Meta-Analysis of the Effects of Sexual Orientation on Earnings*

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Most studies show that gay men earn less and lesbians earn more than their heterosexual counterparts, but the size of estimated sexual orientation differences varies greatly across studies. Using studies published between 1995 and 2012, a meta-regression shows that the gay sample size, sexual orientation measure, and controls for work intensity explain variation in estimates for men. For women, there are few conclusive influences although controlling for work intensity seems to be most important.

Introduction

In 1995, Badgett published the first econometric study of the effects of sexual orientation on earnings. The work followed a body of literature estimating similar models for differences by race and gender using traditional regression techniques (Altonji and Blank 1999). Badgett's study was made possible by the emerging confluence of data on sexual orientation and earnings, a researcher willing to work on a stigmatized subpopulation, and an economics journal willing to publish the results—all elements that were rare or nonexistent prior to that time (Klawitter 1998).

Since Badgett's first article appeared, researchers have used a growing number of datasets and models to explore sexual orientation earnings differences. The body of work that has accumulated addresses the key scientific questions with important policy implications: Do earnings show evidence of labor market discrimination on the basis of sexual orientation?, What other factors interact with sexual orientation in determining earnings?, and What are the contributions of human capital and intrahousehold influences? However, the estimated sexual orientation differences and the answers to these questions are less than uniform across studies, which suggests the necessity of a systematic

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assessment of the literature. Differences in the measures of earnings and sexual orientation, choice of dataset, sample limitations, control variables, and modeling choices might all contribute to the variation in estimates and to conclusions about how sexual orientation affects earnings. Although the number of published studies is still small at thirty-one, a meta-analysis will provide a much-needed summary of the patterns in the findings and guidance for researchers and those interested in collecting new data.

Sexual Orientation and Earnings

Recent studies have found that, before controlling for any explanatory factors, lesbians earn significantly more than do heterosexual women and gay men earn less than heterosexual men. For example, Cushing-Daniels and Yeung (2009) show unadjusted earnings differences equal to a 26 percent premium for lesbians over heterosexual women and a –7 percent penalty for gay men compared to heterosexual men using General Social Survey (GSS) data.[1] The gender gaps are also large, with lesbians and heterosexual women earning 13 and 38 percent less than heterosexual men, respectively. The gender differences in earnings explain the household income patterns: Lesbian couples have household incomes lower than those of married heterosexual couples, while gay male couples have incomes similar despite having individual earnings lower than those of married men (Klawitter 2011: 353).

Following earlier studies of labor market differences on the basis of gender or race, researchers have used multivariate analysis to parcel out variation in earnings levels to that related to expected productivity (through education and experience, occupation, industry, geographic location), tastes (other observed characteristics), and the unexplained component that could be attributable to discrimination. Beginning with Badgett (1995), almost all studies have found that gay men earn less than heterosexual men after controlling for other characteristics (e.g., Carpenter 2007; Elmslie and Tebaldi 2007; Klawitter 2011; Martell 2012). And most, but not all, studies find that lesbians earn more than their heterosexual counterparts (e.g., Antecol, Jong, and Steinberger 2008; Black et al. 2003; Jepsen 2007). However, the size of estimates of sexual orientation differences for both men and women range widely. For example,

[1] Author calculations using unadjusted means from Cushing-Daniels and Yeung (2009). Similarly, Antecol, Jong, and Steinberger (2008), using the 2000 U.S. Census, reported unadjusted hourly earnings lower by 4.6 percent for men and 18 percent higher for women in same-sex couples compared to men and women in married different-sex couples. Both gay men and lesbians had earnings about 25 percent higher than those in unmarried different-sex couples.

Blandford (2003) found a lesbian premium of 30 percent for unmarried lesbians compared to married women, whereas Carpenter (2008b) found that young lesbians earned about 25 percent less than young heterosexual women in Australia. For men, Klawitter (1998) found that men in same-sex couples earned 30 percent less than similar married men, whereas several authors found no significant earnings differences by sexual orientation and point estimates near zero (e.g., Carpenter 2005; Frank 2006). These endpoints are not lone outliers as estimates from other studies fill out the ranges.

In addition to the widely ranging size of estimates, the finding of an earnings premium for lesbians has been a puzzle within the literature. Most early authors, drawing on models of discrimination and surveys of gays and lesbians, expected earnings penalties for both gay men and lesbians. Indeed, this puzzle may have accelerated the growth in the literature as authors worked to assess possible explanations such as controlling for child-rearing and work experience (e.g., Jepsen 2007; Antecol, Jong, and Steinberger 2008; Daneshvary, Waddoups, and Wimmer 2009). The meta-analysis here is aimed at systematically summarizing evidence about how these and other factors affect the estimated size of sexual orientation differences.

Together, the choice of dataset with its specific sample criteria, earnings or income measure, sexual orientation measure, and potential control variables, along with methods and modeling choices made by researchers, could explain the wide variation in the size and significance of the findings from studies of sexual orientation differences. In addition, the results from the meta-analysis provide insight as to the strength of the evidence of sexual orientation discrimination and the roles of human capital and intrahousehold decisions.

Meta-Regression Methodology and Sample

Meta-analysis provides a framework for systematically synthesizing results from multiple studies (Glass 1976; Hedges and Olkin 1985; Hunter and Schmidt 1990). Meta-regression uses multivariate meta-analysis to estimate the size of a parameter (here the effect of sexual orientation on earnings) and how it varies systematically with characteristics of the data and methods of studies.[2] Unlike most literature reviews, meta-regression creates a replicable and systematic synthesis by requiring identification of the search strategy for studies,

[2] Stanley (2001) provides an outline of meta-analysis focused on studies by economists, but more statistical detail is available in Hedges and Olkin (1985), Hunter and Schmidt (1990), and Stanley and Jarrell (2005).

the metric being summarized, a common set of study characteristics for each study, and an explicit method for summarizing how the metric typically varies with the study characteristics.

Recently, Jarrell and Stanley (2004) and Weichselbaumer and Winter-Ebmer (2005) used meta-regression to assess the econometric studies of the effects of gender on earnings. Both papers found evidence of decreasing but persistent gender differences consistent with discrimination in earnings over time. My article follows similar methods to summarize and understand the recent literature on earnings differences based on sexual orientation.

