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Are Wage Premiums for Black Women Illusory?
A Critical Examination **

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Abstract

Recent evidence documents a wage premium for black women (e.g., Fryer, 2011). However, we find no strong evidence of a premium after accounting for selection into the labor market; years of education attained, conditional on ability; and local cost of living. We find modest evidence of heterogeneous effects by education—small premiums for highly educated black women and penalties for black women with less education. Controlling for actual experience yields estimates at the low end of previously published premiums, but the possibility of discrimination in hiring and firing implies that controls for actual experience may be inappropriate.

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1. Introduction

Estimates of the black-white wage gap inform researchers and policymakers about how well blacks perform relative to whites in the labor market. Following the Smith and Welch (1989) seminal results that quantify the gap and show how it changed from 1940 to 1980, this area of research remains active. The majority of the estimates of the black-white wage gap compute the pay differential faced by men (see, e.g., Neal and Johnson (1996), Black et al. (2009), Lang and Manove (2011)) and typically find a substantial wage penalty for black men relative to white men.[1] The research focus on men rather than women is largely because of the view that selection into and out of work is a more confounding problem for analysis of women than men.[2] However, for researchers and policymakers interested in how blacks are doing relative to whites in the labor market, the female black-white wage gap is equally, if not more, informative because there are more black women working than black men.[3]

Many estimates of black-white wage differences for women find a statistically significant wage *premium* for blacks (Black et al., 2008; Fryer, 2011; Jacobsen et al., 2001; Fisher and

[1] Recent estimates using the Decennial Census show that the black-white log wage gap for men fell slightly between 1990 and 2000, from -.248 to -.226 (Black et al., 2009).

[2] Selection out of work is important for men as well, and trends in male labor force participation differ by race. See Chandra (2003); Neal and Johnson (1996).

[3] See Table A-2 in "The Employment Situation – January 2012" published by the Bureau of Labor Statistics. In January 2012, the number of employed Black or African American men was about 7 million. The corresponding employment level for Black or African American women was about 8 million.

Houseworth, 2011; O'Neill and O'Neill, 2005; Murnane et al., 1995). Black women have lower wages on average, but after controlling for ability (i.e., AFQT score), researchers find higher conditional wages for black women than white women. Estimates of conditional wage premiums for blacks range from 4 percent (O'Neill and O'Neill, 2005) to 12.7 percent (Fryer, 2011). Neal and Johnson (1996) and Fryer (2011) suggest that selection effects may explain away these estimated black wage premiums.

We begin by demonstrating that controls for selection into the labor market are not sufficient to explain the black wage premium in our sample of women in the 2008 National Longitudinal Survey of Youth 1979 (NLSY79). Even after controlling for selection with a popular imputation method, we estimate black wage premiums; we find a black wage premium of .13 log points using OLS and .17 using median regression. We next consider the role of education, even after controlling for ability. Lang and Manove (2011) show the importance of controlling for AFQT score as well as education in estimates of racial wage gaps. Similar to their results for black men, we find that once we account for both AFQT score and education, the estimated wage premium falls substantially.

We argue that failing to adequately control for the local cost of living contributes a great deal to the estimated black wage premiums. To the extent that black and white women face different local costs of living, estimates of raw wage differentials do not provide an adequate measure of labor market disadvantages faced by particular groups (see also Black et al., 2009; DuMond et al., 1999). Specifically, if blacks cluster in urban areas—which feature relatively high costs of living—then estimates of wage gaps that fail to account for these different costs of living will overstate how blacks are performing, relative to whites. We show that including controls for residential location drives the differential between blacks and whites to zero.

From the previous literature, the most consistent evidence of a black wage premium is among highly-educated women (Fisher and Houseworth, 2011; Black et al., 2008). These estimates do control for selection into the labor market, but do not incorporate our two additional innovations—controlling for ability as well as education received and accounting for the cost of living. We estimate wage gaps separately by education level to examine whether estimated wage premiums among highly educated women are robust to incorporating these changes, and we find the estimated premiums are not robust. Among women who attended or graduated from college, the coefficient estimate on black is always positive; however, this coefficient is only statistically significant in one out of four specifications. Interestingly, the situation is quite different among women with lower levels of education. For women who obtain no more than a high school degree, the coefficient estimate on black is always negative, although not statistically significant. Thus, our main finding of no wage differential between blacks and whites once we account for selection, educational attainment, and local cost of living, appears to be the average of the negative coefficient estimate for black women with lower levels of education and the positive coefficient estimate for highly educated black women.

In estimates of wage differentials, researchers typically control for age or potential experience, but workers with the same education and age may have very different work experience histories, which imply very different labor market productivities. If, on average, white women have more years of work experience than black women, then we expect blacks will appear to perform even better, relative to whites, when we control for actual labor market experience. However, racial differences in actual labor market experience might reflect discrimination in hiring or retention, so estimates that control for actual experience address a slightly different question and are not directly comparable to estimates that instead control for age or potential experience. When we control for actual labor market experience in our estimates of wage differentials among women in the NLSY79, our initial OLS and median regression estimates show no wage premium. However, once we account for selection, we observe a statistically significant premium for blacks (.04). We note that this estimate, which we would expect to show blacks performing even better relative to whites, still falls at the *lower* end of the estimated wage premiums documented in the literature. This finding provides further evidence of little to no wage premium for black women.

