SELF-REPORTED SEXUAL ORIENTATION AND EARNINGS: EVIDENCE FROM CALIFORNIA

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Researchers using the 1988–96 General Social Survey (GSS) have found that behaviorally gay/bisexual men earn 15–30% less, and behaviorally lesbian/bisexual women earn 20–30% more, than similar heterosexuals. This study uses confidential data on self-reported sexual orientation for 50,000 adults in California in 2001, providing more than five times as many respondents who identify themselves as sexual minorities as does the GSS. Previous approaches are extended by using more complete data on earnings, work effort, and job characteristics. Apart from the well-documented marriage premium, the author finds no statistically or economically significant independent effect of a gay or lesbian sexual orientation on earnings. There is some evidence that bisexual men and women earn less than heterosexuals. Analysis of more recent GSS data (including data from 1998-2000) suggests the findings of previous studies are somewhat sensitive to the time period considered.

A class of recent papers using the 1988–96 General Social Survey (GSS) to examine the effect of sexual orientation on earnings has generally found that behaviorally gay/bisexual men earn 15–30% less than their heterosexual counterparts, while behaviorally lesbian/bisexual women earn 20–30% more than heterosexual women. Arguing that this pattern of results by sex is inconsistent with simple sexual-orientation-based discrimination theories, researchers have sought explanations elsewhere. Some, for example, have invoked the hypothesis that sexual minority individuals are paid differently from heterosexuals because they do not conform to traditional gender roles: the labor market values gay men's characteristics less, on average, and lesbians' characteristics more, than the characteristics of straight men and women, respectively.

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The data used to generate the results are covered by a confidentiality agreement at the Data Access Center and the UCLA Center for Health Policy Research. Information on how to apply for access can be obtained by contacting the UCLA Data Access Center. For more information about the Data Access Center, see www.chis.ucla.edu/chis_dac.html.

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Others have cited Gary Becker's household specialization model, whereby young lesbians invest more in human capital formation than do their heterosexual counterparts, and young gay men invest less, because of rational, sexual orientation–based expectations about their future partners and domestic arrangements.

A shortcoming common to this entire literature, however, is the lack of a good measure of sexual orientation relevant to labor market performance. This problem arises because the GSS does not include a direct question about an individual's sexual orientation or identity. Indeed, each of the papers in this literature has explicitly acknowledged this limitation of the GSS, and the authors admit that tests of either discrimination or household specialization theories would be enhanced if researchers could identify individuals who are "openly" gay, lesbian, or bisexual. Lacking this information, researchers have imputed sexual orientation based on the sex of one's recent sex partners—a proxy for sexual orientation that suffers from a number of conceptual and empirical difficulties.

These difficulties arise because human sexuality is characterized by a complex set of relationships among attraction/desire, behavior/experience, and identity/orientation. Specifically, sexual behavior and sexual orientation are not perfectly correlated, particularly for women: meaningful proportions of "straight" men and women have had same-sex sexual experiences, while even larger proportions of gay men and lesbians have had different-sex sexual experiences. Further, relying on same-sex sexual behavior to impute sexual orientation makes it difficult to differentiate between people who would consider themselves bisexual and those who would consider themselves gay or lesbian—and these groups may have very different labor market experiences. There are also many people—gay, bisexual, and straight—who have not had sex over the relevant survey window, despite the fact that they likely have a sexual orientation. Finally, the extent to which behavior, same-sex or otherwise, is observable in ways that would affect earnings is unclear. For all of these reasons, it is difficult to interpret the earnings effects of being "behaviorally GLB" (gay, lesbian, or bisexual).

The other key limitation of the GSS is that it produces very small sample sizes of sexual minority individuals. Indeed, the original published study used a sample of 37 gay men and 47 lesbians, while more recent research has used subsequent waves of the GSS to compose samples of no more than 130 gay/bisexual men and lesbian/bisexual women, pooled over 9 years. As the GSS is a national survey, this averages out to only a handful of sexual minority individuals in each state in each survey year. If the effect of sexual orientation varies with space, time, or both (as is likely), then the average effects from pooled GSS data may be misleading.

This paper revisits the question of sexual orientation and individual earnings using confidential data on *self-reported sexual orientation* for a large sample in California. These data include well over five times as many sexual minority individuals as are available in the GSS. By using self-reported sexual orientation, I am able to avoid the problems associated with persons whose behavior does not match their orientation (and vice-versa) or who report no sexual behavior, and I can naturally separate gay men and lesbians from bisexuals by using the individual's self-report. I also make use of confidential access to the universe of earnings responses and more complete information on individual, firm, and job characteristics, including several variables that are unavailable in the GSS: firm size, industry, and job tenure. Finally, I revisit the GSS by adding the 1998 and 2000 waves of the survey to the 1988–96 data to gauge the robustness of previous estimates.

Previous Literature

The literature examining the effect of sexual orientation on individual earnings has grown substantially since Badgett's landmark study in 1995. In that paper, Badgett considered wage discrimination against persons who were behaviorally gay or lesbian in the General Social Survey (GSS) for the years 1989–91. She found that behaviorally gay men (defined a number of ways depending on the presence of a same-sex sex partner) earned between 11% and 27% less than their heterosexual male counterparts; results for lesbians were ambiguous. A family of follow-up studies using more recent waves of the GSS and alternative schemes for coding sexual orientation have confirmed the earnings penalty for behaviorally gay/bisexual men but have also found earnings premiums for behaviorally lesbian/bisexual women, on the order of 20–30% of annual income (for example, Blandford 2000; Black et al. 2003; Berg and Lien 2002).

