The male marital earnings premium in the context of bridewealth payments: Evidence from South Africa

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Abstract

This study explores the nature of the marital earnings premium for African men in South Africa using the September 2004 Labour Force Survey and the Labour Force Survey Panel (2001 – 2004). We show that a robust and positive premium to marriage in cross-sectional estimations is substantially reduced after controlling for individual fixed effects. Furthermore, we find evidence of an additional source of endogeneity created by the positive selection into marriage of men with faster earnings growth in the initial periods of the panel. Our results are to be expected if the payment of bridewealth or *ilobolo*, by a prospective husband to the bride's family, is a significant constraint to marriage for African men.

1 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 thirty 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 labour market). To control for selection on the basis of time-invariant individual attributes, studies have 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 married, 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.

The primary objective of this paper is to investigate the nature of the marital earnings premium among African men in South Africa, using household survey datasets that are currently available.

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What makes a study of the marital earnings premium in South Africa particularly interesting is the payment of bridewealth (known as *ilobolo*) to validate a traditional African marriage. If *ilobolo* payments are a 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. Because our study is limited fundamentally by the nature and quality of available data, our secondary objective, by default, is to outline these limitations, thereby highlighting the kind of information that needs to be collected in South African household surveys (and which is collected routinely in comparable surveys in other countries).

We use the September 2004 Labour Force Survey to show that a robust marital earnings premium exists for African men at the cross-section, and to compare the premium for marriage and cohabitation. To investigate the selection of men into marriage, we use the six waves of the Labour Force Survey Panel from 2001 to 2004. We find that the marital earnings premium falls considerably when we control for individual fixed effects. Furthermore, we show that earnings growth for unmarried men 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 summarise the literature on the marital earnings premium and we describe the practice of bridewealth in South Africa. In section 3, we analyse the marital earnings premium among African men using cross-sectional data. In section 4, we investigate the quality of available panel data in South Africa, and we test for the selection effects of marriage. The last section summarises our empirical findings and highlights simple ways in which data collection marriage in subsequent panel studies in South Africa could be improved.

2 Background and context

Since the 1980s a growing body of literature, predominantly from the United States (US), 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 40 percent, comparable in size to the race and union wage differentials in the US, for example (Korenman and Neumark 1991).

Two main hypotheses have emerged to explain the marital 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 specialisation of labour, with men traditionally specialising 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 labour market regardless of their marital status. In other words, there is a selection of men into marriage based on individual characteristics that are also rewarded in the labour 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 labour 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).

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 percent of the premium (Stratton 2002).

Another possible selection mechanism at play, referred to by only a few studies, derives from the endogeneity of marriage. If men with faster wage growth are more likely to get married, then this selection effect would not be controlled for in fixed effects models. To test if the variation in changes in marital status over time 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 focusses 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 some evidence of specialisation 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".

Other attempts at understanding the causal mechanism driving the productivity effect have involved using hours worked by the wife as a proxy for specialisation 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 specialisation, Hersch and Stratton (2000) use the actual time spent on housework by men, but they find no significant 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 favouritism. 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).

All of the studies cited above have examined the relationship between marital status and earnings

[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 UK – while Blackaby et al (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 et al 2007).

among men in developed countries. In this paper we investigate evidence of a marital earnings premium among African men in a country where bridewealth traditionally is practiced, and where we may therefore expect selection to be a more important part of the explanation for the marital 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 and was substantial enough to require that men left their homesteads to engage in "long periods of wage labour" (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 key to explaining why marriage rates are lower, and mean age at marriage higher, among Africans than among other population groups in the country (Budlender et al 2004, Hunter 2004, Makiwane 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 African respondents aged 60 years or younger in the sample, three quarters reported *ilobolo* payments with marriage. Payment typically involved a combination of cash, cattle and livestock: about 68 percent reported that the *ilobolo* payment included cash; 75 percent reported payments of cattle and a further 13 percent, of livestock.[2] The average value of *ilobolo* reported for Africans married from 1985 to 1998 was approximately R20 000 in 2000 prices[3] (or almost thirteen times the average monthly real earnings of African men in the 1998 sample).