Meta-analysis sample. The sample for this meta-regression includes all English-language journal articles published prior to April 2012 using multivariate analysis of earnings, income, or wages to assess the effects of sexual orientation. I began with studies from a literature review by Badgett et al. (2007), then used Google Scholar to identify any additional studies that address "sexual orientation and earnings" or "gay and earnings," or that cite Badgett (1995) or other early studies including Klawitter and Flatt (1998), Blandford (2003), Allegretto and Arthur (2001), Black et al. (2003), and Carpenter (2005).[3] I also reviewed citations in these and other identified studies for any additional works. This search process yielded published thirty-one articles using seventeen different datasets, several of which provided multiple estimates. Appendix A lists the studies used for this paper.

I included only published articles to better define the population of studies and to capitalize on the peer review process, although this method could introduce bias towards larger and more significant estimates.[4] In general, I chose only one estimate from each study so as to not give more weight to studies that used many models or subsamples, although I did include multiple estimates from articles estimating results for both men and women (most studies), using multiple datasets, separate models comparing gays to married and unmarried heterosexuals, or for mutually exclusive subsamples of gays and lesbians (e.g., those married and unmarried or partnered and unpartnered). I chose estimates from regression models that included the largest set of covariates for the full sample and, if available, used estimates from a Oaxaca–Blinder decomposition or a model with a Heckman selection correction.[5] I also

[3] I use Google scholar rather than Econ-Lit because studies are published in non-economics and international journals.

[4] A perusal of seven unpublished studies available on the Web showed results all well within the range of the published studies used here.

[5] For papers with models using multiple measures of sexual behavior with similar numbers of covariates or methods, I used those with a 5-year window for sexual behavior.

collected all multivariate estimates from each paper (rather than the "best" model) and discuss results using this much larger sample for comparison purposes.[6]

The number of studies is still very small, yielding just thirty-four separate estimates for men and twenty-nine for women. However, a review of studies and preliminary evidence of influence of methodological designs will solidify our understanding of the broad conclusions and help guide researchers in their choices of datasets, outcomes, sexual orientation measures, control variables, and models. I first summarize estimates of the sexual orientation earnings differences by study characteristic, then use multivariate meta-regression as described below.

Meta-regression model. The outcome of the meta-regression model is the estimate of the percentage difference in earnings between gays or lesbians and heterosexuals from each study, s, (Gay% diff):

$$\text{Gay}\% \text{ diff}_s = \delta_0 + \delta Z_s + \varepsilon_s$$

The explanatory factors (Z) are the study characteristics. In the simplest multivariate model, I use only indicators of which dataset was used: U.S. Census 2000, General Social Survey (GSS), other U.S. dataset, or a non-U.S. dataset, with U.S. Census 1990 serving as the reference category.[7] This model provides a baseline estimate of how much variation in estimates of sexual orientation differences is driven simply by the choice of dataset. Because the choice of the dataset often dictates the choice of sexual orientation and earnings measure as well as options for accounting for human capital and work intensity, I then replace the dataset indicators with indicators for the study characteristics. These include the type of earnings and sexual orientation definitions, sample restrictions, regression methods, and control variables. All models are random-effects meta-regressions that I weighted to account for the varying precision in estimates across studies (Harbord and Higgins 2008). The standard errors for the meta-regression are calculated with the Knapp–Hartung estimation method, which limits the rate of false-positives (rejecting the null hypothesis of zero coefficients), but may be overly conservative for small

[6] These models include much larger samples of estimates, but may overemphasize studies with many sensitivity analyses and for many of the models it is difficult to ascertain sample size and other characteristics.

[7] The U.S. Census bureau recoded some couples who were likely different-sex couples with miscoded partner's gender in the 2000 Census as same-sex couples (Black et al. 2007). All but one of the studies using the 2000 census followed the recommended procedures to ensure that these were not included in the sample of same-sex couples. That study had findings that were very similar to those for other studies (Baumle and Poston 2011).

samples (Harbord and Higgins 2008: 498).[8] For the models that don't include indicators for the dataset, I also provide sensitivity analyses that directly account for the interdependence of findings from studies that use the same dataset (Hedges, Tipton, and Johnson 2010). As mentioned above, I also assess the sensitivity of the results to using estimates from all multivariate models in these studies, rather than only the most complete model.

Outcome variable. Most studies used in this meta-analysis used ordinary least squares regression where the outcome is the natural log of annual earnings and the focus is on the coefficient on an indicator of sexual orientation (β_{gay}).

$$\log(\text{earnings}) = \beta_0 + \beta_{\text{gay}}\text{GAY} + \beta_x X + \varepsilon_i$$

Models are usually run separately for men and women and, at a minimum, also control for age, education, race, and geographic location.[9] The coefficient on the indicator of being gay or lesbian β_{gay} shows the gap in average log earnings associated with being a sexual minority, which could reflect discrimination as well as other differences correlated with sexual orientation.[10] Alternatively, some researchers use "decompositions," which allow the coefficients on all explanatory variables to differ by sexual orientation and parse out the total difference in outcomes to that attributable to group differences in observed characteristics and that due to differences in the coefficients (Blinder 1973; Oaxaca 1973).[11] I converted all results to a percentage difference in earnings by sexual orientation that serves as the outcome variable in the meta-regression. I also collected standard errors for these estimates and these are used by the meta-regressions to weight the more precisely estimated parameters more heavily (Harbord and Higgins 2008).

Explanatory variables. The challenge in this meta-regression is to parsimoniously characterize the studies in order to best explain the variation in findings despite having a very small sample of estimates and high degree of

[8] I also tried using permutation-based methods for estimating p values for coefficients (Harbord and Higgins 2008: 502), but these were unstable even when using fifty thousand permutations.

[9] Several studies pool models for men and women using indicators for gender/sexual orientation combinations. This restricts all other coefficients to be the same by gender.

[10] Where models included continuous variables interacted with sexual orientation, I added in the interaction term multiplied by the mean level for the sexual minority group to get an overall difference in earnings. I did not add in interaction terms for discrete variables. I did not include estimates for bisexuals if reported as a separate group.

[11] One study using a decomposition did not include standard errors so was not included (Antecol, Jong, and Steinberger 2008). For studies that use three-part decompositions, I included the portion from coefficients and half of the interaction portion (e.g., Martell 2013).

correlation in study characteristics given the constraints of most datasets. (Appendix B includes descriptive statistics for all study characteristics separately for studies of men and women. The correlation matrix is available from the author by request.)

