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Joanne S. McLaughlin
David Neumark

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ABSTRACT

We present new and rich evidence on intersectional discrimination in labor markets, focusing on wages in the traditional residual wage differential approach to discrimination. We interpret "intersectional discrimination" in the framework of interactions, in which discrimination along intersecting dimensions leads to discrimination that exceeds the sum of its parts. We make three contributions. First, we resolve puzzling contradictory findings on intersectional discrimination in existing research in which studies using similar data and methods reaching diametrically opposite conclusions. Second, we extend the analysis of potential intersectional discrimination to more dimensions than have typically been considered in past research. Third, we explore issues of bias in the wage equations we estimate from selection on employment. Our overall conclusion from these different types of evidence is that there is little or no evidence consistent with intersectional discrimination in wage differentials among the large set of groups (and combinations of groups) we study, and indeed, most evidence points in the opposite direction.

Joanne S. McLaughlin
University at Buffalo
Department of Economics
mcl.joanne.song@gmail.com

David Neumark
University of California, Irvine
Department of Economics
and NBER
dneumark@uci.edu

I. Introduction

Intersectional discrimination has become a dominant paradigm in the social sciences. A Google Scholar search for "intersectional discrimination" leads to about 11,400 citations, 8,100 of them since 2020.[1] The corresponding numbers for "intersectionality" are over 514,000, with about 86,500 since 2020. The concept is invoked across topics as varied as public health (e.g., Bowleg, 2021), feminism (e.g., Christoffersen and Emejulu, 2023), therapy (e.g., Maxey, 2021), and the study of refugees (e.g., Chossière, 2021). A special issue of the *Journal of Social Issues* (Volume 76, Issue 4) considers intersectionality in the context of psychology, intergenerational transmission of trauma and state-perpetrated violence, homeless and housing precarity, stress, prejudice, sexual stereotypes, solidarity and alliances, social justice work, intergroup relations, and scholar-activism in the university.[2] Most germane to this paper, the concept has been widely applied to labor market discrimination.

Much writing on intersectional discrimination in the labor market is outside of the traditional methodology – widely used by labor economists and sociologists – of empirically testing for or measuring labor market discrimination based on differences in wages, hiring, or other outcomes in the job market. For example, much recent work that discusses intersectional discrimination in the labor market focuses on the law (e.g., Atrey, 2019; Nourafshan, 2017). Other work on intersectional discrimination is more empirical, but has not focused on adapting standard tests of labor market discrimination to the intersectional discrimination hypothesis. For instance, McLaughlin (2020) studies intersectional discrimination (by age and gender) against older women based on gender differences in the effects of age discrimination laws.

In this paper, we present new and rich evidence on intersectional discrimination in labor markets, embedding tests for intersectional labor market discrimination in the traditional residual wage differential approach to discrimination. We interpret "intersectional discrimination" in the framework of interactions, in which discrimination along intersecting dimensions leads to discrimination that exceeds the sum of its parts. For example, is the wage penalty for Black women larger than the sum of the wage penalty for Black

[1] These counts are as of November 9, 2025.

[2] See https://spssi.onlinelibrary.wiley.com/toc/15404560/2020/76/4.

men and the gender penalty for White women? In the regression context, this is equivalent to estimating a model for wages with a race dummy variable (Black) and a gender dummy variable (female), as well as a Black-female interaction, and asking whether the estimated coefficient of the latter interaction is negative. This perspective on intersectional discrimination is consonant with Crenshaw's description of the "intersectional experience" as "greater than the sum of racism and sexism" (1989, p. 140).[3]

This is also the perspective that has been adopted in other recent research that tests for intersectional discrimination in labor market outcomes (e.g., Kim, 2002; George et al., 2022; Greenman and Xie, 2008; Mandel and Semyonov, 2016; Paul et al., 2022; White, 2023). This research largely focuses on race and gender. As discussed in more detail in Section III, however, these studies lead to quite contradictory results that have neither been noted very explicitly nor resolved.

We make three contributions to this emerging empirical literature. First, we resolve the puzzling contradictory findings on intersectional discrimination in existing research in which studies using similar data and methods reach diametrically opposite conclusions. Second, we extend the analysis of potential intersectional discrimination to more dimensions than have typically been considered in past research. While most of this research focuses on race and gender, we study race, ethnicity, gender, sexual orientation, and disability – and we define groups as "marginalized" in terms of not just two of these dimensions, but three as well (e.g., Black, female, and disabled). Third, we explore issues of bias in the wage equations we estimate from selection on employment, using both traditional selection bias correction methods as well as a newer method.

Our overall conclusion from these different types of evidence is that there is little or no evidence consistent with intersectional discrimination in wage differentials among the large set of groups (and combinations of groups) we study. Moreover, this evidence turns out to be consistent with the estimates reported in the prior literature, as the studies claiming evidence of intersectional labor market

[3] That said, in Section II we discuss more expansive perspectives on intersectionality that do not necessarily predict this "greater than additive" disadvantage. We think it is useful to use "intersectionality" to refer to the general impact of intersecting identities, and "intersectional discrimination" to refer to the prediction of greater adverse penalties in labor markets (or other markets) from intersecting identities.

discrimination perform the wrong calculations based on their estimates – calculations that, when corrected, also fail to find evidence of intersectional labor market discrimination. Indeed, our evidence generally points in the opposite direction, with doubly (or triply) marginalized groups earning more than would be predicted by the wage penalties associated with each individual group to which they belong.

II. Discrimination, Intersectional Discrimination, and Wage Regressions

In this section we lay out the economic theories and methods we use to think about intersectional discrimination in the labor market.

Models of discrimination

Economists mainly use two different models of discrimination. The first, developed by Becker, assumes a nonpecuniary cost or "distaste" of different economic agents – employers, employees, or customers – from interacting with particular groups (Becker, 1971). Most empirical work on discrimination can be interpreted via the employer discrimination model. The market-level implication of employer discrimination is that employees in groups that experience discrimination will earn less than similarly productive members of other groups – with the lower pay sufficient to make the employer willing to hire from the former.[4] The employer discrimination model maps directly into studying discrimination via wage regressions that estimate group differences in pay controlling for productivity-related characteristics (e.g., Neumark, 1988).[5]

The statistical discrimination model appeals to assumptions about the productivity or quality of different groups, rather than "distaste." If an employer thinks members of a particular group are less productive, even when observable characteristics like education, experience, etc., are the same, then the employer will only hire members of that group at a lower wage (Aigner and Cain, 1977).[6] Whether the assumptions on which employers statistically discriminate are true or not, the predicted effects on wages

[4] In the customer discrimination model, customers require a lower price when buying from the disfavored group, and in the employee discrimination model, workers require a higher wage to work alongside the disfavored group.

[5] The employer discrimination model would also predict hiring discrimination, especially when there are constraints (like minimum wages, or equal pay laws) on paying lower wages to members of protected groups employed at the same firm.

[6] And the same consideration as for the employer discrimination model implies that equal pay constraints will translate into hiring discrimination.

are similar, although more subtle differences can arise. For example, initially erroneous beliefs about differences in productivity can become self-fulfilling (Coate and Loury, 1993). Applied to gender, employers may initially assume that expected tenure is lower for women than for men, and therefore choose to invest less in women's human capital; this can give women less incentive to stay with the firm, leading to employers' expectations about women's lower tenure being fulfilled.[7]

The "residual wage regression" approach to measuring discrimination

Our empirical approach to testing for intersectional discrimination is grounded in the "residual discrimination" approach to measuring discrimination. This approach, which is usually focused on wage discrimination, focuses on the role of discrimination in generating wage gaps between groups. The approach entails estimating wage regressions, trying to control for productivity-related differences between two groups, and interpreting the remaining wage gap between the groups as an estimate of discrimination. These analyses are often done using separate regressions to allow wage differences between groups to vary with other characteristics (e.g., Blinder, 1973; Oaxaca, 1973; Neumark, 1988), but sometimes using more restrictive models with dummy variables to estimate residual differences between groups (e.g., Neal and Johnson, 1996). Although the approach can be most directly interpreted in the context of wage discrimination and the Becker employer discrimination model, it has been used to study other outcomes, like employment and poverty (Nieuwenhuis et al., 2020), hours (Alameddine et al., 2018), or wealth (e.g., Anglade et al. 2017).

While the residual discrimination approach has been and continues to be widely used, it does not necessarily provide the most definitive evidence on discrimination, most importantly because unexplained wage gaps (or other gaps) can potentially be attributed to unmeasured productivity-related characteristics for which the regression fails to account, rather than discrimination.[8] This has led labor market researchers

[7] Drawing on insights from psychology, some economists (e.g., Bertrand et al., 2005) think in terms of implicit discrimination (Devine, 1989; Greenwald and Banaji, 1995). In this model, discriminatory decisions are based on unconscious rather than conscious biases. Unconscious biases may lead to discriminatory behavior by different agents – e.g., recruiters evaluating job applicants – even among decision makers who are not prejudiced or who deliberately try to avoid stereotypes or prejudice.

[8] See, e.g., Goldin (2014), who also notes (as have many others) that discrimination is one possible explanation for residual wage gaps. A second potential problem with the residual discrimination approach is that the regression controls can reflect responses to discrimination, such as lower experience and tenure of women because of

to develop other approaches, including the production function approach to wage discrimination (initially developed in Hellerstein and Neumark, 1999), and field experiments – audit or correspondence studies – to study discrimination in hiring (a burgeoning literature discussed in, e.g., Fix and Struyk, 1993; Riach and Rich, 2002; and Gaddis, 2018). A potential advantage of the residual discrimination approach is its direct focus on wage differences between groups, which is our principal focus. In contrast, experimental studies primarily focus on hiring and have not, for the most part, provided direct evidence on the effects of discrimination on wages (Neumark, 2018).

Finally, it is important to point out that the residual discrimination approach is very much the workhorse of statistical evidence in class action pay discrimination lawsuits (see the historical discussion in Chassonnery-Zaïgouche, 2020, and an earlier discussion in Bloom and Killingsworth, 1982). And in some cases (e.g., race and gender), intersectional claims are clearly permissible, while in others (e.g., race and disability) they may not be, because the anti-discrimination protections covering these categories fall under different statutes (McLaughlin and Neumark, 2024, discuss the legal context). Thus, knowledge of what wage regressions say about intersectional discrimination is of practical importance. Nonetheless, because we recognize that residual wage differences between groups do not represent definitive evidence of "discrimination," we generally refer to our results as "consistent with" wage discrimination – and in particular, intersectional wage discrimination – or not.

Intersectional discrimination as interactions

Using the residual wage regression approach, we interpret "intersectional discrimination" in the framework of interactions, in which discrimination along two (or more) intersecting dimensions may lead to discrimination that exceeds the sum of its parts. For example, in empirical work, if disability discrimination in, say, hiring, is gendered (worse for women than for men), we would expect a negative interactive effect for an indicator for being both disabled and female. In this case, disabled workers in general fare worse than non-disabled women and men (controlling for the effect of gender), but disabled

discrimination on the job (e.g., Gronau, 1988) – sometimes termed "feedback" effects – which can lead to understating discrimination by conditioning on its effects.

women fare worse than disabled men.[9]

This perspective on intersectional discrimination is consonant with some key intersectional discrimination literature – both theoretical and empirical. Writing about race and sex, Crenshaw (1989, p. 140) describes the "intersectional experience" as "greater than the sum of racism and sexism." Similarly, and more explicitly, Hancock (2007) writes: "Attention to complex causality here would require recognition of categories that are multiple but not mutually independent – gender, class, and sexual orientation" (p. 252).

Moreover, this perspective has been adopted in empirical work – indeed, in all of the empirical papers we have identified that test for intersectional discrimination (e.g., Kim, 2002; George et al., 2022; Paul et al., 2022; White, 2023, and others discussed below).[10] For example, in describing their results on race and gender, Paul et al. (2022) write: "… we find the unexplained portion of the wage gap between Black women and White men … is larger than the sum of the two individual penalties… Simply stated, we confirm the existence of an intersectional race and gender penalty for earnings in the case of Black women." (pp. 17-18). And White (2023) writes: "… to determine if the discrimination suffered by members of this worker group is additive or intersectional we must obtain separate discrimination rate estimates for race … and sex …, sum the values to produce the expected discrimination rate …, and compare the resulting value to our estimated discrimination rate… For the native-born, non-Hispanic, black, female worker group … since the estimated discrimination rate is 24.85% and the expected discrimination rate is 12.12%, we have evidence in support of intersectional discrimination." (pp. 112-113).[11]

We recognize, though, that the framing of our empirical analysis ignores two dimensions of

[9] The interaction of membership in two marginalized groups need not always lead to this kind of "additional" disadvantage. For example, Pedulla (2014) suggests that gay black men may face less discrimination than straight black men, because there are perceived as less threatening or are less stereotyped as criminal, and finds evidence consistent with this.

[10] Browne and Misra (2003), writing earlier, also refer to testing for these interactions as providing evidence on intersectional discrimination. However, they also note that "it is precisely in the sorting of individuals into jobs that gender and race appear to intersect in important ways" (p. 498). This perspective is addressed in the more recent empirical papers on intersectional discrimination that, for example, do or do not condition on industry and occupation controls.

[11] This approach has also been used to study the determinants of stereotypes (Heiserman, 2023).

complexity in the evolving research literature on intersectional discrimination. First, intersectional discrimination, of course, captures more richness than is captured in interactions in regression models. Some scholars criticize quantitative analyses of intersectional discrimination, and prefer qualitative research and understanding the "context of lived experience" (Bright et al., 2016; see also Collins, 2022; Hernandez, 2005; Narayan et al., 2021). McCall (2005) provides an overview of the development of different methods and approaches to the study of intersectionality, including: "anticategorical complexity," which argues that social life and interactions are too complex to make any fixed categories meaningful; "intercategorical complexity," which entails researchers adopting existing categories to study relationships of inequality between groups defined by multiple and conflicting dimensions; and "intracategorical complexity," which lies somewhat in between, using categories, but regarding them ambivalently, focusing on groups with intersecting identities that tend to be neglected, and focusing on the lived experience of those in these groups. The intercategorical complexity approach (or what McCall also simply calls "categorical") is the one most closely tied to the empirical analysis of differences in socioeconomic outcomes based on multiple intersecting identities.

All of these approaches can provide complementary evidence, and the first, in particular, can discover new identities and illuminate issues that conventional quantitative analyses cannot do. However, in our view – we believe consistent with McCall – the "empirical interactions" approach is a fruitful way to understand whether types of evidence commonly used to learn about discrimination – both in research and in employment litigation – point to intersectional discrimination. In particular, if there are sources of double (or triple) disadvantage, then a legislative fix to increase the ability to bring intersectional discrimination claims could be warranted (with the caveat that a stronger case may require more rigorous evidence on discrimination than wage regressions can provide).