2. Related literature

Estimates of black wage premiums share one of two common features, and estimates that do not share these features actually find black wage penalties. First, many estimates of premiums restrict attention to late baby boomers (i.e., women born between 1954 and 1965) and include the AFQT score (or another test score) as a measure of ability. Most of these papers use the NLSY79 and include AFQT as a measure of ability (Fryer, 2011; O'Neill and O'Neill, 2005; Neal and Johnson, 1996). Murnane et al. (1995) take a similar approach and find a wage premium for black women in the National Longitudinal Study of the High School Class of 1972 (NLS72) and High School and Beyond Surveys when they include the IRT-scaled mathematics score.[4] These common features are important because other relatively recent studies that do not share these

[4] Participants in the NLSY79 are between the ages of 14 and 22 in 1979. Participants in the NLS72 were seniors in high school in 1972, and participants in High School and Beyond were high school seniors in 1980. Thus, the NLSY79, NLS72, and High School and Beyond surveys reflect women born between 1954 and 1965.

features find wage penalties for black women (Fryer, 2011; Neal, 2004; DuMond et al., 1996).[5] Although Fryer (2011) finds a large positive black wage premium among 41 to 49 year old women in the NLSY79, he documents a wage penalty of -.044 among the much younger women in the NLSY97. DuMond et al. (1999) and Neal (2004) present black wage penalties for women, but their specifications exclude the AFQT score, which is important to include to account for the higher skills that whites have in a group of white and black women with the same years of schooling.[6]

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[5] Two other studies quantify black white wage differentials among women using a different approach than the other papers described in this section (Anderson and Shapiro, 1996; Antecol and Bedard, 2002). Instead of the typical Mincer wage regressions that include a control for black, these papers estimate separate Mincer wage regressions for whites and blacks and use the Oaxaca-Blinder decomposition to describe wage differentials. Antecol and Bedard (2002) attribute all of the difference in log hourly wages to differences in the characteristics of white and black workers (Table 3 and p. 130), although there remains a significant unexplained portion of the wage gap in fixed-effects specifications (Table 4b and p. 133). Anderson and Shapiro (1996) find that between 52 and 83 percent of the variation in wages remains unexplained by worker characteristics and that black women's conditional wages (relative to white women) fell between 1980 and 1988.

[6] The main results in Neal (2004) also use the NLSY79. DuMond et al. (1999) examine a younger cohort of women in Current Population Survey (CPS) data; they use CPS data from 1986 through 1995 and restrict the sample to individuals age 16 and over. Thus, the youngest person in their sample could have been born as recently as 1979.

A second set of papers also find black wage premiums by focusing on highly educated women only (Fisher and Houseworth, 2011; Black et al., 2008). In this paper, we will replicate the black wage premiums among late baby boomer women in the NLSY79 and highly educated women in the NLSY79 (defined as having at least some college). We will then demonstrate that these estimated premiums are not robust to accounting for selection, including years of education, and controlling for local costs of living.

3. Empirical Strategy and Results

To control for the local cost of living, we acquired access to geocode NLSY79 data for the year 2008. The late baby boomer women in our sample are between the ages 43 and 51. We restrict our sample to black and white women, excluding Hispanics and women of other races or ethnicities. Our results are qualitatively similar when we include Hispanics, but we omit them because Hispanic women in the NLSY79 are not representative of the Hispanic population in 2008. We leave further inspection of Hispanic wage differentials for future work.

3A. The role of selection into the labor market on the black wage premium

Fryer (2011) and Neal and Johnson (1996) both cite selection into (or out of) the labor force as a potential explanation for their estimated wage premiums.[7] When wage gaps are computed based

[7] In footnote 7 on page 859, Fryer (2011) writes, "This may be due, in part, to differential selection out of the labor market between black and white women. See Neal (2005) for a detailed account of this." Neal and Johnson (1996) write, "…it is possible that selection effects

on observed wages, only those individuals with a valid wage are included in the calculations. Non-workers might also be an important group to consider in computing black-white wage differentials, especially if blacks and whites face different patterns in the decision not to work. Failing to account for selection out of work by women would result in blacks appearing to perform better than whites if black women with *low* potential wages are more likely to select out of work than white women (as found in Neal (2004)) and white women with *high* potential wages choose not to work.[8,9] We note that Fisher and Houseworth (2011) estimate black wage premiums and control for selection into or out of work, although their focus is on (highly educated) nurses and teachers. [10] They find that controlling for selection into nursing employment explains some, but not all, of the black wage premium for nurses.

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contaminate the estimates of racial wage gaps for women. For all women, the mean of observed wages likely overstates the mean of the wage offer distribution. If this selection effect is most acute in the minority samples, the results in table 1 will understate the wage costs of racial discrimination suffered by women." (page 875)

[8] Neal and Johnson (1996) hypothesize that highly skilled black women might be more likely to work if they have less unearned income than highly skilled white women.

[9] Antecol and Bedard (2002) also account for selection out of work. They do so by exploiting the panel nature of the NLSY79. They find that accounting for selection out of work decreases the share of the difference between black and white wages that is attributable to observable characteristics. When accounting for selection, between 57 and 29 percent of the wage penalty faced by blacks cannot be explained by differences in observable characteristics.

