Footnote 33 describes one of the weighting techniques used. Weighting is discussed at greater length in Appendix 2-6 under the heading 'Stage 1 - Pseudo-benchmarking'.

(PVMP) where attempts are made to measure this. The second concern of this chapter is to examine this proposition. In my first attempt to perform a simple benchmarking operation on the output estimates I made an error, which the CSS used to deny the validity of this proposed consistency test. This rejection of what seemed an eminently sensible suggestion led to the examination (presented in the following chapters) of the conditions under which the proposition is true, by definition, and those conditions when in practice, small deviations are likely to be encountered. The CSS' response also led to a detailed consideration of the residual differences between the different approaches to the estimation of output volumes - especially those routinely used by the them. That is the substance of the remainder of the chapter.

Industry output estimates - thoroughly misleading statistics?

Where possible, it is useful to measure output directly - ostensibly, the PVMPs which the CSS used to estimate were measures of actual physical quantities of commodities produced. In reality though, PVMPs are a mixture of 'direct' and 'indirect' measures. Monthly surveys of production volumes were used to generate the PVMP series ('direct' indices), but for a variety of reasons, these became inaccurate or biased over time. The further one moved from the most recent benchmarking, the less reliable the PVMP became. For this reason, PVMPs had to be benchmarked every few years.² The benchmarks, 'indirect' volume indices obtained by deflating manufacturing census values of gross output by PPIs, can be conceived of as pegs on which the monthly series could be hung. A moment's reflection will make it clear that producing output indices in this way leaves scope for (at least) two different types of error. The first of these arises when gross output is deflated in such a way as to yield an incorrect indirect index. The second occurs under conditions where it not appropriate to use an indirect index obtained by deflating gross output.

Errors of both types have been made by the CSS in the course of the attempts to estimate output levels in manufacturing. As will be apparent from the outline of the structure of this part of the study given in Chapter 2-1, errors of the first type (gross output incorrectly deflated), are the central concern of this chapter. When I wrote the earlier monograph 'Data Problems...' (Meth, 1992), I was unaware of the second type of error - it was only the work on the first draft of 'More Problems...' which sharpened the focus on the question of the conditions under which it was inadvisable to use 'indirect' indices constructed from gross output estimates.

² Benchmarking is discussed in more detail in Appendix 2-6 under the heading 'The mechanics of benchmarking'.

Responding to claims in Meth (1991a) that output had been severely under-estimated, the authorities pointed to the fact that benchmarking had not yet been carried out. Benchmarking was finally completed early in 1992, and from the tone of the explanatory notes that accompanied the benchmarked set of figures published in Statistical News Release P3041.3 of 9 March 1992 it appears that the CSS deemed the errors to have been corrected. The official benchmarked figures differed substantially, however, from the results yielded by a rough-and-ready benchmarking exercise that were published in Meth, 1992, p74. The official benchmarked figures also failed the Euler Consistency Test.

A correspondence on this matter commenced with a letter from me to the CSS on 27 March 1992, a few days after the Statistical News Release P3041.3 of 9 March 1992 was received. It dragged on for months, seeming to run into an impasse in which neither side would yield. In the course of the interchanges, an error of mine, caused by a misunderstanding of the CSS description of the benchmarking process, was exposed. This lowers the most conservative estimates of the expected value of the 1985 output estimate from the value of 117-122 obtained by the method proposed in Meth, 1992, p74, to about 115 points (1979=100). Even after acknowledging this, however, the fact remains that the CSS revised the 1985 value from 104,7 to only 109,5, whereas the burden of the argument in this study is that it should have been much higher. In other words, there was still a dispute over whether the original error was a shade less than 5 percentage points, as the CSS claimed, or was well in excess of 10 percentage points, as my calculations suggested. This error, it must be noted, is of the first type described above, ie, the 'indirect' indices produced in the standard way from gross output estimates are incorrect.

Matters remained in this unresolved state until mid-November 1992, when discussions with various people at the CSS revealed that hairline cracks in the official ramparts had finally appeared. It seems that the argument that turned the debate was a set of tables I produced³ that showed the differences between the benchmarked and unbenchmarking output estimates for the various industries (major groups) that go to make up the manufacturing sector. Some of these differences (errors) are very large, but it appears no-one (with the possible exception of one or two junior officials at the CSS) was aware of them until I performed the rudimentary task of comparing the unbenchmarking and benchmarking estimates. The pattern of errors in the estimates of Physical Volumes of Manufacturing Production (PVMPs) in the industries that make up the manufacturing sector is reproduced in Table 2-3.1 below.

3 These were appended to a letter dated 18 August 1992 addressed to the Head of the CSS.

Table 2-3.1 Errors revealed by benchmarking 3-digit SIC PVMPs to the year 1985
(benchmarked minus unbenchmarked estimate*)

	Food	Beverages	Tobacco	Textiles	Clothing	Leather	Footwear	Wood	Furniture
1978	0,3	1,4	-0,4	2,4	3,0	0,2	0,0	5,2	-1,3
1979	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
1980	-2,6	11,0	2,9	-8,5	6,3	12,3	0,0	-0,9	13,9
1981	-4,6	18,3	6,0	-13,7	11,4	25,7	0,0	-2,3	33,1
1982	-6,1	20,0	7,9	-10,8	14,6	32,0	0,0	-5,7	47,9
1983	-1,8	25,9	13,2	2,9	17,6	35,3	0,0	-0,4	52,8
1984	-3,0	33,0	19,5	3,7	27,4	40,2	0,0	16,3	56,7
1985	-4,7	30,5	20,3	2,6	23,6	31,5	0,0	14,9	53,9
1985	-1,9	26,2	35,9	4,1	23,2	58,9	5,3	11,9	57,1
1986	-5,4	35,7	15,7	2,6	21,9	26,8	0,0	15,3	55,9
1987	-5,2	33,9	3,8	-3,0	23,0	26,8	0,0	14,9	51,7
1988	-7,1	35,8	4,1	-1,8	23,5	29,5	6,5	15,2	58,2
1989	-7,1	37,1	3,9	-3,1	25,0	33,2	6,6	15,5	66,4
	Paper	Printing	Industrial Chemicals	Other Chemicals	Rubber	Plastic	Pottery	Glass	Other Non-met
1978	1,5	0,0	0,6	6,5	-3,9	-0,6	-2,3	-14,8	1,1
1979	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
1980	6,4	0,0	-5,3	2,7	10,3	7,5	18,8	-1,6	1,3
1981	11,4	0,0	1,2	13,8	9,1	16,5	41,8	-1,7	-2,2
1982	11,4	0,0	-1,0	20,1	18,7	17,1	36,0	-1,7	-3,1
1983	16,4	0,0	5,4	20,1	21,9	17,5	49,7	-1,6	-1,5
1984	24,3	0,0	-0,4	41,9	26,6	17,8	67,5	6,3	0,2
1985	18,6	0,0	-0,3	33,4	18,8	19,7	62,8	-1,4	2,8
1985	34,0	-0,4	1,3	36,8	24,1	-10,8	74,1	9,6	9,2
1986	20,8	0,0	-0,3	32,6	19,2	18,9	63,7	-1,5	2,7
1987	20,8	0,0	-0,3	38,9	20,3	20,6	68,3	-1,5	2,8
1988	22,6	0,0	-0,3	39,1	22,2	32,0	74,7	-1,6	3,1
1989	22,5	0,0	0,1	37,6	22,1	52,2	71,7	-1,6	3,1
	Basic Iron	Non- ferrous	Metal prod	Mach- inery	Electrical Mach	Motor Vehicles	Transport Equip	Profess- ional	Other
1978	1,0	0,2	0,2	3,3	-2,5	1,7	4,8	-1,1	14,8
1979	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
1980	1,2	-2,0	3,7	0,5	0,5	0,0	-1,7	3,5	-3,8
1981	0,1	-4,0	8,2	14,3	1,8	3,0	-3,2	8,3	-8,4
1982	1,0	-4,7	10,8	19,7	2,2	2,6	-13,0	19,2	-16,8
1983	-2,5	-7,6	11,6	21,1	4,4	9,7	-5,3	53,1	-11,0
1984	-6,5	-11,6	12,6	15,1	9,5	28,4	-3,9	59,5	-16,7
1985	-6,7	-12,3	10,4	14,8	10,5	28,0	1,7	48,0	-30,1
1985	-7,2	-12,2	9,7	30,8	19,4	16,8	4,7	69,2	-56,2
1986	-7,3	-10,7	9,7	12,7	10,2	25,8	2,4	49,3	-34,2
1987	-7,1	-8,0	9,4	12,0	10,9	29,8	5,2	52,5	-50,3
1988	-6,9	-8,8	8,9	12,0	12,3	35,5	4,5	55,5	-47,7
1989	-6,5	-9,3	7,3	13,9	13,0	34,8	4,6	52,5	-53,4

Note: A positive result indicates that the benchmarked (reconstructed) value is larger than the unbenchmarked and *vice-versa*.

* There are two sets of observations for the year 1985. The second set records the differences between the unbenchmarked value for 1985 and the corresponding value from Statistical News Release (SNR) P3041.3 of 12 November 1993.

Sources: Estimated from Statistical News Release P3041.3, 9 March 1992 (benchmarked); Statistical News Release P3041.3, 12 September 1990 (unbenchmarked) and Statistical News Release P3041.3, 12 November 1993 (reconstructed).

It will be noted that there are two sets of observations for the year 1985. The second set records the differences between the unbenchmarked value for 1985 and the corresponding value from Statistical News Release (SNR) P3041.3 of 12 November 1993 - the CSS' latest attempt at estimating output levels. As may be seen by comparing the two sets of figures for the year 1985, there are substantial differences between the November 1993 estimates and the benchmarked figures published in March 1992. As noted in Chapter 2-1, to distinguish the benchmarked figures from the November 1993 figures, the latter are referred to as the 'reconstructed' figures. Substantial under-estimates were recorded by the benchmarked figures in, for example, the Paper and Machinery industries, whereas the figures for the Plastic and the Motor Vehicle industries were revised downwards.

Grouping these results into classes for the year 1985, and giving first the result for the difference between the unbenchmarked figures and benchmarked figures of March 1992 followed (in parentheses) by the difference between the unbenchmarked figures and the November 1993 estimates, it may be seen that in 16 (14) out of 27 industries, the CSS estimates of output were incorrect by more than 10 percentage points over the 6 years from 1979 to 1985. Of those 16 (14) errors, 6 (8) were greater than 30 percentage points, 3 (3) were between 20 and 30 percentage points, and a further 3 (2) between 15 and 20 percentage points. In Table 2-3.1, a positive result after 1979 indicates that output was under-estimated by the unbenchmarked results and *vice-versa*. It will be noted that the bulk of the revisions are upward. If the error estimate for the year 1978-79 is positive it indicates that the growth rate in those industries was over-estimated by the unbenchmarked figures. Eleven of the positive errors exceed 1 percentage point, and of these, five exceed 3 percentage points.

For years now, the CSS has been asked to provide detailed descriptions of the methods used to revise statistics, and of the extent of such corrections, when these are of significance. Although not opposed in principle to complying, the CSS has generally not done so, claiming when taxed on the matter that the organisation lacks the staff to perform this vital task. Here is the full extent of the guidance offered by the CSS on the errors disclosed by the benchmarking to the year 1985, in the Statistical News Release P3041.3 dated 9 March 1992:

"The effect of benchmarking was that the production index, as from the 1979 census year (1 July 1978 to 30 June 1979) to the 1985 census year (1 July 1984 to 30 June 1985) increased by 17,6% which was 5,1 percentage points higher than the previously derived figure of 12,5% which was obtained before applying benchmarking. The real gross output, as calculated from the 1979 and 1985 census results, shows an increase of 19,2% as from the 1979 to the 1985 census year. In addition to this, the published indices of some major groups for the period 1986 to November 1991 were revised on the basis of new information obtained from respondents or from external sources. The effect of this revision on manufacturing total was minimal (e.g. 0,3 of a percentage point in 1990)."

This was followed by a discussion on the comparability of the 1982 and 1985 census results which alluded to the fact that indices of Physical Volumes of Manufacturing Production (PVMPs) in certain industries differed because of reclassification. Comparability was also said to be affected by the fact that:

"Some large establishments submitted incomplete information in respect of their output/sales in the census, e.g. establishments in the paper and paper products, other chemical products and metal products industries did not report the information on output/sales as fully as it was reflected in the sample survey."

The notes were concluded with the following comment:

"Although the indices for the various major groups were amended to a greater or lesser extent, the indices for all the major groups are shown from 1978 to 1991 in this news release in order to provide users with comparable time series. The information in this news release replaces previously published monthly information."

The matter did not end there, however, for, recognising that these errors were far too large to ignore, the CSS finally took some of the steps that should have been taken when the queries first were raised. Amongst these was a review (in-house) of the survey methodology.⁴ Privately, an admission of one 'cause' of the errors was made (a 'mistaken' decision not to redraw the sample), and assurances have been offered to the effect that this would not happen again. The assurances provided by the CSS will remain somewhat less than worthwhile until such time as that institution begins to function with greater transparency and until such time as the Statistics Council is so constituted that it can exercise the vigilance necessary to ensure that this transparency is not lost.

There is a journalistic phrase - economical with the truth - usually applied to politicians, which can mean anything from outright lies, to the presentation of a report in a form calculated to minimise damage when an event that is unlikely to enhance the reputation of a public figure has occurred. One can only conclude from their conduct in this matter that the CSS is guilty of

4 Some version of the criticisms I have made of the vain attempts to measure PVMPs came to the attention of a member of the House of Delegates who requested an assurance from the (white) Minister responsible for the CSS that the matter had been dealt with (Hansard, 13 May 1993, Col. 8257). The Minister informed his questioner that a comprehensive analysis (omvattende ontleding) had been conducted and that in future the CSS would estimate indirect volume measures from deflated sales figures (Col. 8300). As will become apparent below, however, the proposed changes will not solve the problems caused by rapid structural change in the economy. I asked the Head of the CSS for a copy of the 'comprehensive analysis', but was informed that it was not in a form suitable for handing to a member of the public (Telephone conversation, July 1993). Discussions with Mr Roelf van Tonder of the CSS suggest that at the time, the 'inquiry' consisted only of a few single-page responses from various statistical agencies around the world to questions from the CSS in this regard. It would be interesting to have full details of this inquiry. In a letter from the Secretary of the Statistics Council dated 1 July 1993, it is stated that:

"A report by a subcommittee of the Statistics Council on the above subject is expected at the Council's meeting in August".

The letter to me from the Statistics Council dated 1 October 1993 quotes some of the findings of this report. It has not been possible to obtain a copy of it.

the latter. Despite repeated requests for detailed information of the impact of changes on previous estimates, the CSS persists in supplying the absolute minimum possible. As may readily be seen from Table 2-3.1, individual industry estimates undergo dramatic revision in both the benchmarked and the reconstructed figures. Although the explanatory notes that accompany the latter are slightly more generous than in the past, the guidance offered to users with respect to errors is alarmingly bad. Any user who held the unbenchmarked, or in many cases, even the benchmarked figures in the high regard claimed by the CSS was seriously misled - the CSS ought to have pointed this out. Instead, the reconstructed figures were presented to users with the comforting information that:

"The effect of the benchmarking was that the production index, as from the 1979 census year (1 July 1978 to 30 June 1979) to the 1992/93 year (1 July 1992 to 30 June 1993) increased by 18,6% which is 7,0 percentage points higher than the 11,6% which was derived from the data published in [SNR] P3041.3 of 9 March 1992 and P3041.1 of 28 September 1993." (SNR P3041.3, 12 November 1993)

The bland descriptions in the explanatory notes add little or nothing to users' understanding of the impact of the CSS' errors on previous estimates.

Financial years vs calendar years - a trap for the unwary

Following the publication by the CSS of the benchmarked output estimates in Statistical News Release P3041.3 of 9 March 1992, an attempt was made to understand how these results had been obtained, and then to replicate them. A correspondence stretching over several months ensued. This cast no light on the replication problem, but it did oblige me to reduce my estimate of aggregate output for the year 1985 downward, whittling away the discrepancy or apparent difference between it and the benchmarked CSS aggregate output estimate. By my original calculations, with 1979=100, the difference between the unbenchmarked CSS output estimate for 1985 of 104,7 and my figure of 121,9 pointed to an error of 17,2 percentage points. The newly-benchmarked figure of 109,5 suggested a residual error of 12,4 percentage points. The argument produced by the CSS picks up a mistake in my method of arriving at the figure of 121,9. The CSS acknowledges that the technique which I used to obtain the figure, would, when correctly applied, produce a higher estimate of output than 109,5 (about 4 or 5 percentage points), but claimed that the method used to arrive at the figure of 109,5 was "more correct".⁵ This claim obviously falls away with the publication of the reconstructed output figures, but the argument is worth recalling, because, as suggested above, there is no guarantee that the reconstructed figures would ever have seen the light of day had it not been for the pressure applied to the CSS through the Statistics Council.

⁵ Further details of the CSS response are given below.

Before tackling the question, there are certain matters to clarify. The references above and in Meth (1992) to a discrepancy between various magnitudes are somewhat loose - it is necessary to offer more precise definitions of the different magnitudes involved, so that it is clear what differences are being explained. This will be done in the course of showing why the residual discrepancy in the CSS figures falls from 12 to 5 percentage points.

A letter from the CSS dated 3 July 1992 revealed a misunderstanding on my part of the form in which the manufacturing census data are published - an error that shows how easy it is even for a relatively experienced user to misinterpret the official statistics. Typically, a manufacturing census is described as the '1985 Manufacturing Census' or the '1982 Manufacturing Census' as the case may be. This does not mean that it refers to the calendar year 1985 or 1982 - reading the fine print,⁶ one discovers that '1985' refers to the period 1 July 1984 to 30 June 1985, and so on. It is tempting to claim that such an error is all too easy to make because of the way in which the data are presented. Failure to read the explanatory notes, however, is not an adequate excuse, especially not for one who has pleaded over the years for the provision of more of them!

It is an error of this type which accounts for the down scaling of the expected value of the 1985 PVMP from the 121,8 given in Column 2 of Table 14 in Meth (1992) to the figure of about 115 to be derived below. The 1982 value is revised downwards from 124,8 to about 116. The corrected 1985 value of 115 is close to the 'Conservative' estimate in Column 3 of Table 14 (Meth, 1992), but the 1982 value was out by at least 3,5 percentage points.

In my letter to the CSS of 27 March 1992 protesting the minuscule upward revision of the output estimates consequent upon the benchmarking, a table summarising my (mistaken) understanding of the deflation process was included. This is reproduced below as Table 2-3.2. Based on the figures in this table, I insisted that:

"Setting the level of output equal to 100 in 1979, the value of manufacturing output estimated in this manner climbed to about 122 by 1985. A little arithmetic shows that with 1979 set equal to 100, the newly-benchmarked series has a corresponding value of 109,5 for 1985, up by about 5 points from the 104,7 recorded in the unbenchmarking official estimates. In other words, a truly spectacular under-estimate of 17 percentage points has faded into a modest little oversight."

The response, which was given in the CSS' long-awaited letter of 3 July 1992, drew "...attention to a number of facts which might have been overlooked...". Chief amongst them is the fact that the annual production indices (PVMPs) refer to calendar years and the manufacturing censuses to financial years.

⁶ See for example, the two citations above from the SNR series P3041.3.

Table 2-3.2 Incorrectly deflated gross output and the PVMP

Raw data			
Year	1979	1982	1985
Gross output (Rm)	29 926,6	55 735,6	75 092,2
Production Price Index (constant 1985 prices)	48,6	72,5	100
Source:	Gross output:- <i>South African Statistics 1990</i> , p12.6. PPI:- See text above.		
Deflate to constant 1985 prices			
Output (Rm)	61 577,36	76 876,68	75 092,2
Express in index form with 1979 = 100			
	100,0	124,8	121,9
Official estimates of the physical volume of manufacturing production			
1. Unbenchmarked series			
	1979	1982	1985
	95,5	108,7	100,0
Source:	SNR P3041.3, 12 September 1990.		
Convert so that 1979 = 100			
	100,0	113,8	104,7
2. Newly benchmarked series			
	1979	1982	1985
	91,3	106,6	100,0
Source:	SNR P3041.3, 9 March 1992.		
Convert so that 1979 = 100			
	100,0	116,8	109,5

To show what difference this made to my estimates above, the CSS reply contained a table in which gross output in current prices had been correctly deflated ie, using the 'financial' year (July to June) PPIs, and in which the results were compared with the 'financial' year PVMPs. This is reproduced as Table 2-3.3 below.

Noting that reference was made in a number of official publications of the fact that PVMPs referred to calendar and manufacturing censuses to financial years, the CSS commented that:

"You will no doubt agree that it is important that the production indices and the real gross output should relate to approximately similar periods before any valid comparisons can be made. It is similarly of importance that the periods to which the price indices relate correspond to the period for which the gross output has been calculated."

Touché. Let us guard, however, against conceding too much, and for two reasons. The first of these is that the conduct of the CSS in this matter is not absolutely beyond reproach. For purposes of examining summary data of the manufacturing census results for a number of years, say for example in performing a comparison between gross and net output in manufacturing, either by industry or for all industries, the simplest place from which to draw the basic information is the biennial *South African Statistics*. There one finds the years labelled as 1979, 1982 and 1985, etc (1990, pp12.7ff). The explanatory notes state very properly that

"Returns were rendered...in respect of their financial year which ended on any date between 1 July of one year and 30 June of the following year." (p12.68), but it would make matters a lot easier for users if the CSS stated in addition to this that the date 1979 or 1985 or whatever given in the official publication refers only to the year in which the questionnaires were posted, and that the proper designation of period should be 1978/79 or 1984/85. The old industrial censuses used to be described in this manner, and it does not seem that anything has been gained by abandoning the practice.

Table 2-3.3 Correctly deflated gross output and the PVMP

Raw data		1978/79	1981/82	1984/85
Year				
Gross output (R m)		29 926,64	55 735,63	75 092,19
Production Price Index *		45,16	68,15	92,70
(1985 = 100)				
Deflated to constant 1985 prices				
Gross output (R m)	(a)	66 268,02	81 783,76	81 005,60
	(b)	68 706,20	82 568,38	81 947,69
(b-a)/a %		3,68	0,96	1,16
Expressed in Index form with	(a)	100,0	123,41	122,24
1978/79 = 100	(b)	100,0	120,18	119,27
Official estimates of the physical volume of manufacturing production				
1. Unbenchmarked series		1978/79	1981/82	1984/85
		90,89	112,90	102,28
Converted so that 1978/79 = 100		100,0	124,22	112,53
2. Newly benchmarked series		87,85	110,54	103,29
Converted so that 1978/79 = 100		100,0	125,83	117,58

Notes: * The average of June of one year to July of the next year.⁷
 (a) Total gross output (manufacturing) deflated by total Production Price Index (manufacturing).
 (b) Total of major groups deflated by appropriate sub-indices of the Production Price Index.

Source: Letter from CSS dated 3 July 1992.

The problems to which this confusing labelling can give rise become clear when the output estimates are brought into relation with estimates of the size of the labour force. Here, the CSS stipulates that the respondents to their manufacturing census questionnaires should furnish numbers employed on the last pay-day in the June of the latter of the two years which correctly identify any particular census. In other words, for the 1984/85 manufacturing census, the employment total is that reported on the last pay-day in June 1985 (*South African Statistics*

7 Obviously, this should read...July of one year to June of the next... The statement as rendered above is reproduced correctly from the CSS letter.

1990, p12.69). It has, however, become customary to use the June value to represent the average for the calendar year in question.⁸ In other words, the financial data in the manufacturing censuses refer to one period, and the employment data to a different one.

In any event, the deflators I used in Table 2-3.2 were incorrect. Those in Table 2-3.3 represent correctly what were thought by the CSS at one point to be the appropriate output and price data for the period.⁹ According to these data, there are two discrepancies in the results. Before looking at these, let us define and label the terms used to identify the different magnitudes under consideration. The measure of output obtained by deflating total gross output by the aggregate PPI for manufacturing - (a) in Table above 2-3.3 - will henceforth be referred to as Real Gross Output¹⁰ (Total). The magnitude estimated in (b) above will be referred to as Real Gross Output (Sum of Components). This latter measure is weighted by the distribution of gross output. Finally, there is the Physical Volume of Manufacturing Production (PVMP). This is the sum of individual industry PVMPs, weighted by the distribution of net output in a base year.

Returning now to the two discrepancies revealed in Table 2-3.3, the first is the difference between output in 1984/85 as estimated by the two methods (a) and (b), a matter of some 3 percentage points (122,24 - 119,27). Where the CSS refers in Statistical News Release (SNR) P3041.3 dated 9 March 1992 to 'Real Gross Output', the reference is to (b) - Total of major groups deflated by appropriate sub-indices of the Production Price Index. There is no reference at all in that SNR to (a) - Total gross output (manufacturing) deflated by total Production Price Index (manufacturing). The second discrepancy is that between the CSS' estimate of the PVMP (117,6) and its estimate of the real (constant price) value of gross output derived by method (a), namely 4,6 percentage points. It is interesting that the CSS did not consider it worthwhile drawing this latter discrepancy to users' attention - it is, after all, nearly three times larger than the difference between the (b) result (119,3) and the PVMP (117,6). This omission is significant because the apparent closeness of the two (published) results that actually are compared might be construed as evidence that they are each close to the 'true' value. One could infer this from the fact that two different calculations had produced results that could be argued to be within the limits of experimental error.

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- 8 In 'Data Problems...', I referred to a paper by Archer (1984) which discussed some of the problems involved in this practice. See Meth, 1992, p10, n14.
- 9 Two sets of revisions to the original data have been made by the CSS. The first set remains unpublished, although details were obtained through the Statistics Council. The second set of revised data is given in Statistical News Release (SNR) P3001 of 28 June 1993. Both sets of revisions were substantial. They are examined in Chapter 2-6 below.
- 10 Recall here the difference referred to in Chapter 1-1 between the concepts 'real' and 'constant price'. Strictly speaking, the output measures discussed here are in constant price, rather than real terms. Whether such strict adherence to convention is necessary may be questioned. The term 'real' is used by the CSS - for the sake of continuity, it will be retained.

The difference in 1984/85 of 4,6 percentage points between the benchmarked PVMP (117,6) and Real Gross Output (Total) (122,2) was ascribed by the CSS, with typical but scarcely commendable brevity, to:

"...three factors, namely the implicit index formulae involved, the weighting bases used, and the adjustment of census data." (Letter dated 3 July 1992)

When the newly-benchmarked PVMPs were published by the CSS, only the difference of 1,7 percentage points between the 1985 PVMP and the corresponding Real Gross Output (Sum of Components) was referred to - no explanation was offered for the phenomenon. The difference of 3 percentage points between Real Gross Output (Sum of Components) - the (b) values, and Real Gross Output (Total) - the (a) values, was not visible and hence is not explained by the CSS. The statement in the CSS letter of 3 July 1992 that:

"...the increase in the real gross output for the 1985 census compared with that for the 1979 census amounts to 22,2% (or 19,2% fixed, gross output weights)"¹¹

seems to imply that the explanation lies in the weighting. This is not so, as will be demonstrated. The true explanation for the three percentage-point difference gives us a second reason for guarding against conceding too much to the CSS because of the confusion between financial and calendar years. This reason harks back to the proposition that the sum of the individual components of total output deflated by their own price indices must be roughly equal to the value of total output deflated by the aggregate price index. Chapter 2-5 uses hypothetical data to show quite clearly that this rule is not violated within the currency of any particular base year even if major structural change occurs. That being so, differences of the magnitude of those reported in Table 2-3.3 simply cannot occur, unless by error.

As an aside, it should be noted that although it may well be necessary to use both gross output (or a close proxy of it) and net output for weighting, it would be unfortunate if this introduced significant errors into the estimates. Apropos of the differences between the indices derived from the PVMP and those derived by deflating the Manufacturing Census estimate of gross output by the aggregate PPI, the CSS pointed out that:

"The basis for the calculation of the weighting systems for the volume indices of industrial production and the Production Price Index differ substantially. In the case of the volume indices of production, the net output is used for weighting, whereas in the case of the PPI total sales value of the components is used for weighting." (Letter dated 3 July 1992)

11 This 'explanation' for the difference of 4,6 percentage points in the CSS letter of 3 July 1992 was made after I insisted in my letter of 27 March 1992 that if all of the industry PPIs (sub-indices) were correct, then Real Gross Output (Sum of Components) and Real Gross Output (Total) would be equal.

Presumably, the reason for using sales to weight the PPI has to do with the fact that the producers selling price is an accurate guide to costs of production, including mark-up. As will be discovered on even the briefest acquaintance with the PPI, not one, but three different forms of the PPI are estimated, one of which includes imports.¹² Researchers can choose the index appropriate to their needs. It is unfortunate though that aggregates like the total PVMP and Real Output (Sum of Components) that ought to correspond one to the other (even though they are measured in different ways) should have built into them compatibility problems of a type referred to by Stone and Prais over 40 years ago (1952, p582).¹³ In an enterprise using very scarce resources to collect information on a single sector, data gathered for one purpose cannot be used to backcheck data gathered for a slightly different purpose from the same sector. The discrepancies that are introduced as a result of the use of different bases to weight the various indices may be small, but given the measurement errors of which the CSS is capable, they will not necessarily be so.

To return to the main argument, on being presented with the Table 2-3.3 results, one's first reaction may be, as mine was, to look at the differences between the results in index form, and to conclude, wrongly, that the differences between the CSS (a) and (b) series arise mainly at the end of the period (1984/85). Although there are problems in all years in the form of violations of the rule spelled out above, the major problem is in fact in the year 1978/79.

To get to the bottom of this matter, a rough check on the Table 2-3.3 results was performed by deflating the gross output for each industry by its own PPI, where this could be ascertained. The results are given in Table 2-3.4 below. The only data required are the current price estimates of gross output and the monthly PPIs for each industry (major group). These are reproduced in Appendix 2-6 as Tables A2-6.D2 and A2-6.D7. The financial year PPIs are estimated from Statistical News Release (SNR) P0142.4 of 24 March 1992. Gross output estimates are from *South African Statistics 1990*, pp12.19-12.21.¹⁴ There is a problem with the PPIs in that for some industries for which separate gross outputs are reported there are no separate PPIs. Examples are Paper, paper products and printing; Chemicals and chemical products; Rubber and plastic; Non-metallic mineral products or Motor Vehicles and Transport equipment. Some separate indices make their appearance from 1990 onwards, but in the light

12 See footnote number 9 in Appendix 2-6 for a brief description of the differences between the three. More detailed descriptions are given in the explanatory notes in *South African Statistics 1990*, p8.34.

13 The specific problem to which they refer is the incompatibility which arises when net output indices are weighted by factor costs and final expenditures by market price weights. This is similar to the problems caused by the use of net and gross output, or net output and value of sales.

14 From Table A2-6.D7 it will be observed that total gross output in each of the years concerned is identical to that given in Table 2-3.3 above. This will be shown to be of relevance when the CSS statement about results differing partly because of "...adjustment of census data..." is considered.

of their absence from the published figures in the period before that, it is not unreasonable for the user to assume that separate indices did not exist. For the purposes of the deflating exercise about to be performed, the single index that exists for the group of industries has been used for each of the separate industries concerned.

As will be shown in Chapter 2-5, one would not expect there to be substantial differences between the values of total output in constant prices estimated in the two different ways (deflated total vs sum of deflated components) to arise. The CSS Table 2-3.3 (a) and (b) estimates of gross output deflated to constant 1985 prices fail this test rather spectacularly, especially the 1978/79 figures. An estimate of the percentage difference between the (a) and (b) estimates has been inserted into the table - the 1978/79 divergence is nearly 3,7 percent. Comparing the estimate of the sums of deflated components in Table 2-3.4 with the corresponding (a) values in Table 2-3.3 (100,0; 123,41 and 122,24) it may be seen that the 1984/85 value is so close as to make no practical difference. The 1981/82 estimate is out of line, but by a smaller amount than the CSS estimate.

These results were shown to Mr Roelf van Tonder of the CSS on 22 July 1993 - he was unable to provide any explanation for differences between the CSS Table 2-3.3 (a) and (b) estimates and those in Table 2-3.4. During this visit, the problem of variations in the ratio of net to gross output, and their impact on indirect output estimates was also discussed. No obvious 'official' solution to the serious difficulties caused by the massive changes of this type in the industry 'Other Chemical Products' could be found. It was conceded, however, that changes of that magnitude would give rise to problems. This is discussed at greater length in Chapters 2-4 and 2-5. It was not possible to explore the differences between the Table 2-3.4 and the Table 2-3.3 results because the person responsible was away at the time. A request for an appointment with her on her return from vacation was refused, as was a request for permission to examine the calculations which were used to produce the Table 2-3.3 results. This time, a direct appeal to the Head of the CSS had immediate effect - a letter sent by courier on Friday 30 July 1993 saw Mr van Tonder produce the required information by the morning of Monday, 2 August 1993. The answer - a simple one - is that the CSS deemed it appropriate to use the Petroleum and Coal PPI as the deflator for (part of the output) of the industry 'Other Chemical Products'. Minor problems were reported in a few other industries such as 'Other Manufacturing' and 'Professional Equipment', where the PPIs were held to be so unreliable that the aggregate PPI had to be used to deflate the results for those industries.¹⁵

¹⁵ Telephone discussion with Mr Roelf van Tonder, 2 August 1993. I used the Chemical and chemical products PPI for deflating the output of that industry.

Table 2-3.4 Unrevised gross output estimates deflated by financial year PPIs (1985 = 100)

	1978/79	1981/82	1984/85
Food	10742,5	11157,7	12453,2
Beverage	1568,2	2455,0	2810,0
Tobacco	358,3	456,5	525,3
Textiles	3121,0	3669,5	3233,5
Clothing	1511,2	2165,3	2072,9
Leather	269,6	352,9	398,3
Footwear	637,2	740,7	709,3
Wood	1019,2	1228,2	1287,8
Furniture	597,9	921,9	953,3
Paper	2242,3	2352,2	3245,4
Printing	1548,0	1986,8	2204,7
Industrial Chemicals	3847,0	4570,0	4099,3
Other Chemicals	6728,7	8730,8	10329,1
Rubber	869,3	1117,6	1012,1
Plastic	1003,8	1720,1	1608,9
Pottery	71,9	119,8	109,2
Glass	424,5	592,5	530,3
Other Non-metal Minerals	2102,1	2548,4	2444,4
Basic iron	6303,8	6887,9	6036,6
Basic non-ferrous	2186,3	2554,9	2609,1
Metal Products	5681,5	7217,3	6145,4
Machinery	3611,3	5597,6	4684,9
Electrical machinery	3034,8	4172,3	3960,3
Motor vehicles	4493,9	6908,2	5631,6
Transport equipment	1294,1	1173,4	804,8
Professional	118,7	186,0	311,3
Other manufacturing	825,9	885,0	819,7
TOTAL	66212,8	82468,4	81030,5
Set 1978/79 = 100	100,0	124,6	122,4

Source: Estimated from the data in Tables A2-6.D2 and A2-6.D7 in Appendix 2-6.

