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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 two 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 – with 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
Department of Economics
University at Buffalo
441 Fronczak Hall
Buffalo, NY 14260
mcl.joanne.song@gmail.com

David Neumark
Department of Economics
University of California, Irvine
3151 Social Science Plaza
Irvine, CA 92697
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 8,650 citations, 5,470 of them since 2020.[1] The corresponding numbers for "intersectionality" are over 370,000, with 50,100 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 two intersecting dimensions leads to discrimination that exceeds the sum of

[1] These counts are as of August 23, 2024.
[2] See https://spssi.onlinelibrary.wiley.com/toc/15404560/2020/76/4.

its parts. For example, is the wage penalty for Black women larger than the sum of the wage penalty for Black 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).

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 discrimination perform the wrong calculations based on their estimates – calculations that, when corrected, also fail to find evidence of intersectional labor market discrimination. Indeed, the 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 is based on 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.[3] 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).[4]

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).[5] Whether the assumptions on which employers statistically discriminate are true or not, the predicted effects on wages 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,

[3] 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.
[4] 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.
[5] And the same consideration as for the employer discrimination model implies that equal pay constraints will translate into hiring discrimination.

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.[6]

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).[7] Although the approach can be most directly interpreted in the context of wage discrimination, as it can be tied to the Becker employer discrimination model (Neumark, 1988), 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

[6] 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.

[7] See, e.g., Goldin (2014), who also notes (as have many others) that discrimination is one possible explanation for residual wage gaps.

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

researchers 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 (which can have implications for wages that vary depending on factors such as the underlying model, the size of the minority group, and the share and distribution of discriminatory tastes). Experimental studies have, for the most part, not 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 (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

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).

We recognize, though, that intersectional discrimination is more complex than interactions in

[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 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.

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). We regard this as potentially valuable complementary evidence. However, 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 – points to intersectional discrimination.

It is also important to realize that there is an alternative theory of the role of interactions in labor market outcomes – namely, 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). 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 argument might be viewed as begging the question of why non-interactive penalties continue to exist, but on the margin memberships in multiple protected groups could partially offset discrimination that underlies these penalties.

III. 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. We discuss each of these in turn. 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.

Kim (2009) studies whether Black women face a larger earnings penalty than would be implied by the sum of the race and gender penalty. She cites qualitative research on intersectional discrimination defined as whether the total amount of discrimination Black women face is equal to, less than, or greater

than the sum of its parts. For example, she suggests there could be a positive interaction, so that total discrimination is less, if either Black women "count twice" for affirmative action (Bell et al., 1993), or 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 (an hypothesis that motivates the analysis of McGuire and Reskin (1993), but which they reject). She also discusses the possibility that the interaction of race and gender creates further disadvantage, so that the penalty exceeds the sum of the Black and female penalties. Kim uses the 2002 Current Population Survey Annual Demographic Survey (i.e., the Annual Social and Economic Supplements (ASEC) file). She uses Oaxaca decompositions, estimating separate regressions, and then 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.[11] She concludes that the evidence is consistent with intersectional discrimination as captured in interactions: "black women are underpaid because of their race and gender and that the sum of these two penalties is less than the total amount of the earnings penalties they face" (p. 466). For example, 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 market discrimination.[12]

George et al. (2022) take up a similar analysis, but considering both race (Black) and ethnicity (Hispanic), asking "Is the unexplained wage gap experienced by Black and Hispanic women larger than the sum of the separate unexplained gaps associated with gender, race and ethnicity?" (p. 304). They use Decennial Census data from 1980, 1990, and 2000, and 2010 and 2019 American Community Survey

[11] She uses the "other" group's wage equation coefficients to weight the differences in characteristics, and the mean characteristics for the group in question to weight the differences in coefficient (per the indeterminacy in Oaxaca decompositions discussed in Neumark, 1988). We refer in this paper to "Oaxaca decompositions," although in fact Blinder (1973) suggested the same decomposition at the same time, and it is common therefore to refer to Oaxaca-Blinder decompositions.

[12] 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 Table 4.) Of course, if discrimination influences hours of work or industry/occupation, then controlling for industry/occupation or conditioning on work hours (or weeks) can mask the overall effects of discrimination.

(ACS) data, to provide evidence on this question over a long time span, also using Oaxaca decompositions. They compute weekly wages and use this as their outcome. They find similar results to Kim (2009), 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.[13]

Paul et al. (2022) study the same question, building on Kim's work (2009), in large part by studying changes in the findings over time (1980-2017). They use CPS (ASEC) data and study the determinants of hourly wages, as in Kim, using Oaxaca decompositions. Overall, they confirm Kim's finding (and the evidence in George, et al., 2022) that Black women experience an intersectional wage penalty that is larger than the sum of the separate race and female penalties, with the difference (relate to Kim, 2009) that in their analysis this result holds with industry and occupation controls. They also find that this evidence is robust across years. They adopt the framing of Watson and Lunn (2010), asking whether the race and gender penalties are "additive, subtractive, or multiplicative" (p. 7), and conclude that the results are multiplicative, "meaning Black women are penalized both for their race and gender but also face an additional penalty for holding these two identities simultaneously" (p. 21).

White (2023) reports similar analyses, but with the addition of consideration of 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 (which have to be estimated with ACS data using information on hours and weeks worked). He reports some evidence consistent with intersectional discrimination (e.g., for Black females who are non-Hispanic and native-born), which he describes as largely consistent with Kim (2009), Paul et al. (2022), and George et al. (2022). Indeed, for the majority of the groups defined by the combinations of categories he considers, he finds evidence pointing in this direction, 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

[13] George et al. also use alternative decompositions to measure these differences at different percentiles of the earnings distribution.

threshold he considers).[14]

Curiously, though, the evidence consistent with intersectional discrimination from these studies is not found in other studies even when using similar data and approaches. 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 intersectional discrimination insofar as it is captured by this approach.[15]

Finally, Mandel and Semyonov (2016) explore the same question with Census and ACS data covering 1970-2010. They "predict" evidence against the intersectional discrimination hypothesis for Black women because, in their view, negative stereotypes of Black men are more pronounced. 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.

IV. Why the Contradictory Evidence?

An obvious question from the preceding review of the evidence is why studies – especially when they use similar methods and data – reach diametrically opposite conclusions. It turns out that the answer is simply that some of the papers – indeed, the four studies described above that conclude that there is

[14] In all cases, as best we can tell, he is estimating 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 are 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), Oaxaca decompositions with sample selection corrections introduce complications and ambiguities. (These issues are not the focus of our analysis, although we do estimate some models with selection corrections.)