The second complexity is that the predicted effect of intersecting marginalized identities is not necessarily unidirectional. There is a long-standing alternative theory of the role of interactions in labor market outcomes: namely, that because of anti-discrimination law and affirmative action, individuals who are in multiple protected groups may face an advantage since these policies might encourage employers to hire, for example, Black women rather than Black men (Bell, 1992).[12] While this policy impact is perhaps most likely to affect the hiring margin, that would also result in increased demand and, hence, higher wages. This theoretical ambiguity is reflected more broadly in new sociological perspectives that move beyond the "double jeopardy" (Greenman and Xie, 2008), or doubly (or more) disadvantaged approach (Almquist, 1975), constructing alternative arguments as to why intersecting identities can reduce the penalties associated with each identity separately. For example, Black women's "invisibility" may enable them to avoid some of the lower expectations of White women and Black men, which can imply a positive, rather than a negative, Black-female interaction (Chavez and Wingfield, 2018); Purdie-Vaughns and Eibach (2008) provide a more general perspective on this hypothesis.

One might argue that a "theory" in which intersectionality can imply a disadvantage either greater or less than additive effects is problematic because it is untestable, although there may be more specific tests of some hypotheses about why intersectionality can reduce penalties. To correctly interpret our analysis (and that of many preceding papers) in light of this expanded view of intersectionality, we use the label "intersectional discrimination" rather than "intersectionality," to clarify that we are testing for additional negative impacts (i.e., greater than additive impacts) of intersecting identities.

III. Testing for Intersectional Discrimination

Specifications and hypothesis tests

We study intersectional discrimination based on gender, race (Black or White), ethnicity (Hispanic or non-Hispanic), sexual orientation (gay/lesbian), and disability, based on data from the American Community Survey that are described in more detail later.[13] To simplify, we use regressions with dummy

[12] Alternatively, individuals who are in multiple protected groups may face an advantage, if the devaluation of workers along both dimensions is similar, so that a worker devalued along one dimension is not also devalued along the other (as discussed in McGuire and Reskin, 1993, and Kim, 2009).

[13] We do not consider age in our analysis, because typically older workers earn more than other workers conditional on observables, at least over a large age range. Economists explain the age-earnings profile via the human capital model (e.g., Mincer, 1974) or work incentives (Lazear, 1979). Sociological work on the age-earnings profile has acknowledged the role of learning emphasized by the human capital model, but also discussed changes in physical vs. other demands as workers age, the relationship between who does more physical labor and their education, and the role of institutions – in particular, unions – that weaken the relationship between wages and productivity implied by competitive labor markets (Stolzenberg, 1975). Because older workers tend to be paid more, they have generally not been the focus of studies of pay discrimination, although there is a good deal of research on hiring discrimination based on age (see the survey in Neumark, 2018).

variables and interactions rather than Oaxaca (1973)-Blinder (1973) (henceforth "O-B") decompositions that allow all of the regression coefficients to differ by group. With the large number of groups considered, and multi-way interactions, the latter would become very non-transparent, in part because the "answer" about race, gender, etc., differences can vary with the different values of the control variables. In addition, there is an indeterminacy in O-B decompositions because we do not know the true non-discriminatory wage structure (Neumark, 1988). As we discuss below, however, this difference in regression specification is not germane to our results nor to comparisons with other research on intersectional discrimination.[14]

We estimate all interactions up to 3-way interactions, but not 4- and 5-way interactions. While in principle the latter can be defined, given that we study five groups the cell sizes become tiny, as shown later.[15] Thus, for a generic outcome y (most often log wages), we estimate models of the form:

$$\begin{aligned} y = {} & a + \beta_B B + \beta_F F + \beta_H H + \beta_G G + \beta_D D \\ & + \beta_{BF} B \cdot F + \beta_{BH} B \cdot H + \beta_{BG} B \cdot G + \beta_{BD} B \cdot D + \beta_{FH} F \cdot H + \beta_{FG} F \cdot G \\ & + \beta_{FD} F \cdot D + \beta_{HG} H \cdot G + \beta_{HD} H \cdot D + \beta_{GD} G \cdot D \\ & + \beta_{BFH} B \cdot F \cdot H + \beta_{BFG} B \cdot F \cdot G + \beta_{BFD} B \cdot F \cdot D + \beta_{FHG} F \cdot H \cdot G + \beta_{FHD} F \cdot H \cdot D \\ & + \beta_{FGD} F \cdot G \cdot D + \beta_{BHG} B \cdot H \cdot G + \beta_{BHD} B \cdot H \cdot D + \beta_{BGD} B \cdot G \cdot D + \beta_{HGD} H \cdot G \cdot D \\ & + X\gamma + \varepsilon. \end{aligned} \tag{1}$$

We define the different effects of interest, used in reporting the results from these regressions below, as follows:

Main effects: We define the "main effects" as the estimated coefficients of the dummy variables for a single category (e.g., β_B, β_F).[16] When we refer to main effects of belonging to two (or three) categories, we mean the sum of the main effects. For example, for Black females, the main effect is:

$$\beta_B + \beta_F, \tag{2}$$

and for female Hispanic gays/lesbians, it is:

[14] And we present some selectivity-corrected wage equation estimates, for which the implementation of O-B decompositions is unclear (Neumann and Oaxaca, 2004).

[15] For example, although our main sample size is nearly 1.5 million observations, there are only 4 observations that are Black, Hispanic, gay/lesbian, and disabled.

[16] By "effects," we mean the common econometric parlance to describe dummy variables and interactions – e.g., "main effects." We do not mean "treatment effects" in the causal outcomes framework.

$$\beta_F + \beta_H + \beta_G.$$[17] (3)

2-way effects: When we refer to "2-way effects" for individuals defined by two categories, we mean the effects of a 2-way interaction (e.g., for Black females, β_{BF}). When we refer to "2-way" effects for individuals defined by three categories, we mean the sums of the effects of the three 2-way interactions covering these three groups. For example, for Black females who are also disabled, the 2-way effect is:

$$\beta_{BF} + \beta_{BD} + \beta_{FD}. \tag{4}$$

3-way effects: When we refer to "3-way effects," we mean the effects of a 3-way interaction. For example, for Black females who are disabled the 3-way effect is β_{BFD}.

Total 2-way effects: Total 2-way effects are the sums of the main and 2-way effects. For example, for Black females this is:

$$\beta_B + \beta_F + \beta_{BF}. \tag{5}$$

Total 3-way effects: Total 3-way effects are the sums of the main, 2-way, and 3-way effects. For example, for Black females who are disabled this is:

$$\beta_B + \beta_F + \beta_D + \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD}. \tag{6}$$

2-way – main effects: This refers to the difference between the total 2-way effects and main effects corresponding to the same two groups. For example, for Black females this is:

$$\beta_B + \beta_F + \beta_{BF} - (\beta_B + \beta_F) = \beta_{BF}. \tag{7}$$

By the definition of "main effects" and "total 2-way effects," these equal the 2-way effects.

3-way – main effects: This refers to the difference between the total 3-way effects and the main effects corresponding to the same three groups. For example, for Black females who are disabled this is:

$$\beta_B + \beta_F + \beta_D + \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD} - (\beta_B + \beta_F + \beta_D) \tag{8}$$
$$= \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD}.$$

In terms of testing for intersectional discrimination, the key parameters – when we consider up to three intersecting categories simultaneously – are the estimates of "3-way – main effects." These measure

[17] We could, of course, parameterize our model using single dummy variables for each of these groups. But the parameterization we use yields direct tests of the relevant interactions from the regressions.

the extent to which labor market penalties exceed the penalties associated with each category independently – if in fact they do. That is, if an estimated "3-way – main effect" parameter is negative, the combined penalty for membership in all three groups exceeds the penalty associated with each group in isolation – the core prediction of intersectional labor market discrimination.[18]

We estimate wage models with and without corrections for sample selection bias. We also estimate the wage models with and without controls for industry and occupation, given that hiring into industry and occupation could potentially reflect discrimination making them bad controls (Angrist and Pischke, 2008), and with a restriction to those who work closer to full-time/full-year. Finally, in addition to wage equations, we estimate linear probability models for employment, although the wage estimates are our primary focus.

Related research

We have identified seven studies that test whether interactions between dummy variables for marginalized groups indicate labor market penalties that are greater than the sum of the effects of membership in each group considered individually. Our research is differentiated by extending the analysis of intersectional discrimination to more groups than have usually been considered, and by exploring issues of selection bias using both older and newer methods. In addition, however, we identify a striking puzzle from the prior studies – specifically, that most of them use similar methods and data, yet sometimes reach diametrically opposite conclusions. It turns out that the answer is simply that some of the papers simply do the calculation incorrectly and hence arrive at the wrong answer.

For example, Kim (2009) studies whether Black women face a larger earnings penalty than would be implied by the sum of the race and gender penalty, using 2002 Current Population Survey Annual Demographic Survey (i.e., the Annual Social and Economic Supplements (ASEC) file). She uses O-B decompositions, estimating separate regressions, computes the share of the pay gap between groups explained by differences in coefficients, and then does the calculation based on these decompositions to generate residual estimates of wage discrimination, concluding that the evidence is consistent with

[18] We also reported limited results considering only 2-way intersectional discrimination, in which case, correspondingly, the key parameter is "2-way – main effects," and a negative estimate is, again, consistent with intersectional discrimination.

intersectional discrimination.[19] To illustrate, in one calculation (p. 480), the gender penalty is 15 percent, the race penalty is 9 percent, but the penalty for Black women relative to White men is 27 percent, with the 3 percentage point difference statistically significant, consistent with intersectional wage discrimination.[20] George et al. (2022) consider both race (Black) and ethnicity (Hispanic), using Decennial Census data from 1980, 1990, and 2000, and 2010 and 2019 American Community Survey (ACS) data, also using O-B decompositions and studying weekly wages. They find similar results, with evidence of a wage penalty for black women greater than the individual gender and racial gaps, and they find similar results for Hispanic women. Paul et al. (2022) study changes in the findings over time (1980-2017), using CPS (ASEC) data, and studying hourly wages using O-B decompositions. They also conclude that Black women experience an intersectional wage penalty that is larger than the sum of the separate race and female penalties. And White (2023) extends similar analyses to Hispanics, immigrants, American Indian or Alaska natives, Asian or Pacific islanders, and multi-racial groups, using ACS data through 2020 and estimating models for hourly wages (estimated with ACS data using information on hours and weeks worked). For the majority of groups he considers, he reports evidence consistent with intersectional discrimination (e.g., for Black females who are non-Hispanic and native-born), with the estimated effect minus the expected effect under the null of no intersectional discrimination (based on the sums of separate effects) exceeding 5 percentage points (the threshold he considers).

Curiously, though, other studies using similar data and approaches reach the opposite conclusion. Greenman and Xie (2008) study 2000 Decennial Census data, estimating models for log earnings with dummy variables for a large set of race/ethnic groups, for both women and men (i.e., with a large set of race/ethnicity-by-gender interactions). For almost all groups, their results indicate that minority women's earnings are higher than predicted by the sum of the separate dummy variables, inconsistent with

[19] "[B]lack women are underpaid because of their race and gender and … the sum of these two penalties is less than the total amount of the earnings penalties they face" (p. 466).

[20] She finds that when restricted to full-time/year-round workers, and including industry or occupation controls, the interaction effect is very close to zero and not statistically significant. But the same qualitative difference persists using all workers but with the additional controls. (See her Table 4.)

intersectional discrimination.[21] Mandel and Semyonov (2016) explore the same question with Census and ACS data covering 1970-2010.[22] Although they use decompositions rather than the simpler dummy variable specifications, their results are consistent with the results of Greenman and Xie (2008) in finding that the pay penalty for Black women is less than the sum of the race and gender penalties. Indeed, they report little remaining earnings disparity, since 1980, between Black and White women.[23] The evidence from all of these papers is summarized in Table 1.

While at first puzzling, it is easy to show that the first set of papers – those concluding that there is evidence of intersectional discrimination – simply get the wrong answer. Most of these papers focus on race and gender, so consider the following regression, where *B* and *F* are dummy variables for Black and female, *y* is the log wage, and *X* is a vector of other controls:

$$y = a + \beta_B B + \beta_F F + \beta_{BF} B \cdot F + X\gamma + \varepsilon. \tag{9}$$

As discussed earlier, the test for intersectional discrimination is whether $\beta_{BF} < 0$. We can instead write the expressions for the conditional means of *y* for each group:

$$E(y|B = 0, F = 0, X) = \mu_{WM} = a + X\gamma \tag{10a}$$

$$E(y|B = 1, F = 0, X) = \mu_{BM} = a + \beta_B + X\gamma, \tag{10b}$$

$$E(y|B = 0, F = 1, X) = \mu_{WF} = a + \beta_F + X\gamma, \tag{10c}$$

$$E(y|B = 1, F = 1, X) = \mu_{BF} = a + \beta_B + \beta_F + \beta_{BF} + X\gamma. \tag{10d}$$

Then, by substituting terms from (10a)-(10d), $\beta_{BF} < 0$ is equivalent to:

$$\mu_{BF} - \mu_{WM} - \{(\mu_{BM} - \mu_{WM}) + (\mu_{WF} - \mu_{WM})\} < 0. \tag{11}$$

[21] They describe their results as confirming intersectionality in the sense that "there is no such thing as a pure "gender effect" or "race effect" when it comes to earnings" (p. 1236). While that is true, they are clear that their evidence is not consistent with intersectional discrimination as the research literature on labor market outcomes has framed it. (This discussion reflects our earlier point that a theory of intersectionality that could predict either positive or negative interactions is not a very compelling theory.)

[22] They "predict" evidence against the intersectional discrimination hypothesis for Black women because, in their view, negative stereotypes of Black men are more pronounced.

[23] McGuire and Reskin (1993) consider this question without the explicit framing of intersectional discrimination. Using data from a 1980 telephone survey, they conclude that the earnings penalty for Black women "exceeded that imposed on either Black men and (sic) white women, but was less than their sum … inconsistent with the hypothesis that the cost of being a Black woman exceeds the combined separate negative effects of race and sex" (p. 499). This evidence would, in the more modern framing, be viewed as inconsistent with intersectional discrimination. In the discussion that follows we focus on the other papers, as McGuire and Reskin (1993) use very different data.

In words, the pay difference between Black women and White men is larger than the sum of the pay difference between Black men and White men plus the pay difference between White women and White men – exactly the intersectional discrimination hypothesis for race and gender.

Equation (11) can be rewritten in different ways:

$$\mu_{BF} - \mu_{BM} - (\mu_{WF} - \mu_{WM}) < 0 \tag{12a}$$

$$\mu_{BF} - \mu_{WF} - (\mu_{BM} - \mu_{WM}) < 0. \tag{12b}$$

Each of these, also, reflects the same idea of intersectional discrimination. In equation (12a), if there is intersectional discrimination, the wage gap between Black women and Black men exceeds the wage gap between White women and White men – precisely because Black women not only experience the same gender pay gap as White women, but an additional pay gap relative to Black men (and hence the combined gap is larger than the sum of the gender gap and the race gap).[24] The same applies to equation (12b), but here the interpretation is that Black women experience not only the same race gap as Black men, but an additional pay penalty relative to White women. The studies by Kim (2009), George et al. (2022), Paul et al. (2022), and White (2023) use O-B decompositions instead of regressions, but the idea underlying the calculations they do is equivalent.[25]

To relate these calculations to the papers summarized above, it is easiest to start with Panel B of Table 1 – the papers that do *not* find evidence against intersectional discrimination. Greenman and Xie (2008) use a regression approach just like the one described above, and estimate the coefficient on the Black-female interaction. Given the equivalence of this to equations (12a) or (12b) above, the entry for the calculation they do (in the "Calculation of intersectional discrimination" column) is the same as equation (12a), although here in the table we write this more generically as:

(BF – BM) – (WF – WM), (13)

[24] The expression is negative since both $(\mu_{BF} - \mu_{BM})$ and $(\mu_{WF} - \mu_{WM})$ are negative.