Until the 1990s, datasets combining a measure of sexual orientation and information on wages, earnings, or income were not available other than for convenience samples usually for a local area. Badgett's 1995 paper used data from the General Social Survey (GSS), which added questions on sexual behavior in 1989 (Badgett 1995). Other early studies used U.S. Census data for same-sex couples after respondents were allowed to choose "unmarried partner" as a relationship option starting with the 1990 census (Allegretto and Arthur 2001; Black et al. 2000; Clain and Leppel 2001; Klawitter and Flatt 1998). Since then, sexual orientation questions have been added to other U.S. datasets[12] and to developed country datasets, including those from Canada, Australia, United Kingdom, Netherlands, France, Greece, and Sweden.[13]

Datasets have included one of three types of sexual orientation questions: (1) questions on the gender of past sex partners (sexual behavior), (2) questions on the gender of married or unmarried partners (couple status), or (3) questions on sexual identity (self-identification as heterosexual, gay or lesbian, or bisexual).[14] These questions point to overlapping but distinct constructs of sexual orientation that can be used to identify a subsample of sexual minorities and nonminorities (Laumann et al. 2000; Sexual Minority Assessment Research Team 2009) and that might affect estimates of sexual orientation differences (Black et al. 2000; Carpenter 2008a).

Workplace discrimination may be more likely when gay men or lesbians are more visible and those with a same-sex partner or willing to identify as gay might therefore have lower earnings than those with more hidden sexual behaviors, so studies using sexual behavior measures might find smaller sexual orientation differences. Also, most, but not all, studies using couple status as the indicator of sexual orientation limit their comparison samples to those in married or unmarried different-sex couples and some evidence suggests that

[12] Other U.S. datasets used by studies in the meta-analysis include Current Population Survey, Behavioral Risk Factor Surveillance System, National Health and Nutrition Examination Surveys, and California Health Interview Survey. All U.S. datasets were national with exception of the California survey. I assess the effects of dropping the California survey from the sample.

[13] One dataset, the International Social Survey Programme data, included respondents from the United States, Australia, Ireland, Poland, and Bulgaria.

[14] Studies sometimes also ask about sexual attraction, but those questions have not been used as the primary identifier for sexual minorities in economic studies. Questions on gender identity or transgendered status are also asked on a few surveys, but constitute a separate dimension of sex and sexuality than does sexual orientation.

sexual orientation differences might be larger among those in couples given selection processes for who is coupled (Carpenter 2008a).[15] Many researchers use married couples as the comparison group for those in same-sex couples and this may magnify differences given more intensive household specialization and the possible male "marriage bonus" for married heterosexual couples (Antecol and Steinberger 2009; Zavodny 2007).

Other explanatory variables in the meta-regression describe basic dataset characteristics including the year of the outcome data (the midpoint if a range of survey dates was used), size of the gay sample, and an indicator of whether or not the data are from the United States.[16] Discrimination might be decreasing over time, mirroring the patterns in public opinion on homosexuality or in response to the increasing number of state and local policies prohibiting sexual orientation discrimination (Barron and Hebl 2012; Klawitter 2011; Loftus 2001). Larger samples of sexual minorities are likely to result in more stable estimates of sexual orientation differences but might also be associated with changes in the size of estimates. The "additional" respondents in larger samples may have a different mix of earnings levels if, for example, more low-income gays and lesbians become willing to disclose their sexual orientation.

It is unclear whether sexual orientation differences are likely to be higher or lower in other developed countries than in the United States and the mix of non-U.S. studies here includes some with more regulated labor markets and less discrimination (Sweden and Canada) and some with more market-based policies (Australia and U.K.) (Kelly 2001). I controlled for this with the indicator of whether the study was from the United States and also tried a model that included only U.S. studies to assess the consistency of the results across countries.

In additional models, I add variables describing the study's treatment of work intensity: indicators of the outcome being hourly earnings (rather than annual or monthly earnings),[17] whether the sample was limited to full-time workers, and whether control variables accounted for hours worked or part-time status. These variables are important to understanding the role work choices and intrahousehold time allocation might play in sexual orientation differences, but could also themselves partly capture effects of discrimination. I expected these to be most important in studies of women given greater hours worked for lesbians than for

[15] Some researchers include all or a subset of unpartnered people, treating them all as a single comparison group (along with married and unmarried heterosexual couples) without distinguishing by sexual orientation.

[16] I also tried models that included both the sample size for sexual minorities and the overall sample size. The results are very similar to those presented here and the overall sample size was almost always statistically insignificant.

[17] All of the surveys ask for annual, monthly, or weekly earnings, but some researchers use these and work hours to construct hourly earnings measures.

heterosexual women, especially among those who are partnered (Antecol and Steinberger 2009; Black, Sanders, and Taylor 2007). Gay men have somewhat lower work hours and -weeks than do heterosexual men, although the differences are smaller than those among women (Black et al. 2007). These patterns in work intensity might reflect the influence of gender of partner on household income and for both men and women, controlling for work intensity might decrease the size of the sexual orientation differences.

Other models include indicators for whether the study uses controls for occupation or industry and presence of children in the household. Sexual minorities might try to minimize exposure to discrimination by choosing occupations or industries that are more gay-friendly, which means that controlling for these could lead to an underestimate of the effects of discrimination on earnings. However, other influences might also lead to gays and lesbians making different occupational choices, on average, than do heterosexuals. Although many same-sex couples are raising children, they do so less often than do heterosexual couples, and those raising children may make different work choices that could affect earnings differences (Black et al. 2000). For example, not accounting for child-rearing in estimates for women might result in estimating higher premiums for lesbians (Jepsen 2007).

Finally, I explore the effects of using a Heckman selection model or an Oaxaca–Blinder decomposition model. The selection correction might be important in accounting for the endogenous decision of labor market participation or full-time status, especially among women. Lesbians are less likely than are heterosexual women to work part time or to drop out of the labor market (Antecol and Steinberger 2009) and that selection process might contribute to a lesbian earnings premium. Allowing for differential influence (coefficients) of worker characteristics by sexual orientation with a decomposition might inflate or deflate the estimated differences by more carefully accounting for the variation in earnings associated with the levels of observed characteristics and, alternatively, with their pay-offs.

Some explanatory factors were included in all or nearly all studies so are not included as explanations for the size of estimated of sexual orientation differences: education, race, urban location, and gender are most important here.[18]

Because many of the study characteristics are dictated by the dataset creating correlation in the explanatory factors, I will start my analysis with simple comparisons by one characteristic at a time, then progress to multivariate models with more characteristics.

[18] Also, Jarrell and Stanley (2004) found that male researchers estimated larger gender wage gaps than did females. Many but not all of the studies here have at least one sexual minority researcher, but I do not know the sexual orientation of researchers on all studies so cannot include it here.

Results

Tables 1 and 2 summarize the thirty-four separate estimates of sexual orientation differences for men and twenty-nine for women using separate meta-regressions with no other controls.