The practice and value of bridewealth suggests that selection may account for a larger portion of the marital earnings premium in South Africa than has been found in studies for the United States for example. First, we would predict that men with unobservable qualities that are valued in the labour market will be more able to afford *ilobolo* and get married. Second, while little evidence of the endogeneity of marriage 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 marriage, then faster wage growth may itself be a determinant of marital status.

We would also anticipate significant differences for African 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 specialisation (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 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 amongst 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 Africans than the other population groups. If cohabitation among African couples is a second-best strategy for those who cannot afford to get married, then we would

[2] Most respondents reported that *ilobolo* was "fully paid off" when they started living together (71 percent or 383/542 observations), a further 10 percent reported that the payment was now complete, and the remainder that part of the payment was still owed.

[3] This value is consistent with reports in the literature of *ilobolo* typically ranging from R10 000 to R25 000 (Kaarsholm 2005, Gustafsson and Worku 2006).

anticipate a far lower earnings premium, if any, for men who cohabit.

3 Analysis of the Marital Premium at the Cross-section

3.1 Data and sample

We start the study on the marital earnings premium among African men in South Africa using cross-sectional data collected in the September 2004 Labour Force Survey (LFS 2004:2). The LFS 2004:2 sampled almost 30,000 households, of which approximately 76 percent (or 21,761 households) were African. We choose this nationally representative data set both because it collects comprehensive labour market information and because, in contrast to earlier Labour Force Surveys, the question on marital status distinguishes between marriage and cohabitation.[4] 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 US 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 African men by four categories of marital status: currently married; cohabiting; previously married (divorced or widowed); and never married. In 2004, approximately 43 percent of the sample of employed African men older than 15 years was married, 18 percent reported cohabiting with their partner, and a further 4 percent was previously married. The remaining 36 percent reported never being married.

TABLE 1 HERE

Average hourly earnings are clearly highest among married men, and lowest among men who have never married or who are cohabiting with their partners.[5] However, the table also describes clear differences in the observable characteristics of these samples of employed men: on average, African men who are married are older than never married men and men who cohabit; they are significantly more likely than other men to report post-matric (beyond Grade 12) education; and they are also more likely to work in the formal sector and in a large firm (of 50 employees or more). These characteristics typically are associated with higher earnings.

Men who are married are also more likely on average to live with children, and particularly older children (aged 7 to 14 years).[6] For children younger than 7 years, there is little difference between married and cohabiting men, a finding which is consistent with current research that identifies a large proportion of African children born outside of (customary or civil) marriage (cf. Gustafsson and Worku 2006).

[4] 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 suggests that among Africans, cohabitation may be under-reported 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). However, as Budlender et al (2004: 23) report in their study, we also find here that data on marriage and living together are generally consistent, with clear and expected differences between the two groups of men.

[5] We assigned the mid-point of the earnings bracket to those whose earnings were reported in brackets – this amounted to approximately 22 percent of the sample in the September 2004 LFS.

[6] In the LFS 2004:2, as in other Labour Force Surveys in South Africa, it is not possible to establish consistently whether these children are biologically related to a particular man in the household.

3.2 Estimation

We use a standard Mincerian earnings equation and Ordinary Least Squares (OLS) estimation to test for evidence of a male marital earnings premium at the cross-section. The dependent variable is the log of hourly earnings (W)[7], 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 (X_i), and ε_i is the error term:

$$\ln(W_i) = \alpha + \gamma M_i + \beta X_i + \varepsilon_i \tag{1}$$

Table 2 reports the results from four regressions, with the simplest (I) including only three marital status indicators, the omitted category being never married. Without any other controls, regression I shows that for all three categories of marital status, hourly earnings are significantly higher than for never married men, although the premium is considerably higher for married men in particular. African men who are married are estimated to earn 69.5 percent more than men who have never married, at the cross-section.

TABLE 2 HERE

When other explanatory variables are included in the regression, the size of the marital earnings premium declines considerably but it remains robust and significant. In regression II, where we control for the age and education of respondents, the coefficient on the marriage dummy falls to 0.318. When we control for an extensive range of job characteristics, including whether the individual works in the formal sector and in a large firm, as well as 9 occupation, and 11 industry, categories, it almost halves (to 0.166), but remains significant and sizeable. In regression IV, where we add also the number of children in the household, the coefficient increases slightly to 0.176 (a premium of approximately 19 percent).[8] In contrast, the estimated coefficient for cohabitation declines to zero as soon as we control for individual and job characteristics.

