Marriage, Partnership, Cohabitation and Sexual Orientation: What Males Gain a Wage Premium?

Alison L Booth
Australian National University and University of Essex

Jeff Frank
Royal Holloway College, University of London

January 2007

Abstract

We develop a model of partnership and marriage based upon individual abilities and match-specific returns. A theoretical result is that marriage and same-sex partnership positively select more able individuals, while the effect of unmarried heterosexual partnership is ambiguous. We conduct empirical analysis with a unique new data source on marital status, partnership and sexual orientation of academics and administrators at British universities. We find a statistically significant male marriage premium, an insignificant positive effect of heterosexual unmarried partnership, and no partnership return to male homosexuals. This suggests that selection may play a limited role in the marriage premium. We also provide results on cohabiting versus non-cohabiting partners, and on the academic versus administrative side of universities.

Marriage, Partnership, Cohabitation, and Sexual Orientation: What Males Gain a Wage Premium?

1. Introduction

There are a wide variety of personal relationships chosen by individuals. People can be unattached. They can be heterosexual, bisexual or homosexual. If they form a partnership, they decide whether to live separately or to cohabit. They can marry. They may have children. What are the wage implications of the various forms of household organisation? This is of interest in its own right, but it can also shed light on the gender pay gap. To what extent is the gap sustained by traditional family arrangements supported by inflexible workplace practices?

Empirical studies have consistently shown that married men earn higher wages than women (both married and unmarried) and unmarried men. The premium [Becker (1965)] may be a reward for traditional household organisation where the male concentrates upon market production and the female upon household production. If this is the case, addressing the gender gap may require a change in household arrangements or compensatory changes in workplace practices. Alternatively, the measured differential may not be a direct return to marriage but may instead be reflective of a selection effect where more able males become married and less able males remain single. In this case, the male marriage premium is about the relative pay of married and single men and need have no implications for the overall gender gap. Studies [see for example, US studies including Korenman and Neumark (1991), Akerlof (1998), and Lundberg and Rose (2002), and the UK study by Bardasi and Taylor (2005)] seek to control for the selection effect by using panel data and fixed effects estimation. These studies find that there remains a large married male wage differential after controlling for selection, which may make up half of the overall premium.

Other authors have noted that there are problems with using fixed effects estimators to correct for unobserved individual heterogeneity. Antonovics and Town (2004) observe that the fixed effects estimate may be biased either upwards or downwards since the decision to marry is endogenous and may follow in response to a received or anticipated pay rise. They use data on twins as another way of correcting for unobserved heterogeneity, and find that little if any of the marriage premium is a selection effect. With a model of endogenous marriage selection, Chun and Lee (2001) also find no evidence supporting the selection hypothesis. A highly limited role for selection is also supported by Ginther and Zavodny (1998)'s comparison of 'shotgun' weddings, which might be random, to 'voluntary' weddings where selection may occur. In contrast, Krashinsky (2004) uses a number of alternative methods (including test results and twins) to control for unobserved heterogeneity, and finds no remaining marriage effect.

Therefore, while it is clear that married males are paid more than single males, the nature of the premium – whether it is directly due to marriage or represents selection by ability – is less clear. Even less is known about other, non-traditional relationships. A small literature examines the returns to unmarried cohabitation for men in the US, with mixed results. Stratton (2002) finds a positive return to cohabitation, but not after controlling for fixed effects. Loh (1996), in contrast, finds that the OLS estimated positive and significant return to cohabitation actually increases in the fixed effects estimation. Mamun (2004) finds a positive and significant return to cohabitation for white and black men, but not for Hispanics, after controlling for selection. The data available have limited researchers to examining cohabiting couples and not unmarried partners in general. There is also very little information in the literature about the effects of partnership among lesbian, gay and bisexual people. Carpenter and Gates (2004) compare cohabiting and non-cohabiting gay men and find that their characteristics (such

as education and income) differ significantly. Jepsen and Jepsen (2002) find that there is positive assortative mating (in both non-economic and labour market characteristics) for heterosexual marriages and partnerships, and same-sex cohabiting partnerships.

In this paper, we use a unique new data source from British universities to provide further information on these questions. The data include information on cohabiting and non-cohabiting partnership and childcare responsibilities, as well as marriage, and on sexual orientation. We can therefore examine the returns to different forms of relationships and household organisation. Providing this full set of comparisons is therefore one major contribution of this paper. A second major contribution is to compare jobs with very different degrees of flexibility. Within the university, academic jobs have flexibility in scheduling the hours of work between the university and home, which is particularly advantageous to women raising children. Administrative jobs are more like traditional jobs in the economy. We might therefore expect to see different returns to relationships between the two sides of the university and can then judge the effects of increased job flexibility on the gender gap. University data differ from the economy as a whole in an important way. The employees and jobs (with the exception of the divide between the academic and administrative sides) are relatively homogeneous. This means that we do not examine the extent to which, for example, the gender gap occurs as women take poorer jobs than comparable men. Instead, we see how women are disadvantaged in pay in essentially the same job.

In order to guide the empirical analysis, we develop a new model of partnership and marriage based upon individual abilities and match-specific returns. Under optimal relationship choices for heterosexuals – across marriage, partnership and single status – we find that married individuals are selected, as in the existing marriage literature, to be of higher unobserved ability. But what of unmarried partnered individuals compared to single individuals? Importantly, it turns out that the intuitive result – that they are also

selected to be of higher unobserved ability – need not be the case. This theoretical result may explain some of the inconsistent results in the empirical literature in adjusting for unobserved individual heterogeneity for men in cohabiting relationships. We also apply the model to homosexual relationships and assume that, as with heterosexuals, homosexuals prefer – *ceteris paribus* – higher earning partners. Under this assumption, partnered lesbians and gay men are selected to be of higher average ability than single homosexuals. Since lesbian/gay partners have a similar selection effect as for married heterosexuals, but without the traditional household division of responsibilities between males and females, the return to lesbian/gay partnership may proxy the selection effect in the male marriage premium. This provides a new test for the role of selection in the male marriage premium.