At the August 1993 encounter, the CSS maintained firstly that there can be a difference between Real Output (Sum of Components) and Real Output (Total), and secondly that the difference in the case above is produced mainly by differential movements in the petroleum price index and the PPI for the industry 'Industrial and Other Chemicals' (the published PPI used to obtain the results in Table 2-3.4). It will be shown in Chapters 2-5 and 2-6 that such a result shows merely that an error has been made. If a properly-constructed aggregate PPI of the Paasche type is used, the Real Output (Total) obtained by using it to deflate total gross output is **identical** to Real Output (Sum of Components) obtained by summing the individually deflated components by their own (Paasche) price indices. This is true regardless of changes in price/quantity relativities.¹⁶ If Laspeyres price indices are used, differences between output

16 In national accounting (and in strict economic) parlance, one refers to price and quantity relatives (or relativities), and to price and volume indices. The former, which constitute the stuff of which most indices are constructed, are pure numbers of the form P_t/P_o or Q_t/Q_o , where P_o (Q_o) and P_t (Q_t) are the magnitudes of the variables P and Q in years o and t respectively. For a discussion on the reasons why the concept of a 'quantity index' may possibly be ambiguous, see UN, 1993, p381.

estimated using the two approaches can emerge, but these appear invariably to be trivial, even under conditions of fairly severe structural change. These claims are defended at length in those two chapters - they will not be addressed in any detail here.

Hammering out a research agenda

Emerging from the welter of claims and counterclaims above was a clear research agenda. One of the tasks was to unravel the three-point explanation offered by the CSS in the letter of 3 July 1992 for the 4,6 percentage-point difference between Real Output (Total) and the PVMP, and to bring it into alignment with the recently-acquired information that some of the difference between Real Output (Sum of Components) and Real Output (Total) of 3 percentage points in 1978/79 results from a particular choice of deflators. In effect, the new information seemed to suggest a decomposition of the 4,6 percentage point error into two components. Although the CSS had not acknowledged it, the three-part explanation, to the extent that it had any validity at all, appeared to refer not to the 4,6 percentage-point divergence, but rather to the 1,7 percentage-point difference between Real Output (Sum of Components) and the PVMP. Only detailed examination of the CSS calculations could have shown whether this decomposition was justifiable or not. For the purposes of the investigation to be undertaken, the hypothesis that it was justified had certain uses.

Despite the smallness of the error in question (1,7 percentage points difference in a growth estimate over a six-year period is scarcely visible when considered as an annualised rate) it was considered worthwhile tracing the CSS claims to their roots. This was so because one of the 'factors' advanced as a partial explanation (adjustment of the census data) was manifestly false, and because another (the implicit index formulae involved) seemed to conceal an ignorance of the implications of using Laspeyres price indices where Paasche are indicated. The 'weighting bases used' part of the explanation seemed to be the only bit that can lay any claim to credibility. It was probably a reference to the fact that the PVMP and Real Gross Output (Sum of Components), an indirect volume measure of output, can differ because even though the individual components which go to make up the aggregates are forced into equality through the benchmarking process, the relative weights used to sum these components give rise to differences in the aggregates (weighted sums). In the case of Real Output (Sum of Components), the weights used are the proportional contributions to total gross output, whereas the PVMP is weighted by proportional contributions to total net output.

As far as the 3 percentage-point difference between Real Output (Sum of Components) and Real Output (Total) was concerned, the task was to demonstrate that this could not possibly

arise if the relevant PPIs were correct. In the course of doing this, the 'implicit index formulae' 'factor' could be dismissed as well. This is tackled in Chapter 2-5. So also, is the 'adjustment of the census data' 'factor'.

Overshadowing all three of the 'factors' is the problem caused by the change in the ratios of net to gross output that occurred over the period 1979/85. These changes, it will be argued, were caused primarily by Sasol II and III coming on stream in the 1980s. The substitution of coal for petroleum as a major input (to an extent as yet undeterminable because the necessary information is not available) apparently accounts for almost all of the increase in the ratio of net to gross output in the period 1979-85. For want of a better name, this episode and the unfortunate effects it has on national accounting estimates will be referred to as the 'Sasol Syndrome'.

The only national accounting technique for dealing with a problem of this sort, double deflation, requires data of very high quality if reliable output estimates are to be produced. That means it is going to be difficult, if not impossible, to produce 'correct' output figures for South African manufacturing. A research project must be mounted to tackle this problem. Whatever it may finally reveal, it is certainly not difficult to show that output, even according to the reconstructed estimates, is incorrectly estimated at present. Using a few plausible assumptions and a data set that reproduces some of the South African conditions, the simulations used to establish that Real Output (Sum of Components) and Real Output (Total), correctly estimated, cannot differ significantly, have been extended to reveal the impact of massive changes in the net to gross output ratio in one industry. From this simulation, a set of double deflated output estimates, based on the actual division of gross output between intermediate inputs and net output (value added) in the industry 'Other Chemicals' over the period in question, has been obtained. As noted above, it is difficult to estimate accurate output levels because the price and quantity (volume) data are unreliable. Nonetheless, the results appear to be reasonably robust. This finding is corroborated by the surrogate measure of 'real net output' that is offered in Chapter 2-7.

Chapter 2-4

On the 'correct' ways to produce price and output indices

Introduction

This chapter divides into two parts. In the first, an attempt is made to expose the workings of a particular mindset within the CSS, one which sees officials falling back on conventions as a defence mechanism whenever criticism of their workmanship is made. This defensiveness is compared (unfavourably) with the pragmatism displayed by the architects of the United Nations *System of National Accounts* (SNA). The second part contains a discussion on the problem of 'breakdowns' in additivity in the national accounts. This is followed by an examination of the mechanics of constructing Paasche and Laspeyres indices. It ends with a brief reference to the simulation exercises in the early version of the SNA (UN, 1968).

Out with the PVMP - in with a Monthly Sales Inquiry (MSI)

One of the matters in dispute in the long saga of which it is hoped that the present work will form the final chapter, has been the question of whether or not the CSS had/has the capacity to measure physical volumes of output sufficiently accurately to permit the inclusion of these estimates in whatever calculations users may deem necessary. Chief among these, it could be argued, are such things as industry output growth rates, and labour and capital productivity estimates. The answer to this question was, and remains, in my view, a resounding no. It has now been established beyond all doubt that during at least two important periods in South Africa's recent history, the PVMP became thoroughly unreliable. In a letter to the Head of CSS, I stated with reference to the PVMPs for individual industries (major groups and sub-groups) that:

"As regards the CSS method, you must be aware of the fact that doubts have been expressed as to the usefulness of continuing to estimate separate output indices for each major group or subgroup. Apparently there is little justification for producing these estimates. Obviously, the case against them would be strengthened if they were shown to be unreliable." (27 March 1992)

Insisting that the sub-indices were produced because firms in several industries demand them, the CSS rejected the criticisms offered. The PVMPs serve (at least) two functions, one being to serve as building blocks for estimating the value of aggregate output, the other (apparently)

being a service to certain industries, to permit individual firms to compare their production levels with those of the industry in general. Undoubtedly, if the output volume measures produced by the CSS had been robust and reliable, it would have been reasonable to carry on doing so. As has been shown on more than one occasion, however, this was not the case. That being so, the reasonable thing for the CSS to have done would have been to have acknowledged that there were doubts about the wisdom of continuing to produce these indices.

The quibble here is about the pusillanimous unwillingness of the CSS to concede that which was obvious and which was shortly to be conceded anyway. At the same time as the CSS defended the PVMPs on account of the alleged esteem in which they were held by users, a tentative look at an indirect system of measuring output had already commenced. It was stated in the CSS response to my very first request to the Statistics Council to investigate the problems in the manufacturing sector output estimates (Meth, 1991a), that:

"The CSS has taken note of the methodology being taken into use in the United Kingdom and is considering its application for use in South Africa." (See Appendix 1 in Meth, 1992, p67)

Some time later, this was followed by a remark in a letter from the CSS in which it was stated that:

"...the CSS intends to study the methodology employed by Britain's CSO, as and when resources become available." (Letter dated 10 August 1992)

There never was any sound reason for clinging to the direct method of producing PVMPs. There are other ways to produce output indices, a fact of which the CSS was well aware. There is, for example, an alternative approach used by the British,¹ the Australian and possibly several other national accounting statisticians as well. The well-known Federal Reserve Bank industrial production index in the US (Kennedy, 1993) is another possible model with a long

1 Dissatisfaction with the quality of the PVMPs estimated in the traditional ('correct') method way led to their being all but abandoned for most industries in Britain (Kingaby, 1989; Black, 1991). In their place is a growing dependence on an indirect measure obtained by deflating suitably modified data from a Monthly Sales Inquiry (MSI) by the appropriate PPI. In this connection, Kingaby (1989) notes that:

"More volume indicators are now used for building materials. For most of manufacturing industry the output indicators are sales deflated to base year prices. However where a product is very well defined, homogeneous, and does not change over time then a better output indicator is the volume of production. This removes the measurement error in deriving price deflators. Volume indicators have always been used for certain chemicals and building materials and from this rebasing [the rebasing in Britain to the year 1985] more volume indicators have been used for building materials." (p106)

An industry which must pose considerable difficulties for national accounting statisticians everywhere is the computer industry. According to Kingaby:

"A more appropriate deflator is now used for the computer industry. The deflator for the computer industry is based on a combination of the price of computer hardware and an index of wages and salaries to reflect the software content in sales. More reliable information is now used to estimate the proportions in which these two measures are combined." (p106)

history (Stolper and Doblin, 1941).² In any event, it has now been decided to use an approach something like that used by the British.³

The decision to do so,⁴ and a brief description of the method by which output indices are now calculated is given in SNR P3041.3 of 12 November 1993. In short, the CSS has switched to estimating output indirectly from suitable estimates of the value of sales.⁵ This step, allegedly in accordance with "international guidelines",⁶ will unfortunately not prevent the occurrence of errors of the second type described in Chapter 2-3, ie, errors resulting from the inappropriate use of gross output or some approximation to it such as value of sales, suitably deflated. On the question of the construction of output indices from single indicators, the latest SNA (UN, 1993) states that:

"The volume index used to extrapolate base year value added can itself be calculated either directly from quantity data or by deflating the current value of output by an appropriate price index. If the data on output at current prices are comprehensive and reliable, the latter method is likely to yield the better estimates."
(p392)

The relevant question, it would seem, is whether or not 'comprehensive and reliable' data are available? If there were, then large divergences between Real Gross Output (Total) and Real Gross Output (Sum of Components) like those reported above simply could not arise. Such errors have reportedly not occurred in Britain.⁷ Although this adds weight to the arguments for abandoning the production of PVMPs by the method currently employed in South Africa, it is clear that the mere adoption of the British approach will not, by itself, solve the problems addressed in this study. Unless the problem of the errors in the deflators revealed in Table 2-3.4 above is resolved, and a suitable means can be found for dealing with the impact of rapid

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- 2 The multi-stage process for constructing the index constitutes a useful critique of the proposed new South African method. See Kennedy (1993).
 - 3 It is as well to treat the MSI with caution. Apart from the fact that a 'value of sales inquiry' suffers from the same weaknesses as attempts to measure gross output, it has the additional problem of having to cope with estimates of inventory changes. An engagingly titled article "Production, Sales and the Change in Inventories: An Identity That Doesn't Add Up" (on which, unfortunately, I could not lay hands) apparently tells of the difficulties experienced in this regard in the US. See Miron and Zeldes (1988).
 - 4 See footnote number 4 in Chapter 2-3.
 - 5 Note that surveys which measure the value of sales are already conducted by the CSS. Published estimates in the past have been somewhat irregular - see Meth, 1992, p36. Different measures will be used to estimate output levels in two industries. In 'Petroleum and Coal Products' physical volumes will continue to be measured, and in 'Transport Equipment' manhours will be used. See SNR P3041.3, 12 November 1993, p1.
 - 6 The chapter of the latest SNA dealing with 'Price and Volume Measures' (UN, 1993, Ch XVI) refers to several procedures for estimating indirect volume indices. There is no suggestion that the method now adopted by the CSS falls within the scope of any 'guidelines'. The technique is simply one of several that could be used.
 - 7 This emerged in discussions with British national accounting statisticians that took place during a visit by the author to the Central Statistical Office in Newport, Monmouthshire in June 1992. It is to be hoped that if something similar to the 'Sasol Syndrome' had befallen the British national accounting statisticians, that they would have responded with greater skill than did the officials at the CSS.

changes in the net to gross output ratio, the probability of serious error in the future must remain.

Up until the point at which the CSS abandoned the 'direct' method of estimating output volumes, officials insisted that the method in use was 'correct', the statistical garbage it has produced on occasion notwithstanding. The reply from the CSS of 3 July 1992 to my letter of 27 March 1992 stated that:

"Regarding your comments on the two ways to obtain a benchmark estimate of the real output of the manufacturing sector, I wish to assure you that the method used by the CSS is not only the statistically correct method but also the method recommended by the Statistical Office of the United Nations."

At issue, of course, is the problem of the difference between the CSS' estimates of Real Output (Sum of Components) and Real Output (Total). In defence of the former (the (b) estimates in Table 2-3.3) the CSS cited a passage from a United Nations document which reads as follows:

"A frequent practice is to use Laspeyres price indexes to deflate current price values at the most detailed level for which both prices and values are available." (Letter dated 3 July 1993)

It could be argued that to translate a bald factual statement which says that such and such an activity is a 'frequent practice', into one in which it has become a 'recommended practice', is to take unwarranted liberties with the text. Such a pedantic reading is unnecessary however, and a good defence of the technical reasons for building up the output estimates from highly detailed deflated figures is given in the passage quoted from Rushbrook (1978) in footnote No. 12 below. Unfortunately, it is can also be demonstrated quite incontrovertibly (and will be below) that properly estimated, Real Output (Sum of Components) **cannot** differ from Real Output (Total). Any divergence that does arise simply provides a measure of the errors involved in estimating one or the other.

Statistical Correctness vs International Compromise

One can be gracious about the attempted use of the 'frequent practice - recommended practice' argument to defend poor quality estimates, but there is no cause for extending that generosity to the claim that the method used by the CSS to value output is 'statistically correct'. The literature on index numbers is accessible enough for anyone to discover that the indices in common use in national accounting practice (predominantly the Paasche and Laspeyres) are themselves but crude approximations of theoretic indices. The CSS apparently sets great store

by the fact that in accordance with accepted international practice, it produces Laspeyres output indices, presumably using Paasche price indices wherever possible. But as noted in Chapter 1-2, Hansen and Lucas have argued that:

"...based on their rather implausible economic assumptions, it is no longer justifiable to use the Paasche or Laspeyres formulae, except possibly in situations where a subjective rather than an economic result is desired." (1984, p33)

Without entering into the merits of this claim, it is clear that to talk of 'statistically correct' behaviour in a context in which one seeks to estimate a mathematical proxy for some hypothetical variable is meaningless.⁸ The appeal to authority implicit in the claim to be acting in such a 'scientific' manner looks suspiciously like a diversion aimed at distracting attention from a weak defence.

As observed in Chapter 1-2, notwithstanding the apparent discrediting of Paasche and Laspeyres indices, the latest version of the *System of National Accounts* (SNA) (UN, 1993) still makes extensive use of them, so one is, in a sense, stuck with them. That, however, is cause for acknowledgement of deep dependence on mere convention, not reflex appeal to authority. As far as can be ascertained, when the gentle art of estimating output in constant prices is being conducted, there can be no question of adhering to 'statistically correct' methods. Instead one finds compromises, many compromises - conventions of one sort or another to help national accounting statisticians navigate their way around otherwise intractable problems. The issue of conventions in the construction of national accounts, into which the results of attempts to estimate the volume of manufacturing production eventually find their way, will now be considered at greater length.

Despite claiming on numerous occasions that constructive criticism of CSS methods and results is always welcome, there is a strong resistance within that institution to change. The method by which the value of output in constant prices used to be estimated (the attempt to measure PVMPs directly) is a case in point. In defence of this approach, officials at the CSS responsible for the indices in question appeared to grasp at any argument, no matter how trivial. Criticism of official statistics that makes use of unconventional consistency tests like those suggested in this study was simply dismissed or ignored. Thus, for example, the suggestion that it is appropriate to use Real Gross Output (Total) as a yardstick against which to measure the quality of estimates of Real Gross Output (Sum of Components) was countered by citation of international standards, presumably to demonstrate that permissive conduct in the

⁸ This matter will be raised again in Chapter 2-7, where such fundamental questions as the validity of the aggregation procedures which give rise to a need for indices of different forms are considered.

matter of deflators produces results that are technically incorrect, and hence suspect. Reference was then made to the UN publications "Index Numbers of Industrial Production", ST/STAT/SER.F/1 and "Guidelines on Principles of a System of Price and Quantity Statistics". (Letter from the CSS dated 3 July 1992) This was not the first time that the CSS had referred to proper behaviour in the matter of the use of Paasche and Laspeyres indices. The early statement about the CSS 'taking note of the methodology for measuring output in the United Kingdom' had added to it a rider to the effect that:

"The fact that the deflated series yields a Paasche volume index for the month to month movement of the series needs to be taken into account." (See Appendix 1 in Meth, 1992, p67)

It is by no means clear why deflating the value of sales (a not-too-distant surrogate for gross output, once inventories and sales of factored goods have been accounted for) should generate a Paasche volume index. A similar confusion, and the simple algebra needed to dispel it are discussed below. Be that as it may, if the CSS had been correct about these indices, it might have been appropriate to draw attention to their improper use. Even had this been so, a balance between mere pedantry and scientific caution would still have been necessary. All the indications in the present instance are that the CSS' excessive but misplaced correctness was of little practical significance. In order to show why not, it is necessary to do two things. The first is to demonstrate that the primary United Nations document on national accounts, the SNA, shows evidence of having been written by specialists who displayed a lively awareness of the fact that rigid adherence to 'recommended' international practices was not possible and therefore could not be made mandatory. This is clear in the discussions of the limitations of the data with which national accounting statisticians are forced to work. The second thing to do is to consider the impact on the end results of using indices 'incorrectly', eg, a Paasche where a Laspeyres is indicated. These two tasks are tackled respectively in this and the following part of the work.

As noted earlier, reference is made here to the 1968 Edition of the SNA because at the time that the errors under consideration in the study were made, that edition was all that was available - the latest edition (UN, 1993) is still so 'fresh' that few national accounting statisticians will have had a chance to switch over to the new rules. In any case, the flexibility of the old SNA does not seem to have disappeared in the new one.

Chapter IV of the 1968 SNA contains as clear an indication as may reasonably be demanded of the fact that compliance with the technically correct rules for the construction of national accounts (of which the manufacturing censuses form an important, albeit indirect part) is no

simple matter.⁹ The compilers of the guide show all the signs of being eminently practical persons - they make it quite plain that compromises involving the acceptance of second- or even third-best solutions will often be forced upon the national accounting statisticians. Some of the paragraphs relevant to the matter at hand are reproduced below - with passages of particular interest highlighted in bold typeface. These citations are embedded within the context of each particular paragraph as a whole, to ensure as far as possible that they are not distorted by selective quotation.¹⁰

From the passages quoted below it is obvious that there is little reason for clinging on to those few statements which note that a Paasche index is the technically correct index to use here, or a Laspeyres there. My reading of the United Nations document is that it is above all things, pragmatic in its approach. When 'perfect' data are not available (most of the time) imperfect substitutes must be accepted, albeit with regret.

Most of the statements cited are self-explanatory, and comment can be limited to a contextualisation where necessary. No quotation marks are used, but all of the statements cited are indented for ease of identification. Statements introducing particular paragraphs or my comments on the contents are not indented. The paragraph numbers are those given in the SNA (UN, 1968). The first of these has direct bearing on the qualms which officials at the CSS have about using a Paasche index where a Laspeyres is appropriate, or *vice-versa*.

4.45. Laspeyres and Paasche index numbers are symmetrical: a price index of one form multiplied by a volume index of the other form is equal to the relative change in value. In practice it is usual to construct Laspeyres index numbers of quantities¹¹ and to derive implicit Paasche index numbers of prices by dividing the quantity index numbers into the corresponding changes in values. **In some cases it may be better to construct Laspeyres index numbers of prices and derive implicit Paasche index numbers of quantities. The mixture of forms in this way leads to some inconsistency; but this is often slight and usually worth incurring if it makes possible fuller use of the available information.**

The price approach and the quantity approach

4.48. Since prices and quantities enter in a symmetrical way into the formulae for the various index numbers, alternative approaches are possible even if our aim is to construct only a system of quantity index

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- 9 The fact that procedures laid down can be technically correct at the same time as the indices they produce are meaningless in economic terms, as Hansen and Lucas suggested the Laspeyres and Paasche indices were, needs to be borne in mind.
- 10 At the time that the writing up of this study commenced, the latest SNA became available in draft form (UN, 1992). Minor changes to the 1968 version were made from time to time, but no major revision had been attempted until very recently. For a discussion of the reasons why such revision was long overdue, see Pyatt, 1991. Far from adopting a more hardline position to that taken in the Revision 3 version of the SNA published in 1968, the new SNA continues the tradition of pragmatism established in its predecessor.
- 11 The original SNA was not as scrupulous in its use of concepts as the revised SNA suggests one should be. Recall here the discussion in footnote No. 16 of Chapter 2-3 in which it was stated that the proper term is 'volume index'.

numbers. We can either follow a direct approach and concentrate on obtaining indicators of quantity movements or we can follow an indirect approach, obtain indicators of price movements and derive implicit indicators of quantity movements by dividing the price indicators into the corresponding series of values. If we are able to mix the formulae correctly, for example to construct a price index by Paasche's formula in order to obtain an implicit quantity index on Laspeyres' formula, the two approaches lead to formally identical results. **If this cannot be done, that is to say if all the index numbers we can construct, whether of prices or of quantities, are of Laspeyres' type, some measure of formal inconsistency will result. In practice, a mixture of the two approaches is likely to represent the best use of the data available and they should be regarded, therefore, as complementary rather than competitive.**¹²

4.49. In either case the main problem is to obtain a good measure of the component prices or quantities. From a practical point of view, this is the basic problem of index-number construction and it cannot be surmounted by any amount of mathematical manipulation of the kind described in the annex. It is frequently a simple matter to get a crude measure of quantity, numbers of cars or tons of steel say, and so given value statistics, to derive a crude measure of price. But at any one time there is a wide range of new cars available and as change follows change in successive new models, the quality of the average new car changes too. The same can be said of steel or, indeed, of almost any commodity. As a consequence the crude unit is not likely to be very satisfactory for measuring either prices or quantities. The question then arises: how can it be improved?

After fairly extensive discussion of the ways in which the measures can be improved, the writers of the SNA conclude that:

4.72. It cannot be claimed that this formulation of the problems, and the examples which illustrate it, provide definitive answers to particular questions. The answers reached in practice must depend to some extent on the data available and to some extent on the importance attached to improving these data in specific cases. If, over wide areas, phenomena change in an approximately common ratio, many of the refinements suggested are of only minor importance. What should be questioned is whether, over wide areas, phenomena do always change in a common ratio.

It is more than merely unlikely that the CSS could claim that in the potentially critical matters of changes in quality and technology, probably the most elusive of properties of commodities

12 An instance of the use of Laspeyres price indices where that of Paasche indices is indicated is given in the following passage. The passage is drawn from an article that examined the rebasing of the British national accounts. The author states that:

"In practice, volume changes can be measured directly through indicators of physical quantities multiplied by base year prices only where data are continually available for homogeneous categories of goods and services. In most cases the categories of goods or services for which data are available are far from homogeneous and volume changes have to be estimated indirectly from changes in value deflated by appropriate price indices. In order to yield base-weighted volume series, values should be divided by current-weighted (Paasche) price index numbers. In practice, the individual price index numbers used to deflate each identified category of expenditure may not be current-weighted because the necessary data are lacking. However, because the deflation of current values is carried out in as great a detail as practicable the correct weighting among individual categories is achieved. The incorrect weighting *within* categories (because the deflators used are not current-weighted) is one of the imperfections of the constant price estimates.

Following an analysis of the effects of rebasing the national accounts on 1970 it was concluded that 'given that most of the calculations are carried out at a considerable degree of disaggregation, the use of base-weighted deflators is probably not in practice a serious deficiency in the method of estimation, and it is unlikely to be any more serious than other deficiencies in the data on price movements.'" (Rushbrook, 1978, p106) (Emphasis in original)

and the production processes by which they are made, they have any 'internationally approved method' of carrying out valuations. Judging on past performance, there is little hope that if they did dispose of such tools, they would be able to collect the data necessary to keep them calibrated. It does not seem unlikely that the errors resulting from an inability to measure adequately the changes in these two characteristics will outweigh by far the trivial errors usually involved in using Laspeyres price indices where Paasche is dictated by mathematics.

Indicators of prices and quantities

4.73. Before turning to a numerical illustration of the system of index numbers described in this chapter, let us take a look at the kinds of indicator likely to be available in practice. **Quite apart from the refinements suggested in the preceding chapter, even crude indicators are not always available in an ideal form and so a decision has to be taken as to the best use of what is available; as we have seen, it will be best in some cases to accept direct measures of quantities and in other cases to construct indirect measures by dividing a value series by a price series.**

4.74. The necessity for choice and compromise can easily be seen...

FINAL DEMANDS

4.76. Available indicators are likely to take one or other of the following forms.

(h) *Values deflated by prices.* This method depends on the availability both of a series for the value of expenditure and of an appropriate price index number. As we have seen, there are likely to be many cases in which this indirect approach gives better results than the direct one.

Presumably, the value of sales measured by the CSO Monthly inquiry, although not a 'final demand' in the strict sense, is a measure of the same type.

GROSS OUTPUTS

4.77. Some of the series refer in the first place to the gross output of commodities while other series relate to the gross output of industries and other producers.

(a) *Gross outputs.* These adjusted as far as possible for quality and other changes can be regarded as the standard to which the following alternatives are, in one way or another, inferior;

At the end of the 'inferior' series is this one:

(e) *Values deflated by prices.* In many cases there is a choice between quantity measures of the components of a commodity or industry group and the corresponding measures of prices. **The price approach is often to be preferred because there is likely to be more uniformity in the movement of the component prices than in that of the corresponding quantities. For this reason a sample of prices is likely to be more representative than a sample of quantities; and it will also often be easier to define and collect.**

National accounting data in some countries may well have improved in quality beyond all recognition - to the point, indeed, where compromise solutions such as those proposed above by the compilers of the SNA are now no longer necessary. This, however, would have to be proven rather than simply asserted. In the case of the British national accounts, quality deteriorated to the point where an extensive overhaul became essential.¹³

The quality of the South African national accounts data has undoubtedly improved since the 1968 edition of the SNA appeared, but that does not mean that the need for compromise has disappeared - certain of the data collection problems referred to in the SNA can never be resolved satisfactorily, most especially those referring to volumes of output. That national accounting statisticians in an advanced economy like Britain (with far greater resources than those available in South Africa) have given up trying to measure directly volumes of output in most industries in manufacturing is highly significant.

Does the CSS ever use Laspeyres price index numbers to estimate output levels 'indirectly' where they should (theoretically) be using Paasche - for example, during the benchmarking process? Even if they do not, which seems improbable, given that industry-level PPIs (and, presumably, many other price indices as well) in South Africa are almost invariably Laspeyres price index numbers, their 'statistically correct' behaviour would appear not to be emulated by their British (and other) counterparts. This, it seems, is almost certainly the case in the 'indirect' estimation of constant price output levels for the all-important Index of Production (IOP) in Britain. The concentration on what is in reality a trivial technical detail contrasts very strongly with the almost cavalier disregard for the inaccuracies to which attention is drawn in this study.

13 The reasons why the British statistics, having been amongst the best in the world, suddenly deteriorated, were speculated upon, briefly, in Chapter 2-1. The decline in quality observed provides a warning against excessive decentralisation and government meddling. Several recent issues of the *Economist*, especially that of December 26th 1992-January 8th 1993, p16, have addressed the problem. Among the more strongly argued points in this article is a plea for a change in the attitude towards manufacturing statistics. Commenting on the poverty of official figures, it is observed that:

"...the number-crunchers themselves must bear much blame. They suffer statistical-lag syndrome, preferring perfection (sic) to speed, and convenience to both. In America, three motor companies account for \$150 billion-worth of output; to get the same coverage in the restaurant and drink industry, you would need to survey more than 150,000 firms. Unlike things you can drop on your toe, a unit of output in services is tricky to define and difficult to measure; and it is even harder to take account of its improving quality.

Statisticians must change their priorities, collect fewer detailed figures for manufacturing, and study services. Manufacturers, keen on the myth that only manufacturing matters, always resist any pruning in the statistical surveys that help their business. If they value these data, let them pay for them. Number-crunchers, themselves part of the neglected service industries, should start looking after their own."

Paasche and Laspeyres indices and the loss of additivity

There appear to be a few separate, but related confusions in the thinking of the CSS that need to be addressed. These confusions are concerned with the property of constant price output estimates known as additivity. Additivity is a property of national accounts such that the sum of individually deflated constant price output estimates, be they the components of a particular sector (major division) eg, manufacturing, or the sum of sectoral totals, is equal to the value of the relevant total, obtained by other means. Under certain conditions, additivity is maintained, under others, it is not. The results furnished by the CSS in Table 2-3.3 above represent a breakdown of additivity that is apparently not due to any of the more usual causes of this condition.

Developing an understanding of the causes of breakdowns in additivity, both theoretical and practical or 'real', is a two-stage task. In this chapter of the study, some of the relevant theoretical aspects of index construction will be considered, and an illustration of a specific breakdown in the South African national accounts will be presented. In the next chapter of the study further examination of the conditions in which a breakdown in additivity can occur, and equally importantly, those in which it cannot, will be undertaken. An attempt will also be made to distinguish these breakdowns from the often trivial errors that are introduced into a series when Laspeyres price indices are used instead of Paasche. Two sets of simulations are performed in Chapter 2-5 - the results may be summarised in the following way:

- i During the currency of any particular base year, if properly constructed Paasche price indices are used to produce constant price output estimates, the value of Real Output (Total) is identical to that of Real Output (Sum of Components).
- ii Within fairly broad limits, it appears possible to substitute Laspeyres price and volume indices for Paasche and *vice-versa*, without serious harm to the resulting estimates.
- iii As a corollary to these two propositions, it can be stated that when a divergence between Real Output (Total) and Real Output (Sum of Components) such as that noted in Table 2-3.3 above is observed, the most likely cause is an error of some sort in the estimation of either the sub-sectoral or the aggregate deflators, or both. To confirm the argument, an error of approximately the correct size is induced in the results by using an 'incorrect' price index in one of the industries.
- iv Significant structural changes (ie, major changes in price and/or quantity relativities) compel national accounting statisticians to abandon one or other of the attributes of the national accounting entities that together give the estimates the quality of consistency.

In the course of the lengthy correspondence that has passed between myself and the CSS on the question of errors in the manufacturing estimates, either directly, or through the Statistics Council, a set of CSS positions on particular questions has emerged. These are somewhat fragmentary, either because the views expressed were incidental to other arguments being

conducted at the time, or, as in the important matter to be discussed below, the views articulated were off-the-cuff responses to particular challenges posed. Given this, it is sometimes quite difficult to say with certainty what the CSS view is. With regard to point No. ii above, for example, the need to explore the issue in some detail arises because in response to my claim that it was appropriate to estimate the value of Real Output (Total) by deflating total gross output by the aggregate PPI, and to use the results as a check on the value of Real Output (Sum of Components), the CSS replied as follows:

"...if the total gross value of manufacturing output is deflated by the appropriate aggregate Production Price Index (a base-weighted Laspeyres index) a current weighted (Paasche) volume index is derived, in contrast to the internationally recommended base-weighted (Laspeyres) form that should be used for a regular monthly series of volume indexes of production..." (CSS letter of 3 July 1992)

This statement, which repeats the similar assertion referred to above, is a little confusing because it masks what would seem to be a common national accounting practice, namely, that of applying Laspeyres price indices as deflators to current price output estimates and treating the results as though they were Laspeyres volume indices. Speaking pedantically, one cannot 'deflate' gross output by a Laspeyres price index. Expressed algebraically, the operation would look like this:

$$(\Sigma Q_t P_t)(\Sigma Q_0 P_0 / \Sigma Q_0 P_t)$$

This cannot be reduced any further, and is obviously not an index measure. To produce a Paasche index, one needs first to divide the expression above by the base year price and quantity relativities $\Sigma Q_0 P_0$, admittedly, a simple enough task. The importance of this seemingly petty argument is this - for national accounting purposes, the desired output (volume) indices should be of the Laspeyres form (Rushbrook, 1978, p105) but many of the price indices available are only of the Laspeyres form.¹⁴ When national accounting statisticians encounter this kind of obstacle in practice at the industry level, which they must do in any instance where an industry (major group) price index of the Laspeyres type is built up from the separate indices of the sub-groups in the industry, they simply treat the index as though it were a Paasche. If Rushbrook (1978) is to be believed, this is usually of little or no consequence.¹⁵ He observes that:

14 To the best of my knowledge, there are no published Paasche indices for any of the industries that are the concern of this study. The CSS may well have estimated them, but if they have, they have not made this public knowledge. All of the published PPIs - the indices used by the CSS to deflate the current price estimates of output - are of the Laspeyres type.

15 It appears from the simulations used to generate the results in Chapter 2-5 that, under many circumstances, it probably does not make a great deal of difference whether Paasche or Laspeyres price indices are used - precisely the finding reported by Rushbrook (1978).

"In order to yield base-weighted volume series, values should be divided by current-weighted (Paasche) price index numbers. In practice, the individual price index numbers used to deflate each identified category of expenditure may not be current-weighted because the necessary data are lacking. However, because the deflation of current values is carried out in as great a detail as practicable the correct weighting among individual categories is achieved. The incorrect weighting *within* categories (because the deflators used are not current-weighted) is one of the imperfections of the constant price estimates." (1978, p106) (Emphasis in original)

Referring then to an examination of the importance of this imperfection for the rebasing of the British national accounts to the year 1970, he comments that:

"...it was concluded that 'given that most of the calculations are carried out at a considerable degree of disaggregation, the use of base-weighted deflators is probably not in practice a serious deficiency in the method of estimation, and it is unlikely to be any more serious than other deficiencies in the data on price movements.'" (1978, p106)¹⁶

The point of the digression above is this - I attempted to use an estimate of the value of Real Output (Total) obtained by deflating total gross output by the aggregate PPI as a check on the value of Real Output (Sum of Components). The CSS tried in turn to cast doubts on the validity of the enterprise by pointing out that the resulting volume index was of the Paasche, rather than the Laspeyres form. In doing so, however, they conveniently overlooked the fact that many (or most) of the price indices they compute are of the Laspeyres form - a common problem internationally.

It is not obvious from their vague expression of concern with the form of the index (rather than its content) that people at the CSS are familiar with the debate about when Laspeyres indices can be or are substituted for Paasche or *vice-versa*. Taken together with views expressed in other interactions, it would seem that there is a good case for unpacking the literature on the use of Paasche and Laspeyres indices. The case is strengthened somewhat by the fact that some of the relevant material is not readily available.

My last communication with the CSS on this matter, the letter of 30 July 1993 showing that the discrepancy between Real Output (Sum of Components) and Real Output (Total) could not arise from the use of the published deflators (PPIs), produced, as noted in Chapter 2-3, the verbal response that the difference arose because the petroleum price index had been used to deflate the output of the industry 'Other Chemicals'. My reply to that, also verbal, was to note that a significant difference such as that observed between the (a) and (b) estimates in Table 2-

16 Note that this is no longer automatically so when using chain Laspeyres or Paasche indices. Under certain circumstances, one will perform better than the other, whilst under other circumstances, neither should be used. See UN, 1993, pp389-390.

Rushbrook also sounds a warning which may be of consequence in the South African case. He cautions that: "It is possible, however, that the use of base-weighted deflators may have been a more serious deficiency in the post-1970 era of faster price increases." (1978, p106)

3.3 can only arise if the industry PPI is incorrect, the aggregate PPI is incorrect, or both are incorrect. The response of the CSS to this was non-committal.¹⁷ Under the circumstances, an acknowledgement that what I was claiming was at least worth investigating would seem to have been more appropriate.

