

Do Low-Wage Employers Discriminate Against Applicants with Long Commutes? Evidence from a Correspondence Experiment

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Abstract

I use a correspondence study of the low-wage labor market in Washington, DC to test whether employers discriminate against applicants who live farther from the job location. Fictional résumés randomly assigned to have addresses far from the job location receive 14% fewer callbacks than those with addresses in nearby but similarly affluent neighborhoods. Living 5-6 miles away from the job results in a penalty equal to that received by applicants with stereotypically black names. On the other hand, holding commute distance constant, I find no statistical evidence that employers respond to a neighborhood's affluence.

JEL Codes: J7; R2; J6

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1. Introduction

Does residential location affect success in low-wage labor markets? The urban poor tend to be concentrated in a small number of neighborhoods (Kneebone, 2014). A large literature argues that living in poor neighborhoods hampers labor market success. People moved to the suburbs by the Gautreaux program in Chicago were employed at higher rates than those moving within the city (Rosenbaum, 1995). However, adults provided housing vouchers for low poverty neighborhoods in the Moving to Opportunity (MTO) experiment do not become more economically self-sufficient (Kling et. al. 2007; Ludwig, et. al. 2012).² Many researchers interpret the MTO results as evidence that residential location does not drive labor market success. Alternative interpretations exist for why MTO did not stimulate employment (Quigley et. al., 2008; Galiani, et. al. 2015; Aliprantis, 2017; Chyn, 2017). One alternative focuses on the importance of physical distance. Proponents of the “spatial mismatch hypothesis” (Kain, 1968; Wilson, 1997) argue that distance between poor neighborhoods and jobs limits employment opportunities. This spatial interpretation posits that physical distance drives neighborhood effects, and policies addressing concentrated poverty need to prioritize proximity to employment rather than more affluent neighbors.

I conduct a correspondence experiment in the low-wage labor market in Washington, DC testing whether employers respond differently to distant versus nearby applicants. A research team sends 2,260 fictional résumés to actual job vacancies that require only high school education. Employers call back résumés listing residential addresses in distant, poor neighborhoods less frequently than those listing addresses in nearby, affluent neighborhoods. I then test whether employers respond to distance independent of other attributes of the address, including neighborhood affluence. I match addresses to have different commute lengths but similar levels of

² Recent evidence indicates a stronger role for neighborhood effects among young children in MTO (Chetty, et. al. 2016) and more broadly (Chetty and Hendren, 2016). However, this new evidence has left untouched the debate on whether neighborhood location matters for adult employment.

affluence (education, income, and racial composition) and send applications with matched addresses to the same job. Addresses far from the job location receive 14 percent fewer callbacks than addresses in nearby but similarly affluent neighborhoods. Specifications including address fixed effects, which exploit only changes in proximity of a given address to different jobs, indicate that the main results may conservatively measure the importance of distance. Alternatively, I estimate how callback rates relate to straight line commute distance. To separate the causal effect of commute distance from employer characteristics correlated with longer average commutes, I instrument straight line distance with the randomly assigned address types and/or control for job vacancy fixed effects. Callback rates fall by 1.1 percentage points for every mile an applicant moves away from the job, holding neighborhood affluence constant. Using the same experiment, I can also measure whether employers respond to neighborhood affluence. While I typically find a slight positive relationship between employer responses and neighborhood affluence, these effects are small and statistically insignificant. Employers respond to distance.

The distance effects I measure are statistically and economically significant. Both the simple comparison of means and the instrumental variables model yield statistically significant main effects at the 5% level. To interpret the distance effect, I compare it to the standard discount in callback rates for stereotypically black names (Bertrand and Mullainathan, 2004), which I replicate. Applicants living 5-6 miles further from the job face approximately the same discount as applicants with stereotypically black names. Alternatively, even within the compact city limits of Washington, the average non-white person lives 0.9 miles further from jobs than the average white person. Differences in housing locations would thus increase the gap in callback rates between black and white applicants by 17% beyond the direct response to different names. Employers respond to distance in a manner that is empirically relevant.

This study provides empirical support for one mechanism of neighborhood effects: employer discrimination by commute distance. Ludwig, et. al. (2011) argue for “mechanism experiments” that test the behavioral mechanisms linking policies and outcomes. Knowledge on mechanisms matters because the case for a policy typically hinges on a particular theory. In the present context, subsidizing moves to low poverty but nearby neighborhoods may help if employers redline high poverty areas. However, I find that employers respond to distance rather than affluence. In this case, only housing policies that move residents across larger distances will affect employer decisions. The distinction between neighborhood composition mechanisms and spatial mechanisms remains important regardless of why employers care about distance. Low-wage employers may discriminate against applicants who live far away because distance matters intrinsically: long commutes by bus may lower workers’ productivity and reliability (Zenou, 2002; Van Ommeren and Gutierrez, 2011). Distance may also provide a negative statistical signal (Phelps, 1972) if only low quality workers bother searching for or accepting jobs far from home. The general equilibrium effects of policies that encourage large numbers of people to live closer to jobs may differ depending on why employers care about distance. For example, removing a distance signal could redirect statistical discrimination toward some other attribute, such as race. However, under either motivation moving one applicant to closer, not richer, neighborhoods affects that individual’s employment prospects. Since poor minority applicants tend to live farther from jobs, policies with a spatial focus will matter for employment disparities.

The fact that employers focus on commute distance rather than affluence can help explain disparate results from the small but growing experimental literature measuring employer discrimination by residential location. Bertrand and Mullainathan (2004) find that employer response rates correlate with the neighborhood affluence of addresses randomly assigned to fictional résumés in Boston and Chicago. Bonnet et. al. (2015) demonstrate similar results for

housing applications in Paris while Tunstall, et. al. (2013) find that addresses from poor neighborhoods in the UK receive similar callback rates from employers as addresses in “bland” neighborhoods.³ Differences between these existing studies can be explained by how they treat commute distance. Tunstall, et. al. (2013) send applications matched to have similar commute distances, while Bertrand and Mullainathan (2004) do not control for distance⁴ and thus measure a combination of distance and affluence effects. For comparison, I estimate the combined response of employers to addresses in the present experiment. I measure an overall address effect of the same magnitude as Bertrand and Mullainathan (2004), and commute distance accounts for two-thirds of employers’ overall response to addresses.

In the remainder of the paper, section 2 details theories of spatial mismatch incorporating employer discrimination as well as the context of the experiment. Section 3 describes the design of the experiment, and section 4 presents the results. Section 5 concludes.

