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Narrowing the Gap: New Evidence on Earnings Differentials Based on Sexual Orientation

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NARROWING THE GAP:
NEW EVIDENCE OF EARNINGS DIFFERENTIALS BASED ON SEXUAL ORIENTATION

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Using General Social Survey data from 1989 to 2008, I estimate earnings differentials between heterosexual and queer workers. When following the model specified in earlier studies, I find that queer men earn between 11.6% less than their heterosexual counterparts and that queer women earned approximately 11.6% more than their heterosexual counterparts. When respecifying the model to account for the gender composition of individuals' occupations, I find that queer men's earnings are not statistically different from straight men's earnings, and the earnings advantage enjoyed by queer women drops marginally, to 10.5%. This addition significantly improves upon the explanatory power of the existing model. These findings undermine earlier results, indicating that earnings differentials are not as drastic as has been posited. Much of the differential found in earlier studies can be attributed to occupational characteristics rather than labor market discrimination.

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I. INTRODUCTION

Given the controversy over the federal Employment Non-Discrimination Act (ENDA), economists have been charged with the task of examining earnings differentials between heterosexual and queer workers.[1] Opponents of ENDA and similar legislation often perpetuate the myth of gay affluence while proponents cite supposedly rampant cases of labor market discrimination against queer people. More objective studies have focused on earnings differentials between straight people and queer people. In general, econometric analyses have found that queer men earn significantly less than straight men, while queer women earn significantly more than straight women. In my work I will build off these studies. I first examine whether or not queer people face systematic employment discrimination in the form of lower earnings than their heterosexual counterparts and whether or not these earnings differentials have changed over time. I will also investigate evidence suggesting that earnings differentials between straight people and queer people are attributable to certain characteristics only indirectly related to sexual orientation.

This paper examines earnings differentials between heterosexual and queer workers using General Social Survey (GSS) data. The National Opinion Research Center has conducted this national, random sample survey since 1972, collecting data on income, occupation, demographic and household characteristics, and sexual behavior.

After a brief discussion of the existing literature on the earnings of queer people, I will replicate John Blandford's work from 2003, which tests for earnings differentials based on sexual orientation between 1989 and 1996. Then I will rerun a very similar regression, adding

[1] Though earlier studies have generally marked non-heterosexual individuals as "lesbian/gay/bisexual," academics have largely shifted towards using "queer" as an umbrella term for a wide variety of non-heterosexual orientations.

GSS data available for the years up to 2008 and a time trend variable to Blandford's model.[2] Controlling for human capital investments, demographics, and occupation, I find that queer men earn 11.6% less than straight men. Using the same specification, I find that queer women earn more than their heterosexual counterparts by the same margin. These results roughly match those from earlier studies in sign, though they are slightly smaller in magnitude.

In revising the widely used model, I add a new variable to control for the gender composition of an individual's occupation. There is well-established evidence to suggest that higher representation of women in an occupation is associated with lower earnings (Fuchs, 1971; Sorenson, 1989; Gerhart and Cheik, 1991). Other evidence suggests that queer women are overrepresented in fields typically dominated by men and queer men are overrepresented in fields typically dominated by women (Dunne 1997). As such, inadequate controls for occupational characteristics would force the queer variable to proxy for some of the differences related to occupation. We would expect that this effect would increase the coefficient on *queer* for women and decrease the coefficient on *queer* for men. This theory holds true in the data. When adding the variable for representation of women in an occupation, I find that the differential between straight and queer people's earnings drops for both men and women. For men, the impact of sexual orientation is no longer significant at the 10% level. However, my finding that queer women earn 10.5% more than straight women is still barely significant at the 10% level.

I also conduct a variety of secondary regressions that add several interaction terms to the model. First, I interact the indicator for non-heterosexuality with the time trend and find that the queer variable is no longer significant, either individually or jointly with the interaction term. I

[2] The GSS was conducted only every other year since 1994. As such, my complete sample includes data from 1989, 1990, 1991, 1993, 1994, 1996, 1998, 2000, 2002, 2004, 2006, and 2008.

also add interaction terms between the race variables and the time trend, finding that people of color earned significantly less in later years than they did in earlier years of the sample. Finally, I interact *queer* with the occupational gender composition variable. In this regression, the results suggest that compared to their heterosexual counterparts, queer women are disproportionately negatively affected by higher percentages of women in their occupations.

In the appendices, I perform further regressions to control for changes in the earnings differential over time. To control for potential effects of business cycles, rather than an overarching linear time trend, I add dummy variables for each year in Appendix A. Adding the dummy variables significantly improves the explanatory power of the model. The coefficients on the variables do not change significantly, though many of them become slightly more significant. Then I use these dummy variables and interaction terms for each year multiplied by the queer variable to estimate the effect of being queer in a specific year. This model does not significantly improve on the model that uses the dummy variables for each year. Further, very little new analysis can be gleaned from the addition of these interaction terms.

In Appendix B, I allow for a structural break in the model. For both men and women, I find that the coefficient on the queer variable was of a higher magnitude and more significant during the first half of the sample than during the second half. This may be indicative of the effectiveness of anti-discrimination legislation that has been enacted recently or more tolerant views of non-heterosexuality. The same is true of the coefficient on the variable accounting for representation of women in an occupation. That is, the earnings premium associated with employment in a male-dominated field relative to earnings in female-dominated fields fell over the period 1989 to 2008. These regressions also returned some interesting results on the occupation and race variables. Most notably, I found that people of color experienced a larger earnings disadvantage relative to white people between 1998 and 2008 than they did between 1989 and 1996.

After examining my results in more depth, I will offer a brief discussion of the importance of these findings. Finally, I will provide a short conclusion addressing the possibilities for further research and how my work contributes to the existing literature.

II. PAST EVIDENCE ON THE EARNINGS OF QUEER PEOPLE

In 1995, M.V. Lee Badgett applied the econometric techniques used to examine earnings differentials based on sex and race to an empirical study of sexual orientation discrimination. She used data from the General Social Surveys of 1989 to 1991 to conduct a basic OLS regression on earnings. Based on reported sexual behavior since age 18, Badgett used four different definitions of non-heterosexuality. Depending on the definition used, Badgett found that gay and bisexual male workers earn 11% to 27% less than comparable heterosexual male workers when controlling for potential work experience[3], education, occupation,[4] marital status, and region of residence. Her findings for the earnings differentials between lesbian and bisexual women and heterosexual women were somewhat inconclusive. Badgett found that lesbian and bisexual women earn 13% to 43% less than heterosexual women; [5] however, the coefficients on the *Lesbian/Gay/Bisexual (L/G/B)* variable were not consistently significant. The coefficient on

[3] Potential experience was calculated as age minus education minus 5.

[4] Badgett (1995) used the broad occupational categories "manager," "professional/technical," "clerical/sales," and "craft/operative."

[5] These results differ significantly from those found in other studies. Black, *et al.* (2003) provides a thorough discussion of this inconsistency. In particular, they note anomalous data from Badgett's smaller sample; in particular, Badgett's data suggests that queer women earned significantly less than straight women over the period 1989 to 1991 ($15,056 compared to $18,341). In the larger sample used by other studies, queer women have been found to earn significantly more than straight women on average.

L/G/B was significantly different from zero when women were marked as lesbian, gay, or bisexual if they had had (1) more than one same-sex sex partner, or (2) more than one same-sex partner or at least as many same-sex sex partners as opposite-sex sex partners.

In 2003, Dan Black, Hoda Makar, Seth Sanders, and Lowell Taylor replicated Badgett (1995) and added data from the General Social Surveys through 1996. They used Badgett's definitions of lesbian/gay/bisexual along with two definitions based on more recent sexual behavior. The authors controlled for education, marital status, race,[6] potential experience,[7] occupation,[8] region, and whether the individual lives in a large Standard Metropolitan Statistical Area. Black, *et al.* found that queer men earn 14% to 16% less than comparable heterosexual men, and queer women earn 20% to 34% more than comparable heterosexual women. Their findings are consistent with two theoretical models they discuss, specifically Gary Becker's work on human capital accumulation based on specialization within households and Claudia Goldin's work on paternalistic discrimination against marriageable women.

Also in 2003, John Blandford published "The Nexus of Sexual Orientation and Gender in the Determination of Earnings." Like Black, *et al.*, Blandford used data from the General Social Surveys of 1989 through 1996, though he used different definitions of lesbian/gay/bisexual. Blandford defined an individual as L/G/B if (1) they had had at least one same-sex sex partner in the last year or (2) they had had no sex partners in the last year but had had at least one same-sex sex partner in the last five years. He further defined individuals as "masked" if they fit the criteria to be marked as L/G/B but were also married. To measure the quality of his definition, Blandford used data from the National Health and Social Life Survey (NHSLS), which collects information on both behavior and self-identified sexual orientation, to assess how accurately

[6] Black, *et al.* differentiated between white and non-white workers.
[7] Potential experience was calculated as age minus education minus 6.
[8] Black, *et al.* used three-digit Census occupation codes.

different behavior-based definitions reflect self-reported sexual orientation. Comparing the predictive performance of his sexual orientation proxies to those used by Badgett (1995) and Black, *et al.* (2003), Blandford found that his definition much more accurately reflects self-identification.