Table 1 shows that the studies found, on average, that gay men earned 11 percent less than did heterosexual men although the estimates ranged from 30 percent less to no difference. Studies, on average, found that lesbians earned 9 percent more than heterosexual women and the range across studies was much wider than for men: from 25 percent less to 43 percent more.

Figure 1 is a scatter plot showing the variation in estimates of sexual orientation differences over time from 1989 to 2007. The simple unweighted regression lines show how estimates from studies of both men and women have converged towards zero over time although significant variation remains even in more recent studies. As discussed above, this convergence might be explained by decreasing discrimination or changing samples of sexual minorities, but might also be attributable to changing datasets, study designs, or modeling choices over time.

Table 2 explores the influence of key study characteristics on estimates of the sexual orientation differences.

About thirty percent of the estimates came from the U.S. Census data, with more using the 2000 data than the 1990 data. Another 29 percent of estimates were from the General Social Survey (GSS) and the remaining estimates used other U.S. datasets or data from other developed countries. Among men, the smallest estimates of the impact of sexual orientation come from countries other than the United States (–7 percent), and the U.S. datasets show a relatively narrow range of average estimates of earnings penalties for gay men, from –11 to –16 percentage points. The estimates of the gap for women range from 5 percentage points (other U.S. datasets) to 15 percentage points for GSS data. The large premium for lesbians in GSS studies will show up again below, as these studies share other characteristics including a sexual behavior measure for sexual orientation and an annual measure of earnings.

TABLE 1

SUMMARY STATISTICS FOR META-REGRESSION SAMPLES

	Men	Women
Number of studies	34	29
Average	−11%	9%
Standard error	2%	2%
Range	−30% to 0%	−25% to +43%

Note: Estimates from random effects maximum likelihood meta-regression constant.

TABLE 2

Sexual Orientation Differences by Study Characteristics

Characteristics of Study	Percent of Studies	Average Difference in Earnings or Income by Sexual Orientation	
		MEN	WOMEN
Dataset			
U.S. Census data 1990	9%	−15%	7%
U.S. Census data 2000	20%	−11%	8%
General Social Survey data	29%	−16%	15%
Other U.S. study	11%	−13%	5%
Non-U.S. study	31%	−7%	9%
	100%		
Sexual orientation measure			
Couple status	45%	−13%	7%
Sexual behavior	34%	−16%	15%
Self-identity	22%	−3%	8%
	100%		
Earning or income measure			
Annual or monthly (Individual)	73%	−12%	11%
Hourly (Individual)	27%	−8%	5%
	100%		
Sample limited to full-time workers			
Yes	43%	−13%	8%
No	57%	−10%	9%
	100%		
Method			
Heckman selection correction	19%	−18%	4%
Decomposition	18%	−12%	5%
Neither	62%	−13%	10%
	100%		

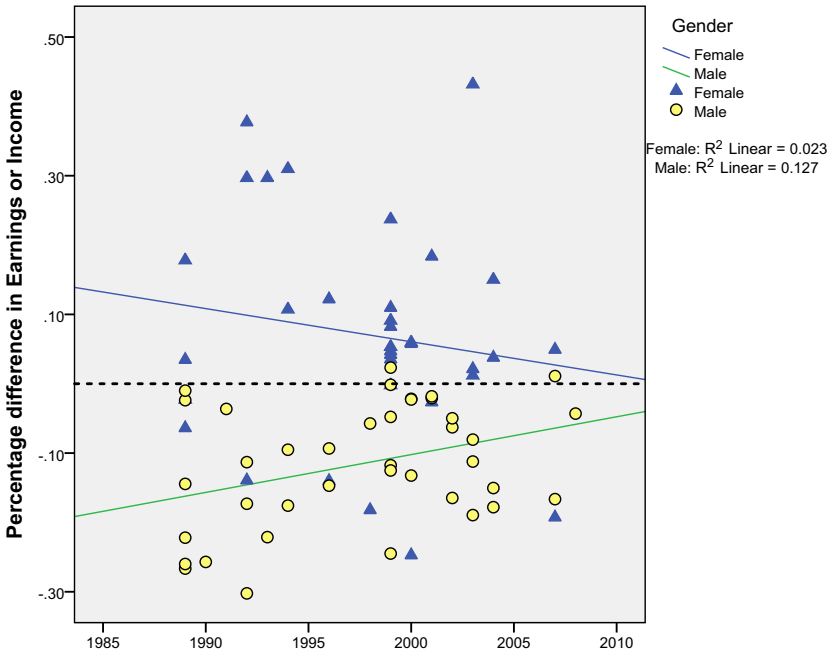
Note: Estimates from random effects maximum likelihood meta-regression constant.

About half the studies, including the U.S. Censuses and many of the non-U.S. datasets, use couple status (being in a same-sex couple) to identify people as gay, lesbian, or bisexual. The 34 percent of studies that use sexual behavior to categorize individuals are almost entirely made up of studies using the GSS.[19] Other U.S. and non-U.S. studies used an explicit question on sexual orientation identity and these studies estimated the smallest effects of sexual orientation for men (–3 percent). Contrary to the theory about sexual behaviors being less overt than couple status or identity, the studies using sexual behavior measures, primarily the GSS, find the largest earnings differences: –16

[19] One of the two non-GSS studies used the International Social Survey Programme data for both men and women (Heineck 2009). The other study with a sexual behavior measure used the National Health and Nutrition Examination Survey (Carpenter 2007). That survey asked about household income, but Carpenter limited his sample to men living alone to get individual income.

FIGURE 1

ESTIMATES FOR SEXUAL ORIENTATION DIFFERENCE FOR MEN AND WOMEN OVER TIME



percent for gay men and +15 percent for lesbians. These larger differences from the GSS could result from the studies including as sexual minorities those who have had same-sex sexual partners in the past who might not be in a same-sex partnership or identify as gay or lesbian. The smaller differences found in studies using couple status is somewhat surprising, especially for women, given that many of these studies compare those in same-sex couples to those in married different-sex couples, which generally gives larger gaps than when comparing to cohabitating different-sex couples. Several studies have suggested that bisexuals may actually have worse earnings outcomes than gay men or lesbians (Carpenter 2005; Cushing-Daniels and Yeung 2009). Sexual identity questions are most likely to be used to estimate separate earnings differences for gay and bisexual respondents and that might explain why the estimates here for gay men and lesbians are smaller in size for studies that use identity measures.

