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ABSTRACT

Is There a 'Marriage Premium' for Gay Men?

It is well-known that married men earn more than comparable single men, with typical estimates of the male marriage premium in the range of 10 to 20 percent. Some research also finds that cohabiting men earn more than men not living with a female partner. This study uses data from the General Social Survey and the National Health and Social Life Survey to examine whether a similar premium accrues to gay men who live with a male partner and whether cohabiting gay men have different observable characteristics than non-cohabiting gay men. Controlling for observable characteristics, cohabiting gay men do not earn significantly more than other gay men or more than unmarried heterosexual men. Cohabiting heterosexual men also do not earn more than non-cohabiting heterosexual men.

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Introduction

The possibility of same-sex marriage has become a much-discussed and polarizing issue in the U.S. in recent years. Beginning with Vermont in 2000, a few municipalities and states have recognized civil unions or some other form of legal partnership by homosexual couples. In 2004, Massachusetts became the first state to allow same-sex marriage. Meanwhile a number of other areas have taken measures to block the possibility of same-sex marriages, and Congress has debated amending the Constitution to ban same-sex marriages. Despite all the media attention given to same-sex marriage, little is known about the possible economic impact of such unions.¹ One potential effect is that gay men who marry might earn a “marriage premium” similar to the substantial premium—about 10 to 20 percent—observed among married heterosexual men compared with otherwise observationally similar unmarried men.

There are several reasons why marriage can lead to higher earnings for men. First, marriage may promote specialization within couples. Becker (1991) argued that men have a comparative advantage in market work while women have a comparative advantage in home production. The marriage premium among heterosexual men is often attributed to such specialization between husbands and wives (e.g., Chun and Lee, 2001; Gray, 1997). Marriage also might raise men’s earnings if it causes other behavioral changes that increase productivity, such as greater commitment to an employer. Employer discrimination in favor of married men would also cause marriage to lead to an earnings premium even absent any productivity effects. However, marriage may be positively correlated with earnings because men are positively

¹ Alm, Badgett, and Whittington (2000) estimate that legalizing gay marriages would increase federal tax revenues because most homosexual couples consist of two earners and many would be subject to the “marriage tax penalty.” In an examination of whether gay-partnership laws in Europe lowered the prevalence of sexually transmitted diseases, Dee (2007) finds negative effects on syphilis rates (and smaller, less precise effects on gonorrhea and HIV).

selected into marriage. Characteristics that make men more productive at work may also be attractive to potential spouses. The literature has reached differing conclusion on how to apportion the male marriage premium between these various causes (e.g., Antonovics and Town, 2004; Cornwell and Rupert, 1997; Dougherty, 2006; Ginther and Zavodny, 2001; Hersch and Stratton, 2000; Korenman and Neumark, 1991; Krashinsky, 2004).

Some of these theories may carry over to marriage, or more generally cohabiting partnerships, between gay men. In particular, positive selection into partnerships may occur among gay men as well as among heterosexual men. Specialization, in contrast, seems less likely to result in a substantial premium for partnered gay men because specialization implies an earnings premium for one partner but a penalty for the other. In addition, as discussed below, the degree of specialization may be smaller for homosexual couples than for heterosexual couples. Lack of legal gay marriage in most areas means that few gay men currently benefit from any discrimination in favor of married men. But it also seems unlikely that any such positive discrimination would apply to gay men even if same-sex marriage became widespread.²

Theory suggests that same-sex cohabiters specialize less than married heterosexuals but is somewhat ambiguous as to whether same-sex couples specialize less than unmarried opposite-sex cohabiters. Married heterosexual couples should be the most likely to specialize given their legal standing, ability to benefit from any comparative advantages due to gender, and high likelihood of having children. Among unmarried heterosexual couples, the choice to not legalize their relationship might signal lack of specialization; however, opposite-sex couples might

² Mueller (2007) suggests that men living in same-sex relationships may experience more negative discrimination than other gay men because they are more likely to be openly gay. Also, Frank (2006) argues that marriage serves as a signal of heterosexuality, and discrimination against gay men may explain at least part of the male marriage premium. Blandford (2003) makes a similar point, noting that estimates of the male marriage premium may be confounded by mixing unmarried heterosexual and homosexual men in the comparison group. If gay men experience an earnings penalty due to discrimination based on their sexual orientation, including these men in the unmarried comparison group inflates estimates of the male marriage premium.

specialize more than same-sex couples because they can reap any comparative advantages due to gender differences. Same-sex couples may be the least likely to specialize because they lack legal standing in many areas, cannot benefit from gender differences (Badgett, 2001; Jepsen and Jepsen, 2006), and—particularly among male couples—are not as likely to have children (Black et al., 2000). However, because same-sex couples cannot marry in most areas, unmarried partnership status does not serve as a signal for them like it does for opposite-sex cohabiting couples, so gay cohabiters might actually specialize more than heterosexual cohabiters who have chosen not to marry.

Empirical results generally indicate that specialization is greatest among married couples followed by unmarried heterosexual couples, with homosexual couples engaging in the least specialization. Badgett (2001: 157) concludes that same-sex couples “display much less specialization either within the household or between household and the labor market” than do opposite-sex couples. Kurdek (1993) reports that gay couples do specialize in certain tasks within the household but that they split household responsibilities more equally than married couples. Consistent with more specialization within marriage, Jepsen and Jepsen (2006) find that married couples are less alike in terms of labor market activity than unmarried cohabiting couples. In addition, they find that differences in labor market activity within same-sex cohabiting couples are sometimes similar to those among roommates whereas opposite-sex cohabiting couples consistently have greater differences in labor market activity than roommates. This suggests gay cohabiters specialize less than heterosexual cohabiters.³ Jepsen and Jepsen (2002) also find that members of male same-sex couples tend to be more alike than members of

³ Research focusing on females indicates that cohabiting lesbians earn more than women in opposite-sex couples (including married women) but that this earnings premium does not appear to be due to specialization (Jepsen 2007).

opposite-sex married or cohabiting couples in terms of earnings even though they are less alike in terms of race, age, and education.⁴

Previous research typically finds that there is an earnings premium among cohabiting men (e.g., Cohen, 1999; Daniel, 1992). This premium, which is relative to men not living with an opposite sex partner (a sample that would include gay men in most studies), is smaller than that among married men. Studies attribute the cohabitation premium more to selection than to specialization (Light, 2004; Stratton, 2002).⁵ If specialization does not significantly contribute to earnings among men in opposite-sex couples, it seems unlikely to boost gay cohabiters' earnings since they appear to engage in less specialization than opposite-sex couples.