Loss of additivity caused by changes in price and quantity relativities is another matter that I have raised in the past with the CSS.¹⁸ 'Breakdowns', the term applied to the phenomenon by the UN, are often only revealed when rebasing, which normally is carried out every five years, takes place. When relative movements of price and quantity are large enough, they merit the description 'structural change', and they usually cause problems for national accounting statisticians. The effect, which becomes visible when rebasing takes place, is most noticeable on output measures.¹⁹ It takes the form of a loss of continuity in one or other of the features (dimensions or properties) of the measures. The more extreme the change, the greater the discontinuity. A choice has to be made as to which of the features concerned will be sacrificed.

Consider, for example, the constant price output estimates produced by deflating gross output by Paasche price indices (the output magnitudes which, when divided by the appropriate base year (fixed) price and quantity relativities, yield Laspeyres volume indices). These have the property, during the currency of any particular base year, that their sum always equals 100 percent of the total of whatever aggregate it is they are part of (the property of additivity referred to above). When, after a period in which structural change has occurred, rebasing²⁰ takes place, and a decision to maintain previously observed volume growth rates is taken, the additivity of the components of aggregates is lost.

17 Telephone conversations with Mr Roelf van Tonder and Dr Treurnicht du Toit, 2 August 1993, and brief conversations with Dr du Toit and Mr John Lynch on 3 August 1993 at the CSS offices in Pretoria.

18 See, for example, the extended discussion in Meth (1992) of the impact of gold price changes on the national accounts. The only response that this elicited was the reflex "...the accounts are constructed according to international standards...".

19 Price indices will also become inaccurate over time as the composition of output (sales) changes. By inaccurate, one means that the base year relativities, which are retained throughout the currency of a particular series (ie, from one rebasing to the next), differ significantly from actual relativities in subsequent years. If the periods between the regular reweighting of the indices are not short enough to keep up with these changes, then the aggregate index could presumably become less reliable than the sub-indices, many of which would not be affected so much by such changes. This would have implications for the use of PPIs as deflators of Real Output (Total) as opposed to Real Output (Sum of Components) over the long term. It is assumed that over the relatively short period at issue here (six years), this problem is not too severe.

20 The price and volume relativities of a particular year - a base year - are used to generate price and volume indices. Rebasing, as will be seen in the next chapter, entails using the price and quantity relativities of some subsequent year - often five years later than the existing base. The change from one set of relativities to the other is called rebasing. If rebasing takes place frequently enough, it is tantamount to producing a linked series.

The problem to which this gives rise has no correct solution - and the implications for the important task of constructing constant price estimates can be serious indeed.²¹ An example is the unfortunate compromise that had to be adopted at the rebasing of the South African national accounts to the year 1980 (Meth, 1992, pp22ff)²² in the wake of the fluctuations experienced during the 1970/80s gold price roller coaster.²³

Ways of handling the breakdown in the accounts to which structural change gives rise are considered at length in the latest SNA (UN, 1993, pp389-390). In that document, the needs of users, especially those who may be unfamiliar with the waywardness of volume indices and constant price output estimates, are treated as being of paramount importance. Generally speaking, the recommendation is that the burden of adjustment be allowed to fall on the property of additivity.²⁴ The contrast between the careful treatment of this problem in the UN work and the bald announcement of loss of additivity by the CSS in the statistical news release which accompanied the first publication 1980-based output estimates could not be more striking. The full extent of guidance offered by the national accounting statisticians to users on the occasion of the publication of these (non-additive) results was a statement to the effect that:

"Due to the composition (weights) of the industrial major divisions in the different base years the individually rebased industrial major divisions for the period prior to 1978, however, no longer add up to the total." (SNR P12.1, 21 January 1986)²⁵

Unless the national accounting statisticians take users into their confidence by acknowledging that really serious problems arise under circumstances such as those experienced in South Africa, the compliance with conventions laid down by the United Nations for the production of the accounts is reduced to empty formalism.

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21. It was shown that the Laspeyres index is quite sensitive to large price changes in Meth, (1992, Table 2, p19), a characteristic revealed when rebasing takes place. A comparison between Panels 2.3, 2.5 and 2.6 showed why, under these conditions, the selection of base year price and quantity relativities becomes critical. Choice of one as opposed to another base year did not merely cause the magnitude of the rate of growth to change - it actually caused the sign of the direction of growth to change. In practice, these sign reversals would not occur because the impact of the changes would be absorbed by sacrificing additivity, thus enabling previously measured volume changes to be maintained.
22. The inability to respond flexibly to the demands of an unstable set of conditions makes the South African national accounts all the poorer - when I criticised the authorities for clinging unthinkingly to what obviously are nonsensical methods of estimating the output of the gold mining industry (Meth, 1983), the response was similar to that made above - these are the internationally recommended practices. (NPI, 1983; Swanepoel and van Dyk, 1983).
23. See Chapter 3 of Moll's PhD dissertation (1990), and his 1992 article in the *Review of Income and Wealth* for a discussion of the ways in which the output of the gold mining industry should be treated.
24. For the construction of price and volume measures, the SNA recommends that:
 "...annual chain indices should be used where possible, although fixed base indices may also be used when the volume measures for components and aggregates have to be additively consistent for purposes of economic analysis and modelling." (SNA, 1993, para 1.17, p4)
25. This interesting characteristic of the national accounts is not referred by the best available guide (Mohr *et al*, 1991) nor is it mentioned in the latest version of the national accounts (1991).

It is a little difficult to illustrate the breakdown caused by rapidly changing gold prices because the enforced choice between sacrificing additivity or sectoral growth rates makes it impossible to perform the necessary calculations. Nonetheless, one can get some indication of the extent of the problem by showing the absurd results caused by the need to maintain growth rates of output. In Table 2-4.1 below, the results of the (illegitimate) operation of calculating proportional contributions of the mining sector to total GDP using the estimates of three different base years as well as the current price estimates are presented.

The calculation is illegitimate because it is known that, at least for the 1980-based results, the components of the constant price GDP estimates sum to more than 100 per cent. The extent of non-additivity is such that the 1980-based sum of the sectoral contributions to GDP in 1970 was approximately 6 per cent greater than the estimate for GDP as a whole. In 1975, by contrast, the sum of components was almost exactly 100 per cent.²⁶ One can obtain a peep at the extent of non-additivity by comparing current- and constant-price estimates in any year and subtracting from the difference between them, the known extent by which the sum of the components of GDP exceeds the total.

Constant price contributions to output are obtained by taking base year contributions and extrapolating these backward at the observed growth rates of physical volumes of mining output. The proportions expressed in this table are the constant price output estimates so obtained, divided by the constant price estimates for total output. By comparing the current price contribution in Column 1 with its constant price base year counterpart (1970 in Column 2; 1975 in Column 3 and 1980 in Column 4) one sees the equality of current and constant price estimates in a base year (give or take a few tenths of a decimal point). Consider for a moment, the year 1970. The current price contribution of 10,0 percent contrasts rather oddly with the 1975-based figure of 18,3 percent or even more so with the 1980-based estimate of 35,4 percent. Even if the mining sector output contribution were reduced by the difference between the sum of the components and the total, the contribution of the sector would still exceed the current price estimate by nearly 20 percentage points - clearly an untenable result. The 1980-based contribution for the year 1975 is 23,7 per cent, whereas the current price estimate is 12,3 - yet in 1975, as noted above, there was the problem of non-additivity. Clearly, these are not figures with which the unwary ought to play.

For the 1975-based figures, the CSS has apparently managed the enviable feat of maintaining additivity. A check on the sum of the constant price estimates for two years, 1965 and 1970,

26 Estimated from the constant price GDP figures in *South African Statistics 1988*, p21.6.

reveals them to add up to precisely 100 per cent,²⁷ and this despite the fact, as is clear from Table 2-4.1 above, that current price and constant price contributions differ extensively in those two years. It is not entirely clear how this was done, but it appears to have something to do with the CSS discovering that the growth rate in mining had previously been incorrectly estimated.

Table 2-4.1 Mining Contribution to GDP, 1960-84 (%)

Year	Current prices	Constant 1970 prices	Constant 1975 prices	Constant 1980 prices
1960	20,5	19,4	19,1	-
1961	13,4	11,2	17,8	-
1962	13,2	11,4	18,2	-
1963	12,6	11,2	17,9	-
1964	12,8	11,3	18,0	-
1965	12,5	11,1	21,1	-
1966	12,3	11,3	20,9	-
1967	11,3	10,5	19,4	-
1968	11,0	10,4	19,2	-
1969	11,0	10,1	18,6	-
1970	10,0	10,1	18,3	35,4
1971	8,8	9,3	16,9	32,7
1972	10,1	8,6	15,6	30,1
1973	12,0	8,6	14,9	28,7
1974	13,3	7,9	13,0	25,1
1975	12,3	7,5	12,3	23,7
1976	11,9	7,6	12,3	23,6
1977	12,4	8,2	12,7	24,5
1978	15,2	8,1	12,5	24,0
1979	18,5	8,2	12,4	23,7
1980	21,7	-	11,5	21,9
1981	23,8	-	10,9	20,7
1982	14,2	-	10,8	20,8
1983	14,6	-	11,0	21,4
1984	13,5	-	10,7	20,8
	1	2	3	4

Note: Slightly different results may be obtained by using CSS data of different vintages. These small differences do not concern us here.

Sources: Estimated from *South African Statistics 1980* and *1982*, pp21.6 and 21.7 in each case, and Statistical News Release P12.1 of 16 July 1985, 8 October 1985 and 21 January 1986.

With 1960 set equal to 100 for the 1970-, the 1975-, and the 1980-based output estimates, the latter two series are identical all the way to 1978, where the provisional figures for 1979 and 1980 for the 1975-based estimates are replaced by slightly lower figures for the 1980-based series. The 1970-based series is entirely different - the first five figures are the same (1960-

27 Estimated from the constant price GDP figures in *South African Statistics 1982*, p21.6.

64), but that is all. Whereas the two later series put the level of output in 1970 as 183,2 and that in 1978 as 158,7, the corresponding 1970-based figures are 160,9 and 168,0.²⁸

What is on display in Table 2-4.1 is simply one of the more spectacular instances of the inability of national accounting procedures to cope with variability of more than some minimal amount. This stems not from the limited capacity of the compilers of the national accounting manuals, but from the inability of economic theory to solve the problem of 'value', which in this, as in so many other instances, presents itself as an index number problem. At this point, the indulgent smile reserved by conventional economists for Marxists and their quaint pre-occupation with value ought to be becoming a trifle forced. A tautological trap into which the absent inner core of value theory leads the unwary operates here - in national accounting terms, the value of output in monetary terms in a base year is set equal to factor cost (factor incomes, incorrectly, in the South African case). By definition, this is equal to the value of gross output (volume of output multiplied by price per unit) minus the value of intermediate inputs (volume of inputs multiplied by price per unit).²⁹

Suppose now that an increase in the unit price of output takes place. This could happen for a number of reasons. One of them, an upward shift in the demand schedule for a good that is completely inelastically supplied, approximates to what happened in the case of gold mining in South Africa in the late 1970s. For arguments' sake, gold may be treated as though it were in perfectly inelastic supply in the short run. Production costs³⁰ may also be treated as fixed in the short run, so an upward shift in the demand function translates directly into an increase in real income (a pure economic rent) for the factors of production employed in that industry.

National accounts rely for consistency on there not being either too many commodities within an economy for which this kind of relationship holds true, or alternatively, any single

28 Estimated from the national accounting results in *South African Statistics 1980, 1982 and 1988*, p21.6 or 21.7.

29 The way in which the NPI and SARB fall into it is described in Appendix 2-1. In brief, I cite an NPI claim to the effect that:

"[Meth] does not differentiate between *income* and *production*. In a national accounts context *production* represents the value added, i.e., the value of output minus the value of intermediate consumption. Income represents the income generated (mainly salaries, wages, interest and profits) in the production process." (NPI, 1983)

I then point out that, unfortunately, economics (and the CSS and SARB) can only define value added (gross output minus the value of intermediate consumption) by stating that it comprises mainly "salaries and wages, overheads and profits", ie, factor incomes. The definition is thus pure tautology - production is represented by factor income which equals factor income!

30 If an income bonanza is treated by both capital and labour as a windfall gain and paid out as a special bonus, then underlying factor costs are unaffected. When the price increase is expected to be of long duration and the factors of production lay (competing) claims to the additional income, and when factor markets are not competitive, costs will rise. The net result will still, however, appear as an increase in factor incomes.

commodities which can distort results significantly. Many economies do have this property, but, as is shown in Appendix 2-1, there are important exceptions. Under normal circumstances, the predominance of the usual inverse relationship between price and quantity demanded (ie, the absence of price/income effects like that described above), serves to protect the national accounts against serious additivity problems like that depicted in Table 2-4.1.³¹ The consequences of relaxing the additivity criterion may thus be trivial under most circumstances. So too, may be the effect of changing price and quantity relativities on attempts to chart the process of structural change in an economy. When, however, the impact of events like changes in the gold price cause breakdowns in national accounting procedures is as serious as that illustrated above, the problem can no longer be brushed aside by appealing to authority. Even though the rebasing to the year 1980 was done under guidance from the UN and OECD, the method chosen by the South African national accounting statisticians to deal with the problem caused by gold was singularly inadequate.

On index construction

One of the central contentions of this study is that the CSS is unable to deal constructively with criticisms of the statistics it produces. Users are treated without the care they deserve because the defensiveness of the institution prevents open acknowledgement of error - sometimes the result of incompetence, but often also the consequence of the extreme difficulty of that which is being attempted.³² That leaves the task of tracking down certain errors to their origins to those with sufficient energy. Even when this has been done, however, the unwillingness to acknowledge the errors publicly still leaves those who undertake such critiques with the task of making available a full account of the proceedings. The manufacturing sector output estimates were riddled with errors. The reconstructed estimates still appear to contain serious flaws. Both the now apparently eradicated and the remaining flaws need to be exposed. It is for this reason that the analysis that follows is conducted.

Before examining these issues and the simulations used to address the various strands of the argument, a word on some of the technicalities of index construction is necessary. In what

31 If the demand for gold had been elastic at the time the rapid price increases occurred, the quantity sold would have fallen more than proportionately and there would not have been the massive increase in factor incomes recorded in the late-1970s and especially in 1980. In that case, the weighting of the gold mining industry in 1980 (see Table 2-2.2) would not have been so large. The additivity problems would then have been much less severe.

32 Technically, the problems to be tackled are not all insoluble. Some inherently difficult tasks, eg, measuring output volumes, are made more difficult still by the fact that the surveys required to measure volumes are expensive to design and administer.

follows, attention will be focussed on certain aspects of index construction that are not dealt with in Chapter XVI of the latest SNA (UN, 1993).³³

Indices of different types or forms have different properties, and there are well-defined theoretical relationships between certain of the indices. Chapter XVI of the latest SNA covers some aspects of index number formulation, including reference to Fisher's Ideal Index and the criteria (tests) which characterise such an index (UN, 1993, pp383ff). This discussion is less comprehensive than the treatment it is given in Hansen and Lucas (1984, pp26-32). Given the limited aim of this study (to evaluate critically the performance of the Laspeyres indices conventionally used to measure output in constant prices), even the UN material goes well beyond what is required. The primary reason for including this discussion is because of the treatment it offers of the reasons for preferring a 'symmetric index', ie, "...one which assigns equal weight to the two situations being compared..." (p12). Fisher's Ideal Index is the geometric mean of the Laspeyres and Paasche indices:

$$F_p = (L_p \cdot P_p)^{1/2}$$

and

$$F_q = (L_q \cdot P_q)^{1/2}$$

where F , L and P stand for Fisher, Laspeyres and Paasche respectively, and p and q for price or volume (quantity) indices. The interest here is in the property of a Fisher index known as 'time reversal'. Time reversal requires that an index for year t based on year o should be the reciprocal of the index for o based on t (UN, 1993, p384). Or, as Hansen and Lucas put it, "...time reversal requires that time reversed indices be reciprocal, i.e, $P_{ot} \cdot P_{to} = 1...$ " (1984, p27) - in the nomenclature they use, P here stands for a price index. By definition, Paasche and Laspeyres indices fail this test - the extent to which they do provides an indication of the severity of the structural change between years o and t . A crude approximation to this test is performed in Appendix 2-6 to see to what extent, if any, the Laspeyres indices used show signs of gross failure of this test. Since significant changes in price and volume relativities are at the heart of the problems caused by Sasol, some reference to the theoretical reasons for the inadequacy of Laspeyres indices would seem to be of importance.

33 Comprehensive coverage in Chapter XVI of the latest SNA (UN, 1993) of several areas either directly or peripherally related to the material to be dealt with in this and the following chapters, is provided. Summarising this seemed somewhat pointless - the relevant sections are reproduced as Appendix 2-4. Rebasings - changing price and volume relativities in an index to reflect those of the new base year, and linking - splicing together series with different price and volume relativities, for example, are analysed in depth and illustrated by way of some simple tables (pp386ff).

Unlike Hansen and Lucas (1984), the compilers of latest SNA appear not to have serious misgivings about the use of Laspeyres and Paasche indices. They argue that given certain plausible assumptions about consumer behaviour, for example, Laspeyres cost of living indices constitute an upper,³⁴ and Paasche volume indices a lower bound to a region within which the true underlying theoretic cost of living index may be said to lie (UN, 1993, p383). In the final analysis, the question of the adequacy of indices of this type has an important empirical component. Under certain conditions, the easily-estimated and comprehended Laspeyres indices may perform well enough for the purpose at hand - in others they will not - the results cited above from Hansen and Lucas being a case in point.³⁵ The choice of index is, or should be, anything but an arbitrary process. The availability of data, however, may often dictate the type of index it is feasible to calculate.

For many practical purposes, the consequences of making less than optimal choices may not be too serious. Anyone who dabbles, for example, in the highly esoteric question of what indices are appropriate for use in different circumstances, stumbles sooner or later against the fact that one of the disadvantages of using Paasche volume index numbers is the fact that:

"...the apparent change in volume between [any two succeeding years] is in fact an amalgamation of real changes in volume and the effects of changes in price structure between the two years." (Rushbrook, 1978, p105).

Strictly speaking, valid comparisons can thus not be made between any year t , and a succeeding or preceding year $(t+1)$ or $(t-1)$. Instead, they have to be made between the base year 0 and the year in question, t , $(t+1)$ or $(t-1)$. That this is so may be deduced quite readily from the expression for a Paasche volume index - $\Sigma Q_t P_t / \Sigma Q_0 P_t$. Although the Paasche volume indices suffer from this drawback, it may well be that under 'normal' circumstances the changes in the price and quantity relativities are not large enough to cause the Paasche indices to differ substantially from their Laspeyres counterparts. Certainly, it would be surprising to find that the differences were greater than some of the CSS errors reported in this study.

Apart from the fact of being sparing in their data requirements, Laspeyres volume indices have certain useful properties, amongst them that of maintaining additivity during the currency of any particular base year. Laspeyres volume indices (and Paasche price indices) are used

34 Further examination of the reasons why Laspeyres indices constitute an upper bound may be found in Braithwait (1980). The effect of upward bias in Laspeyres price indices on estimates of the real wage has been looked at by Pencavel (1977).

35 The somewhat weak 'this topic requires further research' is clearly appropriate here. It would be a great advance if users could be made aware of the conditions under which Laspeyres indices become suspect.

extensively in national accounting - it will be useful to spell out some of the relations between them, and between volume indices in their individual and aggregate forms.

Paragraph 4.45 of the SNA quoted above states that:

Laspeyres and Paasche index numbers are symmetrical: a price index of one form multiplied by a quantity index of the other form is equal to the relative change in value.

Expressed mathematically, a Laspeyres price index multiplied by a Paasche volume index yields the following result:

$$(\Sigma Q_0 P_t / \Sigma Q_0 P_0)(\Sigma Q_t P_t / \Sigma Q_t P_0) = \Sigma Q_t P_t / \Sigma Q_0 P_0$$

And a Laspeyres volume index multiplied by a Paasche price index yields the same:

$$(\Sigma Q_t P_0 / \Sigma Q_0 P_0)(\Sigma Q_t P_t / \Sigma Q_t P_0) = \Sigma Q_t P_t / \Sigma Q_0 P_0$$

Assume for the moment that we are concerned only with the problem of measuring the level of aggregate output, given some mix of individual industry outputs. In other words, assume that we are unconcerned with the intermediate inputs that went into the production of the outputs. This simplifies the algebra and also circumvents the problems associated with the valuation of net output ie, the set of problems examined in Chapter 2-7. This assumption allows some of the complexity to be stripped away without affecting the fundamental reasoning. Incidentally, this is the assumption used by national accounting statisticians when they calculate indirect volume measures from estimates of gross output. Under these conditions, total value (total revenue in current prices - ie, volume (quantity) of output multiplied by price per unit) divided by a price index of the Paasche form would yield the value of output in constant prices - thus for a single industry:

$$(Q_t P_t) / (Q_t P_t / Q_t P_0) = (Q_t P_t)(Q_t P_0 / Q_t P_t) = Q_t P_0$$

If this operation is repeated for each industry and the results are summed over n industries, the total is given by $\Sigma Q_t P_0$. Dividing this by the sum of total revenue in each industry in the base year 0 , yields a Laspeyres volume index - thus:

36 For a single industry producing one commodity, this is tantamount to dividing total revenue by the ratio of prices, ie,

$$(Q_t P_t)(P_0 / P_t) = Q_t P_0$$

$$\Sigma Q_t P_o / \Sigma Q_o P_o$$

This method of estimating the index of total output is similar to what has been described above as the 'Sum of Components' approach to estimating the value of Real Output, except that the results are given in index form, rather than as constant price estimates. Clearly, one could also take the value of total output in current prices $\Sigma Q_t P_t$ and divide it by an aggregate price index to obtain the measure described above as the value of Real Output (Total). When a Paasche price index is used in this operation, a Laspeyres volume index may be estimated by dividing the result of the deflation process by the value of total revenue in the base year, thus:

$$(\Sigma Q_t P_t) / (\Sigma Q_t P_t / \Sigma Q_t P_o) = \Sigma Q_t P_o$$

which, when divided by $\Sigma Q_o P_o$ gives

$$\Sigma Q_t P_o / \Sigma Q_o P_o$$

It is worth noting that in practice, information on the value of output in current prices of each industry $Q_t P_t$ is often not obtained from direct measurement of the separate magnitudes of P and Q . In general, prices are relatively easily measured, but volumes (the central question of this study!) are not. At least as far as the manufacturing sector is concerned, even where Q is measured directly, as has been the case in South Africa with the PVMP series, such volume measures would appear to enter only indirectly, and often in much-modified form into the construction of the constant price output series. The indirect measures obtained from the census estimates of gross output deflated by the separately collected price information are used to keep the PVMP series on track.

If estimates of Q in the form of a set of individual industry output indices (Physical Volume of Production indices) (PVPs, or PVMPs in the case of the manufacturing sector) are to hand, then a different approach may be used to estimate the index of total production. Call this the 'PVP' method. In the absence of measurement errors, the end-results are identical to those obtained above using the 'Sum of Components' method, and they are also equal to the results obtained using the 'Total' method when a Paasche price index is used. The PVPs (Laspeyres output indices) have each to be weighted by the proportional contribution to total output in the base year to obtain an estimate of the contribution to total output in year t - thus:

$$(Q_t P_o / Q_o P_o) (Q_o P_o / \Sigma Q_o P_o) = Q_t P_o / \Sigma Q_o P_o$$

Thereafter, the individual contributions may be summed:

$$Q^1_t P^1_o / \Sigma Q_o P_o + Q^2_t P^2_o / \Sigma Q_o P_o + \dots = (Q^1_t P^1_o + Q^2_t P^2_o + \dots) / \Sigma Q_o P_o$$

$$= \Sigma Q_t P_o / \Sigma Q_o P_o$$

It is perhaps worth noting that the expressions used above to represent individual industry output level $Q_t P_o / Q_o P_o$, total output $\Sigma Q_t P_o / \Sigma Q_o P_o$ and industry weight or proportional contribution $Q_o P_o / \Sigma Q_o P_o$, are all dimensionless. The pure numbers that represent industry output levels correspond to the pure numbers generated by the surveys conducted to measure PVMPs. The weighted sums of these individual industry indices are the aggregate output levels. Weights, derived from manufacturing censuses, are obviously dimensionless, as are the aggregates themselves.

These simple expressions and manipulations form the basis of the argument to be offered in Chapters 2-5 (and 2-6) of the study. In Chapter 2-5, two sets of simulations will be performed. In the first, changes over a period in a hypothetical economy in price and quantity relativities are moderate. In the second set of simulations, the quantity changes are severe. Before turning to Chapter 2-5 and the simulations proper, a brief note on a much older simulation exercise is in order.

An early simulation from the SNA

In a sense, there is a small amount of wheel re-inventing in the simulations performed in the next chapter of the study - a not dissimilar exercise in the SNA predates them by more than 30 years. There are, however, several good reasons for executing these simulations - amongst them, the following:

The 1968 SNA pays insufficient attention to the apparently frequently encountered problem of the need to choose between the maintenance of existing growth rates and the sacrifice of additivity when rebasing takes place. This question is addressed in the most recent SNA (UN, 1993), but that document is not yet widely available.

Although it is a simple matter to show algebraically that when every price index in a particular deflation exercise is a (correctly-measured) Paasche index, that Real Output (Total) must equal Real Output (Sum of Components), this point is not stressed sufficiently in the 1968 SNA.

The 1968 SNA was not concerned to demonstrate the impact on the accounts of errors of various magnitudes in the deflation process.

Most importantly, the simulations serve as the platform for the investigation conducted below into the impact of changing net/gross output ratios on estimates of deflated output.

What is heartening about the SNA simulations is the support they lend to the argument advanced in this study (and in my letter to the CSS dated 13 July 1992) that for many purposes, it does not matter greatly whether one uses a Paasche or Laspeyres index as a deflator (or as a measure of output). The numerical example used in Chapter IV of the SNA makes essentially the same point. Despite the disclaimer in para 4.85 of the SNA to the effect that "[A]n illustrative example can hardly hold much substantive interest...", the tables given immediately below that statement in the SNA are of considerable interest.³⁷ In the first place, they disclose the aforementioned minimal differences between output indices obtained using Paasche and Laspeyres deflators for a variety of industries. Coincidentally, the manufacturing sector output growth over the period considered in one of the SNA tables³⁸ is similar in magnitude to that experienced in the South African economy over the period in dispute, 1979-85. As may be seen by reference to the SNA, the difference between output estimated by the two different methods amounts to a mere two-tenths of a percentage point. Agriculture and mining are satisfyingly badly behaved, exactly as one would have expected them to be (UN, 1968, pp64-65).³⁹

Perhaps even more interesting from the point of view of the argument about the need to make compromises because of the impossibility of performing the calculations 'correctly', is the set of results in SNA Table 4.6 (UN, 1968, p64). Here, one may see quite plainly that the selection of different bases of value has almost as large an effect on the terminal value of output as does a choice between Paasche and Laspeyres indices. More interesting still is the difference between the single- and double-deflated results. It is agreed on all sides that the latter are more desirable than the former (para 4.41), but there is also general agreement that with the exception of agriculture, double deflation is too sensitive to small variations in the basic data to be acceptable as a method of estimating output volumes. The price paid for this compromise is clearly visible. Even the most cursory examination of the relevant tables in the SNA, not to mention all that has been said above that attempts by the CSS to take refuge in so-called 'internationally approved methods' have absolutely no credibility whatsoever.

37 SNA Tables 4.5 and 4.6. See (UN, 1968, p64).

38 SNA Table 4.5. See (UN, 1968, p64).

39 Although they do not misbehave in Table 4.7. See (UN, 1968, p65).

Chapter 2-5

Using simulations to set rules and detect errors

Introduction

Simulations are popular with economists, and with good reason. Not only do they enable the workings of complex inter-related variables to be exposed, they also permit easy explorations of the 'what-if' type. As the compilers of the SNA observed, there is a need to be aware of the limitations of simulation exercises. As long, however, as these limits are recognised, valuable insights may be gained. This chapter uses a pair of simulations to bring out some of the characteristics of national accounting magnitudes created using Paasche and Laspeyres indices. The knowledge gained is then used to confront the (early) explanations offered by the CSS for the errors in the output estimates.

In the first part of this chapter, a simulation is performed in which modest change is experienced in a hypothetical economy. The simulation is used to show that for Laspeyres-type volume indices, Real Output (Sum of Components) and Real Output (Total), correctly estimated, do not differ at all when Paasche price indices are used, and should not differ significantly even when Laspeyres price indices have to be substituted for them. In other words, for all practical purposes, it does not matter greatly whether one uses Paasche or Laspeyres price indices when calculating the values of output in constant prices. By contrast, a relatively small error in the magnitude of the PPI of an industry roughly the size of the industry 'Other Chemicals' is shown to produce an error in the aggregate output estimates similar to that revealed in Table 2-3.3 above.

Another simulation is used in the second part to show that despite major change, the exact (when Paasche price indices are used) or approximate (when Laspeyres price indices are applied) equivalences referred to above hold during the currency of any particular base year. Rebasings and structural change combined can, however, have unfortunate effects on output growth estimates. Some indication of the impact of this combination is given by the second simulation. The simulations appear in the appendices numbered 2-4a and 2-4b respectively.

The third and final part of the chapter tries to thread its way through the explanations offered by the CSS for the observed discrepancies - rejecting most of them in the process. A claim by

the CSS that errors which were uncovered in the census returns could have contributed to the observed errors is examined. It is shown to be an unlikely contender.

Output indices and modest structural change

In the first stage of the examination of the hypothetical economy below, a demonstration of the fact that the 'Sum of Components' and 'Total' approaches to estimating output indices yield identical results when Paasche price indices are used, will be undertaken. It will then be shown that the use of Laspeyres price indices introduces a small but acceptable error into the calculations, in other words, for a given base year, under 'normal' conditions, Laspeyres price indices may be substituted for Paasche with little or no discernible effect on the end-results.¹ After this, it will be shown that a change in base-year weights causes small but acceptable deviations in growth rates estimated on the two base years. A parallel set of calculations performed with the 'correct' data used to generate these conclusions shows that the use of an 'incorrect' deflator in a single industry causes the equality or additivity of the deflated sums of the components to be lost.

A set of fairly simple assumptions has been used to set up a model of a hypothetical economy. It consists of three sectors (major divisions), a , b and c , each of which has three component industries (major groups). Each of these industries produces a homogeneous product, and by assuming that the ratio between net and gross output does not change in any of the industries over the period under consideration, intermediate inputs can be ignored. In other words, it is gross output that is being measured in an annual survey that collects data on total revenue ($\sum Q_t \cdot P_t$) and prices (P_t). Output volumes may be estimated (indirectly) from these data. For ease of setting up the model, I simply assumed a set of volumes, but the type of survey envisaged (similar to the British MSI) would yield values but not volumes. Prices data would be collected in separate surveys. The importance of assuming that it is values, and not volumes that are

1 Terence Moll (*pers. comm.*) has suggested that many hundreds of simulations would be necessary before one could make these claims with confidence. Some care may need therefore to be exercised in accepting this conclusion - detailed empirical analysis, rather than a few simulations is probably required to establish the proposition with confidence. The literature on this point does not appear to be abundant. Hansen and Lucas (1984, p33) show how Paasche and Laspeyres indices differ from other more complex indices, but they do not discuss the characteristics of Paasche or Laspeyres indices at issue here in any detail. They do refer, however, to past empirical work on a related topic, the tendency of direct indices to drift from their annually chain-linked counterparts (p28n). The latest SNA (UN, 1993, p383) looks at Laspeyres and Paasche indices in relation to the underlying theoretic (price) indices they approximate. The Hansen and Lucas footnote cited above ends with a quotation from one of the works they examine to the effect that "Empirical evidence is required..." The same could be said in the South African case. It would not be unreasonable, however, to insist that any claim that the use of Laspeyres instead of Paasche leads to unacceptably high error be supported by evidence.

measured in these hypothetical surveys will become clear when the 'incorrect' price simulation is interpreted.

All of the real world difficulties which persuade the national accounting statisticians to weight the price indices by the distribution of the value of sales are assumed away - the price indices in this economy are weighted by the distribution of gross output. Relative price changes occur within and between sectors, as they do in a real economy, but these changes are nowhere near as severe as the quite exceptional gold price changes experienced in the South African economy. The simulation runs over four consecutive time periods, say, of one year each.

The results pertaining to the first three propositions to be tested are presented in the body of the text (in Table 2-5.1 below). Those concerned with the use of the 'incorrect' price index are given in Table 2-5.2.

The full set of the assumed values and the results of the operations performed upon them are to be found in Appendix 2-4a.² Table 2-5.1 contains four panels - the first three being sets of output estimates in one form or another. The last panel contains estimates of sectoral weights - proportional contributions to total revenue (price multiplied by quantity). It is the changes in the magnitude of this variable that induce the 'instability' (breakdowns) in the output indices when a change to a new base year takes place. As will be seen below, when structural change is moderate, there is little instability in the accounts, but when major change occurs, rebasing is attended by serious problems. The number appearing in the column headed 'Row' in Table 2-5.1 refers to the row in the appendix (and the spreadsheet) from which the information has been extracted.

The first two panels in the table contain estimates of the value of Real Output (Sum of Components) and Real Output (Total). The base year weights used for the calculations in the first two panels are those for the year o , ie, those in the first column in Panel 4. The Panel 1 results are obtained by using a Paasche price index on the current price sectoral values, and the Panel 2 figures from the use of Laspeyres indices. Separate estimates for each of the sectors are given in each case. As may be seen, the differences between the two sets of estimates are trivial.

2 The appendix is divided into two parts - the first deals with the moderate change scenario, and the second with the major changes. The spreadsheet files are on the diskette included with Volume II of the study.

Table 2-5.1 Values of output - moderate structural changes

1. Value of output using Paasche deflator - base year = o					
Year	o	t	t+1	t+2	Row
Sector a	297,0	326,8	387,9	447,5	211
Sector b	996,0	1046,4	1096,8	1243,0	216
Sector c	1761,0	1820,1	1998,9	2055,0	226
Sum of Components	3054,0	3193,3	3483,6	3745,5	232
Total	3054,0	3193,3	3483,6	3745,5	231
Index form - year o=100 (Sum of Components and Total)	100,0	104,6	114,1	122,6	234
2. Value of output using Laspeyres deflator - base year = o					
Year	o	t	t+1	t+2	Row
Sector a	297,0	326,4	387,7	444,0	241
Sector b	996,0	1046,6	1094,8	1244,3	246
Sector c	1761,0	1819,8	1995,5	2055,0	256
Sum of Components	3054,0	3192,9	3478,0	3743,4	262
Total	3054,0	3196,3	3487,2	3748,1	261
Index form - year o=100 (Total)	100,0	104,7	114,2	122,7	265
3. Indices of output - base year = t+2 (Set year 0 = 100)					
Year	o	t	t+1	t+2	Row
Paasche deflator	100,0	104,8	114,8	123,7	237
Laspeyres deflator	100,0	104,7	114,6	123,6	267
4. Weights - current price value relativities (%)					
Year	o	t	t+1	t+2	Row
Sector a	9,7	13,3	12,7	13,5	60
Sector b	32,6	28,4	29,7	29,6	61
Sector c	57,7	58,4	57,6	56,9	62

Source: Appendix 2-4a

As expected, when Paasche price indices ($\Sigma Q_t P_t / \Sigma Q_t P_o$) are used in the deflation process, the value of Real Output (Sum of Components) (Row 232) is identical to the value of Real Output (Total) (Row 231), ie, the sum of the values of the output of each sector, deflated by its own price index is equal to the value of total output deflated by the aggregate index. Expressing these values in index form with the year *o* set equal to 100 is done in the last row of Panel 1.