[25] The first three studies consider race and gender, while White (2023) considers more categories. But the issue and computations are exactly the same in all cases, so we just describe the evidence that follows in terms of race and gender.

because other papers use O-B decompositions for which these terms represent a slightly more complex calculation than just the differences in conditional means described above. For this study, the value of equation (13) they estimate is 0.14 – the opposite of the sign predicted by intersectional discrimination.

Also in Panel B, Mandel and Semyonov estimate O-B decompositions, from which they compute the unexplained wage gap between White and Black men (0.114) and between White and Black women (0.043). The difference between these (also in the "Calculation of intersectional discrimination" column) is positive (0.071), implying that the race gap for women is smaller than the race gap for men – again the opposite of what is predicted by intersectional discrimination.[26] Both of these papers conclude that there is evidence *against* intersectional discrimination, and these conclusions are correct based on the empirical analysis.[27]

However, for the four studies in Panel A, the authors test for intersectional discrimination incorrectly and reach the opposite conclusion to what the data show. For example, Kim (2009) uses O-B decompositions and computes an unexplained wage gap between White and Black women of 0.09 (in logs). The gap between Black men and Black women is 0.15, and the gap between White men and Black women is 0.27. She concludes that there is evidence of intersectional discrimination because 0.27 exceeds (0.09 + 0.15). But note that this computation is:

$$(WM - BF) - [(WF - BF) + (BM - BF)] = 0.03 > 0. \tag{14}$$

The left-hand side of equation (14) can be rewritten in two ways:

$$(BF - BM) - (WF - WM) \tag{15a}$$

or

$$(BF - WF) - (BM - WM). \tag{15b}$$

Equations (15a) and (15b) correspond to equations (12a) and (12b), but note that these expressions are *positive* (0.03), which is the opposite of the prediction of intersectional discrimination. In words, equation (15a) being positive implies that the gender gap for blacks is smaller than the gender gap for whites (because both (BF – BM) and (WF – WM) are negative). And similarly, equation (15b) being

[26] The entry for these calculations is equivalent to equation (12b).

[27] Appendix Table A1 provides the interpretation of the estimates provided by the authors of each paper in Table 1.

positive implies that the race gap for women is smaller than the race gap for men (again, because both differences are negative). These are both exactly the *opposite* of what intersectional discrimination predicts.

The other three papers claiming evidence of intersectional discrimination (George et al., 2022; Paul et al., 2022; White et al., 2023) make exactly the same error.[28] Rather, all four papers do the wrong computation to test for intersectional discrimination. If we use the estimates from these papers but compute the correct expression to test for intersectional discrimination, the estimates yield evidence against intersectional discrimination (and quite similar estimates). This is explained in the last column of Table 1, which notes that the calculation in each paper boils down to either equation (15a) or (15b), and its value is positive, *inconsistent* with intersectional labor market discrimination.

It is useful to clarify this a bit further, because some readers of our paper have suggested that the expression in equation (14) is simply an alternative way to use the different estimated wage gaps to test for intersectional discrimination. One might think that a positive value for equation (14) implies intersectional discrimination because the wage gap between White males and Black females exceeds the sum of the race gap for women (WF – BF) and the gender gap for Blacks (BM – BF).

So why, in more general terms, does

$$(WM - BF) > [(WF - BF) + (BM - BF)] \tag{16}$$

actually imply that there is not intersectional wage discrimination, while

$$(WM - BF) > [(WM - BM) + (WM - WF)] \tag{17}$$

(paralleling equation (11)) implies there is?[29] After all, the left-hand side is the same, and both equations (16) and (17) have a race gap and a gender gap on the right-hand side.

One way to see the answer is to re-arrange the terms in equation (16), yielding

$$(WM - BM) > (WF - BF), \tag{18}$$

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[28] This has nothing to do with using O-B decompositions vs. simpler regressions. We show this in the Appendix (Appendix Tables A2 and A3), where we report estimates focusing on race and gender only, as in most of the papers discussed in this section. We first use O-B decompositions, and then a regression, with very similar results. These estimates, like all the estimates discussed in this section, when computed correctly, indicate evidence against intersectional discrimination.

[29] Note that in this discussion we have flipped the order of the terms in the differences (and thus also the inequality), corresponding to the analysis in Kim (2009), for example.

which clearly is the opposite of intersectional discrimination. In contrast, re-arranging equation (17) implies

$$(\text{WM} - \text{BM}) < (\text{WF} - \text{BF}), \qquad (19)$$

which is intersectional discrimination. Clearly, then, equations (16) and (17) are not two different expressions that both reflect intersectional discrimination.[30,31]

Thus, despite the contrary conclusion some of these studies state, all of them do in fact yield similar evidence against intersectional discrimination with regard to race and gender.[32] By the way, there is earlier evidence in the labor economics literature that does not couch the interpretation in terms of intersectional discrimination, but nonetheless finds evidence against intersectional discrimination. In a highly-cited paper on race differences in earnings, Neal and Johnson (1996, Table 1) report larger racial pay gaps for black men than for black women.[33]

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[30] Indeed, we can interpret this same algebra in a different way that perhaps makes the point even more sharply. In particular, equation (16) can be obtained from starting with the *opposite* of intersectional discrimination, (WM – BM) > (WF – BF) or equivalently (WM – WF) > (BM – BF), adding and subtracting BF on the same side, and re-arranging terms. Clearly, then, equation (16) cannot imply intersectional discrimination. In contrast, equation (17) results from starting *with* intersectional discrimination, (WM – BM) < (WF – BF) or equivalently (WM – WF) < (BM – BF), adding and subtracting WM on the same side and re-arranging terms, so equation (17) *does* reflect intersectional discrimination.

Intuitively, the problem is that the right-hand terms of equation (16) are larger than those of equation (17) when there is intersectional discrimination. That is, (WF – BF) > (WM – BM) and (BM – BF) > (WM – WF), because intersectional discrimination implies that women are penalized for race more than men (the first inequality) and Blacks are penalized more for gender than Whites (the second inequality). So, if there is intersectional discrimination because equation (17) holds, then the positive additions to the right-hand side of equation (16) can flip the inequality – and the algebra above shows that they do.

[31] Another assertion readers have made is that the differences result from using dummy variables and interactions in a single regression vs. separate regressions for each group and O-B decompositions. However, this has no bearing on the issue. Decompositions just give us an estimate of the discriminatory wage gap between any two groups. With the regression model, these are just given by (equations (10a)-(10d), simplifying by treating the β's as estimates):

BF – WM = $\beta_B + \beta_F + \beta_{BF}$
WF – WM = β_F
BF – BM = $\beta_F + \beta_{BF}$
BF – WF = $\beta_B + \beta_{BF}$.

Using decompositions, these gaps are measured differently, running separate regressions with coefficient vectors β_{WM}, β_{WF}, β_{BM}, and β_{BF}. Then in each case, we have (using β_{WM} as the "no-discrimination wage structure":

BF – WM = $\overline{X_{BF}} \cdot (\beta_{BF} - \beta_{WM})$ or $\overline{X_{WM}} \cdot (\beta_{BF} - \beta_{WM})$.

The expressions for the other three differences (WF – WM, BF – BM, and BF – WF) follow, so that using decompositions just provides alternative estimates of the differences between each of the four groups, but the argument about the errors in the calculations in some of the prior studies remains the same.

[32] White (2023) studies many more groups, but the calculations always reflect the same error.

[33] That is, (BM – WM) – (BF – WF) < 0. Rewriting, this implies, for example, (BF – BM) – (WF – WM) > 0, the opposite of equation (12a).

IV. Data

We use the ACS from 2019 through 2021 (Ruggles et al. 2024). The ACS is a nationally representative dataset with a very large sample size that allows us to examine the labor market outcomes of individuals at the intersection of many different demographic categories among those we consider: gender, Black, Hispanic, disability, and same-sex married couples or cohabitants. We limit our analysis to only White or Black (both categories of which can include Hispanics).[34] We restrict our sample to ages 25 or above and less than 55. The former restriction is intended to avoid confounding labor market outcomes with schooling, and the latter to avoid confounding with differential effects of aging across groups (although these differences merit further study). We exclude observations with inconsistent earnings and employment status (i.e., reporting earnings, but that they did not work), allocated earnings, individuals in group quarters, and the self-employed.

Consistent with a vast literature on labor market discrimination, we focus on hourly wages as the outcome of interest, rather than a broader earnings measure, such as annual (or weekly or monthly) earnings. We do this because the hourly wage reflects the price of labor in the labor market, whereas the earnings measures can also reflect labor supply variation that in turn can reflect preferences.[35] Going back to Becker (1971), discrimination is conceptualized as a difference between the wage and the marginal revenue product of a unit of labor (such as an hour); and the principle alternative model – statistical discrimination – similarly focuses on the wage reflecting employers' expectations about workers productivity per unit of time worked. Correspondingly, we believe, the analysis of discrimination in terms of wages pervades the labor economics literature.

ACS respondents report annual earnings (pre-tax wage and salary income), as well as usual hours worked per week and number of weeks worked. We calculate hourly wages using usual hours worked per

[34] We imposed some restrictions on groups considered for tractability; additional research could extend our analysis to even more groups.

[35] In contrast, we doubt there is any first-order argument for why members of a particular group might prefer lower wages, although some of the work cited earlier (Goldin, 2014) draws on a long-running view that women, at least, may be systematically trading off wages for job conditions (for contrary evidence, however, see Jacobs and Steinberg, 1990; see also the rejoinder by Filer, 1990).

week, number of weeks worked, and earnings. However, the ACS does not report the detailed number of weeks the respondent worked before 2019, but rather only categories,[36] meaning that for the data before 2019 we cannot calculate wages very accurately – which is why we start the analysis in 2019.

The ACS does not ask directly about respondents' sexual orientation, so we rely on household structure and gender information to identify same-sex married or cohabiting individuals. We code an individual to be gay or lesbian if the gender of the head of the household and the gender of the spouse or unmarried partner are the same.[37] We define disabled as reporting having one or more of cognitive, ambulatory, independent living, self-care, vision, or hearing difficulties.[38] In all our analyses, we control for education, potential experience, potential experience squared, and state and year fixed effects.

We report descriptive statistics for logs and levels of wages, and group membership, in Table 2. (The table also reports descriptive statistics for employment, which we also examine briefly for reasons explained later.) Looking at log wages (on which we focus the most), the top panel indicates that women earn lower hourly wages than men (included in "all"), Blacks, Hispanics, and the disabled earn lower wages, and gays/lesbians earn higher wages. The second panel gives the means for the 2-way interactions, and the third panel for the 3-way interactions. We defer describing the core results until we get to the regression estimates, which include controls for human capital and other measures. Note that, consistent with the discussion above, we estimate models with up to 3-way interactions despite considering five marginalized categories. We do this because, despite the very large sample, there are very few observations on those falling into four (or five) of these potentially marginalized categories (see Appendix Table A4).

[36] The ranges are: 50-52 weeks, 48-49 weeks, 40-47 weeks, 27-39 weeks, 14-26 weeks, and 13 weeks or fewer.

[37] The ACS has fewer miscoding issues than commonly known issues associated with the Decennial Census. See Burn (2020) for a more detailed discussion.

[38] The survey questions include: "Because of a physical, mental, or emotional condition, does this person have serious difficulty concentrating, remembering, or making decisions?"; "Does this person have serious difficulty walking or climbing stairs?"; "Because of a physical, mental, or emotional condition, does this person have difficulty doing errands alone such as visiting a doctor's office or shopping?"; "Does this person have difficulty dressing or bathing?"; "Is this person deaf or does he/she have serious difficulty hearing?"; and "Is this person blind or does he/she have serious difficulty seeing even when wearing glasses?" Affirmative responses to any of these result in coding as disabled.

In some checks on the robustness of the wage equation results, we restrict the sample to full-time-full-year workers, defined as those who usually worked at least 35 hours per week and more than 27 weeks. We also report estimates from models with occupation and industry controls, which are based on two-digit Census codes (23 and 24 unique categories, respectively).

V. Results

Main results

We first present our analysis of log wages using OLS estimates, reporting evidence that closely corresponds to most of the research cited above. The difference is that we consider many more potential dimensions of intersectional discrimination. The results are reported in Table 3. In the column labeled "Main," we report the coefficients described in equations (2) and (3). These are the estimates of the dummy variables for a single category (e.g., β_B, the estimated coefficient of the dummy for Blacks), or the sums of these estimates (e.g., $\beta_B + \beta_F$, corresponding to Black Female in this column of the table). The "2-way" estimates reported in the table correspond to equation (4), which are the sums of the coefficients on the products of two dummy variables in equation (1). The "3-way estimates" are the estimated coefficients of the triple interactions in equation (1). The "Total 2-way" and "Total 3-way" estimates add the main and interactive effects, as in equations (5) and (6).

Finally, the key estimates – hence the highlighting – are those labeled "2-way – main" and "3-way – main" are the parameters that we estimate to test for intersectional discrimination – i.e., the additional impact on wages of belonging to multiple potentially marginalized groups. It is useful to work through an example in each column. First, consider the estimate of 13.4 (standard error = 0.4) for Black Females. This estimate is the sum of the main and 2-way effects (or the "Total 2-way" effects) minus the main effects, or exactly what is given in equation (7). Writing this out, we have −26.4 – 28.0 + 13.4 – (–26.4 – 28.0) = 13.4. As noted earlier, in this case, the estimate is the same as what the "2-way" column reports. However, this is not the case for the "3-way – main" estimates. In this case, as equation (8) shows, we have to add the 2-way and 3-way fixed effects, with the full expression being, for the Black Female Hispanic example, −26.4 – 28.0 – 14.8 + 13.4 + 17.5 + 3.4 – 5.1 – (– 26.4 – 28.0 – 14.8) = 29.3 (without rounding).

