Minimum Wages and Racial Differences in Hiring: Theory and Evidence from a Field Experiment*

Alec Brandon
Johns Hopkins University

Justin E. Holz
University of Chicago
ANU RSE
(Job Market Paper)

Andrew Simon
University of Chicago
ANU RSE

Haruka Uchida
University of Chicago

October 2022

Abstract

When minimum wages increase, discriminating employers may try to avoid regulatory burdens by substituting away from disadvantaged workers. We test this hypothesis using a resume correspondence study with 35,000 applications around three ex-ante uncertain minimum wage changes. Before the increases, applicants with distinctively Black names were 19% less likely to receive a callback than equally qualified applicants with distinctively white names. Announcements of minimum wage hikes substantially reduce the callbacks for both types of applicants but shrink the racial callback gap by 80% for the subsequent year for which we have data. We interpret our results through a hiring model to show that the gap shrinks because white applicants are more likely to be marginal, partly due to statistical discrimination. We show how researchers or policymakers can use our framework, without policy variation, to predict how labor market policies will change racial disparities in other settings.

*This research was conducted with approval from the University of Chicago Institutional Review Board, IRB18-0873. This study has been pre-registered under AEARCTR-0003333. We thank Dan Alexander, Bocar Ba, Chris Blattman, John Bound, Leonardo Bursztyn, Jeffery Clemens, Steven Durlauf, Yana Gallen, Sara Heller, Rafael Jiménez Durán, Attila Lindner, John List, Jonathan Meer, Anant Nyshadham, David Philips, Mattie Toma, Derek Wu and the audiences at Advances in Field Experiments 2019 Conference, the Institute for Research on Poverty Summer Research Workshop at the University of Wisconsin IRP (SRW), the Online Seminar on Discrimination and Disparities, University of Michigan, Harris School of Public Policy, and the University of Chicago Experimental Workshop for helpful feedback. Jacob Benge, Noelani Bernal, Emilia Van Buskirk, Lawrence Guindine, David Liu, Jim Liu, Kelly Liu, Beatrice Del Negro, Naina Prasad, Diego Ruiz, Steven Shi, Alejandro Turriago, Josh Xu, and Daniel Zhu provided excellent research assistance. This research was funded in part by grants from the UChicago Experiments Initiative, the University of Michigan, Department of Economics and Rackham Graduate School, the Institute for Education Sciences grant R305B150012, and the W.E. Upjohn Institute for Employment Research.

1. Introduction

Twenty-seven million workers earned less than $12 an hour in 2019 (Shrider et al., 2021). Minimum wages are popular policies meant to redistribute income to these workers, while critics argue that minimum wages decrease employment, undermining its goals. A large literature evaluating these claims finds that hikes increase the average earnings of all low-wage workers, with little effect on the number of low-wage jobs (Card and Krueger, 1994; Cengiz et al., 2019) that belie larger changes in hiring and transitions (Dube et al., 2016). An additional redistributional concern is that minimum wage hikes disproportionately harm disadvantaged groups, but there is little empirical evidence evaluating these claims (Friedman, 1966; Stiglitz, 1973; Clemens et al., 2021).

We conduct a field experiment to test whether increases in the minimum wage exacerbate or reduce labor market disparities. We sent nearly 35,000 fictitious job applications to online low-wage job postings by firms in three US states before and after minimum wage increases were announced and a contiguous state that did not change its policy.[1] In each resume, we independently randomized the perceived race, human capital, unemployment duration, and other resume characteristics.[2] Combining randomization with policy variation, we provide causal evidence on how minimum wage increases affect racial hiring disparities.[3]

We find that before the announcement of recent minimum wage increases, job applicants with distinctly Black names were 3.2 percentage points (19%) less likely to receive a callback than applicants with distinctly white names, consistent with previous work (Quillian et al., 2017). The racial callback gap shrinks by about 2.6 percentage points (80%) after announcements indicating that the minimum wages will change. The gap shrinks immediately after the announcement, before it is enacted, and persists for at least the following two years for which we have data.

[1]Two minimum wage changes resulted from referenda passing. The third was due to the state legislator voting to increase the minimum wage.

[2]See Jowell and Prescott-Clarke (1970), Bertrand and Mullainathan (2004), Kroft et al. (2013), and the papers reviewed in Bertrand and Duflo (2017) for other experiments using this approach.

[3]Our approach is similar to Agan and Starr (2018) who study the impacts of "ban the box."

We then illustrate how changes in the racial callback gap due to minimum wage hikes depend on different types of discrimination. We first formalize previous economic arguments about the impact of the minimum wage (e.g. Friedman, 1966; Stiglitz, 1973; Thomas, 2007) in a firm hiring model with both taste-based and statistical discrimination (Becker, 1957; Aigner and Cain, 1977). We show how these mechanisms jointly determine which types of workers are on the margin of receiving a callback and thereby affected by the policy change.

Following Neumark (2012) and Neumark et al. (2019), we separately identify the ratio of the variances of perceived applicant productivity by race, one form of statistical discrimination, as well as a composite term that reflects both taste-based discrimination and statistical discrimination in mean productivity. Because white applicants have less dispersed perceived productivity distribution and are favored in hiring, a larger share of the initially marginal applicants are white, and therefore experience a larger decrease in callbacks. Since the nature and extent of discrimination, along with the frequency of callbacks, all jointly determine the magnitude of the change in the racial callback gap from minimum wage increases, our work provides a framework to understand when and how the policy change will affect the labor market racial disparities. We additionally test whether the decrease in the racial callback gap is due to explanations outside the scope of our model. For example, minimum wage hikes may cause firms to pay more attention to non-race aspects of the resume, change the composition of firms hiring workers, or change the composition of the labor pool. Though these other explanations would not change the policy implication that minimum wages reduce racial disparities in hiring, we address these potential issues to more cleanly isolate the mechanisms.

The richness of our data allows us to rule out each of these competing hypotheses. Exogenous variation in the productivity of the applications allows us to rule out increasing returns to the productivity of Black applicants relative to white applicants. By showing similar effects of the minimum wage across establishments within the same parent company and within establishments themselves, we can rule out changes in the composition of hiring firms driving these results. Finally, our research design incorporates randomized saturation in the portion of Black and long-term unemployed applications, allowing us to identify the spillover effects of one resume on another. Finding no evidence that resumes are rivalrous within an establishment makes changing labor supply of non-experimental applicants an unlikely explanation of the results.

Our paper's central contribution is combining resume-level randomization and state-level policy variation to provide causal evidence that minimum wage hikes reduce racial disparities in hiring.[4] We build on work by Derenoncourt and Montialoux (2021) and Bailey et al. (2021) who both find that the 1966 Fair Labor Standards Act increased wages, especially for Black workers. But, the disemployment effects are sensitive to how employment is measured and which controls are included.[5] Additionally, most previous work focuses on employment, wages, and earnings of overall[6] or teenagers (e.g. Kreiner et al., 2020), or those with low levels of education (Clemens and Wither, 2019). Our results on callbacks complement this literature by allowing us to examine job flows rather than stock, which Meer and West (2016) shows have a delayed response to the minimum wage. Since the correspondence study revolves around job postings, we additionally use the ads' text to inform how minimum wage increases affect which skills employers demand, building on Clemens et al. (2021), to understand labor-labor substitution. We show that although ads request higher-quality workers when the minimum wage increases, there is little evidence that they call back higher-

[4] See Turner and Demiralp (2001), Cengiz et al. (2019), Wursten and Reich (2021), Bailey et al. (2021), and Derenoncourt and Montialoux (2021)

[5] Nonetheless, a very large national minimum wage increase in 1967 for select industries may not have the same effects on racial disparities as more recent state-level changes. For example, Derenoncourt and Montialoux (2021) argue that there was a very small effect on disemployment because labor demand was inelastic, perhaps due to tight labor markets, and that certain important industries had monopsony power (e.g., laundries in the South). They also argue that there was near zero labor-labor substitution from Black to white workers, partly because of the racial segregation of job characteristics. The effects of the 1967 increase should also be considered with the Civil Rights Act since both shaped labor markets and racial disparities at the time, changing access to employment and wages. While the impacts of the minimum wage could be similar across periods because racial discrimination and earnings differences persist to the present day, changes in institutions, labor market conditions, and segregation could lead the policy to have different effects. We, therefore, view our work as complementary to these papers.

[6] Neumark et al. (2007) provide a recent overview of the literature.

quality applicants, that is, those with more education or a lower unemployment duration.[7]

More generally, this finding expands our limited understanding of which policies effectively reduce labor market racial disparities. Previous work has considered the role of anti-discrimination policies like the 1964 Civil Rights Act (e.g. Brown, 1984), Voting Rights Acts (Donohue and Heckman, 1991), along with educational desegregation, attainment, and quality (e.g. Lillard et al., 1986; Card and Krueger, 1992; Johnson, 2019). Our results indicate that minimum wage increases also reduce racial disparities.[8]

Second, we contribute to the literature attempting to parse discrimination mechanisms by explicitly mapping the magnitudes of taste-based and statistical discrimination to policy consequences. We do this in two steps. We first estimate functions of the taste-based and statistical discrimination parameters, without the policy variation, following Neumark (2012).[9] Our large-scale correspondence study confirms that firms hold these beliefs based on who they call back. These results imply that white workers are more likely to be marginal and negatively affected by the policy change. The model implication is consistent with our earlier results using only the policy variation. We then use the policy variation to estimate the model and illustrate how changes in the minimum wage shrink the racial callback gap. Overall, we illustrate when and how statistical discrimination impacts racial differences (Altonji and Pierret, 2001; List, 2004; Autor and Scarborough, 2008; Charles and Guryan, 2008; Ritter and Taylor, 2011; Gneezy et al., 2012; Doleac and Stein, 2013; Fryer et al., 2013; Guryan and Charles, 2013; Heyes and List, 2016). We then go beyond identifying the source of discrimination by explicitly linking the mechanism to the policy response and introducing a framework policy makers can use to predict the effects of the minimum wage before the policy is enacted.

Finally, to make these contributions, we designed a correspondence study that addresses

[7]By studying the distributional implications of the minimum wage, our work also relates to the literature on optimal minimum wages (e.g. Lee and Saez, 2012; Simon and Wilson, 2021).

[8]Similarly, David et al. (2016) show that minimum wages reduce earnings inequality.

[9]The average valuation estimated in the model reflects the sum of taste-based discrimination and difference in beliefs about the average quality of Black and white workers.

several recent methodological critiques of correspondence studies. First, we designed our resumes to avoid the confound of unobserved variance in productivity (Heckman and Siegelman, 1993; Heckman, 1998). Neumark and Rich (2019) notes that nearly all of the correspondence studies conducted over the past twenty years fail to account for differential variances in the perceived productivity of the applicants. After accounting for this possibility in the papers where it is possible, the majority of discrimination estimates become statistically insignificant or change sign.[10] Second, our two-stage randomization procedure allows us to rule out potential spillovers between applications, which has been raised by Phillips (2019), Kessler et al. (2022) and Abel (2017). Finally, by beginning our sample period before states voted on referenda that would decide the minimum wage hikes, we show the importance of accounting for anticipation effects. In our setting, the racial callback gap shrinks immediately after the announcement and stays at the lower level. Had we ignored anticipation effects, we would have erroneously concluded that the minimum wage does not affect callback gaps.

The remainder of this article proceeds as follows. In Section 2, we discuss the natural field experiment in more detail, including the policy variation. Section 3 we introduce our parameter of interest and discuss how our research design identifies that parameter. Section 4 presents the main results and establishes their robustness to alternative specifications. Section 6 describes our strategies for rulling out alternative specifications and presents our findings. Section 7 concludes.

2. Setting and Experimental Design

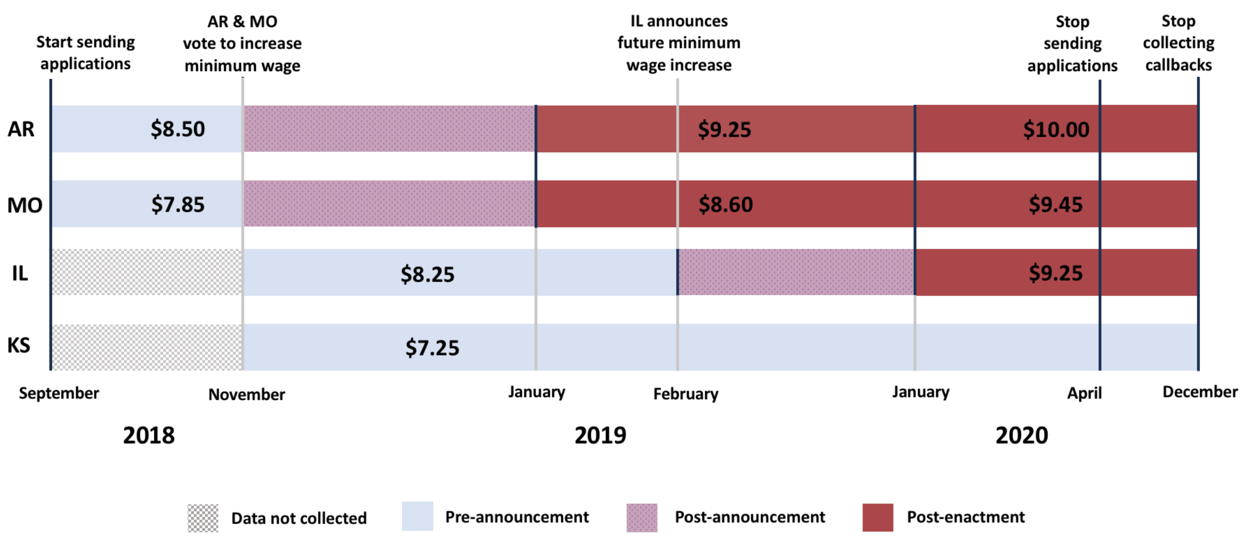
2.1 Setting

We sent fictitious online job applications to minimum wage jobs posted on Indeed.com, the largest online job search website in the United States. We began applying to low-wage job

[10] Neumark (2012), using data from Bertrand and Mullainathan (2004), provides evidence that firms have a higher average valuation of white workers and perceive white workers as less variable in productivity but is unable to reject that there are no racial differences.

postings in Arkansas and Missouri starting on September 18, 2018, and then to postings in Kansas and Illinois from November 2018. We continued sending applications through April 30, 2020. During our sample period, Arkansas and Missouri voted to increase their 2019 minimum wages in November 2018, and Illinois' legislature passed a resolution in February 2018 to increase the minimum wage beginning January 2020.[11] Figure 1 outlines the timeline for each state. For tractability, we categorize periods as (1) before the announcement of a minimum wage increase, (2) after the announcement but before the increase, and (3) after the new minimum wage is enacted.

Figure 1: Experimental Timeline



Note: Figure presents the experiment timeline and minimum wage variation. Blue bars denote the minimum wage is the same as at the beginning of the experiment, with no announced increase. Purple bars denote the minimum wage increase has been announced. Red bars indicate an increased minimum wage.

In Table 1 below, we provide evidence that these policy changes affected the wages of individuals in the labor market using information from the job ads to which we applied. First, we note that not every job post on Indeed.com provides information about the expected wages to applicants. The top part of Table 1 shows that firms are slightly less likely to post

[11]We limited our applications to Little Rock and Fayetteville in Arkansas, Kansas City in Kansas, East St. Louis in Illinois, and Kansas City, Springfield, and St. Louis in Missouri. We did not anticipate the increase in Illinois, so we chose cities in Kansas and Illinois to focus on the border with Arkansas and Missouri.

any wage or an hourly wage after the minimum wages are increased. Firms may choose to post a wage when it is higher than the minimum to attract higher quality applicants. The change in posting could reflect that, after the policy change, more firms bunch at the new minimum wage, and so no longer benefit from explicitly posting their wages.

Table 1: Posted Wages in States Increasing the Minimum Wage

	Pre	Announced	Enacted
All vacancies			
Posts Wage (%)	39	36	36
Posts Hourly Wage (%)	37	32	28
All vacancies that posted hourly wage			
Estimated Hourly Wage	9.25	10.37	11.84
Estimated Hourly Wage $\leq$ State 2018 MW (%)	10	06	3
Estimated Hourly Wage $\leq$ State 2018 MW + 2 (%)	79	65	43
Estimated Hourly Wage $\leq$ State 2020 MW (%)	62	52	35
Estimated Hourly Wage $\leq$ State 2020 MW + 2 (%)	100	84	66

Note: The top panel of the table shows the percent of job ads we apply to that post any way and an hourly wage as a function of the minimum wage policy for Arkansas, Missouri, and Illinois. Ads without an hourly wage may post a daily, weekly or monthly wage. The bottom panel shows the average hourly wage as well as the percent of ads that are close to the 2018 or 2020 minimum wages.

The bottom part of Table 1 shows estimates of hourly wages based on the information provided by the employers. Ads on Indeed vary in how they post information about the expected wage. Some jobs post hourly wages, while others post a daily, weekly, monthly, or annual salary. We infer the hourly wage using the stated salary and information about the the number of hours required by the employer. Table 1 shows that the average hourly wages posted increase and the bottom of the wage distribution shifts to the right. For example, at the start of our experiment in 2018, 62 percent of jobs with a posted hourly wage paid less than the 2020 minimum wage. However, after the minimum wage increases were enacted, the amount of wages posted in this range fell by 27 percentage points. The number of wages at or below the 2020 minimum wage is relatively high (35 percent) because the "Enacted" column includes Arkansas and Missouri in 2019. This provides further evidence that the minimum wages were binding.[12]

2.2 Design

We implemented a $2 \times 2 \times 2$ design, where we randomized the name (to implicitly vary race)[13], unemployment duration (one or twelve months)[14], and human capital (high school graduate or GED) associated with each fictitious application sent to an establishment. All three characteristics were reflected directly in the job application.

All fictitious applicants were 19 to 20-year-old males with either one or two years of prior job experience.[15] Research assistants blind to the experiment constructed the applications directly on Indeed.com, staying consistent with the website format. The age range was chosen to make resumes as comparable as possible and so that our results relate to prior work on minimum wage effects, the majority of which focus on younger applicants. Each application contained a (real) phone number and email, (fictitious) home address, high school completion status, and a short description about previous employment. Prior fictitious work experience was constructed by mimicking sample resumes of real minimum wage job applicants on Indeed.com in the cities of our experimental sample. For each fictitious applicant, we randomly assigned one of the following job titles for prior employment: team member,

[12]Estimated hourly wages can be below the minimum wage for two reasons. First, some jobs, such as tip jobs, are not subject to the minimum wage, but may still increase because employers who are subject to the minimum wage compete with these firms for workers. Second, there may be measurement error induced from our estimation procedure.

[13]For more information about the names used in the experiment, see Appendix A. In Appendix B, we discuss the possibility that the names also signal SES and the implications for our results.

[14]We assign one or twelve months for two months because employed applicants receive fewer callbacks than those who have been unemployed for one month (Kroft et al., 2013) and after twelve months, additional time unemployed does significantly reduce callbacks (Kroft et al., 2013). We also choose contemporary unemployment instead of historical, given that the former is associated with significant effects on callback rates but not the latter (Eriksson and Rooth, 2014).

[15]We chose to only include male names for three reasons. First, many studies find mixed or no average impacts of sex on employer callbacks making detecting any change in baseline discrimination against women would have been infeasible (Nunley et al., 2015; Zschirnt and Ruedin, 2016; Bertrand and Duflo, 2017; Baert, 2018; Kline et al., 2021). Second, further dividing the sample into additional groups would have resulted in substantial reductions in power. Third, Gaddis (2017) finds that respondents' predicted race matches the researchers' intended signal for Black female names less than black male names. But, there is no difference in the ability to perceive male and female white names congruently. Using female names would reduce the quality of the signal differentially by race. However, using only male names on the resumes limits the external validity of our experiment to some extent.

janitor, food preparation, cashier, and store associate.[16]

All applications were sent to job postings listed on Indeed.com that fit our salary and location criteria. We applied to 17,737 vacancies across 14,488 establishments (8,376 firms) low-wage jobs. Since prior work has found that employers disfavor applicants with long commutes (Phillips, 2020), we restricted search criteria to job postings by establishments located within a 10-mile radius of the cities in our sample.[17]

We generally sent two applications to an establishment for each job posting period. If a vacancy remained up for multiple periods, we applied to it in the subsequent period.[18] To the best of our ability, we did not send establishments the same application type (race, unemployment duration, and high school diploma status) more than once, to minimize the risk that they suspect that the applications were fictitious.

We assigned applications to vacancies through three main steps. First, for each job posting, we randomly assigned the first applicant's race, unemployment duration, and human capital (with 50-50 probability for each trait).[19] Then, we randomly assigned the stratification associated with the job posting; half on race, and half on unemployment duration.[20] Finally, all other resume characteristics were randomly assigned.[21]

As we pre-specified, we classified a callback as any email or voicemail from the establishment that specifically requested an interview. This is a standard way of measuring the outcome and followed by previous papers such as Bertrand and Mullainathan (2004) and Kroft et al. (2013). For every application we sent, we observe whether it received a callback

[16]For more information about the construction of the resumes, see Appendix A.1.

[17]More information about the characteristics of our sample is presented in Appendix C.

[18]For example, if a job is posted in Arkansas in October 2018 and taken down in March 2019, we would apply in October during the pre-period, in November or December during the announced period, and finally in January or February in the enacted period.

[19]Appendix C provides evidence that the randomization was successful by showing that observable firm characteristics are balanced across treatments.

[20]This stratification design addresses a critique raised by Phillips (2019), in which he shows that a spillover bias may be possible when multiple applications are sent to the same firm. We use the second level of randomization to address challenges in identifying spillover effects highlighted by Angrist (2014), Baird et al. (2014), and Vazquez-Bare (2022). For other papers that use two stages of exogenous variation to identify spillover effects, see Duflo and Saez (2003), Crépon et al. (2015) and Holz et al. (2019).