[15] 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 intersectionality as the research literature on labor market outcomes has framed it.

evidence of intersectional discrimination – simply do the calculation incorrectly and hence end up with the wrong answer.[16]

The papers described above use Oaxaca (1973) decompositions, but the issue can be illustrated just as simply in regressions using dummy variables. (The difference is that in the latter case, the coefficients of the other variables in the model are assumed to be the same across the groups considered, but the issue regarding the calculation is the same.) 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:

$$y = \alpha + \beta_B B + \beta_F F + \beta_{BF} B \cdot F + X\gamma + \varepsilon. \quad (1)$$

As described in the Introduction, 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:

$$E(y|B = 0, F = 0, X) = \mu_{WM} = \alpha + X\gamma \quad (2a)$$

$$E(y|B = 1, F = 0, X) = \mu_{BM} = \alpha + \beta_B + X\gamma, \quad (2b)$$

$$E(y|B = 0, F = 1, X) = \mu_{WF} = \alpha + \beta_F + X\gamma, \quad (2c)$$

$$E(y|B = 1, F = 1, X) = \mu_{BF} = \alpha + \beta_B + \beta_F + \beta_{BF} + X\gamma. \quad (2d)$$

Then, by substituting terms from (2a)-(2d), $\beta_{BF} < 0$ is equivalent to:

$$\mu_{BF} - \mu_{WM} - \{(\mu_{BM} - \mu_{WM}) + (\mu_{WF} - \mu_{WM})\} < 0. \quad (3)$$

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 intersectionality hypothesis for race and gender.

Equation (3) can be rewritten in different ways:

$$\mu_{BF} - \mu_{BM} - (\mu_{WF} - \mu_{WM}) < 0 \quad (4a)$$

$$\mu_{BF} - \mu_{WF} - (\mu_{BM} - \mu_{WM}) < 0. \quad (4b)$$

[16] In this section we discuss all of the papers covered in the previous section except McGuire and Reskin (1993), as this one uses very different data.

Each of these, also, reflects the same idea of intersectional discrimination. In equation (4a), 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). The same applies to equation (4b), 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 Oaxaca decompositions instead of regressions, but the idea underlying the calculations they do is equivalent.[17]

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 (4a) or (4b) above, the entry for the calculation they do (in the "Calculation of intersectional discrimination" column) is the same as equation (4a), although here in the table we write this more generically as:

$$(BF - BM) - (WF - WM), \tag{5}$$

because other papers use Oaxaca 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 (5) they estimate is 0.14 – the opposite of the sign predicted by intersectional discrimination.

Also in Panel B, Mandel and Semyonov estimate Oaxaca 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 –

[17] 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.

again the opposite of what is predicted by intersectional discrimination.[18] Both of these papers conclude (see "Interpretation" column) that there is evidence *against* intersectional discrimination, and these conclusions are correct based on the empirical analysis.

However, for the four studies in Panel A, the authors test for intersectionality incorrectly and reach the opposite conclusion to what the data show. For example, Kim (2009) uses Oaxaca 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:

$$(WM - BF) - [(WF - BF) + (BM - BF)] = 0.03 > 0. \quad (6)$$

This can be rewritten in two ways:

$$(BF - BM) - (WF - WM) \quad (7a)$$

or

$$(BF - WF) - (BM - WM). \quad (7b)$$

Equations (7a) and (7b) correspond to equations (4a) and (4b), but note that these expressions are *positive* (0.03), which is the opposite of the prediction of intersectional discrimination. In words, equation (7a) 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 (7b) being 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. This has nothing to do with using Oaxaca decompositions vs. simpler regressions.[19] Rather, all four papers do the wrong computation to test

[18] The entry for these calculations is equivalent to equation (4b).
[19] We show this in the Appendix (Appendix Tables A1 and A2), where we report estimates focusing on race and gender only, as in most of the papers discussed in this section. We first use Oaxaca 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 intersectionality.

for intersectional discrimination.[20] 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 "Comment" column of Table 1, which notes that the calculation in each paper boils down to either equation (7a) or (7b), and its value is positive, *inconsistent* with intersectional labor market discrimination.

Thus, the puzzle is resolved. All of the papers, which, as noted above, use similar data and methods, do in fact yield similar evidence against intersectional discrimination with regard to race and gender.[21] 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.[22]

V. Our Analysis

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.[23] To simplify, we use regressions with dummy variables and interactions rather than 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.,

[20] Curiously, Paul et al. (2022) describe the result correctly, although they still interpret the evidence as pointing to intersectionality: "Black men still have a larger unexplained wage gap (0.132 log points) than Black women (0.074 log points)" (p. 17).
[21] White (2023) studies many more groups, but the calculations always reflect the same error.
[22] That is, (BM – WM) – (BF – WF) < 0. Rewriting, this implies, for example, (BF – BM) – (WF – WM) > 0, the opposite of equation (4a).
[23] 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 not been the focus of studies of pay discrimination.

differences can vary with the different values of the control variables. In addition, there is an indeterminacy in Oaxaca decompositions because we do not know the true non-discriminatory wage structure (Neumark, 1988).[24]

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 in Appendix Table A3. (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.) Thus, for a generic outcome *y* (most often log wages), we estimate models of the form:

$$\begin{aligned} y = {} & a + \beta_B B + \beta_F F + \beta_H H + \beta_G G + \beta_D D \\ & + \beta_{BF} B \cdot F + \beta_{BH} B \cdot H + \beta_{BG} B \cdot G + \beta_{BD} B \cdot D + \beta_{FH} F \cdot H + \beta_{FG} F \cdot G \\ & + \beta_{FD} F \cdot D + \beta_{HG} H \cdot G + \beta_{HD} H \cdot D + \beta_{GD} G \cdot D \\ & + \beta_{BFH} B \cdot F \cdot H + \beta_{BFG} B \cdot F \cdot G + \beta_{BFD} B \cdot F \cdot D + \beta_{FHG} F \cdot H \cdot G + \beta_{FHD} F \cdot H \cdot D \\ & + \beta_{FGD} F \cdot G \cdot D + \beta_{BHG} B \cdot H \cdot G + \beta_{BHD} B \cdot H \cdot D + \beta_{BGD} B \cdot G \cdot D + \beta_{HGD} H \cdot G \cdot D \\ & + X\gamma + \varepsilon. \end{aligned} \tag{8}$$

We define the different effects of interest, used in reporting the results from these regressions below, as follows:[25]

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). 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:

$$\beta_B + \beta_F, \tag{9}$$

and for female Hispanic gays/lesbians, it is:

$$\beta_F + \beta_H + \beta_G.^{26} \tag{10}$$

[24] And we present some selectivity-corrected wage equation estimates, for which the implementation of Oaxaca decompositions is unclear (Neumann and Oaxaca, 2004), as noted earlier.
[25] 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.
[26] 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.

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:

$$\beta_{BF} + \beta_{BD} + \beta_{FD}. \quad (11)$$

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:

$$\beta_B + \beta_F + \beta_{BF}. \quad (12)$$

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:

$$\beta_B + \beta_F + \beta_D + \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD}. \quad (13)$$

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:

$$\beta_B + \beta_F + \beta_{BF} - (\beta_B + \beta_F) = \beta_{BF}. \quad (14)$$

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:

$$\beta_B + \beta_F + \beta_D + \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD} - (\beta_B + \beta_F + \beta_D) \quad (15)$$

$$= \beta_{BF} + \beta_{BD} + \beta_{FD} + \beta_{BFD}.$$

In terms of testing for intersectional discrimination, the key parameters – when we consider up to three intersecting categories simultaneously – are the "3-way – main effects." These measure 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.[27]

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, and OLS models for hours and earnings.