As expected, studies using annual or monthly earnings (most of the studies) estimate larger sexual orientation differences than do hourly earnings studies given the effects of fewer hours of work for gay men, and more for lesbians

than for their heterosexual counterparts. Samples limited to full-time workers gave estimates that were similar to other studies for both men and women. Using a Heckman selection correction to account for labor market participation or full-time status gave estimates smaller in absolute size for women as expected and, surprisingly, larger estimates (more negative) for men. Decomposition models averaged estimates that were smaller for women, as with the Heckman correction, and did not affect the estimates for men.

Overall, these simple results highlight some of the key influences on estimated impacts of sexual orientation and multivariate analysis will help to disentangle at least some of them.

Meta-regressions of studies of men. Table 3 shows meta-regression results for men. Column 1 shows the model with only indicators for the dataset used. On average, studies using the 1990 U.S. Census estimated an earnings penalty of –14.2 percent for gay men (the constant). Although none of the dataset differences were statistically significant, those for the 2000 Census and the non-U.S. studies were relatively large, showing earnings penalties smaller by about 4 and 7 percentage points (more positive) than for studies with the 1990 Census. These dataset indicators capture only about 8 percent of the cross-study variation in estimates of sexual orientation differences (adj R^2 =.08).[20]

As described above, each dataset generally has only one measure of sexual orientation so the indicators of dataset and sexual orientation cannot be used together. Model 2 substitutes the basic dataset characteristics of year of outcome (base = 1989), gay sample size (divided by 1000), an indicator of U.S. data, and the sexual orientation measure for the dataset indicators. Together, the Model 2 variables explain a much higher proportion of variation in estimates for men (adj R^2 =.26).

Study estimates did not change significantly with the year of the study after controlling for other factors. The gay sample size and the indicator for U.S. studies were also not significant in this model before controlling for work intensity.[21,22] The Model 2 coefficients suggest that the earnings penalty for gay men is about 6 percentage points smaller (more positive) for studies using a measure of sexual identity than for studies using a couples status measure

[20] This adjusted R^2 is the proportion of between-study variance explained by covariates in the meta-regression (Harbord and Higgins 2008).

[21] To assess whether non-U.S. studies affected the other coefficients, I also ran a model using only the twenty-four estimates from U.S. studies with the covariates shown in Model 3. The coefficients were generally similar to those in Model 3 although the coefficient for hours worked controls was smaller and not significant and the constant was -.30.

[22] I also substituted the overall sample size for the gay sample size but it was not significant and decreased the adjusted R^2.

TABLE 3

META-REGRESSION RESULTS FOR MEN

	(1)		(2)		(3)		(4)		(5)	
U.S. Census data 2000	0.042	(0.058)								
General Social Survey data	−0.016	(0.051)								
Other U.S. study	0.012	(0.058)								
Non-U.S. study	0.072	(0.044)								
Year of outcome (midpoint less 1989)			−0.001	(0.003)	0.0004	(0.003)	0.0005	(0.004)	0.0005	(0.003)
Sample size/1000 (gay)			0.005	(0.003)	0.008**	(0.003)	0.009**	(0.003)	0.008**	(0.004)
U.S. data			−0.064	(0.043)	−0.128**	(0.046)	−0.138**	(0.054)	−0.113**	(0.054)
Sexual behavior measure			−0.008	(0.045)	−0.012	(0.042)	−0.004	(0.043)	−0.014	(0.043)
Sexual identity measure			0.063*	(0.036)	0.041	(0.027)	0.057	(0.039)	0.053	(0.043)
Hourly earnings measure (not annual or monthly)					0.029	(0.028)	0.048	(0.034)	0.026	(0.029)
Full-time workers only					0.142***	(0.045)	0.158***	(0.049)	0.136***	(0.047)
Controls for hours worked					0.082**	(0.030)	0.079**	(0.032)	0.075*	(0.040)
Controls for occupation or industry							−0.035	(0.031)		
Controls for children present							0.015	(0.043)		
Heckman model									−0.014	(0.057)
Decomposition model									0.021	(0.048)
Constant	−0.142***	(0.038)	−0.087*	(0.048)	−0.162***	(0.0422)	−0.157***	(0.044)	−0.170***	(0.045)
N	34		34		34		34		34	
Adjusted R^2	0.08		0.26		0.66		0.62		0.64	

Note: Dependent variable is percentage difference in earnings for gays and heterosexuals. Estimates from random effects maximum likelihood meta-regressions. Robust standard errors in parentheses. Reference categories are U.S. Census data 1990, couples status measure, and OLS model.
*$p < 0.10$, **$p < .05$, ***$p < .01$.

(the reference category).[23] Studies using sexual behavior measures have impacts similar in size to those using couple status. This, like the cross-tabulation results, is consistent with gay (apart from bisexual) identity being less associated with discrimination than sexual behavior or couple status.

Controls for work intensity are added in Model 3 showing that studies that limit samples to full-time workers or control for hours worked find smaller earnings penalties (more positive earnings estimates) for gay men by 14 and 8 percentage points, respectively. The hourly earnings study indicator coefficient was also positive, but not significant. This result is consistent with studies finding fewer hours and less full-time work for gay men than for heterosexual men (Elmslie and Tebaldi 2007; Klawitter 2011). Gender wage gaps could be the key influence here in that earnings for a current or future male partner could encourage gay men to choose more leisure or home work, but discrimination could also limit available work hours. After controlling for work intensity, the coefficients on the gay sample size and U.S. studies indicator become larger and more significant showing that studies with larger gay samples find smaller penalties for gay men and U.S. studies find larger penalties. The proportion of variation explained for men jumps considerably after adding the work effort variables, from R^2=.26 to .66.

Finally, Model 4 adds indicators for whether the study used controls for occupation or industry of employment and whether children are present, and Model 5 adds indicators for Heckman selection correction or decomposition models. None of these are significant. In models not shown, I find no significant differences in the estimates depending on whether the study controlled for health or disability limitations.

On the whole, the results suggest that characteristics of the studies of men explain a hefty proportion of the variation in the estimates of sexual orientation differences—much beyond identification of the dataset. Gay sample size, whether the data come from the United States, the type of sexual orientation measure, and controls for work intensity all contribute to variation across studies. I also replicated Model 3 in meta-regressions that accounted for interdependence of studies using the same dataset and with a sample using all estimates from these papers (n=250) with and without the dataset interdependence (Appendix C). These models generally show the same patterns in size and significance of factors, although with some variation across models.

[23] One study with a measure of sexual identity used data from California rather than the entire U.S. (Carpenter 2005). If I drop that observation from Model 3, the coefficient sizes and significance levels change very little.