A few studies have also considered relative earnings of gay and lesbian couples using data on same-sex unmarried partner households in the 1990 Census. Klawitter and Flatt (1998) and Allegretto and Arthur (2001) both found that men in same-sex unmarried partner couples earn less than men in different-sex couples, with the bulk of the observed gap being attributable to the "marriage premium." Carpenter (2004) extends this result by using public health data that can eliminate some potentially misclassified households based on certain sexual behaviors.

These seemingly puzzling patterns of earnings effects—a gay male earnings penalty and a lesbian earnings premium—have generated a variety of alternative explanations. Some argue that sexual minority individuals are paid differentially because they do not conform to traditional gender roles (Blandford 2000). Others argue that gay men and lesbians choose different levels of work effort given different budget constraints (Berg and Lien 2002).

Black et al. (2003) favored a Becker-style household specialization model in which sexual orientation influences individuals' human capital accumulation decisions when they are young. In a Becker framework, biological considerations (from childbearing, for example) create a comparative advantage for women in home production, while men are relatively skilled at market work. Never-married straight men—who will have wrongly anticipated partnering with an individual better skilled at home production—will have over-invested in human capital relative to their gay counterparts (who had no such presumptions). In contrast, never-married straight women will have wrongly anticipated that they would partner with a market-skilled man, and they will therefore find themselves having under-invested in human capital relative to lesbian women (who had no such presumptions). These behavioral outcomes would be consistent with a gay male earnings penalty and a lesbian earnings premium, even in the absence of discrimination.

The 2001 California Health Interview Survey

The California Health Interview Survey (CHIS) was administered in 2001 to approximately 50,000 households. Each adult respondent aged 18–64 was asked the following: "The next question is about your sexual orientation, and I want to assure you that your answers are completely confidential. Are you gay (, lesbian,) or bisexual?"[1] Fully 99% of respondents answered this question.

Direct self-reports of sexual orientation offer a measure of sexuality that—in the context of labor market analyses—is arguably preferable to the behavioral measures used previously. Specifically, researchers evaluating labor market discrimination must contend with the fact that disclosure

[1]If the respondent answered "yes" but did not further clarify his or her sexual orientation, a follow-up question was asked to differentiate among bisexuals, gay men, and lesbians. Stigma may induce some GLB individuals to state that they are not gay, lesbian, or bisexual. It is likely, however, that those who are thus discouraged from revealing their GLB orientation are similarly unlikely to reveal same-sex sexual behavior. As such, the stigma problem is likely no worse in CHIS 2001 than in the GSS. Further, the stigma associated with GLB status in California in 2001 is likely less severe than for the rest of the United States over the period 1988–96. For a detailed discussion of the biases associated with the disclosure decision in the context of labor earnings, see Badgett (1995).

Table 1. Sample Size and Incidence Rate Comparisons of Gay, Lesbian, and Bisexual Individuals.

	Men		*Women*	
Definition/Source	*Gay*	*Bisexual*	*Lesbian*	*Bisexual*
1. CHIS 2001: Self-Reported Gay, Lesbian, or Bisexual Sexual Orientation	578 (2.8%)	245 (1.3%)	335 (1.2%)	470 (2.0%)
2a. GSS 1988–1996: Same and Different Sex Partners in Last Year		33 (.6%)		29 (.5%)
2b. GSS1988–1996: Only Same Sex Partners in Last Year	139 (2.5%)		88 (1.4%)	
3a. GSS 1988–1996: Same and Different Sex Partners in Last 5 Years		72 (1.6%)		66 (1.2%)
3b. GSS 1988–1996: Only Same Sex Partners in Last 5 Years	115 (2.6%)		78 (1.5%)	
4a. NHSLS: Self-Identified Gay, Lesbian, or Bisexual	27 (1.8%)	11 (.7%)	12 (.6%)	10 (.5%)

For a detailed description of the GSS and NHSLS figures, see Black, Gates, Sanders, and Taylor (2000).

of one's gay, lesbian, or bisexual orientation in the workplace is a necessary (but not sufficient) condition for the existence of empirically and economically important labor market discrimination against sexual minorities. An accurately measured signal of sexual orientation is therefore crucial for credibly testing the discrimination hypothesis. Self-reported sexual orientation is almost surely "closer to" workplace disclosure than is same-sex sexual behavior, in large part because the latter is likely unobservable to employers.[2] A finding of Laumann et al. (1994) proves illustrative on this point: the majority of National Health and Social Life Survey (NHSLS) respondents who reported a same-sex sex partner since age 18—a socially stigmatized behavior itself—did *not* concurrently report a gay, lesbian, or bisexual sexual orientation.[3]

CHIS 2001 produces large absolute and relative sample sizes of gay men, lesbians, and bisexuals, as evidenced in Table 1. Sample size concerns may be important if there are cross-state differences in employer or employee attitudes toward sexual minority individuals, since previous studies have only had a handful of sexual minorities in each state. CHIS 2001 is also very timely. Given the many social changes in the past decade that could easily have affected workplace experiences of gays, lesbians, and bisexuals, the recent data enhance the relevance of the estimates. Finally, CHIS 2001 samples California, where studies of sexual orientation are particularly relevant. Data from the 2000 Census indicate, for example, that fully 15% of the country's same-sex unmarried partner households were in California, and the total number there (ap-

[2]Another way to think about this in the context of the empirical work is that the handful of sexual minority men and women who would admit to same-sex sexual behavior but who would not state a minority sexual orientation (that is, those whom the GSS "finds" but the CHIS "misses") actually create a sample of sexual minority individuals in the CHIS who are *more* likely to be "out" in the workplace, thus providing a better test of the workplace discrimination hypothesis.