What do the results show overall? In every case but two, the highlighted estimates for "2-way – main" and "3-way – main," the estimates are positive, rather than negative, which is evidence *against* intersectional discrimination. Moreover, among the 18 positive estimates, all but two are statistically significant at the 5% level. (And the two negative estimates, for Hispanic Gay/Lesbian, and for Gay/Lesbian disabled, are very small and statistically insignificant.) This evidence, even with the expanded marginalized groups considered, is thus broadly consistent with the prior research largely focusing on race and gender – once the estimates of intersectional discrimination from that prior research are computed correctly.[39]

There is one critical point to make, though. The estimates in Table 3 make clear that each marginalized group earns lower wages than White males. That is, the evidence in this table against intersectional labor market discrimination does not mean that marginalized workers do not experience discrimination. Moreover, they can experience worse discrimination than groups marginalized on only a single dimension (or fewer dimensions) because the sums of, say, the Black and female wage penalties, even when the Black-female interaction is positive (as opposed to negative, as predicted by intersectional discrimination), can exceed either of the separate Black or female penalties. Rather, what the evidence implies is that the labor market disadvantage associated with belonging to multiple marginalized groups is not exacerbated by intersectionality, and if anything, this disadvantage tends to be mitigated.

This same evidence from Table 3 is displayed graphically for all groups in Figure 1, with the upper panel reporting the "2-way – main" estimates of intersectional discrimination, and the lower panel reporting the "3-way – main" estimates. The shading conveys information on statistical significance, with the solid bars indicating statistical significance at the 5% level. This visual impression from this figure makes clear, in our view, how strong and consistent the evidence is against intersectional discrimination in wages.

[39] Because the 2020 and 2021 data partly capture the period of labor market shutdowns owing to Covid, we have checked that the core estimates and conclusions are robust to using only 2019 data. See Appendix Table A5.

We next turn to a variety of different estimation methods and specifications to explore the robustness of our findings. In general, our results continue to point to evidence against intersectional discrimination in wages.

Selection-corrected estimates

Ever since the seminal contributions of Gronau (1973) and Heckman (1979), it has been well-known that regression estimates of wage differences between groups can be biased if there is differential selection into paid work. For example, if fewer lower-productivity women than men (conditional on the observables) choose to work, the wage penalty for women may be biased towards zero. We estimate two versions of the specifications just estimated. The first excludes "employment shifters" from the model for wages – in particular, controls for whether the person has children, including interactions with the demographic controls (including their 2-way and 3-way interactions). The implicit assumption is that children affect home productivity (potentially differently by demographic groups) but not market wages.[40]

Labor economists typically trust selection-corrected estimates more with an exclusion restriction like this, because absent one, the model is identified only off of the assumed distribution of the error terms (Olsen, 1980). However, to explore the robustness of the results, we also report estimates using the same wage equation controls as in Table 3 (i.e., without exclusion restrictions).

We report these results graphically in Figures 2 and 3.[41] The resulting estimates are very similar. In both figures, nearly all of the "2-way intersectional – main" estimates are positive and significant, and only

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[40] The employment – or selection – equation should always include all of the variables from the wage equation, given that the underlying model is one where a person compares wages in the labor market to home productivity (Heckman, 1974). To be clear, though, the exclusion restrictions in these settings can be suspect, and we do not want to push hard on ours. However, other examples are clearly worse. Of the studies discussed in Section II, White (2023) estimates models with Heckman selection corrections (1979). However, this analysis appears problematic. First, he includes marital status and number of children in the selection equation, as well as an indicator for owning a home, and for whether English is commonly spoken at home. The rationale for the "homeowner" control and English language controls is unclear. For example, we would think that language affects market wages (and perhaps does not affect shadow wages). He also appears to include a linear school measure in the selection equation, despite using dummy variables in the wage equations. Since the selection equation should be based on market vs. shadow wages, the model specification is hard to rationalize. It is also unclear what the sample is; Table 3.6 seems to indicate very small percentages of workers not being in the wage sample (e.g., 2.3% for black females), and also does not seem to indicate any non-working white males. Finally, as discussed by Neumann and Oaxaca (2004), O-B decompositions with sample selection corrections introduce complications and ambiguities.

[41] The full regressions are reported in Appendix Tables A6 and A7.

for one case (gay/lesbian disabled) in Figure 3 is there a marginally significant negative effect. More important, every 3-way version of these estimates is positive, most are significant, and the estimated magnitudes are aligned with Figure 1. Thus, the results and conclusions are very robust to correcting for sample selection.

We also explore an alternative method of estimating selection models without an "instrument" (i.e., an excluded variable), developed by D'Haultfœuille et al. (2018). This approach relies on the assumption that as the value of the outcome (in our case, wages) grows, its effect on selection dominates any effects of the covariates. This is referred to as an "independence-at-infinity" condition or assumption, and version of it has been used in a well-known paper on the gender pay gap (Mulligan and Rubinstein, 2008). With this assumption, selection-corrected estimates can be obtained without the distributional and functional form assumptions underlying the Heckman (1979) approach. It also allows for heterogeneous effects of the controls not of direct interest. However, the coefficient of the core variables of interest – Black, female, etc., and the interactions – are assumed to be the same at each quantile. With that assumption, plus independence-at-infinity, one can identify and estimate the coefficients on the demographic controls and interactions from the quantiles at which the identification assumption holds. The estimator chooses the quantiles to use trading off bias (smaller when the quantile is restricted to be higher) and variance (larger the more restrictive the quantile).

We implement this estimator using the *eqregsel* package developed by D'Haultfœuille et al. (2020). There are a couple of complications. First, implementing this model with 3-way interactions led to some highly imprecise results, with standard errors on the 3-way interactions of around 15-20, compared to generally less than 5 (see Table 3, and note that the log wage coefficients are scaled by 100). With many point estimates of interest in a range below 10 or 20, estimates this imprecise are clearly not informative. We suspect this problem comes from the combination of relying on small cell sizes generally for estimating 3-way effects, coupled with relying only on high quantiles. As a result, for this analysis, we focus only on the 2-way interactions, although even the 2-way estimates are often much less precise. Second, the complicated code for statistical inference based on bootstrapping does not make it possible to compute the standard errors of linear combinations of coefficients from this model. Thus, the only estimates for which we can do inference are the main effects for single demographic categories, and the 2-way effects for all 2-way interactions between them – although recall that for the 2-way interactions, these are equal to the 2-way minus main effects, which are the estimates of interest for testing for intersectional wage discrimination.[42]

We report the results in Table 4, where we first report the OLS estimates of the model only including 2-way interactions, and then, in the last columns, report the selection-corrected estimates based on independence at infinity. The first feature of these estimates that is obvious is that the standard errors are much larger than those for the OLS estimates, by about an order of magnitude. The estimates of the interaction coefficients are particularly imprecise, with standard errors centered on 8-10 percent differences in wages. The second feature is that seven of the ten estimates are positive, and two of the three negative estimates are insignificant. The evidence is less one-sided than the OLS or standard selection-corrected estimates. But, especially given the imprecision of the selection-corrected estimates in Table 4, we do not interpret this evidence as overturning our conclusion that there is little or no evidence consistent with intersectional wage discrimination, and a good deal more evidence that is inconsistent with it.

Conditioning on industry and occupation

We next explore whether conditioning on industry and occupation has an impact on our conclusions. To the extent that discrimination influences the industries or occupations in which people work, these are "bad controls" (Angrist and Pischke, 2008) in the sense that they may reflect the very discrimination whose impact on Blacks, females, etc., we are trying to estimate. The estimates are summarized in Figure 4.[43] Comparing Figure 4 to Figure 1, the pattern of estimates is very similar, and

[42] In principle, we can run the model additional times, reparameterized, to get standard errors on all of the estimates for the 2-way specification (main, 2-way, and total 2-way in Table 3). There are two challenges with this approach in this setting. First, this command adopts a data-driven method to choose a quantile index, which uses subsampling as part of its algorithm. Since the code does not provide an option to set seeds, we could not obtain the exact same coefficients in reparameterized specifications. Second, each run takes approximately one month on a 13th Gen Intel Core i9- 3.00 GHz processor with 32 GB of RAM using Stata/MP 18 8-core. Given that we can obtain the key estimates for testing for intersectional wage discrimination from the parameterization we estimate, there is little substantive additional information to be gained from estimating the different reparameterized models.

[43] The full model estimates are reported in Appendix Table A8.

particularly so for the 3-way minus main estimates in the lower panel. There is, again, much evidence inconsistent with intersectional discrimination in wages, and essentially no evidence consistent with such discrimination.

Employment

We next present results for employment as the outcome. We do this to connect with the extensive literature on discrimination in hiring and hence employment, covering race, ethnicity, gender, disability, sexual orientation, and more (e.g., Ameri et al., 2018; Gaddis, 2018; Neumark, 2018; Pager, 2007), while recognizing that this evidence is generally experimental and hence more successfully isolates employer behavior and abstracts from extensive-margin labor supply decisions.

The employment results, presented graphically, are reported in Figure 5.[44] The 2-way intersectional minus main estimates in the top panel are relatively mixed, with more and larger (in absolute value) positive estimates, but also four negative and significant estimates. However, once we define groups along three dimensions, in the bottom panel, the evidence looks similar to wages, with almost no evidence consistent with intersectional discrimination (here, in employment) – with the exception of Black, Hispanic, and Disabled – and most of the evidence in the other direction.[45]

VI. Conclusions

We conceptualize and measure intersectional discrimination in the labor market as implying that groups potentially marginalized on more than one dimension experience labor market penalties that exceed the sum of the penalties for groups defined along one dimension. For example, in a regression of wages on race and gender, intersectional discrimination in wages would imply that the wage penalty for being Black and female exceeds the separate race and gender wage penalties. This approach, while clearly not capturing all of what scholars mean by "intersectional discrimination," does follow a tradition of empirical tests for

[44] Full regression results are reported in Appendix Table A9.

[45] For completeness, we also estimated models for hours and earnings. As reported in Appendix Figure A1, the evidence for hours is even stronger in the latter direction; in particular, in the bottom panel all estimates but one are positive and significant at the 5% level or 10% level (one case), and the one estimate that is not positive is near zero. For earnings (Appendix Figure A2), there is very one-sided evidence against intersectional discrimination – now measured via overall earnings. One might view the earnings results as providing the most comprehensive evidence on labor market disadvantage or advantage, although we might view the wage results as more clearly reflecting discrimination, as discussed earlier. However, the conclusion is the same.

intersectional discrimination in the labor market. We look specifically at race, ethnicity, gender, disability, and sexual orientation, and consider definitions of marginalization up to the triple interactions of these variables – e.g., Black, female, and disabled. We focus mainly on wages, but also present evidence for employment. For wages, we address in alternative ways the potential endogenous selection of people into paid work, which can in principle impact estimated wage differentials between groups defined along one or more dimensions.

Our evidence is quite clear. There is little or no evidence consistent with intersectional discrimination in wages, or in the other labor market outcomes we study. In contrast, most of the evidence is in the other direction, with estimates of wage penalties or other shortfalls for doubly- or triply-marginalized groups being smaller than the sum of the individual group penalties.

We also review a number of prior papers testing for intersectional discrimination in wages in the same way as we do, although most are restricted to just race and gender. These papers present a puzzle: they use quite similar data, but some reach diametrically opposite conclusions from the others. We show, however, that the papers finding evidence consistent with intersectional wage discrimination use calculations for their tests that are incorrect, and when properly computed, the studies *all* point in the same direction. Thus, the combined conclusions from the prior research, properly evaluated, and our research, all point in the direction of little or no evidence on intersectional wage discrimination, and instead towards rather strong evidence against the hypothesis.

We also want to reiterate what this evidence does and does not imply. Evidence against intersectional labor market discrimination does not mean that workers marginalized among multiple dimensions do not experience worse discrimination compared with non-marginalized workers (in our context, White males). Moreover, they can experience worse discrimination than groups marginalized on only a single dimension (or fewer dimensions) because the sums of, say, the Black and female wage penalties, even when the Black-female interaction is positive (as opposed to negative, as predicted by intersectional discrimination), can exceed either of the separate Black or female penalties.[46] However, the evidence suggests that the labor market disadvantage associated with belonging to multiple marginalized groups is not exacerbated by intersectionality, and if anything, this disadvantage tends to be mitigated.

Finally, scholars of labor market discrimination have developed more compelling tests for discrimination than the residual wage regression approach – most notably experimental studies of hiring discrimination. The latter type of evidence has a narrower focus because of the limits of where experiments can be used. Nonetheless, it would be fascinating to develop evidence on intersectional discrimination using these methods to see if our conclusions hold or differ. One challenge, though, is the difficulty of running a sufficiently large experiment to obtain statistically precise estimates of hiring ("callback") differences between groups marginalized on different dimensions of group membership. However, some recent correspondence studies of hiring discrimination have been run at a far larger scale than past work (Neumark et al., 2019; Kline et al., 2022), so this may be possible with either new experiments or re-analysis of data from these or other correspondence studies.[47]

[46] Moreover, in relation to enforcement of anti-discrimination laws, our results do not imply that individual companies do not engage in intersectional discrimination. For suggestive evidence that some do, see Berry et al. (forthcoming).

[47] Indeed, although not their focus, Kline et al. test for interactions across a combination of applicant characteristics and some traditional characterizations of marginalized groups. With regard to race (Black vs. White) and sexual orientation – two groups for which callback rates are lower – they report evidence against intersectional discrimination in callbacks. And Di Stasio and Larsen (2020) study data from a correspondence study conducted in five countries and estimated interactions between race/ethnicity and gender, as well as the percent female in the types of jobs to which they applied (to study the impact of "role congruity"); they are more focused on how different race/ethnicity/gender groups fare in more vs. less female jobs, making it hard to draw clear conclusions about intersectional discrimination.

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Table 1: Evidence on Intersectional Discrimination in the Existing Literature

Source	Unexplained wage gaps reported	Calculation of intersectional discrimination	Estimate presented vs. correct conclusion
A. Studies claiming evidence of intersectional discrimination			
Kim (2009), Tables 3-4	O-B decomp's: WF – BF = .09 BM – BF = .15 WM – BF = .27	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .03	Positive estimate indicates: gender gap for whites > gender gap for blacks; race gap for men > race gap for women; **Correct conclusion:** ***evidence against intersectional discrimination*.**
George et al. (2022), Table 4 (Year 2019 estimates)	O-B decomp.'s: WM – WF = .208 BM – BF = .136 WF – BF = .089 WM – BF = .299	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .074	Positive estimate indicates: race gap for men > race gap for women; gender gap for whites > gender gap for blacks. **Correct conclusion:** ***evidence against intersectional discrimination*.**
Paul et al. (2022), Tables 2-4	O-B decomp.'s: WM – BM = .132 WF – BF = .074 WM – WF = .176 BM – BF = .104 WM – BF = .224	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .046	Positive estimate indicates: race gap for men > race gap for women; gender gap for whites > gender gap for blacks. **Correct conclusion:** ***evidence against intersectional discrimination*.**
White (2023), Tables 4.2, 5.1, Figure 4.1, eq. 3.9 (native-born, non-Hispanic)	O-B decomp.'s: WF – BF = .0595 BM – BF = .0617 WM – BF = .2485	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .1273	Positive estimate indicates: race gap for men > race gap for women; gender gap for whites > gender gap for blacks. **Correct conclusion:** ***evidence against intersectional discrimination*.**
B. Studies *not* claiming evidence of intersectional discrimination			
Greenman and Xie (2008), Table 1	Interaction of dummy variable for black and female in log pay regression (exponential of coefficient – 1) = .14	Interaction coefficient = (BF – BM) – (WF – WM) = .14	Positive estimate in this case implies: gender gap for blacks < gender gap for whites; race gap for women < race gap for men. **Correct conclusion:** ***evidence against intersectional discrimination.***
Mandel and Semyonov (2016), Table 1 (Year 2010)	O-B decomp.'s: WM – BM = .114 WF – BF = .043	(WM – BM) – (WF – BF) = (BF – WF) – (BM – WM) = .071	Race gap for women < race gap for men. **Correct conclusion:** ***evidence against intersectional discrimination.***

Note: For the Kim (2009), George et al. (2020), and Paul et al. (2022) papers, the second column uses the estimates referenced in the text in the third column describing the results in relation to intersectional discrimination. We do not use the μ symbols as in the text because the differences here (e.g., BF – BM) refer to the unexplained gap between groups accounting for both intercepts and differences in regression coefficients, when O-B decompositions are used, as they are in most of these papers. See Appendix Table A1 for text from the papers explaining their calculations and conclusions.

