The Male Marital Earnings Premium in the Context of Bride Wealth Payments: Evidence from South Africa

DANIELA CASALE and DORRIT POSEL
University of KwaZulu-Natal

I. Introduction

A well-documented finding internationally is that men who are married earn significantly more than men who are not married, even after controlling for differences in the observable characteristics of these men. The reasons for the marital earnings premium have been explored extensively in the literature over the past 30 years. The two main and competing explanations are the productivity hypothesis (marriage makes men more productive and therefore they earn more than other men) and the selection hypothesis (men who are selected into marriage are those men who would also do better in the labor market). To control for selection on the basis of time-invariant individual attributes, studies have generally used panel data to estimate fixed effects models. Typically this is found to reduce the size of the marital earnings premium, indicating that selection into marriage does matter. But in most studies, a positive and significant earnings premium to marriage persists. The remaining differential is interpreted as the "returns" to marriage, and much of the literature then explores why marriage would increase men's productivity.

Although fixed effects estimation techniques remove the problem of individual heterogeneity, they do not control for another source of endogeneity bias in the earnings estimation. If men with faster earnings growth are positively selected into marriage, then the fixed effects estimator will continue to overstate any real gains to marriage. Few studies in the literature refer to this possibility, but those that do find no evidence of such selection.

This study investigates the nature of the marital earnings premium among black men in South Africa. What makes a study of the marital earnings premium in South Africa particularly interesting is the payment of bride wealth (known as *ilobolo*) to validate a traditional marriage. If *ilobolo* payments are a

We thank the editors and referees for their constructive and insightful comments and Economic Research Southern Africa (ERSA) for their support in the completion of this study. Please contact the corresponding author, Daniela Casale, at casaled@ukzn.ac.za.

constraint to marriage, then we might expect to find evidence of selection into marriage not only on the basis of unobservable attributes but also on the basis of high earnings growth. Men who experience faster earnings growth, for example, may be able to accumulate *ilobolo* payments more quickly than men whose permanent income is the same but whose earnings grow more slowly over a comparable period of employment.

We use the September 2004 Labor Force Survey to show that a robust marital earnings premium exists for black men in cross-sectional regressions and to compare the premium for marriage and cohabitation. To investigate the selection of men into marriage, we use the six waves of the recently released Labor Force Survey Panel from 2001 to 2004, the first national panel of this kind available in South Africa. We find that the marital earnings premium falls considerably when we control for individual fixed effects. Furthermore, we show that among unmarried men, earnings growth is positively related to the probability of marriage in the subsequent years of the panel, suggesting that the fixed effects estimator may still have upward bias.

In the next section, we summarize the literature on the marital earnings premium, and in Section III we describe the practice of bride wealth in South Africa. We analyze the marital earnings premium using cross-sectional data in Section IV. In Section V, we investigate the quality of available panel data in South Africa, and we test for the selection effects of marriage. The last section summarizes our key empirical findings.

II. Explanations for the Marital Earnings Premium

Since the 1980s, a growing body of literature, predominantly from the United States, has developed to account for the common finding that married men earn significantly more on average than men who are not married. A robust marital earnings differential has been found to exist even after controlling for observable differences between married and unmarried men. Estimates of the conditional marriage premium have generally ranged between 10% and 30%, comparable in size to the race and union wage differentials in the United States, for example (Korenman and Neumark 1991).

Two main hypotheses have emerged to explain the marital earnings premium. The dominant theory, which draws on Becker's (1965, 1981) model of household time allocation, is that marriage makes men more productive. Marriage allows for economies of scale in home production and the specialization of labor, with men traditionally specializing in market activities and women in home production. Married men will therefore have greater opportunities to accumulate human capital in market activities than single men, thereby increasing their productivity and wages.

The competing hypothesis (Nakosteen and Zimmer 1987) is that men who are married would have done better in the labor market regardless of their marital status. In other words, there is a selection of men into marriage based on unobservable individual characteristics that are also rewarded in the labor market and that translate into higher wages. Rodgers and Stratton (2005, 6) provide an extensive list of personal traits that might be valued in both the marriage and labor markets: ability, attitude, self-esteem, congeniality, loyalty, honesty, dependability, leadership, industriousness, and even physical appearance.

Attempts to control for selection into marriage have included estimating cross-sectional earnings equations in a two-stage Heckman selection model (Nakosteen and Zimmer 1987); using twin or sibling data to control for genetic and/or family endowments (Loh 1996; Antonovics and Town 2004); and, most often, using panel data on individuals to control for time-invariant unobservable characteristics in a fixed effects model (Korenman and Neumark 1991; Cornwell and Rupert 1997; Gray 1997; Hersch and Stratton 2000; Stratton 2002; Rodgers and Stratton 2005; Ahituv and Lerman 2007).

The general consensus that emerges from this empirical literature is that selection into marriage matters—but not that much. Regardless of the data or methodology used, a mostly consistent finding is that, even though accounting for selection may reduce the marriage premium, a substantial portion remains. Selection effects are typically found to be responsible for less than 20% of the premium (Stratton 2002).

Another possible selection mechanism at play, referred to by only a few studies, derives from the endogeneity of marriage in a dynamic context. If men with faster wage growth are more likely to get married, then this selection effect would not be controlled for in a fixed effects model. To test for whether the change in marital status is endogenous, Korenman and Neumark (1991) and Gray (1997) look at whether single men, who have faster wage growth in a preceding period, are more likely to get married in a later period. However, neither study finds evidence of such endogeneity.