[3]In contrast, just 4% of women and 13% of men who reported a gay, lesbian, or bisexual sexual orientation did not report a same-sex sex partner since age 18 (Laumann et al. 1994, Figure 8.2).

proximately 92,000) exceeded the number in New York, California's nearest competitor, by more than 40,000.

The CHIS data do have a few limitations. First, there may be issues of external validity associated with a California-specific sample. Because California is thought to be a relatively "enlightened" or "liberal" state, a finding of no earnings differential there does not imply that differentials do not exist elsewhere in the country. Indeed, San Francisco and Los Angeles—home, together, to 25–50% of the CHIS sexual minority sample—both have long histories of employment protection on the basis of sexual orientation. Second, CHIS does not ask respondents if they are "out" at work. While I have argued that self-reported sexual orientation is closer to workplace disclosure than is sexual behavior, information on openness in the workplace—particularly to supervisors—would help disentangle the possible role of discrimination. Finally, CHIS does not include information on sexual behavior. This information would be useful for providing a comparison to the previous studies and predicting which types of people behave in a way that appears "inconsistent" with their self-reported sexual orientation.

Empirical Approach

To estimate the effect of sexual orientation on earnings, I use straightforward log wage regressions. The key variables of interest are dummy variables indicating that the person is gay, lesbian, or bisexual.[4] The dependent variable is derived from individual responses to the question, "What is your best estimate of all your earnings LAST MONTH before taxes and other deductions from all jobs and businesses, including hourly wages, salaries, tips, and commissions?" I construct an hourly wage measure by dividing last month's earnings by self-reported working hours per month (calculated by multiplying hours worked per week by 4.5). I restrict the analysis to those earning between \$1 and \$200 per hour, though the main results are not sensitive to other reasonable topcode choices. Throughout, I consider the natural logarithm of this dependent variable.

I begin by estimating standard log wage regressions via OLS as outlined in Badgett (1995) and updated in Black et al. (2003). For comparability with the previous GSS studies, I begin by considering only non–self-employed full-time workers (at least 35 hours worked last week). I consider men and women separately. The relevant econometric model can be given by

$$\text{(1)} \qquad \text{Log (Hourly Earnings)} = \alpha + \beta_1 X + \beta_2(\textsc{Gay/Lesbian}) + \beta_3(\textsc{Bisexual}) + \beta_4(\textsc{Occup}) + \varepsilon,$$

where X is a vector of demographic variables including age and its square, education (5 categories), a dummy variable indicating whether the respondent is married, race (5 categories), and urban residence (5 categories). GAY/LESBIAN is an indicator variable equal to one if the respondent reports being gay or lesbian, and BISEXUAL is defined similarly. OCCUP is a vector of indicator variables for 15 broad occupation categories. ε is assumed to be a well-behaved error term. All models also include a dummy variable indicating missing data on sexual orientation. In some models I also include controls for job tenure and firm size; however, I exclude these measures from the main analysis because the previous studies have not had access to this information.[5]

[4]Given concerns over question wording, I restrict the entire analysis to those who are not limited in English ability. Individuals are asked "Are you gay [, lesbian,] or bisexual?" While this was intended to yield "yes" or "no" responses, some respondents could have interpreted the question to mean that they had to choose one of the responses, as in "Which of the following are you: gay (, lesbian,) or bisexual?"

[5]A danger in including variables other than the relatively exogenous characteristics of age and race is the problem of "over-controlling." As such, the interpretation of models that include these more extensive controls must recognize that choices about firms (or occupation or human capital decisions, for that matter) may in themselves be responses to discrimination (perceived or otherwise).

Table 2. Male Demographic Characteristics.
(CHIS 2001; Adults Age 18–64; Weighted Means)

Variable	*Gay Men*	*Bisexual Unmarried Men*	*Bisexual Married Men*	*Heterosexual Unmarried Men*	*Heterosexual Married Men*
N: Initial Sample	578	147	98	7,158	8,810
Of Full-Time Workers Earning between $1 and $200 per Hour:					
Earnings Last Month	4,504	3,382	4,076	3,518	5,207
Hours Last Week	45.5	47.9	45.1	45.5	47.2
Less Than HS Diploma	0.02	0.04	0.08	0.08	0.06
HS Diploma	0.11	0.27	0.37	0.30	0.24
Some College	0.32	0.34	0.14	0.30	0.27
College Degree (BA)	0.38	0.23	0.22	0.23	0.26
Some Graduate Work	0.10	0.09	0.16	0.07	0.13
Graduate Degree	0.07	0.03	0.03	0.02	0.05
Age	37.11	37.36	41.62	32.78	41.15
White	0.75	0.42	0.48	0.57	0.62
Black	0.04	0.13	0.07	0.07	0.04
Hispanic	0.10	0.25	0.30	0.22	0.19
Asian	0.06	0.12	0.15	0.10	0.11
Married	0.01	—	1	—	1
Living with Partner	0.34	0.14	—	.19	—
Fraction with Kids	0.06	0.12	0.64	0.29	0.64
San Francisco	0.15	0.06	0.00	0.03	0.02
Los Angeles	0.39	0.40	0.32	0.26	0.23
Urban Residence	0.66	0.61	0.48	0.46	0.33
Rural Residence	0.02	0.06	0.07	0.06	0.09