In Table 3, we report the results for the earnings regressions when the sample of African employed men is sub-divided by employment type. The results show that the marital earnings premium for African men exists at the cross-section for both the self-employed and employees.

The first two regressions report the results for wage employment, with regression VI adding other information about the nature of employment which is collected only for employees, including whether or not the employment is permanent and the length of current employment. Married men are significantly more likely than other men to report being in permanent employment (80 percent compared to 63 percent of men who are not married), and they report a longer length of current employment (11 years compared to 6 years). When these more extensive controls are included in the earnings equation, therefore, we find that the estimated marital earnings premium among employees declines quite considerably from 0.18 to 0.11, although it is still strongly significant.

In regression VII, which is restricted to the self-employed, a coefficient of 0.149 on the marriage dummy is obtained, indicating that married men who are self-employed are estimated to earn about 16 percent more than otherwise (observably) identical men in self-employment. The robust earnings

[7] We imputed an hourly earnings value for approximately three percent of our sample for whom missing values were reported, about half of whom was married. The imputation hardly changes the estimate of average earnings and our results are not affected by this imputation.

[8] 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 23 percent 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) labour migration of either the husband or the wife (see for example, Posel and Casale 2006). We found a comparable marital earnings premium when married men in the sample are restricted to those with resident spouses.

differential for married men with self-employment suggests that the marital premium cannot be explained by employer discrimination in favour of married men. However, it is not possible to conclude therefore that the differential which remains in the earnings regression can be attributed to the effects of marriage itself. Rather, the estimated coefficient may be biased upwards both because of the omission of unobserved time-invariant variables that affect outcomes in both the marriage and the labour market, and because variation in marital status may not be exogenous to 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' dataset, but one which permits fixed effects analysis using panel data.

TABLE 3 HERE

4 Selection and the marital earnings premium

4.1 Data and sample

Two possible sources of panel data in South Africa are the KwaZulu-Natal Income Dynamics Study (KIDS) and the Labour Force Survey (LFS) Panel. The KIDS data, collected over three waves from 1993 to 2004 for one region of South Africa, potentially offer a rich source of information because questions have been asked about the payment of bridewealth or *ilobolo*. These questions have only been included in one wave of the panel (in 1998), however. Also, and more restrictive for our study, comprehensive labour market information is not collected consistently for the employed[9], marriage and cohabitation are not distinguished, and it is difficult to interpret the marital status information collected.[10] In the fixed effects analysis, we are also limited by a very small number of "switchers". Between 1993 and 1998, only eight percent of the sample of employed African men in the panel (representing less than 50 observations in each wave) changed marital status.[11]

An alternative dataset is the LFS panel, pre-released by Statistics South Africa in January 2007 and the first source of national panel data in the country. The LFS panel comprises six waves of data, collected at six month intervals, from September 2001 to March 2004. The advantages of these panel data for our study are that detailed labour market information is collected and the sample is considerably larger than that for KIDS. In the sample of 15,224 employed African men in the panel, 787 individuals switched from not married to married, and 294 from married to not married.

[9] For example, we cannot distinguish formal from informal sector employment. The KIDS data distinguish between "regular", "casual" and "other forms of self employment". This does not automatically translate into formal and informal sector employment – regular employment, for example, may entail working in an unregistered business or without legal and social protection. However, this information is not collected. Occupational categories are also not consistent across the self-employed and employees. The omission of these variables, found to be highly significant in the cross-section earnings estimations using the LFS 2004:2, will generate upward bias in the marriage coefficient.

[10] In both 1993 and 1998, information on marital status is collected through a question about the individual's spouse. In 1993, there were four possible responses: spouse "lives in the household", spouse deceased, no spouse and spouse "absent" (coded 99). In 1998, this response list was expanded to six options: "divorced/separated" and "multiple wives" were included as possible responses. Furthermore, "absent" was replaced by "else" (also coded 99), and "no spouse" was replaced with "not yet married". It's not clear what "absent" means in 1993, particularly in the context of labour migration, where spouses may not live in the household. It seems likely that "absent" conflates divorced/separated and non-resident spouses, and it is also possible that the option "no spouse" includes divorced and separated spouses. The spouse code 99 therefore is unlikely to convey the same marital status information in 1993 and 1998.