Given that a substantial portion of the marriage premium cannot be explained by selection effects, a large part of the international literature focuses on trying to uncover the nature or causes of the productivity effect. Here the evidence is more mixed. Controlling for the number of years married, Korenman and Neumark (1991), Gray (1997), and Stratton (2002) find that wages continue to grow at a faster rate throughout the marriage. This is taken as evidence of specialization occurring over the course of the marriage. However, the results in Cornwell and Rupert (1997) and Hersch and Stratton (2000) imply that the benefits of marriage are better described by an intercept shift rather than a steeper earnings slope for married men. Cornwell and Rupert (1997, 292) suggest that "such a shift in the wage-generating process might be regarded as the effect of 'settling down'—a kind of structural break involving adjustments in market work and homework in the move from single to married life." Ahituv and Lerman's (2007) study suggests a combination of an intercept effect in hours worked and a slope effect in wage rates: marriage quickly increases hours worked but the effect on wage rates occurs over time as marriage continues.

Other attempts at understanding the causal mechanism driving the productivity effect have involved using hours worked by the wife as a proxy for specialization in the household. The prediction is that married men whose wives work longer hours will earn less than married men whose wives work fewer hours or who do not work at all. Gray (1997) and Chun and Lee (2001) find the expected wage penalty for married men whose wives work or work longer hours. In Jacobsen and Rayack (1996), however, the premium on being married to a full-time housewife does not survive the endogeneity correction, suggesting that wives may adjust their working hours in response to their husbands' wages.[1] As a more direct measure of specialization, Hersch and Stratton (2000) use the actual time spent on housework by men, but they find only a marginally significant negative effect of this variable on earnings and little change to the marriage premium itself.

A third explanation for the marriage premium considered in the literature, although to a lesser extent, is employer favoritism. Employers may discriminate against unmarried men (or married men whose wives work) because of a perceived lower need or because of a preference for men who adhere to certain social norms. But evidence of a wage premium also for self-employed married men casts doubt on this hypothesis (Jacobsen and Rayack 1996).

III. Bride Wealth and the Marital Earnings Premium in South Africa

The studies reviewed in the previous section examined the relationship between marital status and earnings among men in developed countries. In this study, we investigate evidence of a marital earnings premium among black men in a country where bride wealth traditionally is practiced, and where we may

[1] Jacobsen and Rayack (1996) and Loh (1996) even find some evidence that working wives may have a positive effect on men's earnings, implying that either complementarities in household time allocation or positive assortative mating may be at play. Similar evidence has been found for the United Kingdom—while Blackaby, Carlin, and Murphy (1998) found a significant negative relationship between wife's working hours and men's wages for some occupations in the early 1980s, a decade later they found that this penalty had been replaced by a premium, albeit small, for almost all occupations (Blackaby, Carlin, and Murphy 2007).

therefore expect selection to be a more important part of the explanation for the marriage premium.

In South Africa, *ilobolo* is paid by a prospective husband to the bride's family to validate a customary marriage. Historically, this payment was in the form of cattle (commonly 11 cows) and was substantial enough to require that men left their homesteads to engage in "long periods of wage labor" (Hunter 2004, 132). In more recent years, the custom of *ilobolo* has changed in that cash has replaced cattle as a means of payment. However, research suggests that the payment of *ilobolo* remains a significant hurdle to marriage and is a key reason for why marriage rates are lower, and mean age at marriage is higher, among black South Africans than among other population groups (Budlender, Chobokoane, and Simelane 2004; Hunter 2004).

National household surveys in South Africa do not collect information on the payment of *ilobolo*. However, information collected in the 1998 wave of a regionally based panel study (the KwaZulu-Natal Income Dynamics Study) gives some indication of the extent to which *ilobolo* is still practiced and its value. Of the 725 married respondents aged 60 years or younger in the sample, three-quarters (542) reported *ilobolo* payments with marriage. Payment typically involved a combination of cash, cattle, and other livestock: about 68% reported that the *ilobolo* payment included cash; 75% reported payments of cattle and a further 13% of other livestock.

The average value of *ilobolo* reported for people married from 1985 to 1998 was approximately 20,000 Rands in 2000 prices[2] (or almost 13 times the average monthly real earnings of black men in the 1998 sample). The full payment of *ilobolo* typically preceded marriage, but almost 30% of respondents (159/542) reported that some portion had been, or was still, owing after marriage.

The practice and value of bride wealth suggests that selection may account for a larger portion of the marital earnings premium in South Africa than has been found in studies of the United States, for example. First, we would predict that men with unobservable qualities that are valued in the labor market will be more able to afford *ilobolo* and get married. We would therefore expect a large fall in the cross-sectional marriage premium when we control for individual fixed effects.

Second, while little evidence of the endogeneity of changes in marital status has been found in the international literature, we might expect a dynamic selection problem for South Africa: if the payment of *ilobolo* is a constraint to

[2] This value is consistent with reports in the literature of *ilobolo* typically ranging from 10,000 Rands to 25,000 Rands (Kaarsholm 2005; Gustafsson and Worku 2006).

marriage, then men with higher earnings growth may be more likely to marry. There are a number of possible reasons for this. Men with higher earnings growth would be able to accumulate *ilobolo* at a faster rate than men with the same present value of lifetime earnings but whose earnings grow at a slower rate over a comparable period of employment. They therefore could be more likely to get married or to be identified as a viable marriage partner. Faster wage growth may also allow men to borrow more easily against future earnings to pay for *ilobolo*. If changes in marital status are endogenous to changes in earnings, then the marriage coefficient derived from a fixed effects model will still overestimate the true returns to marriage.