2. Background

2.1. Spatial Mismatch and Employer Discrimination

Appropriate public policy depends on whether residential segregation contributes to black-white income disparities and, if so, what theoretical mechanisms connect residential location to labor market outcomes. A large theoretical literature identifies several potential mechanisms of spatial mismatch effects (Gobillon, et. al. 2007). Labor market frictions on the worker side including poor information on job vacancies, a lack of connections for referrals, and commuting costs during job search or employment can connect living far from jobs to labor market disparities (Coulson, et. al. 2001; Zenou, 2009). A large empirical literature tests for spatial labor market

³ Duguet, et. al. (2010) also find no effect of the listed town of residence for fictional applicants to French accounting positions; however, their focus on a skilled labor market makes direct comparison difficult.

⁴ Given that their study focuses on racial discrimination this is not a flaw in their study, only an issue for interpretation in the present context.

frictions and spatial mismatch effects using observational data (Zax and Kain, 1996; Holzer and Reaser, 2000; Hellerstein, et. al. 2008; Aslund, et. al. 2010; Marinescu and Rathelot, 2013; Sanchis-Guarner, 2014; Andersson, et. al., 2014; Mulalic, et. al., 2014; Miller, 2015; Manning and Petrongolo, forthcoming). A few experimental studies test whether worker-side interventions, such as transportation subsidies, can alleviate such spatial mismatch effects (Phillips, 2014; Franklin, 2014).

Discrimination by employers according to residential location can also lead to spatial mismatch effects. The standard monocentric spatial equilibrium model of urban housing implies sorting by income level with wealthier residents with greater appetite for cheap land living far from the center city (Fujita, 1991). Zenou and Boccoard (2000) show that a spatial equilibrium model in which black residents have higher commute costs than white residents can lead to housing segregated instead by race with black residents living in the central city. In these models, if employers do not care about the residential locations of their workers, such segregation may not exacerbate racial gaps in employment and wages; however, if employers prefer workers from the more affluent or whiter parts of the city, then housing segregation will contribute to racial gaps in labor market outcomes.

Employers may discriminate against workers from poor, minority neighborhoods because of the neighborhood's affluence. Zenou and Boccoard (2000) model employer discrimination as a simple spatial rule with employers who exogenously refuse to hire workers from poor or black parts of town. Such a rule could be motivated by various underlying goals. As in Becker (1971), employers may have prejudice, refusing to associate with workers from predominantly poor or black neighborhoods. Employers may statistically discriminate, using the characteristics of a person's neighborhood to infer the worker's attributes (Phelps, 1972). Employers may even

respond to differences in the variance of such unobservables (Heckman and Siegelman, 1993; Heckman, 1998; Neumark, 2012).

On the other hand, employers may care about a worker's residential location because it communicates commute distance. Zenou (2002) shows that if longer commutes diminish a worker's productivity via fatigue and unreliability, employers will discriminate based on residential location, and housing segregation will exacerbate racial gaps in labor market outcomes. Van Ommeren and Gutierrez (2011) provide an empirical basis for this theory, showing that worker absences rise when employers move further from an employee's residence. Employers could also statistically discriminate according to commute distance if workers self-select into applying to jobs far or near from home. While employers may use information regarding the affluence of a worker's neighborhood to discriminate in standard taste-based or statistical manners, they may also discriminate in a more fundamentally spatial manner by focusing on commute distance.

Whether policy responses to residential segregation affect racial labor market gaps depends on whether employers care about a neighborhood's geographic accessibility or other attributes, like affluence. If employers care about commute distance because long commutes hurt productivity (Zenou, 2002), public transit investment will be effective in combating employer discrimination. However, an MTO-type housing voucher that helps workers move to lower poverty but nearby neighborhoods will not address this concern. If employers care instead about the affluence of a worker's neighborhood, housing vouchers encouraging integration will affect labor market disparities. Even if employers statistically discriminate, it matters what information they extract from a residential location. If employer's extract some quality signal from a neighborhood's fixed attributes like racial composition, median income, or education levels, a public transit investment will likely not affect employers' decisions (unless it somehow breaks racial sorting in the housing market). On the other hand, suppose employers avoid distant applicants because high-quality

workers choose to work near home. A public transit investment would reduce self-selection by workers and weaken statistical discrimination based on distance. The net effect on racial labor market disparities might be complicated if removing the distance signal leads employers to statistically discriminate on race instead. Regardless, spatial interventions like transportation investment would matter for labor market disparities. Which public policies affect employers' decisions depends on whether employers discriminate according to commute distance or other neighborhood attributes.

Existing experiments provide little evidence on whether employers respond to a worker's commute distance when hiring. Some sources (e.g. Wilson, 1997) provide descriptive evidence that employers prefer workers from nearby neighborhoods, and a large non-experimental literature documents spatial mismatch effects. More precisely quantifying the extent to which employers respond to commute distance faces some empirical challenges, though. Workers applying to jobs far from home differ from those applying to jobs near to home. A small but growing literature uses correspondence experiments⁵ with carefully controlled fictional applicants to address this concern. Bertrand and Mullainathan (2004) send matched fictional résumés to real jobs and find that employers less frequently call back applicants who list addresses in neighborhoods with lower income. However, employers may be responding to either neighborhood affluence or commute distance, which tend to be correlated. Tunstall et. al. (2013) select addresses in neighborhoods with different affluence but similar commute distance and finds no difference in callback rates. While this suggests that employers do not respond to affluence, no similar evidence has confirmed whether commute distance can explain why employers appear to respond to a worker's residential location

⁵ This well-established method has been used to study labor market discrimination by many different factors including but not limited to race (Bertrand and Mullainathan, 2004; Arceo-Gomez and Campos-Vazquez, 2014), immigrant status (Oreopoulos, 2011), unemployment duration (Eriksson and Rooth, 2014; Kroft, et. al. 2013), and age (Lahey, 2006; Neumark, Burn, and Button, 2015; Farber, Silverman, and von Wachter, 2016).

in other studies. In the present study, I undertake a correspondence experiment in Washington, DC that isolates whether employers discriminate according to the applicant's commute distance.

2.2. Spatial Mismatch in Washington, DC

Neighborhood poverty correlates strongly with geographic access to jobs in Washington, DC. Figure 1 displays the poverty rate for different census tracts across the city. Poverty rates display a strong tendency to increase as one travels south and east, as evidenced by the darker shades for those tracts. The area just inside the southeast boundary of the city coincides with the part of the city beyond the Anacostia River and exhibits the city's highest poverty rates. Figure 2 displays heat maps of US Census LEHD data on where workers earning less than \$1,250 per month live and work. The dark lines displays the outline of the city itself with the Virginia suburbs beyond the Potomac River to the southwest and Maryland suburbs to the north and east. People employed in low-wage jobs live disproportionately in the far southeast side of the city, while low-wage jobs concentrate tightly in center of the city. Available suburban jobs mostly congregate to the West. Notably, few firms locate jobs east of the Anacostia River. Thus, these areas remain both high poverty and distant from job vacancies. Figure 3 summarizes this relationship for all census tracts in the city. The fraction of residents with low earnings exhibits a strong positive correlation with distance to jobs ($r = 0.70$). On average, being one mile further from jobs is associated with a 4 percentage point increase in people with low earnings. Similar results obtain for tract education levels or fraction white.