In our empirical analysis, we find a significant return of about 13% to marriage and a positive (about 6%) but insignificant return to unmarried partnership for heterosexual males. These effects are of similar magnitude to those in the literature. For heterosexual females, there is no return to marriage or unmarried partnership. Controlling for the interaction of male with marriage and partnership, there is no independent gender gap – that is, single men do not earn more than single women (or married/partnered women). We find that there is no relation of the marriage premium to childcare responsibilities. We are also able to examine whether there are any differences to the return for cohabiting heterosexuals and for partnered heterosexuals not living together, and find that there is no apparent difference between the two groups. We do find differences between the academic and administrative sides of universities. On the academic side, there is no significant difference between the return to marriage for men and women. This is in contrast to the very large and significant difference on the administrative side.   Finally, we find no return to partnership for homosexual males and

females. Together, these results suggest that the gender gap is largely dependent upon traditional marriage and the lack of compensatory workplace arrangements.

Section 2 discusses the data used in the analysis. Our new theoretical model of marriage and partnership is presented in Section 3. The empirical analysis is in Section 4, while Section 5 presents our conclusions.

2. The Data

The UK Association of University Teachers (AUT) conducted a survey of six representative British universities, between December 2000 and February 2001. The 'Fairness at Work' survey was organised by the AUT Equal Opportunities Committee to examine the treatment of sexual orientation, gender, and ethnicity in the academic labour market. With the permission of the Vice Chancellors of the six universities, a letter was distributed to all staff inviting them to participate in the on-line survey. A total of 813 responses were received, made up of 51% women and 49% men. Academic ranks were held by 53% of the sample and administrative posts by 47%. The survey asked 38 questions covering a broad range of issues including: job characteristics such as salary; personal characteristics such as age, experience, partnership status, childcare responsibilities, gender and sexuality; and workplace characteristics such as type of university and location. These data have previously been used – to study sexual orientation effects on wages and promotions – in Frank (2004), and a fuller discussion of the data appears in that paper.

Means of the variables (or percentage of the sample having the characteristic) used in our analysis are given in Table A.1 in the Appendix. Most of these are self-explanatory with the following exceptions. Age is measured in groups (under 30; 30-39; 40-49; 50-59; over 60), where the first group (under 30) is the base. Experience is measured in years. The precise question used to elicit sexual orientation is: 'Which best

indicates your sexual orientation at present? (Please indicate one) Heterosexual, Bisexual, Lesbian, Gay, Other'. Lesbian, gay and bisexual (LGB) individuals were 14% of the sample.[1] Individuals were asked whether or not they 'currently have a partner', without further definition in the survey, so this may include cohabiting and non-cohabiting relationships. The majority of gay men are in an unmarried partnership (58%). For heterosexual men, 61% are married and a further 25% in a partnership. The majority of lesbians are also in an unmarried partnership (60%). For heterosexual women, 47% are married and a further 25% in a partnership. While there is no direct question on cohabitation, the survey does ask whether 'you and your partner currently have any shared financial commitments (e.g. rent, mortgage, childcare, etc.)' and we will use an affirmative answer to this question as representing 'cohabitation'. Cohabitation in this sense is more common among unmarried heterosexual partners than homosexual partners, but it remains well over 50% in the latter group. Individuals were also asked whether or not they 'have childcare responsibilities'. The nature of these is not defined and may be interpreted by some respondents as asking whether they have children or have some responsibilities, rather than specifically primary childcare responsibilities. Affirmative answers represent a small proportion of each group (no more than 20%), so it is reasonable to expect that individuals are referring to significant childcare responsibilities.

---

[1] It is possible that the purpose of the survey – made clear in the cover letter – induced disproportionate participation by LGB individuals. There is of course no way of confirming if this is the case, since it is extremely rare for surveys to obtain information on this issue, especially for women. Kinsey (1948) estimated 10% of men were homosexual. For discussion, see http://www.newdirection.ca/a_10per.htm. Other sources of information include the US General Social Survey, where 3.2% of men and 2.6% of women can be viewed as homosexual/bisexual, on the basis of behaviour [see Blandford (2003)]. The US Census is viewed as providing only a very limited sample of LGB individuals in same-sex cohabitation. Carpenter (2004) notes that the Census has about 0.18% same-sex households. Black *et al.* (2000) provide a comprehensive discussion of these data sources. Because of the possibility of over-sampling, the 14% figure in our data should not be taken as establishing the actual percentage of LGB staff in British universities. It is also possible that the relatively liberal environment in universities attracts more LGB people. The general issue of the representativeness of the sample is discussed below.

There is now an extensive literature examining pay in universities, as recently discussed by Ehrenberg (2003). To our knowledge, this is the first study on academic incomes that includes data on unmarried partnership, sexual orientation, and childcare responsibilities. Indeed, the survey was designed as an internet questionnaire in the knowledge that individuals are much more likely to answer personal questions on an anonymous web-site than in any written form, and certainly than in an interview.[2] There are a number of advantages to using university data to explore labour market phenomena. The data cover relatively homogeneous groups of workers in the university sector. Often, apparent results in examining gender-related issues derive from occupational segregation and similar factors, so it is useful to focus upon a single type of employer. However, universities are very different on the academic and administrative employment sides. An uncommon feature of the current data is that it covers both the academic and the administrative sides of the universities sampled. Academic jobs contain considerable flexibility that can limit wage penalties due, for example, to the need to arrange childcare during standard working hours. Administrative jobs tend to be more reflective of the private labour market as a whole. Comparisons across these jobs can provide further insights. A major advantage of university data is that we have direct ability measures, notably the class of undergraduate degree and whether or not the individual has a PhD, and can control for these variables. It is possible that estimated selection effects of marriage actually relate to measurable (to the employer) ability and education.

As noted by McFadden and Winter (2001) [see also Couper et al. (2001)], while internet surveys have advantages of 'speed, cost effectiveness, and technical opportunities such as visual presentation of questions', they also suffer from potential sample selection problems. McFadden and Winter refer to the problem of internet access

---

[2] The questionnaire can be viewed at: http://www.ecs-webdesign.co.uk/faws.htm

and the problem of constructing a sample frame. Since we are considering universities where virtually all teaching and administrative staff regularly use computers, access is not of itself a problem. However the degree of privacy of access may differ between academic and non-academic staff, and affect the ease with which different staff will use the internet. The problem of constructing a sample frame, in light of the relatively low response rate, is a more serious problem. The current survey had a response rate of about 15% of possible participants. However, since 2 of the 6 sampled universities had at best very low responses, the response rate in the remaining 4 is over 20%.[3] While poor by comparison with postal and interview surveys, this is not out of line with internet surveys. Vehovar, Batagelj, Manfreda and Zaletel (2002) observe that: 'In addition, in telephone-recruited and e-mail-solicited Web surveys the overall response rate hardly reaches 30% ... regardless of the context.' Response rates of 10 to 30% are most common.