[10] Fisher and Houseworth (2011) attempt to control for selection into (or out of) the labor market in two ways. First, they use the Heckman correction method for selection bias. They also present

In recent work, the most common approach to address selection out of work is to impute a potential wage for the non-workers in the sample, and estimate median regressions of wage differentials (see, e.g., Johnson et al. (2000), Chandra (2003), Neal (2004)). Researchers include non-workers in the estimation sample based on the assumption that the wage the authors impute and the wage an individual could potentially earn (potential wage) fall on the same side of the median. Under this assumption, estimates are consistent for the population median without being sensitive to the chosen imputed value. Evidence shows that excluding non-workers who face low potential wages understates the wage gap, and blacks appear to perform better, relative to whites (see, e.g., Johnson et al. (2000), Chandra (1999), Chandra (2003)). Similarly, excluding non-workers who face high potential wages is also likely to understate the wage gap.

Neal (2004) shows that it is important to account for both types of selection out of work when computing estimates of the wage gap for women. He calculates the black-white wage gap for 1990, and imputes a low potential wage for women who did not work but received government aid between 1988 and 1992.[11] Considering selection out of work for non-workers with low potential wages increases the wage gap between .028 and .037 log points. Some women choose not to work even with a high potential wage. He identifies these women as non-workers who receive no public support between 1988 and 1992 and are married to a high-earning

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a second method adapted from Neal (2004). The authors impute wages for those not employed in nursing in the following way. They create cells by race, education, marital status, and children status, and assign non-working women the median wage for their respective cell.

[11] Women must also have received no postsecondary schooling and received no support from a spouse.

spouse.[12] When Neal (2004) also considers selection out of work by non-workers with high potential wages, the gap increases another .01 to .012 log points. In this paper, we account for differential selection out of the labor force for women with high and low potential wages, following Neal (2004).

We begin by first replicating estimates of the black wage premium and then testing whether the premiums are robust to accounting for selection into (or out of) the labor market, using the methodology described in Neal (2004). In Table 1, we first present results from OLS regression where we update the regressions from Neal and Johnson (1996) and Fryer (2011) using 2008 data from the NLSY79. In regressions that control for AFQT score, as well as age and its square (shown in column (1)), we find a black wage premium of .131 log points. This estimate is quite similar in size to that estimated for 2006 (.127) (see Fryer (2011)). In columns (2) through (4), we follow the existing literature in our attempt to control for selection into (or out of) the labor market. Column (2) presents results from median regression, as a baseline to compare to the OLS results in column (1). Controlling for AFQT score and age, we find the median black woman earns a .175 premium over the median white woman, even higher than the corresponding OLS estimate. It will be most appropriate to compare our results that account for selection with this estimate, since we examine conditional median wages when we account for selection.

In columns (3) and (4), we follow Neal (2004) and address selection into or out of the labor force by including non-working women for whom we can impute a low or high potential

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[12] That is, the spouse makes more than the 90th percentile in the income distribution among men of his own race (75th percentile in some specifications).

wage. We impute a low potential wage of $1 per hour for women who did not work between 2006 and 2008, did not receive support from a spouse during this period, received no postsecondary schooling, and received government aid each year from 2004 to 2008. As shown in Appendix A, we add 64 women to the sample by including women for whom we can impute a low potential wage, two-thirds of whom are black. We also impute a high potential wage of $45 for women who did not work between 2006 and 2008, have at least a high school education, and report average spousal earnings between 2003 and 2007 that place their husband's earnings above the ninetieth percentile of the earnings distribution for men of the same race. As shown in Appendix A, we add 39 women to the sample with this addition, over 90 percent of whom are white. Although these changes to the race-specific potential wage distributions would predict a decline in the estimated premium for black women, these additional women only increase the sample size by four percent. Thus, this first exercise that accounts for selection into the labor market results in no reduction of the black wage premium.

In column (4), we relax the restriction imposed on imputing high potential wages, adding women with high imputed wages whose spousal earnings fall between the 75^{th} and 89^{th} percentile in the distribution of earnings. This allows us to add 21 more women to the sample. Even with this broader imputation rule, we continue to find a positive and statistically significant wage premium for blacks. Thus, we conclude that the black wage premiums documented in the prior literature cannot merely be explained away by accounting for selection, at least not using the method that is most common in the literature. We now examine whether the estimated premium is robust to including controls for years of education and cost of living.

3.B Controlling for Education

Including AFQT score without also including years of education is appropriate only if, conditional on ability, blacks and whites attain the same level of education (Lang and Manove (2011)). In fact, conditional on ability, blacks obtain higher levels of education than whites, and Lang and Manove (2011) show that including both the AFQT score and education causes the male wage gap to widen between .06 and .08 log points.[13]

In Table 2, we now present results of the black-white wage differentials for women where we control for both education and AFQT score. Since this widened the black-white wage gap for men, we expect including educational attainment will reduce the black wage premium for women. In Panel A, we reproduce our estimates on the coefficient for black from Table 1. In Panel B, we present results that also incorporate years of education. In Panel B, the coefficient

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[13] Omitting years of education would be appropriate if the AFQT score already incorporates all of the labor market skills associated with schooling. One could argue that blacks have higher education levels than whites—conditional on AFQT—because school quality is lower for black students, and it takes more of such schooling to achieve the same AFQT score. In this case, including years of schooling would incorrectly reduce conditional wages for blacks, whose schooling adds less to productivity than schooling of whites. However, Lang and Manove (2011) show that a host of school characteristics associated with quality do not substantially change the higher education levels of blacks relative to whites, conditional on AFQT. In addition, NLSY79 sample members obtained a substantial number of schooling years after taking the AFQT, so the years of education variable probably includes additional information about respondents' labor market skills.