Results

Tables 2 and 3 present descriptive statistics on the CHIS 2001 sample separately by sexual orientation for men and women. For completeness, I report sample means separately for gay men and lesbians, unmarried bisexuals, married bisexuals, unmarried heterosexuals, and married heterosexuals. Among full-time working men, married straight men report the highest average earnings last month, $5,207, followed by gay men ($4,504), bisexual married men ($4,076), unmarried straight men ($3,518), and unmarried bisexual men ($3,382). As has been documented previously (Black et al. 2000), self-identified gay men appear to be more highly educated than either straight men or bisexual men, with well over half of the gay male sample having attained at least a college degree. Other notable differences include the fact that gay men are more likely to be white than are either straight or bisexual men, while bisexual men are disproportionately of Hispanic origin. As expected, only about 1% of gay men report that they are "married," compared to about 40% of bisexual men and 55% of straight men. About a third of gay men, however, report living with a partner. Finally, the majority of gay men report that they reside in an urban area, with over half the gay sample residing in either San Francisco or Los Angeles counties.

Table 3 reports the associated mean characteristics of the female sample and indicates that lesbian full-time workers report higher average earnings last month ($3,816) than do unmarried bisexuals ($3,247), married bisexuals ($3,329), unmarried heterosexuals ($3,070), or married heterosexuals ($3,631). Like gay men, lesbians are also disproportionately represented in the highly educated groups: 59% of lesbians

Table 3. Female Demographic Characteristics.
(CHIS 2001; Adults Age 18–64; Weighted Means)

Variable	*Lesbian Women*	*Bisexual Unmarried Women*	*Bisexual Married Women*	*Heterosexual Unmarried Women*	*Heterosexual Married Women*
N: Initial Sample	335	345	134	10,337	11,178
Of Full-Time Workers Earning between $1 and $200 per Hour:					
Earnings Last Month	3,816	3,247	3,329	3,070	3,631
Hours Last Week	45.4	44.5	43.2	43.5	43.1
Less Than HS Diploma	0.00	0.03	0.04	0.04	0.03
HS Diploma	0.19	0.20	0.28	0.26	0.23
Some College	0.22	0.22	0.23	0.34	0.31
College Degree (BA)	0.38	0.42	0.25	0.27	0.28
Some Graduate Work	0.16	0.11	0.14	0.08	0.12
Graduate Degree	0.05	0.03	0.06	0.02	0.03
Age	38.83	35.17	41.42	36.79	41.53
White	0.72	0.64	0.56	0.59	0.63
Black	0.10	0.08	0.10	0.11	0.05
Hispanic	0.15	0.15	0.05	0.18	0.26
Asian	0.00	0.07	0.24	0.09	0.13
Married	0.00	—	1	—	1
Living with Partner	0.52	0.31	—	0.17	—
Fraction with Kids	0.22	0.32	0.61	0.38	0.56
San Francisco	0.09	0.07	0.02	0.04	0.02
Los Angeles	0.23	0.23	0.26	0.30	0.23
Urban Residence	0.49	0.52	0.20	0.45	0.34
Rural Residence	0.05	0.04	0.10	0.05	0.08

have at least a college degree, though over half of unmarried bisexual women are also college-educated. As with the male sample, married women are slightly older than their unmarried counterparts. As expected, lesbians are very unlikely to be married, though over half of self-reported lesbians report that they are living with a partner. In contrast to the male sample, there is less of a pronounced urban residence pattern by sexual orientation for women.

Table 4 shows the job characteristics of full-time workers in the CHIS 2001 sample for selected occupations and industries by sexual orientation. The top panel reports results for men and shows that gay men are disproportionately represented in executive and professional ranks: 40% of the gay male sample fall into these two occupational clusters alone. Gay men and bisexual unmarried men are also more likely to be represented in administrative support positions, while they are less likely than other men to occupy positions in precision craft repair, machinery, and equipment cleaning and moving. With respect to industry, gay men are less likely than the other groups to be in the manufacturing sector, and they are over-represented in real estate.

The lower panel of Table 4 shows similar breakdowns for women. As with gay men, a disproportionate percentage (36%) of lesbians are in the executive and professional groups. Lesbians, however, are less likely than straight women to be in administrative support positions. No clear industrial patterns emerge for the female sample by sexual orientation.