There is reason to believe that, for this typically highly-educated and computer-literate population, the sample selection problem from using an internet survey is more theoretical than real. Frank (2004) compares the characteristics of this sample with other, postal samples of UK academics. These include a study by the Royal Economic Society [Blackaby, Booth and Frank (2005)] and a survey of Scottish academics in Ward (2001). There is some evidence that the current sample is younger and more female than in the postal surveys, as might be expected on the basis of ease of internet usage. However, where comparable estimations can be done across the samples – as with respect to the gender wage gap – the results from the current sample are remarkably consistent with the other UK academic samples. Frank (2004) reports a gender earnings gap of about 9-12%, compared to 9-11% in Blackaby et al. (2005) and 9% in Ward (2001).

---

[3] Respondents did not indicate their employing university, but did indicate regional location. With a few exceptions, the regions listed corresponded to the 4 universities. A few did not, but that may be because

3. The Model

Marriage or partnership may be correlated with higher wages for different reasons. There are a number of possible productivity effects. In a traditional household, the male may devote more effort to market production than does a single male. Some authors [for example, Akerlof (1998)] note that marriage and children can induce men to 'settle down'. The productivity effect might be more symmetric across married men and women if it arises from increasing returns in household production as the couple share responsibilities at home, or from changes in behaviour (perhaps greater investments in human capital) due to risk-pooling. The return to marriage for men may not be reflective of productivity, but may instead follow a taste for discrimination such that married males are paid more than equally productive single males or married women. Alternatively, the correlation between wages and marriage may arise as more productive individuals are selected into marriage or partnership. This selection might occur if individuals are heterogenous in an ability measure – perhaps social ability – that is useful both in market productivity (for example, 'teamwork') and in relationships. In this section, we model the implications of selection for heterosexual marriage and partnership, and for homosexual partnership, with interesting results. While heterosexual married males and homosexual partnered males are positively selected, unmarried partnered heterosexual males may be more or less able on average than single heterosexuals. This potentially explains some of the contradictory results about partnership in the literature – controlling for selection can either increase or decrease the estimated return. Our model suggests an alternative test for selection – comparing married heterosexuals and partnered homosexuals. The reason is that partnered homosexuals will have a positive selection effect (as for married heterosexuals) and possibly increasing returns to household

the respondent filled in their home rather than employment location.

organisation, but will not have a traditional male-female division of labour within the relationship and are less likely to benefit from positive discrimination. The overall return to same-sex partnership is then an over-estimate (by the increasing returns to household organisation) of selection, and the difference in return for males to heterosexual and homosexual partnership can be viewed as a measure of the traditional division of labour and any discrimination.

Suppose that the natural log of the annual salary y_{it} of individual i in time t is given as:

$$y_{it} = \mathbf{x}_{it}'\beta + \alpha_m m_{it} + \alpha_p p_{it} + \eta v_i + e_{it} \tag{1}$$

where x is a vector of exogenous observable characteristics that influence y, β is the vector of coefficients associated with x, m denotes marital status and p unmarried partnership status, and v is an individual-specific unobservable term included to account for the possibility that there may be factors unobserved by the econometrician affecting the individual's market productivity and hence salary. Thus v_i is interpreted as unobserved heterogeneity such as ability. Finally, e_{it} is the unobservable random error term assumed to be distributed independently of the v_i. The coefficients α_m and α_p represent the 'marriage' and 'partnership' premiums, deriving from either productivity or discrimination, which we wish to measure. However, if v_i is positively correlated with marriage and partnership, perhaps because more 'able' individuals have better social skills that encourage successful relationships, then even if e_{it} is $iidN(0, \sigma^2_u)$, estimating (1) by OLS leads to an over-estimate of the α values. If panel data were available, (1) could be differenced to remove any fixed effect of this sort. However, if (for example) marriage is endogenous and in particular depends upon e_{it} – perhaps because individuals

with a positive earnings shock can afford the costs of marriage or have a greater incentive to have binding contracts over the disposition of assets in the event of a relationship breakdown – then even estimating the differenced equation leads to a biased estimate of α_m. In any case, we have only cross-sectional data so we cannot difference (1) to remove any fixed effect. Instead we will seek to identify the impact of marriage on earnings through the unique data we have on sexuality. To do this, we need to clarify the relationship between ability and relationships through a theoretical model.

We consider a simple two-period model of the choice of partnership and marriage for individuals. A discussion of search in a marriage market appears in Burdett and Coles (1999). We want to extend the analysis to allow for unmarried partnership as well as marriage, and adopt a set of very simple assumptions to facilitate the analysis. Our major result on matching, that individuals have lower reservation values for partnership than for marriage, is intuitive and would continue to hold under more realistic assumptions. We consider one-sided search where there are a large number of (identically-placed) individuals outside our sample. They do not discount and are risk-neutral. In period one, the member of our sample meets one 'outsider', and accepts any offer (either marriage or partnership) from the 'outsider'. This meeting is the only marriage/partnership opportunity – in the two periods – for both the member of our sample and the 'outsider'. If the 'outsider' offers marriage, the marriage continues for the full two periods. If the 'outsider' offers partnership, the relationship lasts for one period and both individuals then become and remain single. This reflects the greater level of commitment in marriage than in partnership. During each period of single status, the 'outsider' draws utility from a distribution G(s), and knows the draw in period 1 (but not that in period 2) before deciding on whether or not to make a partnership or marriage offer. Write $P_i(r)$, $r = \{m, p\}$, as the period gain from a relationship with the member of

our sample where the relationship takes the form of either marriage or partnership. We assume that the gain depends upon observable variables $\mathbf{z}_i$ and two sorts of unobservable (to the econometrician) variables, ability v_i and match-specific gain μ_i, and that this part of the gain is the same in marriage or in partnership. The 'outsider' also prefers to form a relationship with a member of our sample if he has a higher income. We assume that income takes the form (1) but with stationary values of x and e over the two periods, and is known to the 'outsider'. The income $y_i(r)$ will vary with the relationship status by the premiums for marriage and partnership. We write the period gain to a relationship as:

$$P_i(r) = \mathbf{z}_i'\gamma + v_i + \mu_i + y_i(r) \tag{2}$$

Write the value of this without the terms $\alpha_m m_{it} + \alpha_p p_{it}$ as $\overline{P}_i$. Then marriage gives a total two period value of $2(\overline{P}_i + \alpha_m)$, partnership an expected value of $\overline{P}_i + \alpha_p + E(s)$, and being single an expected value of $s_1 + E(s)$, where s_1 is the realised value of being single in the first period. The 'outsider' will compare these values in deciding whether to make a marriage or partnership offer.