Third, we would anticipate significant differences for men who marry and those who cohabit with their partners. A small part of the international literature on the marital earnings premium investigates whether there is an earnings premium also for cohabitation. The expectation is that a premium would exist but that it would be smaller than that found for married men. This is because a cohabiting relationship is likely to be less stable and to involve less specialization (as financial responsibilities are generally shared more equally between the partners). Both Loh (1996) and Stratton (2002) find a significant earnings premium for men who cohabit in the United States and also find that the size of the premium is roughly half that for married men, as expected.

Although cohabitation generally can be seen as a middle-class choice in most developed countries, in South Africa it seems to be more prevalent among the poor (Budlender et al. 2004). In their study of marriage patterns in South Africa, Budlender et al. (2004) highlight that cohabitation is more common among blacks than among the other population groups. If cohabitation among black couples is a second-best strategy for those who cannot afford to get married, then we would anticipate a far lower earnings premium, if any, for men who cohabit.

IV. Analysis of the Marital Premium at the Cross Section

A. Data and Sample

We start the study of the male marital earnings premium in South Africa using cross-sectional data from the September 2004 Labor Force Survey (LFS 2004:2), collected by the national statistical agency (Statistics South Africa). The LFS 2004:2 sampled almost 30,000 households, of which approximately 76% (or 21,761 households) were classified as black. We choose this nationally representative data set both because it collects comprehensive labor market information and because, in contrast to the earlier Labor Force Surveys, the question on marital status distinguishes between marriage and cohabitation.[3] Like all national household surveys in South Africa, however, there is very little information collected explicitly on marriage. In contrast to the data sets used in the United States, for example, there are no questions asked about the length of marriage or about time spent on housework. There is also no background information collected, for example, on the education of respondents' parents, which could be used to instrument for marriage.

In table 1, we compare the mean characteristics of employed men by four categories of marital status: currently married, cohabiting, previously married (divorced or widowed), and never married. In 2004, approximately 44% of the sample of employed men older than 20 years was married, 18% reported cohabiting with their partner, and a further 4% was previously married. The remaining 34% reported never being married.[4]

Average hourly earnings are highest among married men and considerably lower among men who are cohabiting with their partners or men who have never married. Table 1 also describes differences in the observable characteristics of these samples of employed men. On average, men who are married are older than never married men and men who cohabit, but they are younger than men who have been previously married. Married men are also more likely than all other men to report postmatric (i.e., tertiary) education.

A larger proportion of married men lives with children, but particularly older children (aged 7–14 years). For children younger than 7 years, there is little difference between married and cohabiting men, a finding consistent with current research that identifies a large proportion of children born outside of (customary or civil) marriage in South Africa (see Gustafsson and Worku 2006).

B. *Estimation*

We use a standard Mincerian earnings equation and ordinary least squares (OLS) estimation to test for evidence of a male marital earnings premium with the cross-sectional sample. The dependent variable is the log of hourly earnings

[3] There is some concern about the reliability of information provided by respondents reporting on marital status in household questionnaires (see Budlender et al. 2004). In particular, Budlender et al. (2004) suggest that among blacks cohabitation may be underreported both because some cohabitors may not be willing to acknowledge that they are not married and because "the term is often misunderstood, especially when translated into different languages" (Budlender et al. 2004, 5). Nonetheless, we find that the data on marriage and living together are generally consistent, with clear and expected differences between the two groups of men (and similar findings are reported in Budlender et al. 2004, 23).

[4] In contrast, among a comparable sample of white men, about 75% were married (1,090/1,451) and only 6% were cohabiting with a partner.

TABLE 1
DESCRIPTIVE STATISTICS OF EMPLOYED BLACK MEN IN SOUTH AFRICA, 2004

	Married	Cohabit	Divorced/ Widowed	Never Married
Hourly earnings	15.151	8.596	12.245	9.110
	(23.032)	(14.706)	(20.283)	(12.689)
Hours worked per week	46.675	48.126	44.643	46.409
	(14.969)	(14.835)	(18.881)	(15.658)
Age	45.091	38.110	49.166	30.855
	(10.382)	(10.054)	(10.974)	(7.886)
No education	.124	.149	.202	.061
	(.329)	(.356)	(.402)	(.240)
Primary	.349	.333	.411	.235
	(.477)	(.472)	(.493)	(.424)
Incomplete secondary	.280	.316	.229	.353
	(.449)	(.465)	(.421)	(.478)
Matric (completed secondary)	.135	.164	.090	.274
	(.342)	(.370)	(.286)	(.446)
Postmatric (tertiary)	.113	.038	.068	.077
	(.317)	(.191)	(.252)	(.267)
Employee	.802	.845	.725	.852
	(.399)	(.362)	(.447)	(.355)
Living in a metropolitan area	.148	.185	.142	.174
	(.355)	(.389)	(.349)	(.379)
No. children < 7 years	.627	.617	.292	.324
	(.891)	(.798)	(.644)	(.754)
No. children 7–14 years	.827	.532	.526	.472
	(1.079)	(.886)	(.937)	(.928)
Number of observations	3,667	1,512	367	2,830

Source. Labor Force Survey 2004:2.
Note. The sample is restricted to employed men older than 20 years for whom a complete set of observations is available. All individuals who reported hours usually worked in excess of 140 hours per week or as zero, although employed, are dropped from the sample. Standard deviations are in parentheses.

(W), the independent variables include a vector of marital status dummy variables (M_i) as well as a vector of other observable individual and job characteristics (χ_i), and ε_i is the error term:

$$\ln(W_i) = \alpha + \gamma M_i + \beta X_i + \varepsilon_i. \quad (1)$$

Table 2 reports the results from three regressions across which the number of covariates is progressively increased. In the simplest estimation (I), three marital status indicators are included (with never married as the omitted category), and a quadratic in age. Men who are married are estimated to earn 54% more, on average, than men who have not married. In contrast, men who cohabit are estimated to earn less than never married men, although the coefficient is small and only weakly significant.