There is little evidence on how earnings among cohabiting gay men compare with other men's earnings. In the only previous study that directly examines whether partnered gay men have higher earnings than otherwise comparable non-partnered gay men, Booth and Frank (2004) do not find a significant difference between the earnings of partnered and non-partnered homosexuals and bisexuals in a sample of British academics that includes both sexes. Studies using U.S. data suggest that men cohabiting with a same-sex partner have lower earnings than married men, while results for men in same-sex cohabiting couples versus men in different-sex cohabiting couples are mixed (Allegretto and Arthur, 2001; Clain and Leppel, 2001; Klawitter and Flatt, 1998). In addition, average household incomes are lower among same-sex male cohabiting couples than among married couples but higher than among opposite-sex cohabiting couples (Carpenter, 2004). Cohabiting gay men (and cohabiting heterosexual men) appear to

⁴ Similar to Jepsen and Jepsen's (2002) results for the U.S., Andersson et al. (2006) report larger average age differences as well as more differences in nativity among registered same-sex couples than among registered opposite-sex couples in Norway and Sweden. Schoen and Weinick (1993) report that cohabiting opposite-sex couples in the U.S. are more alike in educational attainment than married couples, which they interpret as evidence of less specialization.

⁵ However, results in Oppenheimer (2003) and Xie et al. (2003) suggest that selection plays a larger role in marriage than in cohabitation.

more likely to work part-time and less likely to work full-time than married men (Tebaldi and Elmslie, 2006).

There are several studies of the relative earnings of gay men that do not focus on partnership status. This research fairly consistently concludes that gay men earn substantially less than other men with similar observable attributes in the U.S. (e.g., Badgett, 1995; Berg and Lien, 2002; Black et al., 2003; Carpenter, 2007a).⁶ Researchers differ on the causes of this earnings gap, with, for example, some attributing it to sexual orientation discrimination (e.g., Badgett, 1995) and others to gays clustering in traditionally-female occupations (e.g., Blandford, 2003).⁷ Another potential explanation is differences in unobserved labor-market traits. As Black et al. (2003) discuss, gay men may devote fewer resources to accumulating human capital and making other career-related investments because these men do not expect to form a traditional household with one partner specializing in market production and the other specializing in non-market production. Because most gay men may not expect to support a partner and children, they may have less incentive to focus on human capital accumulation and market production.

However, the results in Carpenter (2005) are at odds with these previous findings. Using data from a survey conducted among California residents in 2001 that included self-reported sexual orientation, Carpenter does not find a difference in hourly earnings between gay men and heterosexual men; some results do indicate lower earnings among bisexual men. He concludes that earlier findings of lower earnings among gay men may be due to pooling men who are behaviorally gay and behaviorally bisexual. Other potential explanations for the difference in

⁶ There is a smaller literature on heterosexual-gay earnings differences in other countries, including Plug and Berkhout's (2004) study of the Netherlands and Carpenter's (2007b) study of Canada.

⁷ Arabsheibani, Marin, and Wadsworth (2005) find that cohabiting gay men are paid less than cohabiting heterosexual men with the same characteristics in the U.K. and interpret this result as due at least in part to discrimination. Another potential contributor to the gay earnings penalty is compensating differentials. Carpenter (2007b) finds that gay men in Canada are more likely to have "good" jobs than heterosexual men; gay men's jobs may pay less but have better non-pecuniary characteristics.

results include the more recent time frame of the sample Carpenter uses and its exclusively California composition, either of which might be associated with less discrimination against gays. Interestingly, Carpenter does find that gay and bisexual men earn less than heterosexual men when not controlling for marital status. Similarly, Allegretto and Arthur (2001) and Carpenter (2004) both conclude that the marriage premium, not differences in returns to observable characteristics, is the main cause of the observed gap in household income between gays and heterosexuals, although neither study includes non-cohabiting gay men.

This study uses data from the General Social Survey and the National Health and Social Life Survey to examine whether gay cohabiting men earn a premium like married men and cohabiting heterosexual men. The results indicate that, controlling for observable characteristics, cohabiting gay men do not earn significantly more than other gay men or more than unmarried heterosexual men. The article then examines whether gay men are positively selected into cohabitation based on observable characteristics; lack of such positive selection could explain the apparent lack of a return to cohabitation among gay men. The article also uses data from the 2000 Census to examine the relationship between men's earnings and their spouse or partner's education and annual hours of work in order to examine differences in specialization and assortative mating between heterosexual and gay couples.

Data

The main analysis below combines data from two sources, the General Social Survey (GSS) and the National Health and Social Life Survey (NHSLs), to examine earnings differences among partnered and non-partnered heterosexuals and homosexuals. These two surveys include information about labor market outcomes as well as information about sexual

identification or the gender of sexual partners. Despite their relatively small sample sizes, they have been widely used to examine earnings among gays and lesbians because they are among the few surveys that provide information about sexual partners (e.g., Badgett, 1995; Berg and Lien, 2002; Black et al., 2003; Blandford, 2003).⁸ The GSS was conducted annually from 1972 to 1994 (except not in 1979, 1981, or 1992) and every other year since 1994. The NHSLS was conducted in 1992. This study uses data from the GSS during 1988-2004 and from the NHSLS, or twelve survey waves of data. The NHSLS accounts for about 13% of the observations.