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 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. We calculate hourly wages using usual hours worked per week, number of weeks worked, and respondents' total pre-tax wage and salary income. However, the ACS does not report the detailed number of weeks the respondent worked before 2019, but rather only categories, which is why we start the analysis in 2019. We limit our analysis to only White or Black (both categories of which can include Hispanics).[28]

[27] We also reported limited results considering only 2-way intersectionality, in which case, corresponding, the key parameter is the "2-way – main effects" estimate, and a negative estimate is, again, consistent with intersectionality.
[28] We imposed some restrictions on groups considered for tractability; additional research could extend our analysis to even more groups.

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.[29] We define disabled as reporting having one or more of cognitive, ambulatory, independent living, self-care, vision, or hearing difficulties.[30] In all our analyses, we control for education, potential experience, potential experience squared, and state and year fixed effects.

We report descriptive statistics for outcomes and group membership in Table 2. 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 A3).

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).

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

[30] 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.

Main Results for Log Wages

We begin with the 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 (9) and (10). 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 (11), which are the sums of the coefficients on the products of two dummy variables in equation (8). The "3-way estimates" are the estimated coefficients of the triple interactions in equation (8). The "Total 2-way" and "Total 3-way" estimates add the main and interactive effects, as in equations (12) and (13).

Finally, the key estimates – hence the highlighting – are those labeled "2-way – main" and "3-way – main" are the parameters that 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 (14). 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 (15) 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 single 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.[31]

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.

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 not 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.[32] 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,

[31] 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 A4.
[32] The employment – or selection – equation always includes 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).

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.[33] The resulting estimates are very similar. In both figures, nearly all of the "2-way intersectional – main" estimates are positive and significant, and only 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. 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

[33] The full regressions are reported in Appendix Tables A5 and A6.

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.[34]

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.

[34] 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.

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.[35] Comparing Figure 4 to Figure 1, the pattern of estimates is very similar, and 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, Hours, and Earnings

The focus to here has been on wages. The analysis of discrimination in terms of wages pervades the literature, perhaps in part because the Becker (1971) model of employer discrimination, which can be linked directly to residual tests of wage discrimination (Neumark, 1988), is couched in terms of wages, abstracting from how much (or whether) one works. But an 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), motivates thinking about discrimination affecting the employment margin as well. Hours are also potentially of interest, given that discrimination on the employment margin can directly affect hours, as can discrimination in the assignment of shifts, access to jobs with different hours of work, etc.[36] (We do not condition on positive hours.) That said, in turning to evidence on employment and hours, we recognize that variation in these outcomes can reflect preferences as well.[37] We are not taking a strong stand on whether employment or

[35] The full model estimates are reported in Appendix Table A7.
[36] The potential impact of discrimination on hours has been referenced and studied far less, but some studies that tie discrimination to hours worked include Cain (1986) and Storer et al. (2020) – the latter in the context of irregular scheduling.
[37] 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).

hours differences by race, gender, etc., necessarily reflect discrimination, but want to present a full picture of what the kinds of regressions we have estimated to test for intersectional discrimination in wages produce for these other outcomes.

Finally, we also estimate regressions for earnings (again, without conditioning on employment) to capture what might be considered an omnibus measure of labor market advantage or disadvantage, reflecting differences in wages, employment, and hours. If one is wary of interpreting the estimates for employment, hours, and earnings as reflecting discrimination, the estimates nonetheless provide descriptive measures of differences in labor market outcome by group – including groups potentially marginalized on more than one dimension.

The results, presented graphically as before, are reported in Figures 5-7.[38] For employment (Figure 5), 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 quite 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. The evidence for hours (Figure 6) 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.

Finally, the earnings estimates (Figure 7) very one-sidedly provide evidence against intersectional discrimination – now measured via overall earnings. As just noted, the earnings results provide the most comprehensive evidence on labor market disadvantage or advantage, although we might view the wage results as more clearly reflecting discrimination. However, the conclusion is the same.

[38] Full regression results are reported in Appendix Tables A8-A10.

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 "intersectionality," does follow a tradition of empirical tests for 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, hours, and earnings. 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 – in fact a majority – 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 do want to be clear about 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.[39] 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.[40]

[39] 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).

[40] 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.

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Table 1: Evidence on Intersectional Discrimination in the Existing Literature

Source	Unexplained wage gaps	Interpretation	Calculation of intersectional discrimination	Comment
A. Studies claiming evidence of intersectional discrimination				
Kim (2009), Tables 3-4	Oaxaca 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	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)	Oaxaca 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	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	Oaxaca 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	(WM – BF) – [(BM – BF) + (WF – BF)] = (BF – BM) – (WF – WM) = (BF – WF) – (BM – WM) = .045	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*.**

Source	Unexplained wage gaps	Interpretation	Calculation of intersectional discrimination	Comment
		intersectional race and gender penalty for earnings in the case of Black women." (pp. 17-18) **Conclusion: *evidence of intersectional discrimination.***		
White (2023), Tables 4.2, 5.1, Figure 4.1, eq. 3.9 (native-born, non-Hispanic	Oaxaca 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	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	"…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	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)	Oaxaca 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	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 Oaxaca decompositions are used, as they are in most of these papers.

Table 2: Means of Outcome Variables

	Log Wage	Hourly Wage	Total Earnings	Total Working Hours	Employed
	(1)	(2)	(3)	(4)	(5)
All	3.22	31.79	55126.61	1686.50	0.81
Black	2.99	24.22	37632.49	1503.93	0.74
Female	3.12	26.95	42502.04	1483.42	0.76
Hispanic	3.01	24.78	40527.41	1577.02	0.77
Gay/Lesbian	3.31	36.35	66210.13	1850.18	0.87
Disabled	2.92	15.18	19546.06	802.66	0.41
2-way					
Black Female	2.95	23.36	35256.79	1453.65	0.74
Black Hispanic	3.00	24.55	40309.84	1609.11	0.79
Black Gay/Lesbian	3.05	27.36	46511.98	1708.36	0.82
Black Disabled	2.76	11.15	12462.4	600.09	0.32
Female Hispanic	2.93	20.62	31048.78	1329.51	0.69
Female Gay/Lesbian	3.21	32.36	56574.93	1813.63	0.86
Female Disabled	2.83	13.67	15593.25	719.62	0.39
Hispanic Gay/Lesbian	3.11	29.54	51059.13	1778.31	0.85
Hispanic Disabled	2.85	13.45	18534.74	852.23	0.44
Gay/Lesbian Disabled	2.99	20.98	28500.36	1123.37	0.57
3-way					
Black Female Hispanic	2.98	24.01	37590.39	1506.36	0.77
Black Female Gay/Lesbian	2.97	27.06	41467.44	1705.07	0.82
Black Female Disabled	2.74	12.53	12787.41	633.80	0.35
Female Hispanic Gay/Lesbian	3.03	27.19	43307.82	1746.26	0.84
Female Hispanic Disabled	2.78	12.73	15760.96	785.83	0.43
Female Gay/Lesbian Disabled	2.93	20.07	25780.4	1105.36	0.57
Black Hispanic Gay/Lesbian	3.01	31.11	38057.31	1569.14	0.76
Black Hispanic Disabled	2.92	17.90	19628.72	840.59	0.44
Black Gay/Lesbian Disabled	3.02	23.05	27578.79	1006.13	0.49
Hispanic Gay/Lesbian Disabled	2.92	17.77	25973.13	1188.89	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	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)