Blandford found that queer men earn 30% to 32% less than comparable heterosexual men, and queer women earn 17% to 23% more than comparable heterosexual women. He accounted for marital status, race,[9] residence in a large SMSA, geographic region, occupation,[10] education, and potential experience.[11] When using more narrowly defined occupation classifications, Blandford found that the earnings differential decreases. This finding emphasizes the importance of controls for occupation in the earnings regression. Without these controls, the queer variable is forced to act as a proxy for otherwise disregarded heterogeneity between queer people and heterosexual people.[12]

Berg and Lien (2002) begin with similar data and a similar research agenda as Black, *et al.* (2003) and Blandford (2003), though their econometric approach is different. Marking individuals as queer if they have had at least one same-sex sex partner in the last five years, Berg and Lien use a 21-category discrete-dependent-variables model to analyze earnings differentials associated with sexual orientation. The 21 income ranges reported on the GSS are used as dependent dummy variables. Berg and Lien found that non-heterosexual men earned 22% less

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[9] Blandford distinguished between white, black, and other race, using white as the default race.
[10] Blandford used one- and two-digit occupation codes.
[11] Potential experience was calculated as age minus education minus five.
[12] Interestingly, Blandford also found that controlling for sexual orientation reduces the significance of marital status in the determination of earnings. Most of the existing literature has found a strong earnings effect of marriage, increasing men's earnings and decreasing women's earnings. Blandford's findings suggest that in these regressions, marital status has been forced to proxy for sexual orientation. Many of the non-married men found to be earning less than married men were likely queer men, earning less than heterosexual men due to factors related to their sexual orientation. Similarly, many of the non-married women found to be earning more than married women were likely queer, and thus were likely to earn more than the straight, married women.

than comparable straight men, and non-heterosexual women earn 30% more than comparable straight women.

Other studies analyzing the earnings effects of sexual orientation rely primarily on Census Public Use Microdata Sample (PUMS). While the PUMS data has its benefits, using it for work on sexual orientation is problematic, as the Census does not collect information on self-reported sexual orientation or sexual behavior. Instead, economists use information on cohabitation with an "unmarried partner." Unfortunately, this returns a biased sample, only counting as queer those who cohabitate with their partner. Nonetheless, these studies generally return results that are similar to those discussed above.

Using Census data, Clain and Leppel (2001) found that men living with male partners tend to earn about 22% less than men not living with male partners, and that women living with female partners tend to earn significantly more than women who do not live with female partners.[13] However, the magnitude of the differential changes drastically depending on certain factors such as age, parenthood, and region of residence. In a footnote, they supported Badgett's theory that errors in measuring disclosure of non-heterosexual orientations at work result in a bias against finding evidence of discrimination based on sexual orientation. We must keep this effect in mind as we attempt to estimate labor market discrimination against queer people who may not have disclosed their sexual orientation to employers and coworkers.

Other studies suggest that detailed controls for occupational characteristics cannot be omitted from our model of earnings differentials based on sexual orientation. In a labor market experiment on employers' responses to queer and straight female job applicants, Weichselbaumer (2003) found a strong negative effect related to non-heterosexual orientation.

[13] Clain and Leppel differentiated between men living with male partners, men living with female partners, and men not living with a partner. They maintain the same categorizations for women.

She posits that part of the difference between her study and those using survey data is insufficient controls for occupations in earlier studies. Weichselbaumer focused on only two specific occupation and found strong evidence of discrimination against queer women, whereas other studies, which focused on a larger array of occupations, have found strong earnings advantages for queer women. This suggests that there is some unobserved heterogeneity in occupational sorting in other studies. Dunne (1997) further suggests that queer women are overrepresented in male-dominated fields. Fuchs (1971), Sorenson (1989), Gerhart and Cheik (1991), and other studies agree: workers in occupations dominated by men tend to have higher earnings. Insofar as queer women are overrepresented in male-dominated fields, not adequately controlling for gender composition of individuals' occupations would overestimate queer women's earnings as significantly greater than straight women's earnings. I posit that some of the earnings premium enjoyed by queer women is due to occupational sorting.

III. MARKING RESPONDENTS AS QUEER

Blandford (2003) refined the definition used to mark individuals as queer, bringing proxies based on behavior into closer alignment with proxies based on self-identification. Using the National Health and Social Life Survey, which connects information on past sexual behavior to self-reported sexual orientation, Blandford measured the performance of proxies based on behavior since age 18, like those used by Badgett (1995) and Black, *et al.* (2003), against the performance of proxies based on recent behavior and marital status. Defining as L/G/B those who have had (1) at least one same-sex sex partner in the past year, or (2) no sex partners in the last year and at least one same-sex sex partner in the last five years, Blandford's method correctly identified individuals as queer 91.9% of the time, compared to Badgett's method correctly identifying just 75.7% of queer individuals. Furthermore, his method incorrectly identifies fewer queer people as straight (false negatives) and fewer straight people as queer (false positives).

As such, I will adopt his method and adjust it to fit the larger sample. Between 2004 and 2008, same-sex marriage was legalized in six states. As such, Blandford's definition of "masked" individuals as those who display queer sexual behavior but are married is problematic when using data collected after 2003. Sampling only the years 2004 to 2008, this affects 16 individuals who would otherwise be marked as "masked" but are in fact potentially married to people of the same sex. Unfortunately, there is no way to establish if a married person who is marked as behaviorally queer is living in one of those six states, potentially married to someone of the same sex. In 2008, GSS investigators began asking respondents about the sex of their spouse, civil union partner, or domestic partner, but there is no data about this for the majority of the sample.

In 2008, GSS respondents were asked to identify their sexual orientation as (1) "gay, lesbian, or homosexual," (2) "bisexual," or (3) "heterosexual or straight." Comparing their self-reported sexual orientation to the queer variable I constructed, I find that the behavior proxy is a much better predictor of self-identification for men than it is for women. Table 1 on page 31 displays the results of this comparison. For men, only one of the individuals marked as queer in 2008 self-identified as heterosexual. For women, however, five of the individuals marked as queer in 2008 self-identified as heterosexual. My data suggest that women who have had at least one female sex partner in the last year are more likely to consider themselves straight than men who have had at least one male sex partner in the last year. This finding is consistent with academic and societal understandings of female sexuality.

IV. CHARACTERISTICS OF THE SAMPLE

The sample means for the variables are reported in Table 2 on page 32. Of over 10,000 observations, two percent of these were marked as behaviorally queer men and slightly less than two percent of these were marked as behaviorally queer women. The mean annual earnings of all respondents was almost $33,000, in constant 2000 dollars, with straight men earning the most on average and straight women earning the least on average. In general, men earned more than women, regardless of sexual orientation.

Based on the application of Becker's model of human capital investment, we would expect straight men to invest the most in human capital; however, in this sample, queer people invested the most in education, achieving on average 14.5 years of education. Straight people, both men and women, earned significantly less education on average, or 14.0 years. Heterosexual men were most likely to have achieved a high school diploma as their highest degree, followed by queer men, heterosexual women, and queer women. Queer people were the most likely to have earned a bachelor's degree or a graduate degree, with queer men having more advanced degrees than queer women on average. Queer individuals' higher levels of education should, *ceteris paribus,* increase their earnings relative to those of straight people.

The heterosexual women included in the sample tended to be significantly older than the queer women, and thus have more potential work experience. The men, both straight and queer, were about 40 years old and had about 21 years of potential experience. Straight men were more likely to be white than the other groups, a trait that would tend to push up straight men's earnings. Compared to the other groups, queer men were the most likely to be neither white nor black, though this difference is not statistically significant. Straight women were more likely than other respondents to be black and to work part-time, factors that would tend to push down their earnings, though these differences were not statistically significant. Queer women worked significantly more hours in the last week than straight women, which would tend to increase their relative earnings. Straight men and queer men worked roughly the same number of hours per week.

Queer men were much more likely than other respondents to live in large metropolitan areas and the Northeast. Queer women were significantly more likely to live in the West. Living in these areas is generally shown to be correlated with higher earnings. Based on these factors alone, we would expect queer people to earn more than straight people.

Queer women and straight men had high rates of union membership, probably due to their significantly higher rates of employment in "blue collar" occupations, such as precision production, craft, and repair occupations. Queer women were also more likely than straight women to be employed in high-wage "white collar" occupations, such as professional specialty positions, though this difference is not statistically significant. Straight women were more likely than men and queer women to be employed in technical, sales, and administrative support occupations. Queer men had the highest rates of employment in managerial and professional specialty occupations, occupations which are typically associated with higher earnings. Their overrepresentation in these fields is inconsistent with the finding that queer men earn less than straight men, who have significantly lower representation in these high-wage occupations.

Unsurprisingly, heterosexuals were significantly more likely to be married than queer people. Though straight men are significantly more likely to have had been married than queer men, there is no significant difference between straight and queer women's rates of divorce, separation, and widowhood. Straight men were significantly more likely to be married than straight women and straight women were significantly more likely to be divorced, separated, or widowed than straight men. [14] Straight women had the most children on average, followed by straight men and then queer women, with queer men having the lowest average number of children.