The GSS does not explicitly ask about sexual orientation or identity, but since 1988 the survey has included questions about the gender of sexual partners. Combined with data on household members, the questions about sexual partners allow researchers to infer respondents' sexual orientation and partnership status. The NHSLS has similar questions about the gender of sexual partners but also asks directly about sexual orientation. For this study, men are considered gay if they had exclusively male partners during the last year; men who had partners of both sexes during the last year (i.e., bisexuals) are not included in the main results. Men who report not having any sex partners in the last year are also not included in the main results.⁹ As discussed below, the paper examines the robustness of the main results to using other methods to determine men's sexual orientation.

⁸ The 1990 and 2000 Censuses have much larger samples, but because the surveys did not ask about sexual orientation or sexual partners, gays and lesbians can be identified solely on the basis of living with a same-sex partner. This makes it impossible to compare the earnings of cohabiting and non-cohabiting gays. Black et al. (2000) provide a critical review of the quality of the GSS and NHSLS (as well as the 1990 Census) data for examining gays and lesbians.

⁹ The sample does not include 11 men who are married to a woman but who appear to be gay in terms of having had exclusively male sex partners during the last year (i.e., closeted gays). Badgett (1995) reports that over 40% of her sample of gay men are married, with gay defined as having had more same-sex partners than opposite-sex partners since age 18. Individuals whose reports of the sex of their partners within the last year are inconsistent with reports of the sex of their partners since age 18 are also dropped from the sample, as are individuals whose own sex is reported once as male and once as female in the GSS.

This study classifies men as married, cohabiting with a partner, or not cohabiting. The GSS always asks about household composition but does not consistently ask about cohabiting partners. Similar to Black et al. (2000), this analysis classifies a man as cohabiting if he is unmarried, there is an unrelated, unmarried adult in the household, and the individual reports having a regular sexual partner the same sex as the cohabiter.¹⁰ The NHSLS asks directly about cohabitation and the gender of the cohabiting partner. Individuals are classified on the basis of their current marital or cohabitation status, so the married category combines men in their first and second (or higher) marriages and the cohabiting and non-married/non-cohabiting categories combine men who are divorced or widowed (and not currently remarried) with men who have never been married. The results are qualitatively similar if men who have ever been divorced, separated, or widowed are not included in the analysis; these men are included in the sample for the results shown here because the sample of gay men is even smaller if divorced or widowed men who are now behaviorally gay are excluded.

The GSS and NHSLS report individuals' annual earnings from the last year in categories. As in Badgett (1995), income is imputed as the conditional mean of each income category in the March Current Population Survey (CPS) each year. The income imputation is done separately by race (white, black, and other). The imputed earnings are deflated using the Current Price Index for urban consumers (1982-84=100). The log of real annual earnings is the dependent variable in the analysis. In order to avoid capturing any earnings effects due to part-time status, only full-time workers are included in the GSS and NHSLS samples used here as well as in the

¹⁰ This method would mistakenly classify heterosexual men with female roommates and gay men with male roommates as cohabiting and would misclassify cohabiters who do not report having a regular sex partner as non-cohabiting. Any such measurement error should bias the coefficients on the cohabitation variables toward zero. The robustness section examines the sensitivity of the results. In Carpenter's (2005) sample from the 2001 California Health Interview Survey, about one-third of gay men report living with a partner. Black et al. (2000) estimate that about 28% of gay men in the GSS (1988-1996) and NHSLS are cohabiting with a partner. In the sample here, about 30% of gay men are cohabiting with a partner.

CPS samples used to construct the imputed incomes. The sample is also restricted to men aged 18-64; the NHLS only includes respondents through age 60. Only individuals who answered questions related to all of the variables examined here are included, resulting in a final sample size of 4913 observations. Observations are weighted using the provided weights for the respondent because the surveys purposely oversampled blacks.

Table 1 reports descriptive statistics for the sample stratified by sexual orientation and marriage/cohabitation status. Married heterosexual men, who comprise almost 67% of the sample, have the highest mean annual earnings among heterosexuals and are the oldest group. They are also the most likely to live outside of the 100 largest urban areas (central city and suburbs). Cohabiting gay men have the highest mean earnings and second-highest average age. Cohabiting gay men also have strikingly higher educational attainment, on average, than the other groups. Gay men, both those cohabiting and those not, are more likely to live in urban areas than heterosexual men. Cohabiting heterosexual men are the youngest group and also have the lowest educational attainment and earnings. An important cautionary note to the descriptive statistics as well as the regression analysis below is that the number of gay men in the sample, 111 total, is small. This limits the power to distinguish significant differences between gay and straight men and, even more so, between cohabiting gay men and other men.

Methods

The analysis below uses ordinary least squares (OLS) regressions to examine the determinants of earnings. The basic regression is

$$\ln(\text{Real Annual Earnings}) = \alpha + \beta_1 * \text{Gay} + \beta_2 * \text{Other Characteristics} + \varepsilon, \quad (1)$$

where the error term, ε , is Huber-White corrected for heteroscedasticity. The variable *Gay* is an indicator variable equal to 1 for gay men and 0 for heterosexuals. Some specification control for other characteristics: age (as a quadratic), education (dummy variables for high school graduate, some college, and college graduate, with less than high school graduate as the omitted group), race (black and other, with white as the omitted group), urban status (dummy variables for living in one of the 100 largest urban areas or in the suburbs of those urban areas, with other areas as the omitted group), and Census region (Northeast, South, and Midwest, with West as the omitted group). All regressions include fixed effects for the survey year to control for the national business cycle. The year fixed effects also capture any average difference between the GSS and the NLSHS samples.