[11] Furthermore, all switchers were from not married to married, and less than half of these were clearly first marriages.

However, there are a number of limitations in the length and scope of the panel, and in the nature of information collected, which restrict our analysis. First, the three-year period of the panel is a relatively short span of time over which to examine changes in marital status. Furthermore, because the survey is designed as a rotating panel, with a twenty percent rotation of the sample in each wave, many individuals appear in less than six waves. Second, 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.[12] We therefore will not be identifying any change in marital status which coincides also with a change in the dwelling place. Third, the unit of analysis is the individual. No attempt has been made to link individuals to household members who have remained co-resident over time, and consequently, there are no household-level variables that can be used in our study. Fourth, 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 LFS datasets, as unique identifiers have been replaced. Fifth, 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.

TABLE 4 HERE

In Table 4, we compare the sample of African employed men in the rotating panel with the original 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 other men to move from a dwelling, they are over-represented in the panel, and this obviously becomes more pronounced over the course of the panel.

The conflation of marriage and cohabitation into a single category in the LFS questionnaires clearly creates difficulties for our study. In section 3 (Tables 2 and 3), we presented earnings regressions where the estimated coefficient for marriage was consistently and significantly different to that for cohabitation. Whereas the premium for marriage was positive and robust in all specifications, we found no evidence of a premium to cohabitation. Rather, among employees specifically, the estimated coefficient was negative and weakly significant. The implication is that any earnings differential identified for the combined category of married/living together will underestimate the "true" premium to marriage.

4.2 Estimation

We run two models to estimate the marital earnings premium using the LFS panel dataset. 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:

$$\ln (W_{it}) = \alpha + \gamma M_{it} + \beta X_{it} + \delta_i + \nu_{it} \qquad (2)$$

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

[12] Statistics South Africa (2006) acknowledges also that because their matching procedures (both manual and computerised) found many "mismatches" or inconsistencies, data were edited across the waves.

The pooled estimation ignores the possibility that δ_i may be positively correlated with marriage if unobserved attributes valued in the labour 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:

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

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 5. The results for the pooled regression mirror those detailed in Table 2 for the LFS 2004:2, although the earnings differential of 15.6 percent (a coefficient of 0.145) for the combined category of married/living together is smaller than that found for marriage alone in 2004. In the fixed effects model, the differential remains significant at the ten percent level, but it falls by almost two-thirds of its value to 0.048, or 4.9 percent.[13],[14]

TABLE 5 HERE

The large fall in the premium to marriage when we control for individual fixed effects is what we would have expected given the practice of bridewealth in South Africa. Selection matters, and without controlling for unobserved hetereogeneity in our samples of employed men, the marital earnings premium will have a significant upwards bias. However, the fixed effects estimation does not eliminate another source of potential bias deriving from the endogeneity of marriage. 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 upwards.

We investigate endogeneity in marital status using a probit regression to test whether the nature of earnings growth (ΔW_i) over one 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)

$\Delta W_i = (W_{it+2} - W_{it})/W_{it}$

X_i are individual characteristics (age and years of schooling) in initial wave t.

The sample in the estimation therefore is all African men who were present and employed for more than one 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 284 individual men. Nonetheless, as Table 6 illustrates, we find that the estimated coefficient on earnings growth is positive and significant at the five percent 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.

TABLE 6 HERE

[13] 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 $(\chi^2 = 2364.30)$, 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.

[14] Sample sizes in the LFS panel are also 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 effect 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.

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. One possible instrument which we explored was sex ratios (by district council and by magisterial district), derived from the original LFS cross-sections. The prediction is that in areas with higher ratios of females to males, the probability of men marrying will also be higher. However, in South Africa, sex ratios may misrepresent the marriage market because of the nature of labour migration, and therefore they are not a robust instrument for marital status.