[21]For more information on the job sampling and application process, see Appendices A.2 and A.3.

or not, as each application corresponded to a real phone number and email address. If the establishment contacted the applicant to request additional information, we did not classify that as a callback.

Because there is a lag between our treatment assignment and the observation of the outcome, we had to classify callbacks with respect to the timing of the minimum wage hike. We consider callbacks as an outcome of a particular application, given the timing of the application. For example, suppose we send an application to an establishment before the minimum wage hike is announced, and the establishment calls the applicant back for an interview after the minimum wage hike is enacted. We classify that callback as being part of the pre-announcement period. If anything, this classification strategy biases us towards not finding an effect of the minimum wage on callbacks because we would be misclassifying observations before the minimum wage hike as observations after the minimum wage hike.[22]

3. Minimum Wages and Discrimination

We consider the firm's decision of whether to request an interview with an applicant. The firm is faced with applicants i differentiated by their race $B_i \in \{0, 1\}$ (either white or Black) and quality Q_i which the firm may believe depends on the applicant's race. In our context, quality may represent the firm's perception of the applicant's marginal revenue product of labor. The firm's perception of quality may vary with other characteristics on the resume, such as the applicant's educational attainment or unemployment duration.

The firm decides whether to call back an applicant for an interview $Y_i \in \{0, 1\}$ given the applicant's race and perceived quality. We aim to understand the causal effect of applicant race on callbacks, or the racial callback gap (RCG), and how that parameter varies with the minimum wage. Formally, we follow Arnold et al. (2020) and Arnold et al. (2022) and measure the racial callback gap using the disparity among equally qualified white and Black applicants:

[22] We examine the robustness of these results to alternative classification schemes in Appendix D.

$$RCG = \mathbb{E}\bigg[\mathbb{P}[Y_i = 1|B_i = 1, Q_i] - \mathbb{P}[Y_i = 1|B_i = 0, Q_i]\bigg] \quad (1)$$

The RCG compares the callback probability for white and Black applicants, averaging over the marginal quality distribution. Discrimination against Black applicants occurs when Black applicants have lower callback rates than equally qualified white applicants.

The primary threats to the identification of the RCG are that objective quality may be correlated with the applicant's race and that some signals of an applicant's quality may be observed by the hiring manager but not by the researcher. Our experiment, described in Section 2.2 is designed to address both of these challenges. Randomization of race through manipulation of the applicant's name implies that resume characteristics are independent of the applicant's race. Moreover, since our only correspondence with the firm is the application, we observe the relevant information held by the firm at the time of their decision. Thus, under randomization, we can identify the racial callback gap using the difference in mean callbacks between Black and white applications and discrimination occurs when $RCG < 0$.

To understand how the RCG changes with the minimum wage, we consider a firm hiring model where firms solicit and receive applications for a job that pays the wage, m. For each application it receives, the firm observes each applicant i's race, B_i, and other characteristics, X_i (e.g., educational attainment). Using these observables, the firm forms an inference, Q_i, on i's (unobserved) productivity level. The firm then makes the binary decision of whether to offer the applicant i an interview request. The firm calls back applicant i if the inferred quality is above a threshold, that is if $Q_i + B_i d > v(m)$, where the threshold $v(m)$ is an increasing function of the minimum wage policy and perceptions of quality depend on the extent of racial discrimination or a discriminatory penalty, $d < 0$.

We assume that the joint distribution of (Q_i, B_i, X_i) has a strictly decreasing pdf around the cutoff. The parameter d depends on taste-based discrimination and perceptions of racial differences in average applicant quality, a form of statistical discrimination. More generally, firms may believe that the productivity distributions for Black and white applicants differ.

The change in the RCG from a minimum wage increase is therefore given by:

$$\frac{\partial RCG}{\partial m} = v'(m) \times \left[f(v(m)|B_i = 0) - f(v(m) - d|B_i = 1)\right] \tag{2}$$

where f is the pdf of Q_i conditional on X_i. From McCall (1970), we expect that firms reduce the number of (relatively low-quality) applicants they call back when they face a higher labor cost from the minimum wage hike. The threshold moves to the right, and so $v'(m) > 0$.

The second term on the right hand side compares the share of white marginal applicants to the share of Black marginal applicants. Without knowing more about the productivity distributions and the magnitude of the discriminatory penalty, we cannot sign the second term. Therefore, the minimum wage could increase or decrease the RCG depending on the extent and type of discrimination.

The primary threat to identifying $\partial RCG/\partial m$ is changes in other aspects of the firm's decision that are contemporaneous with the firm's decision. Hence, the identification of this parameter relies on an assumption that the RCG would have remained constant, but for the minimum wage hikes. We overcome these challenges by using comparable states as a control group and unannounced changes in the minimum wage, as described in Section 2.1.

As a special case of the model above, suppose that Q_i is normally distributed such that $Q_i|(B_i = 0) \sim N(\mu_0, \sigma_0^2)$ and $Q_i|(B_i = 1) \sim N(\mu_1, \sigma_1^2)$. In this special case, we allow the means and variances of productivity to vary by race. While randomization ensures that the characteristics of the resumes are independent of race, Q_i is the inference firms make about the applicant's quality given the resume characteristics. So, from the firm's perspective, Black and white applicants with identical resumes may have different perceived productivity distributions. Similarly, nothing in our research design rules out differences in how the firm perceives the variance of the applicant's quality. Under these assumptions, the change in the RCG from an increase in the minimum wage is

$$\frac{\partial RCG}{\partial m} = v'(m) \times \left[\phi((v(m) - \mu_0)/\sigma_0)\sigma_0^{-1} - \phi((v(m) + \gamma - \mu_1)/\sigma_1)\sigma_1^{-1}\right]$$

where ϕ is the standard normal pdf, and $\gamma > 0$ is taste-based discrimination.[23] Both statistical discrimination in perceived mean productivity, $\mu_1 - \mu_0$, and taste-based discrimination, γ, contribute to the discriminatory penalty, d. With a discriminatory penalty, minimum wages can attenuate the RCG if σ_1 is sufficiently larger than σ_0, that is if firms believe that the variance of the quality distribution for Black applicants is larger than for white applicants.[24] Appendix E discusses the implications of alternative assumptions on Q_i. For example, if Q_i is uniformly distributed, the $\partial RCG/\partial m$ only depends on the perceived variance of applicant productivity, and not at all on the discriminatory penalty.

The model considers the decisions of an individual firm when confronted with different labor costs based on the minimum wage. However, policy changes may also affect the composition of firms that hire. If the most discriminatory firms exit the market after a minimum wage increase, then the racial callback gap should also shrink. Additionally, changes in labor costs may also effect how firms screen applicants. A higher minimum wage could benefit high-quality Black applicants and decrease the gap if the increased minimum wage leads firms to more intensely scrutinize resumes, rather than relying on race as a proxy for quality. However, we do not find evidence that either of these effects drive the change in the racial callback gap in our correspondence study.

Outside the scope of both our model and correspondence study, the minimum wage may also change the composition of other applicants if higher wages induce some to enter the labor force. If more high-quality applicants join after the minimum wage increase, then our applicants may be considered relatively low quality. This may be reflected in the extent to which the threshold moves.[25] However, if this had a large effect on our estimates, we would expect the change in the callback gap to change over time with higher minimum wage policies

[23]A more positive γ implies that Black applicants face a higher productivity threshold to receive a callback.

[24]Under normality, this result requires that the baseline callback rate is less than 50 percent for both groups as is the case in our and many other audit and correspondence studies. This is analogous to our earlier assumption that f is decreasing around the cutoff.

[25]For example, if firms only call back a fixed number of top applicants, more high-quality applications from other individuals would reduce the number of our fictitious applicants who are above the firm's threshold implicitly defined by the number of top candidates they callback.

within a state. We instead find that the gap immediately changes after the announcement and persists over our entire sample.

4. Results

4.1 Which Workers Receive Callbacks?

We begin by estimating the causal effect of perceived race, human capital, and duration unemployed on the likelihood of receiving a callback over our full sample from 2018 to 2020 from:

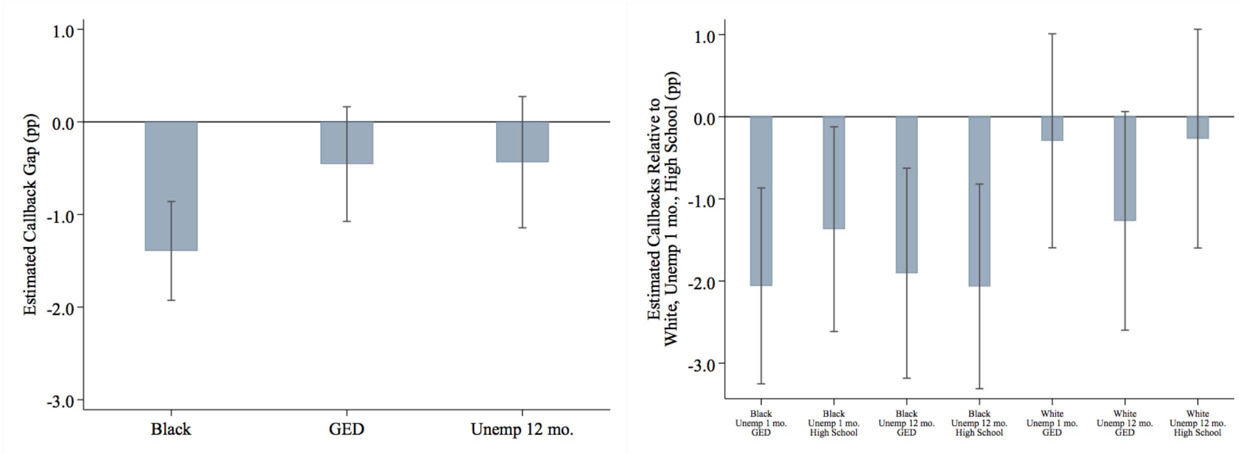
$$Y_{ict} = \alpha_1 \text{Black}_i + \alpha_2 \text{GED}_i + \alpha_3 \text{Unemp12}_i + X_i' \gamma + \eta_c + \epsilon_{ict} \tag{3}$$

where Y_{ict} is an indicator representing whether application i received a callback from job posting by firm j in city c at time t. $Black$, GED, and $Unemp12$ are indicators for whether an applicant was randomized to have a distinctively Black name, hold a GED rather than a high school degree, and be 12 months unemployed rather than 1, respectively. X is a vector of other randomized applicant characteristics, for example, age. In all specifications, we also include city fixed effects, η_c. The α coefficients estimate the causal effect of the characteristic on callbacks.

Overall, the callback rate for white applicants is 11%. Similar to previous correspondence studies, we find that applicants with distinctively Black names are about 1.4 percentage points (12%) less likely to receive a callback than applicants with distinctively white names (Figure 2a). The callback gaps based on prior education and duration unemployed are about one third of the size of the RCG and not statistically significant.

We additionally present average callbacks for each of our eight types of applicants in Figure 2b, relative to white applicants with a high school diploma and who have been unemployed for the previous 1 month. We find very little heterogeneity by education and duration unemployed in the average callback rates for Black applicants. This may be consistent with

Figure 2: Callback Gaps in the Full Sample



(a) Callback Gaps by Randomized Characteristic

(b) Callbacks by Applicant Type

Note: Panel (a) presents estimates and 95% confidence intervals for the callback gaps based on the randomized characteristics: (1) Black-white, (2) GED-high school graduate, and (3) 12 months- 1 month unemployed. Panel (b) similarly presents estimates for the likelihood an applicant receives a callback relative to White applicants with a high school diploma who have been unemployed for 1 month. Both specifications include city fixed effects and control for applicant age and include the full sample of 34,986 observations. Standard errors are clustered by establishment.

"attention discrimination," where hiring managers pay less attention to applications from minority applicants and so do not see their other relevant qualifications (Bartoš et al., 2016). White applicants with a high school degree and who are one month unemployed are 1.3 percentage points more likely to receive a callback than white applicants with a GED and who are 12 months unemployed ($p = 0.062$).

These average callback gaps mask the effects of the minimum wage increases. When facing higher labor costs, establishments may change which types of applicants they call back. In the remainder of the paper, we decompose these average gaps to measure how minimum wage policies affect labor market disparities and consider potential mechanisms.

4.2 The Effect of Minimum Wage Increases on the Racial Callback Gap

Next, we examine how the minimum wage affected racial callback gaps by estimating the following equation:

$$\begin{aligned} Y_{ict} = &\beta_1 \text{Announced}_{ct} + \delta_1 \text{Black}_i \times \text{Announced}_{ct} + \beta_2 \text{Enacted}_{ct} \\ &+ \delta_2 \text{Black}_i \times \text{Enacted}_{ct} + \alpha \text{Black}_i + X_i' \gamma + \eta_c + \epsilon_{ict} \end{aligned} \tag{4}$$

where Y_{ict} is an indicator representing whether application i received a callback from job posting in city c at time t, $Black_i$ is an indicator equal to 1 if the applicant is Black, Announced is an indicator variable that is 1 if the minimum wage increase has been announced but not yet enacted at the time of application, Enacted is an indicator that is 1 after the minimum wage has increased at the time of application[26], and X is a vector of randomized applicant characteristics, including the applicant's length of unemployment, human capital, and age. Announce and Enacted are mutually exclusive. In all specifications, we also include city fixed effects, η_c.

Our research design identifies the causal effect of minimum wage increases on racial callback gaps under an assumption of parallel trends in the racial callback gap using a difference-in-differences design.[27] The first difference comes from randomization, which allows us to measure the callback gap between Black and white applicants, holding all other characteristics fixed that may also affect labor market disparities, like school quality or attainment (Card and Krueger, 1992; Smith and Welch, 1989). The second differences comes from measuring callbacks before and after the announcement of the policy change.[28]

Our results in Figure 3 show that before the announcement of the minimum wage increase,

[26] Appendix D shows that assigning treatment based on the earliest date of callback for vacancies where at least one applicant receives a callback does not affect our results.

[27] Since an observation is at the applicant $\times$ job-posting $\times$ establishment level, this strategy does not rely on two way-fixed effects.

[28] In Appendix G we also consider specifications with firm (e.g. McDonald's) fixed effects to compare the behavior of different establishments that belong to the same parent company, but where one experiences a minimum wage increase and the other does not because they are located in different states.

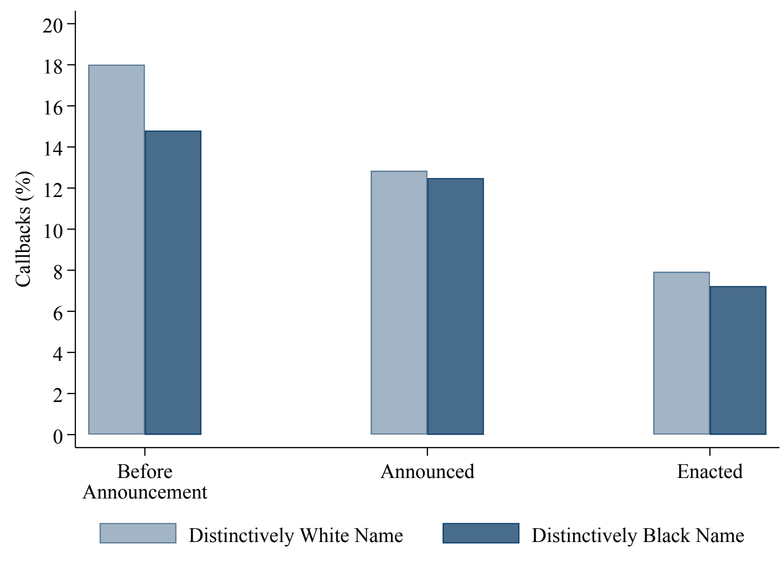
applicants with distinctively Black names are about 3 percentage points less likely to receive a callback. When the minimum wage increase is announced, all applicants are less likely to receive a callback, but the callback rate decreases by less for Black applicants; after the policy is enacted, the racial callback gap shrinks to about 0.7 percentage points but it is still different from 0 ($p = 0.029$). We therefore reject Friedman (1966)'s claim that minimum wage increases exacerbate racial differences in hiring. We also show in Appendix F that minimum wage increases do not affect the callback gaps by education or duration unemployed.

Our results presented in Figure 3 suggest that employers respond immediately to the news that the minimum wage will increase soon. This may be, in some sense, mechanical because there is a relatively short time between the announcement and the enactment so that all applications considered during the announced period would not begin work until after the minimum wage increase is in effect. To further understand of dynamics, we estimate a specification where we divide the enacted period into 60 day intervals and present the estimates in Figure 4. The estimates remain constant for more than a year after the enactment when Arkansas and Missouri increased their minimum wages for a second time. One possible explanation is that hiring managers may have immediately responded to the change in information after the announcement because they anticipate their hires would be employed during this time period. Based on these results, we pool the announce and enacted periods into one "after announced" period for the remainder of the analysis.

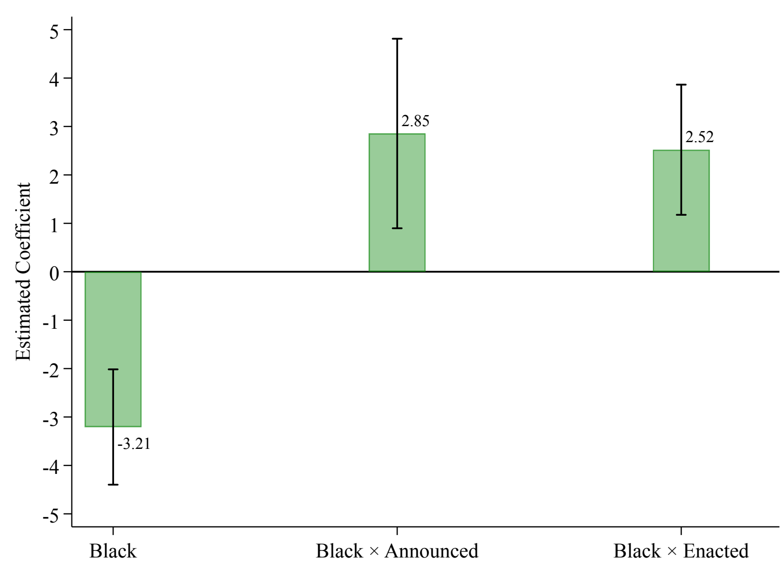
4.3 Robustness

Our research design additionally allows us to consider several alternative specifications, that vary the sample, for example, by focusing border cities (Kansas City and St. Louis) or removing applications in 2020 during the COVID-19 pandemic, or that include additional controls. Figure 5 estimates different versions of Equation 4 and plots both the baseline racial gap in gray and the change in the gap after the minimum wage increase in green, with

Figure 3: The Minimum Wage and the Racial Callback Gap



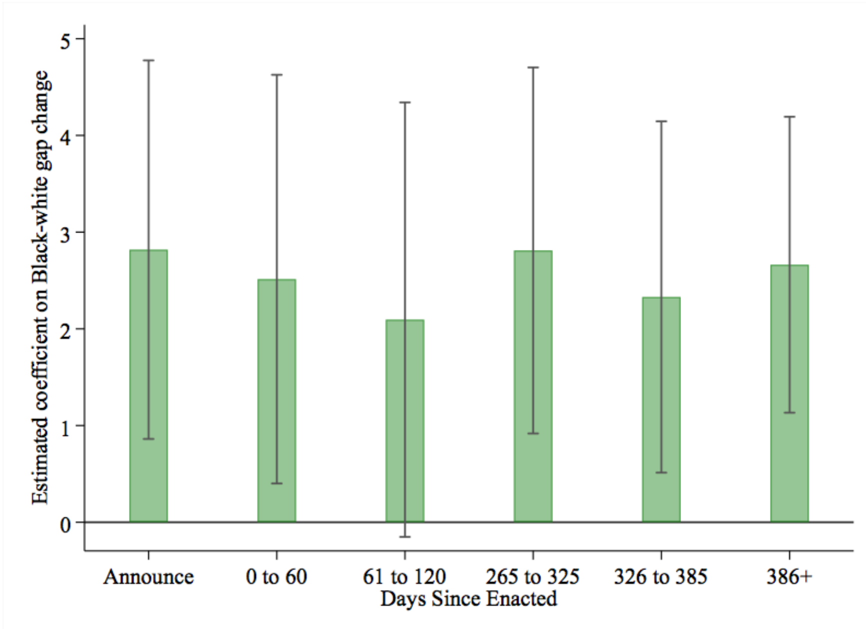
(a) Average Callback Rates by Race and Time Period



(b) Racial Differences

Note: Panel (a) presents estimates of the average callback rates by race and time period based on the estimates from Equation 4. Panel (b) displays coefficient estimates from Equation 4 and 95% confidence intervals. The coefficient estimate on Black represents the baseline difference in callback rates between applications with distinctively Black names and distinctively white names. The coefficient estimate on Black × Announced can be interpreted as the change in the racial callback gap after the minimum wage hike is announced. Similarly, the coefficient estimate on Black × Enacted can be interpreted as the change in the racial callback gap after the minimum wage hike is enacted. Figures include the full sample of 34,986 observations.

Figure 4: The Minimum Wage and the Racial Callback Gap Dynamics



Note: The figure presents estimates and confidence intervals for the change in the racial callback gap by days since the minimum wage is enacted. We group days into 60 day bins to reflect the length of the announce period in Arkansas and Missouri. No applicants were sent between days 120 and 265. The specification includes city fixed effects and controls for GED, duration unemployed, and age. Standard errors are clustered by establishment. Figure include the full sample of 34,986 observations.