The basic regression model is first used to estimate the average difference in earnings between gay and heterosexual men for comparison to previous studies. The paper then focuses on specifications that interact the gay indicator variable with indicator variables for the various marital/cohabitation status groups in order to examine whether the effect of marriage/cohabitation differs for gay and straight men. The marital/cohabitation status reference group in most of these specifications is unmarried, non-cohabiting heterosexual men. In some specifications the data are stratified by sexual orientation, but these results should be viewed with caution since the sample of gay men is so small.

Results

As in other studies, most of the results here indicate that gay men earn less than comparable heterosexual men. As shown in the first column of Table 2, without controlling for

any other characteristics, gay men earn, on average, about 7% less than heterosexual men, although the difference is not statistically significant. Controlling for age, education, race, urban residence, and geographic region increases the gay earnings penalty to over 15% and renders it statistically significant.¹¹ The increase in the gay penalty occurs primarily because gay men tend to have more education than heterosexual men, so controlling for educational attainment reduces gay men's relative earnings. In results not shown here, adding controls for broad occupation and industry or for the number of children slightly reduces the gay earnings penalty to about 13%, and the estimated penalty remains statistically significant.¹²

Controlling for marital status causes the estimated gay earnings penalty to shrink to about 4% and become statistically insignificant, as shown in the third column of Table 2. Carpenter (2005) similarly finds in 2001 California data that the gay earnings penalty is smaller and not statistically different from zero when controlling for marital status. This result also confirms earlier studies that suggest that most of the gay earnings penalty is actually due to the marriage premium among heterosexual men (Allegretto and Arthur, 2001; Carpenter, 2004). The estimate of the marriage premium, which accrues only among heterosexuals, is about 18%, which is in line with previous estimates in cross-sectional data (e.g., Korenman and Neumark, 1991).

The results provide little evidence that cohabiting gay men earn a "marriage premium" analogous to that earned by married men when controlling for observable characteristics. As shown in the first column of Table 3, cohabiting gay men earn about 36% more, on average, than non-cohabiting heterosexual men before controlling for other characteristics. Married

¹¹ Results for indicator variables are interpreted as exponents of the estimated coefficients because the dependent variable is a natural log.

¹² As noted above in the text, the prevalence of gay men in traditionally female occupations may contribute to the gay earnings penalty. The small sample size for gays precludes more detailed analysis of this possibility. The estimated coefficient of the gay indicator variable is -0.124 (0.061) when controls for 5 of 6 broad occupational categories and 8 of 9 industries are included. Only 1 of the cohabiting gay men and 9 of the non-cohabiting gay men report having any children, which is not enough variation to accurately measure any effects of children.

heterosexual men earn about 38% more—an estimate not statistically different from that among cohabiting gay men—than non-cohabiting heterosexual men. Average raw earnings are not significantly different between non-cohabiting gay men and non-cohabiting heterosexual men. Cohabiting heterosexual men earn less on average than non-cohabiting heterosexual men. Despite the magnitude of some of the coefficients, the low adjusted R-squared indicates that marital/cohabitation status and sexual orientation explain little of the variation in earnings across men.

Controlling for observable characteristics eliminates the return to cohabitation among gay men and reduces the return to marriage among heterosexual men. As shown in column 2 of Table 3, adding controls for age, education, race, and place of residence makes the estimated cohabiting, gay coefficient become negative and insignificant and lowers the estimated married, heterosexual coefficient by about half. This suggests that both cohabiting gay men and married heterosexual men are positively selected along some observable characteristics, but there is an additional return to marriage among heterosexual men. This could be due either to selection along other, perhaps unobservable, characteristics or to specialization within married couples that boosts average earnings among married men. In addition to gay cohabiters not earning more than non-cohabiting heterosexuals when controlling for observable characteristics, the cohabiting, gay and cohabiting, heterosexual coefficients in column 2 are not statistically different from each other.

Cohabiting gay men also do not earn significantly more than otherwise similar non-cohabiting gay men. Columns 3 and 4 of Table 3 show results for a sample of gay men only. Because of the small size of the sample of gay men and the relatively little variation in observable characteristics, none of the coefficients, including the cohabiting indicator, reach

statistical significance.¹³ The positive, relatively large point estimates for the cohabiting indicator variable suggest that there may be a sizable cohabitation premium, but the standard errors do not rule out the possibility of no earnings premium or even an earnings penalty. Comparing the estimated coefficients for cohabiting and non-cohabiting gay men in results using the pooled data (columns 1 and 2) also gives ambiguous results. In column 1, the cohabiting, gay coefficient is larger (at the 10% significance level) than the non-cohabiting, gay coefficient. In column 2, however, the two coefficients are not significantly different from each other. These results thus generally fail to indicate a significant return to cohabitation among gay men, but the small sample here limits the ability to draw firm conclusions.

The results also provide no evidence in support of a return to cohabitation among heterosexual men. In both the pooled sample and the sample of heterosexual men, the cohabitation indicator variable for straight men is negative and significant when not controlling for observable characteristics (columns 1 and 5) and positive and insignificant when controls are included (columns 2 and 6). This pattern suggests negative selection along observable characteristics among cohabiting straight men compared with non-cohabiting straight men.

The estimated coefficients for the other variables in the regressions are largely as expected. Earnings exhibit the usual concavity with respect to age, and earnings generally increase monotonically with education.¹⁴ Black and “other race” (which is self-reported and includes Asians and Hispanics in the GSS/NHLS) men generally earn less than white men. Earnings tend to be higher in large urban and suburban areas and in the Northeast. In results not shown here, a Chow test of the equality of the coefficients for gay and straight men other than

¹³ However, an F-test shows that the coefficients shown in column 4 of Table 3 are jointly significantly different from 0 (F-statistic=3.97, p-value=0.00).

¹⁴ Education results for the stratified sample of gay men are not monotonically increasing, but only 3 gay men—all non-cohabiting—are not high school graduates.

the marital/cohabitation variables in columns 4 and 6 does not reject equality of the coefficients at the 5% significance level, suggesting that the two groups have similar returns to observable characteristics.