TABLE 4

META-REGRESSION RESULTS FOR WOMEN

	(1)		(2)		(3)		(4)		(5)	
U.S. Census data 2000	0.021	(0.089)								
General Social Survey data	0.085	(0.099)								
Other U.S. study	−0.016	(0.104)								
Non-U.S. study	0.018	(0.086)								
Year of outcome (midpoint less 1989)			0.0004	(0.007)	0.0001	(0.007)	0.005	(0.007)	−0.0002	(0.01)
Sample size/1000 (gay)			0.0006	(0.004)	0.001	(0.006)	−0.0001	(0.006)	−0.006	(0.006)
U.S. data			−0.0009	(0.071)	0.051	(0.098)	0.101	(0.103)	0.129	(0.111)
Sexual behavior measure			0.077	(0.082)	0.108	(0.094)	0.023	(0.107)	0.130	(0.089)
Sexual identity measure			0.012	(0.070)	0.041	(0.074)	−0.081	(0.102)	0.018	(0.086)
Hourly earnings measure (not annual or monthly)					−0.008	(0.072)	−0.056	(0.075)	−0.064	(0.100)
Full-time workers only					−0.118	(0.093)	−0.087	(0.098)	−0.118	(0.093)
Controls for hours worked					−0.098	(0.081)	−0.059	(0.098)	−0.03	(0.081)
Controls for occupation or industry							−0.075	(0.076)		
Controls for children present							−0.134	(0.103)		
Heckman model									−0.164*	(0.084)
Decomposition model									0.045	(0.124)
Constant	0.068	(0.077)	0.070	(0.101)	0.111	(0.101)	0.212*	(0.121)	0.111	(0.118)
N	29		29		29		29		29	
Adjusted R^2	−0.27		−0.42		−0.52		−0.41		−0.3	

Note: Dependent variable is percentage difference in earnings for gays and heterosexuals. Estimates from random effects maximum likelihood meta-regressions. Robust standard errors in parentheses. Reference categories are U.S. Census data 1990, couples status measure, and OLS model.

*$p < 0.10$.

Meta-regressions of studies of women. Table 4 shows the results for the meta-regressions using estimates for women. None of the models has a positive adjusted R^2 term and only one of the coefficients in any of the models was statistically significant. Although many of the coefficients are as large as those in the models for men, they are imprecisely measured, perhaps because of fewer estimates (twenty-nine versus thirty-four for men) and less variation in study characteristics.[24]

Estimates from Model 1 shows that studies using the 1990 U.S. Census average a 6.8 percent advantage for lesbians over heterosexual women (shown by the constant term), but this is not statistically different than zero. Estimates from studies using other datasets did not differ significantly from the 1990 census estimates, though the GSS studies had a relatively large coefficient of more than 8 percentage points.

Models 2, 3, 4, and 5 show that, for women, the year of outcome and gay sample size had very small, insignificant coefficients. The coefficient on the indicator of U.S. studies was positive and large in Models 4 and 5 after controlling for work intensity, but not significant. The U.S. indicator was also significant in the model that used all estimates in studies, but only when not accounting for the interdependence by dataset (Appendix C, Model 5). The indicators of the type of sexual orientation measure were not significant in any models. The sexual behavior measure had sizeable positive coefficients in several of the models, but this did not hold up in the models that used all estimates in studies (Appendix C).

Models 3, 4, and 5 include variables that account for the amount of time worked; again, these are not statistically significant, although all of the coefficients suggest that these controls result in smaller estimated premiums for lesbians by up to 12 percentage points. Using an hourly earnings measure and controlling for the number of hours worked were significant in the model with all estimates but no interdependence adjustment (Appendix C, Model 5). In results not shown, I limited the model to the twenty-one studies that used U.S. data and the coefficients on each of the work intensity variables was statistically significant and showed that controlling for those decreased the size of the lesbian premium.

Indicators for whether the study controlled for occupation or industry and for the presence of children (Model 4) were not significant, although the coefficient on the children control was large in size.

Model 5 adds indicators of whether the study controlled whether it used Heckman selection or decomposition methods. Only the Heckman method was

[24] For example, 21 percent of estimates for women use the 2000 U.S. Census data, compared to 9 percent for men.

statistically significant and that was only in models that dropped the controls for occupation, industry, or presence of children. In those models, studies that used a Heckman correction had lesbian premiums that were about 16 percentage points smaller. The health control was not significant and when added did not result in large changes in the size or significance of other coefficients (not shown).

In sum, the meta-regressions for women provide little conclusive evidence of what drives the variation in the estimated sexual orientation differences given the dearth of statistical significance. The size of the point estimates suggests that having U.S. data, the sexual behavior measure, controls for work intensity, and use of a Heckman selection model could all play a role.

Discussion and Conclusions

The thirty-one studies of sexual orientation show an average earnings penalty of 11 percent for gay men and an earnings premium of 9 percent for lesbians, but also a wide range of estimates. The meta-regression results from this small but growing literature show strong relationships between study characteristics and the estimated effects of sexual orientation for men, but little clarity for women.

Here I want to return to discussion of the roles of discrimination, human capital, and intrahousehold factors and the lingering questions of whether the conflicting findings for gay men and lesbians are consistent with discrimination and whether human capital or intrahousehold influences can explain the lesbian earnings premium. I draw on the results of the meta-analysis and bring in supporting evidence from individual studies, including some not eligible for the meta-analysis.

Discrimination. The meta-analysis shows evidence consistent with possible discrimination—an earnings penalty—for gay men but not for lesbians. Almost all studies, using a wide variety of methods, have found an earnings penalty for gay men; only a few studies found an earnings penalty for lesbians and most found a significant earnings premium even after controlling for many observable characteristics. There is little here to suggest earnings discrimination for lesbians unless studies have not accounted for some large off-setting positive influences on lesbians' earnings.

The conflicting findings for gay men and women raise the question of whether sexual orientation discrimination is the explanation for the results for gay men. Is there evidence that would support the hypothesis that discrimination affects earnings for gay men, but not for lesbians? Among heterosexual American men, attitudes toward gay men are less positive than those toward

lesbians and, if men are more likely than women to hire, fire, or promote workers, that could result in labor market discrimination against gay men but not women (Herek 2000). Consistent with the hypothesis of discrimination for gay men, jobs in the private sector show larger earnings penalties for gay men than in more highly regulated government sector jobs (Klawitter 2011). Again, the pattern for lesbians is different—significant earnings premiums in the private and nonprofit sectors, none in government employment.