Table 2: Means of Outcome Variables

	Log Wage	Hourly Wage	Employed
	(1)	(2)	(3)
All	3.22	31.79	0.81
White Male	3.35	38.24	0.87
Black	2.99	24.22	0.74
Female	3.12	26.95	0.76
Hispanic	3.01	24.78	0.77
Gay/Lesbian	3.31	36.35	0.87
Disabled	2.92	15.18	0.41
2-way			
Black Female	2.95	23.36	0.74
Black Hispanic	3.00	24.55	0.79
Black Gay/Lesbian	3.05	27.36	0.82
Black Disabled	2.76	11.15	0.32
Female Hispanic	2.93	20.62	0.69
Female Gay/Lesbian	3.21	32.36	0.86
Female Disabled	2.83	13.67	0.39
Hispanic Gay/Lesbian	3.11	29.54	0.85
Hispanic Disabled	2.85	13.45	0.44
Gay/Lesbian Disabled	2.99	20.98	0.57
3-way			
Black Female Hispanic	2.98	24.01	0.77
Black Female Gay/Lesbian	2.97	27.06	0.82
Black Female Disabled	2.74	12.53	0.35
Female Hispanic Gay/Lesbian	3.03	27.19	0.84
Female Hispanic Disabled	2.78	12.73	0.43
Female Gay/Lesbian Disabled	2.93	20.07	0.57
Black Hispanic Gay/Lesbian	3.01	31.11	0.76
Black Hispanic Disabled	2.92	17.90	0.44
Black Gay/Lesbian Disabled	3.02	23.05	0.49
Hispanic Gay/Lesbian Disabled	2.92	17.77	0.60

Notes: The sample size for column (1) is 1,497,372 and columns (2)-(5) is 1,779,539. We use ACS 2019-2021 because ACS does not report detailed weeks worked until 2019. We exclude self-employed, individuals living in group quarters, and individuals with inconsistent earnings and employment status, or allocated earnings. We restrict our sample to ages 25-54 and include only Whites or Blacks (Hispanics can be in either group). We define disabled as one who reports having a cognitive, ambulatory, independent living, self-care, vision, or hearing difficulty (the main text provides the survey questions). We define gay or lesbian as same-sex married or partners in the household.

Table 3: Log Wages, 3-Way Interactions, OLS

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way – main (= 2-way)	SE	3-way – main	SE
Black	-26.4	(0.3)												
Female	-28.0	(0.1)												
Hispanic	-14.8	(0.3)												
Gay/Lesbian	-4.5	(0.7)												
Disabled	-23.8	(0.5)												
Black Female	-54.5	(0.4)	13.4	(0.4)			-41.1	(0.3)			13.4	(0.4)		
Black Hispanic	-41.3	(0.5)	17.5	(1.7)			-23.7	(1.6)			17.5	(1.7)		
Black Gay/Lesbian	-31.0	(0.8)	6.8	(2.8)			-24.2	(2.7)			6.8	(2.8)		
Black Disabled	-50.3	(0.6)	5.2	(1.6)			-45.1	(1.5)			5.2	(1.6)		
Female Hispanic	-42.9	(0.4)	3.4	(0.5)			-39.5	(0.3)			3.4	(0.5)		
Female Gay/Lesbian	-32.6	(0.8)	10.9	(1.0)			-21.6	(0.7)			10.9	(1.0)		
Female Disabled	-51.9	(0.5)	4.3	(0.7)			-47.6	(0.5)			4.3	(0.7)		
Hispanic Gay/Lesbian	-19.4	(0.8)	-1.0	(2.6)			-20.3	(2.4)			-1.0	(2.6)		
Hispanic Disabled	-38.7	(0.6)	9.3	(1.7)			-29.3	(1.6)			9.3	(1.7)		
Gay/Lesbian Disabled	-28.4	(0.9)	-0.2	(3.3)			-28.5	(3.2)			-0.2	(3.3)		
Black Female Hispanic	-69.3	(0.5)	34.3	(1.9)	-5.1	(2.3)			-40.0	(1.5)			29.3	(1.6)
Black Female Gay/Lesbian	-59.0	(0.9)	31.1	(3.2)	-8.7	(3.7)			-36.6	(2.4)			22.4	(2.6)
Black Female Disabled	-78.3	(0.6)	23.0	(2.0)	-3.5	(2.1)			-58.8	(1.3)			19.5	(1.4)
Female Hispanic Gay/Lesbian	-47.4	(0.9)	13.3	(3.0)	5.8	(3.6)			-28.3	(2.5)			19.1	(2.6)
Female Hispanic Disabled	-66.7	(0.6)	17.0	(2.0)	-1.6	(2.3)			-51.3	(1.6)			15.4	(1.7)
Female Gay/Lesbian Disabled	-56.4	(0.9)	15.1	(3.7)	0.5	(4.3)			-40.8	(2.9)			15.6	(3.1)
Black Hispanic Gay/Lesbian	-45.8	(0.9)	23.3	(4.3)	-8.1	(9.8)			-30.6	(10.0)			15.2	(10.0)
Black Hispanic Disabled	-65.1	(0.7)	32.1	(3.0)	4.2	(5.4)			-28.8	(5.5)			36.3	(5.6)
Black Gay/Lesbian Disabled	-54.8	(1.0)	11.8	(4.8)	16.3	(8.1)			-26.6	(8.7)			28.2	(8.8)
Hispanic Gay/Lesbian Disabled	-43.2	(1.0)	8.2	(4.6)	-1.6	(7.1)			-36.6	(7.1)			6.6	(7.2)

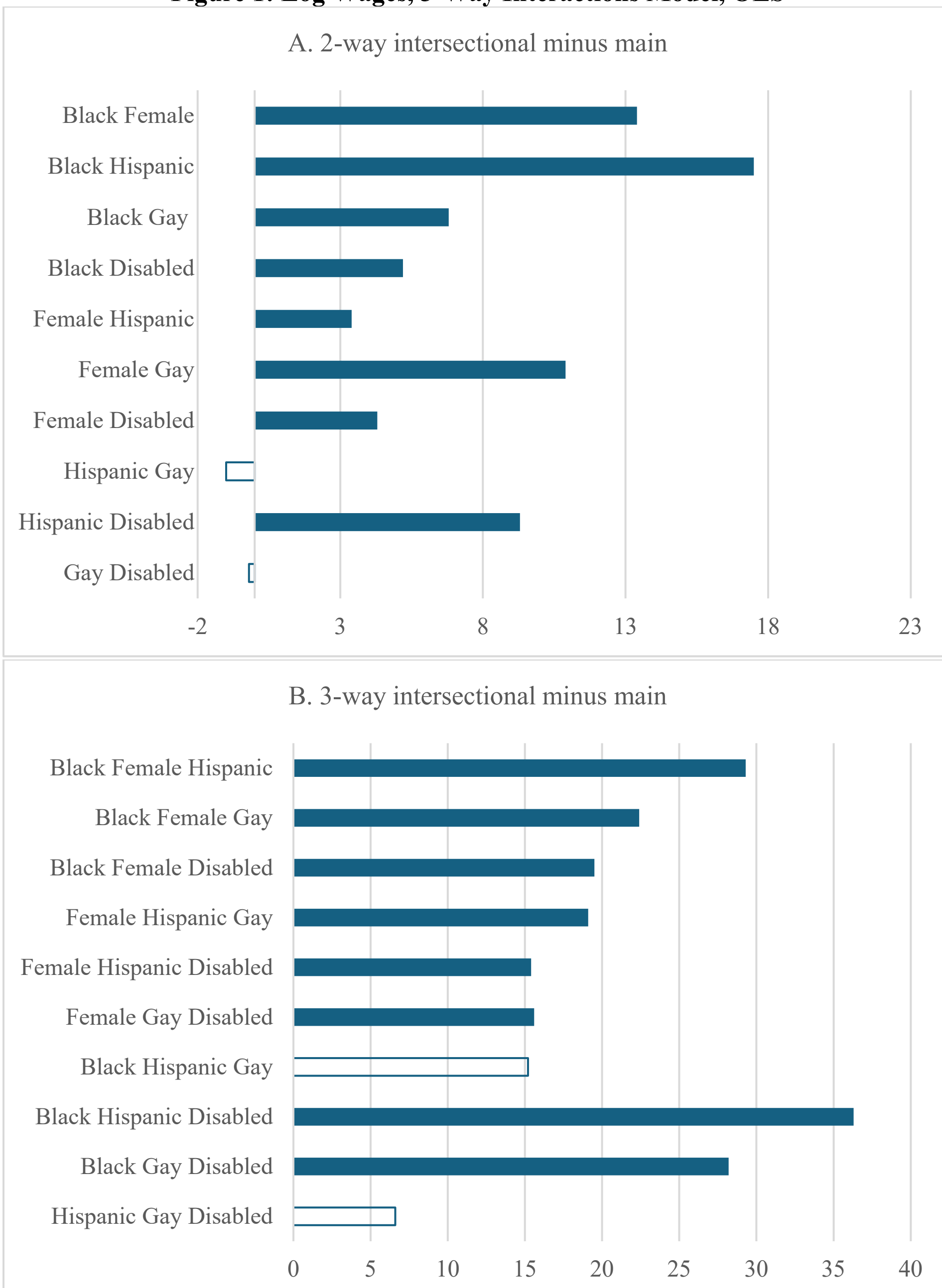
Notes: N = 1,497,372. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent). Notes to Table 2 apply. In all regressions we control for education, potential experience, potential experience squared, and state- and year-fixed effects. Potential experience is defined as age minus year of schooling minus seven. As explained in the main text, the "2-way effects" and "2-way effects – main" in the middle panel are the same; see equations (4) and (7), considering equation (4) for Black females only (as in the middle panel of the table).

Table 4: Log Wages, 2-Way Interactions Model, OLS and Selection-Corrected Based on Independence at Infinity

	OLS								Selection-Corrected			
	Main	SE	2-way	SE	Total 2-way	SE	2-way – main (= 2-way)	SE	Main	SE	2-way – main (= 2-way)	SE
Black	-26.2	(0.3)							-22.0	(1.7)		
Female	-28.0	(0.1)							-29.4	(0.6)		
Hispanic	-14.7	(0.3)							-13.6	(2.0)		
Gay/Lesbian	-4.5	(0.7)							-21.2	(6.1)		
Disabled	-23.7	(0.4)							-20.3	(2.8)		
Black Female	-54.2	(0.4)	12.9	(0.4)	-41.3	(0.3)	12.9	(0.4)			10.0	(2.4)
Black Hispanic	-40.9	(0.5)	14.8	(1.1)	-26.1	(1.1)	14.8	(1.7)			11.5	(9.7)
Black Gay/Lesbian	-30.7	(0.8)	2.3	(1.9)	-28.4	(1.9)	2.3	(2.8)			7.6	(11.4)
Black Disabled	-49.9	(0.6)	3.6	(1.0)	-46.2	(1.1)	3.6	(1.6)			-6.3	(8.0)
Female Hispanic	-42.7	(0.4)	3.2	(0.4)	-39.6	(0.3)	3.2	(0.5)			2.4	(3.0)
Female Gay/Lesbian	-32.5	(0.7)	10.8	(0.9)	-21.7	(0.7)	10.8	(1.0)			36.5	(8.0)
Female Disabled	-51.7	(0.5)	3.8	(0.6)	-47.8	(0.5)	3.8	(0.7)			8.4	(4.5)
Hispanic Gay/Lesbian	-19.2	(0.8)	1.2	(1.8)	-18.0	(1.8)	1.2	(2.6)			21.9	(11.7)
Hispanic Disabled	-38.4	(0.6)	8.7	(1.2)	-29.7	(1.2)	8.7	(1.7)			-10.2	(9.6)
Gay/Lesbian Disabled	-28.1	(0.8)	1.4	(2.1)	-26.7	(2.2)	1.4	(3.3)			-38.5	(13.0)

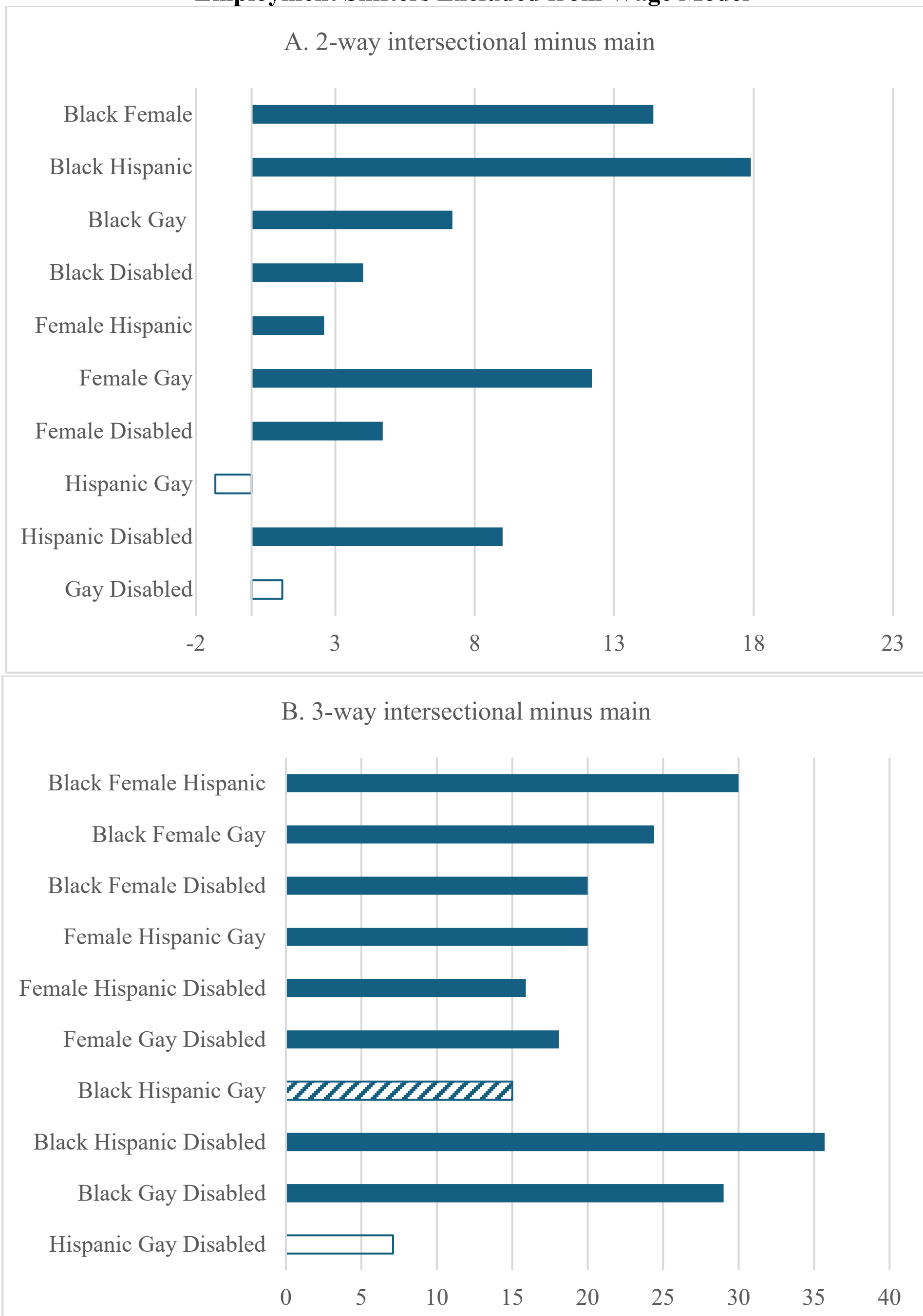
N = 1,497,372. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change(approximately 1 percent). Notes to Table 3 apply.