Washington shares this disconnect between where disadvantaged people live and where low-wage jobs locate with many other US cities. Washington only ranks 33rd out of the 94 most populous Metropolitan Statistical Areas (MSAs) in the US for black spatial mismatch, measured by an index of the dissimilarity between where black residents live and where jobs are located (Stoll, 2005). Washington is also not unusual in the extent of its job sprawl, ranking 37th out of the largest

94 MSAs in the proportion of jobs located more than 5 miles from the center city (Stoll, 2005). In some sense Washington actually provides a challenging test for detecting spatial effects in a labor market. I focus on the area within city limits, which is compact and well-served by public transit relative to other US cities. Driven by federal government demand, DC also has a relatively tight labor market which may drive low-wage employers to be less selective. If employers respond to commute distance in DC, then they likely would in other cities with greater sprawl and poor public transportation. Thus, Washington provides a useful context. It exhibits a typical disconnect between where low-wage workers live and where they work which parallels many other US cities.

3. Experimental Design

I use a correspondence experiment in the pattern of Bertrand and Mullainathan (2004) to study employer discrimination by residential location. From May 2014 through August 2014, I led a research team sending fictional résumés to actual jobs. I independently and randomly assign different characteristics listed on the fictional résumés. Since the experiment can control and randomly assign all information observed by the employer, any correlation of employers' responses with résumé characteristics can be attributed to employer discrimination based on that attribute. I measure employer responses using e-mail and voicemail accounts matching the information listed on the job applications. I record whether employers positively respond to the application; the vast majority of positive responses are requests to setup interview times, requests for specific information about the applicant, or general requests to call back. Henceforth, I will refer to this outcome equivalently as a "callback" or a "response." I do not include negative responses (e.g. rejection e-mails) or automated messages in this measure. I can then interpret differences in callback rates as employer discrimination.

3.1. Treatment

I carefully select the address listed at the top of the résumé to separately vary the applicant's commute distance and neighborhood affluence. Some existing studies with publicly available data randomly assign applicant addresses (e.g. Bertrand and Mullainathan, 2004), but it would be difficult to separate distance and affluence effects in these studies. The publicly available data do not list exact addresses or commute distances. More fundamentally, distance and affluence tend to be highly correlated so that experiments using a representative sample of addresses have low power to disentangle discrimination based on distance versus affluence. I isolate distance and affluence effects by selecting addresses with similar affluence but significantly different commute distance and also addresses with similar commute distance but different affluence. Formally, the experiment implements a 2x2 research design. Given a job location, I select one address each from four categories: near and poor (NP), near and affluent (NA), far and poor (FP), or far and affluent (FA), and I randomly assign the addresses to four job applications. Comparing employer responses for the two far types to the two near types measures the effect of distance holding affluence constant. I likewise measure the effect of affluence holding distance constant.

3.1.1. Measuring Commute Distance and Affluence

Implementing this experimental design first requires defining measures of commute distance and affluence. I measure commute distance simply as the great circle (i.e. "straight line") distance in miles between the address listed on the job application and the employer's location. I choose straight line distance rather than commute time for computational ease. The distinction between straight line distance and commute time can be important in many contexts. However, for designing this experiment, the choice does not greatly affect the results. The two distance measures are strongly correlated in my data ($r = 0.62$), and employers may not be able to parse the difference between the two quickly. In any case, I demonstrate below that the results are robust to measuring distance ex-post using public transit travel time.

I measure affluence as an index of fixed attributes of the neighborhood, weighting particular attributes based on employer preferences. The challenge is to summarize all fixed (i.e. not dependent on the location of the employer) neighborhood attributes such as poverty, racial composition and educational attainment into an index describing employers' perception of the neighborhood. I thus weight neighborhood attributes using employers' responses to those attributes in a prior experiment. Using data from Bertrand and Mullainathan (2004), I can estimate the following probit regression:

$$\Pr[C_i = 1] = \Phi(\beta_0 + \beta_1 Inc_i + \beta_2 FracWhite_i + \beta_3 FracCol_i)$$

C_i is a indicator of whether applicant i received a callback; Inc_i is the log median income of the census tract of the address listed on i 's résumé; $FracWhite_i$ is the fraction of census tract residents who are white; $FracCol_i$ is the fraction of the census tract with at least a bachelor's degree; $\Phi(\cdot)$ is the normal distribution. Appendix Table A.1. presents the results of estimating this equation with data from Bertrand and Mullainathan (2004). The three variables are jointly significant (F-test p-value of 0.005). The coefficients from this estimation provide relative weights for the affluence index based on employer preferences in a prior experiment.

To measure affluence in the present experiment, I apply the coefficients estimated above to 2011 5-Year ACS data for Washington, DC. I calculate expected callback rates for DC census tracts as:

$$\text{Index of Affluence} = \Phi(\hat{\beta}_0 + \hat{\beta}_1 Inc_i + \hat{\beta}_2 FracWhite_i + \hat{\beta}_3 FracCol_i)$$

where the $\hat{\beta}$ coefficients result from the Bertrand and Mullainathan (2004) data but the covariates represent neighborhood characteristics of DC census tracts in 2011. These fitted values are my measure of affluence, which combines census tract income, racial composition, and educational attainment into one measure with different attributes weighted depending on the observed importance placed on these characteristics by employers in the Bertrand and Mullainathan (2004)

sample. More generally, the index is a propensity score that can be used to match census tracts according to how their characteristics are viewed by employers. Addresses with the same propensity score should be treated similarly by employers if neighborhood income, racial composition, and educational attainment fully characterize the information contained in an address.

Any index of affluence will be imperfect. This particular index measures the relative weight employers place on various characteristics using data from Boston and Chicago. Data limitations prevent inclusion of other relevant variables, including distance to the job. However, the index is relatively robust. I can construct similar indices running univariate probit models that consider each of the three attributes separately. As shown in Appendix Table 2, the main index I use is positively correlated with indices created from each of the three components. For race and income, this correlation is above 0.9. It correlates less strongly but still positively with college attainment. Overall, this index provides a reasonable measure of affluence, and as shown below, I will use address fixed effects to control for potential errors in measuring affluence.