The second panel uses Laspeyres price indices ($\Sigma Q_o P_t / \Sigma Q_o P_o$) to deflate the current price values. Small divergences between Real Output (Sum of Components) and Real Output (Total) may be seen to result from this departure from strictly correct conduct. The Real Output (Total) values have been expressed in index form in Panel 2 - a comparison between these and

the corresponding indices in Panel 1 shows that the differences are so slight as hardly to be worthwhile estimating. The tiny discrepancy, caused by the rapid growth in Sector *a* and the consequent difference between current and base weights, amounts to 4.7 units (Rows 261 and 262), an error of slightly more than 0,1 percentage points. Clearly, estimating Real Output (Sum of Components) indices for the Laspeyres-deflated figures would merely belabour the point.

In the third panel, a short-cut method has been used to rebase the series to the year $t+2$.³ The 'Sum of Components' figures were used in the case of the Laspeyres-deflated estimates. At the end of the period under examination, the rebasing causes the output figure to rise by about 1 percentage point - an error of slightly under 5 per cent in the estimate of the absolute percentage by which output grew over the period. The reason for this divergence may be seen in Panel 4, which shows the changing contributions of the different sectors to total output.

The values underlying the performance of Sector *a* were selected to reveal the impact of a large relative change in an industry or sector with a relatively small weight in total production. The almost 40 percent increase in current weight causes the Paasche-deflated output index in Sector *a* to differ from its Laspeyres counterpart by only 1,2 percentage points (Rows 212 and 242), an error of only 0,8 percent. Differences in the Paasche- and Laspeyres-deflated estimates of the value of Real Output (caused almost entirely by the rapid change in Sector *a*), using the 'Sum of Components' method amount to only 2,1 units in almost 3 750 (Rows 232 and 262) - an indistinguishably small error at the aggregate level. Even the Laspeyres-deflated Real Output (Total) figure differs from the Paasche estimate by less than 3 units (Rows 261 and 231).

The information in the appendices is provided so that if required, the results can be replicated. As noted above, some sleight of hand was engaged in to produce the simulations, in that the basic volume and price data given in Rows 11 to 30 would not be measured directly. Rather, estimates of total revenue (typically as given in Rows 47 to 55) would be captured, as would relative prices such as those given in Rows 35 to 43. In real life this would mean that current weights (sectors - $\Sigma Q_{it} \cdot P_{it} / \Sigma Q_t \cdot P_t$ and industries - $Q_{it} \cdot P_{it} / \Sigma Q_t \cdot P_t$) would be estimated directly from the survey data, rather than being derived, as has been done in the appendix. It may be seen that the range of the price changes is quite substantial, as is that of the quantity changes. Comparing the Paasche and Laspeyres price indices for the individual sectors and for the economy as a whole (Rows 77 to 98), one notes that apart from the Sector *a* value in year

3 The full set of estimates, which generate almost identical results, are set out in the second simulation in Appendix 2-4a (Row 269 onwards).

$t+2$, the largest difference between any pair of indices is only four tenths of a percentage point on two observations with approximate values of 221 and 235 respectively.

Moving from this hypothetical world back to the reality of South African statistics, we encounter a residual difference of 4,6 percentage points between the PVMP and Real Gross Output (Total) in the benchmarked results for 1984/85. This was ascribed by the CSS to:

"...three factors, namely the implicit index formulae involved, the weighting bases used, and the adjustment of census data." (Letter dated 3 July 1992)

Real Gross Output (Total) exceeded Real Gross Output (Sum of Components) by about three percentage points, which in turn, exceeded the PVMP by about 1,6 points. Without access to all of the calculations performed by the CSS to obtain these figures, it is impossible to decompose these errors, and to assign this or that proportion to one cause or another. It may well be the case that the first two of the factors listed above explain some of the differences between the existing PVMPs and Real Output (Sum of Components), but it seems improbable that it will ever be possible to get to the bottom of this. The CSS, having conceded by virtue of the publication of the reconstructed estimates that the benchmarked PVMPs were seriously incorrect, is unlikely to supply the information necessary to do so. What these three factors appear not to explain is the difference between Real Output (Total) and Real Output (Sum of Components).

Up until the time of disclosure of the fact that an important reason for the difference between these two magnitudes (the (a) and (b) results in Table 2-3.3), was the use of the petroleum price as deflator for the industry 'Other Chemicals',⁴ no adequate explanation for this three percentage-point difference had been offered by the CSS. With the exception, that is, of the following rather odd argument made in the CSS letter of 3 July 1992. The relevant statement reads as follows:

"I furthermore disagree with the statement in your letter to the Statistics Council that the total volume index as well as the total price index will be more accurate than their sub-indices. While the totals are more reliable (in a statistical sense) since they are based on larger samples than the components they are not more correct in view of the fact that the volume and price trends for the components are not normally the same as those of the totals. Measured against the demand for subindices regarding both volume and price, the CSS cannot do away with these as you suggest."

Understanding how an index can be more reliable - in a statistical sense - than each individual member of a set of less-reliable indices, and can yet be less correct than the sum of these

4 Recall from the discussion of the results in Table 2-3.4 in Chapter 2-3 that the information about the use of the Petroleum PPI was provided by the CSS in August 1993 in response to a direct request to Dr du Toit, the head of the CSS.

indices is no simple matter.⁵ Manufacturing censuses are, or should be, the most accurate 'surveys' of activities in the sector that are available.⁶ The CSS concedes that the aggregate PPIs are more reliable than industry-level sub-indices. In the Annexure to the CSS letter (reproduced as Table 2-3.3 in Chapter 2-3 above) therefore, the 'best' estimate of the value of output in each year, and especially in 1978/79, should be those given in the first row labelled (a), and the best estimate of output in 1984/85 in 1985 prices (in index form), should be that given in the second of two rows labelled (a), namely 122,24. The difference between the PVMP and the deflated value of gross output (compared over exactly the same periods) would then be almost 10 (9,7) percentage points.

The results presented in Table 2-5.1 above suggest strongly that under conditions of moderate structural change in an economy, divergences in price and volume trends of the magnitude referred to by the CSS can make no significant difference to estimates of the real value of output, even when Laspeyres price indices are used to obtain these estimates. What is not shown in Table 2-5.1 is what would happen if a sizable error were made in the measurement of a deflator for a large industry like 'Other Chemicals', or, alternatively, if an incorrect deflator (say, for example, the deflator for Petroleum products) were applied to the current price estimates of the value of output of the industry, 'Other Chemicals'. The impact on the estimates of aggregate output depends on which of the errors is made. If a deflator is incorrectly estimated (ie, it does not measure 'true' movements in prices), but is incorporated correctly into the aggregate deflator, then, providing it is of the Paasche form, real gross output (sum of components) will be equal to real gross output (total). The fact that the value of the output of one of the sectors is not its 'true' value means, however, that the aggregate estimate cannot be a 'true' estimate either.⁷ If, on the other hand, the aggregate deflator is correctly constructed, but an incorrect deflator is applied to the output of one industry as described above, even if the deflators are of the Paasche type, real gross output (sum of components) cannot equal real gross output (total).

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- 5 It may be that statistical theory is able to settle the question of the relative reliabilities of indices prepared from sub-samples and that of an index constructed from all of the observations in the sample - I certainly cannot defend my intuition on theoretical grounds. Although it appears at first glance to be relatively straightforward, it becomes clear, upon very little investigation, that the issue is extremely complex. The literature on the topic is unfamiliar to me, but my colleague, Prof Holden, has referred me to an article that deals with a similar, and possibly even more complex problem (Basevi, 1970). This might form a starting point for anyone wishing to pursue the matter. The arguments here do not rely heavily on its resolution.
- 6 As will be seen below, however, a check of previous survey results revealed substantial errors in the 1982 and 1985 census estimates.
- 7 In other words, any (or all) of the relative prices in rows 35-43 of Appendix 2-4a could deviate from its (their) true value, but as long as the aggregate Paasche price index is correctly constructed, the equality of the sum of components and total measures of real output must hold.

A situation of the latter type is simulated in Appendix 2-4a and some of the results are presented in Table 2-5.2 below.

Table 2-5.2 Values of output - moderate structural changes, incorrect price indices in one industry

1. Value of output using Paasche deflator - base year = 0					
Year	0	t	t+1	t+2	Row
Sector a	297,0	326,8	387,9	447,5	211
Sector b (Correct)	996,0	1046,4	1096,8	1243,0	216
Sector b (Incorrect)	996,0	964,6	1008,4	1074,2	221
Sector c	1761,0	1820,1	1998,9	2055,0	226
Sum of Components (Correct)	3054,0	3193,3	3483,6	3745,5	231
Sum of Components (Incorrect)	3054,0	3111,5	3395,2	3576,7	233
Index form - year 0=100					
Sum of Components (Correct)	100,0	104,6	114,1	122,6	234
Sum of Components (Incorrect)	100,0	101,9	111,2	117,1	236
2. Value of output using Laspeyres deflator - base year = 0					
Year	0	t	t+1	t+2	Row
Sector a	297,0	326,4	387,7	444,0	241
Sector b (Correct)	996,0	1046,6	1094,8	1244,3	246
Sector b (Incorrect)	996,0	965,9	1009,0	1069,4	251
Sector c	1761,0	1819,8	1995,5	2055,0	256
Sum of Components (Correct)	3054,0	3196,3	3487,2	3748,1	261
Sum of Components (Incorrect)	3054,0	3112,2	3392,2	3568,4	263
Index form - year 0=100					
Sum of Components (Correct)	100,0	104,7	114,2	122,7	265
Sum of Components (Incorrect)	100,0	101,9	111,1	116,8	266

Source: Appendix 2-4a

This table contains of only two panels, laid out on roughly the same lines as Panels 1 and 2 of Table 2-5.1, and may be read in conjunction with them. The current price weights (Panel 4 of Table 2-5.1) remain the same, and, for the purposes of the present exercise, we are unconcerned with what happens when base years are changed (Panel 3 of Table 2-5.1). Industry *b2* is buried, of course, in Sector *b*, so for a detailed understanding of the exact changes that occur, it is necessary to refer to Appendix 2-4a. In both Panels 1 and 2 it may be seen that the 'incorrect' Sector *b* and Sum of Components results differ from their 'correct' counterparts. The 'correct' values are given in both Tables 2-5.1 and 2-5.2. The divergences that open up are slightly larger when Laspeyres price indices are used. The figures chosen for these simulations have been selected so as to produce divergences in the indices of output for

the years o and t that differ by approximately the same absolute amounts as the (a) and (b) estimates for 1981/82 and 1984/85 respectively in Table 2-3.3.⁸

The implicit assumption involved in using the aggregate deflators labelled as 'correct' from the previous exercise is that the correct industry deflator is known. In reality, this is unlikely to be the case. The burden of the argument in Chapter 2-7 is, in fact, that the aggregate deflators are wrong because one or more of the industry deflators is wrong. Supposing this conclusion to be justified, it may be seen that the aggregate deflators used in the simulation exercise share with their real life counterparts used by the CSS in the actual deflation processes, the characteristic of being merely the contingent outcome of the summing of a series of individual deflators, any of which may contain errors.

Fraught as is the process of measuring PPIs with the scope for making errors, one can imagine how such a combination of errors as that disclosed above could occur in practice. One of them has been suggested - there must be several other ways in which errors could creep in. Further speculation on what has happened is idle - it is also unlikely that the CSS will disclose the full set of figures. One thing is certain, however, and that is that if the 'incorrect' price index is correctly incorporated into the aggregate price index, the deflated Sum of Components and Totals figures return to equality (in the case of the Paasche) and near equality (in the case of Laspeyres).

The three-percentage point difference between Real Gross Output (Total) (the (a) estimate of gross output deflated by the total PPI in Table 2-3.3), and Real Gross Output (Sum of Components) (the (b) estimate - the sum of the individually deflated components) cannot result from the differential price and volume movements, as the CSS has claimed. By contrast, it can arise very easily from errors in the relevant deflators.

Output indices and major structural change

A similar set of assumptions to those in the previous example will now be used, but the economy will be made to undergo major structural change. The full set of figures on which the

⁸ To produce the divergences, the aggregate indices from the Table 2-5.1 totals are used in combination with 'incorrect' indices for industry $b2$ (Row 44 in Appendix 2-4a). The extent to which the 'incorrect' prices for industry $b2$ differ from the presumed 'correct' (but possibly not true) prices may be seen by comparing the sets of indices in Rows 39 and 44 respectively in Appendix 2-4a.

No attempt has been made in this simulation exercise to open a gap between the two total output estimates for the year o similar to that for the corresponding year in Table 2-3.3, ie, 1978/79 (R66 268m and R68 706m respectively). Clearly, it would be possible to do this by juggling the basic data used to construct the simulation. The benefits of doing so, however, are outweighed by the costs involved.

results below are based is given in Appendix 2-4b. Patterns of relative prices in this simulation are the same as those in the previous one, but the volumes of production change in dramatically different ways. This structural change induces a breakdown, but instead of following standard practice, additivity is maintained and the burden is allowed to fall on growth rates. This gives some indication of how large the impact of a not implausible set of changes can be.

Table 2-5.3 presents the results of the simulation. It also contains three panels in which the same series of tests are performed as in the moderate change scenario. Similarly, in Panel 4 of the table, the current price weights are given. As may be seen from an examination of these figures, the changes are substantial. Panels 1 and 2 yield similar results to those in the moderate change scenario. It is not necessary to comment on these results at any length. Suffice it to say that from within the comparative safety of base year o , the massive growth in Sector c , and the corresponding declines in Sectors a and b , using Laspeyres price indices instead of Paasche yields a difference of no more than 2,4 percentage points in the estimates of the index of output at the end of the period (Rows 94 and 95).

The rebased results in Panel 3 contain an unpleasant surprise for users of the statistics. If the elapsed times between the years o , t , $t+1$ and $t+2$ were not the the (arbitrary) one-year periods assigned to them above, but were instead assumed to be the three-year gaps between industrial censuses, then the growth rate of the total economy using o as base year would be 5,4 per cent *per annum*. Using $t+2$ as base year, this leaps to 7,6 per cent. The figures used to generate this are, of course, quite extreme. They are not, however, beyond the bounds of imagination - the discovery of oil and the development of industries associated with it could easily do this to a medium-sized economy.⁹ It is interesting that this effect can be produced by price changes (as in the gold boom in South Africa) and/or quantity changes, as in the present example.

What this simulation gives, in somewhat exaggerated form, is an indication of the damage done to the accounts by change. The form generally agreed upon for the presentation of the national accounts precludes growth rate changes of the type disclosed here,¹⁰ but the

9 Under these conditions, industries $a1$, $a2$ and $a3$ can be imagined to have been eliminated by the Dutch disease.

10 Sometimes, when rebasing takes place, growth rates in the various sectors change quite substantially. What is probably happening here is that given that rebasing entails a massive amount of recalculation, advantage is often taken of the fact that all of the figures are being reworked, to include such revised data as have become available from censuses. This happened when the rebasing from the year 1970 to 1975 took place. See Meth, 1992, pp15-16 for details of the impact on manufacturing, commerce and finance, and p18 for details of the reversal of a previously positive growth rate in mining to negative growth. On other occasions, as in the rebasing to the year 1980, it is stated quite explicitly that previously estimated growth rates have been retained - see footnote No. 43 on p26 of Meth (1992).

suppression of this type of effect cannot be done without cost. The decision to locate the damage in one area of the accounts is taken on common sense grounds, but in the South African case, without reference to users' needs.

Table 2-5.3 Values of output - major structural changes

1. Value of output using Paasche deflator - base year = 0					
Year	o	t	t+1	t+2	Row
Sector a	297,0	179,0	89,5	29,7	76
Sector b	996,0	1245,0	1098,0	994,0	77
Sector c	1761,0	3042,0	3522,0	4701,0	78
Sum of Components	3054,0	4466,0	4709,5	5724,7	80
Total	3054,0	4466,0	4709,5	5724,7	79
Index form - year o=100 (Sum of Components and Total)	100,0	146,2	154,2	187,4	94
2. Value of output using Laspeyres deflator - base year = 0					
Year	o	t	t+1	t+2	Row
Sector a	297,0	177,2	89,5	29,8	83
Sector b	996,0	1245,2	1094,0	994,9	84
Sector c	1761,0	3060,0	3520,3	4700,5	85
Sum of Components	3054,0	4482,4	4703,8	5725,1	87
Total	3054,0	4446,5	4679,5	5797,7	86
Index form - year o=100 (Total)	100,0	145,6	153,2	189,8	95
3. Indices of output - base year = t+2 (Set year 0 = 100)					
Year	o	t	t+1	t+2	Row
Paasche deflator	100,0	164,8	185,3	239,8	177
Laspeyres deflator	100,0	165,6	185,1	239,8	202
4. Weights - current price value relativities (%)					
Year	o	t	t+1	t+2	Row
Sector a	9,7	5,2	2,2	0,6	46
Sector b	32,6	24,3	22,1	15,3	47
Sector c	57,7	70,6	75,7	84,1	48

Source: Appendix 2-4b

As regards the gold price boom of the 1970s, I pointed out that as a result of the decision to maintain additivity, it became impossible to estimate the contributions of the different sectors to total GDP (Meth, 1992). Economic history became the hostage of the number of tonnes of

gold produced. Whether that particular decision was justified or not is a matter that has not received sufficient attention.¹¹

A similar situation seems now to have arisen in the manufacturing sector. This time, not only has there been no guidance from the CSS in the matter, there has not even been an acknowledgement that a problem has arisen. I refer, of course, to the impact which Sasol II and III had on output estimates in the 1985 manufacturing census. An attempt will be made to show, through further simulations, how serious the effects of this structural change are, and how poorly equipped the accounts are to deal with events of this type. That is done in Chapter 2-6.

Unpacking the CSS explanations for the divergences

At this point in the study it will be useful to pause and take stock of the explanations that have emerged to explain the various errors, and to consider the progress that has been made in testing the validity of these explanations. After this has been done, and the incorrect explanations have been dispensed with, a plausible story can be offered for the differences that remain. It will be recalled that a divergence between the highest and lowest output indices approaching five percentage points required explanation, and it may be recalled as well, that this was attributed by the CSS to the "...three factors..." referred to on page 277 above.

The simulations above have flirted with the first two of these factors, and although the work has been done in purely hypothetical terms, it appears that the results obtained demonstrate quite conclusively that output estimates are relatively insensitive to different 'implicit index formulae' and 'weighting bases' as well. Whether this dispenses with the CSS claim depends, to some extent, on exactly what the CSS meant by these two 'factors', and on whether, in the first place, the 'factors' were supposed to explain the difference between Real Output (Total) and Real Output (Sum of Components) at all. As far as the latter point is concerned, it is assumed, from the context in which they were offered, that this was indeed the intention. As to the 'implicit formulae', if this refers to the Paasche and Laspeyres indices that go into the estimates in various correct and incorrect combinations, then the case is iron-clad.

11 It is not as though the baleful effects of this have not been brought to the attention of the CSS. Kantor commented on the problem in 1983, the FOSATU Challenge (Meth, 1983) took up the issue, Kantor and Abedian (1990) looked at it as well, and of course, through Moll (1992), the question obtained international coverage in the literature.

'Weighting bases' seems most likely to refer to the fact that PVMPs are weighted by the distribution of net output, PPIs by the value of sales, and Real Output (Sum of Components) estimates by the distribution of gross output. Nothing in the simulations above grappled directly with a problem of this sort. The rebasing of the moderate change results in Panel 3 of Table 2-5.1 does, however, constitute a fairly severe change of weighting base, and thus may be argued to serve as a valid test of the CSS proposition. As may be seen, the relative size of Sector *a* rises by nearly four percentage points over the period, while that of Sector *b* falls by three percentage points, but the net effect is to make the rebased total differ from the original value by slightly more than one percentage point. On this basis, the attempted explanation is provisionally rejected - this rejection is bolstered by the attempts in Appendix 2-6 to replicate the CSS PVMPs (benchmarked and unbenchmarked).

That leaves the third factor - the adjustment of census data. As an explanation this can be rejected with greater confidence.

Revisions to the census - a partial explanation?

A long time elapsed between the publication of the 1985 manufacturing census results and the publication of the benchmarked figures based on these results. When the benchmarked results finally appeared, they contained the errors analysed in this study. In addition to this, however, the CSS themselves unearthed a further set of errors which reached back into the manufacturing censuses for 1979 and 1982. An entry point to the story is the report of the discovery by the CSS of mistakes in the census returns of several large firms.¹² These were detected when the CSS compared the sum of the 12 monthly survey results of value of sales estimates (a close, but not exact proxy for gross output) with the census estimates of gross output. Estimates of the size and location of these errors have been through preliminary and final phases. The latest set of revisions was published in Statistical News Release (SNR) P3001, 28 June 1993. Details of the earlier revisions were not published, but use is made here of the older set because that was all that was available to the CSS when 'revisions to the census' was offered as a partial explanation for the errors. These revisions are shown in Table 2-5.4 below.

12 It is by no means obvious why it is the census and not the survey figures that are at fault. Presumably the CSS had to return to these large firms some 5 years after they had completed the returns to ask for corroborating information. In passing, one should spare a thought for the CSS and the difficulties they face in carrying out their thankless task - obstructed at many points by those who are likely to castigate the institution for the inaccuracy of its output.

Without being given any indication of their extent, users were alerted to the fact that there were errors in the published manufacturing census figures when the following note appeared in SNR P3041.3 of 9 March 1992. On piii of this document it is stated that:

"Some large establishments submitted incomplete information in respect of their output/sales in the census, e.g. establishments in the paper and paper products, other chemical products and metal products industries did not report the information on output/sales as fully in the census as it was reflected in the sample survey."

Table 2-5.4 Revisions to manufacturing census gross output estimates (R millions)

	<----- 1978/79 ----->			<----- 1981/82 ----->			<----- 1984/85 ----->		
	Publ'd	Revised	Error (%)	Publ'd	Revised	Error (%)	Publ'd	Revised	Error (%)
Leather	-	-	-	-	-	-	358,4	335,9	-6,3
Furniture	-	-	-	662,9	686,0	3,5	-	-	-
Paper	-	-	-	1618,3	1835,4	13,4	-	-	-
Industrial Chemicals	-	-	-	3083,9	3304,4	7,2	3761,9	3895,0	3,5
Other Chemicals	3030,4	2862,9	-5,5	5891,6	6912,9	17,3	9479,0	10095,9	6,5
Plastic	-	-	-	1183,6	963,1	-18,6	-	-	-
Basic non-ferrous	945,3	759,6	-19,6	1553,1	1263,6	-18,6	2346,6	1883,0	-19,8
Metal Products	-	-	-	5009,8	4962,8	-0,9	5820,1	5796,0	-0,4
Machinery	-	-	-	-	-	-	4352,4	4307,0	-1,0
Electrical machinery	-	-	-	2776,5	2729,8	-1,7	3571,9	3504,1	-1,9
Motor vehicles	-	-	-	-	-	-	5113,9	5020,2	-1,8
Professional	-	-	-	-	-	-	308,1	264,8	-14,1
Other manufacturing	-	-	-	750,3	706,5	-5,8	811,2	750,1	-7,5
TOTAL	29926,6	29573,4	-1,2	55735,6	56569,9	1,5	75092,2	75020,8	-0,1

Source: Revised estimates supplied by the Secretary, Statistics Council, 6 November 1992.
Published estimates are from *South African Statistics 1990*, pp12.6ff.

A verbal request to the CSS for details of these revisions made in August 1992 was politely turned down, but the figures were finally supplied by the Secretary of the Statistics Council in November 1992. The discussion in the passage cited in Chapter 2-3 from Statistical News Release No. P3041.3 of 9 March 1992, about the limited impact of revisions necessitated by the receipt of new information from respondents (0,3 of a percentage point in 1990) may be an oblique reference to a continuing attempt by the CSS to ensure that discrepancies do not creep into the figures by way of inconsistent reporting in the monthly surveys.

Several notable features characterise the revisions shown in Table 2-5.4, the first being that some of the within-group errors¹³ are extremely large. The most important of these is the under-estimation of 1985 output in the industry 'Other Chemical Products' coupled with an over-estimation in 1979. Given the heavy weighting of this industry in total output, the

13 Errors in the estimates for a single industry.

revision could have been expected to have had significant effects. In proportional terms, the revisions to the industry 'Non-ferrous Basic Metals' were the largest, but because of the direction and magnitude of the revisions, the overall impact was negligible. The other revisions were interesting but not highly significant. The between-group results were significantly affected.¹⁴ Although the positive and negative changes almost cancelled out in the 1985 results,¹⁵ the revisions to the 1979 estimates did not - the 1,2 percentage point downward revision to the total is significant. If the ratios of gross to net output had remained unchanged over the period, then the growth rate would automatically have risen as a consequence.

Under what conditions could these revisions form part of the explanation for the discrepancies at issue in this study? The answer must surely be that they cannot explain any part of them at all. From the information given in Table 2-3.3 in Chapter 2-3, it may be inferred that the benchmarked results are based on a census now known to be flawed. As long, however, as the benchmarking was performed on a full set of consistent results (consistent in the sense that totals were identical to the sums of individual industry estimates) either revised or unrevised, the revisions simply cannot have contributed to the observed discrepancies. Rather obviously, if under-reporting of any degree occurred at industry level in the census, then every addition in current prices to the industry total to compensate for this must also increase the overall total by the same amount. These revisions would alter price and quantity relativities, but the simulations in this chapter show quite clearly that output estimates are insensitive, within the currency of any particular base year, to substantial variation in both quantity and price relativities.

Changes in relative prices cannot possibly explain the discrepancy. Presumably, some combination of price changes could be dreamed up to make Real Gross Output (Total) and Real Gross Output (Sum of Components) change by different amounts (using Laspeyres price indices), but the changes would have to be extreme. The only candidate for the label 'extreme' is the mistaken substitution of the Petroleum PPI for the 'Other Chemicals' PPI - in all of the other industries where significant revisions took place, the deviations of the individual industry PPIs from aggregate PPI were negligibly small.¹⁶ A moment's reflection thus makes it clear that the failure of respondents to supply correct output or sales data cannot possibly account

14 Revisions to the estimates for different industries affect relative weights, or proportional contributions of those industries to total output.

15 As an aside, one may note that the fortuitous self-cancelling effect of the reported errors demonstrates the operation in action of a 'law' behind which national accounting statisticians often take refuge! It is this 'law' which partly underlies the claim that their handiwork enables them to pick up trends reasonably accurately, even if the absolute figures they generate are not to be taken too literally.

16 See Tables A2-6.D1; A2-6.D2, and A2-6.D10 in Appendix 2-6.

for any part of the discrepancy in which the whole of manufacturing output is greater than the sum of its parts.

Depending on what assumptions one made about base-year weights and the net/gross output ratio in 'Other Chemicals', these revisions could have had the effect of increasing the growth rate in manufacturing over the period 1979-85 fairly substantially. In other words, what was believed at the time about certain aspects of the behaviour the economy, could, by virtue of these revisions, have been shown to have been false.¹⁷

Interim conclusions

Several conclusions follow from the analysis above. In the first place, whether or not economies experience dramatic structural transformation during the currency of any particular base year, it does not seem as though a great deal of harm will be done if one treats the Laspeyres price indices that are often all it is practicable to estimate, as though they were the Paasche indices required to generate the Laspeyres base-weighted volume series. In the second place, the proposition that the sum of the deflated components of output is (approximately) equal to the deflated value of total output is so close to being true under many conditions that any serious deviation should be investigated as a matter of course. Obviously, if Paasche price indices are available, the two are identical. In the simulations above, if Laspeyres indices are used, then even with large variations in relative prices, the two do not differ by more than a few tenths of a percentage point. Significant deviation from this (relative) equality apparently will only result from massive structural change. When commenting on the newly-benchmarked output estimates, I insisted that the fault must lie in the measurement of the industry-level price indices (letter to the CSS dated 27 March 1992). It is clear from the rebased results in the 'Moderate change' scenario that errors in individual PPIs would have to be very large to have the necessary effect. If, however, there are errors in either or both the industry-level price indices and the aggregate deflator, then the possibilities of opening up gaps as large as those revealed in Table 2-3.3 are much increased. This means that there is almost certainly something seriously wrong with the PPIs used as deflators.

17 It may be recalled that an attempt at assessing the significance of this, especially for those who make use of the CSS figures, was made at the end of Chapter 2-1 of the study.

Chapter 2-6

Major changes in the net to gross output ratio

Introduction

There are several references in the literature, some of which are cited below, to the proposition that indirect volume measures obtained by deflating gross output estimates by a suitable price index are quite reliable as long as the ratio of net to gross output does not vary too greatly. Unfortunately, there is not much guidance available on the question of how to deal with the problem of large and rapid changes in this ratio when they do occur.

This chapter commences with an examination, in general terms, of some of the issues raised by this phenomenon. Thereafter, the results of a pair of simulations which show the impact of changes in the ratio of net to gross output in a hypothetical setting are presented. These simulations reveal how costly (in terms of the credibility of the accounts) the inability of national accounting statisticians to estimate the value of output 'correctly' can be. By reference to the South African benchmarked and unbenchmark estimates, the 'pre-reconstruction' estimates are then shown to be unsatisfactory. In the reconstructed figures, the major group 'Other Chemical Products' is now divided into two major groups - 'Products of Petroleum and Coal' and 'Chemical Products, excluding Products of Petroleum and Coal'.¹ A brief analysis of the reconstructed figures for the former industry suggests that they too are bedevilled by similar problems to those which affected the earlier estimates.

Double deflation, a technique not without problems of its own (some of which are addressed in Chapter 2-7) represents one possible technique for dealing with the problem of sharp variations in the ratio of net to gross output. The simulations conducted below suggest that it is not. A casual reading of the literature may lead one to think that under ideal circumstances, the double deflation approach probably gets closest to being a 'correct' measure (Maurice, 1968, p77). Even if this were true, double deflation could not escape the additivity problems that

1 According to the latest edition of the SIC handbook (Report No. 09-90-02, January 1993, Fifth Edition, p4), these two major groups do not have the same names as the CSS has used in the news release giving physical volumes of output (SNR P3041.3, 12 November 1993). The handbook lists a major group 'Petroleum refineries/synthesisers' with the 3-digit number 332, and another group called 'Manufacture of other chemical products' - SIC 335. SIC 334 is 'Manufacture of basic chemicals' - presumably what used to be called 'Industrial chemicals' - the name that the CSS still apparently uses for this major group in SNR P3041.3. Splitting the previous major group 'Other chemicals' obviously became possible in the new relaxed political climate in South Africa.

occur at rebasing, especially when structural change has taken place. In any case, double deflation demands comprehensive and detailed data which often are not available. National accounting statisticians have long faced these problems, but few, if any, innovative solutions appear to have emerged. The Australian Bureau of Statistics (ABS), for example, uses the deflated gross output approach in the manufacturing sector, but notes that "...errors arising from a breach in the assumption that constant price estimates of gross output and intermediate input grow at the same rate..." can produce "...substantial... bias..." (Aspden, 1990, p5). To minimize errors caused by:

"...changing relativities between output and input, the estimation of manufacturing gross product is carried out at the ASIC [Australian Standard Industrial Classification] class level for those years in which a manufacturing census has been conducted and rebasing is carried out every five years." (Aspden, 1990, p5)²

It is by no means clear that this approach could cope with the problems caused by Sasol's sudden augmentation of the capacity of the 'Other Chemicals' industry and the change in the ratio of net to gross output this entailed.³ The problem in South Africa seems is quite severe - discussions with the relevant personnel at the CSS not only failed to elicit any suggestions as to how the problems caused by changing net to gross output ratios might be approached, but what seems more ominous, they did not reveal any awareness on the part of the authorities that a problem existed.⁴

PVMPs, PPIs and variations in the net/gross output ratio

Foreseeing the problems likely to arise in the measurement of gross and net output, the compilers of the 1968 SNA suggested certain assumptions that could be made when full information was unavailable. Paragraph 4.78, for example states that:

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- 2 In the guide to the Australian National Accounts, Castles notes that:
 "For annual Manufacturing, the gross output method is used to revalue the gross product estimates for about 180 different classes within Manufacturing. They are then aggregated to obtain gross product at constant prices for total Manufacturing. As a result, the assumption of a stable ratio of intermediate input to output (both at constant prices) is made only within each individual class." (1990, p31, para.4.23)
 This yields the magnitude defined above as Real Output (Sum of Components). Real Output (Total) is not estimated by national accounting statisticians, presumably because variations in the ratio of intermediate input to output at the aggregate level could introduce significant errors. The CSS is justified, therefore, in claiming that their methods conform to standard international practice. That, however, does not alter the fact that the failure of Real Output (Sum of Components) to equal Real Output (Total) is evidence of measurement error somewhere along the line. When the divergence becomes large, that is an indication that the error levels have become intolerably high. It signals the need for careful investigation - not for a retreat into the comfort of convention.
 - 3 As a result of the (strategic) decision to build the two Sasols, the mix of inputs in the industry changed substantially. Coal increased in relative importance while imported crude petroleum diminished. This affected input pricing structures, and hence the ratios of net to gross output.
 - 4 Discussions with Mr Roelf van Tonder on 22 July 1993, during which the problem of the 'Sasol Syndrome' was raised.

"The standard quantity measure of value added consists of subtracting from the quantity of gross output a measure of the quantity of intermediate inputs using, in each case, base-year weights. In practice, much cruder calculations are frequently made based on one of the indicators just suggested for gross output. The justification for these approximations is either that intermediate inputs can be assumed to vary in the same proportion as gross output or that the change in the productivity of one or more primary factors can be assumed to be small." (UN, 1968)

There seems to be a fairly widely held view that unless changes over time in the ratio of net to gross output are large, little harm is done in using gross output as a proxy for net output (Black, 1992, Note 2). Kingaby states that:

"Even where at current prices the ratio between gross output and value added changes, the change may be due to differing output and input price movements and hence in constant prices the ratio can be more stable. The rebasing process every 5 years then catches up with changes in the relationship between gross output and value added over this period." (1989, p104)

When, however, events as significant as the coming on stream of Sasol II (March 1980) and Sasol III (May 1982) underlie the change, there is no room for complacency. Such episodes affect both the ratio of net to gross output, and the weights used to estimate the various magnitudes. As was shown in Chapter 2-5, weight changes become significant when rebasing takes place, usually imposing strain on the property of additivity. The shocks to the accounting system caused by the two Sasols, fortunately a relatively rare event, triggered both effects simultaneously. The changes in output volumes that undoubtedly lie behind the changes in the ratios of net to gross output make indirect output estimates unreliable at precisely the point at which growth rates need to be measured carefully. It was noted above that where necessary, standard practice is to maintain growth rates at the cost of additivity, but when true volume changes cannot adequately be ascertained, the decision to maintain or jettison additivity is of little consequence. Maintaining incorrect growth rates is of no use, and abandoning additivity would be a spurious compliance with accounting convention.

In short, it is necessary to determine whether (and if so, when) the use of an indirect volume measure derived from gross output instead of net will result in an under- or over-statement of the level of 'output' in constant price terms. To do so it may be best to commence by stating what the most desirable data package would be, and what stands in the way of its capture. Once that is established, it can be contrasted with what has happened in practice in South Africa.