When additional regressors are included in the regression, the size of the marital earnings premium declines considerably, but it remains large and

TABLE 2

ESTIMATED EARNINGS REGRESSIONS FOR BLACK MEN IN SOUTH AFRICA, 2004

	I	II	III
Married	.432***	.350***	.208***
	(.043)	(.037)	(.032)
Cohabiting	−.068*	−.009	.010
	(.041)	(.036)	(.032)
Divorced/widowed	.229***	.183**	.185***
	(.082)	(.072)	(.054)
Age	.090***	.093***	.049***
	(.013)	(.011)	(.007)
(Age^2)/1000	−1.105***	−.986***	−.464***
	(.154)	(.133)	(.076)
Primary education		.240***	.117***
		(.051)	(.038)
Incomplete secondary		.511***	.281***
		(.052)	(.041)
Matric		.922***	.557***
		(.057)	(.047)
Postmatric		1.780***	1.129***
		(.067)	(.062)
Metropolitan area		.208***	.213***
		(.032)	(.029)
Employee			.315***
			(.041)
R^2	.143	.339	.522
Number of observations	8,498	8,498	8,498

Source. LFS 2004:2.

Note. The sample is restricted to men older than 20 years. The weighted regressions control for clustering and stratification in sample design. Robust standard errors are in parentheses. The omitted marital status and education categories are "never married" and "no schooling," respectively. All regressions include nine dummy variables for province of residence; and estimation III further includes nine occupation and 11 industry dummies that are not reported here.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

significant. The marriage effect falls to 0.35 (or a premium of 42%) when we control for levels of educational attainment in regression II, and to 0.21 (a premium of 23%) when occupation and industry categories are included in regression III. In contrast, the cohabitation effect disappears with controls for education.[5]

[5] The marital earnings premium remains large and significant for different minimum age thresholds defining our sample, and it even increases for men older than 35 (from 0.208 to 0.267). Because we cannot control for years married, this increase may reflect the effects of longer marriages among older men. We also controlled for whether or not a married man's spouse was resident in the household, but this had no effect on these earnings estimations. About 18% of our sample of employed men who report being married also report their spouse not currently resident in the household. The obvious explanation for this is the temporary (or circular) labor migration of either

A substantial earnings premium for men who have been married before is also identified in the regressions. The premium to men who are either divorced or widowed is estimated at 0.185 (or 20.3%), only slightly lower than the premium to men who are currently married. As in the case of currently married men, this premium would be consistent with either productivity or selection mechanisms. Productivity benefits may have accrued to men over the course of their former marriage; and men with characteristics that are valued in the labor market may be more likely to have been married earlier in their lives.

Another possible explanation for the marital earnings premium is that employers discriminate in favor of married men. We test this possibility by estimating the marriage effect separately for men with wage employment and for men in self-employment. If a primary source of the premium derives from employer favoritism, then we would expect no (or a significantly smaller) marriage effect for the self-employed. The results presented in table 3, however, show that this is not the case: among the self-employed, men who are married are estimated to earn about 29% more on average than men with similar observable characteristics, and in the same occupational category and industry, but who have not married. The premium is also higher than that among the wage employed (22%), although the standard error is considerably bigger. Similar results are found among men who are divorced or widowed.

Our estimates of the marital earnings differential in South Africa are larger than cross-sectional estimates reported in studies for the United States: in regressions that use comparable specifications, the estimated marriage effect mostly lies between 0.09 and 0.11 (Korenman and Neumark 1991; Loh 1996; Gray 1997; Chun and Lee 2001; Ahituv and Lerman 2007). In contrast to other studies (Loh 1996; Stratton 2002), we also find no evidence of a positive cohabitation effect.

However, our cross-sectional estimates of the marriage premium may be biased upward, first because of the omission of unobserved time-invariant variables that affect outcomes in both the marriage and the labor market, and second because changes in marital status may not be exogenous to changes in earnings. Our ability to address these problems in the cross section is greatly limited by the availability of appropriate instruments in the LFS 2004:2. We therefore turn to a less detailed, and therefore somewhat "cruder," data set but one that permits fixed effects analysis using panel data.

---

the husband or the wife (see, e.g., Posel and Casale 2006). We found a comparable marital earnings premium (= 0.193) when married men in the sample are restricted to those with resident spouses.

TABLE 3
ESTIMATED EARNINGS REGRESSIONS FOR BLACK MEN IN SOUTH AFRICA, 2004

	Self-employed (IV)	Employees (V)
Married	.253***	.202***
	(.096)	(.033)
Cohabiting	.059	.003
	(.100)	(.031)
Divorced/widowed	.271**	.166***
	(.126)	(.054)
Age	.041**	.048***
	(.014)	(.007)
(Age^2)/1000	−.393**	−.447***
	(.138)	(.076)
Primary education	.163	.101***
	(.111)	(.036)
Incomplete secondary	.316***	.268***
	(.121)	(.038)
Matric	.506***	.574***
	(.136)	(.045)
Postmatric	.755***	1.161***
	(.186)	(.062)
Metropolitan area	.359***	.191***
	(.116)	(.027)
R^2	.509	.521
Number of observations	1,481	7,017

Source. LFS 2004:2.
Note. The sample is restricted to men older than 20 years. The weighted regressions control for clustering and stratification in sample design. Robust standard errors are in parentheses. The omitted marital status and education categories are "never married" and "no schooling," respectively. The estimations also include nine province dummy variables, nine occupation, and 11 industry dummies, which are not reported here.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