Figure 1: Log Wages, 3-Way Interactions Model, OLS



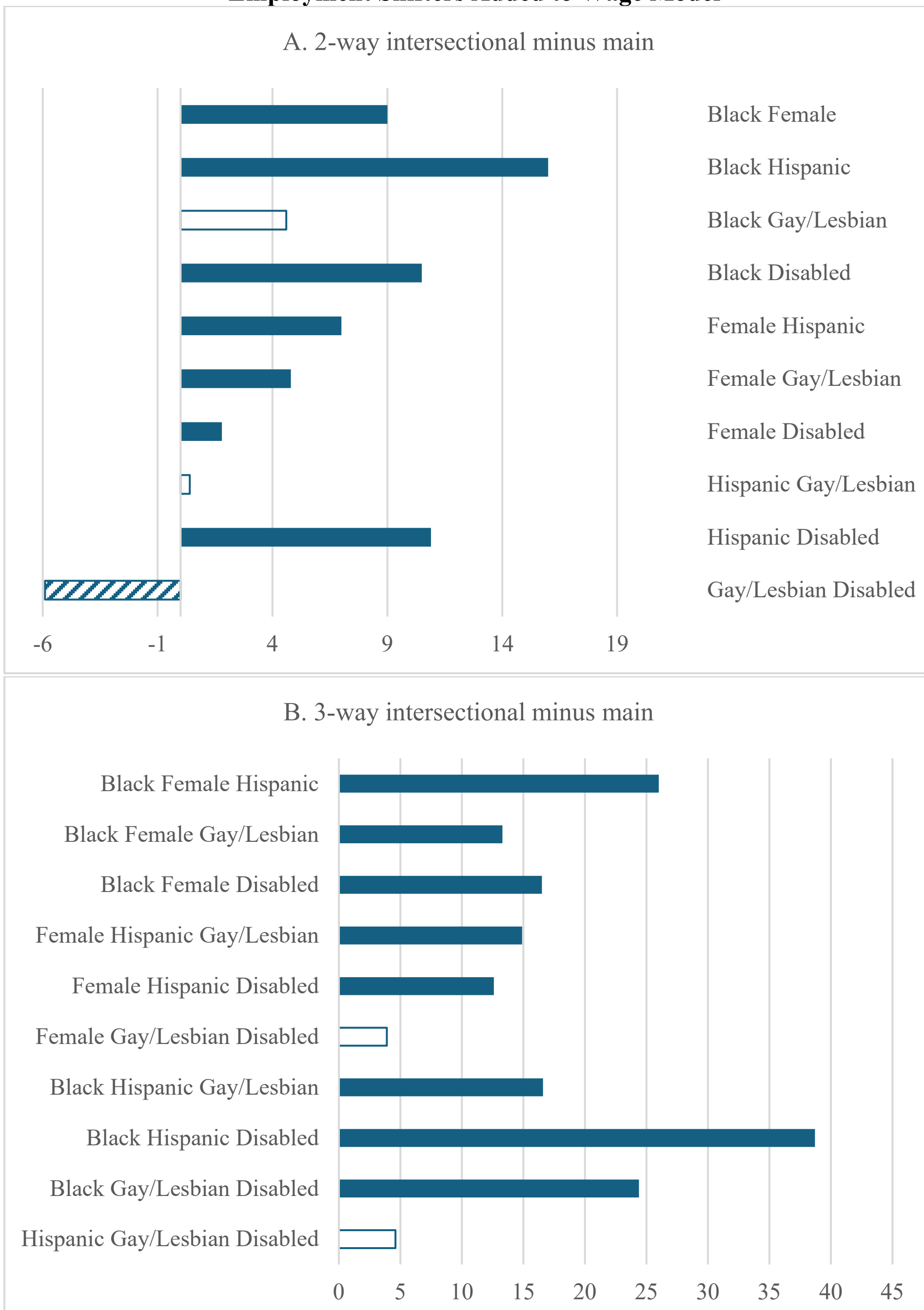
Note: Figure corresponds to Table 3. Solid bars: $p < .05$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

Figure 2: Log Wages, 3-Way Interactions, Sample-Selection Corrected with Employment Shifters Excluded from Wage Model



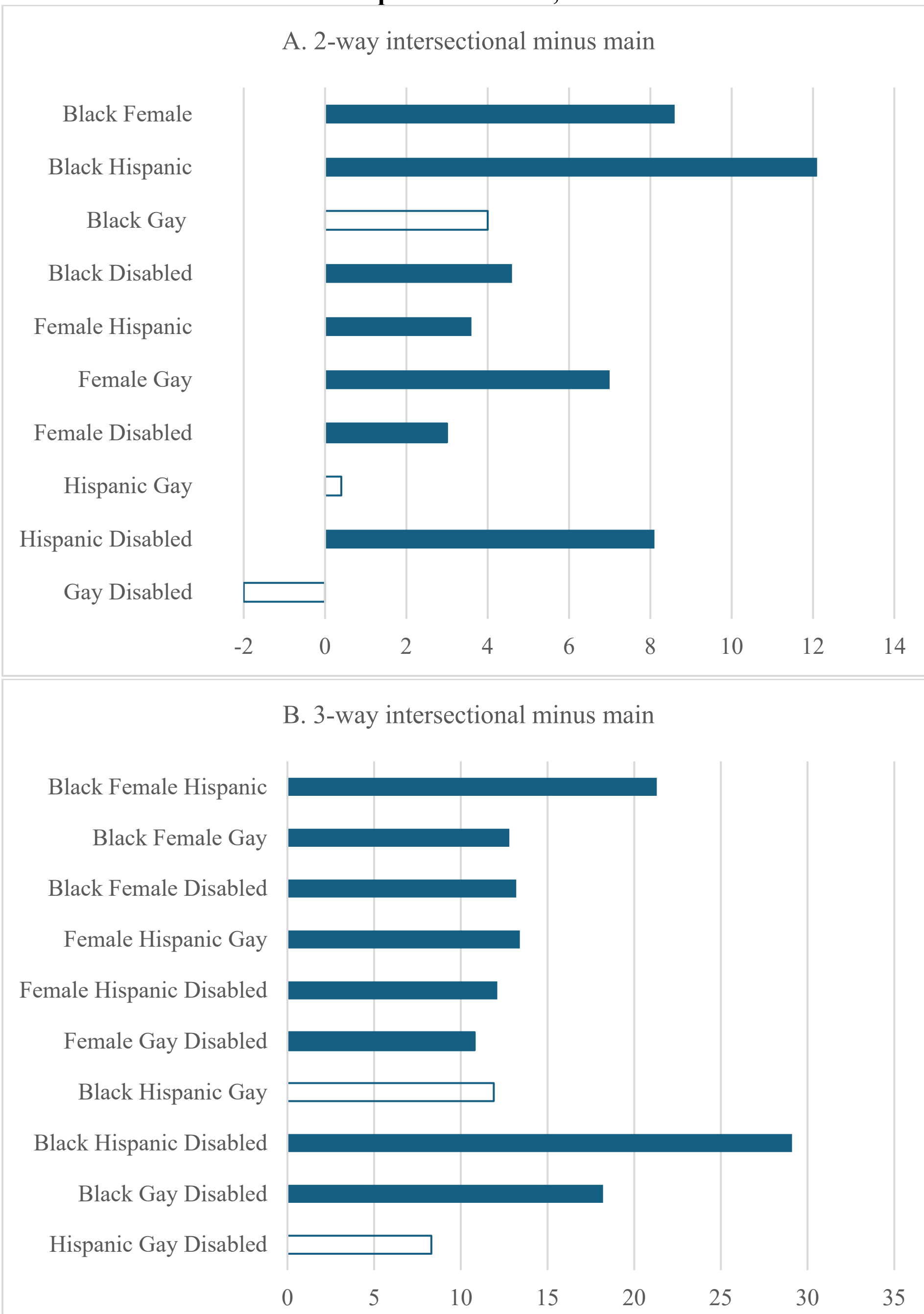
Note: Figure corresponds to Appendix Table A6. Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

Figure 3: Log Wages, 3-Way Interactions, Sample-Selection Corrected, No Employment Shifters Added to Wage Model



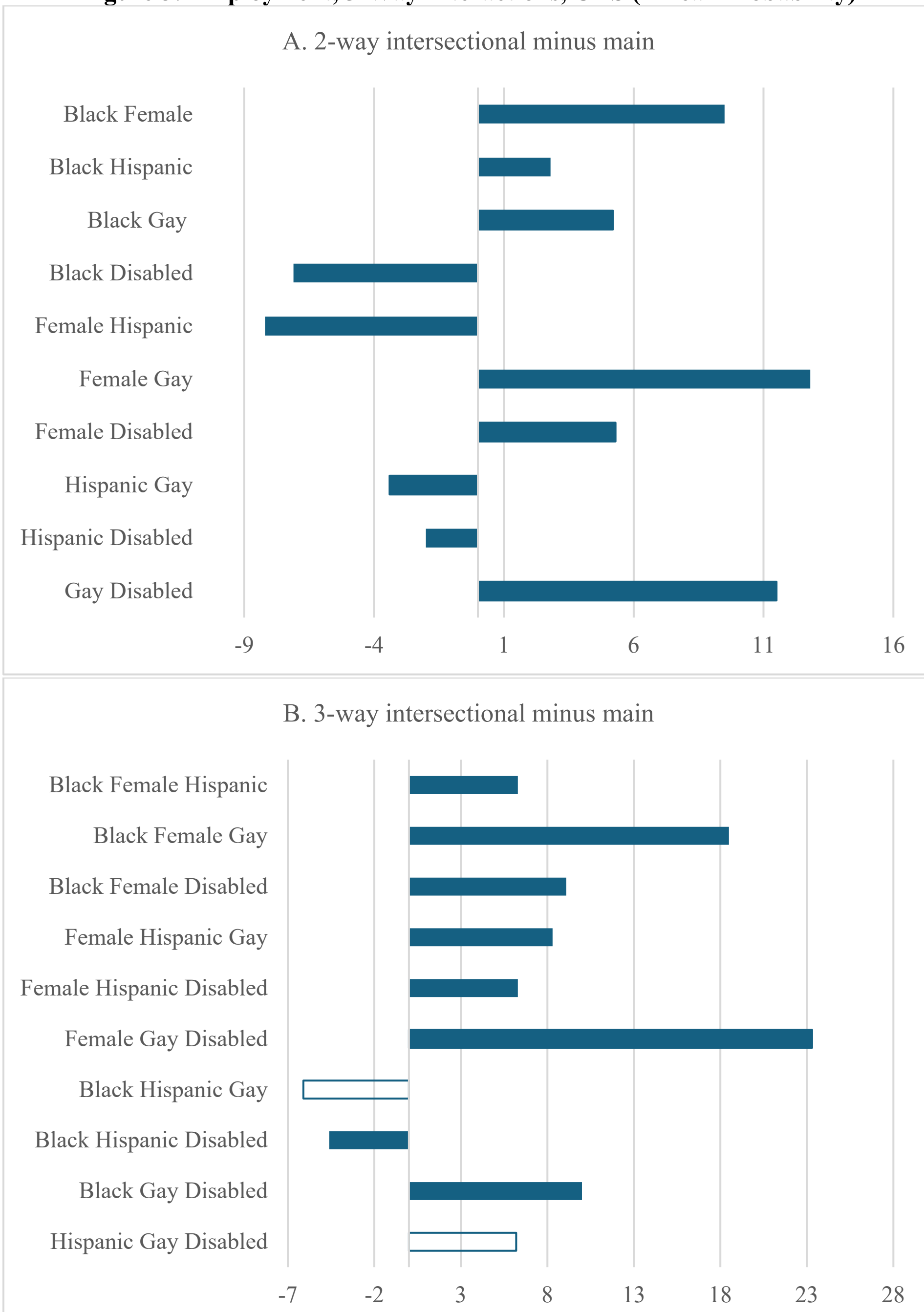
Note: Figure corresponds to Appendix Table A7. Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

Figure 4: Log Wages, 3-Way Interactions Model with 2-Digit Industry and Occupation Controls, OLS



Note: Figure corresponds to Appendix Table A8. Solid bars: p < .05; hollow bars: p > .1. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