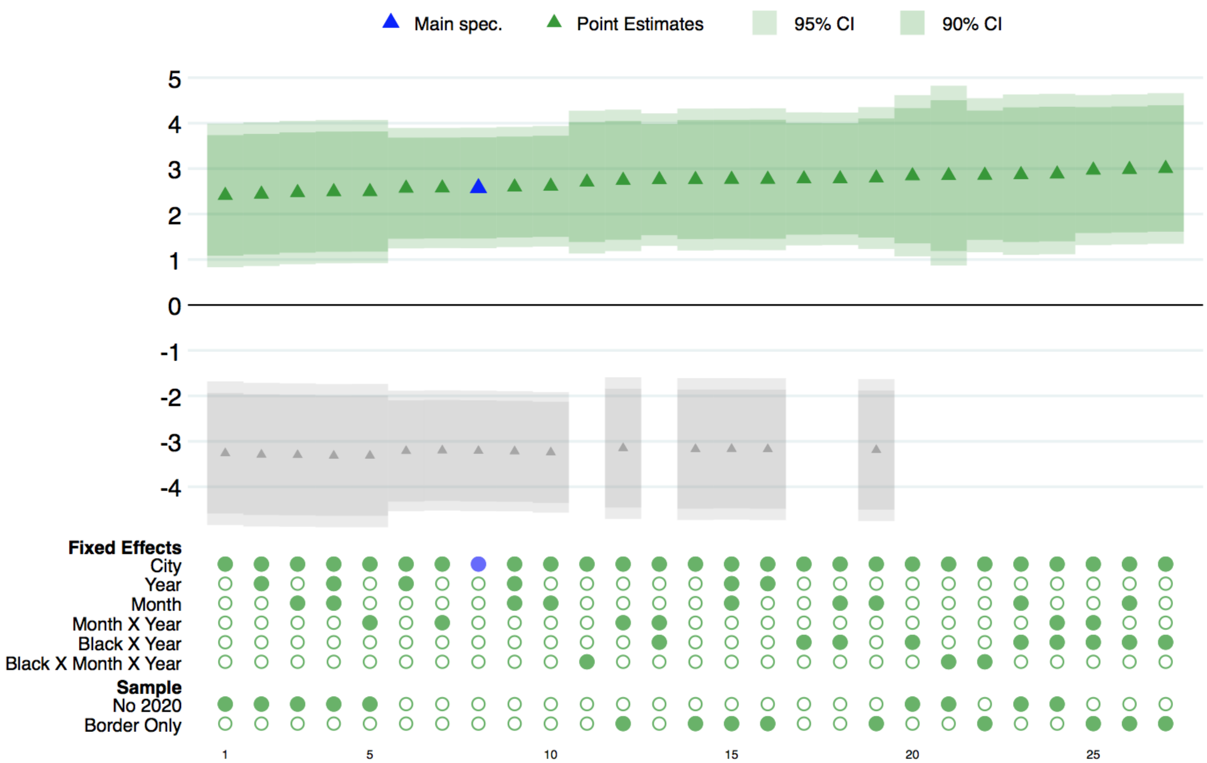
our preferred specification from Figure 3 highlighted in blue.[29] The estimated effects across all specifications are nearly identical, and all point to the same qualitative conclusion that the minimum wage increases reduced the racial callback gap.[30]

In all specifications, we cluster the standard errors by establishment. Alternative levels of clustering have a small effect on the precision of our estimate for the coefficient on Black $\times$ After Announce. We consider three alternatives. First, Agan and Starr (2018) suggest clustering by firm because establishments within a firm may be susceptible to serially correlated shocks. When doing this, the standard errors and p-values minimally change. While randomization is at the job vacancy level, the policy variation is by state, and so Neumark et al. (2019) suggest clustering at that level. Since the sample only has 4 states, we estimate

[29]For specifications where we include either Black $\times$ Year or Black $\times$ Month $\times$ Year fixed effects, the baseline gap is not identified.

[30]See Appendix G for this analysis.

Figure 5: Alternative Specifications



Note: This figure presents a specification curve for the baseline racial callback gap in gray and the change in the racial callback gap from a minimum wage increase. The preferred specification is highlighted in blue. The filled in circles indicate which controls are included. The baseline gap is not identified in specifications that include Black × time period fixed effects. In all specifications, standard errors are clustered by establishment.

p-values using the wild bootstrap (Cameron et al., 2008; Canay et al., 2021). In this case, p-value for the coefficient on Black $\times$ After Announce is still less than 0.001, although the confidence interval is about 1.5 times as large as when we cluster by establishment. Finally, for completeness, when we cluster by city, the p-value is 0.016.

5. Illustrating the Role of Taste-Based and Statistical Discrimination

The results in Section 4.2 show that minimum wage increases reduce the racial callback gap. In line with our model, this may imply that firms believe that the variance of applicant quality is larger for Black applicants. However, the total impact of the minimum wage change depends on both taste-based and statistical discrimination. We follow Neumark (2012) and Neumark et al. (2019) to partially separate these two channels and illustrate the role of taste-based and statistical discrimination for our findings.

As in Section 3, suppose $Q_i|(B_i = 0) \sim N(\mu_0, \sigma_0^2)$ and $Q_i|(B_i = 1) \sim N(\mu_1, \sigma_1^2)$, and that firms require Black applicants to be higher quality to receive a callback due to taste-based discrimination, γ. Then we can partially recover these parameters by estimating a heteroskedastic probit:

$$Y_{ict} = \mathbb{1}\big[\underbrace{\alpha}_{(\mu_1-\mu_0)-\gamma}\text{Black}_i + X_i'\eta + (\text{Black}_i\sigma_1 + (1-\text{Black}_i)\sigma_0)\epsilon_{ict} > 0\big] \tag{5}$$

where $\epsilon_{ict} \sim N(0,1)$ and X_i is a vector of observable randomized characteristics and city fixed effects. α captures both taste-based discrimination and statistical discrimination from perceived differences in the mean of the quality distribution. If observable randomized characteristics and city fixed effects affect firm's beliefs about the applicant's productivity similarly by race, η is the same for Black and white applicants and we can identify σ_1^2/σ_0^2. While the coefficient is assumed to be the same, that does not imply that the effect on callbacks are identical. Both η and σ determine the likelihood of receiving a callback.[31]

[31] For these specifications, we do not include firm fixed effects because of the incidental parameter problem for non-linear models. When estimating a heteroskedastic probit, the problem is even more pronounced since

We first pool the data over the whole sample and estimate $(\mu_1 - \mu_0) - \gamma$ and σ_1/σ_2 (Column 1 of Table 2). To estimate the relative variance, σ_1^2/σ_0^2, shown in Table 2, we use 10 characteristics that affect the likelihood of receiving a callback. These are the randomized resume characteristics for education, duration unemployed, and age, as well as city fixed effects and an indicator for after the minimum wage increase is announced. Using multiple characteristics allows us to test the assumption that η is the same for Black and white applicants. If the assumption holds, the marginal effects of a characteristic on callbacks only differs by race because of the difference in the variance of the unobservables. As Neumark (2012) notes, the ratios of the estimated probit coefficients for Black and white applicants, for each characteristics, should therefore be the same. We estimate a probit regression including the 10 characteristics and their interactions with an indicator for whether the resume has a distinctively Black name, and fail to reject that the 10 ratios are same (p-value = 0.96).[32]

We find that $(\mu_1 - \mu_0) - \gamma < 0$, meaning that firms discriminate against Black applicants through some combination of taste-based and beliefs about mean productivity. We also find that $\ln\left(\sigma_1^2/\sigma_0^2\right) > 0$, implying that firms believe the variance of the quality distribution is larger for Black applicants. These estimates are similar to Neumark (2012) who finds suggestive evidence that the perceived productivity variance of Black applicants is larger using data from Bertrand and Mullainathan (2004). Although, he is does not have power to reject that the two variances are the same.[33]

We then measure the extent to which the callback threshold changes after the minimum wage increases using the following equation,

$$Y_{ict} = \mathbb{1}\big[\underbrace{\alpha}_{(\mu_1-\mu_0)-\gamma}\text{Black}_i + \underbrace{\beta_1}_{-v'(m)}\text{After Announced}_{ct} + X_i'\eta + (\text{Black}_i\sigma_1 + (1-\text{Black}_i)\sigma_0)\epsilon_{ict} > 0\big]. \quad (6)$$

it would be similar to estimating a firm-by-race fixed effect.

[32] When excluding the indicator for after the minimum wage increase is announced, as in Column (1) of Table 2, the p-value is 0.88.

[33] The results and framework are also consistent with interviews with hiring managers who are more likely to perceive racial differences in applicants than in their own employees (Pager and Karafin, 2009).

Table 2: Unobserved Productivity Differences and the Minimum Wage

	Callback	
	(1)	(2)
$(\mu_1 - \mu_0) - \gamma$	-0.40**	-0.38**
	(0.16)	(0.12)
$-v'(m)$		-0.45***
		(0.04)
$\ln\left(\sigma_1^2/\sigma_0^2\right)$	0.22*	0.21**
	(0.10)	(0.08)
N	34,990	34,990

Standard errors are clustered by establishment. Both specifications include city fixed effects and applicant characteristics. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.10$

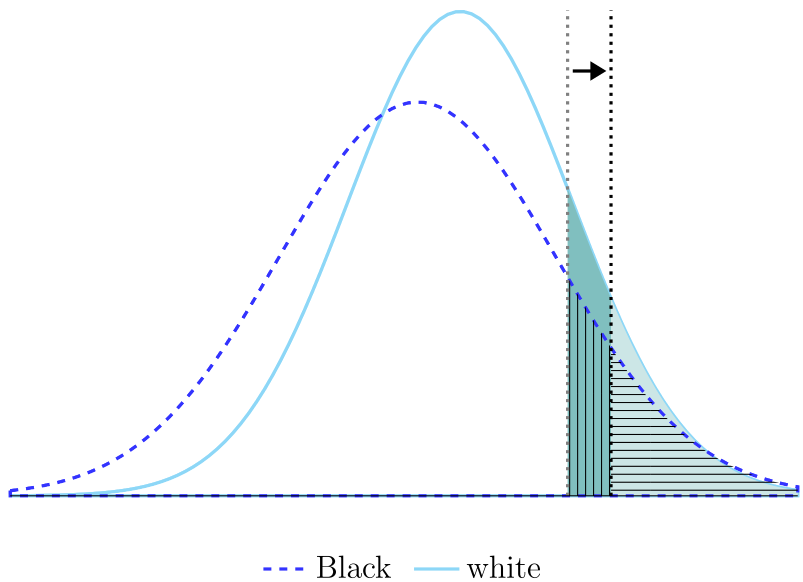
In Column (2) of Table 2, we provide estimates from Equation 6. Consistent with the intuition from McCall (1970), we find that the threshold moves to the right ($v'(m) > 0$), and so fewer applicants receive an interview request after the policy change. Our estimates of the taste-based and statistical discrimination parameters remain nearly the same.

We now illustrate in Figure 6, using our estimates from Column (2) of Table 2, that differences in the perceived variance of applicant quality for Black and white applicants, or statistical discrimination, can help explain why the racial callback gap shrinks with minimum wage increases. The quality distribution of white applicants is given by the solid curve, and the distribution for Black applicants is the dashed curve. The dotted vertical lines represent the callback minimum quality threshold.[34]

Based on the discriminatory penalty and perceived variance of the productivity distributions between Black and white applicants, a larger number of white applicants receive callbacks before the minimum wage increase; there is relatively more mass under the solid curve to the right of the vertical dotted line than the dashed curve. After the new minimum wage is announced, the callback threshold shifts to the right and establishments only call

[34]We cannot separately identify differences in perceived mean quality and differences in the callback threshold by race (γ). For exposition, the figure assumes that Black and white applicants face the same threshold but that μ_1 and μ_0 are different. This assumption does not affect the interpretation of the results since our discussion is based on the relative mass of applicants around the threshold. Alternative assumptions would shift both the distribution and threshold by the same constant.

Figure 6: Minimum Wage Increases and Callbacks by Applicant Productivity



Notes: This figure plots the share of applicants who receive a callback by race based on our estimates from Table 2. The solid normal distribution shows the perceived productivity distribution for white applicants, while the dashed line for Black applicants. The vertical dotted lines represent the callback thresholds before and after the minimum wage increase. An applicant receives a callback if he is to the right of the threshold, corresponding to the shaded region.

back higher quality applicants. Initially marginal applicants, those between the two dotted lines, no longer receive a callback. We find that, in our setting, because white applicants have a smaller perceived variance and are favored in hiring, a larger share of the initially marginal applicants are white, and therefore they experience a larger decrease in callbacks.[35] Minimum wages erode favoritism towards modest-productivity white applicants.

Our framework estimates the relative perceived variances of the productivity distributions for Black and white applicants, but firms may not be accurate. By randomization in the correspondence study, the productivity distributions should be identical. However, they may have inaccurate beliefs (Bohren et al., 2019) or limited previous experience hiring different types of applicants (Lepage, 2021).

These results highlight that minimum wage increases are not guaranteed to lead to reductions in the racial callback gap. Instead, the effects of the minimum wage critically depend on the nature and extent of discrimination in the low-wage labor market. However, the unambiguous sign of $v'(m)$ allows researchers or policy makers to determine how a minimum wage will affect the racial callback gap even *without* variation in the minimum wage. In any low-skill labor market, one can conduct a simplified correspondence study that varies the race of the applicant and the perceived quality of the applicant to identify d and $\ln\left(\sigma_1^2/\sigma_0^2\right)$ using Equation 5. Then, given these parameters, the model predicts the sign of $\partial RCG/\partial m$.

6. Assessing Alternative Explanations for Why the Racial Callback Gap Shrinks

Our experiment allows us to rule out three competing mechanisms driving the reduction in the racial callback gap. First, increases in labor costs may affect the extent to which hiring managers rely on race as a signal of worker quality rather than other observable characteristics. Second, increases in the cost of employing applicants may affect the composition of establishments posting job ads. Finally, we investigate the possibility that non-experimental job applications respond to the increase in the minimum wage, changing the composition of

[35] Based on our estimates, the qualitative finding also requires that the baseline callback probability for both groups is less than 50 percent.

applicants and, thereby, the probability of a callback. We find limited evidence of any of these channels, suggesting that statistical discrimination and taste-based mainly drive our findings.

6.1 Observable Signals of Worker Productivity

Bartoš et al. (2016) shows that because reviewing applications is costly, prior beliefs that minorities are less productive than non-minorities lead managers to spend less time reviewing a Black applicant's resume. Minimum wage hikes increase the costs of hiring a bad applicant. They, therefore, may lead managers to rely more on additional signals of worker quality, such as their human capital or previous relevant experience, reducing "attention discrimination." If increases in the minimum wage induce managers to spend more time reviewing each application, then employers will be more likely to know that Black applicants are high-quality when the minimum wage is high. Thus, attention discrimination in this market implies that the returns to quality increase for Black applicants relative to white applicants following the minimum wage increase.

Using education and previous relevant experience as proxies for quality, we now test whether attention discrimination drives our main results by estimating the extent to which the racial callback gap changes with the minimum wage based on other observable and randomized characteristics from:

$$
\begin{aligned}
Y_{ict} = & \sum_r \alpha_r B_i \mathbb{1}_{ir} + \sum_r \beta_r \mathbb{1}_{ir} \text{After Announced}_{ct} \\
& + \sum_r \delta_r B_i \mathbb{1}_{ir} \text{After Announced}_{ct} + X_i' \gamma + \eta_c + \epsilon_{ict}
\end{aligned} \tag{7}
$$

where $\mathbb{1}_{ir}$ is indicates whether applicant i has randomized characteristic r. We first estimate the effect of the minimum wage increase on the racial callback gap allowing for heterogeneity by each combination of an applicant's human capital and duration unemployment. Figure 7a presents the estimates and confidence intervals for δ_r. While we find the callback gaps shrink the most among applicants with a high school degree and who are 12 months unemployed, we cannot reject that any pair of coefficients are different. The results are consistent with a closing of the racial callback gap for all types since applicants with high school degrees and a twelve month unemployment duration have the largest baseline racial gap. Overall, the results do not suggest that the highest quality Black applicants see the largest relative increases in callbacks, as attention discrimination would suggest.

As a second test of this hypothesis, we estimate the effect of the minimum wage increases on the racial callback gap for applicants with and without previous relevant experience.[36] For each applicant, we randomized the job title associated with their previous job, for example cashier or janitor. We similarly categorize job ads based on the job titles and descriptions for each position. We estimate Equation 7 with r denoting whether or not an applicant had previous relevant experience and present the results in Figure 7b. While the point estimates suggest that the racial callback gap shrinks more for applicants with previous experience, we cannot reject that they are different.

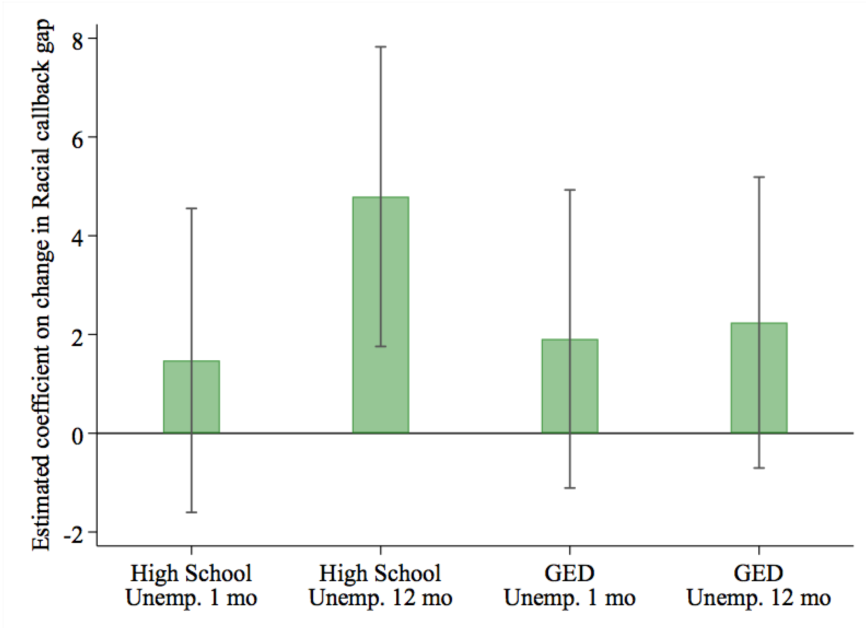
6.2 Changes in the Composition of Job Vacancies

Kline and Walters (2021) shows substantial variability in the extent to which firms discriminate against minority applicants, implying that the composition of establishments hiring in a given period will likely affect the estimates of discrimination we obtain. This composition is potentially a function of the minimum wage, as Luca and Luca (2019) shows that minimum wage increases lead low-quality and low-productivity firms to close, and discriminating firms are likely less efficient (Becker, 1957; Pager, 2016). When facing higher labor costs, establishments may also respond by demanding more skilled applicants, or altering the types of tasks performed on the job (Clemens et al., 2021).

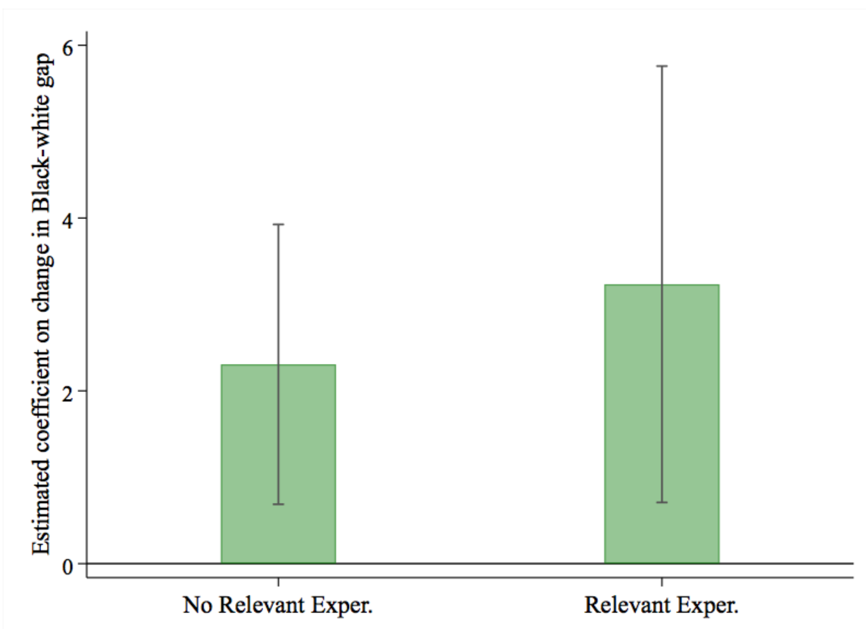
In this section, we use the text from the job ad posting to understand this channel and

[36] These results should be interpreted with caution as the relevant work experience depends on the job ads posted by employers after the minimum wage increases. Since the minimum wage can change the types of jobs applicants recruit for, these designations may be a function of the minimum wage.

Figure 7: Change in Racial Callback Gap by Applicant Quality



(a) Change in Racial Callback Gap by Other Characteristics



(b) Change in Racial Callback Gap by Previous Relevant Experience

Note: Panel (a) presents estimates and confidence intervals for the change in the racial callback gap from the minimum wage increases allowing for heterogeneity by combinations of high school diploma/GED and 1/12 months unemployed. Panel (b) presents estimates and confidence intervals for the change in the racial callback gap from the minimum wage increases allowing for heterogeneity by whether the applicant was randomized to have previous relevant experience. Both specifications control for city FE, and other characteristics. Standard errors are clustered by establishment.

how it relates to our previous changes in the racial callback gap. First, we will show that there are changes in the types of vacancies that are posted after the minimum wage increases. Then, we show that our results are unlikely to be driven by these changes.