Our interest is in determining how $E(v_i)$ varies across our sample with marital and partnership status. Is there a selection effect such that a married or partnered male is of higher expected unobserved ability than a single male? Substituting from (1) into (2), it is helpful to decompose $\overline{P}_i$ into the unobservable heterogeneity v_i and the remaining factors. We have:

$$\overline{P}_i = (1+\eta)v_i + \mathbf{z}_i'\gamma + \mu_i + \mathbf{x}_i\beta + e_i \tag{3}$$

Write $Z_i = \mathbf{z}_i'\gamma + \mu_i + \mathbf{x}_i'\beta + e_i$ with H(Z) as the distribution of Z over i and F(v) as the distribution of v over i. Note that, since each of the random variables (v, μ, e) is individually independently distributed, Z and v are also independently distributed. We

12

also assume that there are bounds on Z and ν denoted by the subscripts m and M. The box in Figure 1 shows the range of possible values. The horizontal axis represents the unobserved ability of the individual, which is useful in both social (relationship) and market activity. The vertical axis represents the other components such as match-specific value, observable value in a relationship and in the market activity, and any earnings shock. Points in the northeast of the box represent individuals with higher relationship value.

In Figure 1, we have drawn two lines: a 'marriage' line such that the 'outsider' is indifferent to marriage or partnership ($\overline{P}_i = E(s) + \alpha_p - 2\alpha_m$) and a 'partnership' line where the 'outsider' is indifferent – for given s_1 – to partnership and remaining single ($\overline{P}_i = s_1 - \alpha_p$). Provided that $2(\alpha_m - \alpha_p) < E(s) - s_1$, these lines define regions where partnership and marriage are, respectively, the optimal offers, as shown in the Figure. Intuitively, this condition holds if the current return to being single s_1 is low and the marriage premium is not too high relative to the partnership premium. Under these circumstances, an 'outsider' meeting a member of our sample with high match-specific and ability values offers marriage. In an intermediate range, the 'outsider' offers partnership. An example might be a woman who wishes to parent a child but has not met a male of sufficiently high match-specific and ability values to make a two period marriage commitment desirable. If the condition does not hold – for example, the marriage premium in the economy is very high – then there is one line [$2(\overline{P}_i + \alpha_m) = s_1 + E(s)$] that divides the box into marriage and single regions.

We now ask whether the average ability of a married person is necessarily greater than the average ability of an unmarried (either partnered or single) person. This will determine if there is a positive selection effect that will bias the OLS estimate of the

marriage premium. From Figure 1 (which is drawn for a particular value of s_1), it is straightforward that – for a given value of Z and s_1 – a married person is of higher average ability than an unmarried one. However, as will be important in the discussion about partnership below, this is not sufficient to establish the result, since the *ex post* distribution of Z and s_1 conditional on being married is not the same as the *ex post* distribution of Z and s_1 conditional on being unmarried. However, what is also the case is that, for a given value of Z and s_1, the average ability of a married person is at least the expected value of v over the full range of v values, shown as A in the Figure, and that this holds strictly if not all individuals become married. For unmarried individuals, for a given value of Z and s, the expected value of v is bounded from above by the same value and again this holds strictly if not all individuals stay unmarried. Provided there is any Z and s_1 such that there are both married and unmarried individuals, it then follows that the expected value of the unobserved ability is strictly higher for married than unmarried men. The same argument can be applied to comparing the whole group of non-single heterosexuals (including both those that are married and those that have partners) to single heterosexuals – the expected ability is definitely higher for the non-single individuals.

It might be thought that the average ability of unmarried partnered heterosexuals would similarly exceed that of single heterosexuals. Perhaps surprisingly, this need not be the case as can be established by a simple example. Assume uniform probabilities; that $\eta = 1$, that Z takes on the values 30 or 0; that v takes on the values -5 and 5; that there is no marriage or partnership premium; and that s takes on the values 10 and 50. Then it can be observed that marriage occurs only when s_1=10, z=30 and v=5; that partnership occurs only when s_1=10, z=30 and v=-5; and the individual remains single

14

otherwise, with an expected value of $v = -2$. In this example, partnerships occur only when the 'outsider', who has a low return to being single this period, meets a low ability individual who has a high match-specific value. If the member of our sample had a higher ability level, the 'outsider' would offer marriage rather than partnership.

Our model gives us a framework for understanding partnership and marriage but also some clear results. *Married* individuals will be of higher average unobservable ability and therefore the OLS estimate of the marriage premium will tend to be biased upwards by this effect. But with respect to *unmarried heterosexual partnership*, they may be selected as either more or less able than *single heterosexuals*. This may explain some of the results in the literature, where controlling for unobserved heterogeneity by the use of fixed effects estimators in panel data can either raise or lower the estimated partnership premium.