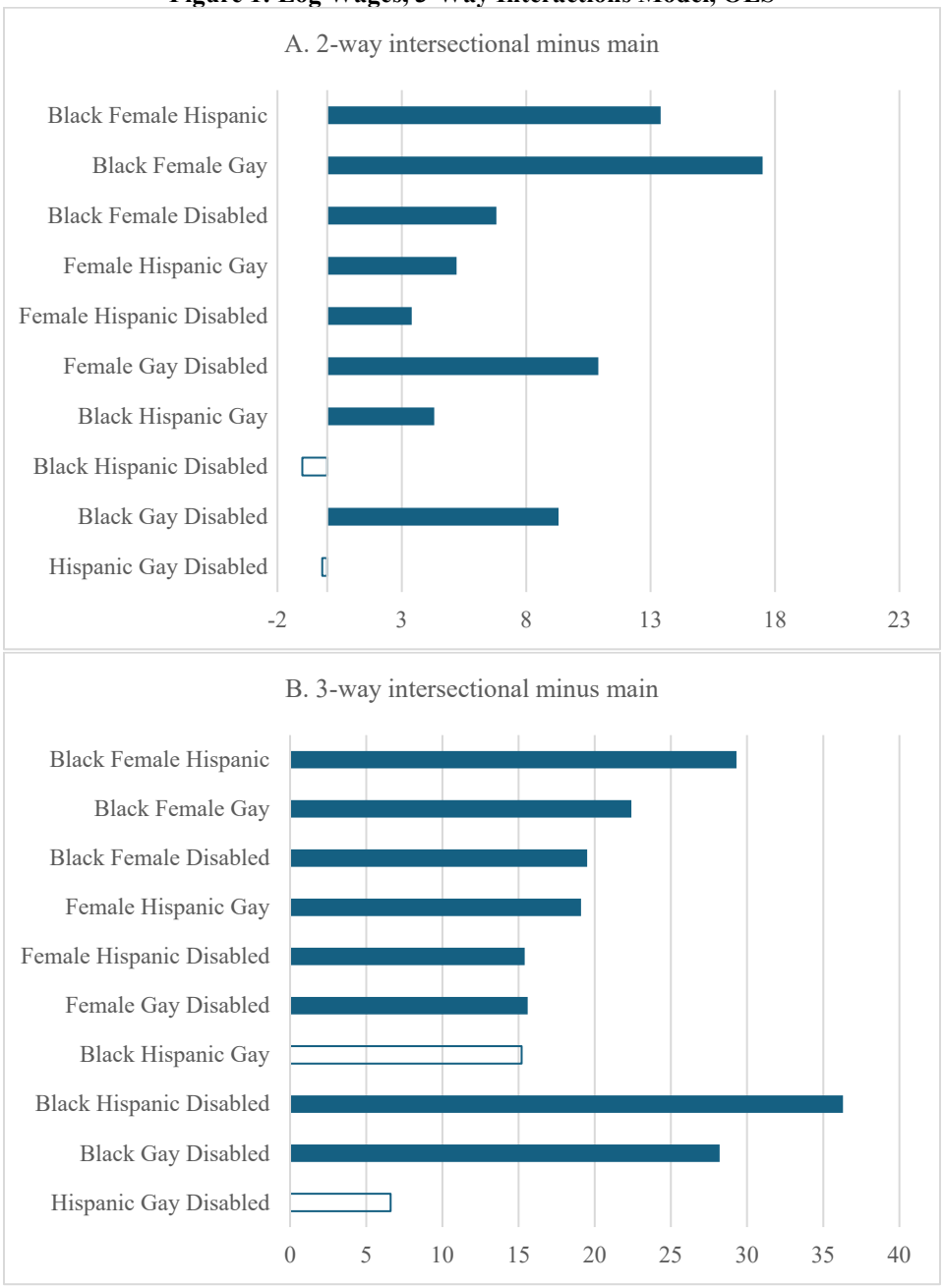
Notes: N = 1,497,372. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent). Notes to Table 2 apply. In all regressions we control for education, potential experience, potential experience squared, and state- and year-fixed effects. Potential experience is defined as age minus year of schooling minus seven.

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	SE	Main	SE	2-way − main	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)

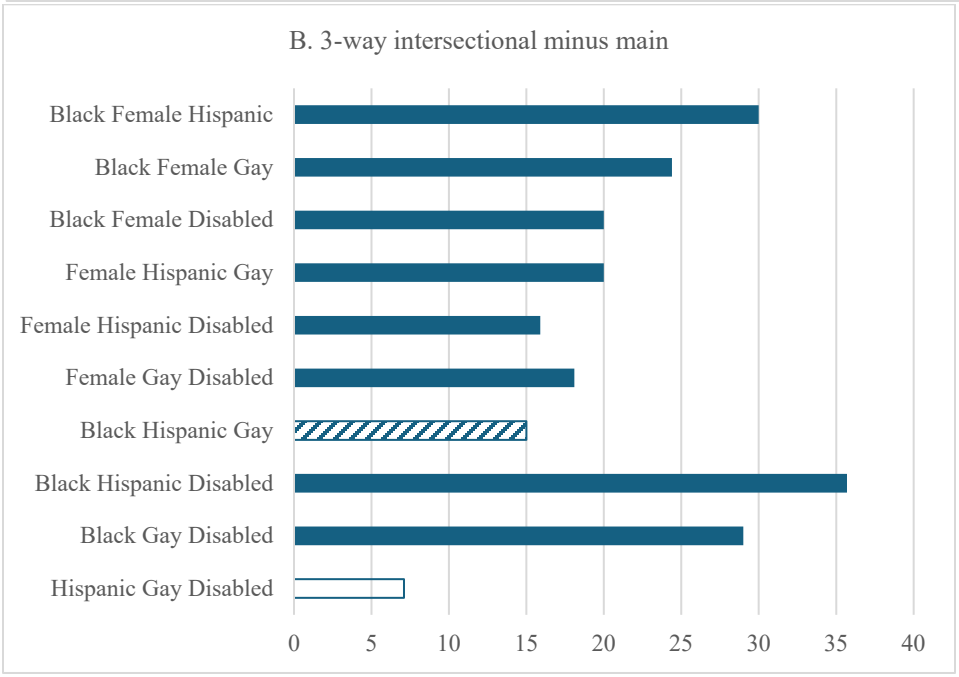
N = 1,497,372. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change(approximately 1 percent). Notes to Table 2 apply.