3.1.2. Selecting Addresses to Separate Affluence from Commute Distance

Given a particular job location, an algorithm selects 4 addresses that separately vary affluence and commute distance. I choose “Near, Affluent” and “Near, Poor” addresses from a grid to be the same distance from the job. Comparing callback rates for NA and NP addresses allows me to measure the effect of neighborhood affluence separately from commuting distance. I can similarly measure affluence effects by comparing “Far, Affluent” and “Far, Poor” addresses, and greater statistical precision can be obtained by combining NA and FA types to compare to NP and FP types. I can also measure the effect of commute distance holding affluence constant. I match NP and FP addresses to be of similar affluence; likewise for NA and FA. Distance effects can then be measured by comparing callback rates for types FP and FA to types NP and NA. Finally, given a

full 2x2 design, I can test for interactions between distance and affluence. The remainder of this section describes in detail an algorithm to select these four types of addresses.

First, I construct an 18x18 equally spaced grid of locations with borders formed by the points of the Washington, DC diamond. I then eliminate addresses outside of Washington, in parks, in bodies of water, and in census tracts dominated by universities (at least 30% college students) or military bases (at least 30% in armed forces). Each remaining point on this grid is paired with an address on the nearest “main street” (defined as streets shown as white or yellow on Google Maps at a particular zoom). I use main streets because a public survey (results available on request) indicated that respondents can more accurately identify characteristics of addresses on such main streets. This alternative performed better than solely manipulating the quadrant of the address or using addresses whose locations are communicated by the alphanumeric system of streets in DC. The background cloud of blue markers in Figure 4 shows the addresses that result from molding a grid to main streets in this manner. These addresses provide the pool of potential addresses for fictional applicants.

Then, for a given job location, I carefully select four addresses that separately vary affluence and commute distance. Figure 4 shows my overall strategy graphically. Consider a job located at the dot in the center of the figure. Given the location of the job vacancy, I first define a “Near, Poor” address. The NP address must be below the 10th percentile of the affluence index among all addresses on the grid and be no more than 1 mile further from the job than the closest such address. From all addresses meeting these requirements, I choose one at random. In practice, this typically results in an address just southeast of the job, as shown by the “Near, Poor” box in Figure 4.

Next, I select “Near, Affluent” and “Far, Poor” addresses to match the relevant characteristic of the “Near, Poor” address. An NA address will have similar commute distance as the NP address but be in a more affluent neighborhood. In Figure 4, a circle centered on the job location shows a

set of addresses equidistant to the job. Thus, an NA address will be on the same circle as the NP address but in a more affluent neighborhood, likely further northwest. Formally, I require that the NA address be the same distance from the job as NP (± 0.15 miles) and select one address at random from those above median affluence.⁶ I choose the “Far, Poor” address to match the affluence of the NP address but have a longer commute distance. In Figure 4, the box selecting the FP address is on a distance circle further from the job but still in the high poverty Southeast section of the city. Formally, I choose an FP address at random from among those that have the same affluence as the NP address (± 0.01 ; or 0.3 s.d.) and are at least two miles further away from the job than the NP address.

Choosing the “Far, Affluent” address involves a tradeoff. FA addresses should match the affluence of the NA address and the commute distance of the FP address. In Figure 4, I would like an address on the same circle as the FP address but with similar affluence to the NA address. However, these two goals may be inconsistent in an actual city. In practice, I balance these two concerns by choosing the FA address that minimizes the following:

$$\left(\frac{Dist_{FA} - \mu_{dist}}{\sigma_{dist}} - \frac{Dist_{FP} - \mu_{dist}}{\sigma_{dist}}\right)^2 + \left(\frac{Affluence_{FA} - \mu_{aff}}{\sigma_{aff}} - \frac{Affluence_{NA} - \mu_{aff}}{\sigma_{aff}}\right)^2$$

where μ 's are means, σ 's are standard deviations, *Dist* is distance to job, and *Affluence* is the affluence index. In words, I translate measurements of the affluence index and distance into z-scores, calculate the squared difference of the FA type z-score from the one it should match (FP for distance; NA for affluence), and then add the two squared differences together. In the ideal, this calculation would result in zero, indicating that the FA matches the affluence of the NA and the distance of the FP exactly. I will consider the extent to which I achieve this ideal below.

⁶ If there is no such address, I choose the most affluent address with sufficiently similar commute distance.

In the end, this algorithm selects 4 addresses for job applications to a given job. Far and near address types are matched to have similar affluence levels. Comparing employer responses for near versus far addresses then measures whether employers care about commute distance separately from affluence; similarly, comparing affluent and poor address types measures affluence effects separately from distance.

3.1.3. Characteristics of Addresses Used in the Experiment

I apply this algorithm to a sample of 565 low-wage jobs in Washington, DC. Figure 5 shows the locations of addresses in all four treatments in response to job locations shown in Figure 6. With job locations concentrated in the center of the city Near, Poor addresses cluster in two high poverty neighborhoods just northeast and just south of the center of the city. Some Near, Affluent addresses cluster in an affluent neighborhood just east of the center of the city while others are spread throughout the Northwest quadrant of the city. The algorithm locates Far, Poor addresses almost entirely in far Southeast DC across the Anacostia River. Far, Affluent addresses concentrate in the far Northwest part of the city with a small cluster in an affluent, predominantly black neighborhood in far Southeast.

The first panel of Table 1 quantifies how the four types of addresses differ. The columns show average characteristics for all four address types. For instance, fictional applicants from NA addresses live on average 3.0 miles from the jobs to which they apply. NP addresses are also 3.0 miles away while FA and FP addresses are further away at 5.3 miles and 5.8 miles. The final two columns measure the differences between treatment types, for instance measuring distance effects by comparing FA and FP addresses to NA and NP addresses. The remaining rows display similar results for variables related to neighborhood affluence. The results indicate that the chosen addresses do generate significant variation in both distance and affluence that matches their assigned treatments. Far addresses are 2.6 miles further away from jobs than near addresses, and

poor addresses are in neighborhoods with \$74,000 lower median income, 50 percentage points fewer college graduates, and 40 percentage points fewer whites.

The gap in distance between near and far addresses is meaningful. Using data from Germany, Van Ommeren and Gutierrez (2011) estimate that reducing average commutes from 9 miles to zero would cut absence rates by 15-20%. Extrapolating to the present experiment, employers could reasonably expect near addresses to have absence rates 5-6% lower than far addresses. Also, low-wage job applicants in Washington tend to travel by bus rather than train (Phillips, 2014). Commutes by bus tend to be slow and unpredictable, making an additional 2-3 miles important. Consider rush hour commutes to downtown from the most common address in the FP and NP groups.⁷ The NP address is 2.7 miles away by straight line, which takes 32 minutes riding a bus. The FP address is 6.1 miles away with a 77 minute commute. The latter trip also requires transferring bus lines, which reduces predictability. A service sector employer seeking punctuality and reliability might wish to cut half an hour off an employee's commute.