Restrictions on the urbanisation of Africans in apartheid South Africa gave rise to patterns of circular or temporary labour migration. Africans, and particularly men, would migrate to places of employment, but they would retain a base in their (mostly) rural households of origin, to which they would return each year, and which was their permanent "home". A key impetus for this labour migration historically was for men to generate income needed for *ilobolo* payments (Hunter 2004). Although restrictions on urbanisation no longer exist, evidence suggests that circular patterns of labour migration have continued in post-apartheid South Africa (Posel and Casale 2006, Posel and Casale 2003).

Individuals in household surveys (and in the national Census), however, are identified and counted at their place of residency (where they spend most of their time). This means that estimated sex ratios, which are calculated for resident individuals, may considerably under-represent the number of men available for marriage in areas from which there is high male labour migration. Furthermore, sex ratios may not be exogenous to earnings. A low ratio of females to males, for example, may proxy for the probability of the man being a labour migrant, and therefore for higher average earnings.

Although selection based on wage growth will upwardly bias the fixed effects estimate of the marital earnings premium, there remain numerous sources of downward bias which 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 respective earnings differentials. To the extent that this represents a "true" return to marriage over cohabitation, rather than selection, our fixed effects estimate on the conflated category will be biased downwards.

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, the number of years married will be smaller for this group than for the average married man. If the benefits of marriage accrue over time, rather than immediately upon marriage, then by not controlling for years married, the fixed effects coefficient for marriage will be biased downwards (Korenman and Neumark 1991).[15],[16]

[15] That the benefits to marriage may accrue over time is consistent with the larger coefficient obtained on the divorced/separated/widowed dummy in the fixed effects regression. The earnings advantage of this group of individuals is likely to reflect a longer period of marriage than for the group of individuals who become married over the three-year period of the panel.

[16] There are two further possible sources of underestimation. The first is an omitted variable problem - because there are no household level variables available in the panel dataset, we cannot control for the number of children in the household, for example. We found that the inclusion of this variable increased the size of the marital earnings premium in the cross-section using the LFS 2004:2, although only slightly from 0.166 to 0.176. The second is possible error in the measurement of marital status. If there is measurement error in the marital status variable, then the attenuation bias will be even greater in the fixed effects estimates compared to the cross-sectional estimates.

5 Concluding comments

African men who are married are estimated to earn between 15 and 19 percent more at the cross-section than other African men in South Africa, after controlling for a wide range of observable characteristics. Much of this premium reflects the positive selection of higher-earning men into marriage. The large fall from the cross-sectional to the fixed effects estimate suggests that those unmeasured characteristics which make men more productive in the labour market also make them more desirable in the marriage market. Furthermore, unlike the few studies conducted for the US, we find evidence that the probability of marriage over a period is positively related to the growth in men's earnings in the preceding period. These findings are consistent with the payment of bridewealth creating a barrier to marriage in South Africa.

We cannot say anything meaningful about the real returns to marriage that remain after eliminating these sources of selection bias. Endogeneity in changes in marital status would suggest that the small premium to marriage estimated in the fixed effects model is still upwardly biased. 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.

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Table 1. Descriptive statistics of employed African men in South Africa, 2004

	Married (n = 3838)	Cohabit (n = 1575)	Divorced/ Widowed (n = 376)	Never married (n = 3220)
Hourly earnings	15.153	8.650	12.575	8.601
	(22.637)	(14.513)	(21.182)	(12.133)
Hours worked per week	46.699	48.189	44.691	45.784
	(14.887)	(14.851)	(18.791)	(16.130)
Age	45.107	38.170	49.223	29.717
	(10.310)	(10.052)	(10.913)	(8.318)
No education	0.121	0.145	0.197	0.059
	(0.325)	(0.352)	(0.398)	(0.236)
Primary	0.338	0.323	0.407	0.250
	(0.473)	(0.468)	(0.492)	(0.433)
Incomplete secondary	0.277	0.317	0.231	0.356
	(0.447)	(0.465)	(0.422)	(0.479)
Completed secondary (matric)	0.136	0.161	0.090	0.257
	(0.342)	(0.367)	(0.287)	(0.437)
Post-matric	0.115	0.038	0.066	0.073
	(0.319)	(0.191)	(0.249)	(0.260)
Employee	0.803	0.843	0.723	0.836
	(0.398)	(0.364)	(0.448)	(0.371)
Formal sector	0.755	0.704	0.588	0.649
	(0.430)	(0.457)	(0.493)	(0.477)
Employment in a large firm	0.351	0.236	0.205	0.239
	(0.477)	(0.424)	(0.404)	(0.427)
Living in a metropolitan area	0.154	0.190	0.141	0.166
	(0.361)	(0.393)	(0.348)	(0.372)
No. children < 7 years	0.625	0.612	0.295	0.338
	(0.884)	(0.791)	(0.650)	(0.759)
No. children 7 – 14 years	0.827	0.526	0.532	0.496
	(1.074)	(0.879)	(0.938)	(0.941)