Our data are cross-sectional and not panel, so we cannot correct for fixed effects in the standard way. What we can do is explore differences in estimated premiums under different circumstances and across groups. One of the most interesting comparisons is between heterosexual partnered (married or unmarried) individuals and homosexual partnered individuals. To what extent is our model applicable to homosexual partnerships? On the assumption that homosexuals also evaluate 'more as more' [as suggested by the results in Jepsen and Jepsen (2002)] they will also, *ceteris paribus*, prefer partners with high incomes and high social ability, so our model is generally applicable. Since marriage is prohibited among same-sex partners in the UK[4], there is a single decision to be made – whether or not to form a partnership – rather than two decisions, whether to form a partnership and then whether or not to get married. By our model, homosexual individuals with a high match-specific component, with a high

---

[4] Civil partnership, with equivalent legal rights to marriage, was introduced in December 2005, after our sample period.

income shock, and with high ability will be selected into partnership, just as heterosexuals with those characteristics are selected into marriage. On the other hand, there is unlikely to be a direct marriage or partnership premium of the sort that holds for heterosexuals arising from traditional male-female division of labour within the household, or a wage premium due to positive discrimination in favour of partnered homosexuals (in the way that married males may benefit from a taste for discrimination). There may be benefits from increasing returns in home production. The OLS estimation of the returns to LGB partnership should therefore be a measure of the selection effect of partnership that would hold for both same-sex and heterosexual couples, and any returns to scale in household organisation. This total return can be compared to the total premium received by the combined group of partnered and married heterosexuals, with the difference potentially representing the traditional male-female division of labour and any taste for discrimination in favour of partnered heterosexuals. Using our unique data-set, we are also able to compare cohabiting to non-cohabiting partners, the partnered/married with and without childcare responsibilities, and academics to administrators, to gain other insights into the partnership and marriage premiums.

4. Empirical Results

4. 1 The Marriage Premium for Heterosexuals

In Table 1, we report ordinary least squares (OLS) estimates of the log annual salary with respect to a number of individual and job characteristics. Column (1) reports estimates obtained from the sample of all <u>heterosexual</u> males and females, with t-statistics given in parentheses. We control for employer fixed effects by using a dummy variable for each employer, but only report a London coefficient to protect anonymity of universities (the large number of London universities making it impossible to identify the particular one in the sample). Age is reported in bands while experience in the university

sector is by year. Human capital is measured by whether or not the individual has a PhD, and by the class of undergraduate degree (where the base is less than a lower second degree, conditional on obtaining a UK undergraduate degree). We allow for the fact that medical staff are on separate, higher pay scales by including a dummy variable 'Medicine subject area'. We have information on subject area of other academics, but non-medical subject has no explanatory impact on pay (which is not surprising, given common pay scales across disciplines other than clinical medicine). The results are consistent with previous studies on academic pay in the UK, such as Blackaby and Frank (2000), in terms of the return to age and experience, to working in London and in the field of medicine, and in the negative pay effect of temporary contracts.

Given our focus on the marriage premium, the most interesting results of column 1 are that there is a significant, 13% marriage premium for men and none for women; there is an insignificant gender premium; and there are insignificant coefficients on childcare responsibilities for men and women. There is a positive but insignificant unmarried partnership premium for men, and a negative but insignificant one for women. Estimating only over the <u>male heterosexual</u> sample (284 individuals), we find a marriage effect of 0.1221 (t-statistic 1.96); over the <u>female heterosexual</u> sample (329 individuals), we find a marriage effect of 0.0166 (0.38).[5] Our results to this point are therefore largely consistent with the literature, with two exceptions. The insignificant gender premium – after controlling for gender-specific marriage and partnership effects – is unusual, and may arise from the homogeneity of workers and jobs in our sample as discussed below. The other unusual effect is the absence of significant childcare coefficients. As discussed

---

[5] To ensure part-time workers were not affecting the results, we also ran the regressions for only full-time workers, with essentially unchanged results. Although it does not affect the results reported in the text, it is interesting that – as might be expected – part-time working is significantly correlated with childcare responsibilities for women, but inversely correlated with childcare responsibilities for men.

above in the context of the data, this may be because respondents took the question to be asking about whether or not they had children, rather than primary childcare responsibilities.

Turn now to the coefficients for unmarried partnership for men and women. Unlike the data used in the existing literature (which typically only include cohabiting relationships), our 'partnership' variable may include both cohabiting and non-cohabiting relationships. Further, if we use the information on shared financial commitments as a measure of cohabitation, we can examine the effects of marriage, cohabitation, and unmarried non-cohabiting partnership. For men, these have the coefficients of 0.1282 (t-stat 2.27) for marriage; 0.0745 (1.14) for cohabitation; and 0.0229 (0.31) for unmarried non-cohabiting partnership. While this is suggestive that a higher degree of commitment (cohabitation *vs* non-cohabitation) in a relationship is reflected in a higher wage premium, tests of significance cannot distinguish between these two coefficients. None of the coefficients are significant for women.

A major advantage of using university data is that it provides a relatively homogeneous group of workers, and effects are therefore not due to occupational segregation or related factors. However, the two sides of the university – academic and administrative jobs – are very different in their characteristics, with administrative jobs being more like the general job market in the economy. For this reason, we estimate separate regressions, reported in Columns (2) and (3) of Table 1. The main result of interest to us is that the male marriage premium remains robust in the regression over the sample of administrators, despite the smaller sample, and indeed is quantitatively much larger than in the full sample at 21%. As with the full sample, the administrative sample shows no partnership premiums, no female marriage premium and no childcare effects. However, among academics, there are similar insignificant positive marriage premiums for both men and women of about 7%.

One explanation for this differing behaviour – and in particular the lack of a male marriage premium amongst academics – is that the flexibility of the academic job limits the gains to marital home production allocation, and similarly limits any penalties. An alternative explanation, also relying on the flexibility of the academic job, is that there is a selection effect. These jobs may be particularly attractive to highly-qualified married women, so the married women in our academic sample may on average have high unobservable ability. However, academic jobs are relatively low-paid given the educational levels required, and married men not requiring job flexibility may select into better-paid employment elsewhere in the economy.

A very interesting result is that – once we control for partnership status by gender – there is no remaining gender gap, either in the full sample or in the two sides of the university (academic and administrative). Single males do not receive a premium over married or unmarried females. This differs from results in the literature, and is probably due to the homogeneity of our sample. We are comparing males and females in essentially the same job, and therefore do not have the job segregation effects included in other studies. This emphasises the importance of the causes of the male marriage premium. If it is primarily due to a selection effect, then standard approaches to the gender gap (such as equal pay monitoring) are relevant. However, if the premium is reflective of traditional household arrangements and inflexible work arrangements, elimination of the gender gap requires changes in household and work structures.