V. Selection and the Marital Earnings Premium

A. Data and Sample

Two possible sources of panel data in South Africa are the KwaZulu-Natal Income Dynamics Study (KIDS) and the Labor Force Survey (LFS) Panel. The KIDS data, collected over three waves from 1993 to 2004 for one of South Africa's nine provinces, KwaZulu-Natal, potentially offer a rich source of information because questions have been asked about the payment of bride wealth or *ilobolo*. However, these questions have only been included in one wave of the panel (in 1998). Also, and more restrictive for our study, comprehensive labor market information is not collected consistently for the employed, marriage and cohabitation are not distinguished, and it is difficult to interpret the marital status information collected. In a fixed effects analysis, we are also limited by a very small number of individuals who changed marital status over the panel.[6]

An alternative data set is the LFS Panel of 2001–4, made available by Statistics South Africa in January 2007 and the first source of national panel data in the country. Although the biannual LFSs are released as cross-sectional data sets, the survey is designed as a rotating panel, with a 20% rotation of dwelling units planned in each wave. The LFS Panel therefore consists of a subset of individuals living in those dwelling units that were matched across six waves of the LFS, from September 2001 to March 2004 (in September 2004 a new master sample was drawn).[7] The advantages of these panel data for our study are that detailed labor market information is collected and the sample is considerably larger than that for KIDS. In the sample of 12,568 employed black men in the panel, 1,549 individuals changed marital status. Among the marriage switchers specifically, about 70% were from not married to married, and 30% were from married to not married (i.e., to widowed, divorced, or separated).

However, there are a number of limitations in the scope of the panel, and in the nature of the information collected, which restrict our analysis. First, the tracking unit for the panel is the dwelling place rather than the household, and the panel therefore consists only of those individuals who stayed in the same dwelling; individuals who left the dwelling could not have been matched over time.[8] We therefore will not be identifying any change in marital status that coincides also with a change in the dwelling place. Second, the unit of analysis is the individual. No attempt has been made to link individuals to household members who have remained coresident over time, and consequently, there are no household-level variables that can be used in our study. Third,

[6] Furthermore, of the 40 "switchers" out of a sample of about 500 employed black men in the panel in 1993 and 1998, all were from not married to married, and fewer than half of these were clearly first marriages, indicating potentially large measurement error.

[7] Although the survey was conceptualized as having a 20% replacement of dwelling units in each wave, in practice, a rotation rate of less than 20% occurred in some of the waves, so that a small percentage of the total matched individuals (about 5%) remain in the panel for six waves, rather than the expected five waves (more details can be found in Statistics South Africa 2006).

[8] Statistics South Africa (2006) acknowledges also that because their matching procedures (both manual and computerized) found many "mismatches," data were edited across the waves. Although we found no inconsistencies in the age of our respondents across the waves of the panel, about 7% of the sample "lost" years of schooling as the panel progressed. The majority of these inconsistencies (about 70%) were heaped at matric (grade 12), suggesting that reporting on matriculation, in particular, may be inflated. We assumed that a lower level of educational attainment, reported in a subsequent wave of the panel, was the "true" level and we adjusted years of schooling downward to match this level. Our results are robust also to dropping these mismatches from the sample or to adjusting education upward in subsequent waves.

TABLE 4
SAMPLE CHARACTERISTICS OF EMPLOYED BLACK MEN, 2001 AND 2003

	2001		2003	
	Matched Sample in Panel	Full Cross-sectional Sample	Matched Sample in Panel	Full Cross-sectional Sample
Proportion married/living together	.670	.645	.692	.625
	(.470)	(.479)	(.462)	(.484)
Proportion divorced/widowed	.041	.043	.049	.044
	(.199)	(.212)	(.216)	(.206)
Proportion never married	.289	.313	.258	.330
	(.453)	(.464)	(.438)	(.470)
Age	40.278	39.290	40.387	39.326
	(11.302)	(11.410)	(11.386)	(11.318)
Years of schooling	7.759	7.665	8.423	8.017
	(4.147)	(4.153)	(4.030)	(4.087)
Proportion with postmatric	.089	.081	.097	.083
	(.285)	(.273)	(.296)	(.277)
Hourly earnings	10.379	9.539	12.924	11.866
	(13.784)	(12.890)	(31.605)	(25.084)
Number of observations	4,616	9,210	4,349	8,402

Source. LFS 2001:2; LFS 2003:2; LFS Panel.
Note. The data are not weighted. Standard deviations are in parentheses. The sample is all men aged 20 years and older with employment. All individuals who reported hours usually worked in excess of 140 hours per week or as zero, although employed, are dropped from the sample.

no weights have been provided for the LFS Panel, and there is no obvious way of generating these weights. It does not seem possible to link individuals in the panel back to their information (both individual and household) in the original cross-sectional LFS data sets, as unique identifiers have been replaced. Fourth, and frustratingly for our particular study, the LFSs prior to September 2004, and therefore all the LFSs included in the panel, do not distinguish between marriage and cohabitation.

In table 4, we compare the sample of employed men who were matched in the panel with the full sample surveyed at the cross section, for September 2001 (or wave 1 of the panel) and September 2003 (wave 5 of the panel). The differences in the average characteristics of the samples are not that large, given the concerns raised above. However, because married or cohabiting men are less likely than never married men to move from a dwelling, they are overrepresented in the longitudinal data, and this obviously becomes more pronounced over the course of the panel. Analogously, never married men who are geographically more mobile are underrepresented in the longitudinal data.