A few studies included here have tried to assess the importance of discrimination through a proxy for whether people were openly gay. These studies used current marital status and a sexual behavior measure to assess the earnings influence of openly living as gay ("unmasked") versus having same-sex sexual partners but being married to an opposite sex partner ("masked") (Blandford 2003; Cushing-Daniels & Yeung 2009). These studies have found greater earnings penalties for gay men who were unmarried than those who were married, which is suggestive of potential discrimination for those who are more visibly gay.[25] The results for lesbians were mixed, but the most recent study found greater earnings premiums for heterosexually married lesbians than for unmarried lesbians (consistent with the discrimination story) (Cushing-Daniels and Yeung 2009: 173).

Finally, if discrimination is driving earnings differences, especially for gay men, then policies designed to limit discrimination could be effective. Two studies in the sample found evidence that state antidiscrimination policies significantly increased earnings for gay men: Baumle and Poston (2011) found a significant impact of state antidiscrimination policies on annual earnings and Klawitter (2011) found evidence that state (but not local) antidiscrimination policies were associated with greater weeks worked for gay men especially in private-sector jobs. However, Klawitter did not find significant impacts on hourly earnings or hours worked per week for gay men and that weakens the support for the discrimination explanation.[26] Neither study found similar evidence of policy impacts for lesbians.

On the whole, evidence from multiple sources suggests that discrimination might be worse for gay men than for lesbians and that might explain the divergence in earnings effects.

Human capital. Many authors have suggested that the explanation for the lesbian advantage lies in greater accumulation of human capital, especially

[25] The studies found the rates of being "masked" (married) were roughly similar for men and women (between 20 and 26 percent of gay or bisexual men or women).

[26] Antidiscrimination policies may be more effective in hiring or firing than in internal promotion, work time, or pay policies and that could explain the pattern for men.

work experience. Both gay men and lesbians have more education, on average, than do heterosexuals (Antecol, Jong, and Steinberger 2008; Black et al. 2000; Carpenter 2004; Elmslie and Tebaldi, 2007; Zavodny 2007) and results from decomposition studies show that education contributes the most to earnings differences by sexual orientation (Antecol, Jong, and Steinberger 2008; Daneshvary, Waddoups, and Wimmer 2007). All studies in the meta-regression include measures of education and age—both important albeit imperfect proxies for human capital. But as the meta-regression shows, these controls do not eliminate the estimated lesbian earnings premium although they may contribute to its insignificance.

One explanation for the lesbian advantage might be differences in work experience, not captured even in studies including measures of "potential experience." Unfortunately, none of the major datasets has information on actual work experience and the proxy normally used for experience (age less years of education minus 5) will likely miss key differences in work history for lesbians and heterosexual women given patterns of work force attachment. The use of potential rather than actual experience could lead to an underestimate of the role of experience in explaining sexual orientation differences as it has with gender differences (Regan and Oaxaca 2008; Weichselbaumer and Winter-Ebmer 2005) and that might partly explain the lesbian earnings premiums.

Potential experience pays off at a higher rate for lesbians than for heterosexual women, perhaps because of the greater actual experience for lesbians at each level of potential experience (Daneshvary, Waddoups, and Wimmer 2007). Also, Daneshvary, Waddoups, and Wimmer (2009) compared lesbians who had been previously married with those never married and found greater lesbian wage premiums for those never married, consistent with differences in human capital accumulation because of differing expectations of future work. Expectations about future household roles could have suppressed both experience and the quality of human capital for previously married lesbians relative to those who presumably expected to be with female partners in the future.

On the whole, human capital differences, in particular the intensity of work experience, might at least partly explain the lesbian earnings premiums. This issue could be examined in future datasets if actual work experience data were available along with information on sexual orientation.

Gender and intrahousehold decisions. The earnings premiums for lesbians and many of the patterns in the meta-analysis for both men and women point to the importance of gender and intrahousehold decision making in explaining sexual orientation differences.

The meta-regressions show that controlling for work effort and selection into participation may offset the negative earnings penalty for gay men and the

earnings premium for lesbians. This is consistent with studies showing fewer hours and weeks of work for gay men and greater work effort for lesbians relative to heterosexuals (Antecol and Steinberger 2011; Baumle and Poston 2011; Tebaldi and Elmslie 2006). As noted above, work intensity could be lower because discriminatory actions create fewer opportunities for jobs or work hours, but work hours are also a critical outcome in intrahousehold decisions about income and home work (especially child rearing). Again, the pattern for gay men is more consistent with an effect of discrimination on work availability than that for lesbians.

The level of expected earnings from a same-sex partner could account for these gender patterns and, unlike the differential discrimination hypothesis, would explain the lesbian premium as well as the gay male penalty. Lesbians expecting their own earnings not to be balanced by higher earnings from a male partner might work more, work in more intensive jobs, and might invest in more human capital (in ways not captured by data). In contrast, gay men might worry less about their own earnings and human capital because of expectations of earnings from a male partner.

In addition to the influence of potential earnings from a partner, cultural differences or norms might affect how gay and lesbian families organize home life. Same-sex couples are less likely to have one partner work only in the labor market and one only in the home, and more likely to have both partners in the labor market even when children are present (Antecol and Steinberger 2009; Black et al. 2007). Same-sex couples are also less likely to be raising children than are different-sex couples (Black et al. 2007); however, controlling for child rearing does not explain the largest differences across studies (as the meta-regression shows) and earnings premiums are found for lesbians with and without children (Jepsen 2007). A few studies that examine household decision making have also suggested that same-sex couples may make decisions differently than do married and cohabitating different-sex couples (Blumstein and Schwartz 1983; Burns, Burgoyne, and Clarke 2008; Klawitter 2008; Kurdek 2005). However, there is also evidence of specialization within same-sex couples, especially among those with children at home (Antecol and Steinberger 2011; Carrington 2000).

These intrahousehold patterns could lead to estimates of larger sexual orientation effects using couples than using all adults. In one of the few studies able to assess that difference, Carpenter (2008a) did find much larger gaps for samples of couples than for all individuals in Canadian data. Here, the meta-regression did show some evidence that studies using couple status measures for sexual orientation (rather than sexual identity) found larger earnings penalties for gay men but not for lesbians. Selection into couple membership could also influence earnings gaps as gay men and lesbians in couples are more

likely to have characteristics associated with higher incomes than are single gay men and lesbians (Carpenter and Gates 2008). Datasets with data that allowed identification of both partnered and single gays and lesbians could allow us to better understand the influence of selection into couples on earnings.