V. REPLICATION OF BLANDFORD (2003)

In order to replicate John Blandford's work from his 2003 paper, I began by recreating his income variable. Blandford used Current Population Survey (CPS) data on median earnings for workers of each sex, race, and year. He then calculated a factor comparing the earnings of each subgroup of race, sex, and year, and then multiplied this factor by the median of the income ranges provided in the GSS data. This method for calculating imputed values is intended to maintain variance within the income range categories. If we had no method for maintaining variance, all workers within a single income range would be coded as having the same earnings. Instead, we can code some workers as earning slightly more than that and some workers as earning slightly less than the median of the income range. For example, in 2000, white men's median weekly earnings were $662 while black men's median weekly earnings were $510. If

[14] Though it seems strange that straight women have a much higher rate of divorce than straight men, this finding is consistent with Census data showing that women are more likely to be divorced than men.

they responded that they made between $10,000 and $12,499 in 2000, white men were coded as earning $12,386.01 and black men were coded as earning $11,927.74. Though this method ensures an effect of race and sex on earnings, it is necessary to maintain variance in the dependent variable.[15]

Generating the necessary orientation, occupation, and demographic variables was straightforward, but I was not able to cull the dataset to match Blandford's exactly. For the sample of male workers, Blandford had 3,039 observations, of which 2,566 were used in his regression: 1,828 married heterosexuals, 1,115 unmarried heterosexuals, 18 masked, and 78 openly queer. Compared to his 3,039, I had approximately 200 fewer observations on men. In my replicating, I had 2,859 observations, of which 1,753 were married heterosexuals, 1,040 were unmarried heterosexuals, 20 were masked, and 73 were openly queer. For the sample of female workers, Blandford had 2,959 observations, of which 2,064 were used in his regression: 1,567 married heterosexuals, 1,316 unmarried heterosexuals, 15 masked, and 61 open. I had a total of 2,641 observations on women, or approximately 600 more observations: 1,387 married heterosexual women, 1,180 unmarried heterosexual women, 16 masked women, and 58 openly queer women. The differences between our samples likely account for the differences between our findings. Tables 3 and 4, comparing the results from my replication to Blandford's original findings for male and female workers respectively, can be found on pages 33 and 34. Following Blandford's model, I estimate:

(1) $$y_i = \alpha + \beta x_i + \gamma z_i + \varepsilon_i,$$

[15] The author thanks John Blandford for his help in replicating this methodology.

where y_i is logged real annual income, x_i is a vector of sexual orientation and marital status indicators, and z_i is a vector of individual characteristics, including controls for race, education and potential experience, regional characteristics, and occupation.[16]

In general, my replication of Blandford's regression on male workers returned very similar results. All of the coefficients were of the same sign except for the dummy variable for farming, fishing, and forestry occupations, which had a very low t-statistic in both of our regressions. Most of the coefficients were significant at similar levels, though I did find that the coefficients on *black* and *other race* were significant at the 10% level while Blandford found they were not significant at any conventional level. Also, he found that living in a large metropolitan area was significant at the 1% level while I found it significant only at the 5% level. I also found that the dummy variable on operation, fabrication, and labor occupations was statistically significant at the 1% level while he found it insignificant at the 10% level. The adjusted R-square was similar in both of our regressions, with Blandford finding a slightly better fit using his sample.

In my replication of Blandford's regression on female workers, all of the significant coefficients were of the same sign, but there were major differences in magnitude and levels of significance. Most importantly, I found larger coefficients on the sexual orientation variables, and I found that the coefficients on *unmarried heterosexual* and *open queer* were significant at the 1% level. Blandford, on the other hand, found that the coefficient on *unmarried heterosexual* was insignificant at the 10% level and that on *open queer* was only significant at the 5% level. The coefficient I found on *unmarried heterosexual* was also five times larger than the coefficient from Blandford's regression, while my coefficient on *open queer* was one-third larger than Blandford's. The additional 600 observations in my sample are likely responsible for the

[16] In replicating Blandford, I used 2-digit Census occupation codes.

differences in the regression results. Nonetheless, I believe that my results are close enough to Blandford's to allow me to move forward, expanding on his work and specifying a new model.

VI. PRIMARY REGRESSIONS

My primary OLS regression begins by slightly modifying Blandford's specification and including additional GSS data. I pooled all cross-sections and maintained Blandford's method for imputing real annual income values using CPS data and the median of the GSS income range categories. Using CPS data, I found median weekly earnings for full-time workers based on sex, race, and the year the data was collected. Since information on Asians' earnings was not collected by the CPS before 2000, I used Hispanic and Latinos' earnings as a proxy for the earnings of "other race" individuals, which is how they are coded in the GSS data. Averaging all workers' earnings for each year, I divided each subgroup's earnings by the average earnings, and then multiplied this factor by the median of the GSS income ranges. To adjust for inflation, I used the consumer price index from the Historical Tables from the Office of Management and Budget's Budget for Fiscal Year 2009. Finally, I logged the real annual income of each individual.

In my specification, I also use the same variables for human capital and the dummy variables for queer, marital status, race, residence in a large metropolitan area, and occupation codes. As stated above, there is no way to be sure if a behaviorally queer person's spouse or domestic partner is of the same sex in the years after 2004, so the "masked" variable is excluded from the regression.[17]

I also added a trend variable to differentiate between the years during which the surveys were conducted. In using panel data with so many cross-sections, it is important to adjust the model to allow for differences between the cross-sections. Since previous studies used much shorter ranges of years, it was not worthwhile to do so. I performed the same regressions with and without the time trend, and found that adding the trend variable significantly increased the ability of the model to explain variance in the dependent variable.

I began by performing two sets of regressions each on male and female workers: one controlling for occupation using one-digit Census occupation codes and one using two-digit Census occupation codes. As occupational categories become more specific, the magnitudes of the coefficients on the queer variable drop. As such, I hypothesized that there is some systematic difference between the distribution of occupations that employ queer people and the distribution of occupations that employ straight people. By neglecting to control adequately for occupation, we would be forcing the queer variable to act as a proxy for occupation. Remembering that Weichselbaumer (2003) found greater labor market discrimination against queer women than other studies that used a wider range of occupations, we have further evidence that more specific controls for occupation reduce the earnings differentials found to be associated with non-heterosexual orientation. In this paper, I will only report findings from the regressions that used two-digit occupation codes.

Tables 5 and 6 on pages 35 and 36 display the results of the first sets of regressions. The first model to be estimated is:

[17] It is important to note that the masked variable was insignificant at the 10% level for both men and women in Blandford's regression, though this was likely due in part to the small sample size (20 men, 16 women).

(2) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i + \varepsilon_i,$$

where y_i is logged real annual income, x_i is a dummy variable for sexual orientation, and z_i is a vector of individual characteristics, including controls for race, education and potential experience, regional characteristics, occupational sorting, t_i is a trend variable of values 1 to 19 corresponding to the years during which GSS data was collected.

In the first model, my results are, in general, consistent with those in Badgett (1995), Black, *et al.* (2003), and Blandford (2003). Controlling for race, human capital, geographic region, and occupation, queer men earn 11.6%[18] less than their heterosexual counterparts. At the same time, queer women earn approximately 11.6% more than their heterosexual counterparts. The magnitudes of these differentials are somewhat smaller than those described in some of the existing literature, though the signs of the coefficients are the same.

In respecifying the existing model, I focused on the importance of sufficient controls for occupational sorting. Understanding that there is some systematic difference between the occupations employing straight people and queer people, I found it prudent to explore Dunne's (1997) observation that queer women are overrepresented in fields typically dominated by men. Workers in these fields typically earn higher wages than those employed in fields dominated by women. If we did not attempt to account for this effect, the queer variable would be forced proxy for some of the differences in occupation in the earnings regression. This would increase the magnitude of the coefficient on *queer* for women. This effect would provide evidence of a wage premium for queer women, though in reality some of the difference is due to occupational characteristics rather than sexual orientation. To the extent that queer men tend to be employed

[18] Where the percent difference in earnings, d, is calculated as the coefficient on queer, $\delta = \ln(1+d)$. This convention is necessary to account for the fact that the dependent variable, earnings, is logged.

in fields that are dominated by women, this effect would also provide evidence of an earnings disadvantage for queer men.

The data confirms this hypothesis. For men, being marked as queer is correlated with a higher percentage of women in the individual's occupation, as we can see from Table 7 on page 37. For women, being marked as queer is correlated with a lower percentage of women in the individual's occupation. Furthermore, for both men and women, mean logged real annual earnings increase as the percentage of women in an occupation falls. Graphs 1 and 2 on page 37 display this relationship. For men, the slope coefficient on the linear trend line is slightly more negative than it is for women, though not significantly so.

In order to control for this effect, I use PUMS data to create a variable that accounts for the representation of women in a specific occupation based on three-digit Census occupation codes. For the occupations that had multiple observations of the percentage of women, I interpolate the percent female of a specific year. Otherwise, I use the single observation that was available for all the years 1989 to 2008.

The second set of regressions adds the variable accounting for gender composition of individuals' occupations:

(3) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i + \varepsilon_i,$$

where z_i now includes the continuous variable for the percentage of women in an individual's occupation, in addition to the individual characteristics included in the first set of regressions.

In the second set of regressions, I find that employment in a female-dominated field has a larger negative effect for men than it does for women. For men, controlling for the percent of women in an individual's occupation reduces the significance of the coefficient on the queer variable to below any widely accepted level of significance. The other variables, including the dummies for occupation categories, remain significant and generally of the same sign and magnitude. An F-test finds that this model provides a significantly better explanation of variance than the original model, which did not include the percent female variable.

The results for women are similar. Controlling for percentage of women in an individual's occupation, I find that queer women earn approximately 10.5% more than straight women. The coefficient on *queer* is of a slightly smaller magnitude than those in the secondary regression, and only barely significant at the 10% level (p=0.097). The coefficients on other variables are of similar magnitudes to those in the primary regression, and are significant at similar levels. The magnitudes of the coefficients on the occupation dummy variables are slightly smaller, though not significantly so for most of them. For women, an F-test shows that the respecified model provides a significantly better explanation of variance than does the original model.

In developing our understanding of the earnings effects of sexual orientation, controls for occupational sorting are exceedingly important. I found that better occupational controls reduce the magnitudes of the coefficients on *queer*. That is, using more specific occupation codes reduces the advantage enjoyed by queer women and the disadvantage suffered by queer men. When I controlled for the gender composition of individuals' occupations, adding another digit of specificity in occupation categories does not change the significance of the coefficient on *queer* for men or women, and only slightly reduces the magnitude of this coefficient for men. I posit that in earlier studies, the combination of the occupation category variables and the queer variable were forced to act as proxies for the level of representation of women in an occupation.