The gap in commutes between near and far applicants is also reasonable. In one sample of significantly disadvantaged, low wage job applicants in Washington, DC (Phillips, 2014) commute distance for job applications has a mean of 5.3 miles and a standard deviation of 4.2 miles. Accepted job offers have a mean commute of 5.6 miles with a standard deviation of 3.9 miles. Similarly, representative data from the US Census's 2014 LEHD dataset indicate that 22% of workers living in DC southeast of the Anacostia River commute more than 10 miles to work.⁸ Finally, I can compute average travel distance to the jobs for the overall white and black populations in Washington, DC. I take 2014 LEHD data on job and home locations by census tract. I assume that all individuals live at the tract centroid. I compute the distance between a tract and

⁷ I calculate commutes to 1600 K St. NW arriving by 9:00 AM on Thursday, October 20, 2016 using the WMATA Trip Planner.

⁸ Figures obtained using the Census's "On the Map" tool to isolate the area east of the Anacostia River.

low-wage jobs as a weighted average, measuring its distance (centroid to centroid) to all other tracts in the Washington, DC area⁹ and weighting each destination tract by the number of primary jobs with earnings less than \$1,250 per month. Black individuals in DC would need to travel on average 10.8 miles to a random low-wage job in the metro area versus 9.9 miles for the average white individual. Thus, the experimental manipulation of commute distance is about 3 times the black-white job access gap in DC. The experimental variation in commute distance meaningfully relates to the differences in job access by race and income level.

The experiment manipulates affluence a bit more strongly but still within a reasonable range. The \$74,000 decrease in median income represents a move from the 17th percentile of census tract median income to the 85th percentile. A 50 percentage point decrease in college completion moves from the 20th percentile of census tracts to the 71st percentile. A 40 percentage point decrease in the fraction of white residents moves a census tract from the 30th percentile to the 64th percentile. Thus, comparisons between the “poor” and “affluent” addresses in this study should be interpreted as the effect of a large move along the empirical distribution of neighborhood affluence.

Panel A of Table 1 also demonstrates one challenge inherent in the process of selecting addresses: the addresses are not perfectly matched. For instance, addresses that I classify as poor versus affluent should be the same distance from jobs. In fact, poor addresses are 0.2 miles further away on average. Similarly, far and near addresses should have similar affluence. While this is true for median income, far addresses tend to be in less educated and less white neighborhoods. These remaining differences inherently occur because the available variation in these variables at actual addresses does not always allow for a perfect match to be made (see above). The matching

⁹ I define this to be DC and nearby suburban areas: Prince George’s County (MD), Montgomery County (MD), Arlington County (VA), Fairfax County (VA), and the cities of Alexandria (VA), Falls Church (VA), and Fairfax (VA).

process does, though, significantly reduce the correlation between commuting distance and neighborhood affluence. I will test the robustness of distance effects to imperfect matching with two distinct strategies. An instrumental variables framework that allows each treatment type to affect both affluence and distance can correct for imperfect matching. Adding applicant address fixed effects removes the influence of any fixed attributes, including affluence. Both of these alternative strategies will indicate that imperfect matching does not drive distance effects.¹⁰

3.2. Other Experimental Design Details

As described in the appendix, a detailed experimental protocol governed other aspects of the process. The overarching goal of the process is twofold. First, when possible I keep the process similar to previous correspondence studies of the labor market. Second, I tailor the process to studying the labor market for low-wage work by applying to jobs with low skill and education requirements. See the appendix for further details. Altogether, the research team sent 2,260 fictional applications to 565 job vacancies.¹¹

Panel B of Table 1 shows characteristics of the job location's census tract. These employer location variables are perfectly balanced by construction because the experiment sends each address type to each job vacancy. Jobs tend to be in high-income, well-educated, and white neighborhoods near downtown. Figure 6 shows a map of these employer locations, which can be compared to representative data on low-wage work locations in Figure 2.B. The geographic distribution of jobs used in the experiment matches the overall tendency of low-wage work in Washington, DC to be located in the center of the city.

¹⁰ Distance results are also robust to limiting the sample to jobs for which near and far addresses match racial composition more closely. Results not included but available upon request.

¹¹ This value was chosen based on ex-ante power calculations. The sample size was chosen to detect a 0.036 change in callback rates for either the “poor” or “far” treatments (i.e. for main effects) or a 0.05 change in callback rates for interactions with 80% power at the 5% level.

Panel C of Table 1 presents summary statistics of the various résumé characteristics as well as their balance across address treatment types. The typical fictional applicant has graduated from high school, is 41 years old, has been unemployed for 3 months, and has 8 years of listed work experience. The sample is evenly split between male and female; 25% have ambiguous names with the remainder split evenly between black and white names.

As expected, most applicant characteristics show differences that are small both economically and statistically. Having a white name, age, work experience, and sex are all statistically balanced. By chance, résumés with the “far” treatment are 6 percentage points more likely to have black names and have work gaps that are 5 days longer. These differences are statistically significant though economically small and ultimately not of major concern. One might be concerned that this imbalance could lead to lower callback rates for far addresses, leading to an overestimate of the effect of discrimination by commuting distance. However, controlling for these characteristics does not change the main results significantly (see results below). Additionally, it appears that randomly high values of these “negative” characteristics are counterbalanced by other factors. I measure overall quality of all the applicant characteristics on the résumé by regressing a callback dummy on the listed applicant characteristics and a set of 32 dummies for the interaction of the 4 different job experience profiles with 8 different job types. This model and results are shown in Appendix Table 3. The fitted values of this regression measure the overall quality of non-address-related characteristics on the résumé. The final row of panel C displays balance on this measure of overall résumé quality. All four types of addresses have predicted callback rates between 18.3% and 19.2% based on observable characteristics, and the difference between near and far addresses is statistically insignificant. Randomization of résumé characteristics has ensured that résumés in different treatment categories are on average of similar quality, except for the listed address.

3.3. Regression Framework

I will test for the effects of commute distance and neighborhood affluence using a regression framework. A simple regression yields differences in callback rates:

$$Y_{aij} = \beta_0 + \beta_1 Far_{aij} + \beta_2 Poor_{aij} + \epsilon_{aij} \quad (1)$$

Y_{aij} is a dummy for whether applicant i with address a applying to job j receives a positive callback; Far_{aij} is a dummy for the Far Poor and Far Affluent treatments; $Poor_{aij}$ is a dummy for the Near Poor and Far Poor treatments; and ϵ_{aij} is an error term. Estimates of β_1 and β_2 thus measure the gap in callback rates for far versus near and poor versus affluent neighborhoods, respectively.