Robustness

As noted above, the GSS does not ask directly about sexual orientation. The above results classify men's sexual orientation based on whether they had partners of only one gender during the last year (and men who had no partners are not included in the sample). Robustness checks in other studies (e.g., Black et al., 2003; Carpenter, 2007a) typically indicate that results are not sensitive to how sexual orientation is determined in the GSS/NHLS data because most individuals tend to be classified the same under various methods.

The results here are reasonably robust to using other ways to classify men's sexual orientation. The first column of Table 4 shows the results if men's sexual orientation is based on whether they had partners of only one gender during the last 5 years (instead of during the last year). Men with partners of both sexes are not included in this sample. Using this stricter classification of gay men gives estimates of the gay earnings penalty similar to those in Table 2 and estimates of the returns to marriage and cohabitation similar to those in Table 3. The second column of Table 4 shows the results if men's sexual orientation is based on the gender of the majority of their sex partners since age 18. There is no evidence of a gay earnings penalty in those results. This suggests that men who currently exhibit heterosexual behavior but have more male partners than female partners over the last five years are relatively high earners, or men

who are currently gay but have had more female partners are relatively low earners. The estimated returns to marriage and cohabitation are similar to the earlier results.¹⁵

Some studies combine men who have exclusively same-sex partners with men who have partners of both sexes. Classifying as gay the 22 men who report having both male and female partners (i.e., bisexuals) during the last year has little effect on the estimates of the gay earnings penalty or the returns to marriage and cohabitation, as shown in column 3 of Table 4.

This analysis determines men's sexual orientation based on the gender of their sexual partners. But not all men are sexually active. The final two robustness checks examine the robustness of the results to including men who report having no sex partners during the last year. Column 4 of Table 4 shows results if the 20 unmarried men who have not had a sex partner in the last year and who live with an unrelated, unmarried adult male are included as cohabiting gay men.¹⁶ The estimates of the gay earnings penalty are not affected by including these men in the sample as gay men. The raw estimate of the earnings premium among cohabiting gay men (relative to non-cohabiting heterosexual men, shown in panel D of column 4) is slightly smaller than earlier estimates. This is the expected pattern if this method mistakenly classifies some non-cohabiting heterosexuals as cohabiting gays, given that non-cohabiting heterosexuals have lower raw earnings than cohabiting gays (as shown in Table 1).

Column 5 shows results if all men who report not having a sexual partner in the last year are included in the sample. These men are classified as married heterosexuals if they are married (with a female spouse reported as part of the household in the survey) and as non-cohabiting heterosexuals if they are not married. Including these men adds 70 married men (about 2% of

¹⁵ The sample sizes for columns 1-3 in Table 4 are less than the 4913 observations in the other tables because the 1988-1990 GSS surveys did not ask the gender of sexual partners during the last 5 years and the 1988 survey did not ask the number of partners of each gender since age 18.

¹⁶ The mean age of these men is 38, which is between the mean ages of cohabiting and non-cohabiting gay men.

the married sample) and 354 unmarried men (about 18% of the non-cohabiting, heterosexual sample). The results are generally similar to the earlier results. Interestingly, the largest estimate of the marriage premium in all of panel E occurs when this classification method is used. This suggests that unmarried men who are not sexually active have relatively low earnings compared with married men and with other unmarried men.

Selection

The above results suggest that much of the marriage premium among heterosexual men and the entire cohabitation premium among gay men are due to selection associated with observable characteristics. One way to further examine the role of selection based on observable characteristics is to estimate the determinants that a man is married or cohabiting and directly test whether education or other observable characteristics affect the likelihood of living with a partner or spouse.¹⁷ Therefore, separate logit regressions are estimated among gay and straight men for the likelihood of cohabiting and being married, respectively. A multinomial logit regression among straight men is also estimated in order to examine the likelihood of being married or cohabiting relative to not living with a spouse or partner (i.e., non-cohabiting heterosexual men are the baseline group in the multinomial logit model). Because the sample of gay men is small and has less variation in characteristics than the sample of straight men, the covariates in the logit model of cohabitation status among gay men are limited to age (and its square), an indicator variable for having a college degree, and an indicator variable for being nonwhite. In the logit and multinomial logit models for heterosexuals, the covariates are age (and its square), 3 of 4 indicator variables for education, and 2 of 3 variables for race.

¹⁷ Standard techniques for examining the role of selection along unobservable characteristics, such as individual fixed effects or siblings comparisons, cannot be implemented in the GSS/NHLS data.

The results are mixed. As column 1 of Table 5 shows, none of the variables is significantly associated with the likelihood of living with a partner among gay men. Carpenter and Gates (2006), in contrast, find that education is positively associated with cohabitation among gay men. Age and race do influence the likelihood of being married among straight men. As the results in column 2 indicate, older heterosexuals are more likely to be married, and blacks are less likely to be married. The multinomial logit results for marriage and cohabitation among heterosexual men also indicate that older men are more likely to be married and black men are less likely to be married (column 3).¹⁸ Interestingly, being older does not affect the likelihood of cohabiting with a woman (column 4). More educated men are also less likely to cohabit. This is consistent with the raw earnings differentials, which indicate that cohabiting heterosexuals earn less than non-cohabiters.

Specialization

Part of the literature on the male marriage premium examines the effect of wives' education and hours of work on their husbands' earnings. This research indicates that—contrary to the basic Becker (1991) specialization model—wives' education has a positive effect on men's earnings (e.g., Jepsen, 2005; Lefgren and McIntyre, 2006). As discussed by Benham (1974) and Grossbard-Shechtman (1993), this suggests that a wife's education contributes to her husband's productivity; it is also consistent with positive assortative mating. Some research also finds that men's earnings are negatively associated with their wife's hours of work, especially for men in managerial occupations (Hotchkiss and Moore, 1999; Pfeffer and Ross, 1982).¹⁹ Such

¹⁸ The results of either a Small-Hsiao or a Hausman test of the independence of irrelevant alternatives (IIA) assumption required by the multinomial logit model fail to reject the IIA assumption.