Source: LFS 2004:2

Notes: The sample is restricted to employed African men older than 15 years for whom a complete set of observations is available. All individuals who reported hours usually worked in excess of 140 hours or as zero, although employed, are dropped from the sample. A "large firm" comprises 50 employees or more. The data are not weighted. Standard deviations are in parentheses.

Table 2. Estimated earnings regressions for African men, 2004

	I	II	III	IV
Married	0.528***	0.318***	0.166***	0.176***
	(0.024)	(0.025)	(0.022)	(0.022)
Cohabiting	0.081**	0.003	0.008	0.012
	(0.028)	(0.026)	(0.022)	(0.023)
Divorced/widowed	0.150***	0.148***	0.137***	0.137***
	(0.063)	(0.056)	(0.043)	(0.043)
Age		0.105***	0.048***	0.048***
		(0.005)	(0.004)	(0.004)
$(Age^2)/100$		-1.123***	-0.471***	-0.471***
		(0.062)	(0.047)	(0.048)
Primary education		0.195***	0.093***	0.092***
		(0.032)	(0.026)	(0.026)
Incomplete secondary		0.504***	0.253***	0.250***
		(0.032)	(0.028)	(0.028)
Matric		0.958***	0.494***	0.491***
		(0.037)	(0.033)	(0.033)
Post-matric		1.787***	1.049***	1.041***
		(0.045)	(0.046)	(0.046)
Metropolitan area		0.222***	0.184***	0.181***
		(0.026)	(0.023)	(0.023)
Formal sector			0.476***	0.473***
			(0.028)	(0.028)
Large firm			0.221***	0.219***
			(0.018)	(0.018)
Employee			-0.002	-0.004
			(0.036)	(0.036)
No. children < 7 yrs				-0.009
				(0.011)
No. children 7 – 14 yrs				-0.020***
				(0.008)
R^2	0.055	0.365	0.565	0.566

Source: LFS 2004:2

Notes: The sample is restricted to African men older than 15 years. The data are not weighted. Robust standard errors are in parentheses. The omitted marital status and education categories are "never married" and "no schooling" respectively. Regressions II to IV also control for province of residence, and estimations III and IV included 9 occupation dummies and 11 industry dummies which are not reported here. *** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

Table 3. Estimated earnings regressions for African men, 2004

	Employees V	Employees VI	Self-employed VII
Married	0.180***	0.110***	0.149***
	(0.023)	(0.022)	(0.062)
Cohabiting	0.007	-0.036*	-0.007
	(0.023)	(0.022)	(0.076)
Divorced/widowed	0.115***	0.080**	0.174**
	(0.046)	(0.046)	(0.100)
Age	0.051***	0.035***	0.032***
	(0.005)	(0.004)	(0.008)
$(Age^2)/100$	-0.500***	-0.374***	-0.300***
	(0.053)	(0.051)	(0.088)
Primary education	0.103***	0.122***	0.051
	(0.026)	(0.025)	(0.074)
Incomplete secondary	0.260***	0.282***	0.231***
	(0.028)	(0.027)	(0.081)
Matric	0.537***	0.555***	0.361***
	(0.033)	(0.032)	(0.096)
Post-matric	1.089***	1.114***	0.668***
	(0.047)	(0.044)	(0.150)
Formal sector	0.432***	0.285***	0.599***
	(0.028)	(0.027)	(0.099)
Permanent employment		0.218***	
		(0.020)	
Length current employment		0.029***	
		(0.003)	
Length current employment^2		-0.001***	
		(0.0001)	
R^2	0.570	0.598	0.505