We investigate further in table 5 whether the subsamples of married and not married men remaining in the panel are different from the samples taken from the LFS cross-sectional data. There is some evidence that both currently and previously married men in the panel are more educated and more highly

TABLE 5
SAMPLE CHARACTERISTICS BY MARITAL STATUS, 2001 AND 2003

	2001		2003	
	Matched Sample in Panel	Full Cross-sectional Sample	Matched Sample in Panel	Full Cross-sectional Sample
Married/living together:				
Age	43.680	42.582	43.559	43.042
	(10.377)	(10.746)	(10.316)	(10.424)
Years of schooling	7.361	7.203	8.055	7.535
	(4.275)	(4.222)	(4.169)	(4.207)
Proportion with postmatric	.097	.084	.103	.087
	(.296)	(.277)	(.304)	(.282)
Hourly earnings	11.488	10.357	14.510	13.547
	(14.674)	(13.347)	(36.931)	(30.259)
Number of observations	3,093	5,937	3,011	5,254
Divorced/widowed:				
Age	48.162	47.625	48.565	47.710
	(10.245)	(10.562)	(11.384)	(10.821)
Years of schooling	6.634	6.291	7.051	6.576
	(4.228)	(4.248)	(4.620)	(4.305)
Proportion with postmatric	.052	.056	.098	.064
	(.223)	(.230)	(.298)	(.246)
Hourly earnings	9.648	9.484	12.540	11.625
	(13.046)	(14.315)	(17.066)	(15.156)
Number of observations	191	392	214	373
Never married:				
Age	31.247	31.337	30.334	31.163
	(7.765)	(8.247)	(7.156)	(8.013)
Years of schooling	8.845	8.805	9.667	9.123
	(3.589)	(3.738)	(3.161)	(3.555)
Proportion with postmatric	.077	.079	.081	.079
	(.267)	(.269)	(.273)	(.270)
Hourly earnings	7.911	7.861	8.748	8.716
	(11.190)	(11.493)	(11.497)	(11.207)
Number of observations	1,332	2,881	1,124	2,775

Source. LFS 2001:2; LFS 2003:2; LFS Panel.
Note. The data are not weighted. Standard deviations are in parentheses. The sample is all men aged 20 years and older with employment. All individuals who reported hours usually worked in excess of 140 hours per week or as zero, although employed, are dropped from the sample.

paid on average than those in the cross-sectional sample, but the differences are relatively small and statistically insignificant. The average characteristics of never married men in the longitudinal sample are very similar to those in the cross sections.

B. Estimation

We run two models to estimate the marital earnings premium using the LFS Panel data set. First, to provide a benchmark for comparison, the panel structure of the data is ignored and the six waves are simply pooled. We use OLS to estimate the earnings equation:

$$\ln(W_{it}) = \alpha + \gamma M_{it} + \beta X_{it} + \delta_i + v_{it}, \tag{2}$$

where W_{it} represents the hourly earnings of individual i in time t, M_{it} is a vector of marital status variables, χ_{it} is a vector of individual and employment-related explanatory variables, δ_i is the time-invariant error capturing unobserved individual-specific characteristics, and v_{it} is the idiosyncratic or time-varying error.

The pooled estimation ignores the possibility that δ_i may be positively correlated with marriage if unobserved attributes valued in the labor market are also valued in the marriage market. We control for these individual effects in the second model by estimating the fixed effects or within transformation:

$$\ln(W_{it}) - \ln(W_i) = \gamma^{FE}(M_{it} - M_i) + \beta^{FE}(X_{it} - X_i) + v_{it} - v_i, \tag{3}$$

where for any variable Q, Q_i represents the mean value for individual i over the t periods.

The two estimations are reported in table 6. To better gauge the nature of the longitudinal sample, we also present the results of a pooled OLS regression on the full sample of cross-sectional data from which the panel is drawn (that is, the six surveys from September 2001 to March 2004). A comparison of columns 1 and 2 shows little difference in the estimated coefficients across the two sets of pooled samples. The coefficient on marriage is somewhat higher for the matched sample from the LFS Panel (0.165 compared to 0.152), suggesting that more highly paid married or cohabitating men may be less mobile and therefore more likely to remain in the panel.[9]

In the fixed effects estimation, reported in column 3, the marriage effect remains significant at the 5% level, but it falls by 60% of its value to a

[9] The estimates in table 6 of the divorced/widowed premium from the two pooled regressions are much lower than the premium that was identified using the LFS 2004:2 cross section only in table 2 (0.081 and 0.086 compared to 0.185). Unlike our estimates of the marital earnings premium, the estimated premium to divorced or widowed men is highly variable across the earlier years of the LFS, with the premium being highest in the LFS 2004:2. Consequently, the comparable estimate for divorced or widowed men based on the pooled data from 2001 to 2004 is significantly lower.

TABLE 6
POOLED AND FIXED EFFECTS EARNINGS ESTIMATIONS

	Full Cross Sections (2001–4) (OLS on Pooled Data) (1)	Panel (2001–4) (OLS on Pooled Data) (2)	Panel (2001–4) (Fixed Effects) (3)
Married/living together	.152***	.165***	.065**
	(.008)	(.013)	(.028)
Divorced/widowed	.081***	.086***	.088**
	(.017)	(.024)	(.043)
Age	.053***	.056***	
	(.002)	(.003)	
(Age^2)/1000	−.489***	−.522***	−.074
	(.002)	(.028)	(.133)
Years of schooling	.052***	.053***	
	(.001)	(.002)	
Years of schooling × Tertiary	.046***	.031***	
	(.001)	(.002)	
Employee	.385***	.419***	.179***
	(.010)	(.014)	(.026)
R^2	.534	.543	.066 (within)
Number of observations	53,223	28,269	12,568

Source. LFS 2001:2; LFS 2002:1; LFS 2002:2; LFS 2003:1; LFS 2003:2; LFS 2004:1; LFS Panel (2001–4).
Note. The sample is restricted to employed black men older than 20 years. The data are not weighted. Standard errors are in parentheses. The omitted marital status variable is "never married." All estimations include nine occupation, 11 industry, and five wave (time) dummies. The estimations in cols. 1 and 2 also control for province of residence.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

premium of 6.7% (from 0.165 to 0.065).[10] Although our cross-sectional estimates of the marital earnings premium are considerably larger than those estimated in studies for the United States, the fixed effects estimate is very similar (Korenman and Neumark 1991; Cornwell and Rupert 1997; Gray 1997; Ahituv and Lerman 2007), suggesting larger individual fixed effects in the South African sample.