VII. SECONDARY REGRESSIONS

Building off the model outlined in equation (3), I added a variety of interaction terms with the time trend. The first set of secondary regressions interacted the variables *queer* and *time*:

(4) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i, + \theta x_i t_i + \varepsilon_i,$$

where $x_i t_i$ is the interaction of the indicator for queer and the time trend. For men, this model significantly improves on that used in the primary regressions. For women, this model does not provide a significant improvement in explanatory power. *Queer* and *queer*time* are not jointly significant at any conventional levels for men or women.

I also added interaction terms between *black* and the time trend and *other race* and the time trend. I estimated the equation:

(5) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i, + \theta x_i t_i + \varphi r_i t_i + \varepsilon_i,$$

where $r_i t_i$ is a vector of interactions between the race indicators for black and other (non-white, non-black) race and the time trend. For both men and women, this model provides significantly better explanation of variance than both the primary regression model and the model using only the interaction between *queer* and *time*. The results from these regressions are reported in Tables 8 and 9 on page 38.

For both men and women, the coefficient on *queer* is greater in magnitude than in the primary regressions, though it is no longer significant at any conventional levels. Further, the coefficients on *queer* and *queer*time* are not jointly significant. For men, the coefficients on *time* are significantly greater than zero, whereas the coefficients on *time* were positive but not consistently significant for women. Also noteworthy are the coefficients on the interactions between the race variables and the time trend. The coefficients on *black*time* and *other* *race*time* are both significantly less than zero. The finding that non-white workers' earnings fell over the period 1989 to 2008 suggests that anti-discrimination legislation protecting people of color has become outdated and ineffectual since its adoption.[19]

In a final set of regressions, I added interaction terms between the variable for queer and the variable for gender composition of an individual's occupation.[20] I estimated the equation:

(6) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i, + \theta x_i t_i + \varphi r_i t_i + \lambda x_i p_i + \varepsilon_i,$$

where $x_i p_i$ is the interaction term between the queer indicator variable and the variable controlling for the percentage of women in an individual's occupation. The results of these regressions are displayed in Tables 8 and 9 on page 38. For women, adding this interaction significantly improves on the explanatory power of the model outlined in equation (4). Furthermore, *queer* and *queer*percent female* are jointly significant, as are *queer* and *queer*time*.

The magnitudes and levels of significance for the key variables of interest also change drastically for the female respondents when adding *queer*percent female*. The coefficient on *queer* becomes much larger and much more significant. Furthermore, the coefficient on *queer*percent female* is significantly less than zero. This finding suggests that compared to straight women, queer women are disproportionately negatively affected by higher proportions of women in their occupations.

Unlike for the regression on female workers, the addition of the *queer*percent female* term does not significantly improve upon the explanatory power of the model outlined in equation (4) for male workers. Furthermore, *queer* and *queer*percent female* are not individually

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[19] This finding is supported by the results from the model allowing for a structural break, as described in Appendix B.

[20] The author thanks Lee Badgett for suggesting this addition to the model.

or jointly significant, nor are *queer*, *queer*percent female*, and *queer*time*. As such, I conclude that adding this interaction term does not add any new information to our analysis.

Aside from in the regression on female respondents including the *queer*percent female* interaction, the coefficient on *queer* was not significant for any of the secondary regressions, nor was it jointly significant with the *queer*time* interaction. Disturbingly, the coefficients on the variables interacting *race* and *time* were significantly less than zero, indicating that people of color suffered a significantly larger earnings disadvantage in the later years of the sample. While the coefficients on the *queer*time* interaction terms were negative, these results were not significant. From this, we may conclude that queer people face less discrimination today than they did 20 years ago, and that people of color face more discrimination today. It seems plausible to imagine that recent anti-discrimination legislation has helped close the earnings gap between straight people and queer people but has largely ignored people of color. The policy implications of such an analysis are clear.

VIII. DISCUSSION

In testifying before the Ohio Senate, Barry Sheetz and David Miller from Citizens for Community Values asserted that the Equal Housing and Employment Act, intended to protect queer Ohioans from housing and employment discrimination, was unnecessary because lesbian, gay, and bisexual people are wealthier than heterosexual people even without this legislation. Similar arguments have been raised in the debate over the federal Employment Non-Discrimination Act (ENDA), which would prohibit employment discrimination on the basis of sexual orientation. These arguments primarily rely on studies that employ biased samples and inappropriate statistical comparisons, perpetuating the myth of gay affluence. The need for econometric analysis of earnings differentials between heterosexual and queer workers is apparent, both to provide insight on important legislation and for understanding the importance of other common variables in earnings regressions.

Sexual orientation may affect earnings for a variety of reasons, including different incentives for human capital investments and both direct and indirect discrimination. Applying Gary Becker's model of human capital to non-heterosexual workers, Black, *et al.* (2003) argue that optimal human capital allocations would encourage queer women to invest more in human capital. Since queer women, unlike straight women, do not expect to have a male partner to earn higher wages and specialize in labor market production, they must make labor market decisions based on their need to earn self-supporting wages. In the sample used in this study, queer women's significantly higher levels of education probably contributed to their significantly higher earnings when compared to straight women. Becker's model further indicates that queer men should invest less than straight men in human capital accumulation, as a male partner is likely to earn more than a female partner would. However, the data used in this sample does not support this theory: though queer men had significantly more education than their straight counterparts, they still earned less.

Elaborating on Becker's model of specialization, we would expect straight men to spend more hours on labor market production than queer men to support wives and children. This trend would increase straight men's wages compared to queer men's wages for causes unrelated to discrimination or sexual orientation. However, in the General Social Survey data, straight men and queer men reported having worked approximately the same number of hours in the last week. Queer women, however, reported having worked significantly more hours in the last week than straight women. This is consistent with the hypothesis that some of the earnings advantage enjoyed by queer women is due to specialization in labor market production.

The other primary school of thought attempting to explain earnings effects of sexual orientation employs earlier work done on the earnings effects of race and gender discrimination. Badgett (1995) provides a particularly thorough discussion of this theory, distinguishing between the effects of direct and indirect discrimination. Disclosure of a queer employee's sexual orientation may result in direct discrimination such as harassment or loss of promotions, but the effects of indirect discrimination can be just as significant. When closeted employees expend time and energy to avoid disclosing their sexual orientation, their productivity falls, reducing their earnings and expected future earnings.

In addressing the potential effects of labor market discrimination, it is important to note that though the regressions discussed in this paper suggest that the earnings gap between straight and queer people has fallen over time, this does not necessarily indicate that queer people no longer face discrimination. As such, we cannot use this solitary finding to argue that queer people do not need legal protection from discrimination. In fact, we may posit that the findings in this paper suggest that *recent* anti-discrimination legislation is working, and should be supported with even more comprehensive measures.

Since 1989, there has been significant progress in expanding legal rights and protections for queer people, including *Lawrence v. Texas* (2003), which overturned all state and local anti-sodomy laws. As of February 2010, 21 states and the District of Columbia prohibit employment discrimination based on sexual orientation for all employees; an additional nine states prohibit sexual orientation-based employment discrimination against state employees. The vast majority of these laws were passed after 1996. It was only in 2009 that the Matthew Shepard and James Byrd, Jr. Hate Crimes Prevention Act expanded earlier hate crimes legislation to include crimes based on sexual orientation.

We may conclude that the recent advances in legal protections for queer people have helped diminish the earnings differential between straight people and queer people, whereas people of color faced greater earnings disadvantages in more recent years because they have been largely ignored by recent anti-discrimination legislation. Though this study establishes only correlation, not causation, public policy and political theory support the hypothesis that anti-discrimination legislation becomes less effective over time if not regularly updated and expanded. As such, we may interpret the evidence as encouraging greater legal protections for historically disadvantaged populations, rather than challenging the necessity of these protections. Without continuous updates and innovations to anti-discrimination legislation, the protections in place become ineffective over time. Economists must continue to study earnings differentials between marginalized and dominant groups both to assess the efficacy of new legislation and to improve the quality of econometric models.

Importantly, the existing literature on earnings and sexual orientation suggest that variables that are commonly found to have a significant effect on earnings become less significant when sexual orientation is taken into account. For example, several studies conducted over the past 20 years have found that variables controlling for race were insignificant when analyzed alongside variables controlling for sexual orientation (Badgett 1995, Blandford 2003). Furthermore, Blandford (2003) provided an analysis indicating that earnings differentials generally attributed to marital status might in fact reflect previously unobserved effects of sexual orientation. These findings indicate that more work needs to be done to understand the earnings effects of sexual orientation and how other variables are affected by the inclusion or omission of controls for sexual orientation.

In designing new studies on earnings differentials between straight and queer people, we need to work to develop a more consistent methodology. The methods of defining sexual orientation based on past sexual behavior have been inconsistent across the existing literature. This accounts for many of the differences in the results of earlier studies, particularly between Black, *et al.* (2003) and Blandford (2003). Though they used the same data, Black, *et al.* found that queer men earned about 14% to 16% less than straight men. Blandford, on the other hand, found that queer men earned about 30% to 32% less than straight men. Other differences stem from the specificity of the controls for occupational sorting used in OLS regressions, as using more specific occupation categories tends to reduce the magnitude of the coefficient on the queer variable. Without adequate controls for occupation, some of the heterogeneity between individuals is ignored. Other variables, including *queer*, are forced to proxy for the effects of differences related to occupation.

Additional research must be done on the relationship between cohabitation and the earning effects of sexual orientation; so far, these studies have employed Census data. As discussed above, using Census data is a flawed method for studying effects of sexual orientation. Nonetheless, Clain and Leppel (2001) offer compelling evidence that the earnings effects of cohabitation with same-sex individuals are similar to those associated with same-sex sexual behavior. As such, further research is needed to disentangle these effects, using data that collects information on sexual behavior or self-identified sexual orientation, like the General Social Survey or the National Health and Social Life Survey, along with information on cohabitation.