6.2.1 Association Between the Minimum Wage and Skills Demanded

We examine the association between the minimum wage and skills demanded by dividing establishments into four mutually exclusive groups: (1) those who only hire before the minimum wage is announced, (2) those who only hire after the minimum wage is announced, (3) the pre-period for those who hire both before and after the announcement, and (4) the post announcement-period for those who hire both before and after the announcement. Whether and when an establishment chooses to hire is endogenous, but splitting our sample in this way helps illustrate both changes in composition and policy responses. We then estimate the following regression:

$$\begin{aligned} C_{p(e,c,t)} = {} & \beta_1 \mathbb{1}[\text{Hiring Both, Before}]_e + \beta_2 \mathbb{1}[\text{Hiring Both, After}]_e \\ & + \beta_3 \mathbb{1}[\text{Only Hiring After}]_e + \eta_c + \epsilon_{p(e,c,t)} \end{aligned} \tag{8}$$

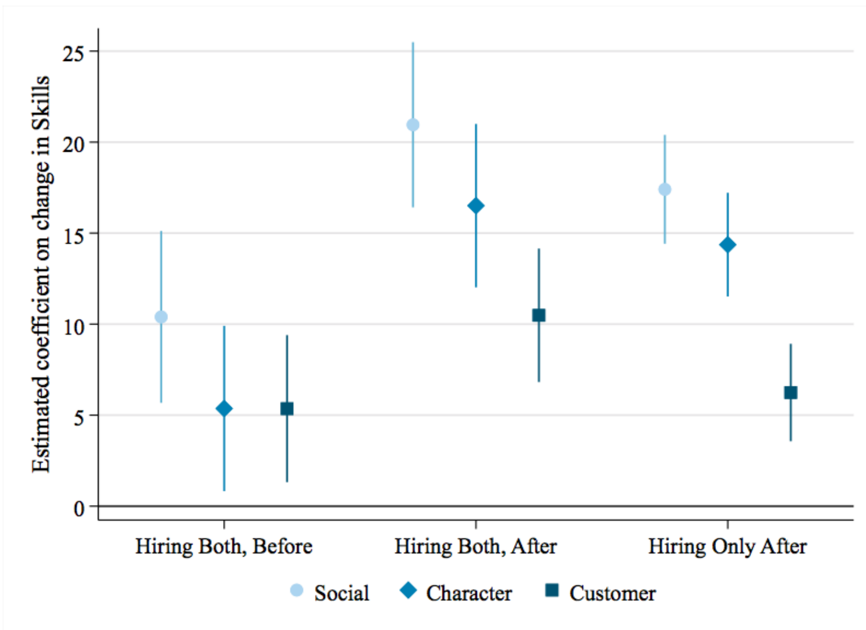
where $C_{p(e,c,t)}$ is an indicator representing whether posting p by establishment e in city c at time t lists a given characteristic. The βs give the relative likelihood of including that characteristic in the post for each type of establishment. From this specification, we consider the role of changes in hiring behavior and selection into hiring. Here, we focus on how the minimum wage increases affect the skills employers demand. We present the estimate for the effect of the minimum wage on character, customer service, and social skills in Figure 8. In Appendix H, we additionally consider how the minimum wage affects the likelihood that the position is full or part time, and the types of tasks required on the job.

We classify whether a job requires social, character or customer skills based on keywords from Deming and Kahn (2018) and Atalay et al. (2020).[37] In addition to cognitive skills,

[37] Based on their definitions, we use the following rules to capture whether a job ad requests a given

Deming and Kahn (2018) note that social skills are important predictors for productivity and wages, and capture "nonroutine analytic" tasks on the job that may justify a hire wage. We include "character" skills to capture the importance of non-cognitive skills and personality in the labor market. Finally, we include "customer service" skills since they are likely most relevant for low-wage employees.

Figure 8: Minimum Wages and Skills Demanded



Note: This figure plots the estimates and 95% confidence intervals for likelihood an establishment's job ad lists a social, character, or customer skill. All specifications control for city FE. Standard errors are clustered by establishment.

Overall, our results indicate that employers demand applicants with more social, character and customer skills after a minimum wage increase. Establishments that hire both before and after the increase is announced respond to the higher wage floor by seeking applicants with more social, customer, and character skills– β_2 is significantly higher than β_1 in all three specifications ($p < 0.005$). Establishment composition may also play a role since establishments who only hire before the announcement post ads with fewer skills than all

skill. (1) character: character, energetic, detail oriented, meeting deadlines, multi-tasking, time management; (2) customer service: client, customer, customer service, patient, sales; and (3) social: collaboration, communication, negotiation, presentation, social, teamwork.

other types.

6.2.2 Robustness to Changes in Firm Composition

Having established that firm job postings change after the minimum wage hike is announced, we now establish that these changes do not drive our results. First, we add firm fixed effects to Equation 4 and compare the behavior of different establishments belonging to the same parent company. Some establishments experience minimum wage increases, while others do not because they are located in different states.[38] This specification limits the role of compositional changes by focusing on the within-firm variation. In this specification, shown in Column (2) of Table 3, the estimates on Black and Black × After Announce are slightly smaller in magnitude, but not different from Column (1).

Next, we estimate a version of Equation 4 that includes establishment fixed effects to limit the role of establishment compositional changes further. The magnitude of the coefficients are smaller in magnitude, but the results still indicate that minimum wage increases shrink the racial callback gap by 56 percent (Table 3 Column (3)). Overall, this pattern of results suggests that establishment composition likely plays a role, but it does not fully explain why the minimum wage shrinks the racial callback gap. Appendix G presents a specification curve showing that these qualitative results are robust to alternative specifications, as in Figure 5.[39]

As a second exercise, we again divided our establishments into four mutually exclusive groups by when the establishment was hiring and when we applied. If establishment composition solely contributes to the shrinking of the racial gap, then we would expect no differences in the behavior of establishments that were hiring both before and after the minimum wage announcement. We would expect there only to be a difference between establishments that only hired either before or after the minimum wage hike. Instead, if establishments are only

[38]Since an observation is at the applicant × job-posting × establishment level, this strategy does not rely on two-way fixed effects.

[39]The appendix also presents the number of postings by unique firms and establishments over time.

Table 3: The Minimum Wage and the Racial Callback Gap

	(1)	(2) Callback	(3)
Black	-3.21***	-2.94***	-2.33***
	(0.61)	(0.65)	(0.58)
After Announce	-9.09***	-6.61***	-8.50***
	(0.82)	(1.00)	(1.31)
Black × After Announce	2.58***	2.11**	1.30*
	(0.68)	(0.73)	(0.65)
Firm Fixed Effects	No	Yes	No
Establishment Fixed Effects	No	No	Yes
N	34,990	34,990	33,398
R^2	0.02	0.36	0.62

Note: All specifications include city fixed effects and controls for applicant duration unemployed, human capital, and age. 2 observations are excluded from Column (3) with establishment fixed effects because they are the only applications to those establishments. Standard errors are clustered by establishment.*** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1

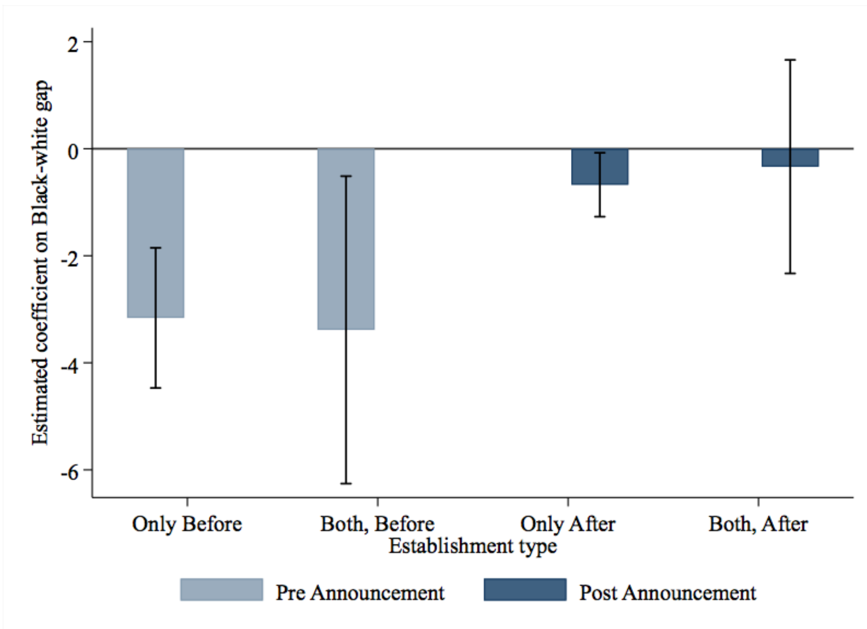
changing their behavior because of the policy, then we should see similar patterns between establishments hiring in both periods and those hiring only before and after the hikes. We estimate:

$$Y_{ict} = \sum_{e} \omega_e \mathbb{1}_e + \sum_{e} \delta_e \mathbb{1}_e B_i + X_i' \gamma + \eta_c + \epsilon_{ict} \tag{9}$$

where $\mathbb{1}_e$ is an indicator representing whether the establishment is of type e. The main parameters of interest are the four δs, which correspond to the racial callback gaps by establishment type and period. Since we previously found that establishments change their behavior after the announcement, we again pool the announced and enacted periods into an "after announced" period for power. We plot the δ coefficients and their confidence intervals in Figure 9.

The effects of minimum wage increases by establishment type suggest that establishment composition matters relatively little. The callback gap shrinks by about 3.0 percentage

Figure 9: Racial Callback Gap by When Establishment Hires



Note: The figure presents estimates and confidence intervals for the change in the racial callback gap from the minimum wage across our three establishment types: (1) only hire before minimum wage is announced, (2) only hire after minimum wage is announced, and (3) hires both before and after. We split type (3) into the before and after the announcement periods. The specification control for city FE, and other characteristics. Standard errors are clustered by establishment.

points among establishments that hire both before and after the announcement ($p < 0.095$). We additionally find a 2.5 percentage point difference between establishments that only hire after and those that only hire before ($p = 0.001$), which is not statistically different from the 3.0 percentage point change among establishments that hire in both periods. Instead, our results imply that hiring managers change their behavior in response to the higher minimum wage, consistent with the discussion from Section 5. They use a different threshold for deciding who to callback, which mostly crowds out white applicants.

By combining data on callback from our correspondence study with information contained in the job ads, we build on Clemens et al. (2021) to show that establishments demand more skilled applicants after the minimum wage increase, but this does not seem to disadvantage Black applicants regardless of their education or previous duration unemployed. The results on establishment composition are based on our sample of job postings on Indeed.com. If the minimum wage changed the platform some establishments use to search for applicants, we would have missed those jobs, creating selection into the sample. While we tried to apply to every relevant job on Indeed.com, our resources were somewhat limited, and we could not apply to all postings.

6.3 Changes in Non-Experimental Applications

Next, we consider relaxing the view that the firms' beliefs about the distributions of worker productivity are primitives and the minimum wage only changes the firm's hiring threshold. One plausible concern is that the minimum wage may change the search efforts of non-experimental applicants, increasing the competition for callbacks. However, for changes in the applicant pool to bias our results, the minimum wage must change the search effort of other applicants, applications to the same vacancy must be rival in callbacks, and the effects of additional resumes must affect callbacks differentially by race. If any of these factors are economically insignificant, then changes in labor supply will not affect our interpretation of the results. Therefore, we address the validity of each of these factors in turn.

Labor market search-and-matching models predict that increases in the minimum wage lead to an increase in the supply of applicants and search intensity, leading to better job matches (Acemoglu, 2001; Flinn, 2006; Ahn et al., 2011). However, empirically, Adams et al. (2022) finds evidence of increases in search effort of those previously looking for minimum wage jobs following minimum wage hikes and no changes to the composition of those searching for minimum wage jobs. Moreover, the intensive margin changes quickly dissipated one month after the hike. Thus, if changes in search effort were driving our results, we would expect to see the callback gap rebound shortly after the minimum wage hike. However, as we showed in Figure 4, the reduction in the RCG persists throughout the sample period, suggesting that changes in search effort are not a substantial concern in our experiment.

Next, we investigate whether applications for the same vacancy are rival in callbacks. While we cannot measure non-experimental resumes, our randomization strategy allows us to investigate whether experimental applications for the same vacancy are rivalrous.[40] We can think of resume rivalry as a spillover effect. Since we sent two resumes to each firm in our study, the spillover effect is the effect of one application on the probability of a callback for the other application. We identify these spillover effects using the following regressions

$$Y_{j,i} = \alpha + \tau_d D_{j,i} + \theta_{d0} D_{j,-i}(1 - D_{j,i}) + \theta_{d1} D_{j,-i} D_{j,i} + X'_{j,i}\gamma + \xi_{j,i}, \tag{10}$$

where each vacancy is indexed by $j = 1, ..., J$ with 2 applications per vacancy, so that each application i to vacancy j has 1 other application. The variable $D_{j,-i} \in \{\text{Black}_{j,-i}, \text{Unemployed 12 months}_{j,-i}, \text{GED}_{j,-i}\}$ is the treatment status of the other application sent to vacancy j. Finally, $X_{j,i}$ is a vector of application characteristics.

Because we randomize the pair type, these regressions can be though of as a partial

[40] Previous papers have examined this possibility and found mixed evidence. Abel (2017) finds that a firm's hiring decision in South Africa depends on the applicant pool's composition. Phillips (2019) finds evidence that applicants who compete against higher quality applicant pools receive more callbacks. Similarly, Kessler et al. (2022) finds that candidates in the incentivized resume rating task are rated less favorably when their application follows a white male application. Radbruch and Schiprowski (2021) finds that the previous applicant interviewed by a firm affects the subsequent decision of the evaluator. However, in larger samples, Kline et al. (2021) do not find any evidence of rivalry between resumes.

population experiment or randomized saturation design. Potential outcomes in this type of experiment are now given by $Y_{j,i}(D,T)$. Recall that vacancies were first randomized to be of type $T_j \in \{\text{Race pair, Unemployment duration pair}\}$. Vazquez-Bare (2022) shows that under the additional assumption of exchangeability, the parameters from this regression map into the direct and spillover effects of resume characteristics.[41]

The coefficient $\tau_d = \mathbb{P}[Y_{j,i}(1,0) - Y_{j,i}(0,0)]$ represents the direct effect of being treated (having a distinctively Black name, a long unemployment duration, or a GED). In the case of a distinctively Black name, it is the racial callback gap when no other fictitious resumes are sent to the firm. The coefficient $\theta_{d0} = \mathbb{P}[Y_{ij}(0,1) - Y_{ij}(0,0)]$ is the average spillover effect of having a treatment resume for control resumes (white, short unemployment duration or high school). Similarly, θ_{d1} is the average spillover effect from the firm receiving other treated applications on treated applicants, $\mathbb{P}[Y_{ij}(1,1) - Y_{ij}(1,0)]$. If our estimates are not confounded by spillovers from other applications, we would find that $\tau_d > 0$ while $\theta_{d0} = 0$ and $\theta_{d1} = 0$.

Estimates of Equation 10 appear in Table 4. Column (1) of Table 4 replicates our estimate of Equation 3 with the spillover sample.[42] These estimates closely match those of the full sample. Columns (2) through (4) of Table 4 display estimates of Equation 10 with different definitions of treatment.

We cannot reject the null hypothesis of no spillovers onto treatment or control applications in any specification. Moreover, Column (2) of Table 4 shows that the spillover coefficients are both an order of magnitude smaller than the direct effects. Including these terms reduces the precision of the estimate of the direct effect but does not affect its size. Column (3) shows the spillovers of treatment resumes on other treatment and control resumes are larger in magnitude for the long unemployment duration. In contrast, Column (4) shows that the spillovers from the GED treatments are also economically small. Moreover, introducing these terms into the regression never meaningfully changes our estimate of the racial callback gap.

[41] Exchangability in this context means that potential callbacks depend on how many Black experimental applications the firm receives, but not the exact characteristics of the resumes.

[42] This sample excludes 2,595 establishments that only receive one application and four establishments that erroneously receive too many applications.

Together, these results suggest very limited evidence of rivalry between applications.[43]

Table 4: Direct and Indirect Effects of Resume Characteristics

	(1) Callback	(2) Callback	(3) Callback	(4) Callback
$\text{Black}_{i,j}$	-1.389***	-1.266*	-1.406***	-1.389***
	(0.280)	(0.671)	(0.429)	(0.280)
$\text{Unemp 12 mo}_{i,j}$	-0.510	-0.510	-0.120	-0.510
	(0.377)	(0.377)	(0.494)	(0.377)
$\text{GED}_{i,j}$	-0.530	-0.530	-0.530	-0.505
	(0.330)	(0.330)	(0.330)	(0.536)
$\text{Black}_{i,j} \times \text{Black}_{-i,j}$		0.106		
		(0.680)		
$\text{White}_{ij} \times \text{Black}_{-i,j}$		-0.171		
		(0.628)		
$\text{Unemp 1 mo}_{i,j} \times \text{Unemp 12 mo}_{-i,j}$			0.357	
			(0.557)	
$\text{Unemp 12 mo}_{i,j} \times \text{Unemp 12 mo}_{-i,j}$			-0.426	
			(0.538)	
$\text{HS}_{i,j} \times \text{GED}_{-i,j}$				0.019
				(0.546)
$\text{GED}_{i,j} \times \text{GED}_{-i,j}$				-0.031
				(0.534)
Constant	11.640***	11.560***	11.471***	11.631***
	(0.375)	(0.674)	(0.570)	(0.494)
R-squared	0.011	0.011	0.011	0.011
Observations	32,388	32,388	32,388	32,388

Note: This table reports results from tests of the assumptions that applications at each job have no spillovers onto other applications at the same job. Column (1) displays estimates of Equation 3 using the sample with firms who receive one other resume. This excludes 2,595 establishments who received only one resume and four establishments who received more than two resumes. Columns (2) through (4) estimate versions of Equation 10 with the same sample to investigate spillovers. All specifications control for the applicant's age and the firm's city. Standard errors are clustered by establishment. *** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05.

Finally, we investigate whether the effects of additional resumes affected callbacks differentially by race. To do this, we estimate whether the direct or spillover effects change with the announcement of the minimum wage. If these effects don't vary with the minimum wage, we can rule out changes in the composition of the labor pool to be driving our results. Table 5 shows estimates from a regression allowing for spillover effects to change with the

[43] Appendix Table A4 provides an additional test that resumes are rivalrous with each other. In this table, we use the randomized order of the resumes to test whether there are order effects of receiving a resume and whether any potential order effects influence callbacks differentially by race. We find that resume order is not important either overall, or differentially by race.

announcement of the minimum wage. Similar as before, we begin by estimating Equation 4 using the spillover sample. Column 1 in Table 5 shows that the treatment effects in this sample are simlar to the main results.

Column (2) allows for racial spillovers to change with the increase in the minimum wage. Columns (3) and (4) perform similar tests for the long unemployment duration and GED treatments. The spillover effects increase in magnitude when the minimum wage increase is announced; however, these changes are never statistically significant. The positive coefficients and lack of statistical significance on these coefficients are inconsistent with increased labor market competition. Phillips (2019) points out that the magnitude of the spillovers is decreasing in the applicant pool size, including real applicants. Moreover, these results are inconsistent with higher quality applicants joining the labor market in response to the minimum wage hike. In Phillips (2019)'s model, the firm's response to a new applicant depends on how that applicant compares to the average applicant. When the new applicants who pursue minimum wage jobs after the hike are of higher quality, the spillover effects from an additional resume should decrease. Additionally, we find large and significant decreases in the racial callback gap in all specifications, even after controlling for potential spillovers. Thus, we again find no evidence in favor of changes in the composition of applicants differentially affecting applications by race.

All together, the previous literature and these results suggest that changes in the labor composition are not driving the results. We find limited evidence that resumes were rivalrous with each other. Even if the resumes were rivalrous with each other, the direct effects of treatment are stable changes in the direct effects are not influenced by changes in the minimum wage. While this is not direct evidence that changes in the labor supply do not impact our conclusions, they provide some evidence that the effect of a Black resume is independent of changes in the applicant pool.

Table 5: Direct and Indirect Effects of Resume Characteristics

	(1) Callback	(2) Callback	(3) Callback	(4) Callback
$\text{Black}_{i,j}$	-3.067***	-3.560**	-3.750***	-3.065***
	(0.629)	(1.589)	(0.989)	(0.629)
Unemp 12 $\text{mo}_{i,j}$	-0.531	-0.526	-0.106	-0.531
	(0.376)	(0.376)	(0.492)	(0.376)
$\text{GED}_{i,j}$	-0.565*	-0.573*	-0.572*	-0.521
	(0.329)	(0.329)	(0.329)	(0.533)
Announced	-9.043***	-10.316***	-10.416***	-8.912***
	(0.842)	(1.677)	(1.296)	(0.935)
$\text{Black}_{i,j}$ × Announced	2.391***	3.201*	3.324***	2.390***
	(0.701)	(1.733)	(1.092)	(0.701)
$\text{Black}_{i,j} \times \text{Black}_{-i,j}$		-1.120		
		(1.602)		
$\text{White}_{ij} \times \text{Black}_{-i,j}$		-1.496		
		(1.381)		
Unemp 1 $\text{mo}_{i,j}$ × Unemp 12 $\text{mo}_{-i,j}$			-0.894	
			(1.148)	
Unemp 12 $\text{mo}_{i,j}$ × Unemp 12 $\text{mo}_{-i,j}$			-1.734	
			(1.124)	
$\text{HS}_{i,j} \times \text{GED}_{-i,j}$				0.060
				(0.923)
$\text{GED}_{i,j} \times \text{GED}_{-i,j}$				0.225
				(1.126)
$\text{White}_{ij} \times \text{Black}_{-i,j}$ × Announced		1.691		
		(1.746)		
$\text{Black}_{i,j} \times \text{Black}_{-i,j}$ × Announced		1.936		
		(1.533)		
Unemp 1 $\text{mo}_{i,j}$ × Unemp 12 $\text{mo}_{-i,j}$ × Announce			1.824	
			(1.233)	
Unemp 12 $\text{mo}_{i,j}$ × Unemp 12 $\text{mo}_{-i,j}$ × Announ			1.815	
			(1.207)	
$\text{HS}_{i,j} \times \text{GED}_{-i,j}$ × Announced				-0.078
				(0.882)
$\text{GED}_{i,j} \times \text{GED}_{-i,j}$				-0.440
				(1.235)
Constant	18.084***	18.934***	18.872***	17.989***
	(0.763)	(1.540)	(1.199)	(0.906)
R-squared	0.019	0.019	0.019	0.019
Observations	32,388	32,388	32,388	32,388

Note: This table reports results from tests of the assumptions that spillover effects do not change with the minimum wage. Column (1) displays estimates of Equation4 using the sample with firms who receive one other resume. This excludes 2,595 establishments who received only one resume and four establishments who received more than two resumes. Columns (2) through (4) estimate versions of Equation 10 with the same sample to investigate spillovers. Column (5) displays estimates of Equation 4 using the same sample. Columns (6) through (8) examine whether the minimum wage affected the direct or indirect effects of receiving applications with different resume characteristics. All specifications control for the applicant's age and the firm's city. Standard errors are clustered by establishment. *** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05.