on black is now much smaller in magnitude than the corresponding result from Panel A. Using OLS, in column (1) our estimate of the black wage premium is no longer statistically significant and is less than one third of the size of the corresponding estimate that excluded years of education, .041 versus .131. When we instead use median regression in column (2), our estimate is now statistically significant, but falls by more than half, from .175 to .076. Interestingly, the coefficient estimate now falls slightly when we account for selection, ranging between .055 in column (3) and .059 in column (4). We conclude that once we control for both AFQT score and education, as is now suggested by Lang and Manove (2011), the estimated wage premium decreases markedly. In the next section, we consider the role of cost of living. We expect that controlling for cost of living will further reduce the estimated black wage premium.

3.C. Controlling for Residential Location

Since blacks are more likely than whites to live where costs of living are high, failing to control for where individuals live is likely to result in an overstated black-white wage premium. In this section, we examine the importance of adequately controlling for cost of living. Black et al. (2009) consider the implications for estimates of wage differentials when individuals live in locations with different prices and show that estimates of black-white wage differentials can only be estimated consistently if one accounts for where individuals live. Black et al. (2009) run specifications with fixed effects for location (defined as a metropolitan statistical area or the balance of the state in non-metro areas). They find that controlling for residential location increases the black-white wage gap for males by between 3 and 5 percentage points.

DuMond, Hirsch, and Macpherson (1999) demonstrate a second approach. They estimate black-white and Hispanic-white wage gaps for women in a pooled sample of CPS surveys from 1985 to 1995. They restrict the sample to women living in a CMSA/MSA and include an explicit local cost-of-living measure that they create for 185 CMSAs and MSAs. As expected, they demonstrate that wages of black and Hispanic women fall relative to white women after controlling for cost of living.[14]

We begin by demonstrating that black and white women face systematically different costs of living where they live. If the average local cost of living is higher for black women, then failing to control for residential location will yield wage gap estimates that understate racial gaps in purchasing power. This is what we find.[15] We measure locations as commuting zones (CZs), which are collections of counties defined by the U.S. Department of Agriculture to have significant economic interaction, measured by journey-to-work links (Tolbert and Sizer, 1996). In metropolitan areas, commuting zones and MSAs overlap significantly. The real advantage of using commuting zones as a unit of geography is seen in rural areas. With commuting zones, we do not need to drop all non-MSA areas or pool them together within each state or Census region

[14] O'Neill and O'Neill (2005) include three indicators for region, an indicator for living in an MSA/SMSA, and an indicator for living in a central city. Fisher and Houseworth (2011) include three indicators for region and indicators for isolated rural place, small rural place, and large rural place (urban is the omitted category). Since urban areas tend to have higher prices than rural areas, such a method captures some variation in costs of living, but a more explicit accounting of costs of living improves the reliability of the estimates.

[15] We also find that Hispanic women in the 2009 American Community Survey and the NLSY79 in 2008 live in locations with higher costs of living than both white and black women.

(two common methods). Pooling is costly, since rural areas within a state can vary considerably. Consider Colorado, whose rural areas include both the mountain country featuring tourist towns like Breckenridge but also the much less snow-filled San Luis Valley in the south central part of the state. The average monthly rental price of 2 and 3 bedroom dwellings in the group of counties including Breckenridge is $1,150 but only $540 in the San Luis Valley (American Community Surveys, 2005-09).

Housing is the most important local price in consumers' budgets, and we use it to proxy for local costs of living. Banzhaf and Farooque (2012) compare alternative methods for measuring local housing costs and find that average rental prices perform well: they are closely associated with housing transaction price data (which are more costly to collect), and rental prices are closely associated with measured local amenities and average incomes.[16] Using

[16] Handbury (2012) uses location-specific grocery prices and consumer demand behavior to estimate the relationship between local prices and the cost of attaining a certain level of utility. She finds that cities with relatively high prices (e.g., San Francisco) also have greater availability of products that very high-income consumers enjoy, so it is actually less expensive for a high-income resident to attain a given level of utility there than it would be in a city with lower average prices (where it requires great effort for a high-income consumer to attain the desired goods or find substitutes). However, Handbury (2012) finds that for most households (those with less than about $100,000 annual income per member), average local prices are pretty closely associated with the cost of attaining a given level of utility. We interpret this to imply that for a very large share of the women in our sample, the average local price measure is appropriate to use as a control for local costs in wage regressions. Also, specifications with location fixed effects yield very similar results.

households from the pooled 2005 to 2009 annual American Community Surveys (ACS) data, we calculate average gross monthly rent (which includes utility costs) for 2- and 3-bedroom dwellings in each commuting zone. [17,18] In Table 3, Panel A we present descriptive statistics that quantify the housing costs in CZs where white and black women in the 2009 ACS live. Column (1) shows that whites live in areas with the lowest cost of living and blacks live in areas with the highest cost of living. Blacks in our sample face a mean monthly rent of $910 versus $850 for whites.[19] The remaining columns of the table show that the white and black ranking in costs of living is present at several quantiles of the cost-of-living distribution. We draw on the measures of monthly rent computed from the ACS for our analysis of the NLSY79 data. In Panel B, we show that the black women in our NLSY79 sample also live in areas with higher costs of living.