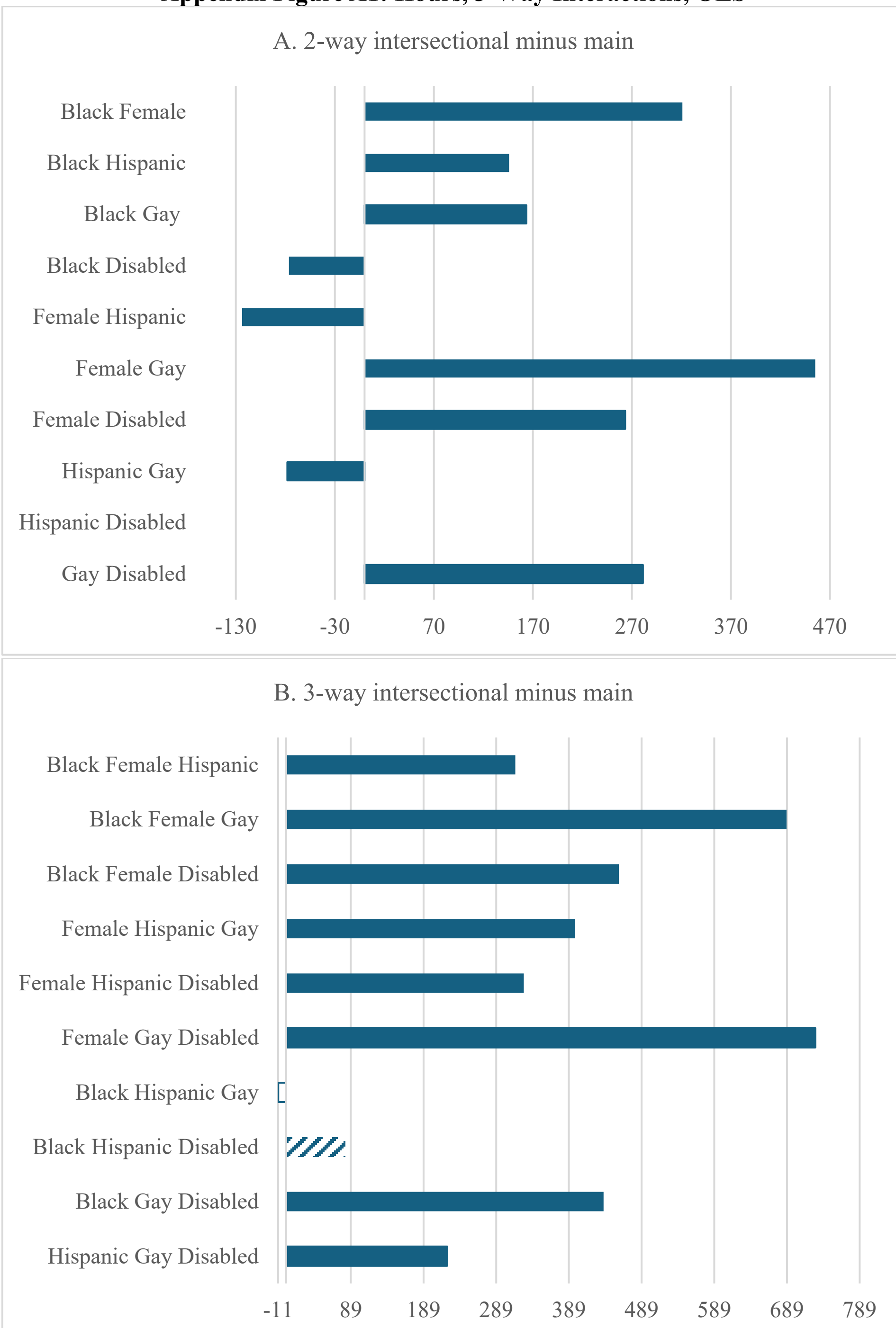
Figure 5: Employment, 3-Way Interactions, OLS (Linear Probability)



Note: Figure corresponds to Appendix Table A9. Solid bars: $p < .05$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 percentage point change. Employment is measured as any employment in the last 12 months.

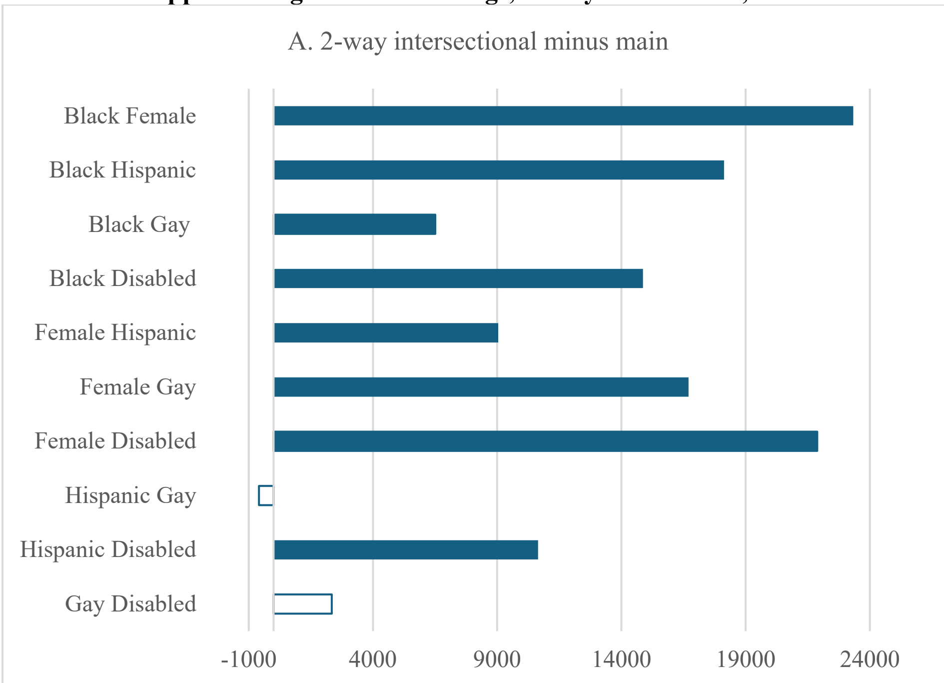
Appendix (Online)

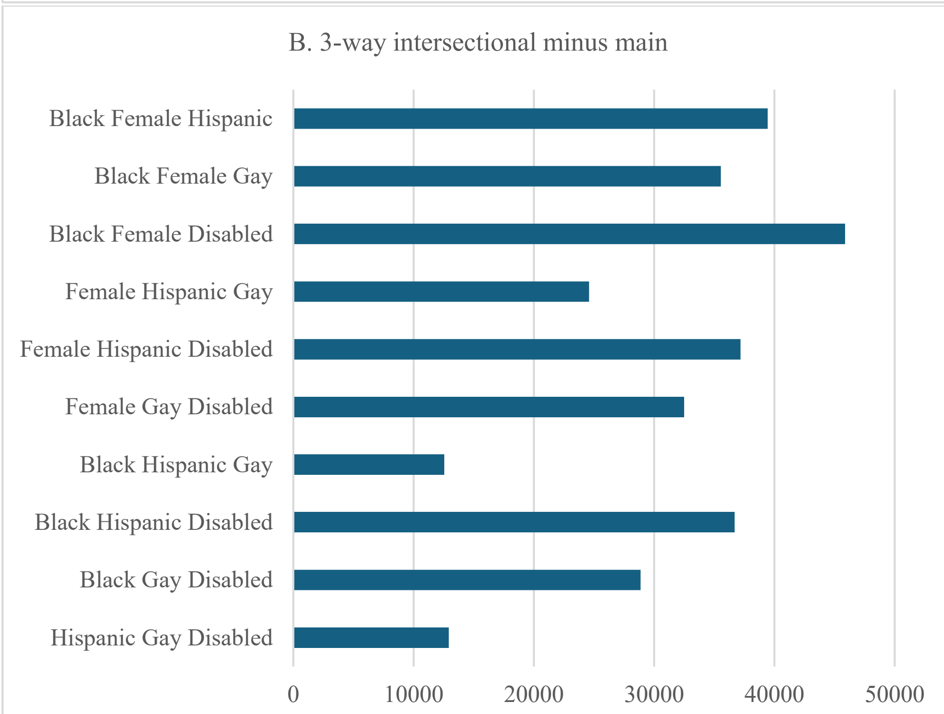
Appendix Figure A1: Hours, 3-Way Interactions, OLS



Note: Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Hours are annual.

Appendix Figures A2: Earnings, 3-Way Interactions, OLS





Note: Solid bars: p < .05; hollow bars: p > .1. Earnings are annual labor income.

Appendix Table A1: Evidence on Intersectional Discrimination in the Existing Literature

Source	Unexplained wage gaps reported	Interpretation	Calculation of intersectional discrimination
A. Studies claiming evidence of intersectional discrimination			
Kim (2009), Tables 3-4	O-B decomp's: WF – BF = .09 BM – BF = .15 WM – BF = .27	" … even if black women had the same amount of observable human capital as white men, they would still earn 27 percent less. This is surprising, since the estimates above generated gender penalties of 15 percent and race penalties of 9 percent… If in a simple world black women faced two distinct and separate penalties, one because of their gender and one because of their race, one would expect total wage gaps to be the simple arithmetic sum of the two, 24 percent… But … [t]here is an additional statistically significant but small wage penalty of 3 percent in addition to separate race and gender penalties. The results imply that black women face earnings penalties because of their race, a separate one because of their gender, plus an additional penalty due to the combination of both their race and gender… I will call this additional penalty the "race and gender interaction penalty." (p. 480) **Conclusion:** ***evidence of intersectional discrimination.***	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .03 **Correct conclusion:** ***evidence against intersectional discrimination.***
George et al. (2022), Table 4 (Year 2019 estimates)	O-B decomp.'s: WM – WF = .208 BM – BF = .136 WF – BF = .089 WM – BF = .299	"If we make the simplifying assumption that the unexplained Wage gap faced by Black women is additive …, we would expect the unexplained portion of the wage gap between Black women and White men to be 0.225 log points … This comes from summing the unexplained gender gap for Black workers (0.136 log points) and the unexplained race gap for female workers (0.089 log points) … [H]owever, the unexplained gap in wages between Black women and White men is always greater than the sum of the unexplained portions of the gender and racial wage gaps… Black women experience more than a double whammy, with a wage loss even greater than one would expect based on the individual unexplained gender and racial gaps." (p. 326) **Conclusion:** ***evidence of intersectional discrimination.***	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .074 **Correct conclusion:** ***evidence against intersectional discrimination.***
Paul et al. (2022), Tables 2-4	O-B decomp.'s: WM – BM = .132 WF – BF = .074 WM – WF = .176 BM – BF = .104 WM – BF = .224	"We would expect an unexplained wage gap between Black women and White men of 0.179 log points if the intra-gender racial wage penalty of 0.074 log points and intra-racial gender wage penalties of 0.104 log points were additively related to the intersectional wage penalties experienced by Black women. Instead, we find the unexplained portion of the wage gap between Black women and White men (0.224 log points) is larger than the sum of the two individual penalties… Simply stated, we confirm the existence of an intersectional race and gender penalty for earnings in the case of Black women." (pp. 17-18) **Conclusion:** ***evidence of intersectional discrimination.***	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .045 **Correct conclusion:** ***evidence against intersectional discrimination.***
White (2023), Tables 4.2, 5.1, Figure 4.1, eq. 3.9 (native-born, non-Hispanic)	O-B decomp.'s: WF – BF = .0595 BM – BF = .0617 WM – BF = .2485	"… to determine if the discrimination suffered by members of this worker group is additive or intersectional we must obtain separate discrimination rate estimates for race … and sex …, sum the values to produce the expected discrimination rate …, and compare the resulting value to our estimated discrimination rate… For the native-born, non-Hispanic, black, female worker group … since the estimated discrimination rate is 24.85% and the expected discrimination rate is 12.12%, we have evidence in support of intersectional discrimination." (pp. 112-113) **Conclusion:** ***evidence of intersectional discrimination.***	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .1273 **Correct conclusion:** ***evidence against intersectional discrimination.***

Source	Unexplained wage gaps reported	Interpretation	Calculation of intersectional discrimination
B. Studies *not* claiming evidence of intersectional discrimination			
Greenman and Xie (2008), Table 1	Interaction of dummy variable for black and female in log pay regression (exponential of coefficient – 1) = .14	"…the statistical interaction between being minority and being female is consistently positive: Among groups who are disadvantaged in terms of earnings relative to whites, the race penalty is always smaller among women than among men. Conversely, for all minority groups the gender penalty is smaller for minority women than white women. Thus, the "double jeopardy" characterization … is a poor description of minority women's earnings." (p. 1236) **Conclusion:** ***evidence against intersectional discrimination.***	Interaction coefficient = (BF – BM) – (WF – WM) = .14 **Correct conclusion:** ***evidence against intersectional discrimination.***
Mandel and Semyonov (2016), Table 1 (Year 2010)	O-B decomp.'s: WM – BM = .114 WF – BF = .043	"…the unexplained portion of the gap – the indicator we used as a proxy for racial discrimination – was much larger among men than among women… Although pointing to the intersection of gender and race, these findings do not support the double disadvantage hypothesis." (pp. 1057-1058) **Conclusion:** ***evidence against intersectional discrimination.***	(WM – BM) – (WF – BF) = .071 **Correct conclusion:** ***evidence against intersectional discrimination.***

Note: See notes to Table 1. We have combined the last two columns of Table 1, but also included the text from the papers explaining their calculation and reason for their conclusions about intersectional discrimination.

Appendix Table A2: Estimate to Test for Intersectional Discrimination by Race and Gender Using O-B Decompositions

Women (BF – WF)			Men (BM – WM)		
Total Gap	Explained	Unexplained	Total Gap	Explained	Unexplained
−0.185	−0.0676	−0.118	−0.318	−0.0586	−0.259
N = 738,618			N = 758,754		
Estimate to test for intersectional discrimination = −0.118 − (−0.259) = 0.140					

Notes: The controls include education, potential experience, potential experience squared, Hispanic, gay/lesbian, disabled, state, and year fixed effects.

Appendix Table A3: Estimate to Test for Intersectional Discrimination by Race and Gender Using Regression with Interaction

Female	−0.274 (0.001)
Black	−0.254 (0.003)
Female × Black (= Estimate to test for intersectional discrimination)	**0.129 (0.004)**
N	1,497,372

Notes: The regression includes the same controls in Appendix Table A2. The table reports estimated coefficients, with standard errors in parentheses.

Appendix Table A4: Cell Size, Wage Analysis Sample

n = 1,497,372	Count	Percent
Female	738,618	49.33
Black	135,181	9.03
Hispanic	103,299	6.90
Gay/Lesbian	21,655	1.45
Disabled	75,372	5.03
2-way		
Black Female	75,414	5.04
Black Hispanic	4,277	0.29
Black Gay/Lesbian	1,604	0.11
Black Disabled	8,091	0.54
Female Hispanic	51,248	3.42
Female Gay/Lesbian	11,200	0.75
Female Disabled	37,457	2.50
Hispanic Gay/Lesbian	1,675	0.11
Hispanic Disabled	4,891	0.33
Gay/Lesbian Disabled	1,395	0.09
3-way		
Black Female Hispanic	2,411	0.16
Black Female Gay/Lesbian	976	0.07
Black Female Disabled	4,804	0.32
Female Hispanic Gay/Lesbian	782	0.05
Female Hispanic Disabled	2,539	0.17
Female Gay/Lesbian Disabled	831	0.06
Black Hispanic Gay/Lesbian	77	0.01
Black Hispanic Disabled	309	0.02
Black Gay/Lesbian Disabled	116	0.01
Hispanic Gay/Lesbian Disabled	100	0.01
4-way		
Black Hispanic Gay/Lesbian Disabled	4	< 0.00
Female Hispanic Gay/Lesbian Disabled	60	< 0.00
Female Black Gay/Lesbian Disabled	76	< 0.00
Female Black Hispanic Disabled	190	< 0.00
Female Black Hispanic Gay/Lesbian	44	< 0.00
5-way		
Female Black Hispanic Gay/Lesbian Disabled	3	< 0.00

Appendix Table A5: Log Wages, 3-Way Interactions, OLS, 2019 Only

	2-way – main	SE	3-way – main	SE
Black Female	14.3	(0.7)		
Black Hispanic	17.5	(2.3)		
Black Gay/Lesbian	8.0	(4.2)		
Black Disabled	2.4	(2.7)		
Female Hispanic	3.4	(0.6)		
Female Gay/Lesbian	11.0	(1.6)		
Female Disabled	3.8	(1.0)		
Hispanic Gay/Lesbian	-7.5	(3.0)		
Hispanic Disabled	11.3	(2.2)		
Gay/Lesbian Disabled	-0.2	(5.7)		
Black Female Hispanic			-44.7	(8.3)
Black Female Gay/Lesbian			30.3	(2.3)
Black Female Disabled			24.7	(3.7)
Female Hispanic Gay/Lesbian			22.3	(2.2)
Female Hispanic Disabled			17.2	(3.3)
Female Gay/Lesbian Disabled			17.0	(2.1)
Black Hispanic Gay/Lesbian			9.6	(5.1)
Black Hispanic Disabled			7.9	(10.1)
Black Gay/Lesbian Disabled			34.1	(8.5)
Hispanic Gay/Lesbian Disabled			13.3	(10.1)

Notes: N = 600,840. See notes to Table 3.