¹⁹ However, Loh (1996) find a positive association between wives' labor market experience and husbands' earnings.

findings suggest that specialization underlies at least part of the marriage premium, although they also are consistent with an income effect within households.²⁰

To further examine the role of specialization within married and cohabiting couples, this analysis turns to data from the 2000 Census 5% public use microdata sample. Although the Census data do not allow for a comparison of cohabiting and non-cohabiting gays, the data can be used to analyze the relationship between men's earnings and their spouse or partner's education and hours of work. Similar to Jepsen (2005), the sample of men consists of husbands and cohabiting men who worked full-time in 1999 (at least 35 hours per week and at least 45 weeks per year), are aged 18-64, and are not in the military. These men are matched with their female spouse or their cohabiting partner.

The dependent variable in the regressions is the natural log of men's annual earnings. The covariates of interest are the spouse/partner's education (measured linearly based on Jaeger, 1997) and hours of work in 1999. The regressions also include controls for men's age (a quartic), education (four dummy variables for high school diploma, some college, college degree, and post-college education), race and ethnicity (dummy variables for black, other race, and Hispanic), urban residence, and state of residence. Separate regressions are estimated for the three groups: married (presumably heterosexual) men, cohabiting heterosexual men, and cohabiting gay men. Observations are weighted using the person weights.

If married couples engage in more specialization than cohabiting couples, the relationship between the spouse/partner's hours of work and men's earnings should be more negative among married couples than among cohabiting couples. However, a larger income effect within married

²⁰ Such results are also consistent with negative assortative mating and with employer discrimination against men with working wives (Jacobsen and Rayack, 1996). Studies typically find that the negative effect of wives' hours of work on men's earnings lessens when controlling for selection and endogeneity using instrumental variables techniques (e.g., Blackaby, Carlin, and Murphy, 1998; Jacobsen and Rayack, 1996).

households than within cohabiting households could also lead to a more negative relationship for married men than for cohabiting men. The analysis therefore uses either spouse/partner's actual hours of work or a predicted measure of their hours; hours are predicted in a first-stage Tobit regression that is identified using the spouse/partner's age, race, and ethnicity.²¹ Using a predicted measure of hours of work for spouses/partners should control for the income effect of higher (lower) husband/partner's earnings reducing (increasing) the spouse/partner's labor supply.

The results indicate a positive relationship between a man's earnings and the education of his spouse/partner. This is similar to previous research and consistent with both a positive productivity effect and positive assortative mating. As shown in columns 1-3 of Table 6, the OLS regressions with the actual measure of spouses/partners' hours indicate that the magnitude of this positive relationship is greatest for married men and smallest for cohabiting gay men. The coefficients on the spouse/partner education variable in the specifications that use the predicted measure of spouse/partner's hours of work (columns 4-6) also indicate a positive relationship, but the relative magnitude is now greatest for cohabiting heterosexual men.

The results for the spouse/partner's hours of work variable are sensitive to whether actual or predicted hours is included in the regression. Married men's earnings are negatively related to spousal actual hours (column 1). Gay men's earnings also negatively related to their partner's actual hours, although the magnitude is much smaller than among married men. Cohabiting heterosexual men's earnings, in contrast, are positively associated with their partner's actual hours. When the measure of predicted spouse/partner hours is used, the estimated relationship

²¹ The Tobit regressions include the spouse/partner's age (a quartic) and race/ethnicity (black, other race, and Hispanic) as well as all of the variables included in the earnings regressions. Separate Tobit regressions were estimated for married women, cohabiting women, and gay men. The analysis does not use the presence and age of children (other identifying variables frequently used in the literature) because so few gay male couples have children and because children may not be exogenous with respect to men's earnings.

between men's own earnings and their spouse/partner's hours is positive for all three groups (with the magnitude increasing for cohabiting heterosexual men). This suggests that an income effect underlies the negative coefficients for married and gay men and lowers the coefficient for cohabiting heterosexual men. In other words, spouses/partners reduce their own labor supply if their husband/partner earns more. Controlling for this effect by using predicted hours yields results that are more consistent with positive assortative mating than with specialization for all three types of couples.

Conclusion

This study examined whether men who cohabit with a same-sex partner earn a return similar to married men or men who cohabit with an opposite-sex partner. The results indicated that gay men earn 15% less than heterosexual men with similar observable characteristics when not controlling for marital status. Cohabiting gay men and married straight men both earn over 30% more than non-cohabiting heterosexual men before controlling for observable characteristics. However, cohabiting gay men do not earn more than non-cohabiting straight men with similar observable characteristics, nor do cohabiting straight men. Cohabiting gay men also do not appear to earn significantly more than non-cohabiting gay men. Married heterosexual men earn a premium of about 18% relative to non-cohabiting heterosexual men when controlling for observable characteristics.

The results on balance suggest that the simple earnings premium to cohabitation among gay men (relative to non-cohabiting straight men) is due to positive selection along observable characteristics. Married heterosexual men earn a premium that appears to be partly due to positive selection along observable characteristics, as the marriage premium is about halved

when controlling for factors such as age and education. However, the persistence of the marriage premium here after controlling for observable characteristics and, in other studies, for unobservable time-invariant characteristics (e.g., Korenman and Neumark, 1991; Ginther and Zavodny, 2001) suggests that specialization also plays an important role in the marriage premium. However, results from the 2000 Census indicate a positive relationship between married men's earnings and their spouse's predicted hours of work. As noted by Loh (1996), this relationship seems inconsistent with the specialization hypothesis.