In Table 4, we show results of log wage regressions that now include controls that account for differences in cost of living. In Panel A, we include fixed effects for the respondent's CZ of residence. This is the approach that Black et al. (2009) took for their sample of men, except that our locations are CZs rather than MSAs (and the rest of the state). When we include

[17] The smallest identifiable area in the ACS is the public use microdata area (PUMA), which is a Census-defined place with population no less than 100,000. This definition does not allow perfect matching of boundaries for all CZs. The method used to convert PUMA averages to CZ averages involves assigning PUMA characteristics to a CZ based on the population weight of the PUMA in the CZ. See McHenry (2011) for a more detailed description of the method.

[18] The housing price calculation follows Moretti (2011), who uses the 1980 and 2000 U.S. Censuses.

[19] Differences in local house prices are similar, since local house prices and apartment rents move together.

CZ fixed effects, we find that, as expected, the black wage premium completely disappears. In each specification (OLS, median regression, median regression with high and low wages imputed to account for selection), the coefficient estimates are very close to zero and never statistically significant, ranging from -.014 to .012.

Of course, wages might be higher in certain (mostly urban) CZs to compensate for the higher cost of living, but wages might also be higher because workers in certain urban areas are, on average, more productive (see Yankow, 2006 and Glaeser and Mare, 2001 on this topic). In Panel B, we test the robustness of the fixed-effects results by controlling explicitly for a measure of the cost of living in one's residential location. This is similar to the approach that DuMond et al. (1999) took for individuals residing in a MSA/CMSA.

We use housing costs to approximate local cost of living as follows. Using the housing rental data for the pooled 2005 to 2009 ACS surveys described above, we first construct a measure of relative housing costs for each CZ. We define relative housing costs as the mean rent in a CZ divided by the average rent over all CZs. Since housing costs comprise only 42 percent of household expenditures in the 2007 consumer price index calculation, we construct a cost of living index that weights our relative housing measure in this way.[20]

In Panel B, we present results with our control for the local cost of living index. We find that estimates of the black-white wage gap are largely robust to these two alternative approaches

[20] That is, the CZ housing cost measure is computed as follows:

$HousingCost_{CZ} = \frac{MeanRent_{CZ}}{(\sum_{CZ=1}^{N} MeanRent_{CZ})/N}$ and the cost of living is computed as

$CostofLiving_{CZ} = .42 * HousingCost_{CZ} + .58 * 1.$

to controlling for local costs of living (CZ fixed effects and CZ housing costs). We find a strong, positive, statistically significant relationship between the local cost of living index and log(hourly wages). However, as in Panel A which included fixed effects for commuting zone of residence, we find no evidence of a black wage premium. The coefficient estimates are small and never statistically significant, ranging from .002 to .024 in OLS and median regression specifications. Accounting for selection into the labor market (columns (3) and (4)) makes little difference to the estimated black-white wage gap in this context.

3.D Premium Estimates by Educational Attainment

Several papers document a black wage premium among highly educated—or highly skilled—black women (see, e.g., Houseworth and Fisher (2011) and Black et al. (2008)). We now separately examine racial wage differences for women with a high school degree or less versus women with at least some college.[21] We present these results in Table 5, separately for women with a high school degree or less and women who attended or graduated from college. In Panel A of Table 5, we find, if anything, evidence of wage penalties for less educated black women. The coefficient estimates are always negative, ranging from -.047 to -.034, though the standard errors are too large to rule out positive and negative effects. For example, in column (1), the 95 percent confidence interval around this OLS estimate includes a premium of .03 and a penalty of -.13.

[21] We would prefer to isolate the most highly educated women with a college degree or more and use four educational categories; however, the sample sizes are too small in the NLSY79 to give us much statistical power. We present the results by four educational categories in Appendix B.

In Panel B, we turn to women with at least some college. Although our coefficient estimates are all positive, the standard errors are large enough that only one estimate is statistically significant. What is notable is that even the upper bound of the 95 percent confidence interval on the OLS estimate in column (1) is smaller than the wage premium estimated by Fryer (2011) (.117 versus .127) and the baseline wage premium we estimated in Table 1 (.131). We also conclude that our baseline estimates of no wage differential between black and white women (shown in Panel A), are likely the average of negative coefficient estimates for less educated black women and small positive coefficient estimates for highly educated black women.

Our estimates to this point do not provide strong support for a black wage premium. Although we replicate the black wage premium reported previously in the literature, this premium falls significantly once we include educational attainment and is completely eliminated as soon as we also control for residential cost of living. Previous work has found particularly strong evidence of a black wage premium among highly educated women, and we find only modest evidence of a premium among this group, and this estimated premium is sensitive to the specification used. In the next section, we evaluate the effect of controlling for actual labor market experience. If there is discrimination against black women in hiring and firing, then we would expect that blacks would appear to perform best, relative to whites, in estimates of black-white wage differentials that control for actual labor market experience. Thus, if we continue to find little evidence of a black wage premium even after controlling for actual labor market experience, we will view these results as strong evidence against the existence of such a premium.