4.2 Comparing the Income Gain from Heterosexual and Homosexual Partnerships

We now turn to the issue of whether the male marriage premium appears to be due to direct income effects of marriage, or whether it is a selection effect. From the theoretical model, we can look at self selection (unobserved ability) effects by comparing the income gain from heterosexual partnerships (including both married and unmarried

partners) to the income gain from homosexual partnerships. The income gain to heterosexual partnerships includes any direct marriage or unmarried partnership premium plus a selection effect. The marriage/partnership direct premium arises either from household organisation (with males perhaps spending more time and effort on market production) or some other productivity effect, or from a taste for discrimination in favour of married/partnered men. The selection effect arises either as the unobserved ability to form relationships ('social skills') is correlated with unobserved workplace productivity ('team player'), or as individuals with higher incomes are – *ceteris paribus* – more attractive as partners. In contrast to gender differences in household organisation in a traditional family, there is no reason to think that household organisation asymmetries impact upon a same-sex partnership. However, if there are returns to scale in household production (for example, it does not take twice as long to wash dishes for two as for one), there may be a positive direct productivity effect for homosexual partners who can put more time/effort into market activities. There is no particular reason to think that there is a 'taste for discrimination' operating in favour of homosexual partners. But, on the assumption that 'more is more' for homosexuals deciding on partnership, as with heterosexuals deciding on partnership, higher 'social ability' or higher incomes should have a positive effect on partnership formation. The conclusion from this is that homosexual partnerships should retain the selection effects of heterosexual partnerships, without the full productivity or positive taste for discrimination effects.

Table 2, Column 1, shows the gain to heterosexual married and unmarried partnerships taken together. There is a statistically significant 10% gain to (combined married and unmarried) partnership for male heterosexuals, with a negative and insignificant effect for female heterosexuals.

The gain to male homosexual partners should maintain the selection effect of partnership, with either a small (returns to scale) or no productivity effect. Column 2 of

Table 2 shows the OLS estimation for lesbian, gay and bisexual individuals. In constructing our sample for this estimation, we needed to make a decision about how to treat LGB individuals who report heterosexual marriage. In the full sample, 3 individuals who declare themselves to be Lesbian are married, as are 4 women who declare themselves to be bisexual; no individuals who declare themselves to be gay men are married, although 4 men who declare themselves to be bisexual are married. This information on marriage about LGB individuals need not be an error in the data, as these individuals might be married for reasons of convenience (such as immigration) or may indeed have full heterosexual marriages. In the reported estimation in Column 2, however, we drop married individuals from the sample since we are unsure as to their characteristics. Including these individuals in the sample has little effect on the results, and in particular the coefficients on partnership remain insignificant.

The main coefficients of interest in Table 2, Column 2, concern the returns to partnership for male and female LGB individuals. The coefficient for males is positive but very small and statistically insignificant, while that for females is negative and statistically insignificant. Taken at face value, the fact that there is no return to partnership for LGB males suggests that there is no selection effect to the male marriage premium. In particular, the overall measured effect should include the selection effect plus any returns to scale gains from partnership, so the absence of an overall positive effect means the selection effect is zero or negative.

However, there are two limitations on this result that we need to consider. One is the underlying assumption in our analysis that LGB individuals form partnerships in much the same way as heterosexuals, with the exception of the legal restriction on marriage. Among male LGB individuals, 58% have partners, compared to the 86% married/unmarried partnership rate for heterosexual males. For female LGB individuals, 60% have partners, compared to 82% of female heterosexuals in the sample who have

either married or unmarried partners. The marriage/partnership rate is clearly higher among heterosexuals in the sample, an effect which continues to hold for men but not for women after controlling for age and other factors. These different rates would be consistent with the idea that there is a direct income effect for heterosexual marriage/partnership which induces greater formation of relationships. The other limitation on our result concerns the imprecision of the estimate, due to the small sample of LGB individuals. We also estimated a combined regression over both the heterosexual and LGB sample, and found that the return to LGB male partnership compared to partnered (married or unmarried) heterosexual males is the negative but statistically insignificant -0.0659 (t-statistic -0.67). Since the return is imprecisely estimated, we cannot definitively conclude that there are differential returns to partnership by sexuality.

5. Conclusions

The literature tends to consider the male marriage premium and the gender gap as independent phenomena. In contrast, our results suggest that, in relatively homogeneous workplaces such as universities, the entire gender gap may be associated with the male marriage premium. To eliminate the gender gap, we therefore need to understand the causes of the male marriage premium. Is it a reward for traditional household arrangements supported by traditional workplace inflexibilities? If this is the case, then appropriate policies concern the elimination of the positive taste for discrimination in favour of married males, addressing the issue of unequal household responsibilities in a traditional marriage, and increasing workplace flexibility to assist in a more equal allocation of household responsibilities. Alternatively, if the male marriage premium reflects a selection effect (so that it is simply a comparison of the pay of less able single men to that of more able married men), then gender pay audits should be sufficient to address the overall gender gap.

Our results support the argument that the male marriage premium is due to traditional attitudes and arrangements with respect to marriage. The male marriage premium is much higher on the administrative side of the university, and indeed there is no significant marriage premium on the academic side. University administrative jobs (and the associated workplace organisation) are comparable with most jobs in the private sector. In contrast, academic jobs offer considerably more flexibility with much research activity and teaching preparation conducted at home during 'unsocial' hours.

Further, if the male marriage premium is due to a selection effect, with more able males being more likely to be married, why doesn't a comparable effect hold for homosexual partnerships? If, for example, 'social ability' increases the probability of forming a successful relationship, and also assists in 'teamwork' at one's employment, why shouldn't this hold across sexual orientation? The negative (but insignificant) return to homosexual partnership (for males or females) in our results suggests the absence of a selection effect to gay partnership and, by analogy, to heterosexual marriage. Our results provide further support to the results in Antonovics and Town (2004) and Chun and Lee (2001) that there is little or no selection effect in the marriage premium.