The results here do not provide much evidence of a return to cohabitation because of specialization among either straight or gay men. Cohabiting couples may have less incentive than married couples to specialize given that most such unions lack legal recognition. This lack of legal standing has many economic implications, including not being able to file joint taxes, not necessarily having legally-enforceable rights to financial support if the cohabitation ends, and not having inheritance rights if one partner dies without a will. Lack of legal standing may be of particular importance among gay couples, who do not have the option to legally marry or register a domestic partnership in most localities. Because heterosexual couples do have the option to marry, cohabitation may signal different levels of commitment and specialization among gay couples than among heterosexual couples. In addition, specialization within gay couples could boost one partner's earnings while lowering the other's earnings. This could cause the zero net effect found here. Such countervailing effects combined with possible discrimination against gay men makes it difficult to interpret the results here as distinguishing between the selection and specialization hypotheses.

An important cautionary note about these findings is that the sample of gay men in this study, as in other research using the GSS/NHLS data, is small. Research using a larger sample

might find that the difference in earnings between cohabiting and non-cohabiting gay men is indeed statistically significant. Future research with data that include how long a couple has been cohabiting and that have information about the division of household responsibilities and labor force participation among both cohabiters would shed additional light on specialization within such couples. In addition, panel data on a large enough sample to include an adequate number of gay men moving in and out of cohabiting relationships might yield interesting results about selection and specialization within gay couples.

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Table 1
Descriptive statistics

	Heterosexual			Gay	
	Married	Cohabiting	Non-cohabiting	Cohabiting	Non-cohabiting
Annual earnings	28,411.6 (21,537.2)	18,661.9 (13,822.1)	21,551.0 (17,591.8)	29,652.3 (30,757.0)	21,072.5 (13,090.2)
Age	41.4	33.6	36.0	40.3	36.5
Education:					
No high school diploma	8.7	16.1	8.2	0.0	3.3
High school diploma	44.9	54.9	48.8	29.1	39.7
Some college attended	13.2	12.3	14.5	3.1	12.4
College degree	33.2	16.7	28.5	67.8	44.7
Race:					
White	87.8	78.7	81.4	88.7	84.9
Black	6.9	12.7	13.2	8.4	9.7
Other	5.3	8.6	5.4	2.9	5.4
Location:					
Central city	15.1	25.2	26.4	42.9	46.0
Suburbs	27.2	27.3	26.7	29.6	20.9
Other	57.7	47.5	46.9	27.5	33.1
Region:					
Northeast	18.1	18.6	18.1	29.2	22.0
South	35.2	34.1	36.2	24.0	34.2
Midwest	26.9	22.4	25.4	17.2	15.7
West	19.8	24.9	20.3	29.6	28.1
N	3213	290	1299	33	78
N from NHSLs	506	64	242	6	13

Note: Shown are means (standard deviations) for men working full-time from the 1988-2004 GSS and the 1992 NHSLs. Annual earnings are deflated using the CPI-U (1982-84=100) and are imputed based on CPS sample means within the GSS/NHSLs brackets (see text for details). All variables except annual earnings and age are shown as percentages. Observations are weighted using the respondent weights (except for the sample sizes).

Table 2
Estimated relationship between annual earnings and sexual orientation

	(1)	(2)	(3)
Gay	-.069 (.066)	-.142* (.061)	-.037 (.063)
Age		.105** (.008)	.095** (.008)
Age squared (*100)		-.102** (.010)	-.093** (.009)
High school diploma		.199** (.039)	.196** (.039)
Some college attended		.325** (.047)	.321** (.046)
College degree		.595** (.041)	.585** (.041)
Black		-.201** (.034)	-.181** (.034)
Other race		-.161** (.052)	-.163** (.052)
Central city		.076* (.030)	.099** (.029)
Suburbs		.202** (.023)	.209** (.023)
Northeast		.085* (.034)	.086* (.034)
South		-.028 (.030)	-.031 (.029)
Midwest		.049 (.031)	.046 (.030)
Married			.168** (.023)
Adjusted R-squared	.011	.238	.247

* $p < .05$; ** $p < .01$

Note: The dependent variable is the natural log of real annual earnings, imputed based on CPS sample means within the GSS/NHLS brackets (see text for details). All regressions include fixed effects for survey year. Standard errors (in parentheses) are Huber-White corrected. Observations are weighted using the respondent weights. The number of observations is 4913.

Table 3
Estimated relationship between annual earnings, marital and cohabitation status, and sexual orientation

	All		Gays		Heterosexuals	
	(1)	(2)	(3)	(4)	(5)	(6)
Cohabiting, gay	.310** (.120)	-.019 (.096)	.242 (.154)	.087 (.152)		
Non-cohabiting, gay	.063 (.080)	-.044 (.079)				
Married, heterosexual	.325** (.029)	.169** (.025)			.325** (.029)	.168** (.025)
Cohabiting, heterosexual	-.118* (.054)	.003 (.050)			-.117* (.054)	.005 (.049)
Age		.095** (.008)		.049 (.044)		.096** (.008)
Age squared (*100)		-.093** (.009)		-.041 (.055)		-.094** (.010)
High school diploma		.196** (.039)		-.118 (.253)		.196** (.039)
Some college attended		.321** (.046)		.174 (.284)		.320** (.046)
College degree		.585** (.041)		.284 (.224)		.588** (.041)
Black		-.181** (.034)		.040 (.160)		-.186** (.035)
Other race		-.163** (.052)		.129 (.211)		-.165** (.053)
Central city		.099** (.029)		.111 (.142)		.097** (.030)
Suburbs		.209** (.023)		.203 (.172)		.211** (.024)
Northeast		.086 (.034)		-.209 (.269)		.093** (.035)
South		-.031 (.029)		-.278 (.193)		-.026 (.030)
Midwest		.046 (.031)		-.019 (.200)		.049 (.031)
Adjusted R-squared	.055	.247	.084	.205	.055	.248
N	4913	4913	111	111	4802	4802

* $p < .05$; ** $p < .01$

Note: The first row in the column headings indicates the sample. The omitted group in columns 1, 2, 5 and 6 is non-cohabiting heterosexual men; in columns 3 and 4 it is non-cohabiting gay men. The dependent variable is the natural log of real annual earnings, imputed based on CPS sample means within the GSS/NHSLS brackets (see text for details). All regressions include fixed effects for survey year. Standard errors (in parentheses) are Huber-White corrected. Observations are weighted using the respondent weights. The number of observations is 2819.