For interpretive reasons, consider continuous measures of treatment:

$$Y_{aij} = \beta_0 + \beta_1 Distance_{aj} + \beta_2 Affluence_a + \epsilon_{aij} \quad (2)$$

$Distance_{aj}$ is the great circle distance between address a and job j while $Affluence_a$ can either be the affluence index or any of its components: log median income, fraction white, or fraction college educated. These continuous treatments are only randomly selected in part. For example, commute distance depends on both the randomly assigned treatment types and how the employer location and city boundaries limit maximum distance. Thus, OLS estimation of (2) includes some bias. I adjust for this bias by including job fixed effects and/or by using the randomly assigned Far_{aij} and $Poor_{aij}$ dummies as instruments for continuous measures of treatment.

As discussed above, matching on observable variables with real addresses leads to inherent imperfections in balance across treatment types. I address this concern in two ways. First, the continuous treatment setup in equation (2) parametrically adjusts for imbalance. I include both distance and affluence as treatments, and I allow both far and poor treatment dummies to instrument for both commute distance and neighborhood affluence. As in a typical model with multiple

continuous treatments, applying IV estimation to (2) will automatically adjust measured treatment effects for imperfect matching, subject to a linearity assumption. Second, I revise my main specification to include fixed effects for the 226 different addresses used in the study.

$$Y_{aj} = \beta_0 + \beta_1 Far_{aj} + \beta_2 Poor_{aj} + \phi_a + \psi_j + \epsilon_{aj} \quad (3)$$

This specification includes address fixed effects, ϕ_a . These fixed effects absorb any fixed characteristics of the applicant's address, whether observed but included imperfectly in my matching algorithm (e.g. racial composition) or unobserved and thus unmatched (e.g. employer perceptions of a neighborhood's average work ethic). This specification identifies treatment effects using applicant addresses that are assigned different treatment status depending on the employer location. For instance, an applicant address in downtown can be "near" for downtown jobs and "far" for jobs near city limits. Equation (3) thus identifies the effects of distance using variation generated by the interaction of applicant and job location. Addresses are rarely assigned both to be "poor" and "rich" for different jobs, making the estimate of β_2 much noisier. I also include job vacancy fixed effects ψ_j in this specification to avoid confounding distance with fixed employer attributes. These two alternative identification strategies, one which leverages the experimental design via instrumental variables and one which uses quasi-experimental variation in commute distance, complement my main specification and ensure that imperfections in the matching process do not drive my results.

4. Results

4.1. Main Results

Panel D of Table 1 presents the simplest version of the experimental results. As predicted, near affluent addresses have the highest callback rate at 0.207, and far poor addresses have the lowest callback rate of 0.170. This gap of 3.7 percentage points confirms the finding of Bertrand and Mullainathan (2004) that employers discriminate by residential location. In the present study, I

aim to test whether commute distance and neighborhood affluence measurably contribute to this discrimination. Applicants listing distant addresses receive significantly fewer callbacks. Applicants from far affluent addresses are called back at a rate 0.177, which is 3.0 percentage points lower than the 0.207 rate of those with near affluent addresses. Applicants from far poor neighborhoods receive 2.5 percentage points fewer callbacks than applicants from near poor addresses. This averages to a 2.7 percentage point decrease in callback rates which is statistically significant (p -value = 0.03). Only a smaller, statistically insignificant gap appears between poor and affluent addresses. Near poor addresses receive a callback rate 1.2 percentage points lower than near affluent addresses. The difference between far affluent and far poor addresses is smaller. Overall, these two differences average to a statistically insignificant 1.0 percentage point decrease in callback rates for poor neighborhoods relative to affluent neighborhoods, holding commute distance constant. Employers respond to commute distance while evidence for responsiveness to neighborhood affluence is less clear.

Table 2 tests the robustness of these differences more carefully using linear regression. Column (1) repeats the simple comparison of callback rates in a regression framework. I pool the treatments into overall near-far and rich-poor comparisons using dummies indicating far addresses (FP or FA) and poor addresses (NP or FP). The results indicate that having an address distant from the job and having an address in a less affluent neighborhood both yield lower callback rates. However, only commute distance is statistically significant at the 5% level. Low-wage employers may avoid those who apply from far away if distance leads the employer to question why the applicant could not get a job near home. This type of distance effect should be stronger for applicants from far, affluent neighborhoods with abundant jobs. Column (2) of Table 2 tests for interactions between affluence and distance and shows no evidence that the two treatments interact significantly; hence, I focus on the pooled results which have greater statistical power. Column (3)

includes job fixed effects while column (4) also adds applicant controls (racial name dummies, female name dummy, years of listed work experience, age, length of work gap, and job category-work history profile dummies). Since neighborhood affluence and distance to the job are randomly assigned, adding controls does not change the main story significantly. The coefficient on distance changes very little and the p-value drops slightly to 0.06. The effect of having an address in a poor neighborhood remains negative but statistically insignificant throughout. Thus, the data provide strong statistical evidence that employers discriminate by distance; evidence for discrimination by neighborhood affluence remains weaker.

I also test for interactions between race and distance. Employers might respond to distance more elastically for black applicants, who on average have more difficulty travelling across space, and less elastically for white applicants. Column (5) of Table 2 tests whether distance effects interact with whether the applicant lists a white or black name, leaving ambiguous names as the omitted group. A negative interaction for black names and a positive interaction for white names would match expectations, but the results show no obvious pattern. The point estimates for white and black name interactions are both negative. The total distance effect of -0.020 for applicants with black-sounding names is not distinguishable from the full sample effect of -0.024. In any case, the sub-group estimates have large standard errors and do not differ from each other statistically. Large standard errors and patternless results suggest the experiment has low power to test for race-distance interactions.¹²

¹² As with race, distance could communicate information about the variance of unobservables. Neumark (2012) provides a method for estimating and correcting for differing variances, but this method does not fit the present data well. The method uses the interaction between treatment and other randomly assigned characteristics to infer differences in variance between treatment and control, but my sample does not have sufficient statistical power to measure precise sub-group effects. When I apply this method to the data, the adjusted estimates prove too noisy to be informative. Results available upon request. In any case, theory suggests that ignoring differing variances will lead to conservatively measured distance effects. The variance of unobservables would likely be greater for distant applicants than for near applicants. The highest ability applicants are particularly motivated to travel over long distances, while local employers reject the lowest ability applicants. However, employers actually prefer this high variance. The probability of success is far less than 0.50, so only applicants with very good draws of unobservable characteristics are

Racial name effects can provide useful context for the main distance results. For comparison, I display the coefficient on a dummy for having a stereotypically black name in column (4). The coefficient of -0.060 indicates that individuals with black-sounding names receive a 6.0 percentage point lower callback rate than those with ambiguous or white names. Since there is not a statistically significant difference between white and ambiguous names, the 6.0 percentage point gap can be interpreted as the standard white/black difference. An applicant living 2.6 miles further from the job receives 2.4 percentage points fewer callbacks, 40% of the penalty received by a stereotypically black name.