7. Conclusion

We conduct a correspondence study to investigate whether increases in the minimum wage exacerbate racial labor market disparities. Before states announce that they will increase the minimum wage, applicants with distinctively Black names are 3.2 percentage points (19%) less likely to receive a callback than applicants with distinctively white names. After the announcements, the racial callback gap shrinks 80%. We provide evidence that the gap decreases because white applicants are more likely to be on the margin of a callback, partly due to a less dispersed distribution of perceived productivity. Both taste-based and statistical discrimination models predict that employers will call back a larger portion of relatively low quality white applicants. When it becomes more costly to employ workers, these are the applicants who are no longer called back for an interview.

Our framework provides a method to predict how minimum wages will exacerbate or attenuate the racial callback gap. The data generated by a correspondence study before a minimum wage hike can capture racial discrimination and statistical discrimination in the mean and variance of perceived applicant productivity by race. Those parameters can provide sufficient information to learn which types of applicants are most at risk from potential future minimum wage increases.

References

Abel, M. (2017). Labor market discrimination and sorting: evidence from south africa. World Bank Policy Research Working Paper (8180).

Abrams, E., J. Libgober, and J. A. List (2020). Research registries: Facts, myths, and possible improvements. Technical report, National Bureau of Economic Research.

Acemoglu, D. (2001). Good jobs versus bad jobs. Journal of labor Economics 19(1), 1–21.

Adams, C., J. Meer, and C. Sloan (2022). The minimum wage and search effort. Economics Letters 212, 110288.

Agan, A. and S. Starr (2018). Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment. The Quarterly Journal of Economics 133(1), 191–235.

Ahn, T., P. Arcidiacono, and W. Wessels (2011). The distributional impacts of minimum wage increases when both labor supply and labor demand are endogenous. Journal of Business & Economic Statistics 29(1), 12–23.

Aigner, D. J. and G. G. Cain (1977). Statistical theories of discrimination in labor markets. Ilr Review 30(2), 175–187.

Altonji, J. G. and C. R. Pierret (2001). Employer learning and statistical discrimination. The Quarterly Journal of Economics 116(1), 313–350.

Angrist, J. D. (2014). The perils of peer effects. Labour Economics 30, 98–108.

Arnold, D., W. Dobbie, and P. Hull (2022). Measuring racial discrimination in bail decisions. American Economic Review 112(9), 2992–3038.

Arnold, D., W. S. Dobbie, and P. Hull (2020). Measuring racial discrimination in bail decisions. Technical report, National Bureau of Economic Research.

Asiedu, E., D. Karlan, M. Lambon-Quayefio, and C. Udry (2021). A call for structured ethics appendices in social science papers. Proceedings of the National Academy of Sciences 118(29), e2024570118.

Atalay, E., P. Phongthiengtham, S. Sotelo, and D. Tannenbaum (2020). The evolution of work in the united states. American Economic Journal: Applied Economics 12(2), 1–34.

Autor, D. H. and D. Scarborough (2008). Does job testing harm minority workers? evidence from retail establishments. The Quarterly Journal of Economics 123(1), 219–277.

Baert, S. (2018). Hiring discrimination: An overview of (almost) all correspondence experiments since 2005. Audit studies: Behind the scenes with theory, method, and nuance, 63–77.

Bailey, M. J., J. DiNardo, and B. A. Stuart (2021). The economic impact of a high national minimum wage: Evidence from the 1966 fair labor standards act. Journal of Labor Economics 39(S2), S329–S367.

Baird, S., J. A. Bohren, C. McIntosh, and B. Ozler (2014). Designing experiments to measure spillover effects.

Bartoš, V., M. Bauer, J. Chytilová, and F. Matějka (2016). Attention discrimination: Theory and field experiments with monitoring information acquisition. American Economic Review 106(6), 1437–75.

Becker, G. S. (1957). The economics of discrimination. Chicago: University of Chicago Press.

Bertrand, M. and E. Duflo (2017). Field experiments on discrimination. Handbook of economic field experiments 1, 309–393.

Bertrand, M. and S. Mullainathan (2004). Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination. American Economic Review 94(4), 991–1013.

Bohren, J. A., K. Haggag, A. Imas, and D. G. Pope (2019). Inaccurate statistical discrimination: An identification problem. Technical report, National Bureau of Economic Research.

Brown, C. (1984). Black-white earnings ratios since the civil rights act of 1964: The importance of labor market dropouts. The Quarterly Journal of Economics 99(1), 31–44.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-based improvements for inference with clustered errors. The Review of Economics and Statistics 90(3), 414–427.

Canay, I. A., A. Santos, and A. M. Shaikh (2021). The wild bootstrap with a "small" number of "large" clusters. Review of Economics and Statistics 103(2), 346–363.

Card, D. and A. B. Krueger (1992). School quality and black-white relative earnings: A direct assessment. The quarterly journal of Economics 107(1), 151–200.

Card, D. and A. B. Krueger (1994). Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. American Economic Review 84(4), 772–93.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019). The effect of minimum wages on low-wage jobs. The Quarterly Journal of Economics 134(3), 1405–1454.

Charles, K. and J. Guryan (2008). Prejudice and Wages: An Empirical Assessment of Becker's The Economics of Discrimination. Journal of Political Economy 116(5), 773–809.

Clemens, J., L. B. Kahn, and J. Meer (2021). Dropouts need not apply? the minimum wage and skill upgrading. Journal of Labor Economics 39(S1), S107–S149.

Clemens, J. and M. Wither (2019). The minimum wage and the great recession: Evidence of effects on the employment and income trajectories of low-skilled workers. Journal of Public Economics 170, 53–67.

Crépon, B., F. Devoto, E. Duflo, and W. Parienté (2015). Estimating the impact of microcredit on those who take it up: Evidence from a randomized experiment in morocco. American Economic Journal: Applied Economics 7(1), 123–50.

David, H., A. Manning, and C. L. Smith (2016). The contribution of the minimum wage to us wage inequality over three decades: a reassessment. American Economic Journal: Applied Economics 8(1), 58–99.

Deming, D. and L. B. Kahn (2018). Skill requirements across firms and labor markets: Evidence from job postings for professionals. Journal of Labor Economics 36(S1), S337–S369.

Derenoncourt, E. and C. Montialoux (2021). Minimum wages and racial inequality. The Quarterly Journal of Economics 136(1), 169–228.

Doleac, J. L. and L. C. Stein (2013). The visible hand: Race and online market outcomes. The Economic Journal 123(572), F469–F492.

Donohue, J. J. and J. Heckman (1991). Continuous versus episodic change: The impact of civil rights policy on the economic status of blacks. Journal of Economic Literature 29(4), 1603–1643.

Dube, A., T. W. Lester, and M. Reich (2016). Minimum wage shocks, employment flows, and labor market frictions. Journal of Labor Economics 34(3), 663–704.

Duflo, E. and E. Saez (2003). The role of information and social interactions in retirement plan decisions: Evidence from a randomized experiment. The Quarterly journal of economics 118(3), 815–842.

Eriksson, S. and D.-O. Rooth (2014). Do Employers Use Unemployment as a Sorting Criterion When Hiring? Evidence from a Field Experiment. American Economic Review 104(3), 1014–1039.

Flinn, C. J. (2006). Minimum wage effects on labor market outcomes under search, matching, and endogenous contact rates. Econometrica 74(4), 1013–1062.

Friedman, M. (1966). Minimum-wage rates. Newsweek.

Fryer, R. G. and S. D. Levitt (2004). The causes and consequences of distinctively black names. The Quarterly Journal of Economics 119(3), 767–805.

Fryer, R. G., D. Pager, and J. L. Spenkuch (2013). Racial disparities in job finding and offered wages. The Journal of Law and Economics 56(3), 633–689.

Gaddis, S. M. (2015). Discrimination in the credential society: An audit study of race and college selectivity in the labor market. Social Forces 93(4), 1451–1479.

Gaddis, S. M. (2017). How black are lakisha and jamal? racial perceptions from names used in correspondence audit studies. Sociological Science 4, 469–489.

Gneezy, U., J. List, and M. K. Price (2012). Toward an understanding of why people discriminate: Evidence from a series of natural field experiments. Technical report, National Bureau of Economic Research.

Guryan, J. and K. K. Charles (2013). Taste-based or statistical discrimination: the economics of discrimination returns to its roots. The Economic Journal 123(572), F417–F432.

Heckman, J. J. (1998). Detecting discrimination. Journal of economic perspectives 12(2), 101–116.

Heckman, J. J. and P. Siegelman (1993). The urban institute audit studies: Their methods and findings. Clear and convincing evidence: Measurement of discrimination in America, 187–258.

Heyes, A. and J. A. List (2016). Supply and demand for discrimination: Strategic revelation of own characteristics in a trust game. American Economic Review 106(5), 319–23.

Holz, J., R. Rivera, and B. A. Ba (2019). Spillover effects in police use of force. U of Penn, Inst for Law & Econ Research Paper (20-03).

Johnson, R. C. (2019). Children of the dream: Why school integration works. Basic Books.

Jowell, R. and P. Prescott-Clarke (1970). Racial discrimination and white-collar workers in britain. Race 11(4), 397–417.

Kaplan, J. (2021). predictrace: Predict the Race and Gender of a Given Name Using Census and Social Security Administration Data. R package version 2.0.0.

Kessler, J. B., C. Low, and X. Shan (2022). Lowering the playing field: Discrimination through sequential spillover effects.

Kessler, J. B., C. Low, and C. D. Sullivan (2019). Incentivized Resume Rating: Eliciting Employer Preferences without Deception. American Economic Review 109(11), 3713–3744.

Kline, P. and C. Walters (2021). Reasonable doubt: Experimental detection of job-level employment discrimination. Econometrica 89(2), 765–792.

Kline, P. M., E. K. Rose, and C. R. Walters (2021). Systemic discrimination among large us employers. Technical report, National Bureau of Economic Research.

Kreiner, C. T., D. Reck, and P. E. Skov (2020). Do lower minimum wages for young workers raise their employment? evidence from a danish discontinuity. Review of Economics and Statistics 102(2), 339–354.

Kroft, K., F. Lange, and M. J. Notowidigdo (2013). Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment*. The Quarterly Journal of Economics 128(3), 1123–1167.

Lee, D. and E. Saez (2012). Optimal minimum wage policy in competitive labor markets. Journal of Public Economics 96(9-10), 739–749.

Lepage, L. P. (2021). Endogenous learning, persistent employer biases, and discrimination. Persistent Employer Biases, and Discrimination (March 2, 2021).

Levitt, S. D. and J. A. List (2007). What do laboratory experiments measuring social preferences reveal about the real world? Journal of Economic perspectives 21(2), 153–174.

Lillard, L., J. P. Smith, and F. Welch (1986). What do we really know about wages? the importance of nonreporting and census imputation. Journal of Political Economy 94(3, Part 1), 489–506.

List, J. A. (2004). The nature and extent of discrimination in the marketplace: Evidence from the field. The Quarterly Journal of Economics 119(1), 49–89.

List, J. A. (2009). The irb is key in field experiments. Science 323(5915), 713–714.

List, J. A., A. M. Shaikh, A. Vayalinkal, et al. (2021). Multiple testing with covariate adjustment in experimental economics. Technical report, The Field Experiments Website.

List, J. A., A. M. Shaikh, and Y. Xu (2019). Multiple hypothesis testing in experimental economics. Experimental Economics 22(4), 773–793.

Luca, D. L. and M. Luca (2019). Survival of the fittest: the impact of the minimum wage on firm exit. Technical report, National Bureau of Economic Research.

Manson, S., J. Schroeder, D. Van Riper, T. Kugler, and S. Ruggles (2022). Ipums national historical geographic information system: Version 16.0. American Economic Review.

McCall, J. J. (1970). Economics of information and job search. The Quarterly Journal of Economics, 113–126.

Meer, J. and J. West (2016). Effects of the minimum wage on employment dynamics. Journal of Human Resources 51(2), 500–522.

Neumark, D. (2012). Detecting discrimination in audit and correspondence studies. Journal of Human Resources 47(4), 1128–1157.

Neumark, D., I. Burn, and P. Button (2019). Is it harder for older workers to find jobs? new and improved evidence from a field experiment. Journal of Political Economy 127(2), 922–970.

Neumark, D., I. Burn, P. Button, and N. Chehras (2019). Do state laws protecting older workers from discrimination reduce age discrimination in hiring? evidence from a field experiment. The Journal of Law and Economics 62(2), 373–402.

Neumark, D. and J. Rich (2019). Do field experiments on labor and housing markets overstate discrimination? a re-examination of the evidence. ILR Review 72(1), 223–252.

Neumark, D., W. L. Wascher, et al. (2007). Minimum wages and employment. Foundations and Trends® in Microeconomics 3(1–2), 1–182.

Nunley, J. M., A. Pugh, N. Romero, and R. A. Seals (2015). Racial discrimination in the labor market for recent college graduates: Evidence from a field experiment. The BE Journal of Economic Analysis & Policy 15(3), 1093–1125.

Pager, D. (2016). Are firms that discriminate more likely to go out of business? Sociological Science 3, 849–859.

Pager, D. and D. Karafin (2009). Bayesian bigot? statistical discrimination, stereotypes, and employer decision making. The Annals of the American Academy of Political and Social Science 621(1), 70–93.

Phillips, D. C. (2019). Do Comparisons of Fictional Applicants Measure Discrimination When Search Externalities are Present? Evidence from Existing Experiments. The Economic Journal 129(621), 2240–2264.

Phillips, D. C. (2020). Do low-wage employers discriminate against applicants with long commutes? evidence from a correspondence experiment. Journal of Human Resources 55(3), 864–901.

Quillian, L., D. Pager, O. Hexel, and A. H. Midtbøen (2017). Meta-analysis of field experiments shows no change in racial discrimination in hiring over time. Proceedings of the National Academy of Sciences 114(41), 10870–10875.

Radbruch, J. and A. Schiprowski (2021). Interview sequences and the formation of subjective assessments.

Ritter, J. A. and L. J. Taylor (2011). Racial disparity in unemployment. The Review of Economics and Statistics 93(1), 30–42.

Romano, J. P. and M. Wolf (2016). Efficient computation of adjusted p-values for resampling-based stepdown multiple testing. Statistics & Probability Letters 113, 38–40.

Rubin, M. (2021). When to adjust alpha during multiple testing: A consideration of disjunction, conjunction, and individual testing. Synthese 199(3), 10969–11000.

Shrider, E. A., M. Kollar, F. Chen, J. Semega, et al. (2021). Income and poverty in the united states: 2020. US Census Bureau, Current Population Reports (P60-273).

Simon, A. and M. Wilson (2021). Optimal minimum wage setting in a federal system. Journal of Urban Economics 123, 103336.

Smith, J. P. and F. R. Welch (1989). Black economic progress after myrdal. Journal of economic literature 27(2), 519–564.

Spitz-Oener, A. (2006). Technical change, job tasks, and rising educational demands: Looking outside the wage structure. Journal of labor economics 24(2), 235–270.

Stiglitz, J. E. (1973). Approaches to the economics of discrimination. The American Economic Review 63(2), 287–295.

Thomas, S. (2007). Basic economics. a common sense guide to the economy.

Turner, M. D. and B. Demiralp (2001). Do higher minimum wages harm minority and inner-city teens? The Review of Black Political Economy 28(4), 95–116.

Vazquez-Bare, G. (2022). Identification and estimation of spillover effects in randomized experiments. Journal of Econometrics.

Westfall, P. H. and S. S. Young (1993). Resampling-based multiple testing: Examples and methods for p-value adjustment, Volume 279. John Wiley & Sons.

Wursten, J. and M. Reich (2021). Racial inequality and minimum wages in frictional labor markets.

Zschirnt, E. and D. Ruedin (2016). Ethnic discrimination in hiring decisions: a meta-analysis of correspondence tests 1990–2015. Journal of Ethnic and Migration Studies 42(7), 1115–1134.

Appendix

A. Details of Experimental Design

A.1 Resume Characteristics and Creation

Names: The names in the experiment are drawn from combinations of first and last names used in Agan and Starr (2018). Each resume was randomly assigned a first and last name from the designated race with replacement. The list of first and last names used in the experiment appears in Table A1 below.

Table A1: First and Last Names Assigned by Race

First Name		Last Name	
Distinctively Black	Distinctively White	Distinctively Black	Distinctively White
Daquan	Cody	Alston	Brennan
Darnell	Douglas	Banks	Fox
Darryl	Dylan	Bryant	Hansen
Denzel	Jacob	Byrd	Hoffman
Dwayne	John	Charles	Kane
Elijah	Kyle	Fields	Meyer
Isaiah	Matthew	Franklin	O'Niell
Jamal	Nicholas	Hawkins	Romano
Jaquan	Ryan	Ingram	Russo
Jermaine	Scott	Jackson	Ryan
Malcolm	Sean	Jenkins	Schmidt
Marquis	Shane	Darryl	Snyder
Maurice	Stephen	Joseph	Sullivan
Reginald	Thomas	Pierre	Wagner
Terrance	Tyler	Robinson	Weber
Terrell		Simons	
Tyree		Washington	
Tyrone		Williams	

Note: This table lists the first and last names assigned by race. The names were taken from Agan and Starr (2018).

To assess whether these names signal the intended race, we use the procedure from Kaplan (2021) to predict the posterior probability that an individual with a given first and last name is of the intended race. The probability for each first name is averaged over each time the name is used in conjunction with the last name in Table A2. As this table shows, the empirical probability that individuals with the names we chose are of the intended race is extremely high. These values are slightly higher for resumes with distinctively Black names than distinctively white names. However, we estimate ITT effects from our experiment, meaning that any interpretation by firms that resumes with distinctively white names are non-white would bias our estimates towards zero.

Table A2: Empirical Likelihood that First Names Signal the Intended Race

Distinctively Black		Distinctively White	
First Name	$\mathbb{P}[Black \vert \text{first name, surname}]$	First Name	$\mathbb{P}[White \vert \text{first name, surname}]$
Daquan	99.3	Cody	87.5
Darnell	99.3	Douglas	91.3
Denzel	98.7	Jacob	79.3
Dwayne	96.7	John	82.9
Elijah	97.9	Kyle	83.4
Isaiah	97.2	Matthew	85.8
Jamal	99.2	Nicholas	76.7
Jaquan	99.6	Ryan	82.7
Jermaine	98.5	Scott	94.6
Malcolm	91.4	Sean	77.6
Marquis	99.5	Shane	88.7
Reginald	97.9	Thomas	94.1
Terrance	98.6	Tyler	89.4
Terrell	97.3		
Tyree	95.7		
Tyrone	98.6		
Total	98.6		85.6

Note: This table reports the first names used in the experiment and the probability that a person with a given name is of the intended race. We calculate the probabilities by recovering the posterior probability that a person with a given first and last name is of the intended race using Kaplan (2021). Then, we average this probability over all iterations of the first names used in the experiment.

Locations: After assigning names to races, we randomly assigned each application to be from one of four cities: St. Louis, Springdale/Fayetteville, Little Rock, or Springfield. In wave 2, we repeated this process for the new cities we added to the experiment: Kansas City and St. Louis, IL. Table A3 shows the average posterior probability that a resume sent in a given state throughout the experiment is of the intended race. Similar to the overall sample, the probability that a resume is of the intended race is exceedingly high for both Black and white resumes but higher for Black resumes. Moreover, these probabilities are similar across states for both Black and white resumes.

Table A3: Empirical Likelihood that Names Signal the Intended Race by State

	Black Resumes				White Resumes			
	AR	MO	KS	IL	AR	MO	KS	IL
$\mathbb{P}[Black \mid \text{first name, surname}]$	97.9	97.4	98.4	97.4	4.3	2.9	4.1	5.2
	(2.7)	(2.3)	(1.8)	(3.5)	(4.7)	(3.6)	(4.4)	(5.9)
$\mathbb{P}[White \mid \text{first name, surname}]$	1.3	1.6	0.8	1.8	84.3	86.0	84.1	84.2
	(2.2)	(1.7)	(1.2)	(2.8)	(7.9)	(5.9)	(8.6)	(7.4)
Observations	4,527	8,050	2,825	1,955	4,471	8,159	2,844	1,878

Note: This table reports the estimated probability that a person with a given name is of a given race by state. Standard deviations are in parentheses. We calculate the probabilities by recovering the posterior probability using Kaplan (2021) with the subject's first name and surname. Then, we average this probability over all applications sent out in the experiment.