Appendix Table A6: Log Wages, 3-Way Interactions, Sample-Selection Corrected with Employment Shifters Excluded from Wage Model

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way – main (= 2-way)	SE	3-way – main	SE
Black	-27.8	(0.3)												
Female	-30.5	(0.2)												
Hispanic	-14.1	(0.3)												
Gay/Lesbian	-5.0	(0.8)												
Disabled	-31.3	(0.6)												
Black Female	-58.2	(0.4)	15.2	(0.4)			-43.1	(0.3)			15.2	(0.4)		
Black Hispanic	-41.9	(0.5)	18.2	(1.7)			-23.7	(1.6)			18.2	(1.7)		
Black Gay/Lesbian	-32.8	(0.8)	7.7	(3.0)			-25.2	(2.9)			7.7	(3.0)		
Black Disabled	-59.0	(0.7)	3.0	(1.3)			-56.0	(1.4)			3	(1.3)		
Female Hispanic	-44.5	(0.4)	1.9	(0.5)			-42.6	(0.4)			1.9	(0.5)		
Female Gay/Lesbian	-35.5	(0.8)	13.4	(1.1)			-22.1	(0.7)			13.4	(1.1)		
Female Disabled	-61.7	(0.7)	5.1	(0.6)			-56.6	(0.7)			5.1	(0.6)		
Hispanic Gay/Lesbian	-19.1	(0.8)	-1.6	(2.5)			-20.7	(2.4)			-1.6	(2.5)		
Hispanic Disabled	-45.3	(0.7)	8.8	(1.5)			-36.6	(1.5)			8.8	(1.5)		
Gay/Lesbian Disabled	-36.3	(1.0)	2.1	(3.1)			-34.2	(3.0)			2.1	(3.1)		
Black Female Hispanic	-72.3	(0.6)	35.2	(1.9)	-4.7	(2.2)			-41.7	(1.5)			30.6	(1.6)
Black Female Gay/Lesbian	-63.3	(0.9)	36.2	(3.4)	-10.0	(3.7)			-37.1	(2.3)			26.1	(2.5)
Black Female Disabled	-89.5	(0.9)	23.3	(1.7)	-2.8	(1.7)			-69.0	(1.2)			20.5	(1.2)
Female Hispanic Gay/Lesbian	-49.6	(0.9)	13.7	(3.0)	7.0	(3.6)			-28.8	(2.6)			20.7	(2.7)
Female Hispanic Disabled	-75.8	(0.8)	15.8	(1.9)	0.5	(2.1)			-59.5	(1.5)			16.3	(1.5)
Female Gay/Lesbian Disabled	-66.7	(1.1)	20.6	(3.5)	-0.4	(4.0)			-46.5	(2.6)			20.2	(2.7)
Black Hispanic Gay/Lesbian	-46.9	(0.9)	24.3	(4.3)	-9.5	(8.4)			-32.1	(8.5)			14.8	(8.6)
Black Hispanic Disabled	-73.1	(0.8)	29.9	(2.7)	5.3	(4.3)			-37.9	(4.5)			35.2	(4.5)
Black Gay/Lesbian Disabled	-64.1	(1.1)	12.8	(4.5)	16.8	(7.1)			-34.4	(7.3)			29.6	(7.4)
Hispanic Gay/Lesbian Disabled	-50.4	(1.0)	9.3	(4.4)	-1.9	(7.5)			-42.9	(7.7)			7.5	(7.8)

Notes: N = 1,779,539. The model for employment, but not for log wages, includes controls for whether a person has children interacted with the main demographic controls and 2- and 3-way interactions. The two-step estimator is used. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent). Notes to Table 3 apply. Estimates correspond to Figure 2.

Appendix Table A7: Log Wages, 3-Way Interactions, Sample-Selection Corrected, No Employment Shifters Added to Wage Model

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way – main (= 2-way)	SE	3-way – main	SE
Black	-23.2	(0.4)												
Female	-22.0	(0.3)												
Hispanic	-16.7	(0.4)												
Gay/Lesbian	-3.3	(0.8)												
Disabled	-5.7	(0.9)												
Black Female	-45.2	(0.6)	9.0	(0.5)			-36.1	(0.4)			9.0	(0.5)		
Black Hispanic	-39.9	(0.5)	16.0	(1.7)			-24.0	(1.7)			16.0	(1.7)		
Black Gay/Lesbian	-26.5	(0.9)	4.6	(3.0)			-21.9	(2.9)			4.6	(3.0)		
Black Disabled	-28.9	(1.1)	10.5	(1.3)			-18.4	(1.7)			10.5	(1.3)		
Female Hispanic	-38.7	(0.4)	7.0	(0.5)			-31.7	(0.5)			7.0	(0.5)		
Female Gay/Lesbian	-25.2	(0.9)	4.8	(1.1)			-20.4	(0.8)			4.8	(1.1)		
Female Disabled	-27.6	(1.2)	1.8	(0.6)			-25.8	(1.0)			1.8	(0.6)		
Hispanic Gay/Lesbian	-20.0	(0.9)	0.4	(2.6)			-19.5	(2.4)			0.4	(2.6)		
Hispanic Disabled	-22.4	(0.9)	10.9	(1.5)			-11.5	(1.7)			10.9	(1.5)		
Gay/Lesbian Disabled	-8.9	(1.2)	-5.9	(3.2)			-14.8	(3.1)			-5.9	(3.2)		
Black Female Hispanic	-61.9	(0.6)	31.9	(2.0)	-5.9	(2.3)			-35.8	(1.5)			26.0	(1.6)
Black Female Gay/Lesbian	-48.4	(1.0)	18.5	(3.5)	-5.2	(3.8)			-35.1	(2.4)			13.3	(2.6)
Black Female Disabled	-50.8	(1.3)	21.3	(1.7)	-4.9	(1.7)			-34.4	(1.5)			16.5	(1.2)
Female Hispanic Gay/Lesbian	-41.9	(0.9)	12.2	(3.0)	2.7	(3.7)			-27.0	(2.7)			14.9	(2.8)
Female Hispanic Disabled	-44.3	(1.2)	19.7	(1.9)	-7.0	(2.1)			-31.7	(1.7)			12.6	(1.5)
Female Gay/Lesbian Disabled	-30.9	(1.4)	0.8	(3.6)	3.1	(4.0)			-27.0	(2.6)			3.9	(2.8)
Black Hispanic Gay/Lesbian	-43.2	(0.9)	21.0	(4.4)	-4.3	(8.6)			-26.5	(8.7)			16.6	(8.8)
Black Hispanic Disabled	-45.6	(1.1)	37.3	(2.7)	1.4	(4.3)			-6.9	(4.5)			38.7	(4.5)
Black Gay/Lesbian Disabled	-32.1	(1.4)	9.2	(4.6)	15.3	(7.1)			-7.7	(7.4)			24.4	(7.5)
Hispanic Gay/Lesbian Disabled	-25.6	(1.2)	5.4	(4.4)	-0.8	(7.6)			-21.0	(7.7)			4.6	(7.8)

Notes: N = 1,779,539. The two-step estimator is used. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent). Notes to Table 3 apply. Estimates correspond to Figure 3.

Appendix Table A8: Log Wages, 3-Way Interactions, with 2-Digit Industry and Occupation Controls, OLS

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way – main (= 2-way)	SE	3-way – main	SE
Black	-19.2	(0.3)												
Female	-19.4	(0.1)												
Hispanic	-11.7	(0.3)												
Gay/Lesbian	-1.7	(0.7)												
Disabled	-18.3	(0.4)												
Black Female	-38.7	(0.4)	8.6	(0.4)			-30.1	(0.3)			8.6	(0.4)		
Black Hispanic	-30.9	(0.5)	12.1	(1.7)			-18.8	(1.6)			12.1	(1.7)		
Black Gay/Lesbian	-20.9	(0.8)	4.0	(2.7)			-16.9	(2.6)			4.0	(2.7)		
Black Disabled	-37.5	(0.6)	4.6	(1.6)			-33.0	(1.5)			4.6	(1.6)		
Female Hispanic	-31.1	(0.4)	3.6	(0.4)			-27.5	(0.3)			3.6	(0.4)		
Female Gay/Lesbian	-21.1	(0.7)	7.0	(1.0)			-14.1	(0.7)			7.0	(1.0)		
Female Disabled	-37.7	(0.5)	3.0	(0.6)			-34.7	(0.5)			3.0	(0.6)		
Hispanic Gay/Lesbian	-13.4	(0.8)	0.4	(2.5)			-12.9	(2.4)			0.4	(2.5)		
Hispanic Disabled	-30.0	(0.6)	8.1	(1.6)			-21.9	(1.6)			8.1	(1.6)		
Gay/Lesbian Disabled	-20.0	(0.8)	-2.0	(3.2)			-22.0	(3.1)			-2.0	(3.2)		
Black Female Hispanic	-50.3	(0.5)	24.3	(1.9)	-3.0	(2.2)			-29.1	(1.5)			21.3	(1.5)
Black Female Gay/Lesbian	-40.3	(0.8)	19.5	(3.1)	-6.7	(3.6)			-27.5	(2.4)			12.8	(2.5)
Black Female Disabled	-57.0	(0.6)	16.2	(1.9)	-3.0	(2.1)			-43.8	(1.3)			13.2	(1.4)
Female Hispanic Gay/Lesbian	-32.8	(0.8)	11.0	(2.9)	2.4	(3.5)			-19.4	(2.4)			13.4	(2.5)
Female Hispanic Disabled	-49.4	(0.6)	14.8	(2.0)	-2.7	(2.3)			-37.3	(1.5)			12.1	(1.6)
Female Gay/Lesbian Disabled	-39.4	(0.9)	8.0	(3.6)	2.8	(4.2)			-28.6	(2.8)			10.8	(2.9)
Black Hispanic Gay/Lesbian	-32.6	(0.9)	16.5	(4.2)	-4.5	(9.9)			-20.7	(10.1)			11.9	(10.2)
Black Hispanic Disabled	-49.2	(0.7)	24.8	(2.9)	4.4	(5.3)			-20.1	(5.4)			29.1	(5.5)
Black Gay/Lesbian Disabled	-39.2	(0.9)	6.5	(4.6)	11.7	(7.9)			-21.0	(8.5)			18.2	(8.6)
Hispanic Gay/Lesbian Disabled	-31.7	(0.9)	6.5	(4.4)	1.8	(6.9)			-23.4	(6.9)			8.3	(7.0)

Notes: N = 1,491,308. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent). Notes to Table 3 apply. Estimates correspond to Figure 4.

Appendix Table A9: Employment, 3-Way Interactions, OLS (Linear Probability)

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way – main (= 2-way)	SE	3-way – main	SE
Black	-7.1	-7.1												
Female	-12.9	-12.9												
Hispanic	5.1	5.1												
Gay/Lesbian	-3.0	-3.0												
Disabled	-39.9	-39.9												
Black Female	-20.0	(0.2)	9.5	(0.2)			-10.5	(0.1)			9.5	(0.2)		
Black Hispanic	-2.0	(0.2)	2.8	(0.8)			0.8	(0.8)			2.8	(0.8)		
Black Gay/Lesbian	-10.0	(0.4)	5.2	(1.4)			-4.8	(1.3)			5.2	(1.4)		
Black Disabled	-47.0	(0.3)	-7.1	(0.5)			-54.1	(0.4)			-7.1	(0.5)		
Female Hispanic	-7.8	(0.2)	-8.2	(0.2)			-16.1	(0.2)			-8.2	(0.2)		
Female Gay/Lesbian	-15.9	(0.3)	12.8	(0.4)			-3.1	(0.3)			12.8	(0.4)		
Female Disabled	-52.9	(0.2)	5.3	(0.3)			-47.6	(0.2)			5.3	(0.3)		
Hispanic Gay/Lesbian	2.1	(0.3)	-3.4	(1.1)			-1.3	(1.1)			-3.4	(1.1)		
Hispanic Disabled	-34.8	(0.3)	-2.0	(0.7)			-36.8	(0.7)			-2.0	(0.7)		
Gay/Lesbian Disabled	-42.9	(0.4)	11.5	(1.7)			-31.4	(1.7)			11.5	(1.7)		
Black Female Hispanic	-14.9	(0.2)	4.0	(0.9)	2.3	(1.1)			-8.6	(0.7)			6.3	(0.8)
Black Female Gay/Lesbian	-23.0	(0.4)	27.4	(1.5)	-7.9	(1.7)			-3.4	(1.1)			18.5	(1.1)
Black Female Disabled	-59.9	(0.3)	7.6	(0.7)	1.5	(0.7)			-50.8	(0.4)			9.1	(0.5)
Female Hispanic Gay/Lesbian	-10.8	(0.4)	1.1	(1.3)	7.2	(1.7)			-2.5	(1.2)			8.3	(1.3)
Female Hispanic Disabled	-47.8	(0.3)	-5.0	(0.9)	11.3	(1.0)			-41.4	(0.7)			6.3	(0.7)
Female Gay/Lesbian Disabled	-55.8	(0.4)	29.5	(1.8)	-6.2	(2.1)			-32.5	(1.4)			23.3	(1.4)
Black Hispanic Gay/Lesbian	-5.0	(0.4)	4.6	(2.0)	-10.7	(4.3)			-11.1	(4.4)			-6.1	(4.4)
Black Hispanic Disabled	-41.9	(0.3)	-6.3	(1.2)	1.7	(2.1)			-46.5	(2.1)			-4.6	(2.2)
Black Gay/Lesbian Disabled	-50.0	(0.4)	9.6	(2.2)	0.4	(3.5)			-40.0	(3.6)			10.0	(3.6)
Hispanic Gay/Lesbian Disabled	-37.8	(0.4)	6.1	(2.2)	0.2	(3.9)			-31.6	(4.0)			6.2	(4.0)

Notes: N = 1,779,539. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 percentage point change. Employment is measured as any employment in the last 12 months. Notes to Table 3 apply. Estimates correspond to Figure 5.