Additional research into the earnings effects of sexual orientation is necessary to understand the earnings effects of sexual orientation, to improve econometric models of the determinants of earnings, and to encourage well-informed design of anti-discrimination and civil rights legislation.

IX. CONCLUSION

The evidence presented in this study, based on widely accepted econometric techniques, undermines existing evidence of significant earnings differentials related to sexual orientation. A simple model confirms earlier findings that queer men tend to earn less than straight men while queer women tend to earn more than straight women. However, these findings become less significant when including more specific controls for occupational characteristics. In the primary OLS regressions, adopting more detailed controls for occupational characteristics reduced the magnitude of the coefficient on queer. My initial model, based off those used in earlier studies, found that queer men earned approximately 11.6% less than straight men while queer women earned approximately 11.6% more than straight women.

However, when respecifying the existing model to control for the representation of women in an occupation, I find smaller and less significant coefficients on the queer variables. For men, this inclusion reduced the magnitude and significance of the queer variable. Sexual orientation was no longer a significant determinant of earnings at the 10% level. For women, the addition of the variable for the gender composition of the occupation slightly reduced the magnitude of the queer variable, though the coefficient on *queer was* still significantly greater than zero at the 10% level of significance.

When adding interaction terms between the time trend and the indicators for being queer, being black, and being of other non-white, non-black races, I find that there has been no significant change in the effect of being queer over the period 1989 to 2008. However, the findings indicate that the earnings disadvantage suffered by people of color has grown over the last 20 years.

In general, these findings reinforce earlier theories that controlling for sexual orientation is important in a model attempting to explain variance in earnings. However, estimating the effects of sexual orientation on earnings is far from straightforward. In particular, I find evidence that previous regressions did not fully account for occupation characteristics. Neglecting to account for occupational gender composition forced other variables, such as occupation and sexual orientation dummy variables, to act as proxies. This finding raises the question of what other previously unobserved factors are correlated with sexual orientation.

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XI. STATISTICAL TABLES AND GRAPHS

Table 1. Performance of Sexual Orientation Proxy in 2008
(Percentages of Self-Identified Queer Population in Parentheses)

	Male	*Female*
Self-Identified Queer Correctly Predicted (Sensitivity)	10 (83.3)	10 (83.3)
Self-Identified Heterosexual Mispredicted as Queer (False Positive)	1 (8.3)	5 (41.7)
Self-Identified Queer Mispredicted as Heterosexual (False Negative)	2 (16.7)	2 (16.7)
Total Self-Identified Queer in GSS 2008 sample	12	12

Notes: Measures of performance based on ability of behavioral proxy to predict self-identified sexual orientation. The behavior proxy defines as queer those who have (1) had at least one same-sex sex partner in the last year, or (2) had no sex partners in the last year and had at least one same-sex sex partner in the last five years. Table format adopted from Blandford (2003).

Table 2. Variable Means and Percentages for All Workers, by Sex and Orientation

Variable	*Men*		*Women*		*Total*
	Queer	*Straight*	*Queer*	*Straight*	
N	210	5,137	177	5,157	10,681
Percent of all observations	2.0%	48.1%	1.7%	48.3%	100.1%
Mean annual earnings (constant 2000$)	$37,167***	$42,968	$27,381***	$23,146	$32,837
Education (years)	14.5**	14.0	14.5***	14.0	14.0
Age	40.1	40.5	37.7**	39.5	39.9
Potential Experience (age-educ-5)	20.6	21.5	18.2***	20.5	20.9
Hours worked last week	45.6	45.8	42.0***	38.8	42.4
Number of children	0.6***	1.6	0.9***	1.7	1.6
Figures below are percentages					
Race:					
White	81.2	84.2	81.4	79.4	81.8
Black	10.5	9.3	12.4	14.5	11.9
Other	7.6	6.5	6.2	6.1	6.3
Part-Time	9.0	9.0	18.1	21.3	15.1
Union Member	8.6	16.4	16.7	12.5	14.4
Resides in large SMSA	15.7***	5.2	7.3	7.5	6.5
Region:					
Northeast	22.9*	17.9	17.5	17.8	18.0
Midwest	20.0*	25.4	22.6	26.0	25.2
South	39.5	41.2	35.7	40.1	40.6
West	22.9	22.5	31.6***	22.1	22.5
Highest degree achieved:					
Less than high school diploma	14.6	17.2	12.0	13.0	15.1
High school diploma	41.4***	51.2	43.5***	54.4	52.4
Associate's or junior college degree	7.6	8.5	11.9	9.9	9.2
Bachelor's degree	26.2**	20.4	22.0	20.9	20.8
Graduate degree	16.2**	11.4	15.3***	8.8	10.3
Occupation:					
Managerial & Professional Specialty	50.5***	34.9	39.5	37.7	36.6
Executive, Administrative, and Managerial	22.9*	18.1	15.3	15.9	17.1
Professional Specialty	27.6***	16.8	24.3	21.8	19.6
Technical, Sales, Administrative Support	23.8**	17.6	23.7***	38.5	27.9
Technicians and Related Support	4.8	4.7	4.0	4.7	4.7
Sales	10.0	7.2	9.6	10.5	8.9
Administrative Support	9.0**	5.7	10.2***	23.2	14.3
Service	9.0	8.3	16.9	15.2	11.8
Private Household Service	0.0	0.0	0.0	0.1	0.5
Protective Service	0.8**	3.1	5.1***	0.7	1.9
Armed Forces	0.0	0.0	0.0	0.0	0.0
Other Service	8.1*	5.3	11.9	13.5	9.4
Farming, Forestry, Fishing	1.4	3.3	1.1	0.5	1.9
Farm Workers	1.4	3.3	1.1	0.5	1.9
Precision Production, Craft, Repair	5.7***	21.1	5.6***	1.7	11.1
Mechanics and Repairers	1.0***	7.8	1.7***	0.4	4.0
Construction Trades	3.3***	8.8	1.7***	0.2	4.4
Extractive	0.0	0.1	0.0	0.0	0.0
Precision Production	1.4**	4.5	2.3	1.1	2.8
Operations, Fabrication, Labor	9.5**	14.8	13.0***	6.5	10.7
Mechanical Operators & Assemblers	3.3*	6.1	6.2	4.2	5.1
Transportation and Material Moving	2.4	3.2	1.7	0.9	2.0
Haulers, Helpers, Laborers	3.8	5.5	5.1***	1.5	3.5
Marital status:					
Married	16.7***	59.4	24.3***	53.6	55.2
Was married	11.9**	17.9	25.4	25.3	21.5

*Significantly different from straight at the 10% level; ** at the 5% level; *** at the 1% level.

Table 3. Accuracy of Replication of Blandford (2003)
Primary OLS Regression Results on Logged Real Annual Income Using One-Digit Occupation Codes, Male Workers
(t-Values in Parentheses)

Variable		*Blandford*	*(1)*
Intercept		8.39*** (92.1)	8.28*** (85.5)
Orientation:			
	Unmarried Heterosexual	-0.15*** (6.1)	-0.22*** (-8.2)
	Masked Queer	-0.20 (1.5)	-0.29** (-2.0)
	Open Queer	-0.38*** (5.1)	-0.46*** (-5.7)
Human Capital:			
	Education (years)	0.08*** (14.4)	0.07*** (12.9)
	Potential Experience	0.05*** (13.5)	0.06*** (17.2)
	Squared Potential Experience	0.00*** (9.4)	0.00*** (-14.2)
Race:			
	Black	-0.06 (1.5)	-0.03* (-0.7)
	Other Race	-0.04 (-0.8)	-0.03* (-0.5)
Resides in Large Metropolitan Area		0.19*** (6.2)	0.10** (2.0)
Geographic Region:			
	Northeast	0.12*** (3.7)	0.08** (2.1)
	Midwest	0.10*** (3.3)	0.07** (2.1)
	West	0.04 (1.3)	0.00 (0.1)
Occupation:			
	Managerial & Professional	0.27*** (6.2)	0.40*** (8.3)
	Technical, Sales, & Admin. Support	0.13*** (2.8)	0.22*** (4.6)
	Service	†	†
	Farming, Fishing, & Forestry	-0.10 (1.2)	0.01 (0.1)
	Precision Prod., Craft, & Repair	0.18*** (4.1)	0.27*** (5.5)
	Operators, Fabricators, & Laborers	0.06 (1.3)	0.15*** (3.1)
Adjusted R-Square		0.31	0.29
N		2,566	2,859

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.
†Default occupational dummy.

Table 4. Accuracy of Replication of Blandford (2003)

Primary OLS Regression Results on Logged Real Annual Income Using One-Digit Occupation Codes, Female Workers

(t-Values in Parentheses)

Variable		*Blandford*	*(1)*
Intercept		7.56*** (72.7)	7.31*** (68.3)
Orientation:			
	Unmarried Heterosexual	0.02 (0.9)	0.11*** (3.7)
	Masked Queer	-0.01 (0.0)	0.25 (1.4)
	Open Queer	0.21** (2.4)	0.28*** (2.9)
Human Capital:			
	Education (years)	0.09*** (14.0)	0.09*** (13.1)
	Potential Experience	0.04*** (9.8)	0.05*** (12.9)
	Squared Potential Experience	0.00*** (7.0)	0.00*** (-10.2)
Race:			
	Black	0.01 (0.2)	0.03 (0.6)
	Other Race	-.011* (1.8)	-0.07 (-1.0)
Resides in Large Metropolitan Area		0.15*** (4.6)	0.15*** (2.8)
Geographic Region:			
	Northeast	0.17*** (4.6)	0.14*** (3.3)
	Midwest	0.06* (1.8)	-0.02 (-0.4)
	West	0.10*** (2.8)	0.04 (1.0)
Occupation:			
	Managerial & Professional	0.50*** (10.9)	0.60*** (12.8)
	Technical, Sales, & Admin. Support	0.36*** (9.0)	0.39*** (9.6)
	Service	†	†
	Farming, Fishing, & Forestry	0.09 (0.4)	0.34 (1.4)
	Precision Prod., Craft, & Repair	0.42*** (4.8)	0.55*** (5.6)
	Operators, Fabricators, & Laborers	0.29*** (5.3)	0.38*** (6.5)
Adjusted R-Square		0.29	0.25
N		2,064	2,641

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.