Unemployment Duration: Within each city and race, we assign half of the resumes to have an unemployment duration of 1 month and half of the cities to have an unemployment duration of 12 months. We chose these two unemployment durations because Kroft et al. (2013) found the largest difference in callbacks between these two lengths. We operationalize these durations by making the employment end date of their previous job to be either 1 or 12 months before the month that our experiment started. Regularly after the beginning of the experiment, we would increase the end date of the previous employment history to maintain the 1-month and 12-month unemployment duration.

Ages and Years of Experience: We assigned ages and years of experience to applicants based on their unemployment duration. If an application was given an unemployment duration of twelve months, we gave the applicant a birth year 20 years before the beginning of the experiment. These individuals were also given an employment start date one year before their employment end date to provide them with one year of work experience. If an application was given an unemployment duration of one month, we randomly assigned half of the applications to be 19 years old and half of the applications to be 20 years old. If the applicant was 19 years old, we assigned them one year of work experience; if the applicant was 20 years old, we assigned them two years of work experience by having a start date two years before their employment end date.

Contact Information: Each application provided firms with three different ways to contact the applicant. First, since each application was sent through Indeed, firms could contact the applicants directly through their Indeed accounts. Second, we manually created e-mail addresses for all of our applicants. Each e-mail account was associated with a resume and created using combinations of the applicant's first name, last name, and arbitrary integers. These e-mail addresses were also used as the login information for the Indeed accounts.

Finally, we provisioned phone numbers from Tresta. We chose phone numbers with area codes local to the cities we operated in, so each application had a local phone number. In waves 1 through 3, we assigned phone numbers to resumes based on their type, defined by the applicant's race, unemployment duration, and human capital attainment. Our randomization strategy ensured that employers would not see two applicants with the same phone number. In waves 4 and 5, we increased the number of phone numbers so that each resume had a unique phone number. Phone calls to each number were automatically directed to a voicemail with a standard, non-personalized message through Tresta.

Addresses: We assigned each application a home address in the cities where we submitted applications. To obtain the addresses, we went on apartment finder websites like Zillow, Apartments.com, and Domu.com and found 1 bedroom apartments with rental prices that were a third of a full-time minimum wage worker's income for a month. We always chose apartments near the cities we operated in because Phillips (2020) found evidence that firms are less likely to call back applicants with long commutes. Addresses were randomly assigned to applications without replacement.

Educational History: We randomly assigned each application to have a high school diploma or a GED within each race, unemployment duration, and city. If the individual had a high school diploma, we recorded their graduation date as 18 years after their birth date, as determined by age. We assigned each high school graduate to one of five local high schools within each city and race-unemployment duration pair. If the applicant had a GED, we did not list a high school on their application.

Employment History: Each applicant was assigned to one previous employer. To obtain a list of potential employers, we downloaded the available resumes from other applicants on Indeed.com seeking employment at low-wage jobs in the cities we apply to and the surrounding areas. For each previous job, we assigned the previous job title of a cashier, food preparation, team member, store associate, or janitor.

We only chose job titles that were relevant to the previous employer. For example, an applicant whose previous employment was at Walgreens could be a team member but could not work in food preparation. However, someone who previously worked at Arby's could have worked in food preparation. For each of the four job types, we generated a bank of generic work descriptions based on those from the other applications we found on Indeed.com and randomly assigned these descriptions to each resume. For example, a store associate's work description might have been "Clean and stocked the store. Provided customer service," and someone who worked in food preparation would have a description similar to "Took food orders, handled payments, and prepared food."

Miscellaneous resume characteristics: Resumes also included other small characteristics like a mission statement which we randomly added to each resume based on a bank of statements we created in line with those posted on indeed.com. It was common for applications to require idiosyncratic questions, which we could not prepare before the application began. Following Agan and Starr (2018), we instructed RAs, who were blind to the purpose of the study, to answer these questions positively and in line with their judgment. Before working, we instructed these research assistants about what employers generally look for in an application and how to answer these questions in a way that would increase the likelihood of receiving a callback. We did not include any references on the job applications because it was expensive to create additional phone numbers for fictitious references. Moreover, Agan and Starr (2018) found that no employers ever called the phone numbers for their references, suggesting that employers did not pay attention to them.

A.2 Job Sampling

We developed code that scraped vacancies from indeed regularly throughout the experimental period. During Waves 1 through 3, the scraping was done every day. During Waves 4 and 5, we changed this process to once weekly. The sampling procedure followed the following process.

1. Go to indeed.com advanced job search.

2. Limit jobs to those with salary estimates of $16,000 to $21,000. This range corresponds to $7.70 - $10.10 per hour or jobs with the following keywords: “retail”, “food+prep”, “fast+food”, “janitor”, “maid”, “cashier”, “retail+salespeople”, “cooks”, and “building+cleaners.”

3. Limit the location to within 10 miles of the following areas: Little Rock, AR, St. Louis, MO, Springfield, MO, Fayetteville, AR, Springdale, AR, Kansas City, MO, Kansas City, KS, East St. Louis, IL, Granite City, IL, Overland Park, KS, Shawnee, KS, Olathe, KS, Cahokia, IL, Washington Park, IL, Alton, IL, and Belleville, IL.

4. Use a fuzzy merge with a manual check to ensure that we remove jobs from the same establishments during the same wave.

5. Exclude jobs that are unusual or seem not to be minimum wage jobs (e.g., jobs at the National Guard).

6. When more jobs are available than we have the ability to apply to that period, we prioritize the jobs that were posted most recently.

A.3 Application Submission

After we have the set of jobs we intend to submit to, we randomly assign resumes to the application using the following approach.

1. For each job, independently randomly assign the applicant's race, unemployment status, and educational status with a probability of 1/2.

2. Randomly draw profiles with replacement from the set of resumes created using the procedure in Appendix A.1.

3. Generate an alternate resume of the same type as in the previous step if the randomly chosen applicant has a work history at its vacancy firm. When applying to firms, research assistants manually checked whether the application firm and the firm on the resume's work history match. If they match, the research assistant applied with the alternate resume.

4. Randomize, with 50 percent probability, whether the other resume sent to the firm is of the opposite race or the opposite unemployment duration. Other characteristics are randomized.

5. Randomly draw profiles with replacements from the set of resumes that share the same type determined in the previous step.

6. Generate an alternate resume for the second resume using a similar procedure as the alternate for the first resume.

After each randomization, we gave our research assistants a list of jobs, a link to the job ad, and the Indeed login information they needed to apply for the job. With some time lag, a second research assistant sent the second resume to the same firm using the second profile. Each research assistant was given a spreadsheet with only the information they needed to complete their tasks, so they were not aware of the identity of the other fictitious applicants. The randomization procedure ensured that the characteristics of the resume were independent of the application order.

Table A4 shows that we find no evidence that application order played a role in a firm's callback decision. There is no effect of being the second application sent to the firm overall or by the applicant's race. These results are robust to using resumes that received an opposite race resume (Column 1), an opposite unemployment duration resume (Column 2), or the full sample. While we cannot know with certainty that the firms viewed the applications in the order they were received, this table provides suggestive evidence against the sequential spillover effects found in Kessler et al. (2022).

Table A4: Effects of Resume Order

	(1) Callback	(2) Callback	(3) Callback
Black	-0.0171**	-0.0207***	-0.0188***
	(0.00625)	(0.00628)	(0.00442)
Second Application	-0.00107	-0.00364	-0.00231
	(0.00659)	(0.00583)	(0.00441)
Black × Second Application	0.00870	0.0119	0.0102
	(0.0111)	(0.00931)	(0.00727)
Constant	0.111***	0.111***	0.111***
	(0.00469)	(0.00469)	(0.00337)
Sample	Race Pair	Unemployment Duration Pair	Full Sample
R-squared	0.012	0.010	0.011
Observations	17,569	17,417	34,986

Note: This table displays the probability of being called back for a job interview by race, the order of the application, and the interaction between race and the order of the application. All specifications control for applicant age and firm city fixed effects. Column (1) shows the results for race pairs. That is, firms who recieved a second resume from the opposite race. Column (2) shows the results for unemployment duration pairs. Column (3) shows the pooled estimates, controlling for the subject's pair type. Standard errors are clustered at the establishment level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

It was not always possible to send a complete set of two resumes to the same firm within a wave. It was also not always possible to apply to the same firm across different waves. This was mostly due to firms removing their job ad from Indeed. There were also new jobs posted continuously throughout the experiment. As a result, the sample sizes change heterogeneously across waves.

A.4 Measuring Outcomes

Each e-mail and Indeed message was assigned to a firm using the information provided by the employer. In the few instances when the employer did not provide enough information to assign the application to the firm, we would respond to the e-mail and ask the employer for the information. Any voicemails were reviewed by research assistants and assigned to a firm using the information provided by the employer. When the employer did not leave enough information to attach the application to a firm, we called back the numbers from a different phone number, told them we received a missed call from that number and asked them for the firm's identity. We recorded all callbacks that occurred until August 2020. Regardless of the callback date, we assign the callback based on the application date. We investigate the implications of this decision in Appendix D.

Following Bertrand and Mullainathan (2004) and Kroft et al. (2013), we instructed research assistants to record any contact from the employers from these three sources. For contact to be considered a "call back", we required that the firm makes an explicit request for the individual to come in for an interview. We do not classify other types of communication, such as clarification about a question on the application, as a callback. This choice was made because it is difficult to map this type of communication to discrimination. For example, an employer who reaches out for additional information from a minority application more frequently may do this either because she is more interested in hiring the minority applicant or because she screens the minority applicant more carefully before hiring. Conversely, interview requests are a stronger signal that the employer is interested in hiring the applicant.

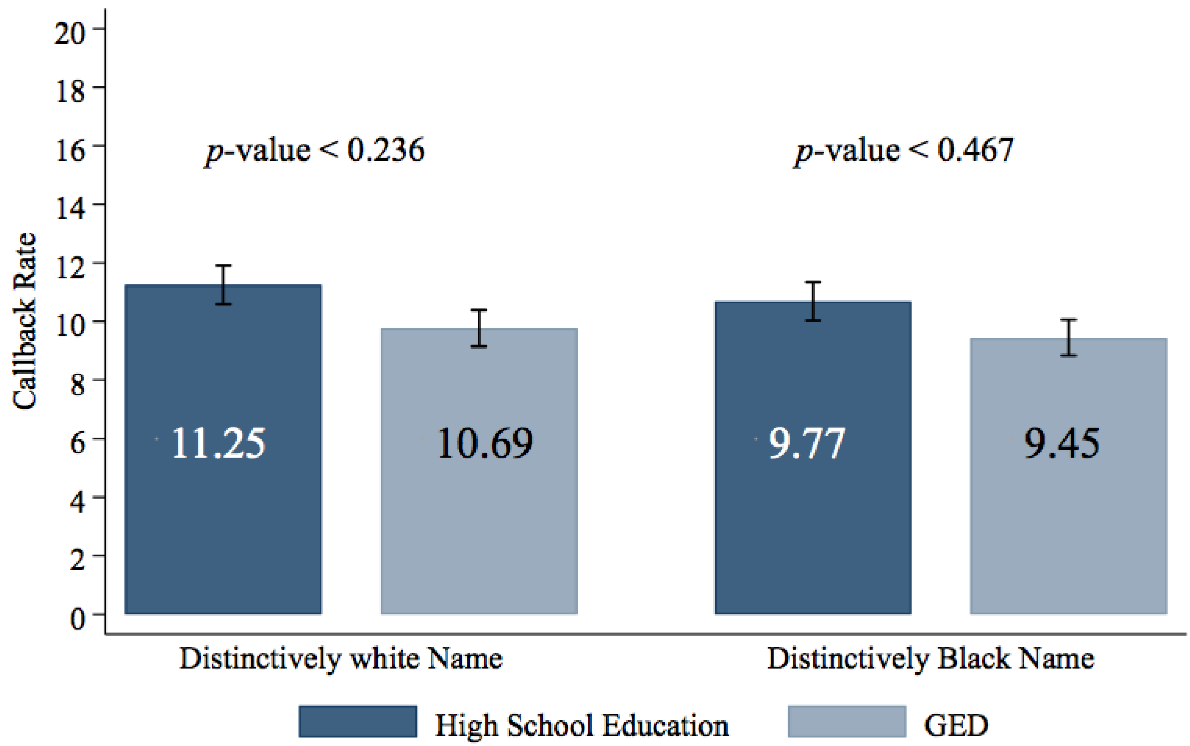
B. Applicant Names Potentially Signaling Socio-Economic Status

The primary treatment in this paper is the manipulation of the name on the resume. While we have interpreted differences in outcomes resulting from the name as racial effects, the name may also signal other characteristics. Indeed, Fryer and Levitt (2004) find that children with distinctively Black names tend to have lower socioeconomic status (SES) than children. Moreover, Gaddis (2015) finds that applicants with distinctively Black names that suggest the mother had low education are especially penalized.

Despite these findings, previous correspondence studies have found that using distinctively Black and white names successfully signals the applicant's race without unintentionally signalling SES (Bertrand and Mullainathan, 2004; Kline et al., 2021). We believe this confound is even less of a concern in our setting as we apply to low-wage entry-level jobs that likely primarily attract applicants of low socioeconomic status. Our resumes also provide the applicant's address, work history, educational attainment, and high school. These, perhaps more concrete, signals of SES likely reduce the employer's potential reliance on the applicant's name as a signal of SES. Nevertheless, we investigate the possibility that the experiment measures disparities by SES rather than racial disparities.

If distinctively Black names primarily signal SES, we would expect that applications with other attributes that signal high SES will benefit Black applicants more than white applicants. Perhaps the strongest signal of social status in our experiment is whether the applicant has a GED instead of a high school degree. Figure B1 displays callback rates by race and whether the applicant has a GED. We find that the returns to a high school education are similar for white and Black applicants and, if anything, a little larger for Black applicants. The lack of treatment effect heterogeneity on this dimension is inconsistent with the name on the resume signaling SES.

Figure B1: High School Education Effect by Race



Note: This figure displays the callback rates by the race of the applicant separately by whether the applicant has a high school education or GED. Error bars represent 95% confidence intervals. P-values refer to tests that the difference between education status within applicant race is significant different from zero.

Next, we link each applicant's address to a census tract and obtain information about individuals living in the tract containing the fictitious applicant's address from IMPUS Manson et al. (2022). We also obtain information about the applicant's high school Great Schools Score and expenditure per student from greatschools.org for the applications where the applicant had a high school degree. The experiment used 34 high schools and 146 unique census tracts. For each of the census tracts, we obtain information on variables associated with socioeconomic status. Table B1 lists these variables and their summary statistics.

Table B1: Socioeconomic Status Variables

	count	mean	sd	p10	p90
Great School Summary Rating	17,581	3.1	2.3	1.0	6.0
Expenditures Per Student	17,581	12,046	4,398	9949.0	16551.9
Percent of White Residents	34,986	61.0	26.1	19.5	88.1
Percent Of College Educated Residents	34,986	28.3	15.1	10.1	47.7
Percent Under Poverty Line	34,986	21.4	12.5	6.50	39.4
Median Household Income	34,986	45,822	19,090	23078	73872
Median Rent	34,986	857	166	666	1,082
Missing High School Characteristics (%)	34,986	53.4	49.9	0.0	100
Missing any Census variable (%)	34,986	0.93	9.59	0.0	0.0

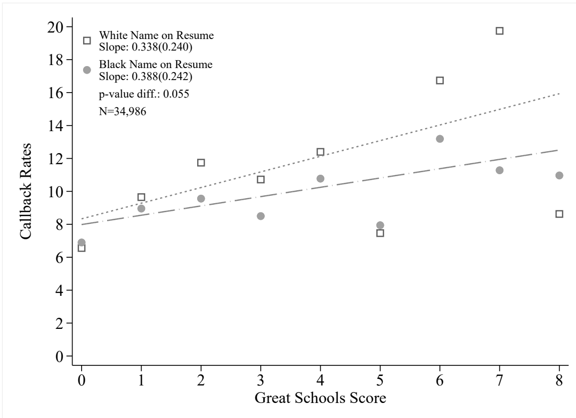
Note: This table displays the summary statistics for socioeconomic status variables. High school variables are obtained from greatschools.org. The value of these variables is missing if the subject has a GED rather than a high school diploma, or if the highschool does not appear on greatschools.org. Census tract level variables are obtained from Manson et al. (2022) and linked to the resume through the address that appears with the application.

Table B1 shows substantial variation in socioeconomic status signals across resumes as measured by the resume's census tract and high school characteristics. We can use this variation to understand whether distinctively Black names primarily signal race or socioeconomic status. A necessary condition for SES signals to confound our results is that employers value hiring applicants of higher SES. While plausible in other settings, we find limited evidence of this in the labor markets we examine.

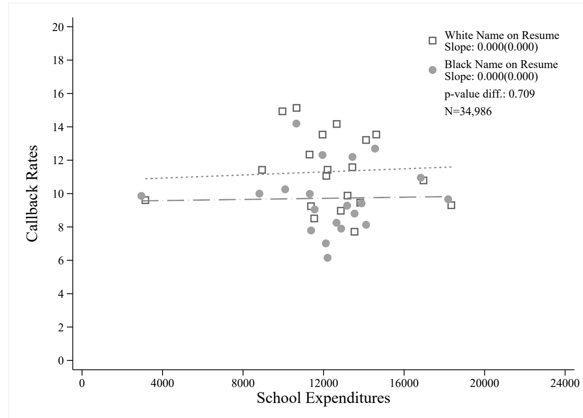
Figure B2 displays bin scatters of our SES variables and callback rates separately for resumes with distinctively Black and white names. We only find a positive association between the Great School Score and the portion of white residents and callback rates. In contrast, we can detect no significant correlations between high school expenditure per student, the whiteness of the census block, how college-educated the census block is, the portion of the census block living in poverty, or the census block's median income and callback rates. While these relationships are not identified, they suggest modest returns to high SES in this labor market.

Figure B2: Relationship between SES Variables and Callbacks by Race

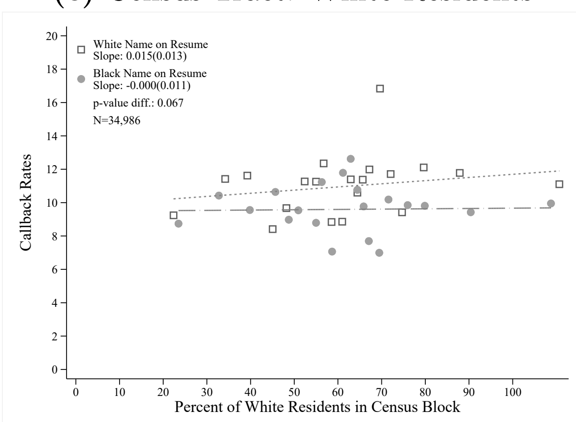
(a) High School: Great School Score



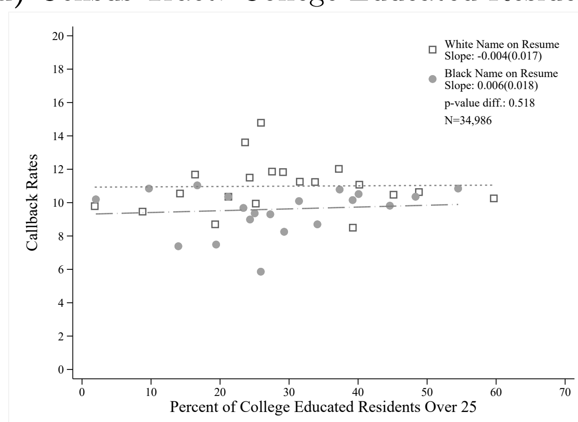
(b) High School: Expenditures per Student



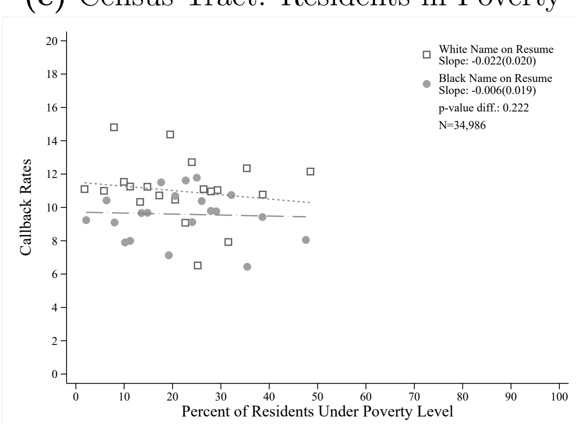
(c) Census Tract: White Residents



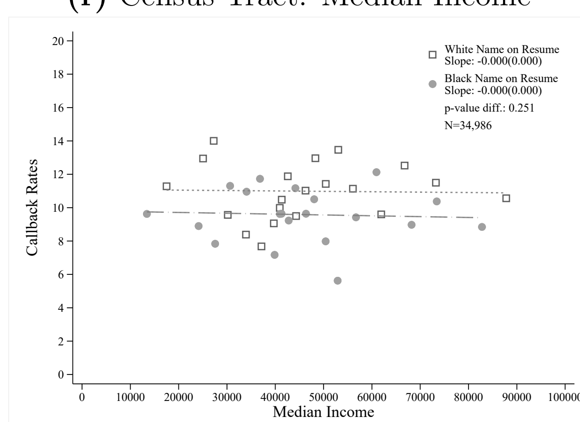
(d) Census Tract: College Educated Residents



(e) Census Tract: Residents in Poverty



(f) Census Tract: Median Income



Note: This figure displays the relationships between socioeconomic status variables and callbacks separately by the applicant's race. High school variables are obtained from greatschools.org. The value of these variables is missing if the subject has a GED rather than a high school diploma, or if the highschool does not appear on greatschools.org. Census tract level variables are obtained from Manson et al. (2022) and linked to the resume through the address that appears with the application.