It is possible that measurement error in marital status is contributing to the large fall in the marriage coefficient, but the conflation of married and living together in the survey questionnaire removes the obvious source of

[10] As expected, given the likely correlation between the time-invariant individual effects and the explanatory variables, a Hausman test rejected the null hypothesis that there is no systematic difference between the coefficients from a random and fixed effects model ($\chi^2 = 783.22$), suggesting that a fixed effects model is more appropriate. We also tested for, but found no evidence of, serial correlation among individual errors in the estimation.

Sample sizes in the LFS Panel are too small to test whether there are differences in the fixed effects estimates for marriage among employees and the self-employed, and for other population groups in South Africa. We find positive coefficients on marriage for these further estimations, but the fixed effects estimates are not significant. We cannot exclude the possibility that this is because of very small samples of switchers and therefore large standard errors in the estimations.

TABLE 7
THE PROBABILITY OF MARRIAGE AND EARNINGS GROWTH

Earnings growth (from t to t + 2)	.002**
	(.001)
Age	.168
	(.157)
(Age^2)/1000	−2.196
	(2.277)
Years of schooling	.100**
	(.050)
Years of schooling × Tertiary	−.045*
	(.027)
$\chi^2(5)$	12.45
Number of observations	243

Source. LFS Panel (2001–4).
Note. The sample is restricted to employed black men older than 20 years. The data are not weighted. Standard errors are in parentheses. We excluded four outliers with reported earnings growth, from September 2001 to September 2002, of 500% or more.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

reporting errors by respondents (see n. 3 for further discussion). The significant fall in the marriage premium is also consistent with strong selection effects on unobservable time-invariant characteristics, predicted by the practice of bride wealth.

However, the fixed effects estimation does not eliminate the potential bias that arises if there is a dynamic selection problem. If men with faster earnings growth are selected into marriage because they are more able to afford *ilobolo* payments, then the fixed effects estimate for marriage will still be biased upward.

We investigate this potential source of endogeneity in marital status using a probit regression to test whether the nature of earnings growth (ΔW_i) over 1 year of the panel influences the probability of marriage occurring (ΔM_i) over the remaining periods of the panel. We estimate:

$$\Pr(\Delta M_i) = \alpha(\Delta W_i) + \varphi X_i + \varepsilon_i, \tag{4}$$

where for a total T waves of the panel, starting in wave t, and for individual i, $\Delta M_i = M_{iT} - M_{it+2}$ (= 1 if the man married/started living together and 0 otherwise), $\Delta W_i = (W_{it+2} - W_{it})/W_{it}$, and χ_i are individual characteristics (age and years of schooling) in initial wave t. The sample in the estimation, therefore, is all men who were present and employed for more than 1 year (or more than three waves of the panel) and who were "never married" in the first year. This restricts our sample size dramatically to only 243 individual men. Nonetheless, as table 7 illustrates, we find that the estimated coefficient on earnings growth is positive and significant at the 5% level. Higher earnings growth in an earlier period increases the probability of "marriage" in the subsequent period. Furthermore, because we cannot identify switchers who change from living together to married, we may be underestimating the strength of this relationship.

The LFS Panel provides no family background or other variables with which to instrument for marriage in the fixed effects estimation and, thereby, address the problem of endogeneity.[11] Although selection into marriage on the basis of earnings growth will upwardly bias the fixed effects estimate of the marital earnings premium, there remain numerous sources of downward bias in the estimate that we also cannot control for. The most obvious comes from the conflation of marriage and cohabitation. Where we are able to distinguish between marriage and cohabitation at the cross section, we find significant differences in the two coefficients: the marriage effect is large and positive, but the cohabitation effect is not significantly different from zero.[12] To the extent that this difference represents a true return to marriage over cohabitation, rather than selection, our fixed effects estimate on the conflated category will be biased downward.

We may also be underestimating any real effects of marriage on earnings because we do not have information on the number of years married (or living together). In the fixed effects model, the marriage premium is estimated on the basis of changes in marital status over the course of the panel. Consequently, marriage duration will be smaller for this group than for the average married man. If the benefits to marriage accrue over time, then by not controlling for years married, the marriage effect will be biased downward (Korenman and Neumark 1991; Gray 1997; Stratton 2002; Ahituv and Lerman 2007). The slightly larger coefficient obtained on the divorced/widowed dummy compared to the married dummy in the fixed effects regression (reported in table 6) is consistent with marriage's effects on wage rates continuing over the course of marriage. The earnings advantage for men who became divorced or widowed

[11] One possible instrument for marriage is local sex ratios (by district council), where we would predict that in districts with higher ratios of unmarried females to males, the probability of men marrying will also be higher. However, sample sizes by district council in the full LFS cross sections are not sufficiently large to generate robust local sex ratios in South Africa over time. There is a further concern that sex ratios may misrepresent the marriage market because of the prevalence and nature of circular labor migration (Posel and Casale 2003, 2006). Estimated sex ratios, which can only be calculated using the available information on resident household members, may underrepresent the number of men available for marriage in areas from which there is high male temporary labor migration.