Source: LFS 2004:2

Notes: The sample is restricted to African men older than 15 years. The data are not weighted. Robust standard errors are in parentheses. The omitted marital status and education categories are "never married" and "no schooling" respectively. The estimations also include 9 province dummy variables, 9 occupation and 11 industry dummies, and the number of children in the household, which are not reported here.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

Table 4. Sample characteristics of African employed men, 2001 and 2003

	2001		2003	
	Panel	**Original**	**Panel**	**Original**
Percentage married/living together	64.99	62.30	67.71	60.90
	(47.70)	(48.47)	(46.76)	(48.80)
Percentage divorced/widowed	3.9	4.11	4.73	4.34
	(19.36)	(19.87)	(21.23)	(20.36)
Hourly earnings	9.59	8.81	10.65	9.47
	(13.16)	(12.21)	(25.87)	(20.26)
Hours worked per week	47.94	48.61	46.91	47.43
	(16.59)	(16.09)	(15.68)	(15.07)
Age	39.73	38.71	39.91	38.78
	(11.81)	(11.89)	(11.69)	(11.69)
No education	9.53	11.02	8.48	9.66
	(29.36)	(31.32)	(27.86)	(29.54)
Primary	32.89	33.40	25.83	29.77
	(46.99)	(47.17)	(43.78)	(45.73)
Incomplete secondary	32.61	30.96	38.83	32.10
	(46.88)	(46.24)	(48.74)	(46.69)
Completed secondary (matric)	16.21	16.58	17.25	19.34
	(36.86)	(37.19)	(37.79)	(39.50)
Post-matric	8.76	8.03	9.61	8.35
	(28.28)	(27.18)	(29.48)	(27.66)
Employee	81.18	83.43	82.22	84.43
	(39.09)	(37.18)	(38.24)	(36.26)
Formal sector	71.03	73.33	72.71	74.05
	(45.37)	(44.22)	(44.55)	(43.84)
Employment in a large firm	25.43	29.85	28.68	31.98
	(43.55)	(45.76)	(45.23)	(46.64)

Source: LFS 2001:2; LFS 2003:2; LFS Panel
Notes: The data are not weighted. Standard deviations are in parentheses. The sample is all African men aged 15 years and older with employment. Hourly earnings are in real terms, using 2000 as the base year and CPI deflators provided by Statistics South Africa. All individuals who reported hours usually worked in excess of 140 hours or as zero, although employed, are dropped from the sample. A "large firm" comprises 50 employees or more.

Table 5. Pooled and fixed effects earnings estimations

	Pooled	LFS panel
Married/living together	0.145***	0.048*
	(0.012)	(0.027)
Divorced/widowed	0.095***	0.067*
	(0.022)	(0.0407)
Age	0.044***	
	(0.002)	
(Age^2)/100	-0.422***	0.041
	(0.024)	(0.128)
Primary education	0.142***	
	(0.016)	
Incomplete secondary	0.323***	
	(0.017)	
Matric	0.534***	
	(0.019)	
Post-matric	0.987***	
	(0.024)	
Urban area	0.142***	
	(0.010)	
Formal sector	0.492***	0.200***
	(0.014)	(0.019)
Large firm	0.263***	0.066***
	(0.010)	(0.013)
Employee	-0.030**	0.058**
	(0.016)	(0.026)
R^2	0.593	0.065(within)

Source: LFS Panel (2001 – 2004)

Notes: The sample is restricted to employed African men older than 15 years. The data are not weighted. Standard errors are in parentheses. In both regressions, the omitted marital status variable is "never married"; in the pooled regression, the omitted education category is "no schooling". The estimations also include 9 occupation, 11 industry and 6 wave dummies, not reported here; and the pooled estimation controlled further for province of residence. *** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

Table 6. The probability of marriage and earnings growth

Earnings growth over period (t+2) - t	0.0019**
	(0.0009)
Age in initial wave t	0.0169
	(0.0155)
Years of schooling	0.0633*
	(0.0338)
$\chi^2_{(3)}$	8.45
Number of observations	284

Source: LFS Panel (2001 – 2004)
Notes: Standard errors are in parentheses. The estimation excluded four outliers who reported earnings growth in excess of 600 percent between September 2001 and September 2002. *** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.