Table 5 presents the baseline regressions for men and women, following estimation of equation (1) above. Columns (1) and (4) include controls for the sexual minority indicators and a dummy variable for missing data on sexual orientation, as well as age and race. Each successive column in-

Table 4. Job Characteristics.
(CHIS 2001; Adults Age 18–64; Full-Time Workers
Earning between $1 and $200 per Hour; Weighted Means)

Variable	*Gay/ Lesbian*	*Unmarried Bisexual*	*Married Bisexual*	*Unmarried Heterosexual*	*Married Heterosexual*
Men					
Selected Occupations:					
Exec./Admin./Mgmt.	0.21	0.13	0.11	0.10	0.13
Professional Specialty	0.19	0.16	0.16	0.13	0.12
Sales	0.12	0.05	0.07	0.10	0.10
Admin. Support	0.13	0.12	0.04	0.06	0.04
Precision Craft Repair, Machine Operator, Material/Equip. Mover, and Handler/Equip. Cleaner	0.05	0.18	0.34	0.28	0.25
Selected Industries:					
Construction	0.01	0.00	0.02	0.02	0.02
Manufacturing	0.02	0.09	0.16	0.17	0.17
Retail Trade	0.05	0.05	0.09	0.06	0.08
Real Estate	0.11	0.05	0.05	0.04	0.04
Women					
Selected Occupations:					
Exec./Admin./Mgmt.	0.18	0.15	0.22	0.15	0.15
Professional Specialty	0.18	0.15	0.08	0.14	0.13
Sales	0.07	0.08	0.05	0.10	0.09
Admin. Support	0.08	0.12	0.08	0.18	0.17
Precision Craft Repair, Machine Operator, Material/Equip. Mover, and Handler/Equip. Cleaner	0.05	0.06	0.05	0.04	0.04
Selected Industries:					
Construction	0.01	0.01	0.00	0.01	0.01
Manufacturing	0.03	0.07	0.11	0.05	0.05
Retail Trade	0.05	0.04	0.00	0.05	0.05
Real Estate	0.06	0.07	0.05	0.08	0.08

cludes more covariates. The estimates for men in columns (1)–(3) indicate that self-reported gay men do not have significantly lower hourly earnings than similarly situated heterosexual men. The coefficient estimates are very small and statistically indistinguishable from zero in all cases, even after I include only the purely exogenous characteristics (age and race). There is some evidence that bisexual men earn less than other men, though inclusion of occupation dummies eliminates the statistical significance of the bisexual male earnings penalty. These relatively sparse specifications explain over 40% of the variation in earnings.

For women in the rightmost columns (4–6), a similar pattern appears. Coefficient estimates on the lesbian indicator variable are small and statistically indistinguishable from zero, and this null finding is robust with respect to inclusion of occupation dummies. Congruent with the results for men, there is some evidence that bisexual women earn less than other women. In the fully saturated model with controls for occupation, self-reported bisexual women experience an earnings penalty of about 10.6%, and this estimate is marginally significant at the 10% level.[6] Moreover, these observed pat-

[6]For both men and women, the other control variables produced the predicted patterns of coefficients. Nonwhites earn less, while urban residents and the more highly educated earn more.

Table 5. Baseline GLB Earnings Differentials.
(Each Panel Is a Separate Regression)

	Men			*Women*		
Group	*Controls: Age and Race (1)*	*(1) + Marital Status, Education, Urban Residence (2)*	*(2) + Occupation (3)*	*Age and Race (4)*	*(4) + Marital Status, Education, and Urban Residence (5)*	*(5) + Occupation (6)*
Gay/Lesbian	–0.033	–0.008	–0.021	0.038	–0.021	–0.027
	(0.74)	(0.18)	(0.48)	(0.67)	(0.34)	(0.46)
Bisexual	–0.165***	–0.133**	–0.100	–0.044	–0.087	–0.106*
	(2.26)	(1.96)	(1.51)	(0.69)	(1.53)	(1.88)
R-Squared	.22	.37	.41	.16	.30	.34
Total N	9,245	9,245	9,245	8,912	8,912	8,912
Number Gay/Lesbian	307	307	307	179	179	179
Number Bisexual	123	123	123	195	195	195

Absolute values of t-statistics are in parentheses. The dependent variable is the natural logarithm of average hourly earnings. The sample consists of "full-time" non–self-employed workers (at least 35 hours last week) earning between $1 and $200 per hour.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

terns for women persist in additional analyses that attempt to account for differential selection into labor market activity that may be correlated with earnings, as well as the possibility that the usual measures of labor market experience artificially understate true experience for lesbians. Specifically, controlling for "potential experience" or the number of children (or the interaction between the two) does not change the results, and a Heckman selection correction model similarly reveals no economically or statistically significant lesbian earnings differential.

In Table 6 I investigate several extensions to the baseline model for both men (top panel) and women (bottom panel). In each case, the coefficients on the GAY/LESBIAN and BISEXUAL dummy variables are reported, though the regressions include controls for all the variables as in Table 5 (including occupation dummies), unless otherwise noted. Column (1) reports the baseline model estimates. In column (2) I include all part-time workers, as defined by those who report having worked at least 20 hours last week. Inclusion of these workers has no effect on the GAY/LESBIAN estimates for men or women, though the BISEXUAL male indicator variable is statistically significant.

In column (3) of Table 6 I report coefficient estimates from a model that excludes the dummy variable for being married. The concern here is that because marriage is an institution that is legally unavailable to gay men and lesbians in the United States, it does not conceptually make sense to "control" for marital status.[7] One way to gauge the importance of this issue is simply to exclude the "married" dummy. Column (3) of Table 6 indicates that marriage is an important determinant of the earnings differential between gay and straight men.

[7]A handful of states have recently recognized same-sex marriage and same-sex civil unions. At the time the 2001 CHIS was administered, however, same-sex marriage was not legal in California.