4. Role of Actual Labor Market Experience

Black women have significantly fewer years of actual labor market experience, relative to whites.[22] Therefore, comparing white and black women with the same *potential* experience would tend to understate the human capital white women have developed. We expect that estimates of black-white wage differences that only include controls for a worker's age or potential experience, such as the estimates described above, would result in blacks appearing to earn lower wages, relative to whites. Antecol and Bedard (2002, 2004) show that since labor market attachment differs by race, actual experience explains much more of the wage gap than potential experience.[23] Antecol and Bedard (2002) find that length of work experience accounts for between 54-61 percent of the black-white wage gap among women.

Most estimates of black-white differences in wages do not include actual labor market experience. This may be due to data availability or it may be due to the fact that estimates of wage differentials often attempt to quantify racial discrimination, and differences in actual labor market experience may arise due to discrimination in hiring and retention. If minority women achieve lower levels of actual experience because they are the last hired and the first fired by discriminatory employers, then estimates of black-white wage differences that control for actual labor market experience do not capture the effect of this important discriminatory mechanism.

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[22] In results available upon request, we find that black women have between .6 to 2 fewer years of actual labor market experience than white women, after we control for age, educational attainment, and in some specifications, AFQT score.

[23] Fisher and Houseworth (2011) also include controls for years employed in the nursing profession.

For this reason, specifications that do not control for actual experience incorporate a potentially fuller picture of labor market discrimination and differential opportunities across groups.

In Table 6, we now include years of actual labor market experience and its square as independent variables in log wage regressions. In every case (OLS, median regression, and the two imputation methods), the coefficient estimate on black is larger in magnitude than in regressions that instead controlled for a woman's age. However, the OLS estimate in column (1) and the median regression estimate in column (2) both fail to achieve statistical significance. Thus, even when we control for differences in human capital attributable to differences in hiring and retention, we find no evidence that, without accounting for selection, there are black wage premiums. We now find a black wage premium when we account for selection in column (3); however, it is worth noting that this estimated premium of .041 is dramatically smaller than the most recent estimates in the literature (e.g., .127). This suggests that different labor market experiences of black and white women play an important role in observed differences in the return to hours worked. This finding has important implications for policies to reduce racial disparities in promotion, tenure, and hiring.

Since the strongest evidence we find for a black wage premium is sensitive to the chosen specification and controls for actual labor market experience--which may control for discrimination in hiring and firing--we interpret the estimates in Table 6 to be in the high range of likely relative wages of black women. Consequently, we argue that there is little evidence of a black wage premium among women overall.

5. Conclusion

In this paper, we update estimates of female wage differences by race. We find no evidence of widespread wage premiums for black women in our analysis of all black and white women once we account for selection into the labor market, actual educational attainment, and cost of living in one's residential location. Contrary to the hypotheses proposed in the literature, we find no evidence that accounting for selection explains the entire wage premium. Instead, we find these estimated wage premiums are sensitive to controls for educational attainment and residential location. Thus, our work offers little support for the wage premiums estimated elsewhere in the literature, and we show the typical explanation for the premiums—selection into or out of the labor force—does not explain the prior estimates.

One strand of the literature documents wage premiums among more highly educated black women. When we examine racial wage differences between women of different levels of education, we find weak evidence of a wage premium for more highly educated black women who have at least some college and coefficient estimates are always negative (though never statistically significant) for black women with no more than a high school degree. This suggests that our finding of no wage differences reported above may simply be the average of small positive and negative coefficient estimates for different groups of women. This finding has important policy implications for efforts to improve labor market outcomes for black women: policymakers should focus on labor market experiences of less educated black women, who seem to be the most disadvantaged.

We then broaden the question we ask of our data and consider actual labor market experience. Since whites acquire more experience than similar blacks, we expect that blacks will appear to perform poorly, relative whites, in estimates that exclude actual labor market experience. Of course, racial differences in labor market experience may arise from discrimination in hiring or firing, which is an important outcome on its own, so we interpret these results as falling in the high range of black relative wages. We note that even these estimates are at the low end of the wage premiums reported in the literature. This supports our interpretation of little evidence of a wage premium.

In future work, we seek to examine wage differences among different cohorts and ethnicities. We intend to replicate the work presented in this paper for a younger cohort—women in the NLSY97. This is especially important to do in light of recent evidence from Fryer et al. (2011) that finds evidence of *lower* offer wages for black women than white women, but we note that the women in their sample are younger than the women in the NLSY79. [24] These age differences are important if discrimination in offer wages declines as individuals age and potential employers have more information with which to judge a worker's quality. A second important difference between the Fryer et al. (2011) estimates and the work presented in this paper is that the UI data contain estimates of racial differences in offer wages, whereas our NLSY79 data capture racial differences in wages at a point in time—which include offer wages for new hires as well as wages for workers with more tenure with their existing employer. By examining the NLSY97 data, we will discover whether our methods reveal a black wage premium and also what role job tenure has played in racial differentials among younger women.

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[24] The mean age for black women in our analysis is 46.6 in the NLSY79 data, nearly ten years higher than the mean age in the data Fryer et al. (2011) use (37.6) (New Jersey Unemployment Insurance data). A similar difference exists for the white women; the mean age of white women in the NLSY79 is 46.7, two years older than the white women in the UI data (44.7).