4.2. Continuous Measures of Distance and Affluence

To draw a more direct comparison with the previous literature and to check for robustness to imperfect matching of treatment types, I can replace the 2x2 design and treatment dummies with continuous measures of distance and affluence. Figure 7 demonstrates that callback rates correlate with distance to the job, holding affluence constant. For both poor and affluent addresses, callback rates decrease monotonically out to 7 miles, only showing an uptick for the furthest 5% of addresses. In the full sample, local linear-smoothed callback rates fall from 0.24 at 1 mile from the job to 0.18 at 4.5 miles away. This drop matches the 6.0 percentage point callback penalty incurred by having a stereotypically black name. Aside from clear interpretation, continuous treatments also provides a simple parametric method for adjusting for imperfect matching of near and far addresses. Far addresses come from slightly less affluent neighborhoods than near neighborhoods (see previous discussion). Models including continuous treatments in both distance and affluence will adjust linearly for this fact. Column (1) of Table 3 shows the results of a simple OLS regression of callback rates on commute distance and the affluence index. An applicant living 1 mile further

worth hiring. Employers prefer interviewing high variance groups with more applicants in the tails, and netting out this variance effect would then increase the magnitude of the distance penalty.

from the job location receives response rates lower by 0.85 percentage points, though statistical significance is somewhat weak ($p = 0.11$). At a descriptive level, callback rates appear to move with commute distance.

However, simple correlations may estimate employers' responsiveness to commute distance with bias. Commute distances depend not on only the randomly assigned treatment category, near or far, but also on the employer's location. For example, "far" applicants sent to employers in extreme Northwest DC will tend to have a longer commute than "far" applicants sent to jobs downtown. But downtown and remote employers may differ in ways other than commute distance. Thus, the simple correlation between callback rates and commute distance may combine the negative causal effect of distance with the effect of employer fixed characteristics. Simple correlations would estimate effect of distance with bias. Either an instrumental variables strategy or job fixed effects can remove this bias. Unlike the employer's location, the applicant's location is random. I use the randomly assigned applicant address treatment categories as instruments for continuous measures of distance and affluence. Alternatively, I can estimate a specification controlling for job fixed effects. Either method should remove bias from employer characteristics correlated with distance and recover the causal effect of distance.

Using job fixed effects and IV, I find that models with continuous measures of treatment provide similar distance effects to the simple analysis of treatment categories. Column (2) of Table 3 adds job fixed effects to the simple OLS model. The distance effect increases in magnitude slightly to -0.0089 and becomes statistically significant at the 10% level, suggesting that the simple OLS results in column (1) were biased toward zero. Columns (3) through (6) show the IV results. Columns (3) and (4) show the dual first stages of the IV. As expected from Table 1, the instruments are strong with the far indicator (t-stat of 48.2) providing a stronger instrument for distance, though the poor neighborhood indicator still matters (t-stat 4.6). The poor dummy correlates with distance

because of imperfect matching of distance across affluent and poor types. Similarly, the fourth column shows that the poor dummy provides the stronger instrument for affluence though the far dummy also matters. The IV estimation will thus measure the extent to which the “far” group has both longer commutes and less affluent neighborhoods and account for this fact in estimating how commute distance relates to employer responses. Column (5) shows the second stage of the IV. A 1-mile increase in distance to the job decreases callback rates by 1.1 percentage points. Removing the bias in OLS from employer locations makes this effect somewhat larger in magnitude and statistically significant at the 5% level. The affluence measure again shows no statistically significant relationship with callback rates. Finally, column (6) shows a specification using both IV and job fixed effects. As expected, job fixed effects do not change the results since the IV framework already corrects for this source of bias. Models with continuous measures of treatment confirm that callback rates fall as applicants live farther from the job location.

Table 4 confirms that the results of the instrumental variables model are robust to alternative measures of distance and affluence. The first column replicates the results of a simple IV model with no controls; callback rates diminish by 1.1 percentage points per mile, and affluence demonstrates a positive but statistically insignificant relationship with employers’ responses. Column (2) shows that these results change little using log median income instead of the affluence index.¹³ Column (3) shows that the results remain very similar if I use all 4 treatment types as instruments rather than just the far and poor dummy variables. Column (4) demonstrates that distance effects extend out several miles. A quadratic term in distance is not statistically significant, but even taking its positive sign and magnitude at face value indicates that callback rates decrease out to 5 miles from the job. Column (5) shows that measuring distance as public transit commute times rather than linear distance does not change the results greatly. I can measure public transit

¹³ Results are also similar for fraction white and fraction college-educated.

travel time between job and home locations using the Washington Metropolitan Transit Authority's "Trip Planner."¹⁴ Ten minutes of additional commute time lowers callback rates by 3 percentage points. Great circle and commute time measures of distance provide similar results because the two are highly correlated in this context; as column (6) shows, attempting to include both in the regression leads to statistical imprecision.

4.3. Interpreting the Magnitude of Distance Effects

The continuous measure of distance provides a natural way to gauge the magnitude of the estimated effects. Commute distances for far and near types differ on average by 2.6 miles. Inserting this difference into the IV model implies a gap in callback rates of 0.029 ($2.6 * 0.011 = .029$) between far and near types, which matches the results from Table 2. Alternatively, a person assigned an address 5.5 miles away will face a similar impediment to their job application as someone assigned a stereotypically black name ($5.5 * 1.1 = 6.0$). Even modest racial gaps in job access in Washington, DC could contribute noticeably to racial differences in the labor market. As documented above, the average black person in Washington, DC lives 0.9 miles further from low-wage jobs than the average white person does. According to the model in column (6) of Table 3, the average black person looking for low wage work would thus receive about 1.0 percentage points fewer callbacks than the average white person if only residential location mattered. This gap represents 17% of the direct effect of having a stereotypically black name. Given the large distance effects that I measure, even small differences in geographic access to employment can lead to meaningful racial gaps in treatment by employers.