In an ideal (from a national accounting statistician's point of view) world, there would be no technical progress, no quality changes, and price and quantity relativities, both of inputs and of outputs, would preferably remain unchanged. If they were to change, however, they would do so gradually. Firms would produce homogeneous commodities, and the notions of firm and

establishment would be co-terminous. Firms would also respond promptly and accurately to survey and census questionnaires. Productivities of the co-operating factors of production would be measurable and factor rewards would be fully determined by relative productivities.⁵ It would help if there were also perfect competition, constant returns to scale and full employment. Under these conditions, changes in the ratio of net to gross output would not be an issue - they would simply reflect market determined changes in the relative costs of the inputs into production, and market determined changes in the patterns of final demand. Given this, a simple physical measure of output could be constructed which would reveal accurately the progress of the economy. There would still be additivity (or growth rate) problems when rebasing occurred, but these would be trivial.

The real world is not so obliging - it violates, to a greater or lesser degree, each one of these conditions. Each breach poses problems for economic theory - specifically, that part of it known as value theory. The clearest example of the failure of value theory, and of national accounting, is given by the gold price surge of the 1970s (and its collapse in the 1980s!). As I and others have shown, the implicit value theory underlying the national accounts simply cannot deal with problems of this sort (Moll, 1992; Meth, 1991b). The terms of trade adjustments, another set of conventions (Gutmann, 1981) that are supposed to cope with such events are sometimes simply not adequate (Meth, 1991b).⁶ Some violations are more important than others, but all of them, large or small, can be traced back to a problem with which value theory cannot cope in the real world.⁷

Suppose however, that these problems can be ignored. Suppose also that the economy in question is reasonably open, with external competition providing protection against too much gouging by local producers. This latter condition helps to guard against the delusion that producing large quantities of import-substituting goods at high relative prices constitutes an unambiguous welfare improvement. Under these conditions, a physical volume index of production, when properly estimated, would provide a reasonably good measure of national

5 For good measure, all of the theoretical problems associated with operationalising marginal productivity theory would have been solved as well. On this, see Thurow (1980).

6 These problems are addressed in Appendix 2-1.

7 Changing price and volume relativities do not present insuperable problems unless the changes are large. Technical and quality changes are probably much more important - the article in *The Economist* quoted from above, citing research work done in the USA, argues that output in that economy has been understated because the means of measuring it are inadequate. This is true for industries such as those producing electronic machines like computers where measurement techniques cannot cope with the constant revolutionising of product and production technologies. Ten years, possibly a respectable interval between reconstructions of the sample frame for major surveys like the Production Price Index or the Consumer Price Index, is more than a lifetime in computers. The problem is much worse when it comes to the service sector - improvements in the quality of the service to customers in the transport industry, for example, facilitated by computer-assisted route planning, cause mileage, and hence output to drop. (December 26th 1992-January 8th 1993, p16).

economic progress. Unfortunately, it seems to be difficult, if not impossible to estimate this measure properly (ie, sufficiently accurately) in a complex and dynamic sector such as manufacturing. Presumably it was these difficulties that caused the British national accounting statisticians to all but abandon the attempts to measure physical progress in this way. One element of the decision to change to the system presently used in Britain must surely have been a perception that in an open economy with significant competition, the PPI can track prices sufficiently accurately.

The record of the attempts by the CSS to measure the PPI is not an altogether happy one, as the discussion at the end of this chapter on the use of the Petroleum PPI as a deflator for the 'Other Chemicals' industry makes clear - that of the attempts to measure the PVMP is, as documented above, and in Meth (1992), much less satisfactory. It may well be that with a great deal of effort the PVMP survey could be made trustworthy. Against the heavy commitment of resources required to make this happen, one must balance the fact that an apparently satisfactory alternative exists that does not require such a commitment. That is what the British appear to have done - they decided in favour of the simpler alternative - the CSS has now followed suit.

In casting off a desirable but troublesome economic indicator, something, undoubtedly, is lost. Even though the PVMP is imperfect, it has the virtue of being estimated independently of the indirect measures of output, and is thus potentially capable of serving as a backcheck on those measures. But there can be no doubt that a measure which drifts somewhere between ten and 20 percentage points from what is probably its true value in six years, as the South African aggregate PVMP has, is incapable of throwing much light on the question of a change in the ratio of net to gross output from 37,8 to 41,5 over the same period.

Simulating the economic impact of a Sasol-type episode

The simulated double deflation reported on below is a relatively crude attempt to get to grips with the problem of rapid changes in net to gross output ratios. Despite its simplicity, it exposes a major weakness in the accounting procedures. It seems also to give at least some indication of the size of the error in the manufacturing sector output estimates that has arisen because of the change in net to gross output ratios.

To build up the basic data for this particular simulation exercise, the full workings of which may be seen in Appendices 2-5a and 2-5b, values and prices for the moderate change scenario

in the previous chapter were used as a starting point. After some panelbeating, the hypothetical figures were moulded so that a single industry, *b2*, had a set of performance results not unlike those reported in the industry 'Other Chemicals' over the period 1978/79-87/88. These derive from the ratios presented in Table 2-6.1 below. Aggregate output figures, however, are not much like those reported by the CSS. How much of a disadvantage this is, it is not easy to say.

In brief, the salient ratios in Appendix 2-5b are:

- i The gross output weight of industry *b2* (Row 105)
- ii Actual ratios of net to gross output in industry *b2* (Row 128)
- iii The same ratio for the total economy (Row 133)
- iv The net output weight of industry *b2* (Row 163)
- v The weights of intermediate inputs in industry *b2* (Row 217)

A comparison with the relevant ratios in Table 2-6.1 shows how close each is to its equivalent. The only exception is No. iii. If the changes in the net to gross output ratio are the primary cause of the deviation of the indirect output estimate from its double deflated counterpart, as seems likely, then the Row 133 estimates are conservative in the sense that they would understate the increase in output caused by the change. In other words, the simulated changes are less extreme than those reported for the real economy.

Table 2-6.1 Changing the net to gross output ratio in 'Other Chemicals'

	1978/79	1981/82	1984/85	1987/88
Other Chemicals:				
Gross Output (Rm)	3 030	5 939	9 729	13 308
Net Output (Rm)	974	1 992	4 957	5 972
Net/Gross Output %	32,1	33,5	51,0	44,9
Total Manufacturing:				
Gross Output (Rm)	29 769	55 652	75 351	118 243
Net Output (Rm)	11 238	21 718	31 297	47 783
Net/Gross Output %	37,8	39,0	41,5	40,4
Gross Output in Other Chemicals as % of Total Gross Output	10,2	10,7	12,9	11,3
Net Output in Other Chemicals as % of Total Net Output	8,7	9,2	15,8	12,5
Intermediate Inputs:				
In Other Chemicals as % of Gross Output in Other Chemicals	67,9	66,5	49,0	55,1
In Total Manufacturing as % of Gross Output in Total Manufacturing	62,2	61,0	58,5	59,6
In Other Chemicals as % of Intermediate Inputs in Total Manufacturing	11,1	11,6	10,8	10,4

Source: Raw data are from Statistical News Release (SNR) P3001, 28 June 1993

As may be seen from the table, net output (in current prices) in the industry 'Other Chemicals' grows much more rapidly than gross output over the period 1978/79-84/85 (a little over sixfold vs something over fourfold). The effects are dramatic - the net to gross output ratio in 'Other Chemicals' increases substantially, pulling up the corresponding ratio for the sector as a whole. Intermediate inputs, rather obviously, fall, both as a percentage of total gross output, and much more sharply as a percentage of gross output in 'Other Chemicals'.

Moving from Table 2-6.1 to the simulations presented in Appendices 2-5a and 2-5b, one obtains a set of results which cast doubts on the accuracy not only of the benchmarked results, but also of the reconstructed estimates. As in the previous simulation exercise, the economy consists of nine industries aggregated into three sectors, with the basic price and volume (quantity) data juggled to produce the desired ratios discussed above. The key assumption used to construct the simulations is that the data are available to permit the double deflation exercise to be performed satisfactorily. It is a simple matter to compare the results of this 'correct' method of valuing output with those obtained using the indirect method, ie, deflating gross output. Only Paasche price indices (the 'statistically correct' indices) are estimated. Using these, one finds that as long as the ratio of net to gross output remains unchanged, as it is in the first of the two simulations (Appendix 2-5a), the two approaches yield identical results. This may be seen by comparing the two sets of output estimates in Rows 291 and 295 (Sector *a*), Rows 299 and 303 (Sector *b*), Rows 307 and 311 (Sector *c*), and Rows 315 and 319 (Total economy), with each other. These results are presented in Table 2-6.2. To observe the full workings of the simulation, it is necessary to refer to the appendix.

Table 2-6.2 Single and double deflation - no changes in net/gross output ratio

Year	1979	1982	1985	1988	Row
Gross output - Sector a	100,0	110,0	130,6	150,7	291
Net output - Sector a	100,0	110,0	130,6	150,7	295
Gross output - Sector b	100,0	107,4	117,5	127,5	299
Net output - Sector b	100,0	107,4	117,5	127,5	303
Gross output - Sector c	100,0	103,4	113,5	116,7	307
Net output - Sector c	100,0	103,4	113,5	116,7	311
Gross output - Total economy	100,0	105,3	116,4	123,4	315
Net output - Total economy	100,0	105,3	116,4	123,5	319

Source: Appendix 2-5a

Note: Gross output is obtained by single deflation, and net output by double deflation.

As soon as the ratio is allowed to change, as it is in the simulation performed in Appendix 2-5b, a gap is opened between the two sets of estimates. In this case, the differences appear

between the Row 299 and 303 estimates (Sector *b*), and the Row 315 and 319 (Total economy) figures. Given that the performance of industry *b2* is similar in most respects to the real-world performance reported for 'Other Chemicals', it does not seem unreasonable to suggest that the error caused by using the deflated value of gross output as an indirect measure of output volumes could cause the true level of output to be understated by at least seven percentage points in 1984/85, and by somewhere in excess of four percentage points in 1987/88.

Table 2-6.3 Single and double deflation - changes in net/gross output ratio in Sector *b* (and the Total economy)

Year	1979	1982	1985	1988	Row
Gross output - Sector a	100,0	110,0	130,6	150,7	291
Net output - Sector a	100,0	110,0	130,6	150,7	295
Gross output - Sector <i>b</i>	100,0	107,4	117,5	127,5	299
Net output - Sector <i>b</i>	100,0	108,0	140,0	141,5	303
Gross output - Sector <i>c</i>	100,0	103,4	113,5	116,7	307
Net output - Sector <i>c</i>	100,0	103,4	113,5	116,7	311
Gross output - Total economy	100,0	105,3	116,4	123,4	315
Net output - Total economy	100,0	105,4	123,3	127,7	319

Source: Appendix 2-5b

Note: Gross output is obtained by single deflation, and net output by double deflation.

To generate these results, all that it is necessary to do is to perform a simple arithmetical transformation on the ratios of net to gross output. The mechanism used is the 'Live table'⁸ for varying this ratio in Rows 147-156 of Appendix 2-5b. This has been done instead of the somewhat more complicated operation of varying both the price and volume variables in the intermediate inputs tables. In principle, there is nothing that prevents one from adopting the latter rather than the former approach - it is merely more difficult to do. A glance at the relative price tables for gross outputs (Rows 72-81) and intermediate inputs (Rows 197-206) reveals that the same price structures have been used in both cases. Any attempt to bring the simulation closer to the real economy would entail getting the aggregate price and the *b2* price indices for both magnitudes (gross output and intermediate inputs) correct. Since the official figures for gross output are in dispute, and since there are no published intermediate input prices, there seems little point in attempting that exercise here. The line of inquiry opened up by the examination of the impact of changing net to gross output ratios appears, however, to have potential, so it will be pursued. The upwardly revised output estimates that derive from the exercise carried out above look as though they would be in line with the values predicted in

8 The group of values in this block in the spreadsheet is given the somewhat awkward name 'Live table', for want of a better one. Changing any of the values of net output ($Q.p - q.p$) in the block alters the ratio of net to gross output, causing a ripple effect through the whole spreadsheet.

Meth (1992, p55), my error in using calendar year deflators instead of financial year notwithstanding. An attempt at confirming this result using a different (and non-approved) proxy measure for output will be made in Chapter 2-7, but ways will now to be sought to probe the barriers to the solution of the problem posed by the poverty of the data.

By way of an introduction to a more detailed examination of the manufacturing sector data, changes in the proportional contributions (weights) of net and gross output to total production are given in Table 2-6.4 below. Many of the individual industry results in the 1979, 1982 and 1985 manufacturing censuses have been revised, some of them more than once. The second set of revised estimates, which were published in June 1993, have been used to construct Table 2-6.4.

Table 2-6.4 Value changes in South African manufacturing 1979-85

	<----->				<----->		
	Proportional distribution		Net output		Ratio of net to gross output		
	Gross output		1979	1985	1979	1982	1985
Food	15,0	15,4	11,6	11,5	29,4	30,3	31,0
Beverages	3,1	3,8	2,8	2,8	34,7	35,1	31,1
Tobacco	0,6	0,7	0,6	0,7	35,8	16,7	38,7
Textiles	5,1	4,0	5,0	3,7	37,7	38,8	39,0
Clothing	2,7	2,6	3,0	2,9	42,0	43,9	47,3
Leather	0,5	0,4	0,4	0,4	33,4	35,1	34,3
Footwear	1,0	0,9	1,1	1,0	44,5	48,3	47,8
Wood	1,4	1,6	1,7	1,8	44,6	47,2	47,0
Furniture	0,9	1,2	1,2	1,4	48,3	48,8	49,0
Paper	3,4	4,0	3,7	4,2	40,7	45,6	43,0
Printing	2,4	2,7	3,7	3,8	58,6	58,4	58,7
Industrial Chemicals	6,1	5,6	5,3	4,7	32,5	31,7	34,7
Other Chemicals	10,2	12,9	8,7	15,8	32,1	33,5	51,0
Rubber	1,4	1,2	1,8	1,4	47,5	52,6	49,1
Plastic	1,6	1,9	1,8	2,1	42,3	44,1	45,4
Pottery	0,1	0,1	0,2	0,2	59,9	67,6	61,0
Glass	0,6	0,7	0,8	0,9	52,7	56,6	53,8
Other Non-metal Mineral	3,0	3,1	4,0	4,0	50,8	51,1	54,7
Basic iron	9,3	7,5	10,4	7,6	42,1	41,7	42,1
Basic non-ferrous	2,6	2,5	2,5	2,4	37,5	37,4	40,1
Metal Products	8,5	7,9	8,9	8,0	39,5	42,5	42,1
Machinery	5,8	5,5	6,9	6,0	44,8	47,0	45,3
Electrical machinery	4,7	4,7	5,3	4,8	42,4	41,9	42,9
Motor vehicles	6,4	6,7	5,0	4,8	29,1	31,3	30,2
Transport equipment	1,6	1,0	1,9	1,4	43,8	40,8	59,2
Professional	0,3	0,4	0,3	0,4	45,3	47,1	51,6
Other manufacturing	1,8	1,1	1,4	1,0	29,1	34,0	40,4
TOTAL	100,0	100,0	100,0	100,0	37,8	39,0	41,5

Source: Estimated from the data in SNR P3001, 28 June 1993.

The first two panels in the table show the weights, or proportional distribution of values in manufacturing, as measured both by gross and by net output for the years 1979 and 1985, and the third panel the ratios of net to gross output for the years 1979, 1982 and 1985. Rather obviously, if an industry has a lower than average net to gross output ratio, its proportional contribution to gross output is larger than to net output and *vice-versa*. Scanning the performances posted above, one does not detect many anomalies. Plausible hypotheses could be advanced to explain the behaviour of most industries, and where there are substantial declines in the one figure, as in 'Industrial Chemicals' and 'Basic Iron', these are accompanied by declines in the other. As far as proportional contributions to output are concerned, one industry stands out - 'Other Chemicals'. By 1985, this single industry accounted for nearly one-sixth of total value added in manufacturing! Quite remarkably, the ratio of net to gross output falls in only one industry - 'Beverages'. In some of the others, the increases are trivial, and in certain cases, the results are unreliable ('Other Manufacturing' and 'Professional Equipment'), but in many instances, the increases appear to be large enough at least to warrant further investigation.⁹

Without conducting such an examination, endorsement of the compromise made by the British national accounting statisticians to measure intercensal output values must, of necessity, be qualified. In the absence of reliable PVMPs to function as a backup to the indirectly estimated output values, some procedure needs to be devised to take account of changes in the net to gross output ratios. It may well be that with exception of the industry 'Other Chemicals', and perhaps a few others like 'Clothing', 'Paper', 'Plastic' and 'Non-ferrous metals', the observed changes in the proportional distribution of values between industries cannot make more than the most trivial difference to the set of 'indirect' volumes that would be obtained in this way. This cannot, however, be taken for granted.

Much of the rise in the ratio of net to gross output for the manufacturing sector as a whole of about 10 per cent is accounted for by the change in the single industry, 'Other Chemicals'. When this industry, which contains most of South Africa's petroleum industry, is removed from total output, the ratio of net to gross output rises from 38,3 percent in 1979 to 40,1 percent in 1985. Although there is no simple way to be sure of it, a change of this magnitude may be small enough to ignore - a rise of almost four percentage points almost certainly is not. Until very recently, it was not possible for those outside of the inner circle of government

9 Two other values in 'Ratio of Net to Gross Output' panel of Table 2-6.4 stand out as anomalous - the 1981/82 figure for the Tobacco industry, and the 1984/85 figure for Transport Equipment. Both have been checked (the basic data may be found in Table A2-6.D9 in Appendix 2-6) and are correct. The net output figure supplied by the CSS for Tobacco is probably incorrect. The Transport Equipment result may be correct (ie, a large change in the ratio may have taken place), but fortunately, the industry accounts for so little of total net output that for the purposes of the exercise being conducted here it may be ignored.

servants privy to the secrets of petroleum transactions to unravel the mysteries of the industry.¹⁰ The recently published reconstructed output estimates reduce the ignorance level somewhat, and one can now begin to assess the impact of the industry on the national accounts, but it is impossible for an outsider to get to grips with the Petroleum PPI problem.

These limitations notwithstanding, it was still possible, using the information available prior to November 1993, ie, before the publication of the reconstructed PVMPs, to reject the then existing CSS account of the performance of the industry 'Other Chemicals', and hence, of the manufacturing sector as a whole. Known estimates of output in the industry in the period prior to November 1993, as well as those that could be made using the deflators available, are summarised in Table 2-6.5. The first panel of the table contains the 'old' unbenchmarked estimates. When these are read in conjunction with the benchmarked figures directly below them, they show how massively output was under-estimated. To have been wrong by almost 20 percentage points in 1981/82, by about 30 in 1984/85 and by nearly 40 percentage points in 1987/88 is a spectacularly bad performance. Since production in this industry is heavily concentrated in the large firms,¹¹ and since it was probably only two new plants that caused the errors in the estimates, one is more than justified in raising queries about the survey technique used to measure output.

The results in Panels iii and iv of the table constitute the beginnings of an attempt to replicate the PVMPs in Panel ii. Since it is known that benchmarked PVMPs are obtained by deflating gross output estimates by the relevant PPIs, one should be able to perform this exercise oneself relatively easily if the required data are to hand. If the two sets of PPIs are applied to the current price estimates of gross output, then one obtains the figures given in Panel iii of the table.¹² In the process, one obtains a partial¹³ explanation for the differences between the (a) and the (b) figures in Table 2-3.3.

The next step is to express these results in index form, and then compare the outcomes with the PVMPs in Panel ii. It is obvious from the Panel iv results, especially for the critical year

10 A recent dissertation on the plastics industry by Crompton (1994) looks exhaustively at the available published and unpublished sources of information on the petro-chemicals industry in South Africa. His work contains comparative costings for various petro-chemicals, but the kind of detail sought here was not given in the work.

11 According to the 1985 Manufacturing Census, the industry consisted of 597 establishments. Of these, the 108 establishments with a gross output in excess of R10 000 000, ie, 18 percent of the total, accounted for more than 91 percent of total net output (*South African Statistics 1990*, p12.34).

12 The relatively small differences between the figures obtained using the combined 'Chemicals' industry PPI as deflator in this table, and those in Table 2-3.4, result from the fact that the latter are based on the unrevised estimates.

13 It is not a full explanation because it has still not been established precisely which deflators were applied to which estimates of gross output to yield the Table 2-3.3 results. This is another of those questions the answer of which is no longer worthwhile attempting to obtain.

1984/85, that neither the Petroleum and Coal PPI nor the Other Chemicals PPI gives a 'correct' result, although that yielded by the Other Chemicals PPI is much closer to the benchmarked PVMP (9,4 percentage points) than is the figure that results from using the Petroleum and Coal PPI (17,7 percentage points). Clearly, neither index was applied to the total value of output in the industry - indeed, one would not expect it to have been, since it is known that the industry produced a broad range of commodities, including petroleum products.

Table 2-6.5 Performance of the industry 'Other Chemicals' - pre-November 1993

i Unbenchmarked PVMPs				
	1978/79	1981/82	1984/85	1987/88
	100,0	112,9	113,8	116,7
	1979	1982	1985	1988
	100,0	105,4	108,2	124,1
ii Benchmarked PVMPs				
	1978/79	1981/82	1984/85	1987/88
	100,0	126,5	148,2	151,6
	1979	1982	1985	1988
	100,0	125,5	141,6	163,1
iii Deflated values of Gross Output (Constant 1985 Rm)				
	1978/79	1981/82	1984/85	1987/88
PPI - Other Chemicals	6 729	8 801	10 601	9 217
PPI - Petroleum & Coal	8 791	8 641	11 476	13 141
iv Deflated Gross Output Expressed in index form 1978/79=100				
	1978/79	1981/82	1984/85	1987/88
PPI - Other Chemicals	100,0	130,8	157,6	137,0
PPI - Petroleum & Coal	100,0	98,3	130,5	149,5
v Petroleum & Coal PPIs (1985=100)				
	1978/79	1981/82	1984/85	1987/88
	34,5	68,7	84,8	101,3
	1979	1982	1985	1988
	48,1	71,0	100,0	104,5
vi Other Chemicals PPIs (1985=100)				
	1978/79	1981/82	1984/85	1987/88
	45,0	67,5	91,8	144,4
	1979	1982	1985	1988
	48,1	72,7	100,0	154,4
vii Estimate implicit deflators from PVMPs				
	1978/79	1981/82	1984/85	1987/88
1978/79=100	100,0	154,9	216,6	289,7
viii Convert P&C PPI and OC PPI to 1978/79=100				
	1978/79	1981/82	1984/85	1987/88
P&C PPI	100,0	199,4	245,9	293,8
OC PPI	100,0	149,8	203,8	320,6

Source: Estimated from Statistical News Release P3041.3, 9 March 1992 (Benchmarked) and Statistical News Release P3041.3, 12 September 1990 (Unbenchmarkd)
 PPIs are estimated from the monthly figures given in Statistical News Release No. P0142.4 of 24 March 1992.
 Current price gross output estimates are from P3001, 28 June 1993.

Working backwards, one can estimate the weighted index (composed of the Petroleum and Coal PPI and the Other Chemicals PPI) which would yield the observed PVMPs. This has been done in Panel vii. Below this in Panel viii, the PPIs given in Panels v and vi have been converted to the base year 1978/79 so that a comparison with the Panel viii figures is possible. Knowing the weighted index and the two unweighted indices of which it is composed makes it possible to calculate the weights. By inspection, one may see that for the years 1981/82 and 1984/85, the value of the weighted index is close to that of the Other Chemicals PPI, implying that the proportion of industry output which has the Petroleum and Coal PPI applied to it is small. By 1987/88, however, the picture had changed, and the weighted index had moved very close to the Petroleum and Coal PPI. An attempt to estimate the relative weights of the contributions to total output of the industries (sub-groups) that make up the industry (major group) 'Other Chemicals' generated implausible results. This problem will be referred to again below when the reconstructed PVMPs are discussed.

If statistics are published with the intention of permitting users to apply them appropriately, then the manner in which, in this particular instance, the information on output and price levels was presented is guaranteed to thwart any sensible application. Of course, the need for secrecy impinged on the way in which the data were published, but even so, the presentation of the PPIs was particularly confusing. Once again, one is moved to complain about the poverty of the explanatory notes that accompany CSS statistical publications.

Having been provided by the CSS with the (minimal) information that the Petroleum and Coal PPI had been used as deflator on (part of) the output of the industry 'Other Chemicals', it was possible, on revisiting the published PPIs (and with a little detective work that should not be necessary), to go some way towards solving the riddle of which indices had been used where. Four separate indices, listed in the order in which they appear, are published: (i) Chemical and chemical products; (ii) Industrial chemicals; (iii) Other chemicals including products of petroleum and coal, and (iv) Products of petroleum and coal. Unbroken series from 1970 until 1991 are given for (i) and (iv), whereas (ii) and (iii) start in 1990. The key to the exercise (for those users whose hobby is solving thrillers from well-hidden clues) is the set of weights given with each series. Because the aggregate PPI of which the individual PPIs are components is for the 'production' sector (agriculture, mining, manufacturing, and electricity, gas and water), the sum of the industries making up the manufacturing sector is 69,52.¹⁴ Raising these so that

14 Here again, the CSS fails to inform users of what the 'sector' under consideration consists. Unless they actually add the different weights, inexperienced users could easily be misled into thinking that the weights given for each industry represent a percentage contribution to manufacturing. This is not a fatal error, but it is one that could easily be averted by the provision of proper notes.

individual industry contributions sum to 100, the following pattern of weights emerges: i - 6,65; ii - 3,16; iii - 16,12; iv - 12,64. A little arithmetic shows that the sum of (i) and (iv) is equal to that of (ii) and (iii), ie, 19,29. If one does not look at the weights when extracting the information (as I did not), but looks instead at the titles of the tables (as I did), it does not seem unreasonable to conclude that industry (i) (Chemicals and chemical products) is made up of two component industries (ii) (Industrial chemicals) and (iii) (Other chemicals including products of petroleum and coal). The order of presentation, with the separate series giving results from 1990 onwards, following directly below what appears to be the combined series, certainly encourages such a misinterpretation. Users might be tempted to conclude that the relative weights for the component industries of the 'whole' chemical industry are now 3,16 and 16,12, as opposed to 6,65 and 12,64 in the period before 1990 (SNR P0142.4, 24 March 1992, pp29-30). That, however, would be incorrect. It is now known, courtesy of SNR P3041.3 of 12 November 1993 that the PPI for (i) above (ie, Chemicals and chemical products) has been applied to 'Industrial chemicals and chemical products excluding products of petroleum and coal' for purposes of deflation (p37). In other words, the original presumption about which PPI to use was correct.

Another difficulty is that the list of industries for which PPIs are presented does not correspond to the list of industries that contribute to the aggregate PVMPs. If the CSS cannot perform a simple task like presenting a coherent set of PPIs (correct or incorrect) to users then the conceptual difficulties involved in estimating the volume and price indices for the chemical industry make it highly likely that confusion will reign there as well. Almost the full catalogue of rigours which a national accounting statistician would hope to be able to avoid have been imposed upon the hapless compilers of the series involved. In the first place, there was not one, but two major shocks to the series - the first when Sasol II came on stream, and the second when Sasol III followed it. At the time they were constructed, these plants were amongst the largest engineering undertakings in the world. The composition (quality) of output would almost certainly have changed as a result, and so too would the composition of inputs. Major technological change occurred, and hence production functions are likely to have been seriously affected. The system was also subject to severe shocks through rapid changes in the world price of crude petroleum.¹⁵ Domestic prices, although related to these movements, were manipulated by the state for strategic reasons.

15 The price per tonne of crude oil (Rotterdam spot price) rose from about 90 dollars in 1978, to 210 in 1979, peaking at about 260 in 1980, and tumbling thereafter to about 105 in 1985. See UNIDO, 1990, Figure IV.17, p273. To see the true effect on the South African economy, of course, one would need to convert these figures into rands.

With the publication in November 1993 of the reconstructed PVMPs the output of the industry 'Other Chemicals' was decomposed into those of its component industries, 'Chemical Products, excluding Products of Petroleum and Coal' and 'Products of Petroleum and Coal'. The relative performances of the two industries, as indicated by the reconstructed indices, along with attempts to construct weighted output indices are given below in Table 2-6.6 for both financial and calendar years. It is instructive to begin by looking at 'Products of Petroleum and Coal', for it suggests that the CSS' 'new' method of estimating indices may be as thoroughly misleading as that which it has recently abandoned. As may be seen in Panels i and ii, the growth performance of the industry 'Chemical Products, excluding Products of Petroleum and Coal', as measured in the financial years differs somewhat from that reported in the calendar years, although the differences are not uncomfortably large. For 'Products of Petroleum and Coal', however, there is apparently a 35 percentage point jump in the average output level from the 12-month period July 1987 to June 1988 to the period January 1988 to December 1988. The benchmarked PVMPs in Panel ii of Table 2-6.5, it will be observed, do not behave in this erratic manner. No explanation of this phenomenon is available, but it seems likely that the PPI is involved in some way.

Table 2-6.6 Performance of the two industries 'Chemical products excluding Petroleum and Coal' and 'Products of Petroleum and Coal' - post November 1993

i Calendar year PVMPs				
	1979	1982	1985	1988
Chemical products	100,0	113,9	117,8	137,3
Prod of Petrol & Coal	100,0	122,7	169,5	184,2
Weighted PVMP	100,0	117,2	145,1	156,9
ii Financial year PVMPs				
	1978/79	1981/82	1984/85	1987/88
Chemical products	100,0	120,0	122,5	137,5
Prod of Petrol & Coal	100,0	107,5	155,6	149,4
Weighted PVMP	100,0	115,1	144,8	143,5
iii Relative weights				
	1978/79	1981/82	1984/85	1987/88
Chemical products				
Gross output base	4,75	4,63	4,71	5,34
Net output base	5,18	5,54	5,18	6,21
Petroleum and coal products				
Gross output base	5,43	6,04	8,20	5,91
Net output base	3,49	3,63	10,66	6,29

Source: Estimated from Statistical News Release P3041.3, 12 November 1993.

In the explanatory notes that accompanied the reconstructed figures, the CSS pointed out that for the industry 'Products of Petroleum and Coal', attempts to measure physical volumes of output would continue to be made. This was justified by a reference to similar practice by

Statistics Canada (SNR P3041.3, 12 November 1993, p1).¹⁶ A commitment to direct measurement is all very well, but as argued earlier, the need to benchmark the PVMPs makes them vulnerable to the vagaries of an unstable deflator. The attempt to escape this in the 'new beginning' appears not to have been very successful. In the latest figures (with 1990 set equal to 100), the PVMP for the industry 'Products of Petroleum and Coal' falls from 71,6 in 1978 to 56,8 in 1979 (SNR P3041.3, 12 November 1993, p19) - a highly unlikely occurrence, one is inclined to suspect.

Direct comparisons with earlier estimates are not possible because the data for industry (sub-group) were buried as part of those for the major group 'Other Chemical Products'. It is known, however, and reported above in Panel iii of Table 2-6.6, that the contribution to net output (the basis on which weighted PVMPs are constructed) of the industry 'Products of Petroleum and Coal' to the total for the major group was almost two-thirds that of the other industry 'Chemical Products, excluding Products of Petroleum and Coal' for the years 1978/79 and 1981/82, and that in succeeding years its contribution overshadowed that of the other industry. Irrespective, therefore, of which year is used to supply a set of relative weights, a major output decline in this industry would have had a substantial impact on the PVMP for the major group 'Other Chemical Products'. The unbenchmarked results for the major group 'Other Chemical Products' for the year 1978 to 1979 recorded an increase from 88,0 to 92,4 (SNR P3041.3, 12 September 1990, p15), whilst the corresponding benchmarked figures show a modest decline from 71,8 to 70,6 (SNR P3041.3, 9 March 1992, p14) (1985=100 in both cases). No new material on volumes can have come into the possession of the CSS - it is simply not possible to return to producers after a lapse of so many years. The 'old' data have been reworked, and in the process, a new error has been introduced. The most likely culprit is the PPI - with 1990 equal to 100, the calendar year PPI for Products of petroleum and coal 1978 was 20, whereas that for 1979 was 32,0 (SNR P0142.4, 24 March 1992, p30). That rise is large enough to produce the reported decline in output.

Suspending this examination of the likely rôle of the 'Petroleum and Coal Products' PPI for the moment, and directing attention to the question of the combined (weighted) output of the industries, certain difficulties of index construction will now be raised. The CSS describes the process of constructing a weighted index as follows:

"Firstly a weight is allocated to every major which reflects the importance of the major group. The weights (weighting factors) for each intercensal period with January of the census year as base are based on the net output data obtained from the 1979, 1982, 1985 and 1988 manufacturing censuses. The indices of all the

16 As noted in Chapter 2-4, this is also done in Britain when the commodity concerned is 'well defined and homogeneous', as is the case with 'certain chemicals' and 'building materials'.

major groups are multiplied by the applicable weighting factors and aggregated." (SNR P3041.3, 12 November 1993, p2)

Reference is then made to the table of weighting factors in the SNR from which the (net output) figures in Table 2-6.6 are drawn, and the periods for which they apply, ie, 1979-81; 1982-84; 1985-87 and 1988-93.¹⁷ The series are linked:

"...by using the ratio of the January index which ends the one intercensal period and the January index which commences the following intercensal period." (SNR P3041.3, 12 November 1993, p2)

If one assumes away the difficulties caused by technical, output composition and quality changes within any individual industry, then a PVMP series can give a reasonable account of growth performance. Problems do arise, however, when industries grow at very different rates. Constructing an aggregate index under such conditions usually involves either frequent reweighting, as described above, or, if the results for the year 1984/85 can be treated as an aberration, a base year may be selected so as to avoid the shock to the economic system.¹⁸

The use of a particular set of weighting factors for a limited period (three years in three of the instances above) amounts to a change in base year relativities every three years, ie, it is tantamount to rebasing the output series every three years. In other words, the impact of the growth experience of the industry 'Products of Petroleum and Coal' has (and is allowed to have) much greater consequences for aggregate output in the period 1985-87 than it did in the period 1979-81 or 1982-84. This is also true for the longer period 1988-93, a provisional conclusion which may have to be revised once the 1991 census results become available.

If structural change in any period is not too marked, then as has been shown in Chapter 2-5, not too much damage would be done to the accounts by using the base year relativities over a fairly long period. Such is not the case in the present example. The two (incorrectly) weighted PVMPs in Table 2-6.6 give an indication of the implications of using inappropriate weights to generate aggregate PVMPs. Neither PVMP has been constructed correctly - they are simply the weighted averages of output in each year, using that year's weights. A comparison between these weighted PVMPs, which, apart from their end values, do not differ too much from each other, and the benchmarked PVMPs in Panel ii of Table 2-6.5 is of interest.¹⁹ The respective 1984/85 and 1985 values are quite close, but the 1981/82 and more particularly, the 1982

17 The reason that the last period is five years rather than three is obviously the absence of the manufacturing census data for 1991.

18 This latter procedure is suggested by Moll (1992, p194) for dealing with the gold price surge of the late 1970s, in particular the year 1980.

19 This crude technique for obtaining an approximation to the 'true' PVMPs was also used to estimate the aggregate output indices in Table 2-3.4 (Chapter 2-3) above.

values are not. It is possible that a properly constructed weighted index, in which the linking process was properly carried out might see the 1984/85 and 1985, and 1987/88 and 1988 values converging on each other. It is difficult to see, however, how the almost ten-percentage point difference in the 1981/82 figures could be reduced. By inspection, one may see that the weighted indices in Panel ii of Table 2-6.6 look approximately correct. In Panel iii, it may be seen that there is not sufficient change in the relative weights between 1978/79 and 1981/82 to make much of a difference to the 1981/82 figure.