We also seek a better understanding of Hispanic wage premiums. We excluded Hispanic women from the sample used to construct the results shown in this paper because Hispanic women in the NLSY79 are not representative of the Hispanic population in 2008. Nevertheless, we did find large conditional wage premiums among Hispanic women in the NLSY79, and they were not sensitive to the innovations that we show matter so much for estimates of the black wage premium. In future work, we will take a closer look at this robust wage premium for Hispanic women in the NLSY79 and NLSY97. The Hispanic women in the NLSY97 in 2008 are more representative of Hispanics in their age cohort throughout the U.S. population, relative to the NLSY79: respondents to the NLSY97 are only constrained to have been in the country for about 10 years by 2008, whereas women in the NLSY79 by 2008 had to be in the U.S. for about 30 years (so the sample misses a greater share of immigration).

While it is encouraging that our results suggest there are no wage gaps by race after decades of wage penalties for black workers, we caution policymakers that there is still much to do to improve labor market outcomes and the efficacy of widespread public programs for racial and ethnic minorities. We find that, even after controlling for age, black women have 1.5 to 2 fewer years of actual work experience than white women. Since additional years of work experience boost wages, we draw attention to experience gaps by race. This may have important policy implications for discrimination in retention, hiring, and promotion.

We also note that black women are more likely to live in areas with a high cost of living, and that this has important policy implications, especially for federal tax and transfer policies that do not vary by cost of living. Recent research on tax policy has shown that since the federal tax code does not contain cost of living adjustments, urban dwellers receive less purchasing power from the Earned Income Tax Credit (EITC) (see, e.g., Fitzpatrick and Thompson, 2010) and pay more in federal taxes (see, e.g., Albouy (2009)), relative to rural dwellers. With a higher share of black women living in such high cost-of-living areas, black women likely receive less from the EITC and pay more in federal taxes than otherwise-similar white women. Therefore, even though we find no evidence of racial disparities in conditional wages there remain important racial disparities under the tax code.

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Table 1: Role of Selection Out of Work in Estimates of Racial Differences in Ln(Hourly Wages) for Women in 2008, Results from the NLSY79

	(1)	(2)	(3)	(4)
	OLS	Median Regression	Median Regression	Median Regression
		No imputation	*Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	*Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
Black	.131***	.175***	.177***	.172***
	(0.028)	(0.031)	(0.028)	(0.025)
AFQT score	.271***	.300***	.325***	.328***
	(0.014)	(0.013)	(0.015)	(0.014)
N	2,586	2,586	2,689	2,710

Dependent variable is the natural log of the hourly wage, or the log of the imputed wage. Each regression also includes age (in years) and its square. In column (1), heteroskedasticity robust standard errors in parentheses. Columns (2) through (4) contain results from median regression where standard errors are computed by bootstrap (50 replications). In columns (3) and (4), wages are imputed for women who are detached from the labor market but for whom we infer high or low potential wages based on education and household income (see text for details). In column (3), high potential wages are imputed for women whose spouse earns above the 90th percentile, and in column (4), high potential wages are imputed for women whose spouse earns above the 75th percentile.

Table 2: Role of Years of Education and Selection Out of Work in Estimates of Racial Differences in Ln(Hourly Wages) for Women in 2008, Results from the NLSY79

	(1) OLS	(2) Median Regression *No imputation*	(3) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	(4) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
Panel A: No control for years of education				
Black	.131***	.175***	.177***	.172***
	(0.028)	(0.031)	(0.028)	(0.025)
Panel B: Control for years of education				
Black	.041	.076**	.055*	.059**
	(0.029)	(0.033)	(0.033)	(0.027)
Years of Education	.078***	.073***	.081***	.081***
	(0.006)	(0.008)	(0.007)	(0.007)
AFQT score	.152***	.191***	.201***	.204***
	(0.017)	(0.021)	(0.020)	(0.019)
N	2,586	2,586	2,689	2,710

Panel A repeats results from Table 1. Panel B adds years of education to those specifications. See notes to Table 1.

Table 3: Local Cost of Living by Race, Average Monthly Rent for 2 and 3-bedroom Property

		Percentile in the Distribution of Monthly Rent				
	Mean	10th	25th	50th	75th	90th
Panel A: 2009 American Community Survey						
White	850 (254) [441,548]	557	651	789	1,014	1,278
Black	910 (266) [61,005]	572	681	878	1,080	1,309
Panel B: 2008 National Longitudinal Survey of Youth, 1979						
White	827 (243) [1,716]	528	648	787	980	1,187
Black	866 (247) [1,077]	603	669	854	998	1,278

Table notes: Summary statistics about the cost of housing where respondents live using the ACS and NLSY79 samples. The cost of housing measure is the average monthly rent for 2- and 3-bedroom single-family dwellings in the respondent's commuting zone (CZ). CZ-average monthly rent data calculated using the pooled 2005-2009 ACS samples from IPUMS (Ruggles, et. al. 2010). We calculate average "gross monthly rent" over households in each PUMA and aggregate to CZs with averages weighted by population overlaps between PUMAs and CZs. Left-most column shows for each respondent category the mean, standard deviation (in parentheses), and sample size (in brackets) of residence CZ average rental prices. The remaining columns show percentiles of the residence CZ average rental price distribution within each respondent category (e.g., the 10^{th} percentile of CZ rental price averages among white women in the ACS sample).