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



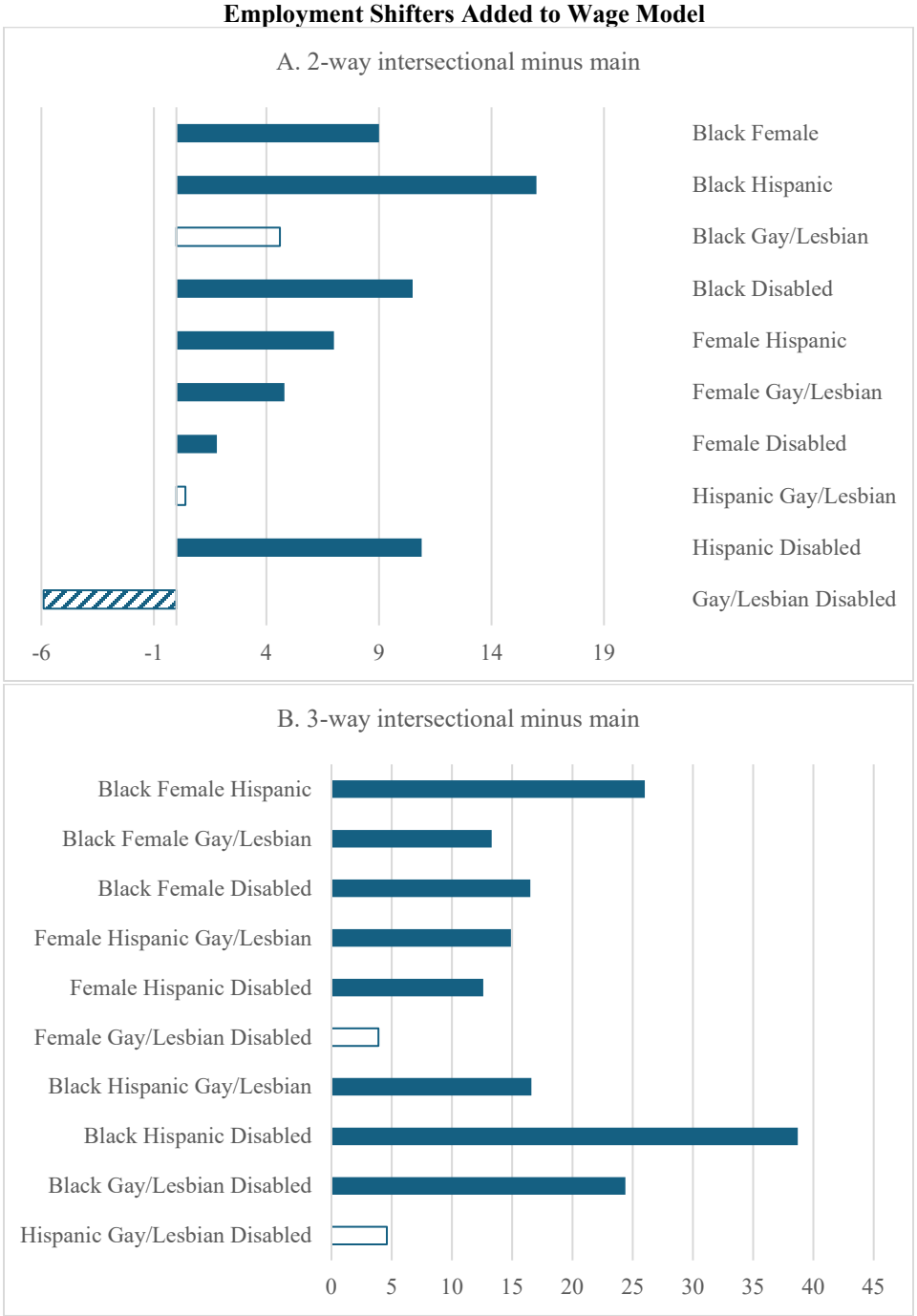
Note: Figure corresponds to Table 3. Solid bars: $p < .05$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

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



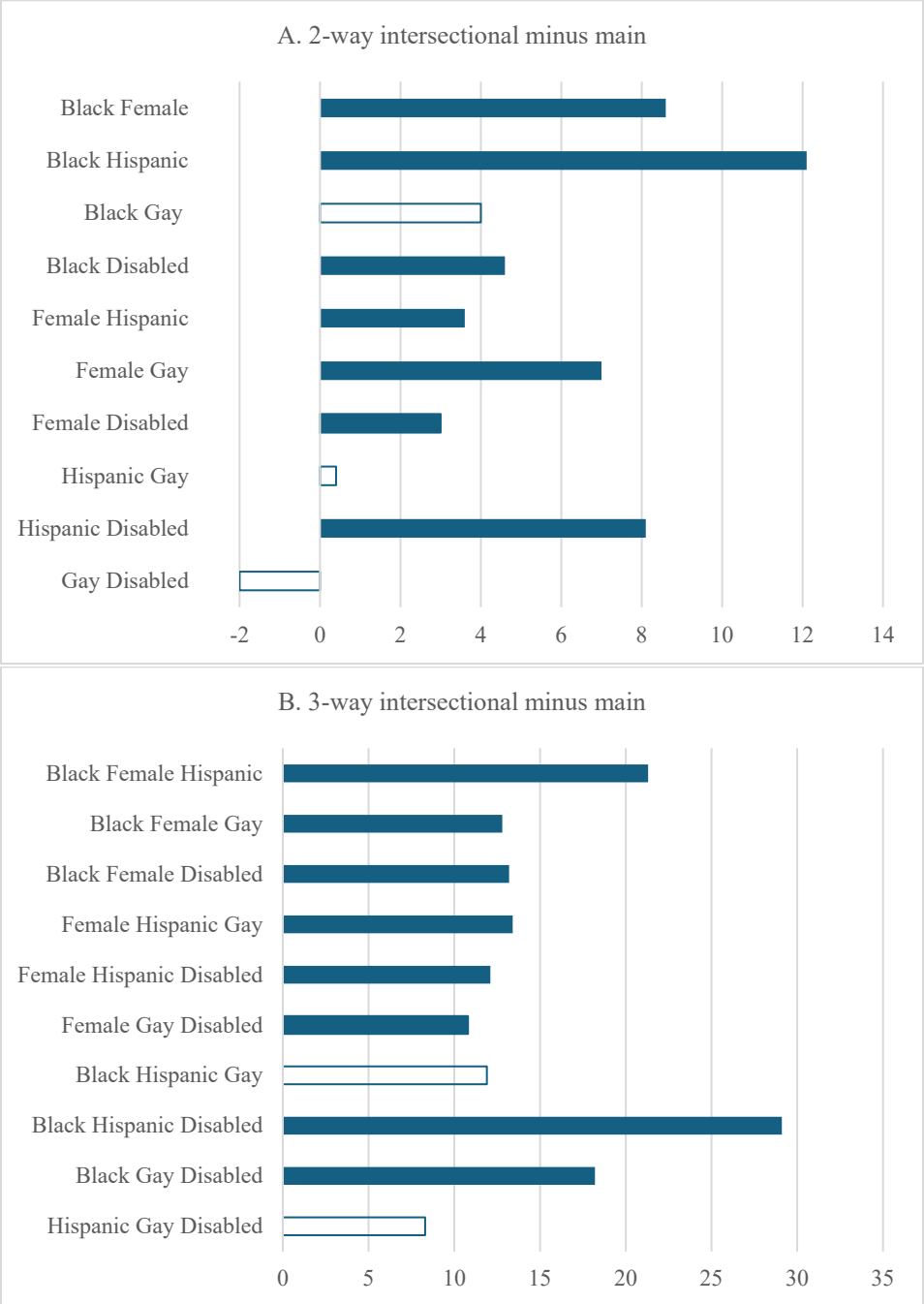
Note: Figure corresponds to Appendix Table A5. Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