†Default occupational dummy.

Table 5. Primary Regression Results on Logged Real Annual Income, Full-Time Male Workers
(t-Values in Parentheses)

	(2)	*(3)*
Variable		
Intercept	7.88*** (97.5)	8.11*** (94.9)
Queer	-0.11** (-2.1)	-0.07 (-1.4)
Percent Female	-	-0.48*** (-8.0)
Married	0.23*** (10.6)	0.22*** (10.3)
Time	0.02*** (8.2)	0.02*** (7.9)
Human Capital:		
Education (years)	0.07*** (16.5)	0.08*** (17.1)
Potential Experience	0.06*** (21.0)	0.06*** (20.6)
Squared Potential Experience	0.00*** (-17.9)	0.00*** (-17.5)
Race:		
Black	-0.35*** (-9.9)	-0.33*** (-9.5)
Other Race	-0.34*** (-8.2)	-0.33*** (-8.0)
Resides in Large Metropolitan Area	0.05 (1.1)	0.06 (1.3)
Geographic Region:		
Northeast	0.08** (2.6)	0.09*** (2.9)
Midwest	0.03 (1.3)	0.03 (1.3)
West	0.01 (0.2)	0.01 (0.2)
Occupation:		
Managerial & Professional		
Exec., Admin., Managerial	0.73*** (14.6)	0.67*** (13.1)
Professional Specialty	0.59*** (11.3)	0.54*** (10.2)
Technical, Sales, & Admin. Support		
Technician & Related Support	0.61*** (9.7)	0.48*** (7.4)
Sales	0.48*** (8.4)	0.45*** (8.0)
Admin. Support (including clerical)	0.33*** (5.6)	0.36*** (6.1)
Service		
Private Household	0.09 (0.2)	0.29 (0.7)
Protective Services	0.51*** (7.0)	0.34*** (4.5)
Armed Forces	††	††
Other Service	†	†
Farming, Fishing, & Forestry		
Farming	0.22*** (3.1)	0.05 (0.8)
Precision Prod., Craft, & Repair		
Mechanics and Repairers	0.59*** (10.5)	0.37*** (5.9)
Construction Trades	0.44*** (8.0)	0.21*** (3.5)
Extractive Occupations	0.72* (2.0)	0.51 (1.4)
Precision Production	0.60*** (9.3)	0.45*** (6.7)
Operators, Fabricators, & Laborers		
Machine Operators & Assemblers	0.52*** (8.7)	0.42*** (7.0)
Transportation and Material Moving	0.33*** (4.7)	0.16 (2.2)
Haulers, Helpers, Laborers	0.18*** (2.9)	0.01 (0.1)
Adjusted R-Square	0.31	0.32
F-statistic on explanatory power compared to *(2)*		63.9***
N	5,347	

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.
†Default occupational dummy.
†† No relevant observations in occupational category.

Table 6. Primary Regression Results on Logged Real Annual Income, Full-Time Female Workers
(t-Values in Parentheses)

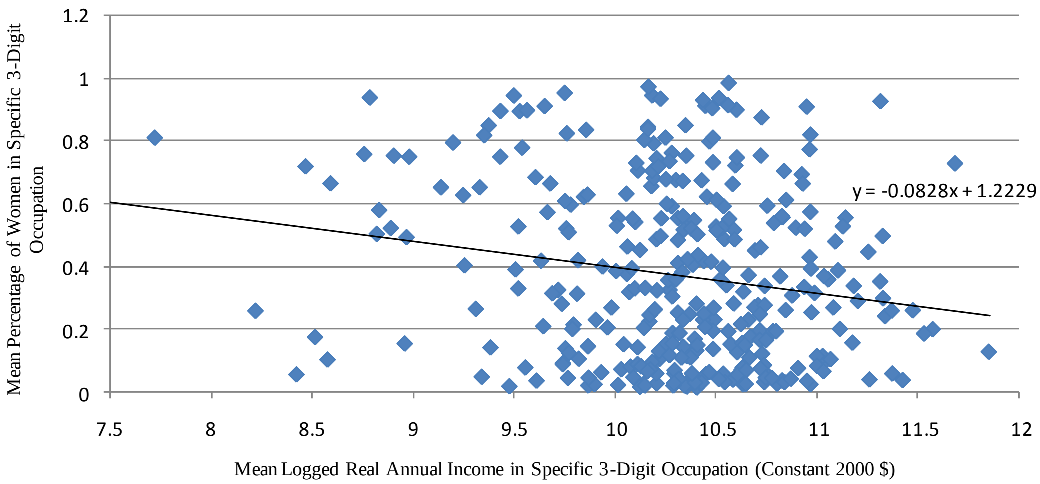
Variable	*(2)*	*(3)*
Intercept	7.34*** (88.8)	7.46*** (79.1)
Queer	0.11* (1.8)	0.10* (1.7)
Percent Female	-	-0.16*** (-2.7)
Married	-0.11*** (-4.9)	-0.11*** (-4.8)
Time	0.00 (1.2)	0.00 (0.3)
Human Capital:		
Education (years)	0.10*** (18.6)	0.09*** (18.6)
Potential Experience	0.06*** (18.4)	0.06*** (18.4)
Squared Potential Experience	0.00*** (-14.7)	0.00*** (-14.8)
Race:		
Black	-0.10*** (-3.0)	-0.09*** (-2.9)
Other Race	-0.24*** (-5.4)	-0.24*** (-5.4)
Resides in Large Metropolitan Area	0.15*** (3.5)	0.14*** (3.5)
Geographic Region:8		
Northeast	0.10*** (3.1)	0.10*** (3.2)
Midwest	0.00 (0.0)	0.00 (0.0)
West	0.07** (2.5)	0.07** (2.5)
Occupation:		
Managerial & Professional		
Exec., Admin., Managerial	0.77*** (19.1)	0.72*** (16.3)
Professional Specialty	0.54*** (13.4)	0.53*** (13.1)
Technical, Sales, & Admin. Support		
Technician & Related Support	0.63*** (11.1)	0.61*** (10.6)
Sales	0.25*** (5.8)	0.21*** (4.7)
Admin. Support (including clerical)	0.44*** (12.2)	0.45*** (12.3)
Service		
Private Household	-0.42*** (-3.7)	-0.39*** (-3.5)
Protective Services	0.34*** (2.9)	0.26** (2.1)
Armed Forces	††	††
Other Service	†	†
Farming, Fishing, & Forestry		
Farming	0.46*** (3.2)	0.39*** (2.6)
Precision Prod., Craft, & Repair		
Mechanics and Repairers	0.80*** (4.8)	0.69*** (4.1)
Construction Trades	0.29 (1.4)	0.18 (0.9)
Extractive Occupations	††	††
Precision Production	0.57*** (5.6)	0.51*** (5.0)
Operators, Fabricators, & Laborers		
Machine Operators & Assemblers	0.47*** (8.0)	0.43*** (7.0)
Transportation and Material Moving	0.13 (1.2)	0.07 (0.6)
Haulers, Helpers, Laborers	0.30*** (3.4)	0.23** (2.5)
Adjusted R-Square	0.26	0.26
F-statistic on explanatory power compared to *(2)*		7.5***
N	5,344	

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.
†Default occupational dummy.
†† No relevant observations in occupational category.

Table 7. Representation of Women in an Individual's Occupation by Sex and Sexual Orientation
(All numbers are in percentages)

Men		*Women*	
Straight	*Queer*	*Straight*	*Queer*
30.0	46.5	65.2	50.9

Graph1. Mean of Male Respondents' Logged Real Annual Income by Mean of Percentage of Women in Respondents' Occupation



Graph 2. Mean of Female Respondents' Logged Real Annual Income by Mean of Percentage of Women in Respondents' Occupation

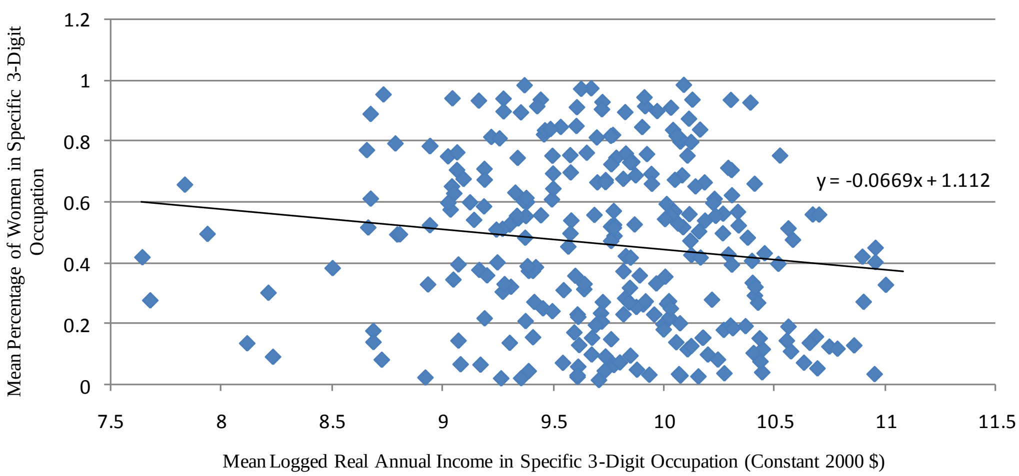


Table 8. Relevant Results from Secondary Regressions on Logged Real Annual Income, Full-Time Male Workers
(t-Values in Parentheses)