Table 4: Impact of Controls for Cost of Living on Estimates of Racial Differences in Ln(Hourly Wages) for Women

	(1) OLS	(2) Median Regression *No imputation*	(3) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	(4) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
		Panel A: Include Commuting Zone Fixed Effects		
Black	.004 (.035)	.012 (.044)	-.008 (.044)	-.014 (.048)
Years of Education	.078** (.006)	.074** (.008)	.080** (.008)	.079** (.007)
AFQT score	.140** (.018)	.165** (.018)	.178** (.022)	.180** (.022)
N	2,586	2,586	2,689	2,710
		Panel B: Include Local Cost of Living Index		
Black	.002 (0.028)	.024 (0.031)	.011 (0.030)	.005 (0.032)
Years of Education	.073*** (0.006)	.069*** (0.006)	.075*** (0.005)	.075*** (0.005)
AFQT score	.139*** (0.017)	.171*** (0.015)	.184*** (0.019)	.183*** (0.016)
Local Cost of Living Index	.752*** (0.069)	.835*** (0.075)	.875*** (0.073)	.875*** (0.084)
N	2,586	2,586	2,689	2,710

See notes to Table 2.

Table 5: Racial Differences in Ln(Hourly Wages) for Women, by Education Level, Results from the 2008 NLSY79

	(1) OLS	(2) Median Regression *No imputation*	(3) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	(4) Median Regression *Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
		Panel A: High school or less		
Black	-.047 (0.041)	-.037 (0.040)	-.034 (0.050)	-.034 (0.047)
N	1,133	1,133	1,206	1,216
		Panel B: At least some college		
Black	.038 (.040)	.074* (0.038)	.050 (0.042)	.043 (0.032)
N	1,453	1,453	1,483	1,494

Panel A selects only women with a high school degree or less education, but who never attended college. Panel B selects only women who attended or graduated from college. Each regression also includes the local cost of living index. See notes to Table 2, Panel B.

Table 6: Impact of Actual Years of Work Experience on Estimates of Racial Differences in Ln(Hourly Wages) for Women in 2008

	(1)	(2)	(3)	(4)
	OLS	Median Regression	Median Regression	Median Regression
		No imputation	*Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	*Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
		Panel A: Baseline results		
Black	.002	.024	.011	.005
	(0.028)	(0.031)	(0.030)	(0.032)
		Panel B: Control for actual labor market experience		
Black	.025	.032	.041**	.037
	(0.026)	(0.023)	(0.019)	(0.026)
Years of Experience	-.0004	.004	.037***	.030**
	(0.008)	(0.008)	(0.011)	(0.009)
Years of Experience^2	.095***	.079***	.002	.015
	(0.020)	(0.018)	(0.026)	(0.022)
Years of Education	.0641***	.059***	.062***	.063***
	(0.006)	(0.004)	(0.005)	(0.006)
AFQT score	.087***	.086***	.104***	.108***
	(0.016)	(0.015)	(0.016)	(0.018)
Local Cost of Living Index	.730***	.872***	.857***	.869***
	(0.063)	(0.051)	(0.064)	(0.051)
N	2,586	2,586	2,689	2,710

Panel A repeats results from Panel B in Table 4, where the specifications also include controls for age, age squared, the local cost of living index, AFQT score, and years of schooling. Panel B replaces age with years of labor market experience.

Appendix Table A: Sample Size by Race, With and Without Wage Imputations

	2008 NLSY79
<u>OLS Regression Sample:</u>	
Black	1,000
White	1,586
Total	2,586
<u>Median Regression Sample:</u>	
Low imputed wages:	
Black	43
White	21
Total	64
High imputed wages, spouse earns above 90th percentile:	
Black	3
White	36
Total	39
Total Sample Size	2,689
High imputed wages, spouse earns between the 75-89th percentile:	
Black	4
White	17
Total	21
	2,710

Table notes: Authors' calculations for the year 2008 in the NLSY79. We impute a low wage for women who have not worked in the past five years (have no valid wage in the 2006 or 2008 surveys), received government welfare payments in all of the previous five years, have no post-secondary schooling, and have either no spouse or whose spouse has no income identified for the past five years. We impute a high wage for women who have no valid wage in the 2006 or 2008 surveys, have at least a high school diploma, and have a spouse who earns more than the 75th or 90th percentile among men's earnings in his race category in the NLSY79 (the 75th percentile is \$46,000 for blacks and \$70,000 for whites; the 90th percentile is \$67,000 for blacks and \$110,000 for whites).

Appendix Table B: Racial Differences in Ln(Hourly Wages) for Women, by Education Level, Results from the 2008 NLSY79

	(1)	(2)	(3)	(4)
	OLS	Median Regression	Median Regression	Median Regression
		No imputation	*Impute low potential wages; impute high potential wages if spouse earns above the 90th percentile*	*Impute low potential wages; impute high potential wages if spouse earns above the 75th percentile*
		Panel A: Less than high school		
Black	-.081	-.089	-.149	-.149*
	(0.082)	(0.099)	(0.099)	(0.084)
N	255	255	293	293
		Panel B: High school degree		
Black	-.041	.014	-.008	-.002
	(0.047)	(0.054)	(0.043)	(0.049)
N	878	878	913	923
		Panel C: Some post-secondary schooling, no college degree		
Black	.058	.107*	.086*	.081
	(0.054)	(0.056)	(0.050)	(0.055)
N	813	813	819	823
		Panel D: Bachelor's degree or more		
Black	.034	.048	-.003	-.003
	(0.060)	(0.068)	(0.072)	(0.072)
N	640	640	664	671

Each panel reflects specifications with a separate sample defined by education attainment, denoted in the panel title. See notes to Table 5.