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



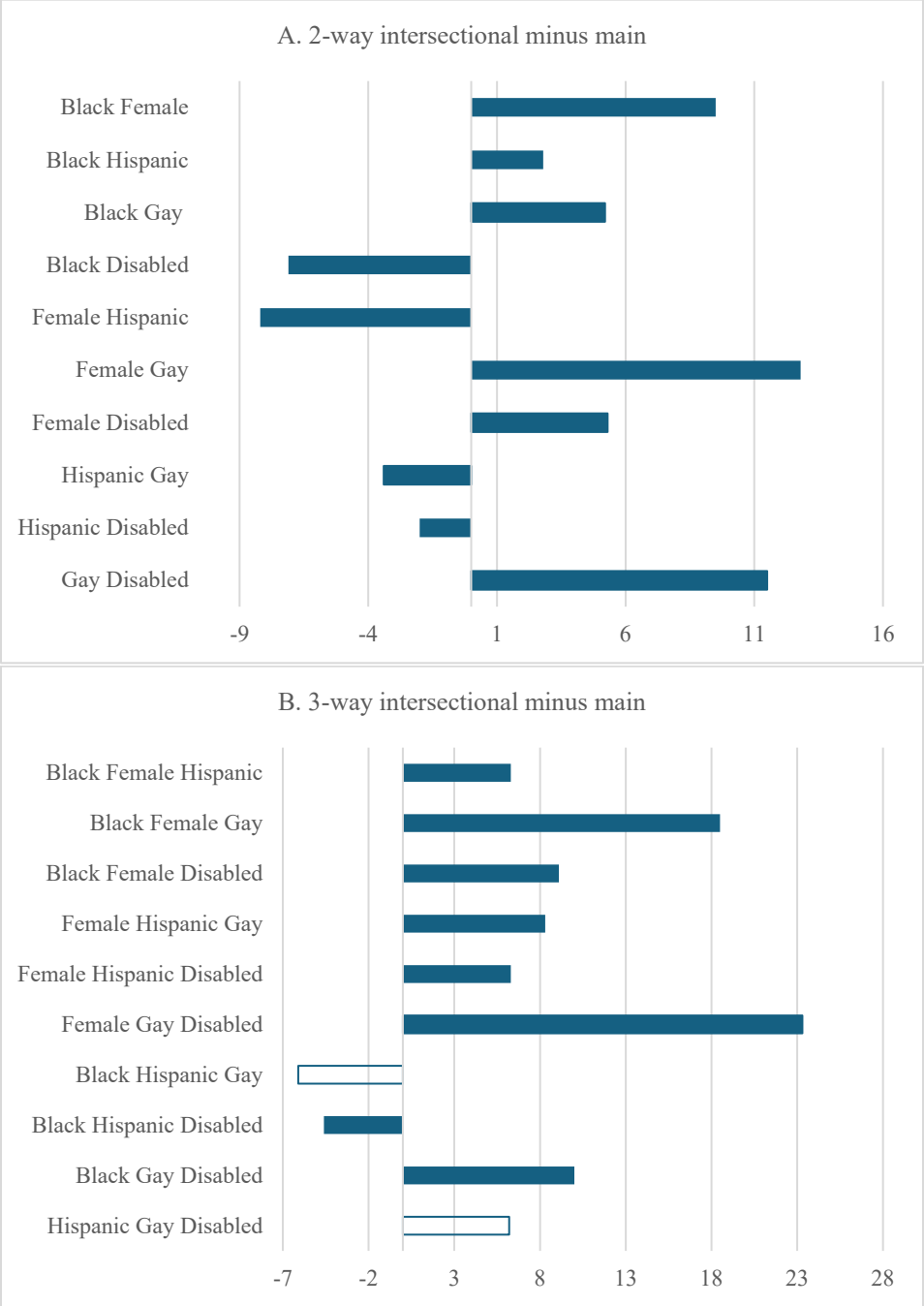
Note: Figure corresponds to Appendix Table A6. Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

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



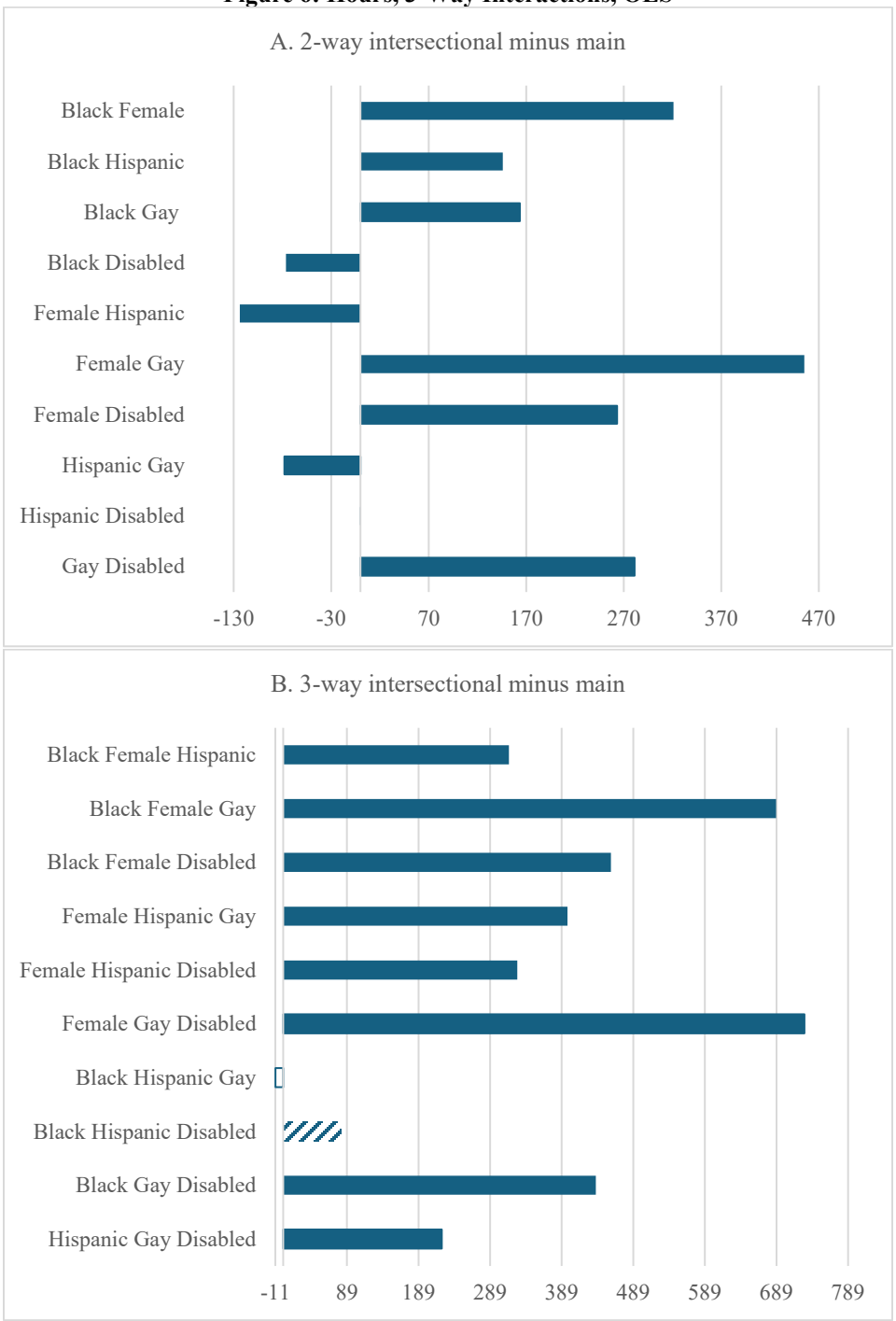
Note: Figure corresponds to Appendix Table A7. Solid bars: $p < .05$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 log point change (approximately 1 percent).