Variable	*(4)*	*(5)*	*(6)*
Intercept	8.11***(94.8)	8.07*** (94.3)	8.12*** (94.6)
Queer	-0.15 (-1.3)	-0.17 (-1.4)	-0.22 (-1.5)
Percent Female	-0.48*** (-8.0)	-0.48*** (-8.2)	-0.49*** (-8.0)
Married	0.22*** (10.3)	0.22*** (10.4)	0.22*** (10.3)
Time	0.01*** (7.6)	0.02*** (9.5)	0.01*** (7.6)
Queer*Time	0.01 (0.7)	0.01 (0.8)	0.01 (0.7)
Black*Time	-	-0.04*** (-5.7)	-
Other Race*Time	-	-0.03*** (-3.3)	-
Queer*Percent Female			0.15 (0.7)
Adjusted R-squared	0.32	0.32	0.32
F-statistic on joint significance of *queer* and *queer*time*	1.2	1.5	1.2
F-statistic on joint significance of *queer* and *queer*percent female*	-	-	1.1
F-statistic on explanatory power compared to primary regression *(3)*	31.5***	13.8***	0.6
F-statistic on explanatory power compared to secondary regression *(4)*	-	20.4***	0.5
N		5,347	

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.

Table 9. Relevant Results from Secondary Regressions on Logged Real Annual Income, Full-Time Female Workers
(t-Values in Parentheses)

Variable	*(4)*	*(5)*	*(6)*
Intercept	7.50*** (78.9)	7.44*** (78.6)	7.45*** (78.9)
Queer	0.18 (1.3)	0.18 (1.3)	0.46*** (2.6)
Percent Female	-0.15*** (-2.7)	-0.15*** (-2.8)	-0.13** (-2.4)
Married	-0.10*** (-4.8)	-0.10*** (-4.8)	-0.11*** (-4.9)
Time	0.00 (1.2)	0.01** (2.4)	0.00 (1.2)
Queer*Time	-0.01 (-0.7)	-0.01 (-0.7)	-0.01 (-0.6)
Black*Time	-	-0.02*** (-2.6)	-
Other Race*Time	-	-0.01* (-1.7)	-
Queer*Percent Female			-0.55** (-2.5)
Adjusted R-squared	0.26	0.26	0.26
F-statistic on joint significance of *queer* and *queer*time*	1.6	1.6	4.8***
F-statistic on joint significance of *queer* and *queer*percent female*	-	-	4.1**
F-statistic on explanatory power compared to primary regression *(3)*	0.5	3.2**	3.5**
F-statistic on explanatory power compared to secondary regression *(4)*	-	4.5**	6.5***
N		5,334	

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.

XII. APPENDIX A: SECONDARY REGRESSIONS USING YEAR DUMMIES

I also added dummy variables for the years the data were collected to my model. The addition of the dummy variables, rather than the time trend, was to estimate the potential effect of changing business cycles over the course of the time period studied. I first added dummy variables for each year that the data were collected, using 1989 as the default year. I estimated the equation:

$$\text{(A.1)} \qquad y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i + \varepsilon_i,$$

where y_i is logged real annual income, x_i is an indicator of sexual orientation, z_i is a vector of individual characteristics including the representation of women in an occupation, and t_i is a vector of dummy variables for the years that GSS data was collected. Performing an F-test, I found that the regression of male workers did not provide a significantly better fit than the primary regression using the time trend. However, the regression of female workers using time dummies performed significantly better than the primary regression without time dummies. I found the F-statistic on this test to be greater than the critical value for the 1% level of significance.

The coefficients on the relevant variables from this regression are displayed in Table A1 on page 44. For men, the coefficients on the time dummies followed the hypothesized pattern: in the early 1990s, the coefficients are negative. After 1998, the coefficients are positive, indicating that men's earnings were greater after 1998 than they were in 1989. These coefficients are only significant at conventional levels for the years following 2000. For the years 1990 through 2000, men's earnings were not significantly different from their 1989 levels.

For women, the coefficients on the year dummy variables are somewhat more ambiguous. Only the coefficients on the dummies for 1998 and 2000 are significant at conventional levels. Between 1989 and 2008, the signs on these coefficients change six times. Furthermore, there is no discernable pattern in the levels of significance of the coefficients on the time variables. For both men and women, adding the year dummies increased the magnitude and significance of the coefficients on *queer*, though not significantly so.

To further supplement our understanding of the earnings effects associated with sexual orientation, I added interaction terms between the queer variable and the dummy variables for years. Hypothesizing that tolerance of queer workers has increased over time, I would expect that labor market discrimination against queer people would have fallen over the period of the dataset. As such, we should expect the earnings disadvantage suffered by queer men to fall over time. However, since evidence of discrimination against queer women workers is inconsistent, there is no obvious hypothesis about changes in queer women's earnings over time. The model to be estimated is:

(A.2) $$y_i = \alpha + \beta x_i + \gamma z_i + \delta t_i + \theta x_i t_i + \varepsilon_i,$$

where $x_i t_i$ is a vector of interaction terms between *queer* and the year dummy variables.

For women but not men, this model was a significant improvement over the regression using the time trend variable. The results of this regression are shown in Table A2 on page 45. However, adding the interaction terms does not significantly improve the model compared to the regression using the dummy variables for years (equation A.2). For men, adding the year dummy variables and interaction terms increases the coefficient on queer from -0.07, which corresponds to an earnings disadvantage of 7.3%, to 0.13, which corresponds to an earnings advantage of 13.9%. The addition of the interaction terms also decreases the t-statistic on the coefficient on *queer* from -1.4 to 0.3. For women, the new specification increases the magnitude of the coefficient on queer from 0.10 to 0.17; that is, the earnings premium rises from 10.5% to 18.5%. However, the t-statistic falls from 1.7 to 0.6 to the point that the coefficient on *queer* is no longer significant at any conventional levels. The coefficients on the year dummies are similar in this regression to those in the regression without the interaction terms.

Testing for joint significance of the interaction terms, I find that the interaction terms themselves are not jointly significant at any conventional levels. For women but not for men, the queer variable and the interaction terms are jointly significant at the 10% level of significance, though just barely. I conclude that the addition of interaction terms does not significantly improve the explanatory power of the model, nor does it contribute much to our understanding of earnings differentials based on sexual orientation.[21]

XIII. APPENDIX B: SECONDARY REGRESSION WITH STRUCTURAL BREAK

My next regression to estimate changes over time added a structural break to the model. I estimated the equation:

(4) $y_i = \alpha + \beta x_i + \gamma z_i + \varepsilon_i$ for the years 1989 to 1996,

$y_i = \alpha' + \beta' x_i + \gamma' z_i + \varepsilon_i$ for the years 1998 to 2008,

where y_i is logged real annual income, x_i is an indicator of sexual orientation, and z_i is a vector of individual characteristics including the representation of women in an occupation.

[21] I also performed a regression using just the interaction terms between *queer* and the year dummies. F-tests showed that this model was not a significant improvement over the model using both year dummies and the interaction terms. Therefore, I have omitted the results of this regression.

I chose 1996/1998 as the dividing line for three important reasons. First, in the regressions using dummy variables for each year (equations A.1 and A.2), when there is a clear switch from negative to positive coefficients on the years, it generally occurs between 1994 and 2000. Second, setting the break point between 1996 and 1998 makes the sample sizes of each as close to equal as possible. Third, both Blandford (2003) and Black, *et al.* (2003) limited their sample to the years 1989 through 1996. Choosing this specific dividing line will help me evaluate why their results are different from those in my secondary regression. If I find that the results for the years 1989 to 1996 are significantly different from those in Blandford and Black, *et al.*, I will have further evidence to believe that the addition of the variable accounting for occupational gender composition is a valuable contribution to the literature.

For both men and women, a Chow Test finds that the structural break in the sample significantly improves the explanatory power of the model compared to that using the time trend. The results of these regressions are displayed in Tables B1 and B2 on pages 46 and 47. For both men and women, I find that the coefficient on *queer* is much larger and more significant in the earlier sample than in both the later sample and the whole sample. [22] From this, I conclude that between 1989 and 1996, queer men faced a significant earnings disadvantage and queer women faced a significant earnings advantage compared to their heterosexual counterparts. In the later years, however, the earnings differentials were not statistically significant. The same trend holds for the percent female variable: as time progressed, the negative effect of high levels of female participation in an occupation became smaller and less significant.

The structural break also provides valuable insight on some of the other variables used in the regression. Most interestingly, I find that the coefficients on *black* and on *otherrace* are much

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[22] In the first half of the sample, the coefficient on *queer* for female workers is of the same sign and magnitude as that found in Blandford (2003). For men, the coefficient on *queer* for 1989 to 1996 is much closer in magnitude to that found in Blandford (2003), though it is still only half as large.

more negative and much more significant in the later years than they were in the earlier years. The coefficients on *black* and *otherrace* for the first half of the sample roughly correspond to those found by Blandford (2003). The finding that non-white workers' earnings fell over the period 1989 to 2008 suggests that anti-discrimination legislation protecting people of color has become outdated and ineffectual since its adoption.