[12] If we reestimate the OLS earnings regression using the LFS 2004:2, but combining married and living together, the "marriage" effect falls from 0.208 to 0.117.

over the panel is likely to reflect a longer period of marriage than that for the group of individuals who became married over the 3-year period of the panel.[13]

VI. Concluding Comments

Black men in South Africa who are married are estimated to earn about 23% more at the cross section than men who have never married, after controlling for a wide range of observable characteristics. The premium in a fixed effects model, however, is considerably smaller (about 7%), suggesting larger selection effects in South Africa than those typically found in developed countries. Furthermore, unlike the few studies conducted for the United States, we find evidence that the probability of marriage is positively related to the growth in men's earnings in the preceding period. Our findings are consistent with the payment of bride wealth in South Africa creating a barrier to marriage.

The additional source of endogeneity in marital status would suggest that the small premium to marriage estimated in the fixed effects model is still biased upward. However, there are good reasons to suspect that the fixed effects estimator also has downward bias. Simple changes in the collection of data in household surveys—ensuring that marriage and cohabitation are listed as two distinct responses in a question on marital status, and including a question on the number of years married—would eliminate these sources of downward bias and would greatly increase what we can say about the economic returns to marriage for men in South Africa.

References

Ahituv, Avner, and Robert I. Lerman. 2007. "How Do Marital Status, Work Effort and Wage Rates Interact?" *Demography* 44, no. 3:623–47.

Antonovics, Kate, and Robert Town. 2004. "Are All the Good Men Married? Uncovering the Sources of the Marital Wage Premium." *American Economic Review* 94, no. 2:317–21.

Becker, Gary. 1965. "A Theory of the Allocation of Time." *Economic Journal* 75, no. 299:493–517.

———. 1981. *A Treatise on the Family*. Cambridge, MA: Harvard University Press.

Blackaby, David H., Paul S. Carlin, and Philip D. Murphy. 1998. "What a Difference a Wife Makes: The Effect of Women's Hours of Work on Husbands' Hourly Earnings." *Bulletin of Economic Research* 50, no. 1:1–18.

———. 2007. "A Change in the Earnings Penalty for British Men with Working Wives: Evidence from the 1980's and 1990's." *Labor Economics* 14:119–34.

Budlender, Debbie, Ntebaleng Chobokoane, and Sandile Simelane. 2004. "Marriage

[13] That there is very little difference in the coefficient for divorced or widowed men once the individual fixed effects have been controlled for may indicate that among married men, there is a different selection effect for men who divorce or become widowed over the panel or that the earnings premium derives also from productivity benefits of the previous marriage.

Patterns in South Africa: Methodological and Substantive Issues." *South African Journal of Demography* 9, no. 1:1–26.

Chun, Hyunbae, and Injae Lee. 2001. "Why Do Married Men Earn More? Productivity or Marriage Selection." *Economic Inquiry* 39, no. 2:307–19.

Cornwell, Christopher, and Peter Rupert. 1997. "Unobservable Individual Effects, Marriage and the Earnings of Young Men." *Economic Inquiry* 35, no. 2:285–94.

Gray, Jeffrey S. 1997. "The Fall in Men's Return to Marriage: Declining Productivity Effects or Changing Selection?" *Journal of Human Resources* 32, no. 3:481–504.

Gustafsson, Siv, and Seble Y. Worku. 2006. "Marriage Markets and Single Motherhood in South Africa." Tinbergen Institute Discussion Paper 102/3, Tinbergen Institute, Amsterdam.

Hersch, Joni, and Leslie S. Stratton. 2000. "Household Specialization and the Male Marriage Wage Premium." *Industrial and Labor Relations Review* 51, no. 1:78–94.

Hunter, Mark. 2004. "Masculinities and Multiple Sex Partners in KwaZulu-Natal: The Making and Unmaking of *Isoka*." *Transformation* 54:123–53.

Jacobsen, Joyce P., and Wendy L. Rayack. 1996. "Do Men Whose Wives Work Really Earn Less?" *American Economic Review* 86, no. 2:268–73.

Kaarsholm, Preben. 2005. "Moral Panic and Cultural Mobilization: Responses to Transition, Crime and HIV/AIDS in KwaZulu-Natal." *Development and Change* 36, no. 1:133–56.

Korenman, Sanders, and David Neumark. 1991. "Does Marriage Really Make Men More Productive?" *Journal of Human Resources* 26, no. 2:282–307.

Loh, Eng Seng. 1996. "Productivity Differences and the Marriage Wage Premium for White Males." *Journal of Human Resources* 31, no. 3:566–89.

Nakosteen, Robert A., and Michael A. Zimmer. 1987. "Marital Status and Earnings of Young Men: A Model with Endogenous Selection." *Journal of Human Resources* 22, no. 2:248–68.

Posel, Dorrit, and Daniela Casale. 2003. "What Has Been Happening to Internal Labor Migration in South Africa, 1993–1999?" *South African Journal of Economics* 71, no. 3:455–79.

———. 2006. "Internal Migration and Household Poverty in Post-Apartheid South Africa." In *Poverty and Policy in Post-Apartheid South Africa,* ed. Ravi Kanbur and Haroon Bhorat, 351–65. Pretoria: Human Sciences Research Council Press.

Rodgers, William M., and Leslie S. Stratton. 2005. "The Male Marital Wage Differential: Race, Training and Fixed Effects." IZA Discussion Paper no. 1745, IZA, Bonn.

Statistics South Africa. 2006. "The South African Labor Force Panel Survey Methodology Document." National Statistics System Division, Pretoria.

Stratton, Leslie S. 2002. "Examining the Wage Differential for Married and Cohabiting Men." *Economic Inquiry* 40, no. 2:199–212.