Table 1. Log Salary Regressions –Marriage Premium for Heterosexuals

Independent variables	[1] OLS Log Salary (All heterosexuals)	[2] OLS Log Salary (Academics)	[3] OLS Log Salary (Administrators)
Constant	9.4790	9.6736	9.4267
	(102.50)	(85.44)	(62.52)
Gender & Family			
Male	-0.0128	0.0479	-0.0849
	(0.21)	(0.67)	(0.85)
Childcare responsibilities – male	-0.0028	-0.0370	0.0158
	(0.07)	(0.87)	(0.20)
Childcare responsibilities – female	0.0154	-0.0164	0.0318
	(0.38)	(0.34)	(0.46)
Married – male	0.1320	0.0771	0.2121
	(2.39)	(1.21)	(2.19)
Married – female	-0.0051	0.0642	-0.0555
	(0.11)	(1.11)	(0.71)
Unmarried partnered – male	0.0652	0.0526	0.0584
	(1.11)	(0.76)	(0.59)
Unmarried partnered – female	-0.0866	-0.0793	-0.0732
	(1.15)	(-0.88)	(0.58)
Other Attributes			
Age 30-39	0.1362	0.1147	0.1455
	(3.56)	(2.57)	(2.26)
Age 40-49	0.2010	0.2083	0.2003
	(4.09)	(3.42)	(2.51)
Age 50-59	0.2923	0.3293	0.2322
	(5.20)	(4.56)	(2.65)
Age over 60	0.3621	0.3768	0.3527
	(3.46)	(3.24)	(1.60)
Experience	0.0219	0.0099	0.0277
	(4.18)	(1.56)	(3.11)
Experience sq	-0.0004	-0.0001	-0.0005
	(2.78)	(1.07)	(1.97)
London	0.1006	0.0084	0.2081
	(1.20)	(0.09)	(1.35)
White	-0.0591	-0.0182	-0.0849
	(0.97)	(0.25)	(0.86)
First class degree	-0.0070	-0.0082	-0.0463
	(0.21)	(-0.23)	(-0.62)
Upper second class degree	0.0324	-0.0024	0.0723
	(1.14)	(0.07)	(1.52)
PhD	0.1062	0.0740	0.1650
	(3.54)	(2.25)	(2.87)
Academic staff	0.0789		
	(2.90)		
Medicine subject area	0.1847	0.1953	0.1699
	(5.96)	(5.89)	(2.79)
Temporary post	-0.1711	-0.2383	-0.1184
	(5.71)	(6.37)	(2.42)
Full time post	0.3535	0.3347	0.3472
	(8.86)	(6.61)	(5.51)
Sample size	613	337	276
Adjusted R squared	0.54	0.58	0.49

Notes: (i) t-statistics are given in parentheses. (ii) Institution-specific fixed effects are included in the regressions. (iii) Age is in groups with baseline of under 30.

Table 2. Log Salary Regressions – Marriage/ Partnership for Heterosexuals, Partnership Premium for Lesbian/Gay/Bisexual

Independent variables	[1] OLS Log Salary (All heterosexuals)	[2] OLS Log Salary (lesbian/gay/bisexual)
Constant	9.4809	9.4812
	(102.55)	(38.60)
Gender & Family Attributes		
Male	-0.0148	0.0212
	(0.25)	(0.20)
Childcare responsibilities – male	0.0107	0.0397
	(0.28)	(0.31)
Childcare responsibilities – female	0.0173	0.0848
	(0.44)	(0.62)
Partnered – male	0.1048	-0.0114
	(2.01)	(0.13)
Partnered – female	-0.0126	-0.0507
	(0.29)	(0.53)
Other Attributes		
Age 30-39	0.1405	0.1816
	(3.70)	(1.86)
Age 40-49	0.2121	0.4007
	(4.39)	(3.26)
Age 50-59	0.3043	0.5455
	(5.52)	(2.91)
Age over 60	0.3762	0.1569
	(3.62)	(0.46)
Experience	0.0217	0.0224
	(4.15)	(1.42)
Experience sq	-0.0004	-0.0007
	(2.70)	(1.16)
London	0.1055	0.2377
	(1.25)	(0.86)
White	-0.0672	0.0031
	(1.11)	(0.03)
First class degree	-0.0056	0.0673
	(0.16)	(0.76)
Upper second class degree	0.0294	0.0316
	(1.03)	(0.45)
PhD	0.1049	0.0913
	(3.51)	(1.20)
Academic staff	0.0803	0.1851
	(2.96)	(2.56)
Medicine subject area	0.1871	0.2104
	(6.04)	(2.87)
Temporary post	-0.1733	-0.1968
	(5.79)	(-2.93)
Full time post	0.3545	0.0731
	(8.90)	(0.77)
Sample size	613	93
Adjusted R squared	0.54	0.60

Notes: (i) Estimates for partnered men and women in Column [1] are for marriage/partnered interacted with gender, relative to the base of single for the sub-sample of heterosexuals, whereas those in Column [2] are for partnership interacted with gender relative to the base of single for the sub-sample of all LGB individuals. (ii) t-statistics are in parentheses. (iii) Institution-specific fixed effects are included in the regressions. (iv) Age is in groups with baseline of under 30.

References

Akerlof, George A, 'Men Without Children', *The Economic Journal*, 1998, Vol 108, 287-309.

Antonovics, Kate and Robert Town, 2004, 'Are All the Good Men Married? Uncovering the Sources of the Marital Wage Premium', *American Economic Review, Papers and Proceedings*, Vol 94, 317 -

Badgett, M. V. Lee, *Money, Myths and Change: The Economic Lives of Lesbians and Gay Men,* 2001, University of Chicago Press.

Bardasi, Elena and Mark Taylor, 2005, 'Marriage and Wages', Working Papers of the Institute for Social and Economic Research, University of Essex, **http://www.iser.essex.ac.uk/pubs/workpaps/pdf/2005-01.pdf**

Becker, Gary S, 'A Theory of the Allocation of Time', *The Economic Journal*, 1965, Vol 75, 493-517.

Black, Dan, Gary Gates, Seth Sanders and Lowell Taylor, 'Demographics of the Gay and Lesbian Population in the United States: Evidence from Systematic Data Sources', *Demography*, 2000, Vol 37 (May), 139-54.

Black, Dan, Hoda Makar, Seth Sanders and Lowell Taylor, 2003, 'The Effects of Sexual Orientation on Earnings', *Industrial and Labor Relations Review*, Vol 56(April), 449-469.

Blackaby, David and Jeff Frank, 'Ethnic and Other Minority Representation in Economics, *The Economic Journal*, 2000, Vol 110 (June), F293-F311.