All in all, a most baffling business, and one which raises an interesting question - if the major cause of the difference between the benchmarked and the reconstructed figures is the error made in estimating the level of output in the industry 'Products of Petroleum and Coal', why has the PVMP for the industry not grown much more substantially? As noted above, 'proper' construction of the weighted output index for the industry does not look as though it would raise the end values by much. It may be worthwhile recalling that the difference between the aggregate benchmarked PVMP and its reconstructed counterpart for 1985 was 8,3 percentage points and for 1988 it was 7,4 percentage points. The proportional contribution to total output (weight) of the industry 'Products of Petroleum and Coal' was only about six percent in 1988 - clearly, it would take a huge revision in output levels in a single industry to have such an impact on total output.

To explain adequately the differences between the reconstructed PVMPs and the benchmarked figures would entail an attempt to replicate the reconstructed PVMPs. Without considerable assistance from the CSS, such a project is unthinkable - for a start, it is not known what adjustments have been made to the figures for the value of sales before these were deflated to yield the indirect output estimates now underpinning the PVMPs. This matter will be pursued no further - the most likely explanation for differences between the reconstructed PVMPs and the benchmarked figures seems to be that they reflect the sum total of many upward revisions, rather than a single major change caused by the rectification of the error made in the estimates of the output in the industry 'Products of Petroleum and Coal'. That this is probable may be seen by glancing at the two sets of errors for the year 1985, given in Table 2-3.1. There appear to be several fairly substantial upward revisions in the reconstructed figures. An understanding of the rôle of the 'Petroleum and Coal Products' industry will be pursued instead by examining the prospects for determining output levels by estimating the value of Real Net Output.

Before doing so, a curiosity raised by the index weighting procedure followed by the CSS will be considered. It was noted above that the manner in which the output series had been

weighted (and linked) amounted to a rebasing every three years. In the limit, as the period between rebasings becomes shorter, the resulting index approximates a chained Laspeyres index. After all, chain indices are produced by "...cumulating the short-term movements..." in price and volume series (UN, 1993, p387). 'Short-term' is a somewhat arbitrary description - the practical reason for stipulating short periods is the need to match products in consecutive periods. As the period grows in length, old products disappear and new ones appear. Quality also changes over time. If, however, relative stability in price and quantity relativities persisted for periods of roughly three years in a particular economy, then that period would qualify as 'short-term'. This seemingly esoteric argument is offered to make a point about the impact of the see-saw behaviour of the net output weight of the 'Petroleum and Coal Products' industry disclosed in Panel iii of Table 2-6.6 above.

In the theoretical discussion on index construction in Chapter 2-4, it was noted that under certain conditions, the use of chained Laspeyres indices was not advisable. If the period of rebasing were anything less than three years, or alternatively, if three years were regarded as 'short-term', then the conditions recorded in Table 2-6.6 would be likely to raise questions about the use of chaining. The latest SNA states that:

"A chain Laspeyres, or Paasche, index should not be used if the chaining involves an economic detour; i.e., linking through a period, or periods, in which the sets of relative prices and quantities differ more from those in both the first and last period than the latter do from each other." (1993, p388)

Both the net and the gross output weights for 1984/85 constitute such a period, and under these circumstances, a substantial difference would open up between the Laspeyres and Paasche indices, indicating that chaining was not appropriate. Although the South African conditions were remarkably unstable over the period, it is not suggested that the output indices have fallen foul of this problem (goodness knows, there is enough wrong with them in other respects) - the detour above was undertaken to show what can go wrong. The reason for raising the point is a simple one - the use of particular index formulae cannot be justified by mere reference to international best-practice. A procedure which gives acceptable results under one set of conditions may not do so under others - the process of adjusting to changes involves the constant exercise of judgement as to what is appropriate for the particular circumstances. There is, above all, a need to be flexible in matching available measuring tools with the conditions in which they are to be applied.

On the future of the Petroleum and Coal Products PVMP

In conclusion, it is argued that the CSS' new method of valuing output in manufacturing appears not to be able to escape some of the problems that beset its predecessor. There are grave doubts about the wisdom of using the Petroleum PPI as an industry deflator. These doubts are exacerbated by the unknown impact of the timing of price increases in relation to the dates at which the manufacturing censuses were conducted, especially the 1978-79 census. In addition to this, as net output rises as a proportion of gross output, the influence of the petroleum price diminishes, and the other factor costs involved become more important. This means that the crucial 1984/85 deflator is more than usually sensitive to two separate sets of price changes, neither of which is easy to measure accurately.

The travails of the Petroleum PPI, PVMP and the deflated gross output estimates illustrate the breakdown problem in national accounting with peculiar intensity. Additivity need not even be mentioned, nor need there be any pretence that in the rebased accounts, that previously observed growth rates have been maintained - these were given by the unbenchmarked PVMPs, and as may be seen by the CSS' own calculations, have proved to be hopelessly incorrect. It seems unlikely that a seriously under-staffed CSS could possibly have given this complex issue the attention it deserves, a claim that is more than amply borne out by the huge gaps between the benchmarked and unbenchmarked PVMPs, and more recently by the large difference between the benchmarked and the reconstructed aggregate PVMPs for the critical year 1985.

Returning to the broader question of changes in the ratio of net to gross output, it is clear that a detailed investigation is called for. A sufficient number of large-scale deviations have been identified in Table 2-6.4 to justify such an enterprise. There are not so many possible explanations for the changes in the ratio of net to gross output over the period as to make an investigation impossible. There is, as noted in Chapter 2-3, a powerful contender - it seems highly likely that as the two Sasols started to produce, there would have been a slowdown in the rate of growth of the price of intermediate inputs for the industry as a whole. Instead of the industry expanding by using more (expensive) crude petroleum inputs, it grew by using (cheap) coal in two plants that account for some large proportion of total output in the industry. The calculation involved is not simply a comparison between the respective trend lines of the costs of the two inputs, but rather an examination of the absolute changes involved when inputs with price P_1 and energy content E_1 are substituted for inputs with price P_0 and energy content E_0 .²⁰

20 How much substitution took place in the period under consideration here is not known, but in 1993, Sasol was said to produce over 40 percent of the transport fuels. Source: interview with Engen Chief Executive Officer, Rob Angel on Radio South Africa, 13 October 1993

If the price of intermediate inputs falls, or more realistically, does not grow as quickly as output prices, then the ratio of net to gross output must rise, other things being equal. In industries other than 'Products of Petroleum and Coal', the circumstances may have been such as to allow producers to engage in price over-recovery.²¹ This could lead to the observed effect. Whether or not it is sensible to talk about price over-recovery in an industry like petroleum, where the state has intervened so thoroughly, is a moot point. Where 'gouging' takes place, unaccompanied by 'genuine' reductions in the prices of inputs relative to outputs, then the use of a deflated gross output measure may overstate output levels, something which an accurate physical volume measure should reveal. Where the relative price changes result from the Sasol-type experience, then extreme care needs to be taken in using indirect output measures based on gross output. Even if based upon a correctly measured PPI, it is by no means certain that such measures would not understate output levels.

As far as the other affected industries are concerned, there are several possibilities that need to be considered. One is that more value has been added to the same quantity of raw materials to produce commodities of better quality. Physical volume measures used under these circumstances would simply register zero gains, or even deteriorations in productivity. This does not seem a very likely explanation in the case of the industry 'Products of Petroleum and Coal', but it may well apply to some of the others.

A rising net/gross output ratio might also be observed if the costs of labour and/or capital rise relative to other inputs, without a corresponding increase in physical outputs. This is a possibility in the South African case, but the absence of a reliable PVMP makes it even more difficult than it otherwise would be to check. The only way to determine whether these increases are 'justified' is by reference to changes in the 'productivity' of each factor of production, jointly and severally. Measuring these changes is easier said than done. Even relatively sophisticated cost accounting devices like the NPI's REALST model which compares the rates of growth of factor incomes with the rate of growth of output cannot operate without some reasonably accurate measure of the physical volume of output. Nor can REALST overcome the basic problems of attribution involved in the measurement of the productivity of co-operating factors (Meth, 1991c and 1991d).

The question of what impact changes in the net to gross output ratio will have is thus not so readily answered - if net output rises as a proportion of gross, then the use of a survey such as

21 Price over-recovery is a term used by the NPI, usually in conjunction with results that flow from the use of the REALST model. It refers to the ability of a firm to increase profits (or reduce losses) simply by raising prices. In a competitive market, this would not be possible. This constitutes a form of 'gouging' of excess profits out of consumers or end-users.

the MSI (based as it is on gross output) could cause the constant price output estimates so derived either to overstate or understate the 'true' value of production. Unless a reliable physical volume index exists (and even the most reliable of these cannot measure quality and technological changes adequately in the medium term), no amount of juggling with national accounting tools and conventions will answer the questions posed above. In circumstances where the change in the ratio of net to gross output is held to constitute a 'problem' it may be that simulations of the type I have used above, but in much greater detail, could uncover the reasons. It goes without saying that if the existing PPIs are inaccurate they will hinder any such undertaking. Although the problems encountered in making physical volume measurements may not be capable of solution, matters would certainly be helped if the reliability of the PPIs could be improved and if the production of the manufacturing censuses could be speeded up, and their frequency increased. It is a matter of some concern that the full results of the 1988 Manufacturing Census are not yet available. In Britain, as Kingaby points out, there is an annual census of production (1989, p105). It is scarcely imaginable that the results of this activity only become available after a delay of four or five years. With these difficulties in mind, we turn now to Chapter 2-7, where an attempt is made to examine the theoretical and practical problems of estimating real net output.

Chapter 2-7

"...it must be admitted that forming clear expectations about the relationship between alternative [Constant Dollar Value Added] indices is a rather more hazardous business than is commonly recognized." (David, 1962, p154)

Real Net Output to the rescue?

Introduction

Upon discovering that there has been a change in net output in current prices that is proportionately much larger than changes in price levels, an intuitive response is to suspect that it would, or could translate into a similarly large change in constant prices. Where the increase is accompanied by rapid changes in the ratio of net to gross output, as has happened in the case examined in this study, the possible use of deflated net output, if only as a backup, deserves serious consideration. Because of the conceptual difficulties involved, it is conventionally¹ held that net output in current prices ought not to be deflated to yield an output measure. Unfortunately, indirect volume estimates become so unreliable when the ratio of net to gross output changes significantly that they cannot be used - the problem of estimating real net output simply has to be confronted. Certainly, the problem ought not to be dealt with by ignoring it, as the CSS has done, nor is it sufficient to dismiss it by referring in passing to the fragility of the other indirect measures.

Two obstacles stand in the way of anyone attempting to estimate real net output, the one empirical, the other theoretical. It is not clear which of the two is the more formidable. It has been noted that the CSS has now switched from using gross output as a basis for estimating indirect output indices for manufacturing in favour of some estimates of the value of sales. In my first published report on the problem of the errors in the manufacturing sector output estimates, I went through this exercise, and offered the results (tentatively, given their apparent fragility) as an additional critique of the then existing (unbenchmarked) estimates. Those figures are reproduced below in Table 2-7.1, alongside the CSS' three attempts to estimate the value of manufacturing output.

1 See for example, the discussion in Meth (1992, p48) on the problems British national accounting statisticians have identified when attempts are made to estimate a real (constant price) income figure from the value added data.

Table 2-7.1 Deflated sales and three CSS attempts at estimating manufacturing output

Year	Unbench- marked	Bench- marked	Recon- structed	Deflated sales
1979	100,0	100,0	100,0	100,0
1980	110,4	110,3	111,8	111,9
1981	117,5	119,8	122,1	121,8
1982	113,8	116,8	115,7	116,7
1983	107,4	112,6	113,7	114,3
1984	110,4	117,3	120,3	121,5
1985	104,7	109,5	117,8	116,1
1986	103,2	108,0	116,7	111,0
1987	105,9	111,1	120,2	111,7
1988	113,8	118,8	126,2	119,3
1989	114,3	119,7	127,9	122,0
	1	2	3	4

Source: Deflated sales and Unbenchmarked figures: Meth (1992, Table 9).
 Benchmarked estimates: Meth (1992, Table 14).
 Reconstructed figures: SNR P3041.3, 12 November 1993.

Once again, one is struck by the extent of the concession to error made by the CSS, and the closeness of the attempts to estimate alternatives to their figures (at least up until 1985) in the period before they proved willing to acknowledge the error of their ways. Even so, notwithstanding the fact that the set of value of sales figures used to estimate the Column 4 results above had been cleaned up by the CSS (SNR P3041.4, 4 December 1990) so that some of the sillier anomalies pointed to in Meth (1992, p36) had been removed, I placed on record my reservations about their trustworthiness (1992, p36). A glance at the reconstructed figures above for 1986 onwards in relation to the value of sales counterparts shows that that scepticism was justified. Clearly, the sales figures have been cleaned up yet again. Although the revised figures move in the direction one would want them to for the purposes of the arguments advanced here, one is still justified in asking why they should be regarded as being any more reliable than those they replace.

In order to discover what changes have been made to the value of sales figures, the usually arduous process of making inquiries of the CSS would have to be gone through. In this case, such effort would be wasted because it is known that the value of sales figures must suffer from the same defect as the gross output figures, ie, they cannot take cognisance of changes in the net/gross output ratio. That raises an interesting problem - is one obliged to come to grips with the most recent set of changes to the estimates, knowing them to be based on shaky foundations, when one is about to embark on an exercise of a very different type? Trying to understand the reconstructed (value of sales-based) figures could involve almost as much effort again as has been expended on trying to understand the gross output-based figures. The

sensible course of action, it would seem, is to press ahead with the examination of the net output problem, acting as though the reconstructed (November 1993) figures were merely another set of benchmarked PVMPs. Unless that is done, the study could drag on forever. The problem of the adequacy of the value of sales-based figures can be treated separately from the problems addressed here. In recognition of the fact that new figures are available, a brief comparison will be made of the differences between these figures and the unbenchmarked estimates to see whether those industries in which the net/gross output ratio changed substantially were more susceptible to errors in volume levels. Other than that, the reconstructed figures will not be analysed any further here.

This brings us to the second obstacle confronting the study at this point - the formidable theoretical problems of estimating real net output. The problem here will be shown to be twofold - in the first place, deflating net output (value added) is problematic. Even more serious however, is the problem that arises when the conditions for a value added aggregate to exist - the separability of capital and labour (a concept explained below) from intermediate inputs such as energy and materials - are not met. The distinct possibility arises that in the production function for an industry like Sasol's oil from coal, and the heavy chemical industry more broadly, substitution may be between capital and energy and materials rather than capital and labour. Techniques exist for testing for this, but (long) time series data are required so that the different models and parameters can be estimated (Norsworthy and Malmquist, 1983). For the South African manufacturing sector, for most of the critical variables there are but four observations available in the relevant period (1979, 1982, 1985 and 1988) - for some of them, none at all. Regression analysis can be a sensitive business at the best of times - the circumstances spelled out above would seem to rule out its use altogether.

That being so, one is left dangling, so to say. Yet something must be done - a means has to be found to estimate the value of Sasol's contribution to the economy. One possible line of attack is suggested by Moll (1990). Confronted by a disjunction between the assumptions underpinning the national accounts (competitive, efficient markets functioning in equilibrium and rational individual behaviour) and the reality of the South African economy, he advocated a pragmatic view of the problem, stating that:

"...[his] approach ignores the theoretical baggage and regards the national accounts and productivity research as techniques or tools, to be evaluated by their usefulness. If they give useful results they should be maintained or improved, if not, they should be modified or scrapped - a 'commonsense' approach with some support in recent philosophy of sciences debates (Fine, 1986). But the specifics are tricky: for what purpose are the figures useful? how useful? how should the system be modified? Lacking a "pragmatic criterion of predictive success" (Hesse, 1980: 190) by which to ensure instrumental advance in the long run, economics must resort to somewhat *ad hoc* measures of usefulness." (p71)

There are not many alternatives, in the present situation, to the course of action suggested here - the theoretical problems will be spelled out, but they must be ignored, and a crude attempt at constructing output estimates that take account of the changing net/gross output ratio changes will be made. Of course, the questions regarding the usefulness of the result must be faced - all one can say in their defence is that the existing estimates, because of their manifest failure to pay specific attention to the problems faced here, are unlikely to be more useful for serious academic analysis.

The order in which the material is presented is as follows: first, a brief survey of some of the relevant literature will be given. In the survey, the general problems of aggregation and index number construction are considered, and an attempt is made to relate the problems identified in abstract terms to the peculiarities of the South African chemical industry, and in particular, the coal-based Sasol plants. Since it is proposed that real net output be estimated, the next step is to examine the concepts of net output and value added, and the ways in which estimates of the real value of this variable have been or may be estimated. Thereafter, the results of the calculation exercise undertaken in Appendix 2-7 will be commented on. It is not claimed that the results produced are anything but crude. They are unlikely, however, to be as crude as some of the error-laden CSS estimates, and for that reason alone are worthwhile presenting. Developing practical procedures to address the formidable deflation problem in national accounting practice addressed here should be given some priority by the UN.

Index numbers and the limitations of the SNA

Although there have been important theoretical advances in recent times in index number specification and construction, the quality of the indices commonly used² to represent economic reality may be deficient because the search for universally acceptable methods of preparing national accounts, as embodied in the SNA, leads to the use of inferior methods. Recommended index forms that perform tolerably well under 'normal' conditions, could well be inadequate under stress. Acceptance of simple forms like Laspeyres may be done for reasons of economy (in terms of data required) but the end result could be extremely misleading. This is important because serious breakdowns in the performance of these indices strike at the heart of the discipline. Hansen and Lucas, noting that:

2 There could be important differences between what academics do with national accounting data and the ways it is used by more 'popular' analysts, such as financial and economic commentators employed by the 'serious' press and other media, or possibly even the economists employed by the state. Academics have the time to compute sophisticated measures like Divisia or Törnqvist indices (Star, 1974; Moll, 1990) - it is unlikely that others will. Unfortunately, non-academic users of national accounting data are likely to be more influential in the processes of policy formation.

"...the purpose of index numbers is to enable the quantitative treatment of useful composite commodities..."

observe that:

"It should hardly be necessary to argue that economics as we know it depends entirely on our ability to measure and theorize about the economic properties of such composite commodities as "real wages" and "producer goods". If one would pay the price of rigor and deal only with homogeneous goods, economic theory and policy based on any level of aggregation would evaporate. Composite commodities are completely integral to economic inquiry in exactly the same way generality is integral to all scientific inquiry. On the basis of similarities we group specific items into categories which allow us to generalize about untested properties and to predict unseen or future events. Whether categories exist in reality or only in the human mind, this assumption is absolutely essential to all rational and organized human thought.

Our problem, then, is that we are attempting to deal quantitatively with an idealized concept rather than more mundanely with a real measurable phenomenon. This is fundamental and endemic to the social sciences generally, although perhaps less serious in economics. Therefore, as a measure of our idealized concept we look for a "proxy" which is a real and measurable phenomenon in some sense congruent with or consequent upon our idealized concept." (1984, p26)

Whether or not attempts by economists to create adequate composites have succeeded is a matter for debate - some take the strong view that they have not. Commenting on the weaknesses of the aggregation procedures which underpin index number construction, Gorman distinguishes two types of aggregation - 'economic' or 'endogenous' aggregates and 'statistical' or 'mechanistic'. Of the former he states that:

"They have proved pretty comprehensive failures, though they have yielded useful byproducts. The conditions for Capital or Labour aggregates to work perfectly, for instance, are completely unrealistic, so that they seem unlikely to work decently in practice except in very favourable circumstances. Even if they did work on the production side, it would be because they reflected its structure: commonly quite different from that of the household sector, so that they could not normally be used in a complete model of the economy." (1985, p83)³

An aggregation process that lumps petrol and associated by-products produced from crude petroleum with those produced from coal may be justifiable in some sort of taxonomic sense, but it certainly raises interesting problems for economic analysis. Leaving that aside, however, questions also arise about what it is that economists are looking for when they attempt to measure real output. An article by David cites a suggestion made by Geary in 1944, ie, almost a decade before the first version of the SNA appeared, to the effect that:

"...indices of gross output be computed "to show the 'visible' quantum of goods available and the net index to show the trend in the amount of work done in the industry." " (David, 1962, p149n)

3 The problems of aggregation have been addressed by a veritable multitude of commentators. The well-known 'Capital Controversy' between the two Cambridges, centred around the Cambridge, UK, argument that it is impossible to construct an index of the quantity of capital (Harcourt, 1972). As Nadiri (1970, p1144) points out, similar problems apply to labour.

Implicit in this is at least the possibility that the two indices, gross and net output, could change at different rates. Otherwise, why bother to compute separate indices? The switch from one form of intermediate input to another in the case of the 'Petroleum and Coal Products' industry in South Africa, in conjunction with the practice of using the deflated value of gross output to yield proxy measures of both the volume of 'the visible quantum of goods' and the 'amount of work done' poses an interesting problem. For a given 'visible quantum of goods' in this case, say, 1 000 000 litres of 93-octane fuel, not only does the value (and type) of intermediate inputs change, but so too, in all probability, will the 'amount of work done in the industry'. The latter change occurs because the production process of oil from coal (adding value to coal) is different from the conventional refining of crude petroleum. Clearly, such changes render simple-minded application of rules which permit estimates of deflated gross output to be used as a surrogates for volume measures (PVMPs) questionable.

Accustomed as they are to conceiving of change in marginal terms (and of assuming that individual producers can make but a negligible impact on total output), economists may be forgiven for overlooking the possibility of technical 'progress' of the Sasol type. Certainly, when the SNA came to be formulated, no provision for dealing with instances of this sort was made. The latest SNA pays as little attention to the problems addressed here as did its predecessors in 1953 and 1968. Weaknesses of the 1968 SNA, far too numerous to consider here, are discussed in a note by Schimmler (1983), who observes in the summary to it that it:

"...gives instances in which traditional national accounts, as established according to existing rules and statistics, may not suffice for actual data requirements, yielding differences in growth rates of several percentage points." (1983, p323)⁴

A flurry of activity during the 1980s at the United Nations preceded the publication of the latest SNA. Practitioners in the field had ample opportunity to contribute to the deliberations - one notable occasion being the Nineteenth General Conference of the International Association for Research in Income and Wealth, held in 1985. Some of the major contributions were published in a special edition of *The Review of Income and Wealth*, and it is clear from even the most cursory reading of one of them, a paper by van Bochove and van Tuinen (1986), that fundamental questions about the very nature of the SNA were posed. These authors, both senior officials in the Netherlands Central Bureau of Statistics, concluded that "...the purposes of the SNA...frequently conflict with one another..." (p127), primarily because of the multiplicity of uses to which the accounts are put (p128). Whether or not the problems to which they refer have been addressed satisfactorily in the latest version of the SNA is a

⁴ Star discusses this problem in passing in an extensive analysis of the correct size of the Abramowitz residual (1974, p127).

question which lies well beyond the scope of this study. It is clear, though, that at least one of the major problems referred to by Schimmler has not been tackled. In discussing some of the conceptual dilemmas faced by national accounting statisticians, he observed that:

"...it may be recalled that output shows different growth rates from value added (which is output minus input) when growth rates of outputs and of inputs differ - which is normally the case, and this will be particularly substantial at times of major structural changes." (1983, 329)

There is no indication that the latest SNA has even considered this particular problem - the Chapter XVI on Price and Volume Measures (UN, 1993) does not refer to it. That being so, users are on their own - and free to specify any index, within reason, whose computation can be defended. For the purposes of the present argument, the interest is in gauging the impact of the two Sasols on the manufacturing sector output estimates. The measure most likely to give insight into this is real net output (value added in real terms or at constant prices). That, after all, will give some indication of the fruits of the massive capital investment that took place, and of the human labour that has gone into the production process. The problem is to find an adequate way of converting it from the current price terms in which it is measured to real or constant price terms.

Net output - what is it?

The problems of measuring real net output have not yet been solved, either officially or unofficially, even though they have attracted the (sporadic) attention of economists over a lengthy period.⁵ Not only that, although net output and value added (in their current price form) are used interchangeably by many economists, others appear to take the two concepts to mean quite different things. This may be of more than mere semantic significance, as is suggested below. More broadly, there is a debate about the appropriateness of using the GNP (GDP) or value added approach to productivity analysis (Nadiri, 1970; Norsworthy and Malmquist, 1983).

To commence the discussion, the meanings of these concepts will be explored. First, the definition of net output used in the manufacturing censuses conducted by the CSS:

5 A recent review paper by Deaton (1993) that looks at the quality of data for development in relation to the econometric techniques applied to these data, refers to the conceptual index-number problems encountered in the use of concepts such as GDP. These apply both to domestic analyses and to international comparisons of income and product. Deaton makes no references to any advance on the standard works (see below) on this topic. This, coupled with the treatment the matter receives in the latest SNA (UN, 1993) must incline one to believe that the basic problems remain unresolved.

"Net output is the gross value of output less cost of materials used. It represents the value added to the cost of materials by the process of production. Broadly speaking, it comprises salaries and wages, overheads and profits."

It is not possible to see from the manufacturing censuses precisely which items of expenditure are treated as 'overheads', a matter on which the financial (business) accounting profession is somewhat hazy. Overheads, would however, certainly include rent and the large items of depreciation and interest charges, both of which are part of 'gross operating surplus'. Materials costs are described thus:

"The cost of materials has been calculated by adding the value of opening stocks to, and deducting the value of relevant closing stocks from the purchases of materials and payments to other establishments for the manufacturing or reconditioning of goods.

Materials include the following:

- Raw materials, semi-processed goods and components for use in the manufacturing process as well as raw materials for use in the erection of buildings or the manufacture of equipment by own employees and capitalised.
- Fuel, light, power, steam and gas.
- Packing materials.
- Consumable and maintenance stores." (Report No. 30-01-01(1985), ppxi-xii)

Definitionally at least, the concept of net output used in the manufacturing censuses ties in quite closely to the national accounting concepts. In the explanatory notes to the CSS version of the accounts the following definitions are provided:

"Factor income, payments: Remuneration for the services of the production factors, labour and capital.

The gross domestic product at factor incomes may be defined as the total remuneration, before deduction of depreciation allowances, of the production factors, labour and capital, employed in the domestic sector." (*South African Statistics 1990*, p21.31)

The notes make it clear that depreciation is part of gross operating surplus. The current price GDP series for the manufacturing sector (*South African Statistics 1990*, p21.9) cannot be reconciled exactly with the manufacturing census estimates⁶ (Report No. 30-01-01(1985), p2 and SNR P3001, 28 June 1993, p6), although they are close enough to suggest a firm relationship. That they cannot be reconciled is interesting, for they appear to refer to the same entities. As is made clear in SNR P3001, of 28 June 1993 gross output in manufacturing has subsidies added to it and indirect taxes deducted - net output is then derived by subtracting the value of intermediate inputs. GDP used always to be referred to in *South African Statistics* as

6 Recall that the census figures are for financial years and the national accounts estimates for calendar years.

being at 'factor incomes', but that is precisely what gross output minus taxes and intermediate inputs plus subsidies is.

Whilst on the topic, the fate of the concept 'factor incomes' in the latest SNA deserves a mention. Embedded in an examination of the different sets of prices (basic, producer's or factor cost) at which gross value added can be measured is the following discussion:

"6.230. The conceptual difficulty with gross value added at factor cost is that there is no observable vector of prices such that gross value added at factor cost is obtained directly by multiplying the price vector by the vector of quantities of inputs and outputs that defines the production process. By definition, "other taxes or subsidies on production" are not taxes or subsidies on products that can be eliminated from the input and output prices. Thus, despite its traditional name, gross value added at factor cost is not strictly a measure of value added.

6.231. Gross value added at factor cost is essentially a measure of income and not output. It represents the amount remaining for distribution out of gross value added, however defined, after the payment of all taxes on production and the receipt of all subsidies on production...

6.232. Claims on gross value added, other than payments of taxes, less subsidies, to government used to be described as "factor incomes". While the concept of factor income is no longer used in the System, gross value added at factor cost could be interpreted as measuring the value of the fund out of which so-called "factor incomes" can be paid: it follows that it is equal to the value of the "factor" incomes generated by production." (UN, 1993, p154)⁷

Internationally, the conventions and concepts employed are a little different, and unfortunately, not consistently so. The concept 'gross value added' in the latest SNA (when measured as described above) is close to the CSS' 'net output' as used in the South African manufacturing (and other sectoral) censuses. In the SNA, this definition is given:

"The gross value added of an establishment, enterprise, industry or sector is measured by the amount by which the value of the outputs produced by that establishment, enterprise, industry or sector exceeds the value of the intermediate inputs consumed..." (1992, p21)

'Gross' as used here, refers to the fact that a deduction for the consumption of fixed capital has not been made. The terminology used elsewhere is not the same - in Australia, for example, the production (value added) approach to the measurement of GDP is defined as follows:

"The unduplicated value of industrial production can be measured by taking the market value of the goods and services produced by an industry (called its *gross output*) and deducting the cost of goods and services used up by the industry in the productive process (*intermediate consumption*) which leaves the value added

7 The CSS still uses the concept 'GDP at factor incomes'. See *South African Statistics 1993*, pp21.7ff.

by the industry (also called its *gross product*).⁸ GDP is then obtained by summing the gross product of all industries." (Castles, 1990, p7, para.2.23. See also para.2.15) (Emphasis in original)

Of note in the discussion above is the fact that no firm distinction between value added and net output (or whatever concept is used as equivalent for it) is made in the sources quoted. By contrast, a number of academics distinguish quite clearly between the two. Star, for example, argues that the error caused by the inappropriate use of value added rather than the output concepts implied by production theory accounts for some significant part of the Abramowitz residual.⁹ Another large part of this residual, the difference between recorded economic growth and the half to two-thirds of it which economists have been able to account for, is said by Star to be caused by using aggregated rather than disaggregated data (1974, p123). In brief, Star operates with:

"...a production function with n inputs and one output. The dependent variable, *output*, is net of any production used within the industry. The total production of the industry including that production used within the industry is *gross production*. In order to be consistent, the material inputs (as compared to capital, labor and land) need to be divided into two components: materials that come from outside the industry, which I shall call *outside materials*, and materials that are obtained from within the industry, either at the final output stage or at some intermediate stage, which I shall call *inside materials*. Inside materials can then be subtracted from gross production to get output. Usually, however, these data are not available, and one is forced to subtract the total materials (inside and outside) from gross production ending with *value-added (VA)*. Value-added may also be thought of as output less outside materials. Value-added is not used because it is correct but because it seems better than using gross production and because the alternative of using output is simply not usually available." (1974, p127) (Emphasis in original)

At this point in the text there is a footnote citing Kendrick to the effect that:

"Real net output estimates consistent with national income, rather than value-added would be preferable. Again due to lack of data, we have to be satisfied with real value-added estimates as a first approximation to the real net measure." (p127n)

Unless national accounting statisticians in the US had developed some satisfactory deflator to allow single deflation along lines to be discussed below (and currently accepted by the UN as a second-best solution to the problem of estimating value-added in constant prices), the real value-added estimates referred to here must have been obtained by double deflation. In any event, Star continues by stating that:

"This practice [of using value-added] probably is derived in part from the national accounts in which all intermediate goods net out and in part from a lack of a suitable alternative."

8 As a matter of interest, the use of gross, as opposed to net concepts has been critically analysed by Bos (1992). He argues that a net concept is to be preferred because it does not ignore capital consumption as a category of cost, and he points out that for certain policy purposes, eg, wage negotiations, pensions, fringe benefits, government debt, the differences he finds between Net Domestic Product (NDP) and GDP (NDP growth was apparently systematically lower than GDP growth) may be of substantial importance (1992, p276).

9 That part of the growth in output not accounted for by the increase in the factors of production.

The merits of Star's position will not be pursued here¹⁰ - suffice it to note others have commented in similar vein. In a comparative study of manufacturing sector productivity in the US and Japan, Norsworthy and Malmquist argue that the GNP (value-added) framework may be defensible for very large aggregates, such as the US economy, but that it is inappropriate for "smaller sectors" (1983, p947). Apropos of the concepts themselves, these authors note that they use the more familiar term "value-added" rather than "gross product originating" because the latter is less readily distinguished from "gross output". They do insist, though, that it is the "gross product originating concept in GNP accounting" to which they and others refer (p947n).

In an explanation of the difference between the two concepts which, at first sight, seems somewhat less obscure than that offered by Star, David (1962) distinguishes between value added and gross product originating by noting that value added is the:

"...value of gross sales, at producer's prices, less the cost of raw materials (including fuel and energy) purchased from other industries (and/or firms within the same industry) at delivered prices."

Gross product originating, on the other hand, can be defined as:

"...the market value of the industry's output, less the expenditure on purchases of all intermediate goods and services."

David observes that:

"Alternatively, gross product originating could be obtained as the sum of factor payments (i.e., income originating) and non-factor charges. In practice, the problems of allocating the non-factor charges, especially indirect business taxes, have, along with other difficulties, led to the use of the residual definition." (p148)

Reading and rereading these definitions, it is by no means clear what significance to attach to the apparently slight differences between. What emerges from the somewhat unsettling exploration above is the conclusion that although apparently there is agreement on the definitions and meaning of the concepts 'net output' and 'value added' among practising national accounting statisticians and UN officials (despite the fact that the names that the different entities go under differ from country to country), these concepts are taken by several academics to differ, one from the other. In passing, we note that the CSS simply equates net output and value added in production, and we note further that this concept is (a little confusingly) 'gross', because depreciation allowances have not been deducted from it.

10 If Star's claims have not been refuted, and a scan through the growth accounting literature has not turned up a refutation, then it is, at first sight, a little surprising that his approach appears not to have carried over into the prolonged debate to which the search for explanations of the residual has given rise.

The broader context within which this debate is situated will be returned to below. For the purposes of this study, there is no alternative at this point to working with the official definition and the figures which it generates. Even as we do so, though, we should be aware of the unresolved problems arising from Star's (1974) claim that the use of one concept rather than another can lead to misinterpretation of the growth data. We should bear in mind as well, a finding by Norsworthy and Malmquist (1983) which suggests that the use of value added instead of gross output data leads to serious error - a view recently challenged by Yuhn (1991). In other words, caution is necessary.

Estimating real net output

It may be recollected that in Chapter 2-6 there was a short discussion on the national accounting statistician's ideal world. In that discussion is embedded an implicit ordering of output measures according to quality. Direct measures are better than indirect, so best of all, if it were measurable, would be an estimate of physical volumes that took into account quality and technological changes.¹¹ With such a measure to hand, it would be possible to decompose price and productivity changes in production with precision. Where indirect measures have to be used, the preferred approach is double deflation. Where the data permit, this way of estimating real value added has long suggested itself as the obvious method. Constant price estimates of value added and gross product originating emerge as residuals from the separate deflation of input and output values, but are nevertheless "...not infrequently interpreted as representing real commodity flows" (David, 1962, p149). As Arrow (1974, p4) points out though, Stone and Prais had demonstrated as long ago as 1952 that inconsistencies were:

"...likely to show up with special force in those items which represent differences between well-defined flows - items such as value added or balance of payments."¹²

11 Recall here the suggestion by Geary, cited above, that measures of the 'visible quantum of goods' and the 'amount of work done in an industry' be computed. As long as there are no quality and technological changes, the former serves as an exact proxy of the latter. Direct measures are preferred because estimates of the amount of 'work done' (value added) can obviously only be obtained indirectly. Their estimation leads one into the difficulties of valuing the services of 'capital' and of producing constant price estimates of the value of labour services.