If distinctively Black names primarily signal socioeconomic status, we would also expect that applications with distinctively Black names are helped more by living in high SES areas or by coming from a higher quality school. To examine this, we test whether the correlations between our SES variables and callback rates are stronger for applicants with distinctively Black names. In all instances, we either find no evidence of a correlation between SES variables and callback rates for both races or that the correlation is more positive for applications with distinctively white names. These results suggest that Black names are not helped more by living in areas with high SES, implying that Black names are not primarily signaling social status.

Finally, we investigate whether controlling for our SES measures affects our main results' conclusions. Table B2 shows estimates of our main effects. Columns (1) through (3) display the overall effect of the applicant's race on callbacks, adding either our SES variables or high school and census tract fixed effects. Columns (4) through (6) display the effects of the minimum wage, adding these same variables. If distinctively Black names primarily signal socioeconomic status, we would expect that adding these variables would lessen the relationship between the race coefficients and callbacks. We find no evidence that this is the case, providing more evidence that names in our study signal race rather than socioeconomic status.

Table B2: Callback Rate and Socioeconomic Status

	(1) Callback	(2) Callback	(3) Callback	(4) Callback	(5) Callback	(6) Callback
Black	-1.39***	-1.44***	-1.09**	-3.21***	-3.23***	-3.16***
	(0.27)	(0.28)	(0.36)	(0.61)	(0.62)	(0.78)
After Announcement				-9.09***	-8.99***	-8.57***
				(0.82)	(0.83)	(0.89)
Black × After Announce				2.58***	2.52***	2.74**
				(0.68)	(0.68)	(0.86)
Unemp 12 mo.	-0.44	-0.47	-0.15	-0.46	-0.51	-0.21
	(0.36)	(0.37)	(0.47)	(0.36)	(0.37)	(0.47)
GED	-0.46	1.78*	0.00	-0.49	1.16	0.00
	(0.32)	(0.82)	(.)	(0.31)	(0.81)	(.)
Constant	11.43***	15.70***	10.91***	17.91***	22.89***	17.09***
	(0.36)	(2.00)	(0.36)	(0.74)	(2.12)	(0.79)
SES Controls	NO	YES	NO	NO	YES	NO
SES Fixed Effects	NO	NO	YES	NO	NO	YES
R-squared	0.011	0.011	0.020	0.018	0.018	0.026
Observations	34,986	34,986	34,986	34,986	34,986	34,986

Note: This table displays estimates of the effects of resume characteristics and minimum wage changes on the callback rate. SES Controls refers to whether our set of SES controls, presented in Table B1 are included in the regression. SES Fixed Effects refers to whether resumes include either a GED or one of thirty-five different randomized high schools along with fixed effects for the census tract of the address randomly assigned to the applicant. There are 146 different census tracts in the sample. All specifications include city fixed effects and controls for applicant age. Standard errors clustered by establishment. *** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05.

C. Sample Characteristics and Treatment Balance

In this section, we describe the sample characteristics and present statistics about treatment balance. Column (1) of Table C1 shows the average characteristics for the firms in our sample across all applications. The table is broken down into four panels. Panel (a) displays information about whether job hours are part time, full time, or unstated along with whether the job posts an hourly wage and the estimated hourly wage conditional on posting it. Because some jobs state that both full and part time jobs are available, these categories are not mutually exclusive. Panel (b) displays the job skills requested in the job ad, calculated using the procedure from Spitz-Oener (2006). Job tasks refers to the types of tasks requested in the application text using the procedure from Atalay et al. (2020). Finally, the job titles from the ad are also shown in panel (d).

Overall, about 36% of jobs post a wage. We believe that the majority of these jobs pay minimum wage and choose not to post it because that is not a selling point of the job. Of those that post it, the mean wage is $13 an hour. The median wage in our sample is lower, $10. This value should be interpreted with caution because we have to estimate the hourly wage for many jobs based on the stated weekly, monthly, or annual salary and the expected wages.

Columns (2) and (3) of Table C1 show the average characteristics across the race of the applicant. All characteristics shown are determined pre-treatment and should not be affected by the treatment assignment. There is variation in the means because only some firms receive one resume of each type (see Section 2.2). Column (3) reports p-values for the null hypothesis that the average characteristics are equal across the two treatment categories. Consistent with successful random assignment, the observable characteristics are balanced across treatment groups.

In Table C2, we present an alternative version of the randomization balance test, breaking down the sample by the resume type. This type is defined by the race, unemployment duration, and educational attainment. Similar to Table C1, this shows that overall the observable characteristics of advertisements are balanced across treatments. In the two instances where we can detect differences in means across resume types, the differences are not economically meaningful.

Table C1: Race Treatment Balance Across the Whole Sample Period

		Treatment Arm		
	All	Black Applicant	White Applicant	p-value test
	(1)	(2)	(3)	(4)
a. Job Hours and Pay:				
Part Time Job (%)	34.831	34.703	34.960	0.478
	(0.255)	(0.359)	(0.361)	
Full Time Job (%)	40.916	40.914	40.919	0.990
	(0.263)	(0.371)	(0.372)	
Job Hours Unstated (%)	38.120	38.230	38.011	0.560
	(0.260)	(0.367)	(0.368)	
Has Posted Wage (%)	36.669	36.566	36.772	0.580
	(0.258)	(0.364)	(0.365)	
Estimated Hourly Wage (Dollars)	13.010	13.050	12.971	0.433
	(0.071)	(0.101)	(0.100)	
b. Job Skills:				
Social Skills Demanded (%)	57.795	57.946	57.642	0.422
	(0.264)	(0.373)	(0.374)	
Customer Skills Demanded (%)	78.443	78.170	78.718	0.087
	(0.220)	(0.312)	(0.310)	
Character Skills Demanded (%)	44.724	44.800	44.646	0.689
	(0.266)	(0.375)	(0.376)	
Other Demanded Skill (%)	12.476	12.474	12.479	0.983
	(0.177)	(0.249)	(0.250)	
c. Job Tasks:				
Non-Routine Manual (%)	73.724	73.543	73.906	0.283
	(0.235)	(0.333)	(0.333)	
Routine Manual (%)	51.146	51.211	51.081	0.736
	(0.267)	(0.377)	(0.379)	
Other Task (%)	18.035	18.041	18.031	0.972
	(0.206)	(0.290)	(0.291)	
d. Job Title on Ad:				
Cashier (%)	3.587	3.550	3.624	0.603
	(0.099)	(0.140)	(0.142)	
Food Service (%)	6.777	6.678	6.876	0.304
	(0.134)	(0.188)	(0.192)	
Janitor (%)	8.223	8.114	8.333	0.283
	(0.147)	(0.206)	(0.209)	
Team Member (%)	5.948	5.983	5.913	0.694
	(0.126)	(0.179)	(0.179)	
Other (%)	21.737	21.614	21.856	0.435
	(0.221)	(0.311)	(0.313)	
Observations	34,987	17,549	17,437	

Note: This table lists the averages of firm characteristics across the full sample period. Standard errors are reported in parentheses. The statistics in panel (a) are based on the job text obtained from the advertisement. Posted hourly wages are estimated using the information about the salary and the number of hours worked. Estimated hourly wages are based on 12,829 observations rather than the full sample because not every job ad includes a wage. The statistics in panel (b) are predicted skills from the advertisement text using the procedure from Spitz-Oener (2006) These categories are not mutually exclusive. The statistics in panel (c) are predicted tasks from the advertisement text using the procedure from Atalay et al. (2020). These categories are not mutually exclusive. The statistics in panel (d) are predicted from the advertisement text using keywords. The categories are not mutually exclusive. Column (1) is based on the entire subject pool. Columns (2) through (3) are based on the firms selected to receive resumes of a given type. Column (3) reports the p-value of a test of equal means across the resume types. Standard errors are clustered at the establishment level.

Table C2: Treatment Balance Across the Whole Sample Period

		Treatment Arm								
	All	B01GED	B01HS	B12GED	B12HS	W01GS	W01GED	W12GED	W12HS	p-value test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Job Hours and Pay:										
Part Time Job (%)	34.831	34.586	35.130	34.516	34.578	34.908	35.142	34.413	35.371	0.947
	(0.255)	(0.718)	(0.720)	(0.717)	(0.720)	(0.718)	(0.729)	(0.723)	(0.720)	
Full Time Job (%)	40.916	40.123	40.791	41.405	41.338	41.949	40.522	41.073	40.127	0.584
	(0.263)	(0.740)	(0.741)	(0.743)	(0.746)	(0.744)	(0.749)	(0.748)	(0.738)	
Job Hours Unstated (%)	38.120	39.143	38.631	37.381	37.764	37.315	38.216	37.882	38.632	0.471
	(0.260)	(0.737)	(0.734)	(0.730)	(0.734)	(0.729)	(0.742)	(0.738)	(0.733)	
Has Posted Wage (%)	36.669	35.543	36.266	36.880	37.580	36.498	37.611	37.049	35.960	0.306
	(0.258)	(0.723)	(0.725)	(0.728)	(0.733)	(0.726)	(0.739)	(0.735)	(0.722)	
Estimated Hourly Wage (Dollars)	13.010	13.150	12.758	13.108	13.179	12.970	13.162	13.008	12.742	0.508
	(0.071)	(0.234)	(0.191)	(0.205)	(0.180)	(0.190)	(0.201)	(0.215)	(0.194)	
b. Job Skills:										
Social Skills Demanded (%)	57.795	57.599	58.140	58.549	57.493	58.074	57.080	58.326	57.088	0.721
	(0.264)	(0.746)	(0.744)	(0.743)	(0.748)	(0.744)	(0.755)	(0.750)	(0.745)	
Customer Skills Demanded (%)	78.443	79.426	77.558	78.081	77.612	78.696	78.854	77.729	79.574	0.041
	(0.220)	(0.610)	(0.629)	(0.624)	(0.631)	(0.617)	(0.623)	(0.633)	(0.607)	
Character Skills Demanded (%)	44.724	45.409	45.339	44.088	44.363	44.333	44.853	44.889	44.520	0.785
	(0.266)	(0.752)	(0.751)	(0.749)	(0.752)	(0.749)	(0.759)	(0.756)	(0.748)	
Other Demanded Skill (%)	12.476	11.643	12.847	12.437	12.970	12.423	12.599	12.673	12.228	0.595
	(0.177)	(0.484)	(0.505)	(0.498)	(0.509)	(0.497)	(0.506)	(0.506)	(0.493)	
c. Job Tasks:										
Non-Routine Manual (%)	73.724	73.935	72.715	73.761	73.763	74.040	73.614	73.474	74.479	0.650
	(0.235)	(0.663)	(0.672)	(0.663)	(0.666)	(0.661)	(0.673)	(0.671)	(0.656)	
Routine Manual (%)	51.146	51.629	50.864	51.205	51.146	50.943	50.908	50.486	51.970	0.894
	(0.267)	(0.754)	(0.754)	(0.754)	(0.757)	(0.753)	(0.763)	(0.760)	(0.752)	
Other Task (%)	18.035	17.635	18.736	18.031	17.759	17.670	18.444	18.571	17.459	0.638
	(0.206)	(0.575)	(0.588)	(0.580)	(0.579)	(0.575)	(0.592)	(0.591)	(0.571)	
d. Job Title on Ad:										
Cashier (%)	3.587	3.828	3.774	3.251	3.346	3.543	3.377	3.446	4.121	0.342
	(0.099)	(0.290)	(0.287)	(0.267)	(0.272)	(0.279)	(0.276)	(0.277)	(0.299)	
Food Service (%)	6.777	6.699	6.594	6.799	6.622	6.586	7.243	6.892	6.793	0.905
	(0.134)	(0.377)	(0.374)	(0.380)	(0.376)	(0.374)	(0.396)	(0.385)	(0.379)	
Janitor (%)	8.223	8.020	7.844	8.231	8.364	8.290	8.570	8.603	7.880	0.745
	(0.147)	(0.410)	(0.405)	(0.414)	(0.419)	(0.416)	(0.427)	(0.426)	(0.405)	
Team Member (%)	5.948	6.266	6.048	6.139	5.477	5.792	5.659	5.897	6.295	0.630
	(0.126)	(0.366)	(0.359)	(0.362)	(0.344)	(0.352)	(0.353)	(0.358)	(0.366)	
Other (%)	21.737	21.668	21.714	21.919	21.150	21.349	22.427	21.924	21.739	0.862
	(0.221)	(0.622)	(0.622)	(0.624)	(0.618)	(0.618)	(0.637)	(0.629)	(0.621)	
Observations	34,987	4,389	4,398	4,398	4,364	4,403	4,294	4,324	4,416	

Note: This table lists the averages of firm characteristics across the full sample period. Standard errors are reported in parentheses. The statistics in panel (a) are based on the job text obtained from the advertisement. Posted hourly wages are estimated using the information about the salary and the number of hours worked. Estimated hourly wages are based on 12,829 observations rather than the full sample because not every job ad includes a wage. The statistics in panel (b) are predicted skills from the advertisement text using the procedure from Spitz-Oener (2006) These categories are not mutually exclusive. The statistics in panel (c) are predicted tasks from the advertisement text using the procedure from Atalay et al. (2020). These categories are not mutually exclusive. The statistics in panel (d) are predicted from the advertisement text using keywords. The categories are not mutually exclusive. Column (1) is based on the entire subject pool. Columns (2) through (9) are based on the firms selected to receive resumes of a given type. Column (9) reports the p-value of a test of equal means across the resume types. Standard errors are clustered at the establishment level.

D. Robustness to Variation in Measurement of Treatment Timing

In our preferred specifications, we assign each application to the minimum wage policy at the date the application is submitted. Since most applications do not receive any contact from the potential employer, this date is the only point of reference we observe as researchers. However, significants delay between when the application is received and when it is reviewed will lead us to misclassify the treatment of interest. In particular, since applications cannot be reviewed before we apply, we will label some "treated" observations that are reviewed after the minimum wage increase as "untreated" since we applied before the change.

We test whether potential misclassification affects our results. We assign treatment, or the minimum wage policy, based on the date of the earliest contact we receive for a given job ad, which is not necessarily a callback. For example, if we sent applications to a job ad before the minimum wage is announced, but then one applicant is called back after the minimum wage is enacted and the other is never contacted, we code the time period as "Enacted" for both applications. The date is not defined for job ads where no applicants are contacted and they are therefore excluded.

Column (1) of Table C3 presents the results with this alternative measurement of treatment timing and shows that the main result still holds– minimum wages shrink the racial callback gap. However, the magnitudes are much larger. This is because we are conditioning on receiving some contact from the employer, which are primarily callbacks and callbacks to white applicants. In the full sample, many employers call back neither of our applicants, and so appear to not discriminate in the data. Since this new definition of treatment timing meaningfully changes the sample, we additionally estimate our preferred specification that defines treatment by application date using this restricted sample. The results are again similar.

Table C3: Alternative Measurement of Treatment Timing

	Callback	
	(1)	(2)
Treatment Assignment By:	Callback Date	Application Date
Black	-8.33***	-7.82***
	(1.57)	(1.61)
Announced	-7.75**	-8.69***
	(2.85)	(2.59)
Enacted	-9.96***	-13.48***
	(2.11)	(2.05)
Black × Announced	7.30*	4.40
	(2.92)	(2.94)
Black × Enacted	4.29*	4.05+
	(2.07)	(2.09)
N	8,700	8,700
R^2	0.02	0.02

Note: Both specifications include city fixed effects and controls for applicant age. Standard errors are clustered by establishment. *** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05, + p-value $< .1$

E. Alternative Assumptions on the Productivity Distributions by Race

In the model in Section 3 and our empirical application in Section 5, we focus on the case where employers perceive that applicant productivity follows a normal distribution. The normal distribution assumption is a useful case because it allows us to consider the role of three types of discrimination that are often discussed in the literature— taste-based, statistical in means, and statistical in variance. However, it is a particular functional form assumption that may not hold. The equation for the change in the RCG with respect to the minimum wage also implies that other distributions with similar densities, like the logistic distribution, will lead to similar qualitative conclusions.

We now assume that employers perceive that applicant productivity, Q_i, is uniformly distributed. Specifically, $Q_i|(B=0) \sim U(a_W, b_W)$ and $Q_i|(B=1) \sim U(a_B, b_B)$. Therefore, the likelihood of receiving a callback may differ for white and Black applicants because employers perceive the supports of the two distributions to differ in addition to a discriminatory penalty, d, for Black applicants. Under this assumption:

$$RCG = \frac{b_W - v(m)}{b_W - a_W} - \frac{b_B - (v(m) - d)}{b_B - a_B} \tag{11}$$

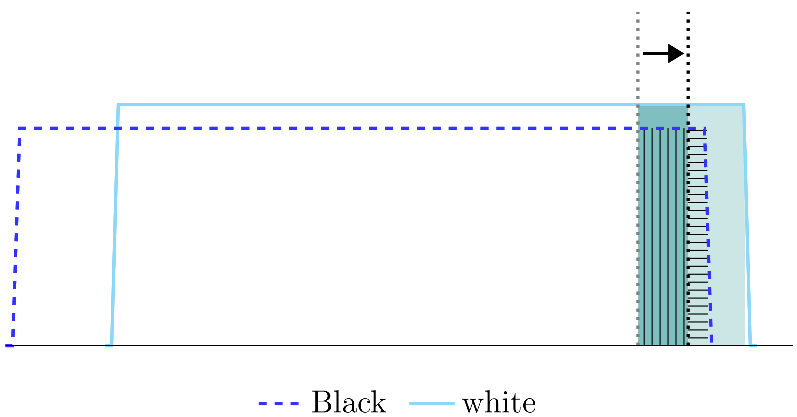
In this case, the RCG may be positive because employers believe that there are more high productivity white applicants, for example because b_W is larger than b_B, or because of the discriminatory penalty. Based on this expression, the change in the RCG from a minimum wage increase is:

$$\frac{\partial RCG}{\partial m} = v'(m) \times \left[\frac{1}{b_W - a_W} - \frac{1}{b_B - a_B}\right] \tag{12}$$

$$= v'(m) \times \sqrt{12}\left[\frac{1}{\sigma_W} - \frac{1}{\sigma_B}\right] \tag{13}$$

where σ is the standard deviation of $Q|B$. While taste-based and statistical discrimination both determine the RCG, only statistical discrimination determines how the gap changes with a minimum wage increase. Using our estimates from the correspondence study, we plot the implied productivity distributions for Black and white applicants in Figure E1 to again show how the perceived variance, or the length and height of the pdf, leads white applicants to be more negatively affected by minimum wage increases. Since the length of the pdf for white applicants is shorter, and therefore the height is larger, a relatively larger share of the marginal applicants are white.

Figure E1: Minimum Wages and Callbacks when Productivity is Uniformly Distributed



F. Effects of Minimum Wages on Education and Duration Unemployed Gaps

We extend Equation 4 to consider whether minimum wage increases affect the callback gaps between GED and high school graduates as well as 12 month and 1 month unemployed applicants. We find no evidence that the minimum wage increases meaningfully affect the callback gaps by education and duration unemployed. Including these additional terms do not affect our estimates for the change in the racial callback gap.

Table F1: The Minimum Wage and Callback Gaps

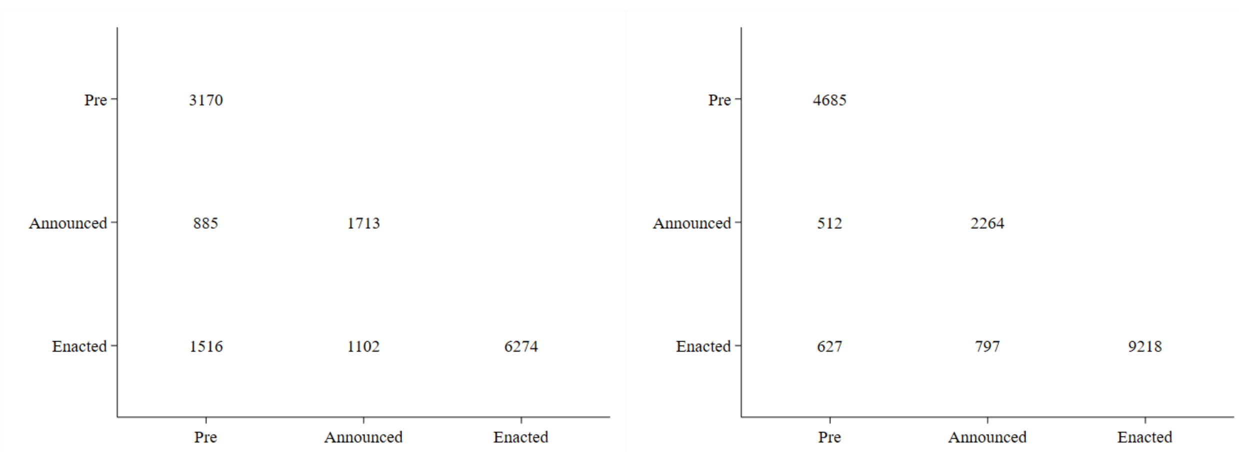
	Callback	
Black	-0.032***	-0.029***
	(0.006)	(0.01)
GED	-0.007	-0.008
	(0.007)	(0.008)
Unemployed 12 months	-0.010	-0.013+
	(0.007)	(0.01)
After Announced	-0.097***	-0.071***
	(0.010)	(0.012)
Black × After Announced	0.026***	0.021**
	(0.006)	(0.007)
GED × After Announce	0.004	0.005
	(0.008)	(0.009)
Unemployed 12 months × After Announce	0.008	0.006
	(0.007)	(0.008)
Firm Fixed Effects	No	Yes
N	34,990	34,990
R^2	0.02	0.36

Note: Both specifications include city fixed effects and controls for applicant age. Standard errors are clustered by establishment. *** p-value < 0.001, ** p-value < 0.01, * p-value < 0.05, + p-value $< .1$

G. Robustness of Firm Composition Results across Alternative Specifications

We consider alternative specifications to estimate the change in the racial callback gap after a minimum wage increase using within firm and within establishment variation. We first present the number of firms and establishments posting across periods in Panels (a) and (b) of Figure G1, respectively. The diagonal elements give the number of unique firms or establishments in our sample in a period. For example, we applied to 1,713 unique firms between the announcement and enactment of the increase. Off-diagonal elements give the number of unique firms or establishments that we applied to across two periods. For example, there are 1,516 unique firms where we applied to a posting both before the minimum wage was announced and after it was enacted. These 1,516 are a subset of the 6,274 unique firms in the Enacted period and the 3,170 in the pre period. The matrix is symmetric and so we only present the lower triangle. Using the variation shown in this figure, we estimate specifications similar to Equation 4 that include firm and establishment fixed effects and present the results in Figure G2.