Table 6. GLB Earnings Effects.
(Each Column Is a Separate Regression)

Group	*Baseline Model (1)*	*Include Part-Time Workers (2)*	*No Marital Status Control (3)*	*(1) + Job Tenure and Firm Size (4)*	*35 and Under (5)*	*Over 35 (6)*
Men						
Gay Male	–0.02	–0.03	–0.12***	–0.02	–0.06	0.03
	(0.48)	(0.60)	(2.80)	(0.55)	(0.83)	(0.60)
Bisexual Male	–0.10	–0.13**	–0.12*	–0.11*	–0.17	–0.04
	(1.51)	(2.00)	(1.83)	(1.82)	(1.64)	(0.50)
R-Squared	.41	.41	.40	.45	.36	.35
Total N	9,245	9,950	9,245	9,245	3,221	6,024
Gay Male N	307	341	307	307	115	192
Bisexual Male N	123	132	123	123	38	85
Women						
Lesbian	–0.03	–0.06	–0.06	–0.01	0.03	–0.04
	(0.46)	(0.99)	(1.01)	(0.11)	(0.21)	(0.67)
Bisexual Female	–0.11*	–0.12**	–0.12**	–0.09	–0.09	–0.13*
	(1.88)	(2.24)	(2.11)	(1.60)	(1.01)	(1.84)
R-Squared	.34	.35	.34	.39	.36	.27
Total N	8,912	11,007	8,912	8,912	2,782	6,130
Lesbian N	179	196	179	179	43	136
Bisexual Female N	195	247	195	195	83	112

See notes to Table 5.

Excluding the married dummy results in a statistically significant 12% earnings penalty for gay men, while the bisexual male earnings penalty is also estimated at 12% (marginally significant at the 10% level). For women, however, excluding the married dummy does not materially alter the baseline results: bisexual women are estimated to earn 12% less than straight women, while lesbians earn 6% less, though the latter estimate is not statistically significant.

In column (4) I incorporate more detailed labor market information available in the CHIS. In particular, I include dummy variables for different lengths of job tenure at one's current position (5 categories), as well as firm size (5 categories). For both men and women, inclusion of this additional information leaves the null finding for a gay or lesbian sexual orientation unchanged. In columns (5) and (6) I investigate whether splitting the sample by age affects the overall pattern of results; it does not.

The pattern of results found here—in which the wages of gay men and lesbians do not significantly differ from those of similarly situated straight people, despite modest evidence that bisexuals earn less—appears inconsistent with the findings of previously published studies (described above). In particular, the coefficient estimates on the gay (–0.02) and lesbian (–0.03) dummy variables from CHIS 2001 data are noticeably smaller than previously published estimates using the GSS. These differences are unlikely to be explained by different models, since the specifications in Table 5 are very similar to the previous approaches. Indeed, replicating the empirical specifications in Black et al. (2003) and Blandford (2000) as closely as possible using CHIS 2001 leaves the key patterns unchanged: CHIS 2001 data consistently indicate that gay men and lesbians earn about the same as unmarried straight individuals, while bisexuals may earn less (results not reported).

How divergent, though, *are* the GSS and CHIS patterns? For men, I argue that the two sets of results are actually quite consistent. To see this, recall that Black et al.'s preferred conceptual approach assigned sexual orientation based on the relative degree of same-sex sexual exclusivity the respondent reported over the past 5 years. Their argument (with which I agree) is that exclusively same-sex sexual behavior over the past 5 years is likely more correlated with being openly gay or lesbian than is behavior over the past 12 months or weaker requirements about the number of recent same-sex sexual partners. As such, separating behaviorally bisexual individuals from behaviorally homosexual individuals comes closer to the conceptual distinction in the CHIS (using self-reports) than combining the two groups.

In fact, all of the evidence supporting a statistically significant sexual minority male earnings penalty combines behavioral homosexuals and bisexuals. Indeed, the estimates in Black et al. (2003) that separate behaviorally homosexual men from behaviorally bisexual men produce a pattern of results that is qualitatively identical to the CHIS results presented above. Their "Gay" male dummy variable is never statistically distinguishable from zero at standard confidence levels when behaviorally homosexual men are separated from behaviorally bisexual men for either the one-year or five-year sexual behavior definition. Furthermore, the Gay coefficient is statistically indistinguishable from zero for their preferred measure using sexual behavior in the last five years even when behaviorally bisexual men are combined with gay men (their Table 7, column 4, lower panel). Incorporating occupation codes and family structure variables (their Table 8) further attenuates the behavioral Gay coefficient, and it remains statistically insignificant in all subsequent specifications.

To further investigate the sensitivity of the previous GSS estimates, I considered more recent waves of the GSS (1998 and 2000) by pooling the data with the earlier years to obtain larger sample sizes of sexual minority individuals. Neither approach yielded a statistically significant earnings penalty for behaviorally gay men. Table 7 presents author estimates from the GSS, following the preferred specification reported in Black et al.'s Table 7, column (5) (separately considering behaviorally homosexual and bisexual individuals based on sex of sex partners in the previous five years). Column (2) of Table 7 shows estimates from GSS data that include only the most recent years available—1998 and 2000; the results indicate no statistically important earnings difference for either behaviorally gay or bisexual men in the more recent GSS data. These results are confirmed in the pooled 1988–2000 analysis (column 3).