Table 4
Robustness of results to other definitions of sexual orientation

	Single-sex partners in last 5 years (1)	Sex of majority of partners since age 18 (2)	Including bisexual men as gay (3)	Including unpartnered men with male roommate as gay (4)	Including unpartnered men as heterosexual (5)
<u>A. Gay indicator variable, no controls for other characteristics</u>					
Gay	-.069 (.076)	.003 (.072)	-.108 (.067)	-.078 (.062)	-.055 (.066)
<u>B. Gay indicator variable, controls for observable characteristics</u>					
Gay	-.167* (.071)	-.050 (.065)	-.161** (.062)	-.139* (.058)	-.122* (.061)
<u>C. Gay indicator variable, controls for observable characteristics including marital status</u>					
Gay	-.060 (.073)	.043 (.064)	-.057 (.063)	-.034 (.059)	-.002 (.062)
<u>D. Married and cohabiting indicator variables, no controls for other characteristics</u>					
Cohabiting, gay	.303* (.128)	.373** (.124)	.320** (.118)	.228* (.102)	.317** (.116)
Non-cohabiting, gay	.086 (.097)	.112 (.083)	.022 (.083)	.063 (.080)	.071 (.079)
Married, heterosexual	.358** (.032)	.342** (.031)	.325** (.029)	.325** (.029)	.333** (.026)
Cohabiting, heterosexual	-.067 (.056)	-.132* (.059)	-.118* (.054)	-.118* (.054)	-.108* (.053)

(continued on next page)

Table 4, continued

	Single-sex partners in last 5 years (1)	Sex of majority of partners since age 18 (2)	Including bisexual men as gay (3)	Including unpartnered men with male roommate as gay (4)	Including unpartnered men as heterosexual (5)
E. Married and cohabiting indicator variables, controls for other characteristics					
Cohabiting, gay	-.027 (.102)	.049 (.100)	.004 (.096)	-.019 (.086)	.018 (.093)
Non-cohabiting, gay	-.064 (.097)	.035 (.076)	-.077 (.077)	-.044 (.079)	-.006 (.078)
Married, heterosexual	.174** (.028)	.182** (.027)	.170** (.025)	.169** (.025)	.209** (.022)
Cohabiting, heterosexual	.051 (.051)	-.004 (.054)	.003 (.049)	.003 (.049)	.038 (.048)
N	4048	4220	4935	4933	5337

* $p < .05$; ** $p < .01$

Note: The comparison group in panels A-C is heterosexual men, and non-cohabiting heterosexual men in panels D and E. All regressions include fixed effects for survey year. Regressions in panels B and E include the same controls for observable characteristics as Table 2, column 2. Regressions in panel C add a married indicator variable for heterosexuals. Standard errors (in parentheses) are Huber-White corrected. Observations are weighted using the respondent weights.

Table 5 Estimated relationship between observable characteristics and marital/cohabitation status

	Gay	Heterosexual	Heterosexual	
	Cohabiting	Married	Married	Cohabiting
	(1)	(2)	(3)	(4)
Age	.307 (.176)	.251** (.023)	.252** (.024)	.023 (.049)
Age squared	-.003 (.002)	-.002** (.000)	-.002** (.000)	-.001 (.001)
High school diploma		.037 (.121)	-.108 (.135)	-.572 (.201)
Some college attended		.119 (.144)	-.085 (.157)	-.888** (.285)
College degree	.799 (.459)	.207 (.144)	-.040 (.140)	-1.209** (.238)
Nonwhite	-.266 (.627)			
Black		-.733** (.110)	-.751** (.114)	-.087 (.211)
Other race		-.076 (.144)	.014 (.161)	.390 (.251)
Log likelihood	-64.646	-2761.145	-3486.182	
N	111	4802	4802	

* $p < .05$; ** $p < .01$

Note: The first row in the column headings indicates the sample: gay or heterosexual men. Columns 1 and 2 show estimated coefficients from logit regressions in which the dependent variable equals one for the cohabitation/marital status indicated in the second row of the column headings. Columns 3 and 4 show results from a multinomial logit regression with non-cohabiting, unmarried heterosexual men as the reference group. Standard errors (in parentheses) are Huber-White corrected. Observations are weighted using the respondent weights.

Table 6 Estimated relationship between annual earnings and spouse/partner's education and hours of work

	OLS with actual hours			OLS with predicted hours		
	Heterosexual Married (1)	Heterosexual Cohabiting (2)	Gay Cohabiting (3)	Heterosexual Married (4)	Heterosexual Cohabiting (5)	Gay Cohabiting (6)
Spouse/partner's years of education	.025** (.000)	.020** (.001)	.017** (.002)	.011** (.000)	.016** (.001)	.012** (.003)
Spouse/partner's annual hours of work- (in thousands)	-.082** (.006)	.024** (.002)	-.015** (.006)	.076** (.004)	.056** (.012)	.078** (.025)
Adjusted R-squared	.283	.252	.272	.271	.251	.272
N	1,746,350	140,742	17,722	1,746,350	140,742	17,722

* p < .05; ** p < .01

Note: The dependent variable is the log of men's annual earnings. Data are from the 2000 Census and only include men who worked full time. Regressions also control for men's age, education, race/ethnicity, urban residence, and state of residence. Columns 4-6 use a measure of predicted hours from a Tobit regression that includes the spouse/partner's age and race/ethnicity as well as the variables included in the earnings regression. Standard errors (in parentheses) are Huber-White corrected. Observations are weighted using the person weights.