Blackaby, David, Alison L Booth and Jeff Frank, 2005, 'Outside Offers and the Gender Pay Gap: Empirical Evidence from the UK Academic Labour Market', *The Economic Journal*, Vol 115, F81-107.

Blandford, John M, 2003, 'The Nexus of Sexual Orientation and Gender in the Determination of Earnings', *Industrial and Labor Relations Review*, Vol 56, 622-42.

Burdett, Kenneth and Melvyn Coles, 1999, 'Long-term Partnership Formation: Marriage and Employment', *The Economic Journal*, Vol 109, F307-34.

Carpenter, Christopher, 2004, 'New Evidence on Gay and Lesbian Household Incomes', September 2001, *Contemporary Economic Policy*, Vol 22, 78-94.

Carpenter, Christopher and Gary Gates, 2004, 'Partnership-Based Selection Among Gay Men and Lesbians: Implications for Census Research', *Proceedings of the Social Statistics Section, American Statistical Association*

Chun, Hyunbae and Injae Lee, 2001, 'Why Do Married Men Earn More: Productivity or Marriage Selection?', *Economic Inquiry*, Vol 39, 307-19.

Cohen, Philip N, 2002, 'Cohabitation and the Declining Marriage Premium for Men', *Work and Occupations*, Vol 29, 346-63.

Couper, Mick P, Michael W Traugott and Mark J Lamias, 'Web Survey Design and Administration', *Public Opinion Quarterly*, 2001, Vol 65 (2: Summer), 230-54.

Ehrenberg, Ronald G., 'Studying Ourselves: The Academic Labor Market', *Journal of Labor Economics*, 2003, Vol 21 (2: April), 267-88.

Frank, Jeff, 'Gay Glass Ceilings', 2004. Royal Holloway University of London Discussion Paper No 2004-20. Forthcoming in *Economica*. http://web.rhul.ac.uk/Economics/Research/WorkingPapers/pdf/dpe0420.pdf

Ginther, Donna and Madeline Zavodny, 'Is the Male Marriage Premium Due to Selection? The Effect of Shotgun Weddings on the Return to Marriage', *Journal of Population Economics*, 2001, Vol 14 (2: June), 313-28

Jepsen, L. K. and C.A. Jepsen, 'An Empirical Analysis of Same-Sex and Opposite-Sex Couples', *Demography*, 2002, Vol 39 (3), 435-53.

Kinsey, A.C., W. B. Pomeroy & C. E. Martin, *Sexual Behavior in the Human Male*, 1948, Philadelphia, W.B. Saunders.

Korenman, S and David Neumark, 'Does Marriage Really Make Men More Productive?, *Journal of Human Resources*, 1991, Vol 26 (Spring), 282-307.

Khrashinsky, Harry, 'Do Marital Status and Computer Usage Really Change the Wage Structure?, *Journal of Human Resources*, 2004, Vol 39 (Summer), 774-91.

Loh, E S, 'Productivity Differences and the Marriage Wage Premium for White Males', *Journal of Human Resources*, 1996, Vol 31 (Summer), 566-89.

Lundberg, Shelly and Elaina Rose, 'The Effects of Sons and Daughters on Men's Labor Supply and Wages', *Review of Economics and Statistics*, 2002, Vol 84 (2: May), 251-68.

McFadden, Daniel and Joachim Winter, 'Experimental Analysis of Survey Response Bias Over the Internet: Some Results from the Retirement Perspectives Survey', October 2001, mimeo.

Mamun, Arif, 'Is There a Cohabitation Premium in Men's Earnings?', University of Washington Center for Research on Families Discussion Paper 2004 – 02.

Stratton, Leslie S, 'Examining the Wage Differential for Married and Cohabiting Men', *Economic Inquiry*, 2002, Vol 40 (2: April), 199-212.

Vehovar, Vasja, Zenel Batagelj, Katja Lozar Manfreda and Metka Zaletel, in 'Nonresponse in Web Surveys' in Robert Groves, Don Dillman, John Eltinge and Roderick Little, eds., Survey Nonresponse (Wiley Series in Survey Methodology), New York: John Wiley & sons, Inc., 2002.

Ward, Melanie E, 'The Gender Salary Gap in British Academia ', *Journal of Applied Economics*, 2001, Vol 33 (13: October), 1669-81.

Weiss, Yoram, 'The Formation and Dissolution of Families: Why Marry? Who Marries Whom? And What Happens upon Divorce?' Chapter 3 in MR Rosenzweig and O Stark (Eds.) *Handbook of Population and Family Economics,* 1997, Vol 1A/B, Elsevier.

Appendix Table A.1.
Means and Percentages of Variables

Independent variables	Mean – whole sample (heterosexuals)	Mean – academics (heterosexuals)	Mean – administrators (heterosexuals)	Mean – whole sample (LGB)
Salary	£25,609	£28,085	£22,879	£25,514
Age (banded)	2.48	2.51	2.44	2.34
Experience (yrs.)	10.21	10.88	9.38	9.10
Percentage of the Sample with the Characteristic:				
London	0.20	0.21	0.18	0.23
White	0.95	0.96	0.95	0.91
First class degree	0.22	0.32	0.10	0.23
Upper second class degree	0.36	0.37	0.36	0.39
PhD	0.47	0.67	0.23	0.34
Academic staff	0.54	1.00	0.00	0.46
Medicine subject area	0.22	0.27	0.15	0.23
Temporary post	0.44	0.45	0.43	0.43
Full time post	0.88	0.90	0.85	0.89
Male	0.46	0.51	0.39	0.53
Percentage of the Male/Female Sub-sample with the Characteristic:				
Childcare responsibilities – male	0.17	0.19	0.14	0.07
Childcare responsibilities – female	0.15	0.14	0.17	0.06
Married – male	0.61	0.65	0.54	0.00
Married – female	0.46	0.45	0.47	0.00
Non-married partner – male	0.25	0.23	0.28	0.58
Non-married Cohabitor - male	0.16	0.16	0.16	0.32
Non-married partner – female	0.36	0.37	0.35	0.60
Non-married Cohabitor - female	0.22	0.22	0.22	0.37
Sample size	613	337	276	93

Figure 1. Choice of Marriage and Partnership