Some of the changes in coefficients on the occupation variables are also noteworthy. For one, working in technical support occupations became more lucrative for women over time, while the coefficient on this variable dropped for men. The coefficients on several occupation variables also became significantly greater than zero for women in the later sample. This is especially true of "blue collar" jobs, including protective service, construction, and transportation, hauling, and moving. This may be indicative of a greater acceptance of women in historically male-dominated fields, a trend that would help narrow the gap between both men's and women's earnings and straight and queer people's earnings.

XIV. STATISTICAL TABLES FROM APPENDICES

Table A10. Relevant Results from Regression on Logged Real Annual Income, Full-Time Workers Using Year Dummies
(t-Values in Parentheses)

Variable	*Male Workers*	*Female Workers*
Intercept	8.19*** (87.5)	7.54*** (74.6)
Queer	-0.07 (-1.3)	0.11* (1.8)
Percent Female	-0.47*** (-8.0)	-0.16*** (-2.9)
Married	0.22*** (10.2)	-0.10*** (-4.8)
Year:		
1989	†	†
1990	-0.02 (-0.4)	-0.15** (-2.2)
1991	-0.01 (-0.2)	-0.04 (-0.6)
1993	-0.00 (0.0)	0.07 (1.1)
1994	-0.03 (-0.6)	0.05 (0.9)
1996	0.06 (1.1)	0.01 (0.3)
1998	-0.01 (-0.3)	-0.25*** (-4.5)
2000	0.06 (1.1)	-0.19*** (-3.3)
2002	0.15*** (2.7)	0.03 (0.6)
2004	0.16*** (2.9)	0.01 (0.2)
2006	0.20*** (3.6)	0.00 (-0.1)
2008	0.23*** (4.0)	0.07 (1.1)
Adjusted R-Square	0.32	0.27
F-statistic on explanatory power compared to primary regression *(2)*	1.33	10.6***
N	5,347	5,334

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.
†Default year dummy.

Table A11. Relevant Results from Regression on Logged Real Annual Income, Full-Time Workers Using Year Dummies and Interaction Terms
(t-Values in Parentheses)

Variable	*Male Workers*	*Female Workers*
Intercept	8.19*** (87.1)***	7.55*** (74.2)
Queer	0.13 (0.3)	0.17 (0.6)
Percent Female	-0.47*** (-8.0)***	-0.16*** (2.9)
Married	0.22*** (10.3)***	-0.10*** (-4.7)
Year:		
1989	†	†
1990	-0.02 (-0.4)	-0.15** (-2.2)
1991	-0.01 (-0.2)	-0.04 (-0.6)
1993	0.00 (0.0)	0.06 (0.9)
1994	-0.02 (-0.3)	0.06 (1.1)
1996	0.07 (1.3)	0.02 (0.3)
1998	-0.01 (-0.2)	-0.25*** (-4.3)
2000	0.05 (1.0)	-0.18*** (-3.2)
2002	0.15*** (2.7)	0.03 (0.6)
2004	0.15*** (2.7)	0.02 (0.3)
2006	0.21*** (3.7)	-0.01 (-0.3)
2008	0.23*** (4.0)	0.09 (1.5)
Interaction Terms:		
Queer*1989	†	†
Queer*1990	-0.13 (-0.3)	0.26 (0.5)
Queer*1991	-0.23 (-0.5)	0.13 (0.2)
Queer*1993	-0.10 (-0.2)	0.33 (0.9)
Queer*1994	-0.44 (-1.0)	-0.33 (-1.0)
Queer*1996	-0.36 (-0.8)	-0.04 (-0.1)
Queer*1998	-0.25 (-0.6)	-0.13 (-0.4)
Queer*2000	-0.03 (-0.1)	-0.05 (-0.2)
Queer*2002	-0.16 (-0.4)	-0.03 (-0.1)
Queer*2004	0.06 (0.1)	-0.05 (-0.1)
Queer*2006	-0.34 (-0.8)	0.21 (0.6)
Queer*2008	-0.16 (-0.3)	-0.56 (-1.6)
Adjusted R-Square	0.32	0.27
F-statistic on explanatory power compared to primary regression *(2)*	1.1	5.77***
F-statistic on explanatory power compared to regression using year dummies *(A.1)*	0.86	1.42
F-statistic on joint significance of *queer* and interaction terms	0.93	1.57*
N	5,347	5,334

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.
†Default year dummy or default interaction dummy.

Table B12. Relevant Results from Regression on Logged Real Annual Income, Full-Time Male Workers with Structural Break (t-Values in Parentheses)

Variable	*1989-1996*		*1998-2008*
Intercept	8.19*** (71.4)		8.30*** (68.6)
Queer	-0.18** (-2.2)		-0.03 (-0.4)
Percent Female	-0.56*** (-7.0)		-0.45*** (-5.2)
Married	0.20*** (6.8)		0.23*** (7.6)
Human Capital:			
Education (years)	0.08*** (12.8)		0.08*** (12.3)
Potential Experience	0.06*** (14.4)		0.06*** (15.1)
Squared Potential Experience	0.00*** (-11.7)		0.00*** (-13.1)
Race:			
Black	-0.04 (-0.8)		-0.51*** (-10.6)
Other Race	-0.01 (-0.1)		-0.48*** (-9.0)
Resides in Large Metropolitan Area	0.07 (1.2)		0.03 (0.5)
Geographic Region:			
Northeast	0.09** (2.2)		0.09** (2.2)
Midwest	0.07** (2.1)		0.00 (0.0)
West	0.03 (0.9)		-0.01 (-0.2)
Occupation:			
Managerial & Professional			
Exec., Admin., Managerial	0.65*** (9.2)		0.67*** (9.4)
Professional Specialty	0.54*** (7.5)		0.53*** (7.2)
Technical, Sales, & Admin. Support			
Technician & Related Support	0.54*** (5.9)		0.45*** (5.0)
Sales	0.40*** (5.2)		0.51*** (6.2)
Admin. Support (including clerical)	0.40*** (5.0)		0.36*** (4.2)
Service			
Private Household	0.82 (1.3)		-0.06 (-0.1)
Protective Services	0.36*** (3.5)		0.31*** (2.8)
Armed Forces	††		††
Other Service	†		†
Farming, Fishing, & Forestry	0.06 (0.6)		0.04 (0.4)
Precision Prod., Craft, & Repair			
Mechanics and Repairers	0.34*** (4.1)		0.39*** (4.3)
Construction Trades	0.15* (1.8)		0.25*** (2.8)
Extractive Occupations	0.41 (1.1)		0.81 (1.1)
Precision Production	0.46*** (5.1)		0.43*** (4.5)
Operators, Fabricators, & Laborers			
Machine Operators & Assemblers	0.42*** (5.2)		0.41*** (4.7)
Transportation and Material Moving	0.17 (1.6)		0.18* (1.7)
Haulers, Helpers, Laborers	0.02 (0.2)		0.00 (0.0)
Adjusted R-Square	0.32		0.32
F-statistic on Chow test		2.18***	
N	2,380		2,967

*Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.
†Default year dummy or default interaction dummy.
†† No relevant observations in occupational category.

Table 13. Relevant Results from Regression on Logged Real Annual Income, Full-Time Female Workers with Structural Break (t-Values in Parentheses)

Variable	*1989-1996*		*1998-2008*
Intercept	7.61*** (58.8)		7.40*** (57.0)
Queer	0.17* (1.8)		0.07 (1.0)
Percent Female	-0.21*** (-2.7)		-0.13 (-1.6)
Married	-0.10*** (-3.2)		-0.12*** (-3.8)
Human Capital:			
Education (years)	0.09*** (13.1)		0.10*** (13.4)
Potential Experience	0.05*** (12.1)		0.06*** (13.9)
Squared Potential Experience	0.00*** (-9.5)		0.00*** (-11.2)
Race:			
Black	0.04 (0.9)		-0.19*** (-4.2)
Other Race	-0.08 (1.1)		-0.30*** (-5.1)
Resides in Large Metropolitan Area	0.16*** (2.9)		0.09 (1.5)
Geographic Region:			
Northeast	0.14*** (3.2)		0.07* (1.7)
Midwest	-0.01 (-0.1)		0.01 (0.2)
West	0.05 (1.4)		0.08** (2.0)
Occupation:			
Managerial & Professional			
Exec., Admin., Managerial	0.64*** (10.1)		0.77*** (12.7)
Professional Specialty	0.52*** (9.1)		0.55*** (9.7)
Technical, Sales, & Admin. Support			
Technician & Related Support	0.52*** (6.7)		0.68*** (8.1)
Sales	0.19*** (3.1)		0.22*** (3.4)
Admin. Support (including clerical)	0.43*** (8.8)		0.44*** (8.3)
Service			
Private Household	-0.33*** (-1.9)		-0.44*** (-2.9)
Protective Services	-0.01 (-0.1)		0.46*** (2.8)
Armed Forces	††		††
Other Service	†		†
Farming, Fishing, & Forestry	0.23 (0.9)		0.48*** (2.6)
Precision Prod., Craft, & Repair			
Mechanics and Repairers	0.58** (2.5)		0.78*** (3.2)
Construction Trades	-0.14 (-0.6)		0.53 (1.6)
Extractive Occupations	††		††
Precision Production	0.47*** (3.5)		0.52*** (3.4)
Operators, Fabricators, & Laborers			
Machine Operators & Assemblers	0.43*** (5.6)		0.35*** (3.7)
Transportation and Material Moving	-0.21 (-1.4)		0.31* (1.8)
Haulers, Helpers, Laborers	0.28** (2.3)		0.16 (1.2)
Adjusted R-Square	0.26		0.27
F-statistic on Chow test		2.09***	
N	2,403		2,931

*Statistically significant at the 10% level; ** at the 5% level; ***at the 1% level.
†Default year dummy or default interaction dummy.
†† No relevant observations in occupational category.