If it were possible to obtain direct measures of output which took account of technological and quality changes, the Sasol-type problem with which this part of the study is concerned would not arise. The fact that the ratio of intermediate inputs to net output had changed would be picked up immediately. This study owes its existence to the fact that accurate direct measures cannot be made.

12 Aspden (1990, p5) shows that when measuring growth between any two consecutive years, for any given independent, identically distributed error level in the gross output and intermediate input data, double deflation yields a Relative Standard Error four times as large as that given by single deflation.

Double deflation has other entertaining characteristics - Gutmann points out that:

"It should be noted that the double deflation method is strictly conventional and may sometimes lead to ambiguous results (a positive value-added in nominal price versus negative real value-added)." (1981, p436)

This issue is addressed by David (1962), who turns what appears to be "...just retribution for bad data and/or poor deflators..." into a plausible economic story (p149).

Double deflation has something of a stench in economic circles - not only in the USA but also in the UK.

In practice, the data requirements for double deflation render the technique of estimation unusable in most industries - because of the sensitivity of the results to errors in the various inputs into the calculations, especially the deflators, this approach is not in wide use - certainly not in South Africa.

Of the single-deflated indirect volume indices, that derived from value of sales, the measure used by the British is probably less desirable than the corresponding index derived from census estimates of gross output. Derived as it is directly from concepts which have a clear meaning in a production framework, gross output has to be estimated anyway to make economic sense of changes in physical outputs - an engineering magnitude. Its drawback is that it requires for its estimation a quite comprehensive information-gathering exercise - usually only undertaken when an industrial census is conducted. Not many countries have the resources to conduct these on anything more frequent than a triennial, or at best, a biennial basis. Simpler-to-measure magnitudes, such as value of sales, on which regular monthly surveys are relatively easy to conduct, are used instead.

Summarising the position thus far, it has been concluded that double deflation is not possible in the South African case, deflated gross output is unacceptable because of the changing net/gross output ratio problem, and the conventional wisdom is that there are serious conceptual problems involved in deflating value added (net output) - as Rushbrook has pointed out:

"The quantity and price components of value added are not separately measurable since value added is the balance between the value of gross output and the value of inputs from other industries." (1978, p106n)¹³

Value in production is added by the inputs or factors of production, capital and labour. As is well known, the use of inputs as surrogates for output is generally regarded as problematic because changing productivities will cause output levels to be over- or under-estimated (Maurice, 1968, p78; UN, 1968, p59). Yet there appears to be at least one way out of this

(continued)

that:

"...if the index is continuously "chained" and the relative weights in the index are continuously altered to reflect factor shares, the double-deflation method yields a Divisia index for value added."

The practical significance of this is limited. Unfortunately, the same cannot be said of the question with which Sims ends his contribution - "Does the notion of real value added" he asks, "make any sense?" (p471) This is not easily answered.

13 As David has pointed out:

"Producers market their output and not their value added, which is why prices of the latter are not directly observed and it is handled as a residual for purposes of deflation." (1962, p1253)

impasse¹⁴ - and it has been provided by the UN. It was with some surprise that I discovered the license being granted in the latest SNA (UN, 1992, p24; UN, 1993, p391) for single deflation of [gross] value added to be performed "...directly by a price index for gross output..." when double deflation is not feasible. The condition for this operation was that "...good data on gross value added at current prices..." be available. Admittedly, the practice is put forward only as "...an acceptable second-best solution when the data are not sufficiently reliable and robust to permit the use of double deflation...", but this is a considerable advance on the blanket prohibition apparently in force in analyses such as that by Rushbrook cited above.

Assuming then that the CSS data on value added are good, what is required is a suitable deflator. Before the search is joined for this elusive index, a couple of small detours are necessary. The first of these is through the outskirts of a discussion on the question of what is known as the 'separability' problem, and the second is concerned with real product, real income and the bridge between them, the terms of trade adjustment.

The separability problem

By comparison with the somewhat murky theoretical underpinnings of the invitation by the UN to estimate real net output by a second-best single-deflation technique, those of a handful of methods of estimating real value added proposed by Arrow (1974) are much more rigorously defended. Unlike some of the authors referred to above, Arrow deems it:

"...a reasonable goal to measure real value added in terms of capital and labor or to deflate money value added, which may be thought of as the money value of the *net* output of the industry, by some suitable index number." (p3)

This he sees as part of the larger project of constructing a full set of national income accounts in real terms (p4). Arrow argues that:

"To assess more deeply the relative merits of alternative measures of real value added, it is necessary to ask what its economic meaning is, that is, what we are trying to measure. I will argue first that the most natural meaning, indeed the only one I can think of, arises from the estimation of production functions. The output of any commodity is determined by the inputs of a number of commodities, of which some are

14 Estimates of net output are scarcely novel - David (1962), cites an article by Berlinguette and Leacy which describes how the Canadian Dominion Bureau of Statistics prepared:

"...annual and quarterly indices of industry "net output" by deflating census value added. The indices have been used to project estimates of gross domestic product by industry of origin available for benchmark dates..." (p148).

I could not lay my hands on a copy of the Berlinguette and Leacy piece, and so was unable to establish what deflators were used. Could this be a reference to the double-deflated values referred to above?

primary factors and others are produced goods, which we will refer to as *materials*. The attribution of a special role to primary factors, capital and labor, and the construction of an aggregate for them can be justified only for the usual reasons: that their use in production is *separable* from that of materials." (Emphasis in original)

This condition holds only if the standard form of the function:

$$Q = Q(K, L, M)$$

where Q refers to output, and K , L and M stand for capital, labour and materials, can be assumed to take on the special nested form:

$$Q = Q[V(K, L), M]^{15}$$

The 'separability assumption', as this is known, requires that:

"...the marginal rate of substitution between K and L in the production of Q is independent of M ..."

This can be visualised as a metaphor in which capital and labour co-operate:

"...to produce an intermediate good, real value added (V), which in turn cooperates with materials to produce the final product..."

Arrow observes that this:

"...empirical and refutable assumption about the nature of production functions...would seem to be reasonable for a broad variety of cases..."

15 There is a note in the Arrow article, the significance of which may easily be missed by those not steeped in the intricacies of the value theory debate, and this despite the fact that Arrow draws attention to its importance. Of the seemingly innocent variables used in his article he makes the following remark:

"Note that Q , K , L , and M are observable variables, but V is not. In the language used sometimes by statisticians, particularly those working with psychological data, V is a *latent* variable, while the others are *manifest* variables. This remark has important implications. It shows that we can never hope to achieve a *unique* measurement of V . (1974, p5)

The precise reasons why this is so are not of concern here to me - my interest in the formulation is the similarity which it has to the transformation problem in Marxist theory. As is well known, Marxists work in value as well as price categories, a characteristic of the approach which has led critics to dismiss it, chiefly on grounds of the redundancy or immeasurability of 'value'. But Desai (1974) has argued that it is:

"...the task of the [Marxist] theorist to show that *one can go back to value relations given only price information*. The transformation problem can be posed in modern terms with which econometrics has made us familiar. In terms of econometric theory, the price relations are observed *reduced form* equations whereas the value relations are the unobservable *structural form equations*. A test of the validity of the model (its identifiability) is that one should be able to go back from the reduced form to the structural form uniquely and vice versa. In Neoclassical economic theory, preferences and technology are the structural relations which explain the observed price-quantity data."

Of course, as Desai points out, Marxists would reject these relations, but that is not the point - what matters is that Arrow's concept of value is similar, in certain important respects, to that used by Marxists.

The details of the different approaches, the variants of them proposed by Arrow need not be considered here. It is worthwhile noting, however, that Arrow makes use of the theoretical property of duality whereby both production functions and price indices (cost functions) may be estimated from the same (minimal) data set (p5).¹⁶ Arrow's methods either use:

"...an appropriate maintained hypothesis about the structure of the real-value-added production function to compute an index of real value added from the data." (p11)

or they employ the cost function approach to define an "appropriate price deflator" - one which reveals the minimum cost of "...producing a unit of real value added..." (p11). Adding in materials yields a further set of estimates (also with variants). All of these estimates depend on there being "...a number of observations of the same production function..." Arrow does examine the implications of relaxing this assumption that the function does not shift (pp16ff), but the more serious problem, at least as far as this study is concerned, arises because of the separability assumption.

Denny and Fuss (1977) point out that prior to the early 1970s, separability, "...a pivotal concept in production function estimation..." and the existence of aggregate inputs were assumed *a priori* (p404). These authors developed a set of tests for separability on the basis of which they tested a number of hypotheses about the nature of the production function in US manufacturing over the period 1929-68. The details of the results need not detain us here. What is of interest, however, is that the method proposed by Denny and Fuss was picked up by Norsworthy and Malmquist (1983) and used in a comparative study of productivity in Japan and the US. After testing for separability, they conclude that the conditions for the existence of a direct aggregate of capital and labour are not met and that the appropriate output measure for investigation in both countries is gross output (pp953-954). In other words, they reject a K, L approach in favour of a K, L, E, M (capital, labour, energy and materials) approach. This does not affect output growth rates much, but when it comes to estimating multifactor productivity growth, the differences yielded by the two methods are startling. In general, the K, L approach as compared with the K, L, E, M approach overstates productivity growth quite significantly - for the years 1965-73, multifactor productivity growth was estimated at 0,59 per cent per annum by the latter, and 1,55 for the former in the US. For the period 1973-77 the corresponding figures were 0,38 and 1,12. For Japan, the contrasts were much larger - serving to remove much of the basis for claiming that that country had experienced a productivity miracle. For the earlier period the results were 0,91 and 2,03 respectively, and for the later, 1,64 and 3,67. Capital productivity growth in the earlier period was negative in both

16 Norsworthy and Malmquist (1983, p952) also make use of production functions and their 'dual' cost functions, as do Denny and Fuss (1977, p412).

approaches, but strongly so, as measured by the *K, L, E, M* method (-4,09 per cent per annum). In the later period it was only mildly positive (p957).¹⁷

Intuitively, one suspects that this has significance for the South African case. The *K, L* approach is appropriate where raw materials may be transformed by varying combinations of labour and capital. Arrow warns, however, that capital and materials may be a more natural aggregation in other cases, and cites power generation as an example. In such a case, increased capital spending serves to economise on fuel (Arrow, 1974, pp4-5). It may well be that the natural aggregation in the case of Sasol, and the heavy chemical industry more generally, is also capital and materials (and in the basic metals industry, capital, energy and materials) - in which event, the theoretical basis for estimating real value added in a production function framework collapses.

Pausing for a moment to reflect on the implications of the arguments outlined above, it is perhaps useful to recall that one of the primary purposes of measurements of the type discussed in this study, and indeed, one of the most basic concerns of economics, is the way in which welfare, as reflected in the amounts of commodities and services (Geary's 'quantum of goods') available for consumption (and production) has changed over time. To gauge this, the highly imperfect measures analysed in this study have been devised. Arrow says that he is interested in the 'economic meaning of real value added'; Norsworthy and Malmquist suggest that it may not be reasonable to ignore intermediate inputs, meaning that: '...in the production of *Q, M* may not be independent of ...the marginal rate of substitution between *K* and *L*', nor indeed of energy. In other words, a given quantum of intermediate inputs may, with technological change over time, be transformed into a greater quantum of final outputs (Arrow, 1974, p6).

The relatively simple procedures by which output is 'valued' in the national accounts¹⁸ cannot cope with these complexities. It seems though, that studies to determine the extent to which problems of the type suggested by Arrow and by Norsworthy and Malmquist plague the national accounts will have to remain the province of academics. Except in the case of unusually well-endowed national statistical bureaux, eg, Statistics Canada, official statisticians would appear to lack the resources to mount such projects.

In the South African case, a starting point for an academic study would be a thorough examination of the Sasol data. Unfortunately, given the paucity of information available for

17 Norsworthy and Malmquist's findings have been challenged by Yuhn (1991), who argues that because of deficiencies in their specification of the conditions for separability, the finding that gross output is an appropriate basis for analysing capital-labour substitutability does not hold. (p1012)

18 Above all, the deflation of gross output to yield constant price output estimates, on the assumption that changes in the ratio of net to gross output are not significant.

the industry, it is not possible to perform the calculations necessary to test for separability. The potentially rewarding piece of research suggested by the review of the separability issue above will simply have to wait until some researcher is given access. In short, at least for the meanwhile, we are driven back into the type of approach suggested above by Moll (1990, p71). In pursuit of a pragmatic solution, above all, one which does not fall foul of the changing net/gross output ratio trap, a brief examination of the problems of finding an appropriate deflator will now be conducted. Common ground, in the form of a search for appropriate deflators, may now be seen to exist between the UN recommendation and the spirit of the cost function approach as used by Arrow and the other workers cited above - the task is to find this deflator. Any objections to this procedure must be tempered by the knowledge of the weakness of the SNA conventions that invite economists to proceed in the hope that the ratio of net to gross output will not change too rapidly, and what is more, provide little guidance on what to do when the inevitable does occur.

A digression on real product vs real income

Before commencing that exercise, I would like to make a slight detour, to revisit an old disagreement between myself on one side, and the National Productivity Institute and the South African Reserve Bank on the other. Geary is cited above as stating that:

"...[a] net index [shows] the trend in the amount of work done in [an] industry..."

Read quickly, this seems a fairly straightforward statement - work done sounds like a synonym for economic activity. On reflection, it is clear from a reading of Arrow (1974) that no simple meaning attaches to the expression.

It was precisely into this area of conceptual darkness that I stumbled when I wrote my first critique of the attempts by the NPI to estimate productivity growth rates in South Africa (Meth, 1983). The rapid gold price increases in the late 1970s produced chaos in the national accounts - illustrated most simply by the conflicting estimates of the proportional contribution of the mining sector to total output as measured using the current price estimates and the estimates for different base years.¹⁹ I argued that price increases caused 'real product' in the mining industry, as measured by conventional national accounting techniques, to part company

¹⁹ This may be seen in Table 3 in Meth (1992, p21).

with 'real income'. Insisting on the identity of Total Product and Total Income,²⁰ I proposed an alternative means of valuing 'real income' for the mining sector, and added this, somewhat naively, to 'real product' from the other sectors to obtain an upwardly revised estimate of real GDP - a "curious procedure", as Moll (1992, p192) has pointed out.

Instead of acknowledging that the problem with which I was trying to grapple resulted from the overload on the SNA, and that the terms-of-trade adjustments designed to take care of such events as the gold price bonanza were far from infallible (Meth, 1991b), the NPI, following the Reserve Bank (Swanepoel and van Dyk, 1983), attempted to rebut my arguments simply by claiming that I had confused income and production (NPI, 1983). That, of course, was true, but its verity did not dispose of the problem. The following (long) quote from an article by Olgaard (1981) is reproduced to show that, at the time, other researchers were struggling with the same problem, and that their doubts were being aired in the premier learned journal in the field of national accounting. Discussing the concept of real product, Olgaard states that:

"The traditional domestic product figures by industries at constant prices may conveniently be labelled *real product* figures. They express *quantity changes exclusively*, assuming relative prices to have remained constant. Hence they are useful for some purposes, mainly related to analysis which takes the starting point on the production side, e.g. traditional analysis of productivity.

But when it comes to analysis of changes of the industrial pattern over time, i.e. of the (shares of) *total income earned by the various industries* over time, the real product figures are not very useful. They may even be claimed to be misleading, since such income figures ought to reflect not only changes of quantities, but also changes of *relative prices*. In the following, such figures will be labelled *real income* figures.²¹

It is one of the contentions of the present paper that it ought to be standard procedure for the Central Statistical Offices to publish time series of real income figures by industries in addition to (the traditional) real product figures." (1981, p166) (Emphasis in original)

Argument is then offered on the way in which the concept of real income should be understood, and how appropriate deflators ought to be determined. Once the assumption of a closed economy is dropped, the need to make terms-of-trade adjustments arises. On this question, Olgaard takes a position that differs from the conventional, and in defending it, he harks back to "battles" fought over the issue in the 1950s. The view he defends is apparently

20 This argument has a reasonable basis - located in the proposition that since any year could, in principle, be nominated as a 'base' year for national accounting purposes, the identity of production, income and expenditure must be maintained. The problem lies not so much with my reasoning as with the fact that the production of constant price accounts throw up a number of severe difficulties, especially as far as exports and imports are concerned (Gutmann, 1981). Attempts to solve these problems have, under certain circumstances, been far from successful (Kravis, 1982).

21 There is a footnote in the text at this point which states that:
 "Hence the label of "income" is used in a way different from the traditional one. According to traditional definitions, the difference between "product" concepts and "income" concepts relates to net foreign transfers. In the present context such transfers are assumed not to exist."

an extension of that held by the losers in those battles, a defeat he describes as "unfortunate" (p168). It is not the intention to enter that debate here - it is, however, worthwhile pointing out that significant terms-of-trade effects, both internal and external, could conceivably have arisen as a result of the construction of the Sasols in South Africa.

Assume for a moment, that after 1979, the oil producer's cartel had held, and that the members of the cartel had been able to increase prices steadily - to a point where the price of products refined from crude petroleum was well above that of the corresponding Sasol products. Assume as well, that the South African state had been able to muster sufficient resources to build a large enough number of Sasols to meet all domestic needs, and still have some export capacity. Under these conditions, the world petroleum price would have been irrelevant to South African producers, except insofar as it set a ceiling on the price they could have obtained for their (much lower cost) export surplus. Real (national) income would have increased by virtue of the terms-of-trade improvement implicit in this process - the cessation of petroleum imports, whose relative price had risen, would have meant that P_m would have fallen relative to P_x . Depending on the level at which the authorities pegged domestic prices, changes in the internal terms-of-trade²² could also have occurred, accompanied by significant redistributions of real income. Even with modest gouging in the domestic market, any notion that 'value added' could, under the circumstances, be regarded as having much connection with 'the amount of work done' is plainly flawed. In this sense, oil shares some of gold's unusual properties. It also harks back to the validity of the separability assumption routinely (unthinkingly) deployed in conventional analysis.

Two observations are in order - in the first place, changing relative prices often have a complex relationship with quantities - simple inverse relations exist only in introductory chapters of first-year texts. Real product and real income will be systematically related to each other only under competitive conditions. That relationship will be strained when technical progress of the kind caused by a shift from traditional refining techniques to Sasol's production methods takes place during an accounting period. In other words, it is necessary to exercise care in interpreting the results obtained by deflating value added in an industry such as Sasol, where the hand, not only of the South African state, but more generally, that of the international forces struggling over access to cheap oil may be felt. In the second place, and harking back to the earlier disagreement I had with the authorities over this question, it is somewhat alarming that an institution like the South African Reserve Bank, which is, after all,

22 Probably the most well-known internal terms-of-trade effects are those experienced by agricultural producers when, during an early development phase, the state attempts to secure cheap inputs for a growing industrial sector at the expense of farmers. The results of such exercises are not always happy. See for example, Nove's discussion of the 'Scissors Crisis' in the USSR in 1923 (1978, pp93ff).

responsible for producing part of the national accounts, should have failed to take Olgaard's arguments into account when replying to my criticisms of those accounts. The NPI cannot now, and certainly could not then, summon up the intellectual resources that would enable it to intervene in debates of this sort, but the same should not be able to be said of the Bank.

The intricacies of Olgaard's argument, and in particular, the manner in which he avoids the deflator problems with which I have wrestled elsewhere²³ are not directly relevant here. Instead of delving deeper into this debate, the course of action suggested in the latest version of the SNA - a single deflation of net output by "...a price index for gross output" (UN, 1993, p391) - will be pursued. In doing so, it is necessary to face, once more, the problems of the appropriateness of that index.

The search for an appropriate deflator

More than 30 years ago, Paul David had an article published on "The Deflation of Value Added" (1962). Remarking on the increasing attention being focussed on the issue at the time, he spelled out some of the numerous analytical benefits of having such a magnitude to hand. The article is a head-on confrontation with several of the index-number problems, in particular, those that arise when what he calls the Fabricant-Geary deflation method is used. This technique is none other than the double-deflation method for producing Laspeyres volume indices. One of the problems - actually, the clutch of problems usually manifested as differing growth rates when base year relativities are changed at rebasing - is of particular interest - it is that:

"...indices computed with different price weights need not tell the same story; in extreme cases late year [several years after the base year] prices may yield negative CDVA [Constant Dollar Value Added] in preceding years, or *vice versa*." (David, 1962, p153)

David notes that this can affect both aggregate output estimates as well as estimates of the output of a single industry producing a homogeneous commodity (p153). He reviews a number of alternative approaches to deflating value added, and finding fault with most of them, ends the article with the statement that:

"Economists and statisticians have learned to live with other sorts of index number problems and it is quite possible that the index number problem posed by the Fabricant-Geary deflation method may not be found intolerable. But, before that decision is reached, the matter does seem to warrant further greater attention and further empirical investigation than it has heretofore received." (p155)

23 See Meth (1983). This subject was also discussed at some length in Meth (1992, pp46ff).

The matter may well have received that attention but if it has, I have not been able to find a record of it. The new SNA, as we have seen, recommends sacrificing additivity in order to preserve previously observed growth rates. To show how refractory the deflation problem is (before exercising some license with the PPI!), the alternatives canvassed by David will be summarised below.

The first of them, proposed by Kuznets, deflated value added using a Paasche price index composed of the weighted average of the difference between a gross output index (such as the PPI) and an input index (David does not say which inputs - presumably intermediate inputs are intended). This formulation cannot, however, escape the index number problem outlined above. A similar procedure using Laspeyres price indices instead substitutes the index number problem for the difficulties of interpreting Paasche volume indices (p150).

Next up is a suggestion that an alternative approach from the expenditure side be considered. Deflating the components of final demand (*C*, *I* and *G*) would also strike the index number problem inherent in the deflation of any aggregate, but David suggests that it "...seems advisable to choose a path along which only one dragon (a familiar one at that), rather than two, will lie in wait." (p154). This, as he observes, is all very well at the aggregate level, but what about estimates at industry level? In his response, he confronts a problem raised obliquely in the discussion of the Euler Consistency Test in Chapter 2-2. He notes that:

"The identity between aggregate payments to factors and the value of the net commodity output of the economy (available for final demand) at factor cost, is really most artificial when translated to the industry level. As there is no marketed, or observable flow of net commodity output from a sector, one might logically seek to measure the real product-equivalent of the income generated by each industry."

To deflate that income, however, one needs to know the prices that income receivers pay in making final purchases. Thus while the approach is formally consistent to expenditure-type deflation at a national level, the task of "...securing appropriate deflators" for each industry would be Herculean. As a possible shortcut, he suggests constructing weighted price indices for the expenditure of property-income and service-income receivers (surplus and wages) for each industry. This was suggested in the Fosatu Challenge - see Meth (1983, pp7-8). While it may provide some indication of the real income of the different factors, it clearly cannot overcome problems such as those caused by episodes like the rapid gold price increases referred to in this study - in short, it does not provide a measure of Real Value Added.

Finally, David makes reference to the "momentary appeal" which using as deflator the "simple device" of the industry's index of gross output (the industry PPI) has. The central weakness of this approach, as he points out, is that unless it is "...adorned by a number of restrictive

theoretical assumptions" it conceives implicitly of factor payments being made in the commodities produced, valued at prices in the base year (p155). On that note, he ends his review - his concluding paragraph expressing the possibility (hope?) that the index number problems "...may not be found intolerable."

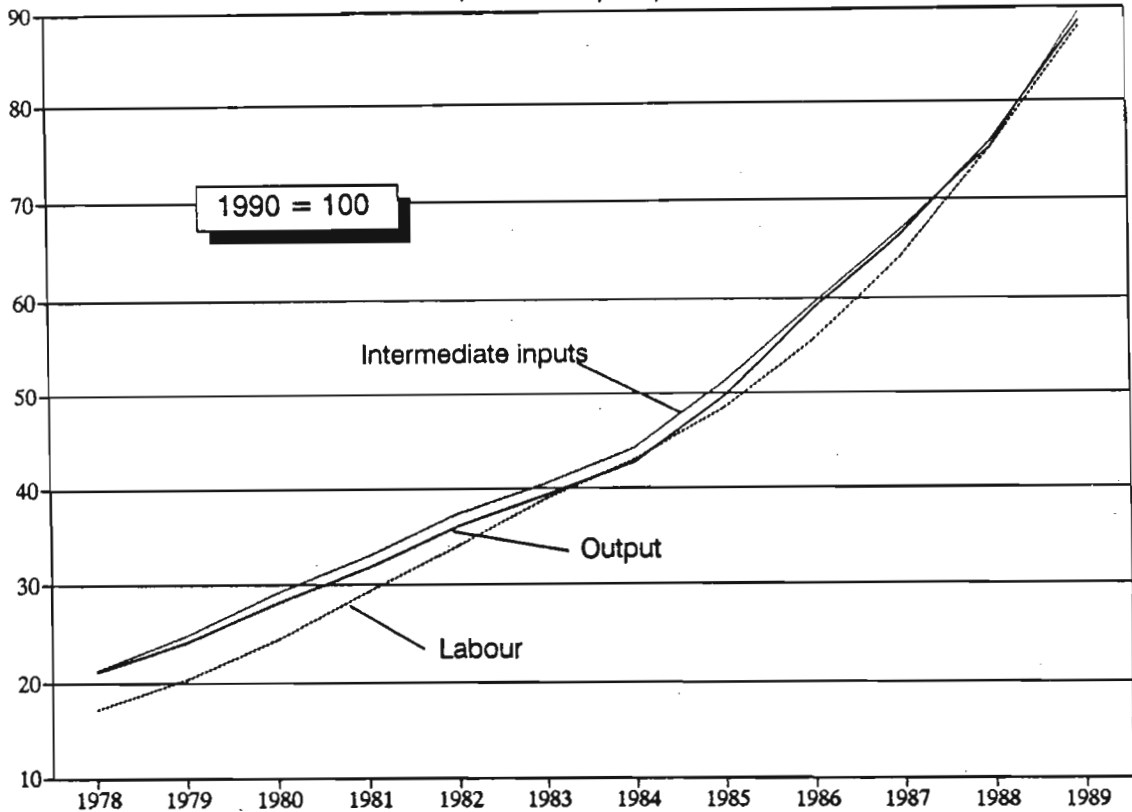
What can one gain from this? A large question mark hangs over the PPI, and an expenditure-side approach is infeasible. The Kuznets-type double-deflation index has possibilities, but the data on inputs are neither easy to obtain on a financial-year basis, nor, in the case of the data on capital, are they particularly reliable.

That leaves us with the PPIs. Even though it will be difficult to attach clear and rigorous economic meaning to the resulting estimates, there seems to be little alternative to using these, the only available indices. Depending on what has happened to the prices of intermediate inputs, the PPI could well be close enough to what a true index of the (weighted) cost of labour and capital might be to allow it to be pressed into service for the exercise proposed here. Rather obviously, the extent to which the PPIs are affected by changes in the relative prices of all of the inputs into production, including intermediate inputs, will determine their suitability for use as a proxy deflator for use on value added.

Using the IDC dataset (IDC, 1992), it is possible to examine some of the components of the PPIs for differences in the rates of growth of the various components. How reliable the IDC figures are is not known - prices of intermediate inputs, for example, are derived from the somewhat suspect CSS input/output tables - but there is nothing else available. Estimates are provided of the cost of labour, of intermediate inputs and of output at the major division (sector), major group and sub-group level.²⁴ It is possible, therefore, to examine each PPI used in the deflation exercise for divergent trends in its component indices. Whether such an operation is warranted is open to question - for the purposes of the exercise to be carried out here, it would seem sufficient to look only at the aggregate indices and, if one is satisfied that the behaviour of the components of the overall index is satisfactory, to invoke the informal national accounting rule that differences in individual industry series will cancel out. Given the likely shakiness of industry-level indices, it is probably not worth while getting into too much detail. There are only 11 observations over the relevant period, so resort cannot be had to sophisticated statistical tools - it is probably sufficient merely to cast an eye over the results as portrayed graphically in Figure 3 below.

²⁴ According to Mr J Swanepoel of the IDC, estimates of changes in the cost of capital are not made because of the extreme difficulty of doing so. Telephone conversation, 15 March 1994. Notwithstanding the absence of deflator for the components of gross operating surplus, especially capital, the IDC has succeeded in producing estimates of 'real' (constant price) profits (IDC, 1992).

Figure 3
Manufacturing - Intermediate Input,
Labour, and Output price indices



It is clear from this set of graphs that the two components of the PPI for which data are available tracked each other and the overall output index reasonably closely. Labour inputs did tend to grow a little more rapidly in price at the beginning of the period, but the differences do not appear to be large. To the extent that the labour input price over the period 1979-89 grew faster than intermediate input prices (both local and imported input prices) (IDC, 1992, p6.5), the use of the PPI as deflator would tend to overstate the growth of value added. Counterposed to this however, were persistent reports that capital was under-priced during at least part of the period. The mire into which such an endeavour will lead should be plain to see. This problem can only be tackled by an economist with full access to all of the relevant information at the disposal of the CSS.

A further reason why care should be exercised in using the PPI for the purposes intended here exists - one that has to do with the issue of weighting. On examination, the problem turns out, however, not to be too serious. The aggregate PPI is weighted by the distribution of the value

of sales, a variable whose proportional distribution is closer to that of gross output than it is to net output. Presumably, no-one would attempt to argue that because of this, the sub-indices of which it consists cannot be used as deflators on net output figures for individual industries. Aggregate PPIs play no part in the benchmarking process - aggregate PVMPs are derived by summing individual PVMPs - and in any event, as has been demonstrated above, the sensitivity of an aggregate index like the PPI (a weighted average) to relatively small changes in weights is negligible. A more important question is whether or not the individual industry sample surveys (sub-samples) for the PPI are representative.²⁵ Either they are, or they are not. If some of the sub-samples are not, then the inaccuracy of a PPI weighted by value of sales will differ from one weighted by net output only to the extent that the industry or industries for which individual subsamples are incorrect make differing proportional contributions to total output. It is known that the PPIs for certain industries are highly unreliable, and it is suspected that certain others are of dubious accuracy. That knowledge, however, does not influence the broader choice to make use of the PPIs.

Economists wishing to perform statistical analyses are frequently placed in situations where second- or even third-best is all that is available. Certainly, the UN suggestion that single-deflation of value added be tried is made with the clear knowledge that this is so. That being the case, no further apologetics are called for - the PPIs are far from ideal, but there is nothing better on offer. One could argue, on the basis of the information presented above, and with some confidence, that it is not too unreasonable to use the PPI as a proxy deflator on the current price estimates of net output. What the meaning is of the constant price magnitudes that emerge is, of course, another matter altogether, and one which no guidance is available from the theory. In this connection, it is interesting to note that the IDC publishes estimates of value added at constant prices without any warning to users that the magnitudes in question are of more than dubious theoretical standing. These indices are produced using the CSS' PVMP series (IDC, 1992, p1.7).²⁶ Unfortunately, this makes a comparison between the IDC results and those to be presented below impossible - the IDC dataset was constructed on the basis of the pre-March 1992 CSS estimates - a set of figures that have been revised twice since that date. As Table 2-3.1 shows, those PVMPs are so inaccurate as to be useless.

25 Recall here that three different PPIs are estimated, one of which includes imports and one of which excludes exports. It was argued above that the index 'Total output of South African industries' which appears to include imported inputs and exports, but to exclude imported manufactured goods for local consumption, is the appropriate PPI to use as a deflator on manufacturing output. See *South African Statistics 1990*, p8.34.

26 Some of the indices may have been estimated using the PPI as deflator of current price estimates of value added, according to Mr Swanepoel of the IDC. Telephone conversation, 15 March 1994.

With that, it would appear that the possibilities of the South African datasets have been exhausted. Theoretical leads having also been pursued into a *cul-de-sac*, it would seem that further argument would be to little avail - all that is left is to wade into the data, most of which is to be found in Appendix 2-7.

The results

The layout of Appendix 2-7, from which the results below are drawn, is similar in some respects to that of Appendix 2-6. The new information that became available with the publication of SNR P3041.3 of 12 November 1993 has made possible the separation of the 'Other Chemicals' industry into 'Other Chemical Products' and 'Petroleum and Coal Products'. In Appendix 2-6 a compromise threads its way through the calculations, because the attempt to replicate the CSS results had, of necessity, to use the 'old' output data (Table A2-6.D7). This meant that most of what followed referred to the non-separated 'Other Chemical' industry. This, and my ignorance of what exactly it was the CSS was using to deflate the different components of the output of that industry led to the awkward compromise in which two deflators - the 'Other Chemicals' PPI and the 'Petroleum and Coal' PPI were used to establish ranges.

In **Step 1** of Appendix 2-7 this type of approach is replicated, mainly so that the consequences of doing so can be compared with the outcome of the calculations in **Step 2**. In this second step, the 'Other Chemicals' industry is split and the deflators which the CSS says are appropriate have been used. This gets around the cumbersome business of presenting alternative estimates. According to the CSS, the appropriate deflator to use on the output of the industry 'Petroleum and Coal Products' is the 'Petroleum and Coal' PPI. As I argued in Chapter 2-6, this is almost certainly incorrect. Because, however, the use of the Petroleum PPI tends to bias the results downwards in the critical year of 1985, its use here is defensible.

A summary of the Step 1 results is given in Table 2-7.2. In all cases where a Net Output (Sum of Components) is estimated, use is made of both 'Petroleum' PPI and the 'Other Chemicals' PPI as deflators. As may be seen, both sets of estimates (Rows 257 and 260) are much higher than their corresponding Real Gross Output counterparts in Table A2-6.1 of Appendix 2-6 - the crucial 1984/85 Petroleum PPI deflated estimate exceeding the CSS figure of 119,3 by more than 14 percentage points.

Table 2-7.2 Real Net Output - Summary of Step 1 and 2 results

1978/79=100	1981/82	1984/85	1987/88	Row
Real net output (Sum of Components)				
Step 1i (Pet PPI)	126,0	133,6	135,0	257
Step 1i (OC PPI)	129,3	136,0	132,2	260
Real net output (Total)				
Step 1ii (Agg PPI)	128,1	135,7	132,1	274
Aggregate 'pseudo' PVMPs (1978/79=100)				
1978/79 weights				
Step 1iv (Pet PPI)	126,9	133,1	135,2	352
Step 1iv (OC PPI)	129,8	136,8	133,7	353
1984/85 weights				
Step 12v (Pet)	126,0	133,6	135,0	388
Step 12v (OC)	129,3	136,0	132,2	390
DIY benchmarked calendar year figures (1979=100)				
	1982	1985	1988	Row
1978/79 weights				
Step 1vi (Pet PPI)	118,0	123,8	134,8	426
Step 1vi (OC)	120,8	127,2	133,3	428
1984/85 weights				
Step 1vii (Pet)	117,3	124,3	134,8	464
Step 1vii (OC)	120,3	126,4	131,7	466
Step 2 (1979=100)				
	1981/82	1984/85	1987/88	Row
PVMPs linked using SNA method				
Real Net Output	128,2	136,1	133,7	847
Real Gross Output	123,1	122,5	122,6	895
Calendar year estimates				
	1982	1985	1988	Row
Real Net Output	117,3	126,7	132,6	852

Source: Appendix 2-7

As expected, Real Net Output (Sum of Components - OC deflated estimates) and Real Net Output (Total) (Row 274) are very close, thereby confirming once more the proposition that the whole should not differ much from the sum of its parts. Aggregate pseudo PVMPs are also very close to their corresponding Real Net Output estimates (Rows 388, 352 and 257; 390, 353 and 260). It appears not to make a great deal of difference whether or not one uses the 1978/79 or the 1984/85 weights (Rows 388 and 352; 390 and 353).