Closer inspection of the existing results for men—combined with an updated analysis of more recent GSS data—therefore uncovers an alternative interpretation for understanding the earnings of sexual minority men. First, there is no statistically significant difference between the earnings of men who are most likely "gay"—as measured either by direct self-reports or by sexual behavior based on the exclusivity and recency of same-sex sexual contact—and the earnings of similarly situated heterosexual men (or, putting it another way, the main source of the observed earnings difference is the well-documented "marriage premium"). Second, bisexual men do not earn significantly more than similarly situated heterosexual men. The CHIS 2001 estimate for bisexual men is negative and marginally statistically significant (depending on the specification), while the sign of the GSS estimates depends on the time period chosen but is never statistically significant. Finally, the earnings difference between gay and bisexual persons appears to be on the same order as that between heterosexuals and non-heterosexuals.

Important differences remain when one compares the female earnings estimates across the two data sources, however. Recall that the previous papers provided strong

Table 7. Sensitivity of GSS Estimates to Choice of Time Period.
(Sexual Behavior Based on Sex of Sex Partners in Previous Five Years)

	Men			*Women*		
Variable	*GSS 1988–96 (1)*	*GSS 1998–2000 (2)*	*GSS 1988–2000 (3)*	*GSS 1988–96 (4)*	*GSS 1998–2000 (5)*	*GSS 1988–2000 (6)*
No Sex	–0.22***	–0.27***	–0.24***	0.01	–0.05	0.004
	(2.75)	(2.53)	(3.63)	(0.20)	(0.65)	(0.07)
Behaviorally Bisexual	–0.15	0.13	–0.05	0.15	–0.39***	–0.07
	(1.11)	(0.57)	(0.66)	(1.14)	(2.72)	(0.70)
Behaviorally Gay/Lesbian	–0.12	–0.008	–0.10	0.26*	0.19	0.27***
	(1.25)	(0.07)	(0.84)	(1.90)	(1.35)	(2.68)
Total N	2,032	1,188	3,220	1,778	1,077	2,855
Behaviorally Gay/Lesbian N	45	40	85	24	25	49
Behaviorally Bisexual N	21	9	30	27	23	50

Absolute values of t-statistics are in parentheses. The sample consists of "full-time" workers and those with a job but not at work when surveyed. All models include controls for education (in years), potential experience, squared potential experience, a "white" dummy, a "large metropolitan area" dummy, and indicators for 4 geographic regions. Models are estimated using midpoint regression (Black et al. used "interval" regression).

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

support for the existence of a lesbian earnings premium in the 1988–96 GSS annual earnings data, while 2001 CHIS data indicated no significant lesbian effect using average hourly earnings. In results not reported, the null finding for lesbians was unchanged when I considered a measure of monthly earnings (unadjusted for hours). This is important, as it suggests that differential labor supply intensity associated with sexual orientation—an explanation of the lesbian GSS premium favored by Berg and Lien (2002)—is unlikely to explain the differences between the GSS and CHIS results.[8]

There is some evidence that temporal differences may contribute to this divergence in findings, as shown by the updated GSS estimates using data from the 1998 and 2000 waves, as well as the pooled 1988–2000 sample. Column (5) of Table 7 shows that the lesbian earnings effect in the 1988–96 data falls by over 25% in the 1998–2000 data, and it is statistically indistinguishable from zero despite the fact that sample sizes of sexual minority women in the 1998–2000 data are almost identical to those in the 1988–96 sample. It remains true, however, that the pooled GSS sample exhibits a large and statistically significant lesbian earnings premium. Notably, the bisexual female coefficient is estimated to be large, negative, and statistically significant (though the magnitude is too large to be believed—39% of real annual income).

Finally, it is also likely that spatial differences between the two samples explain some of the divergent patterns. Though state identifiers are not publicly available in the GSS, regional variables available in Berg and Lien (2002) indicate that only 28% (23%) of the behaviorally homosexual men (women) in the GSS reside in the Pacific states (California, Oregon, and Washington). As noted above, California has a unique history with respect to tolerance and civil rights protection. Indeed, in 1992 California passed a statewide ordinance banning employment discrimination on the

[8]All of the studies, including mine, restrict the main sample to full-time workers.

basis of sexual orientation. It might be that in 2001, sexual orientation mattered less for earnings there than it did in other areas in the 1990s.

Discussion

A number of important issues concerning the interpretation of these results should be noted. First, it is interesting to ask how well previously proposed theories explain the observed earnings effects associated with sexual orientation. The lack of gay and lesbian earnings penalties is generally inconsistent with employer wage discrimination against sexual minority individuals.[9] In a simple model of discrimination, employers may have a personal distaste for gay men, lesbians, and bisexuals. If sexual orientation is demographically identifiable, employers may choose to employ GLB individuals less or pay them less conditional on employment (if it takes time to learn an employee's sexual orientation, for example). The CHIS data offer little support for the first effect—lower employment for sexual minority individuals than for straight individuals. Regressions predicting full-time employment do not show that gay men experience statistically significant employment penalties, and they suggest that lesbians are *more* likely to work full-time than are other women.

With respect to the second effect—lower earnings conditional on employment—the support for a discrimination story is mixed. If self-reported gay men and lesbians are more likely to be "out" in the workplace compared to bisexuals, then one would expect discrimination to result in larger and more salient penalties for gay men and lesbians than for bisexuals—a pattern opposite the one observed in the CHIS. Moreover, models that treat all sexual minorities as one group (that is, including bisexuals with gay men and lesbians) result in statistically insignificant earnings effects associated with sexual orientation. Both of these facts—a larger penalty for bisexuals compared to gay men and lesbians, and the lack of a significant earnings penalty when the groups are pooled—are generally inconsistent with simple discrimination stories.