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



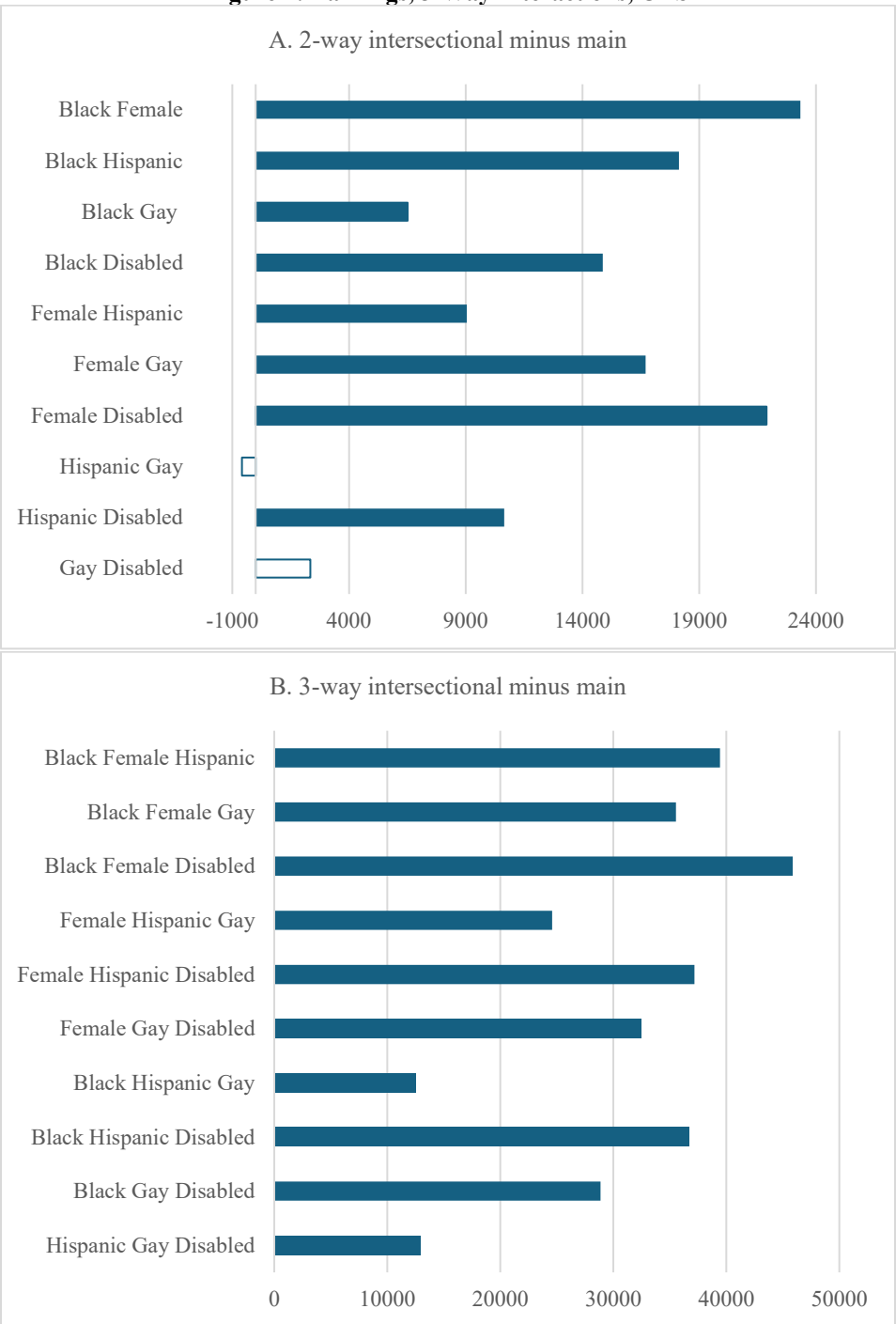
Note: Figure corresponds to Appendix Table A8. Solid bars: $p < .05$; hollow bars: $p > .1$. Coefficients are multiplied by 100 so a coefficient of 1 represents a 1 percentage point change. Employment is measured as any employment in the last 12 months.

Figure 6: Hours, 3-Way Interactions, OLS



Note: Figure corresponds to Appendix Table A9. Solid bars: $p < .05$; diagonal pattern bars: $.05 < p < .1$; hollow bars: $p > .1$. Hours are annual.

Figure 7: Earnings, 3-Way Interactions, OLS



Note: Figure corresponds to Appendix Table A10. Solid bars: $p < .05$; hollow bars: $p > .1$. Earnings are annual labor income.

Appendix

Appendix Table A1: Estimate to Test for Intersectional Discrimination by Race and Gender Using Oaxaca 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 A2: 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 A1. The table reports estimated coefficients, with standard errors in parentheses.

Appendix Table A3: 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 A4: 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 A5: 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	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 A6: 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	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 A7: 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	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 A8: 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	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.