Figure G1: Firm and Establishment Postings Over Time

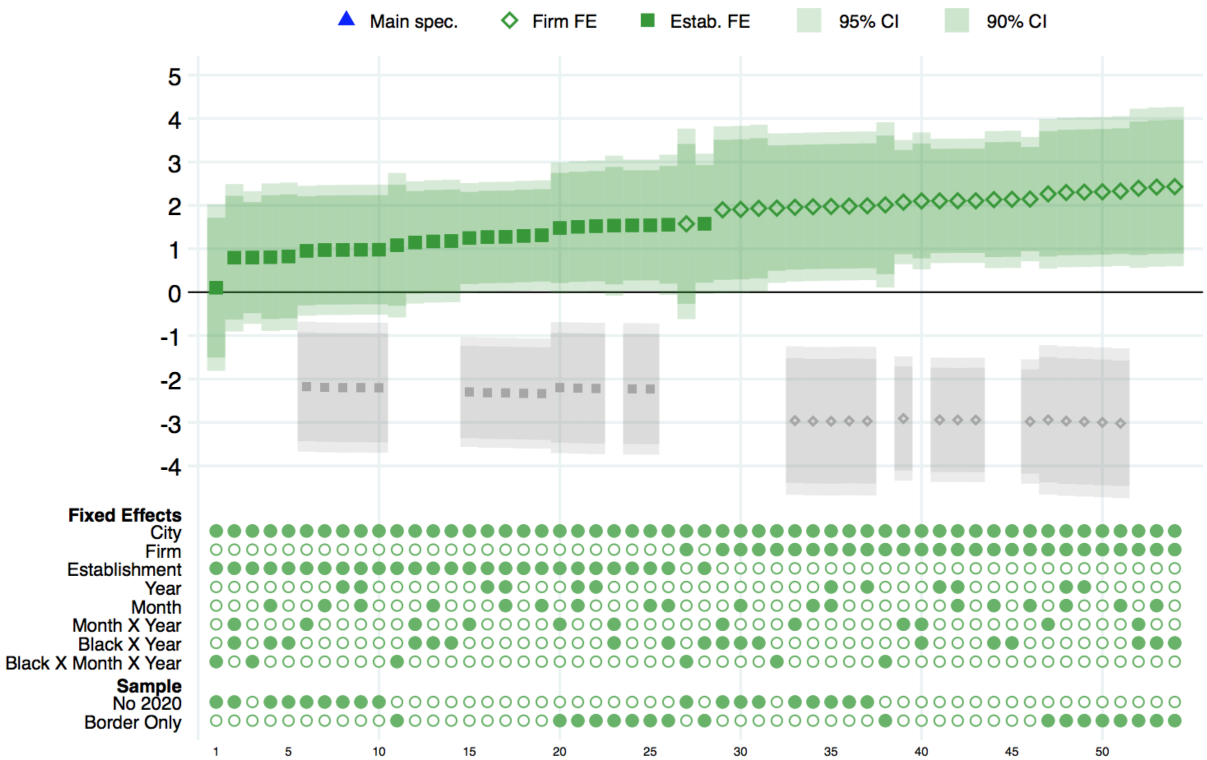


(a) Firms

(b) Establishments

Note: Elements of Panel (a) show variation across time in postings by firms in our sample. Diagonal elements give the number of unique firms in a given period. Off-diagonal elements give the number of unique firms that appear in both periods. Panel (b) similarly presents the number of unique establishments posting across periods. The matrix is symmetric and so we present the lower triangle.

Figure G2: Specification Curve for the Role of Firm Composition



Note: Each specification considers an alternative set of fixed effects or limits the sample to test the robustness of the effect of minimum wage increases on the racial callback gap. All specifications include either firm or establishment fixed effects as well as applicant age and city fixed effects. Standard errors are clustered by establishment.

H. Additional Changes in Characteristics Listed on Job Ads

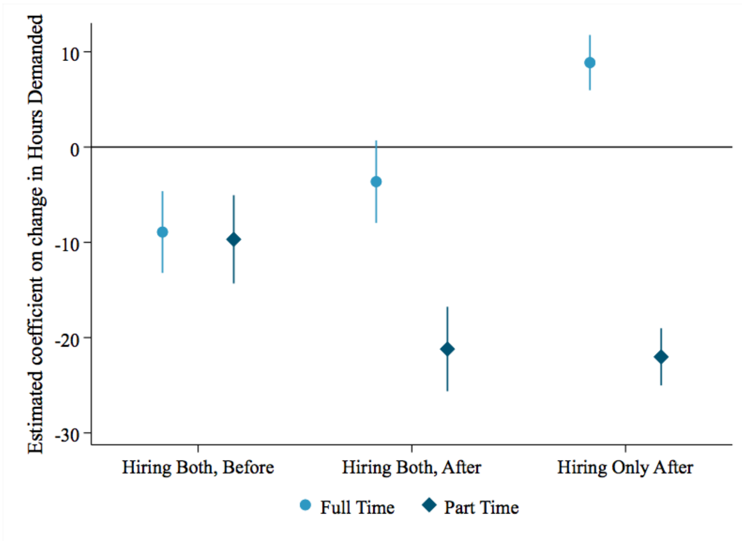
As in Section 6.2, we estimate the likelihood that a job posting from an establishment lists certain job or applicant characteristics, relative to those who only hire before the announcement. We consider whether the ads lists full vs part time employment and type of tasks required on the job based on the text of their job ads based on Spitz-Oener (2006), and Atalay et al. (2020).

Based on the categorized information in the job ads, we first estimate whether establishments shift towards or away from full-time positions. We estimate two versions of Equation 8 with full-time and part-time as the dependent variables. The β_1 coefficients on "Hiring Both, Before" show that, before the minimum wage increase is announced, ads by establishments who post in both periods contain less information about the hours demanded than those that only hire before the increase. Since β_1 is significantly different from 0, the results suggest these establishment may be different.

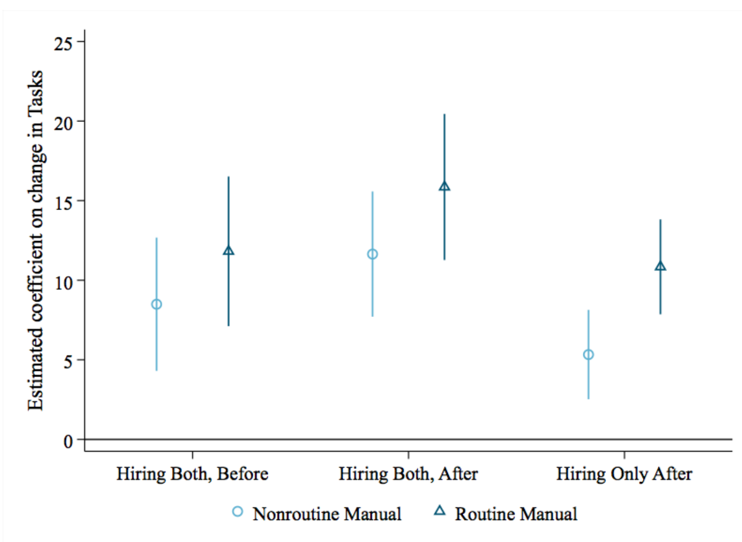
After the minimum wage announcements, establishments that hire in both periods change the hours demanded after the minimum wage increase to post more full-time ($p = 0.010$) and fewer part-time jobs ($p < 0.001$), based on the comparison of β_2 and β_1 shown in Figure H2a. We see a similar pattern when comparing those that only hire after to those that only hire before. Overall, these results suggest that establishments respond to the minimum wage conditional on hiring, and that the composition may change as well.

In Figure H2b, we similarly consider how the minimum wage affected the tasks applicants would perform on the job and provide evidence that the minimum wage affects who hires and behavior among hiring establishments. Before the increase, those who only hire before the announcement are less likely to list information about the type of task than those who hire in both period. Then, after the increase, establishments that hire in both periods become slightly more likely to list both nonroutine and routine manual tasks in their ads ($p < 0.10$).

Figure H2: Minimum Wages and Job Ad Content



(a) Work Hours Demanded



(b) Likelihood Task Listed in Job Ad

Note: Panel (a) plots the likelihood the ad list full or part time employment opportunities relative to establishments that only post before the minimum wage increase is announced, and the 95% confidence intervals. Panel (b) estimates for the likelihood the ad contains a given task relative to establishments that only post before the minimum wage increase is announced. All specifications control for city FE. Standard errors are clustered by establishment.

I. Multiple Hypothesis Testing

In this section, we will establish that the results in the paper are not false positives resulting from not adjusting the significance value of our hypothesis tests in a manner that reflects the multiple comparisons (List et al., 2019, 2021). We do this by testing families of null hypotheses from the body of this paper using the procedure from Westfall and Young (1993). This procedure, controls for the family-wise error rate and allows for the dependence amongst p-values within a hypothesis.[44] We control for the family-wise error rate within families, but not across families.

We follow Rubin (2021) and correct for multiple comparisons when performing disjunction testing, but not when performing conjunction or individual testing.[45] Tables I1 and I2 below display reproductions of the coefficient estimates and p-values from the main body of the paper along with the Westfall and Young (1993) adjusted p-values. For each hypothesis test, we include the exhibit reference and our family designation. Hypotheses not included in this table were determined to either by conjunction or independent tests.

Table I1 shows that none of the conclusions from these analyses change when correcting for multiple hypothesis testing. Family 3 conducts multiple hypothesis testing on the hypotheses from Table F1, which are a superset of those presented in Figure 3 and help us rule out the race effects as the result of a multiple hypothesis problem involving testing the change in three different characteristics with the minimum wage. While the main results of the paper are robust to this correction, there are a few instances where we lose enough power from using the family-wise approach that we can no longer detect differences from zero in Family 2 (see Figure 2b). We can no longer detect differences between the highest quality

[44]This procedure is similar to the Romano and Wolf (2016), List et al. (2019) and List et al. (2021) procedures. However, it has an additional assumption of set pivotality, which is not assumed by the the three proceeding procedures. We use this procedure because it can account for clustered standard errors and fixed effects, which are key to our design.

[45]Rubin (2021) refers to disjunction testing as tests that require *at least one* hypothesis to return a significant result before rejecting a joint null hypothesis. The *conjunction testing* approach requires all statistical tests to be significant as a decision rule. The individual testing approach is one where the researchers only make decisions about each constituent null hypothesis separately.

Black resume and the highest quality white resume (one-month unemployment duration and high school education). We can also no longer detect differences between resumes from a white applicant with a 12-month unemployment duration and a GED and the highest quality white resume. Despite these differences, the conclusion from this family that the treatment effects found in Figure 2a are driven by race does not change.

Table I2 also shows that none of the conclusions from Figures 7a or 7b are changed when using a family-wise error approach.

Table I1: P-value Corrections for Multiple Hypothesis Testing on Disjunctive Hypotheses: Part I

	Exhibit	**Family**	**Outcome**	**Variable**	**Coeff.**	**p-values**	
						Unadjusted	Westfall and Young (1993)
(1)	Figure 2a	1	Callback	Black	-1.39	< 0.001	< 0.001
(2)	Figure 2a	1	Callback	GED	-0.44	0.229	0.278
(3)	Figure 2a	1	Callback	Unemp 12 mo.	-0.46	0.149	0.278
(4)	Figure 2b	2	Callback	B01GED	-2.06	< 0.001	0.002
(5)	Figure 2b	2	Callback	B01HS	-1.37	0.031	0.105
(6)	Figure 2b	2	Callback	B12GED	-1.90	0.004	0.012
(7)	Figure 2b	2	Callback	B12HS	-2.07	0.001	0.004
(8)	Figure 2b	2	Callback	W01GED	-0.29	0.659	0.883
(9)	Figure 2b	2	Callback	W12GED	-1.27	0.062	0.170
(10)	Figure 2b	2	Callback	W12HS	-0.27	0.695	0.883
(11)	Figure 3 (Table F1)	3	Callback	Black	-3.22	< 0.001	< 0.001
(12)	Figure 3 (Table F1)	3	Callback	GED	-0.74	0.298	0.462
(13)	Figure 3 (Table F1)	3	Callback	Unemp 12 mo.	-1.04	0.119	0.309
(14)	Figure 3 (Table F1)	3	Callback	Post	-9.68	< 0.001	< 0.001
(15)	Figure 3 (Table F1)	3	Callback	Black $\times$ Post	2.58	< 0.001	0.001
(16)	Figure 3 (Table F1)	3	Callback	GED $\times$ Post	0.36	0.647	0.645
(17)	Figure 3 (Table F1)	3	Callback	Unemp 12 mo. $\times$ Post	0.82	0.238	0.462
(18)	Table 3, Column (2)	4	Callback	Black	-2.94	< 0.001	< 0.001
(19)	Table 3, Column (2)	4	Callback	Post	-8.50	< 0.001	< 0.001
(20)	Table 3, Column (2)	4	Callback	Black $\times$ Post	1.30	0.045	0.034
(21)	Table 3, Column (3)	5	Callback	Black	-2.33	< 0.001	< 0.001
(22)	Table 3, Column (3)	5	Callback	Post	-8.50	< 0.001	< 0.001
(23)	Table 3, Column (3)	5	Callback	Black $\times$ Post	1.30	0.045	0.034

Notes: This table displays the coefficients, p-values, and Westfall and Young (1993) adjusted p-values for different analysis done in the main body of the paper. Family refers to the set of hypothesis for which the family-wise error rate is controlled. The Variable column refers to the variable for which we estimate the coefficient and conduct the hypothesis test. Post refers to the indicator equal to 1 if the period if after a minimum wage hike announcement. All standard errors are clustered at the establishment level and all Westfall and Young (1993) adjusted hypothesis tests use 999 bootstrap iterations.

Table I2: P-value Corrections for Multiple Hypothesis Testing on Disjunctive Hypotheses: Part II

	Exhibit	**Family**	**Outcome**	**Variable**	**Coeff.**	**p-values**	
						Unadjusted	Westfall and Young (1993)
(24)	Figure 7a	6	Callback	B01HS × Post	1.47	0.348	0.159
(25)	Figure 7a	6	Callback	B12HS × Post	4.79	0.002	< 0.001
(26)	Figure 7a	6	Callback	B01GED × Post	4.79	0.002	< 0.001
(27)	Figure 7a	6	Callback	B12GED × × Post	2.24	0.136	0.087
(28)	Figure 7b	7	Callback	Black × No Relevant Exp. *times* Post	2.31	0.005	< 0.001
(29)	Figure 7b	7	Callback	Black × Relevant Exp. *times* Post	3.23	0.012	0.001

Notes: This table displays the coefficients, p-values, and Westfall and Young (1993) adjusted p-values for different analysis done in the main body of the paper. Family refers to the set of hypothesis for which the family-wise error rate is controlled. The Variable column refers to the variable for which we estimate the coefficient and conduct the hypothesis test. Post refers to the indicator equal to 1 if the period if after a minimum wage hike announcement. All standard errors are clustered at the establishment level and all Westfall and Young (1993) adjusted hypothesis tests use 999 bootstrap iterations.

J. Pre-Specified Analysis and Deviations from Pre-Analysis Plan

We submitted our pre-analysis plan (PAP) on September 18, 2018, when we started collecting data for the experiment. The Pre-analysis plan "An audit study on minimum wage legislation" can be found at the AEA Registry under ID number AEARCTR-0003333. We amended the pre-analysis plan twice. Once to add the Illinois and Kansas sample when both Missouri and Arkansas minimum wage referenda passed. The second time was to prolong the sample period because we had not yet reached our desired sample size. Similar to most studies in economics, we deviating from our pre-analysis plan in a few ways.[46]

The original PAP specified our main results, presented in Figure 2a and a version of Equation 4 that had no enactment effects, Illinois, Kansas, and allowed the unemployment duration effect to change with the announcement. While we deviate from the PAP, these deviations do not affect our results or conclusions. Figure 5 shows that our results are robust to changing the sample period. Figure 4 shows that the effects are concentrated around the announcement period, so allowing for enactment effects does not change our results. Similarly, Table F1 shows that allowing the effect of the unemployment duration to change with the minimum wage announcement does not affect the results.

The paper's results are identical to those from the pre-analysis plan, but the expanded sample has precise estimates. Finally, the analysis of the mechanisms and robustness checks in the appendix were not pre-registered. We include thee anlayses in the paper to help establish the credibility of the main results and help us understand how to interpret the effects.

[46]See Abrams et al. (2020) for a discussion of posting and adhering to pre-analysis plans in economics.

K. Ethics Appendix

In this section, we describe the ethical considerations of the experiment. We first note that we underwent ethical review at the Human Subjects Committee at the University of Chicago (IRB18-0873), which played an important role in ensuring that the experiment upheld high ethical standards despite our decision not to consent subjects (List, 2009). Next, we follow the framework of Asiedu et al. (2021).

1. **Equipoise:** In our experiment, each of our resumes is similar to the others by design. Therefore, we do not expect that any treatment arm clearly dominates another treatment arm from the perspective of the employer. The resumes that are least likely to elicit a response may be better from the hiring manager's view because she will not spend time calling the fictitious applicant for an interview. However, we believe that this benefit is small. Moreover, past research on correspondence studies suggests meaningful uncertainty about the relative likelihood of callbacks from each treatment arm (Neumark and Rich, 2019). That said, the subjects in our experiment would be better off in the status quo world of no resumes. However, learning about discrimination due to taste-based or statistical motivations is not otherwise feasible. We believe, and the IRB agreed, that the benefits from the knowledge outweigh the small costs to the employer.

2. **Role of the Researchers with Respect to Implementation:** The researchers had direct decision-making power over whether and how to implement the experiment. We did not disclose the experiment to the participants before they received a resume.

3. **Potential Harms to Research Participants from the Interventions:** The experimental design potentially harms our subjects. Employers' time is scarce, and we are having them spend it reviewing applications that are fictitious without obtaining the involved parties' consent or compensating employers for their time. Moreover,

Bertrand and Duflo (2017) notes that when an applicant declines an offer, employers may learn that applicants with similar attributes are unlikely to accept offers. They claim that this may lead to employers being less likely to offer jobs to candidates that share those attributes in the future. They also note that after receiving rejections from candidates, the employers may believe that the market is tighter than previously expected, which would be beneficial for real candidates, but detrimental for employers.

While our experiment potentially harms our subjects, identifying racial discrimination from taste-based or statistical motives requires randomly assigning race, which is only available in a field experiment with fictitious applicants. Both of these mechanisms ended up being important drivers of the treatment effects in our study.[47] So, we believe that the benefits to society outweigh the harms to the subject. Moreover, obtaining consent for the experiment would likely bias our estimates against finding discrimination as employers may change their behavior if they knew that they were participating in a study (Levitt and List, 2007).

Moreover, we designed our experiment to limit the potential harm to participants. To the best of our ability, we sent establishments no more than two resumes in each wave. This helped us minimize the burden on firms. Kessler et al. (2019) estimate that it takes employers about thirty minutes to review ten revenues. This finding suggests that managers spent about six minutes on our experiment in each wave. Second, because few resumes are sent to each firm, we cannot determine whether an individual firm is discriminating. Finally, we never record the identities of the hiring managers at a firm.

4. **Potential Harms to Research Participants from Data Collection or Research Protocols:** We do not believe subjects experience any harm from data collection. Calling back a subject is no different from the firm's actions in everyday life. The firm's responses were anonymized so that no individual could link a particular firm's

[47] Kessler et al. (2019) have a method of detecting implicit discrimination against subjects without using deception. However, this method cannot measure discrimination from statistical or taste-based motives.

callback decisions to the hiring manager's identification.

5. **Financial and Reputational Conflicts of Interest:** Brandon, Holz, Simon, Uchida, or any of the research assistants did not receive any form of financial compensation as part of the study. The research questions pursued in this study are novel and different from prior work conducted by the PIs. We perceive no reputational conflicts of interest.

6. **Intellectual Freedom:** This study was conducted without collaborating with organizations. The study was conceived and designed by the PIs, who maintained intellectual freedom throughout all stages of the project. At no point did an outside partner have undue influence on the analysis or the interpretation of the results.

7. **Feedback to Participants and Communities:** We intend to share our results with policymakers after our work is subject to peer review.

8. **Foreseeable Misuse of Research Results:** We recognize that the results are relevant for public policy in labor markets. We advise policymakers to acknowledge that only one of several potential relevant outcomes is studied in our setting. While our paper concludes that increases in the minimum wage reduce racial disparities in hiring decisions, we cannot speak about racial disparities in other labor market outcomes that may be relevant for optimal policy. Future research should investigate whether the minimum wage reduces turnover, separations, and workplace amenities, and policymakers should acknowledge the uncertainty around the effects on these outcomes when making policy decisions.