It is possible, however, that the pattern of estimates for bisexuals as compared to gay men and lesbians reflects differences in earnings effects of discrimination, traceable, in turn, to systematic differences across these groups in the development of sexual identity and sexual orientation. Suppose that among those with same-sex sexual attraction, certain unobserved differences account for the fact that some label themselves as "gay" or "lesbian" while others choose the term "bisexual." These same differentiating characteristics—which might be anything from family background to political activism—could also be correlated with earnings. In the presence of discrimination against sexual minorities, it could be simply that people who choose to call themselves "gay" or "lesbian" as opposed to "bisexual" are more likely to have developed mechanisms for reducing the economic effects of such discrimination. Indeed, the pattern of demographic characteristics suggests this dynamic may be important: men who describe themselves as "gay" are more likely to be white and more likely to be highly educated than men who describe themselves as "bisexual." These patterns would be consistent with a discrimination story in which choice of label is endogenous.

The pattern of evidence presented in this paper also provides mixed support for a Becker-type household specialization story. As noted above, the Becker model implies that lesbian women will earn more than heterosexual women, while gay men will earn less than straight men. The data strongly suggest that—on average—gay men and lesbians earn about the same as similarly situated straight men and women.

[9]Of course, it is completely plausible that gay men, lesbians, and bisexuals experience other forms of labor market discrimination that are unmeasured in the CHIS. Harassment in the workplace regarding sexual orientation (for example, offensive jokes), glass ceilings, and overall negative attitudes will generally not be reflected in models of earnings or employment.

Putting aside the nonconformity of the overall pattern of earnings effects with the Becker household specialization story, do the data support the human capital predictions of the Becker model? Specifically, recall that the gender-specific specialization argument implies that men who anticipate partnering with men will under-invest in human capital relative to heterosexual men; and in contrast, women who anticipate partnering with women will over-invest in human capital relative to heterosexual women. While the prediction for women is borne out in the data—lesbians have higher levels of educational attainment than never-married straight women—the associated prediction for gay men is contradicted by CHIS 2001. Indeed, with respect to *observable* measures of human capital, gay men are much more likely than straight men to have a college degree, and they are several times less likely than straight men to be high school dropouts. Overall, the patterns of earnings effects and human capital accumulation provide mixed support for Becker household specialization.

A final issue concerns how best to interpret the sensitivity of the gay male earnings effect to inclusion of a control for marital status. To provide a point of comparison with the previous GSS studies (all of which accounted for marital status in some way), the baseline estimates presented here include the dummy variable for being currently married. This resulted in important qualitative differences across the two sets of findings: whereas previous studies have found that a large gay male earnings penalty persists even after controlling for marital status, I find no independent effect of a gay sexual orientation in similarly specified models.

Alternatively, one could view the two estimates (with and without the marital status control) as representing the range of possible earnings effects associated with sexual orientation for men. Indeed, this is the approach taken in a recent paper by Allegretto and Arthur (2001). Specifically, they found a small differential between men in same-sex couples and men in unmarried different-sex partnerships (favoring straight men) and a large differential between men in same-sex couples and men in traditionally conceived heterosexual marriage. A similar interpretation emerges in the current context: if gay men are more similar to married men than to unmarried men, then the estimate excluding the "married" dummy in Table 6—indicating a statistically and economically significant 12% earnings penalty—may be more appropriate.

Ideally, one would identify those gay men in partnerships who would marry were they not legally barred from doing so. Absent information on the length and nature of the "unmarried partner" relationships in the data, I experimented with a variety of alternative approaches. For example, in a model controlling only for whether an individual is in any type of partnership (not limited to marriage per se), the gay male earnings penalty fell by 40% and was statistically significant only at the 10% level. Including controls for both marital status and partnership reduced the gay coefficient further, rendering it very similar to the baseline estimates in Table 5. These exercises confirm that there is something conceptually distinct about traditionally conceived heterosexual marriage that contributes positively (negatively) to earnings for straight (gay) men.

How one interprets the results from models that exclude the marital status control depends largely on what one believes is the true source of the male marriage premium: productivity or selection. While this issue has not been settled in the literature, advances by Korenman and Neumark (1991) and Ginther and Zavodny (2001) suggest that productivity underlies the bulk of the marriage bonus. Alternatively, it could be that heterosexual marriage signals to employers that an individual is *not* gay, in which case the negative and statistically significant coefficient on the GAY dummy in models that exclude marital status may indeed reflect a type of discrimination. Further exploration of this issue is beyond the scope of the current data but could be undertaken by future research.

Conclusion

This paper has revisited the effects of sexual orientation on individual earnings by using a data source that—while specific to the state of California—is much more comprehensive than the sources on which previous studies have relied. After I account for the marriage premium, I find little evidence that gay men or lesbians in California are paid differently from similarly situated straight men and women. In contrast, there is modest evidence that bisexual men and women in California earn about 10% less than heterosexual men and women.

Several key contributions are offered in addition to the new earnings estimates. First, the results suggest that neither simple discrimination nor Becker household specialization can fully explain the CHIS 2001 patterns. Second, I find important qualitative and quantitative differences between gay and bisexual persons, suggesting it may be unwise to group all sexual minorities together in the labor market context. Finally, I extend recent GSS work and show that the previous estimates appear somewhat sensitive to choice of time period.

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