Implications for researchers. The research on the influence of sexual orientation on labor market outcomes has blossomed over the last 15 years and is likely to become more extensive and richer as new datasets collect information on sexual orientation. The results here suggest a number of important lessons for researchers looking at earnings. Most importantly, datasets with new combinations of earnings and sexual orientation measures, along with information on human capital and demographics, will allow us to better understand the independent contributions of these elements to the size of estimated sexual orientation differences.

The meta-regression results show the importance of a researcher's ability to control for factors critical to parceling out contributions to earnings differences such as information on work intensity, occupation or industry, and household membership. Controlling for work intensity by limiting samples to full-time workers, controlling for hours, or using Heckman selection models does affect the size of sexual orientation differences. Having data on actual work experience and on desired work hours would help researchers to control for human capital and work choice. Future work to untangle the knots of past work experience, intrahousehold choices of work hours, and possible employer actions limiting hours will aid in distinguishing between discrimination and other influences on earnings.

New data collection is critical to our ability to further understand sexual orientation among single and partnered adults—few datasets have information on the effects of selection into partnership, the impact of having multiple potential household earners, and possible differential treatment by employers ("marriage bonus"); these factors are all confounded in studies of couples. Also, more generally the impact of using alternative measures of sexual orientation is not well understood given our inability to compare the constructs within one dataset. We need datasets sizable enough to support econometric analysis that include information on partnership, sexual identity, and sexual behavior. New strategies such as oversampling of sexual minority populations or regular special modules asking about sexual orientation would go a long way to getting better estimates of earnings differences in national datasets other than the U.S. Census (Gates 2010).

The rapid maturing of the economics literature addressing issues of sexual orientation has quickly created a more nuanced understanding of the

complexities of differences in labor market outcomes as well as a broader set of topics. How much differences are driven by discrimination is still debatable, but the critical importance of intrahousehold influences has been thrown into relief.

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APPENDIX A

Studies in Meta-Analysis

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APPENDIX B

DESCRIPTIVE STATISTICS FOR ESTIMATES OF MEN AND WOMEN

	Men				Women			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Earnings difference	−13%	0.09	−0.30	0.00	10%	0.15	−0.25	0.43
U.S. Census data 1990	0.12	0.33	0.00	1.00	0.07	0.26	0.00	1.00
U.S. Census data 2000	0.09	0.29	0.00	1.00	0.21	0.41	0.00	1.00
General Social Survey data	0.32	0.47	0.00	1.00	0.28	0.45	0.00	1.00
Other U.S. study	0.12	0.33	0.00	1.00	0.10	0.31	0.00	1.00
Non-U.S. study	0.35	0.49	0.00	1.00	0.34	0.48	0.00	1.00
Years since 1989 (midpoint)	8.38	5.74	0.00	19.00	8.76	4.93	0.00	18.00
Sample size (1000s)	108.31	351.58	0.15	1898.66	110.21	314.56	0.72	1515.21
Gay sample size (1000s)	2.08	4.97	0.02	20.69	3.04	6.11	0.02	21.80
U.S. study indicator	0.65	0.49	0	1	0.66	0.48	0	1
Sexual behavior measure	0.38	0.49	0	1	0.31	0.47	0	1
Sexual identity measure	0.21	0.41	0	1	0.24	0.44	0	1
Couple status measure	0.35	0.49	0	1	0.45	0.51	0	1
Hourly earnings measure (not annual or monthly)	0.68	0.47	0	1	0.76	0.44	0	1
Full-time workers only	0.44	0.50	0	1	0.48	0.51	0	1
Controls for hours worked	0.32	0.47	0	1	0.31	0.47	0	1
Controls for occupation or industry	0.76	0.43	0	1	0.83	0.38	0	1
Controls for children in household	0.47	0.51	0	1	0.52	0.51	0	1
Heckman selection correction	0.15	0.36	0	1	0.28	0.45	0	1
Decomposition analysis	0.18	0.39	0	1	0.10	0.31	0	1
Controls for health or disability	0.06	0.24	0	1	0.24	0.44	0	1
Observations		34				29		

Note: Unweighted descriptive statistics.

APPENDIX C

Sensitivity Analyses Using Correlated Error Terms from Same Dataset and Multiple Estimates for Each Study

	Men						Women					
	Single Estimate Depend. Errors (1)		All Estimates (2)		All Estimates Depend. Errors (3)		Single Estimate Depend. Errors (4)		All Estimates (5)		All Estimates Depend. Errors (6)	
Year of outcome (midpoint less 1989)	0.007**	(0.002)	−0.002	(0.001)	0.005	(0.003)	0.001	(0.003)	0.0005	(0.004)	0.001	(0.004)
Sample size/1000 (gay)	0.001	(0.002)	0.005***	(0.002)	−0.001	(0.003)	0.0001	(0.005)	0.001	(0.002)	0.0006	(0.009)
U.S. data	−0.118**	(0.039)	−0.080***	(0.021)	−0.088	(0.064)	0.046	(0.073)	0.059**	(0.028)	0.059	(0.093)
Sexual behavior measure	0.001	(0.022)	0.008	(0.016)	0.004	(0.031)	0.111	(0.073)	−0.005	(0.034)	−0.008	(0.075)
Sexual identity measure	0.045	(0.025)	0.075***	(0.012)	0.078**	(0.028)	0.041	(0.080)	0.031	(0.022)	0.032	(0.087)
Hourly earnings measure (not annual or monthly)	0.042	(0.022)	0.034***	(0.012)	0.03	(0.018)	−0.001	(0.064)	−0.071***	(0.014)	−0.071	(0.033)
Full-time workers only	0.125***	(0.024)	0.064***	(0.019)	0.094*	(0.043)	−0.107	(0.076)	0.011	(0.022)	0.010	(0.037)
Controls for hours worked	0.075*	(0.031)	0.063***	(0.013)	0.081*	(0.035)	−0.085	(0.078)	−0.046*	(0.025)	−0.045	(0.063)
Constant	−0.175***	(0.032)	−0.113***	(0.018)	−0.133**	(0.032)	0.100	(0.091)	0.051	(0.051)	0.050	(0.140)
N	34		250		250		29		245		245	
Adjusted R^2	Not avail.		0.14		Not avail.		Not avail.		0.26		Not avail.	

Note: Dependent variable is percentage difference in earnings for gays and heterosexuals. Estimates from random effects maximum likelihood meta-regressions. Models 1, 3, 4, and 5 account for interdependence of error terms from same dataset with hierarchical models in ROBUMETA (Hedges, Tipton, and Johnson 2010). Models 2, 3, 5, and 6 include all estimates in studies. Reference categories are U.S. Census data 1990, couples status measure, and OLS model.

*$p < 0.10$, **$p < .05$, ***$p < .01$.