Converting the pseudo PVMPs into calendar year figures, one gets a similar spread (and correspondence). As may be seen by comparison with the Appendix 2-6 results, deflating net instead of gross output makes a substantial difference to the results. So much for the crudeness of using differing assumptions about which PPI is appropriate, and of playing around with pseudo PVMPs. In Step 2, not only are the deflation procedures tightened up, the estimates are also improved by the use of a more accurate linking method. Table A2-7.S2i contains

estimates of net output prepared from the weights given in SNR P3041.3 of 12 November. This process yields the separate components of the industry 'Other Chemicals'. Real net output is estimated in Table A2-7.S2ii and this is converted to index form in Table A2-7.S2iii. In Tables A2-7.S2iv(a) and (b), estimates of Q are made from the gross output estimates in Table A2-6.D9 and the net output estimates in Table A2-7.S2i. The gross output figures have been included so that a comparison between the results of this more accurate way of estimating aggregate PVMPs may be made with those in Appendix 2-6. They are also included so that the differences between Real Net and Real Gross Output as measured using the same approach can be obtained. The calculations are based on the fact that V (total 'value', ie, gross or net output in current prices) is equal to P (the PPI) multiplied by Q . A comparison of Tables A2-7.S2vi(b) and A2-7.S2iii shows that the indices with the base year 1978/79 are identical, as one would expect them to be.

Differences between Real Net and Real Gross Output are given in Table A2-7.Svii(a). A negative sign (most of them are) means that Real Net Output exceeds Real Gross.²⁷ As may be seen, some of the differences are huge, especially the result for the Petroleum and Coal Products industry. It is obvious that if the same deflator is used on both net and gross output, as has been done here, then output growth as measured by the two indicators (Real Net Output and Real Gross Output) can only differ if the ratio of net to gross output changes over the period in question.

Assume for a moment that it were possible to separate value added into a price and volume component - that volume component would be the analogue of Real Net Output. Now let the price level at time o be P_o , and that at time t , P_t ; the volume of net output at time o equal to q_o and at time t , q_t ; and the volume of gross output at time o be Q_o and at time t be Q_t .

Current price and Real (constant price) Net Output in the base year o would be equal to $q_o P_o$.

Current price and Real (constant price) Gross Output in the base year o would be equal to $Q_o P_o$.

In year t , Net Output in current prices would be $q_t P_t$

and Real Net Output would be $q_t P_o$

27 It is as well to recall here that what is being compared is the deflated value of net output and the deflated value of gross output. If a perfect set of deflators were available for every year, and the ratio of gross to net output did not change, no differences would emerge.

Gross Output in current prices in year t would be $Q_t P_t$

and Real Gross Output in year t would be $Q_t P_o$

Expressed in index form, growth in Real Net Output would be:

$$(q_t P_o - q_o P_o) / q_o P_o$$

This simplifies to $(q_t - q_o) / q_o$ (1)

And growth in Real Gross Output would be:

$$(Q_t P_o - Q_o P_o) / Q_o P_o$$

which simplifies to $(Q_t - Q_o) / Q_o$ (2)

The ratio of net to gross output in the base year would be:

$$q_o P_o / Q_o P_o$$

which simplifies to q_o / Q_o

The ratio of net to gross output in year t would be:

$$q_t P_t / Q_t P_t$$

which simplifies to q_t / Q_t

The condition for equality of growth rates as measured by Real Net and Real Gross Output to be equal is that the ratios of net to gross output in period t must be the same as it is in period o , ie:

$$q_o / Q_o = q_t / Q_t$$
 (3)

When this condition is met, (1) is equal to (2), ie:

$$(q_t - q_o) / q_o = (Q_t - Q_o) / Q_o$$
 (4)

Plugging any arbitrarily selected values into (4) illustrates readily that as long as (3) is true, so too is (4).

All of this may appear to be somewhat trivial - the convention developed to deal with changes in the net to gross output ratio, ie, to ignore them, certainly does little to discourage such a response. The divergences in Table A2-7.S2vii(a) suggest, however, that changes in the ratio of net to gross output need to be treated with circumspection - there are likely to be times when these changes cannot safely be ignored. It was noted above that with the new information that was presented in SNR P3041.3 of 12 November 1993 it became possible to separate Other Chemicals into its two components. The results show that net to gross output in the residual Other Chemicals industry went up by five percentage points, but that ratio in the Petroleum and Coal industry more than doubled. Complacency would be in order if we were assured that deflators moved in a direction which neutralised the changes. Since it cannot readily be shown that this is the case, caution is called for.

Table A2-7.S2vii(c) contains estimates of the change in net to gross output ratios extracted from the Table A2-7.S2vii(b) estimates of these ratios. If one assumes, obviously arbitrarily, that a change in the net to gross output ratio of say, three percentage points signals the possibility of problems, one can then draw from Table A2-7.S2vii(c) a list of industries deserving of some scrutiny. From the table it would appear that Beverages, Tobacco, Clothing, Paper, Printing, Petroleum and Coal, Rubber, Pottery, Glass, Metal Products, Machinery and Transport Equipment fall into this category. It may be possible to establish that the changes have not done serious damage to the output estimates. This, however, is an empirical matter, not something which can be settled *a priori*.

In Tables A2-7.S2viii and A2-7.S2ix the deflated results from Tables A2-7.S2iv(b) and (a) respectively, are linked. The Table A2-7.S2viii figures yield the single set of estimates that will be offered as the upper limits of the most likely estimates of the output of the manufacturing sector that can be prepared with the existing information. The technique here is to estimate growth from 1978/79 to 1981/82 using 1978/79 weights (P79Q79 to P79Q82), from 1981/82 to 1984/85 using 1981/82 weights (P82Q82 to P82Q85) and lastly from 1984/85 to 1987/88 using 1984/85 weights (P85Q85 to P85Q88). A series is produced by taking the two values shared by a link year, eg, P79Q82 and P82Q82, and scaling the value for P82Q85 by the ratio of P79Q82 to P82Q82. The results of this activity are shown in the penultimate panel of Table 2-7.2 (Row 847). Below them in Row 852 are the corresponding calendar year estimates. These have been made by a short-cut method using the ratio of financial to calendar year values from the unbenchmarked series given in Table A2-6.D10 of Appendix 2-6. These

ratios are similar to both the corresponding benchmarked and aggregate reconstructed results. The precipitous decline from 1981/82 to 1982 may be a bit excessive - it makes the climb to 1984/85 seem a trifle far-fetched, but other than that, the series shows relatively consistent growth over the period. This is most easily seen by referring back to Figure 1 in Chapter 2-1, where the six squares plot the results generated in this appendix. All of the results, with the exception of that for 1982, fall into or above the region bounded by the Exuberant and Conservative series.

Directly below the real net output results is the corresponding set of gross output estimates in Table A2-7.S2ix. As may be seen, these turn out to be remarkably similar to the OC deflated figures in Rows 803 and 820 of Appendix 2-6.

With that, the end of this part of the investigation is reached. It is not possible to proceed much further using the available data. All that remains is to ask how well the upward revisions proposed here can be tolerated? Unfortunately, there are no criteria by which to answer such questions. Sticking to the standard practice of keeping sectoral growth rates constant at the unrebased levels, the abandonment of the principle of additivity led, as noted in Chapter 2-4, at the 1980 rebasing, to severe disjunctions (Meth, 1992, pp17ff). It may well be, however, that changes of the magnitude reported above are regarded by the CSS as little more than a hiccough. After all, as noted in Meth (1992, pp15-16) when the rebasing from the year 1970 to 1975 took place, estimated growth rates during the period 1960-70 in Manufacturing and Commerce changed from 8,4 and 7,8 per cent respectively, to 5,4 and 6,0 per cent, apparently without causing alarm. Equally interesting was the change in the growth rate in Manufacturing over the period 1970-79. With the rebasing, this rate went from 2,6 to 5,0 per cent *per annum*. The mere one to two percentage point change in output levels that would result from using 1978/79 as opposed to 1984/85 weights reported in Table 2-7.2 seems insignificant when judged by the past performance of the CSS in this regard. The manufacturing data therefore escape the nightmare which rebasing inflicted on gold mining output estimates.

Chapter overview and summary of findings

The chapter commences with the observation that there exist both theoretical and empirical obstacles to the estimation of real net output. In the case of the present study, the most obvious empirical obstacle is that created by the fact that the CSS, having all but abandoned attempts to measure PVMPs, has switched to the estimation of indirect indices based on value of sales. Not only do the figures appear, once more, to be incorrect (a proposition whose verification

would require a repeat performance of the study conducted in these pages) - the new estimates are also inevitably infected by changes in the ratio of net to gross output. Two theoretical problems exist - the first of these is concerned with the fact that deflating value added is problematic because the choice of deflators cannot be settled satisfactorily. The second problem is that concerning the separability assumption. The theoretical problems are grappled with further on in the chapter - the empirical or data problems are ignored.

Next, a glance at the failure of the compilers to address the conceptual problems of aggregation and the representation of aggregates by relatively crude indices is ventured.²⁸ Despite criticism of the previous SNA (UN, 1968) which referred specifically to the problem of changing net to gross output ratios, the latest SNA ignores the problem.

Another area of conceptual confusion is the absence of an agreed meaning for the concept of net output. A brief look at some of the definitions used elsewhere suggests that the differences could, on occasion, be significant.

Following this, the problem of estimating real net output is addressed. Double deflation does not solve the problems caused by changing net to gross output ratios, and single deflating value added has traditionally been frowned upon. Yet the latest SNA, somewhat surprisingly, seems to approve of single deflation of this magnitude. That being so, the task is to find a suitable deflator.

Before this can be done, the thorny problem of separability has to be confronted. In essence, value added only has meaning if capital and labour can be aggregated, ie, 'that their use in production is separable from that of materials'. An awkward finding for this study is that separability may not be feasible when heavy industries like the basic metals and chemicals dominate a manufacturing sector. Under those conditions, the appropriate separation to make could be capital, energy and materials. Tests can be conducted to test this hypothesis, but the data to do so are not available in the South African case. The study is left in the uncomfortable position of having to assume that it is appropriate to make the standard assumption about separability. Further research is required to determine whether or not this assumption is appropriate - speculation on the possible consequences of a breakdown of the assumption at this point is pointless.

28 It may be recalled that in Chapter 1-4 a comparison of the relatively primitive concepts used (primarily, the trusted Laspeyres and Paasche indices) with the much more sophisticated alternatives available suggested that pragmatism may not always be justifiable. Some of the more exotic indices were observed to be data-demanding, but indices like Törnqvist are not.

Two more digressions are undertaken before the results of the attempts to estimate real net output are presented. The first is through the minefield created by the blurring of the concepts of real product and real income - a subject treated at some length in Appendix 2-1. This problem is linked to that treated in the next digression, an examination of the question of appropriate deflators for estimating real net output. In essence, the objections to the variety of ingenious methods used to perform this deflation are examined. In the end, the only feasible deflator turns out to be the PPI. The properties of the various PPIs are considered for their possible impact on the results.

Finally, the results of the calculations performed in Appendix 2-7 are presented. Referring back to Figure 1 in Chapter 1-2, these values of real net output are indicated by the square markers. They fall, it will be recalled, either inside, or close to the region bounded by the 'Exuberant' and 'Conservative' output series I proposed three years ago.

Chapter 2-8

Conclusions and Policy Recommendations

Introduction

What, exactly should go into the concluding chapter of a work such as this is not immediately obvious. One would not, for example, wish to repeat the chapter outlines given at the start of each of the two parts of the study. In announcing what the intention of each chapter is, most of these summary introductions announce as well the findings of the chapter. It seems most profitable to use this space to highlight the few findings of note, and then to speculate a little on how these may be of use.

Several different ways of categorising these findings could be devised - one group could lump together those findings about which one could say - that is obvious. A peculiar quality of these obvious and well-known truths is the extent to which their neglect or flouting in practice renders them an object worthy of repeated rediscovery. Another would be a group of findings about which one is obliged to repeat that often weak-sounding conclusion - more research is required. Yet there is no reason to be bashful about the exposure of the extent of ignorance - to lay out a research agenda and to make suggestions as to how it may be tackled is a useful contribution. Finally, one could put into a (very small) basket, the things that may be called new knowledge. As always, pigeon-holing is an imprecise sport - some arbitrariness, resulting from overlap, is only to be expected.

That's obvious!

Methodologically speaking, there would seem to be a need for a greater recognition on the part of researchers of how deeply political the activity of research in economics is. Writers on the topic of theory choice, while acknowledging that such choices have to be made, and acknowledging too, the extreme difficulties which poor measures imply for such choice, do not generally pursue this problem all the way to the metaphysic in which it usually ends.

Institutions structured along the lines of the NPI are inherently contradictory. Conflicts between the different goals of such an organisation are inevitable. Moral hazard raises its head as soon as an institution is required both to popularise a cause and to create the information on which analysis of the progress of the cause is based.

Replicability of results, even if only approximately, is such an obviously desirable characteristic of a research or empirical enterprise that one can hardly imagine anyone denying its importance. Like certain customs in Prince Hamlet's Denmark, this one is more honoured in the breach than in the observance. It has been gratifying to be able to use the failure to ensure replicability as an intellectual weapon against the NPI and the CSS.

Mere science is no match for the dominant ideology and an entrenched bureaucracy. Creating democratic structures that can challenge the powerful is an important task. The Statistics Council in the old South Africa, poorly designed, and dominated by the Pretoria old guard, was a particularly weak shield against the excesses of the CSS (and even more so, the SARB).

More research is required!

Theories of the state, as they apply to institutions such as the NPI, are not all that well developed. Speaking more generally, a comprehensive leftwing account of the relationship between apartheid and capitalist development in South Africa has yet to appear. Existing work is strong on historical detail but weak on economic theory.

Internationally-accepted procedures for valuing output break down in the face of rapid changes in the ratio of net to gross output. At present, no procedure exists for dealing with this problem, which is probably far more widespread than is generally known.

Generically, the gold valuation problem is part of the same phenomenon. Instead of the breakdown being caused by changes in input prices, as was the case in the oil-from-coal industry, its origins lay in rapid changes in output prices.

Rather obviously, both of these problems may be subsumed under the broader heading 'The failings of value theory'. Without wishing to foreclose on the future, it must be noted that it is not clear that more research will solve the theoretical problems. The question then becomes one of determining empirically, the points at which the phenomena referred to above create serious problems in the national accounts.

Part of the 'more research' that should be done is a thorough investigation, by suitably-qualified impartial experts, of the CSS' attempts to estimate manufacturing sector output, as well as of their attempts to estimate terms of trade effects.

Much ink has flowed during the course of the exchanges which have brought matters to their present pass. It seems immodest to claim that without my prodding, the CSS would not have published the upwardly revised output figures, yet equally, it seems falsely modest to pretend that persistent pressure did not have that effect. The appointment of a special committee to examine my complaints, unsatisfactory though its procedures were, did at least cast the spotlight on an area which the CSS could well have ignored for sheer lack of skilled manpower. The recognition of the Statistics Council of my rôle in this affair, although it is more than a mere 'thank you for your concern' (see the letter from the Statistics Council dated

1 October 1993 in Appendix 2-2), does not go far enough in acknowledging the magnitude of the errors that the CSS has admitted elsewhere to having made.¹ Personal communications with one or two of the more progressive individuals at the CSS have elicited the grudging compliment that it is the activities of the few individuals in South Africa who scrutinise the statistics, that keeps the CSS on its toes, so to speak. The appointment of a new and responsive Statistics Council should ensure that users are not abused again in the future.

This is news!

A procedure something like the Euler Consistency Test, applied at regular intervals to output estimates, has the potential to help the statistical authorities avoid making embarrassingly large errors. Alternatively, if the authorities refuse to avail themselves of what looks like a useful tool, critical academics can keep the national accounting statisticians on their toes by its use.

On two separate occasions since the late-1970s, the CSS has made major errors in the manufacturing sector output estimates. On neither occasion did the authorities behave in the appropriate manner towards users. In defence of the CSS, the SARB claims that news of the first of these episodes was widely disseminated. A search through the most popular of the weekly financial periodicals failed to reveal any trace of awareness of this major error. On the second occasion, the same strange method of making the error 'widely known' was employed, with the result that no-one except dedicated readers of the explanatory notes of the CSS' Statistical News Releases can have known of the errors.

Following in the footsteps of the CSS, the NPI has used these incorrect figures (supplemented by errors of their own making) to produce substantially incorrect analyses of the productivity performance of the South African manufacturing sector. That the NPI has acknowledged both that their analyses overstated the case against labour, and that persistence with the critique of the CSS output estimates has borne fruit, is an encouraging sign.

The wage/productivity debate once more

Having started the research project of which this study is part with a foray into the field of productivity statistics - primarily because it was believed that the state and capital were using the poor quality output of the NPI as a lever in debates over the relationship between wages

¹ This admission comes in the form of the inadequately-publicised 'reconstructed' estimates in SNR P3041.3 of 12 November 1993. The five or six weeks that elapsed between the sending of the Statistics Council letter and the publication of the reconstructed figures probably saw a flurry of calculation. It is difficult to believe, however, that some rough estimate of the size of the aggregate error had not already been placed before the committee when the deliberations took place.

and productivity - it seems fitting to end it with a comment on that debate, and with a few remarks on the possible future of the NPI and the CSS.

Much of the debate over wages in South Africa is common currency. Some of it, however, is so sensitive that the sub-text underlying it is not often openly canvassed. There is a tendency for people to skirt around certain aspects of the debate, telling much of the larger story, but leaving unspoken, the full implications of some of the harsh choices that have to be made. That debate has been touched on in Chapter 1-3, but the discussion there was silent on the question to be raised here - namely, what is the appropriate wage policy to adopt under conditions in which there are three groups, a small, mobile, highly-skilled minority; a larger group (still a minority) of relatively well-paid, well-organised, predominantly semi-skilled workers, and a very large group of people on the margins of the modern economy, many of whom teeter on the edge of subsistence, all with competing claims to the total social product?

For purposes of exposition, some of the complexities can be assumed away - amongst them the fact that there are more than two race groups in South Africa - for simplicity's sake, the workforce is treated as though it contained only two groups - white workers, mainly skilled and semi-skilled, and black workers, predominantly Africans in unskilled and semi-skilled jobs in the formal and informal sectors. This ignores the fact that there has been quite substantial movement of black workers up the skill ladder, but it does not alter the larger picture very much.

The origins of the South African labour market problem lie in the well-known fact that white workers (excluding immigrants who arrived on these shores already educated and trained, but including all South African-educated managers, professionals, etc), have, through the political process, managed to monopolise most of the skilled positions in the economy. Many of these workers are very highly skilled and hence, internationally mobile. A high differential between the earnings of white skilled and semi-skilled workers (the civilised wage) was maintained for many decades. Average white standards of living remain high, the much-resented progressive tax system notwithstanding. In absolute terms, before the collapse of the Rand in 1985, white average earnings compared favourably with those in the top group of OECD economies.

Even with the protective barriers erected by a compliant nationalist government, high white earnings could not be sustained without black wages being kept at a suitably low level. Hence the extreme differentials observed over much of the period of South Africa's post-war phase of industrialisation. With the growing assertiveness of the resurgent black unions from the mid-1970s onwards, political mechanisms for maintaining differentials (chiefly repressive

legislation) came under growing strain - economic forces began increasingly to assume responsibility for maintaining differentials at the slightly improved levels to which a decade of worker activism had managed to reduce them. Supported for a while by a fragile social consensus that promoted the tendency towards reductions in differentials through the process of awarding large increases to the lowest paid (black) workers, and small increases or even decreases to most whites, redistributions of income referred to in Chapter 1-2 occurred in some industries over the decade 1975-85.

Growing mass unemployment applied strong downward pressure to black wages at the same time as organisational strength put upward pressure on the wages of those black workers fortunate enough to be employed in industries organised by unions, chiefly Cosatu affiliates. At the same time, limited economic growth placed a cap on the growth of white wages. The great debate over the way forward to renewed economic growth commenced in earnest, and export competitiveness, never far below the surface, emerged as one of the major pre-conditions for recovery.

The dilemma is as follows - if black worker wages are allowed to rise at a rate in excess of the growth of productivity without a corresponding reduction in white wages, the wage/productivity relativity, already unfavourable by international standards, will deteriorate still further. In the process, the differential between the earnings of black workers in formal employment, and those not (to say nothing of the unemployed) will rise even further. Heavy-handedness in the attempt to hold down white skilled wages will see these workers, already concerned about erosion of living standards over the past decade or so, emigrating in increasing numbers, raising in the process the scarcity rentals of those who remain.

Even if wage growth for all workers is kept in line with average productivity growth, the gap between those in formal employment, and those who are not so fortunate, will continue to widen - unless (improbably) productivity growth in the (low-productivity) informal sector could be rapid enough to prevent this. At the same time, equal proportional earnings increases to white and black workers will result in the maintenance of existing differentials between those two groups, ie, to the consolidation of the apartheid income distribution.

In the long term, an increase in the supply of skilled workers will exert downward pressure on wage levels, with consequent reductions in the levels of inequality. How strong this effect will be is not easy to predict - the experience of blacks moving into previously 'white' jobs has sometimes had the effect of lowering the average wage (truck drivers being a case in point). But movement into managerial and professional positions is unlikely to be accompanied by falling average earnings.

Overlaying all of this is inflation, seemingly endemic. If every worker could be persuaded to bargain for an increase equal to the expected rate of inflation plus some fraction of the growth in productivity² (or, in the event of negative productivity growth, the expected rate of inflation minus the decrease in productivity), with the rest of the productivity growth being devoted to a reduction in the rate of growth of prices, then inflation could slowly be brought under control. The owners of capital would, of course, have to exercise similar restraint.

Not much imagination is required to see that the social contract required to bring about such a state of affairs would be difficult to achieve even in a country with relative social homogeneity and little history of oppression and class warfare. In a heterogeneous country like South Africa, racked by decades of vicious class and racial struggles, the chances that such a pact could be concluded must be slight. Yet this is what the set of forces called 'economic reality' is urging upon leaders, with not wholly unpredictable results, to wit, an increase in militancy, precisely at the time when the spirit of reconciliation is supposed to smother such struggles.

The new 'reality' is articulated most forcibly by economic experts who lose no opportunity to point out to the new élite, the labour aristocracy now represented, according to them, by the unions affiliated to Cosatu, that wage increases beyond a certain (fuzzily specified) size are not in the national interest.³ Invoking the national interest is problematic, yet in truth, there is such a thing, in the name of which sacrifices must, on occasion, be made. The problem is that the burden is seldom equally shared. What one sees instead is the odd spectacle of privileged professionals (paid six-figure salaries) calling on the new aristocracy (being paid between R10 000 and R20 000 a year) to exercise restraint so that the very poorest may share in the fruits of economic growth and development. Now it may well be that such restraint could have the desired effect of stimulating growth (through exports?) and a slow rise in all incomes. Regardless of whether it does or not, the effect on the living standards of the expert advisers (and of many other skilled workers) is likely to be minimal. The incongruity of such behaviour seems lost on those for whom 'economic reality' is paramount.

2 Even the NPI recognises the unlikelihood that anything less than this will be demanded by workers. Visser observed in 1985, for example, that:

"Wage claims are based on expectations about the inflation rate. It is naive to expect workers to accept wage increases equal to or lower than productivity growth rates." (*Sunday Times*, 15 September 1985)

3 Sometimes those responsible for excess wage demands are identified, sometimes not. The end-result, however, is the same - the claim that lower wages are necessary, desirable and/or inevitable. Take, for example, the *Financial Mail's* Board of Economists in 1988 (André Hamersma, Aubrey Dickman, Brian Kantor and Raymond Parsons) discussing the topic of the structural weakness of the South African economy. In response to a question about the rate at which the economy could grow, Dickman said:

"The wage-setting process must take place in market realities and of course that means lower wages. You'll get them anyway, through the exchange rate."

Kantor then commented that:

"High wages bias development against the formal sector. If the formal sector - as it clearly does - feels obliged to pay what objectively looks like above-market clearing wages for much of its labour, it is prejudicing growth" (*Financial Mail*, June 17 1988)

As noted in Chapter 1-2, those researchers busy exploring the possible terms of a social compact, contract (it goes by many names), are probably closest to getting workable policy proposals thrashed out. Organised workers have less and less need of independent intellectuals to plead their case, but outside of the organised working class, there is a great deal of persuading to be done. This demands a rigour and commitment to seeing the world as it is, rather than as a (potentially) frictionless, conflict-free general equilibrium system. The work in this regard of the likes of Peter Moll (1990) or of Nicoli Nattrass (1992) deserves to become more widely known.

A home for the NPI and a review for the CSS?

As noted in Chapter 1-3, the future of the NPI has not yet been settled. At present, responsibility for it sits somewhat awkwardly with the Minister without Portfolio. At some stage in the not-too-distant future, this will have to be resolved. The CSS is also located, less uncomfortably, in that Ministry. The analysis conducted in this study leads to a few suggestions about what should be done with both institutions. The CSS poses fewer problems than the NPI. Its unambiguously professional status and relatively conflict-free *raison d'être* mean that tidying-up operations only are required. The scope of these, should not however be under-estimated. Apart from the moves to create a properly representative and competent Statistics Council referred to in Chapter 2-1, a full-scale review of the activities of the CSS is necessary to change its direction. A professed commitment to the ideals expressed in the United Nations Handbook of Statistical Organisation (UN, 1980) provides no guarantee that the CSS will deliver the services required of it.

The NPI is another matter altogether. In the first place, a re-organisation of the statistics-producing function is required. As has been pointed out above, one of the most basic precepts regulating statistical authorities is that the attempt to secure acceptance of official statistics can succeed only if the impartiality and neutrality of the producer of statistics is beyond reproach. This means that although analysis is permitted, advocacy of any kind is strictly forbidden. What is more, such analysis as is conducted must be subject to rigorous peer evaluation. The best producers of productivity statistics in the world, for example, the Bureau of Labor Statistics in the USA, Statistics Canada and the Australian Bureau of Statistics, adhere scrupulously to these rules, knowing full well that failure to do so means losing the confidence of users. As far as may be ascertained, trade unions in all three countries accept the productivity estimates and analyses without demur.⁴

⁴ As a guard against excessive admiration of these institutions, it is as well to recall at this point, the complaints referred to in Chapter 1-1 about the quality of the output of the BLS.

Mindful of the useful contribution which the NPI has made in the field of practical improvements to the productivity performance of a large number of enterprises and organisations, both state and private, a raft of supporters has argued quite strongly in favour of its continued existence. If the NPI is to retain the odd corporate status it presently enjoys (non-profit company) it is necessary for the productivity statistics function currently produced by the NPI to be produced by some other body. The task might have to be carried out by the CSS, or possibly by a new institution operating under the umbrella of the newly-formed National Economic Development and Labour Council (NEDLC). This, of course, cannot prevent the NPI from continuing to publish works such as *Productivity Focus*, or even *Productivity Statistics* (which are sold by subscription to private clients), but it would serve to lift the productivity statistics out of the political arena. In the event of a disagreement on performance, the official statistics could easily be subjected to scrutiny of the Statistics Council.

As to the organisational form of the NPI, there are many possibilities. One of these is that the organisation specialise increasingly in the consulting (advisory) work in which it has a strong comparative advantage. Another is that it be absorbed into the NEDLC and thoroughly restructured. If that does happen, devising suitable organisational forms to accommodate the private consultancy activities will constitute a serious challenge to those charged with the responsibility for its restructuring. My inclination is towards the hiving off of the consultancy aspects of the organisation from its public service activities. At a National Manpower Commission workshop on "Policy options to improve union-management co-operation with a view to advancing productivity as a national goal" held at Midrand in October 1993, I gave a paper called "Some thoughts on productivity in the new South Africa" (Meth, 1993c). It would be useful to be able to refer readers to this document, saying, that is what I think needs to be done to create a viable institution to foster productivity growth in South Africa. Unfortunately, like so much of the work that I have done on productivity, this paper too is unpublished. I take the liberty, therefore, of reproducing that part of it containing the policy prescriptions and cautionary notes which have particular relevance for this study.

Constructing a new bi-partite institution

Interventions by economists in the debate on the institutions and processes required to facilitate productivity improvements in South Africa can help to derail the process if the wrong advice is proffered and it falls on receptive ground. For example, misbegotten attempts by economic commentators to capture the moral high ground, either on behalf of what they conceive of as their 'constituency', or for the more amorphous 'national interest' can, if heeded by the

negotiators at the 'coal face', effectively paralyse the workings of the institutions that will be formed in the reconditioned South Africa.

Patient and careful work is required to build bridges between historical antagonists. This is especially so since many of the causes of past antagonisms still loom large over the parties. The theoretical perspective within which I operate, and from which I draw this lesson says that economic relations in capitalist economies are fundamentally contradictory or conflictual. That means that many of the antagonisms are structural, and hence cannot be eradicated by a process of consensus building, no matter how abundant is the supply of goodwill. That does not mean that consensus cannot be reached in a significant number of areas. It does mean though that the participants in the process would do well to be of modest ambition. It also means that patience is called for, and that results will not come overnight. Attempts at consensus building are not unique to South Africa, as everybody knows - there is much to be learned both from failures and from successes elsewhere. Canada is a case in point. The Canadian Labour Market and Productivity Centre (CLMPC), a bi-partite body formed in 1984 to:

"... promote dialogue between Canada's economic stakeholders and to develop joint business/labour recommendations on approaches to improving the operation of the labour market and Canada's productivity performance." (CLMPC, 1992, p9)

was virtually paralysed for 5 years because labour and capital simply could not reach sufficient agreement on matters of consequence. In recent years, enough realism has been injected into the proceedings to make significant advances possible. The longish passage below from a statement by the Economic Restructuring Committee on the starting point for their deliberations shows the kinds of accommodations that have been made. Jointly-drafted, it reads as follows:

"From the outset of this economic restructuring project, we have been guided by the belief that effective and equitable restructuring must promote several key social and economic objectives:

high and rising standards of living marked by full employment, reduced structural unemployment and low inflation;

wealth creation to ensure economic and social progress;

societal equity to provide a sharing of the costs and benefits of change; and

economic development that is sustainable in environmental terms.

While business and labour continue to place different priorities on these objectives, we believe they are all critical elements in meeting our restructuring challenges.

We recognize that business and labour have different perspectives on the underlying causes of the structural problems that face our economy, and on the most effective responses to these problems. These differences

are most evident in policy areas such as monetary and fiscal policy, trade policy, deregulation, and government deficits. Through ongoing follow-up discussions, business and labour will continue to explore areas of possible consensus on these issues.

At the same time, however, we have found common ground that provides a starting point for effective and equitable restructuring of the economy. The common ground includes the following *guiding principles*." (CLMPC, 1993, p1)

The second of the guiding principles is headed "Promoting Productivity and Security", and it reads as follows:

High productivity and security are keys to a high-value added strategy. High productivity is essential to generate the returns which provide income and employment security. Income and employment security strengthen the commitment to innovation, quality, customer satisfaction, and the other factors that lead directly to higher productivity. Though business and labour emphasize different aspects of this interrelationship, there is a common understanding that both are critical factors in ensuring effective and equitable restructuring." (p2)

There follows a series of recommendations, many of which are similar to the sorts of things that Cosatu has proposed over the years. The discussion of the recommendations on 'Enterprise and Workplace Reorganization' is particularly interesting - the following passage from it illustrates the progress made by this bi-partite body in working towards consensus. The relevant passage states that:

"Despite the value of increased labour involvement in decision making, too many workplaces plan work reorganizations and technological changes without labour's input. A recent survey by the Conference Board of Canada found that "ninety percent of organization respondents said that the decision to implement technological change is 'solely a management prerogative. Half of these respondents qualified their answers with the comment that while they believe the final decision is management's, unions should be consulted and allowed input." Work reorganization developed in a joint and equal process with labour tends to provide better results for both management and labour. A study of 56 unionized plants in the United States found that those firms that sought co-operative relations with their unions reported a 29 percent increase in value-added per worker over the course of a decade. Firms that did not pursue such relations with the union reported a 15 percent decline over the period." (CLMPC, 1993, pp29-30)

From the foregoing the following lessons may be drawn:

- i From all that appears in the CLMPC 'statement', it is clear that areas of difference are recognised as being as important as areas of agreement, and are respected as such. These differences stem mainly from the different theoretical perspectives deployed by the two sides.
- ii Since it seems to be accepted that important differences of principle exist and will continue to do so, there is no need for any posturing over the 'big' questions - struggles over redistribution, struggles over wages and struggles over economic democracy in such sensitive areas as managerial prerogative.
- iii It seems unlikely that anything other than a bi-partite body with equal representation of employers and labour can reach the kinds of agreement embodied in the 'statement'.

It is **recommended** that an investigation into the most appropriate form for a body for fostering productivity improvements in South Africa be conducted post-haste. The merits of a bi-partite body similar to the Canadian CLMPC should be carefully considered.

The other recommendation which I wish to reproduce takes the measurement question a fair distance beyond the problems encountered in valuing manufacturing sector output. Under the heading "Need for more sophistication in the measurement of productivity", I argued that development in South Africa in the medium-term is likely to include a vast increase in the basic services previously denied some large section of the population. These include health and education, security (policing) and infrastructural provision. In addition to that, there is also likely to be substantial growth in low-wage, low-productivity employment.⁵ This latter may not be much to the liking of organised labour, but so-called 'formal-sector' employment growth is going to be slow relative to the growth in the number of potential workers. The services involved in every instance above are notoriously difficult to value - in the past many of them have been measured by such useless indicators as the number of persons involved in delivering the service concerned. The implicit zero productivity growth assumption embodied in this procedure is unscientific and seriously misleading. In other words, welfare⁶ could improve significantly even though productivity and GDP growth are sluggish.

If those responsible for measuring changes in productivity proceed in the manner in which they have done previously, the future is likely to be as badly misrepresented as was the past. The use of such crude measures as per capita GDP in a vain attempt to capture medium-term changes in welfare must cease, and be replaced by serious scholarship. The record of the NPI in this regard is not good, and although there is evidence that this is finally beginning to change, the intellectual resources presently devoted to this complicated and difficult task are inadequate. The question of what constitutes an appropriate set of productivity and welfare measures needs to be carefully rethought, relevant surveys must be devised and administered by an institution that enjoys the support of the majority of the population. It is **recommended**

5 This growth pattern is likely to lead to an outcome not unlike the much-agonised over productivity decline in the USA. The reasons for this differ in important ways, but there are sufficient similarities to make aspects of a recent theory of productivity growth (decline), asymptotic stagnancy (Rowthorn, 1992), seem applicable to the South African case. In terms of this approach, the poor productivity performance of the US economy is explained by the fact that productivity growth is likely to be dominated by the least, rather than the most dynamic sectors of the economy. Limits to the absorptive capacity of the dynamic sectors means that employment growth takes place in the stagnant sectors. Under these conditions, even without change in productivity growth rates, overall productivity performance must decline. Thus in an "... asymptotically stagnant system, what ultimately matters is not the rate of productivity growth in the most dynamic sectors, but the least." (1992, p489) One important difference is that the characterisation of government employment as being in the 'least productive' sector does not necessarily follow, at least not in South Africa.

6 The basic question of whether output measures are to be viewed from the point of view of production possibilities or from the point of view of welfare was raised by Hicks as long ago as 1940. See Hjerrpe (1980, p239).

that an intellectual resource capable of tackling measurement problems be created. This must be located within an official institution whose neutrality is beyond question. The body must be capable of producing high-quality academic analysis that stops well short of any advocacy.

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