Appendix Table A9: Hours, 3-Way Interactions, OLS

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way − main	SE	3-way − main	SE
Black	-263.0	(3.9)												
Female	-514.2	(1.5)												
Hispanic	56.6	(4.0)												
Gay/Lesbian	-132.8	(8.4)												
Disabled	-972.8	(4.4)												
Black Female	-777.2	(4.4)	321.0	(5.1)			-456.2	(3.3)			321.0	(5.1)		
Black Hispanic	-206.3	(5.8)	146.0	(20.5)			-60.3	(19.9)			146.0	(20.5)		
Black Gay/Lesbian	-395.8	(9.3)	163.4	(34.6)			-232.3	(33.5)			163.4	(34.6)		
Black Disabled	-1235.8	(6.1)	-76.3	(10.8)			-1312.1	(9.3)			-76.3	(10.8)		
Female Hispanic	-457.6	(4.5)	-123.6	(5.5)			-581.2	(4.1)			-123.6	(5.5)		
Female Gay/Lesbian	-647.1	(8.6)	455.1	(11.6)			-192.0	(8.0)			455.1	(11.6)		
Female Disabled	-1487.0	(4.9)	263.2	(5.8)			-1223.9	(3.8)			263.2	(5.8)		
Hispanic Gay/Lesbian	-76.2	(9.4)	-78.0	(29.4)			-154.2	(28.1)			-78.0	(29.4)		
Hispanic Disabled	-916.2	(6.2)	-0.6	(16.9)			-916.8	(16.0)			-0.6	(16.9)		
Gay/Lesbian Disabled	-1105.6	(9.6)	281.1	(38.1)			-824.5	(37.2)			281.1	(38.1)		
Black Female Hispanic	-720.6	(6.3)	343.3	(23.1)	-28.1	(26.4)			-405.3	(17.0)			315.2	(18.0)
Black Female Gay/Lesbian	-910.0	(9.7)	939.6	(38.9)	-251.1	(43.7)			-221.5	(27.7)			688.5	(29.3)
Black Female Disabled	-1750.0	(6.6)	507.9	(15.7)	-50.1	(14.3)			-1292.3	(8.3)			457.7	(10.3)
Female Hispanic Gay/Lesbian	-590.4	(9.7)	253.4	(34.5)	143.6	(42.0)			-193.4	(30.1)			397.0	(31.6)
Female Hispanic Disabled	-1430.4	(6.6)	138.9	(20.4)	188.1	(21.6)			-1103.4	(13.3)			327.0	(14.6)
Female Gay/Lesbian Disabled	-1619.9	(9.9)	999.4	(42.0)	-271.3	(46.8)			-891.8	(29.4)			728.0	(31.1)
Black Hispanic Gay/Lesbian	-339.2	(10.4)	231.4	(50.8)	-242.3	(100.8)			-350.0	(101.8)			-10.8	(102.7)
Black Hispanic Disabled	-1179.1	(7.6)	69.1	(29.5)	12.1	(44.7)			-1097.9	(46.5)			81.2	(47.6)
Black Gay/Lesbian Disabled	-1368.6	(10.5)	368.2	(52.8)	68.5	(78.6)			-931.9	(79.5)			436.7	(80.7)
Hispanic Gay/Lesbian Disabled	-1049.0	(10.6)	202.5	(51.7)	19.0	(89.0)			-827.5	(89.7)			221.5	(90.7)

Notes: N = 1,779,539. Hours are annual. Notes to Table 3 apply. Estimates correspond to Figure 6.

Appendix Table A10: Earnings, 3-Way Interactions, OLS

	Main	SE	2-way	SE	3-way	SE	Total 2-way	SE	Total 3-way	SE	2-way − main	SE	3-way − main	SE
Black	-26245.0	(209.3)												
Female	-35333.4	(116.4)												
Hispanic	-14242.6	(272.5)												
Gay/Lesbian	-4724.2	(878.1)												
Disabled	-38601.2	(199.8)												
Black Female	-61578.3	(273.3)	23332.8	(257.0)			-38245.5	(173.7)			23332.8	(257.0)		
Black Hispanic	-40487.6	(369.3)	18128.9	(1007.6)			-22358.7	(969.2)			18128.9	(1007.6)		
Black Gay/Lesbian	-30969.1	(912.6)	6509.0	(2426.1)			-24460.1	(2294.6)			6509.0	(2426.1)		
Black Disabled	-64846.1	(316.2)	14868.0	(393.6)			-49978.1	(303.7)			14868.0	(393.6)		
Female Hispanic	-49576.0	(324.2)	9040.3	(306.3)			-40535.7	(196.2)			9040.3	(306.3)		
Female Gay/Lesbian	-40057.5	(896.9)	16699.7	(1066.8)			-23357.8	(625.6)			16699.7	(1066.8)		
Female Disabled	-73934.5	(264.1)	21882.5	(243.0)			-52052.0	(152.1)			21882.5	(243.0)		
Hispanic Gay/Lesbian	-18966.8	(928.8)	-587.7	(2373.5)			-19554.5	(2226.8)			-587.7	(2373.5)		
Hispanic Disabled	-52843.8	(363.2)	10647.2	(603.7)			-42196.6	(535.7)			10647.2	(603.7)		
Gay/Lesbian Disabled	-43325.3	(909.3)	2342.0	(1715.8)			-40983.3	(1512.9)			2342.0	(1715.8)		
Black Female Hispanic	-75820.9	(429.6)	50502.0	(1178.6)	-11066.0	(1299.3)			-36385.0	(912.2)			39435.9	(986.6)
Black Female Gay/Lesbian	-66302.5	(940.0)	46541.5	(2908.1)	-10992.1	(2679.0)			-30753.0	(1285.4)			35549.5	(1598.4)
Black Female Disabled	-100179.5	(383.6)	60083.3	(683.2)	-14210.1	(514.4)			-54306.2	(289.8)			45873.3	(423.9)
Female Hispanic Gay/Lesbian	-54300.1	(955.7)	25152.3	(2885.2)	-562.2	(2612.1)			-29710.0	(1303.9)			24590.1	(1626.0)
Female Hispanic Disabled	-88177.1	(423.1)	41570.0	(862.3)	-4389.7	(738.9)			-50996.8	(415.9)			37180.3	(542.4)
Female Gay/Lesbian Disabled	-78658.7	(936.2)	40924.2	(2358.7)	-8462.6	(2030.9)			-46197.1	(1085.4)			32461.6	(1443.8)
Black Hispanic Gay/Lesbian	-45211.7	(971.0)	24050.2	(3656.0)	-11520.2	(4119.2)			-32681.8	(4342.5)			12530.0	(4526.8)
Black Hispanic Disabled	-79088.7	(458.6)	43644.1	(1310.3)	-6976.6	(1610.1)			-42421.2	(1633.1)			36667.5	(1719.6)
Black Gay/Lesbian Disabled	-69570.3	(951.3)	23719.1	(3153.5)	5150.0	(3725.1)			-40701.2	(4047.9)			28869.1	(4248.0)
Hispanic Gay/Lesbian Disabled	-57567.9	(967.6)	12401.5	(3135.4)	492.2	(3014.5)			-44674.2	(2995.7)			12893.7	(3247.0)

Notes: N = 1,779,539. Earnings are annual labor income. Notes to Table 3 apply. Estimates correspond to Figure 7.