The continuous measures also provide a means of directly comparing the relative magnitude of distance and affluence effects. Consider someone who moves to a more affluent but also more distant neighborhood. Since the variables have different scales, I use the model in column (6) of

¹⁴ All travel times are measured at 8:30 AM on Friday, January 9, 2015.

Table 3 to test whether a one standard deviation increase in both distance (4.26 miles) and affluence (0.036) affects callback rates. It is worth noting that I compute these standard deviations within the boundaries of Washington, DC, and a move to the suburbs would likely involve similar changes in affluence but a much larger change in distance to jobs. Changing this combination of attributes would decrease callback rates by 0.04, though this is only statistically significant at the 10% level. This comparison suggests that distance matters not only in an absolute sense but also relative to affluence.

Results with continuous measures can also be used to test whether the distance effect I measure is large enough to account for the previously documented relationship between callback rates and neighborhood income. For instance, Bertrand and Mullainathan (2004) examine how callback rates respond to neighborhood characteristics, but because their focus is on racial discrimination, they do not measure commute distance. They omit distance, estimating regressions of the following form:

$$Y_{aij} = \alpha_0 + \alpha_1 \text{Log Median Income}_a + u_{aij}$$

They estimate α_1 at 0.018, indicating that doubling median income raises callback rates by 1.8 percentage points. However, if the true model includes distance, then estimates of α_1 will include both distance and affluence effects. I cannot replicate their approach exactly because I have chosen addresses to remove the correlation between distance and income; however, I can find the estimate of α_1 implied by my results using the standard omitted variable bias formula:

$$\hat{\alpha}_1 = \frac{\partial Y}{\partial \text{Log Med. Inc.}} + \frac{\partial Y}{\partial \text{Distance}} * \frac{\partial \text{Distance}}{\partial \text{Log Med. Inc.}} = 0.006 + (-0.011) * (-1.08) = 0.018$$

I draw the first two values from column (2) of Table 4. I compute the distance-income gradient using a simple regression on ACS data across all census tracts in DC and find that doubling neighborhood income is associated with being 1.08 miles closer to the jobs in my sample.

Replicating Bertrand and Mullainathan’s specification during my sample period with representative addresses would thus yield an estimate of 0.018 for the coefficient on log median income, of which 0.012 results from commute distance. The overall effect exactly matches their results from an earlier time period in Boston and Chicago, though the proportion attributable to distance may be larger or smaller depending on context. For example, affluence correlates less strongly with commute distances in Chicago than DC,¹⁵ but employers may also respond more elastically to distance in a more dispersed city like Chicago. In DC, the distance and affluence effects I measure can explain the previously documented relationship between callback rates and neighborhood income. Commute distance contributes roughly 2/3 of the overall effect.

4.4. Controlling for Fixed Address-Specific Attributes

I can use applicant address controls to test the robustness of my results to imperfect matching of neighborhood attributes. The experimental design could overestimate distance effects due to imperfect matching on neighborhood education, race, and income or because I cannot match on unobservable neighborhood characteristics. This problem will confront any experimental design attempting to isolate distance effects given the available variation from actual addresses and the multi-dimensionality of affluence. However, the design of the experiment allows me to tackle both of these issues convincingly using applicant address fixed effects. An address fixed effect will absorb any remaining differences in neighborhood income, education, and racial composition due to imperfect matching. More importantly, address fixed effects control for differences in unobserved aspects of affluence across different addresses. This specification relies on applicant addresses that are assigned as “far” for some résumés and “near” for other résumés. Variation in the employer location drives these “switches.” This sub-sample covers 987 out of 2,260 applications. Using only

¹⁵ Using the same LEHD data as in Figure 3, the correlation coefficient between the fraction of low wage workers and commute distance for Chicago census tracts is 0.27, while it is 0.70 in DC.

variation in commute distance within the same listed address, I can measure a pure distance effect separate from all fixed attributes of the address.

Table 5 displays results exploiting within-address variation in commute distance. Column (1) replicates column (4) of Table 2 for comparison. Columns (2) through (4) control for applicant addresses with increasing precision. Column (2) controls for quadrant dummies (defined by location relative to the US Capitol Building), column (3) includes tract fixed effects, and column (4) includes address fixed effects. These results allay concerns that distance discrimination has been conflated with imperfect matching due to either observed or unobserved variables. The measured gap between far and near addresses actually increases in magnitude to 7.4 percentage points as I control for the applicant's address with greater precision. Even with a much larger standard error, the effect remains statistically significant at the 5% level. Given the large standard error resulting from using address fixed effects, I conservatively reference the specifications without address fixed effects as the main results. However, the results including address fixed effects indicate that my main results do not overestimate discrimination by commute distance.

Why does including address fixed effects lead to larger point estimates? First, address fixed effects focus attention on a sub-sample of employers who may respond more sensitively to distance. Column (5) shows estimates from a specification without address fixed effects that limits the sample to only those addresses with some within-address variation in the far versus near treatment. In this sample the distance effect is 5.8 percentage points, which is larger than the full sample results. The composition of the switchers appears to matter, and one might be concerned that affluent addresses drive these results. The near and far regions overlap more for affluent addresses than for poor addresses (see Figure 5). Switching addresses tend to be in the Northwest quadrant of the city as addresses in the Southeast and Southwest quadrants tend to be consistently classified as far and near, respectively; 41% of affluent addresses are labelled far between 5% and 95% of the

time. Only 15% of poor addresses switch this often. In column (6), I address this concern by splitting out distance effects by affluence in the address fixed effects model. As expected, the distance effect for poor addresses is imprecisely measured, but its point estimate is similar to the effect for affluent addresses. A stronger response to distance in the sample of switchers accounts for much of how address fixed effects increase the distance effect but not because distance matters more in affluent areas.

Second, the fixed effects control for some bias due to local but unobserved characteristics. Census tract fixed effects control for many locally unobserved characteristics but do not limit the effective sample significantly because most tracts include both far and near applicants. Hence, the difference between columns (1) and (3) suggests that controlling for the applicant's location does adjust for some unobserved characteristics. Identifying these variables proves difficult. For example, column (7) includes a measure of job access, the average distance from the applicant's residential address to all jobs in the experiment. Employers might positively respond to a candidate who perseveres in applying to jobs despite limited local opportunities; however, the strong correlation between average distance to jobs and distance to the particular job in question makes the results quite noisy. Whatever unobserved local factors affect the employer's decision appear to vary within relatively small areas of the city and be unrelated to income, race, and education. Taken together, these results suggest that both narrowing the sample and bias reduction drive the difference between the main estimates and the address fixed effects estimates.

The results for affluence in the address fixed effects specifications prove somewhat less helpful. While assignment to far versus near types can vary with the location of the job, very few addresses are assigned as poor sometimes and affluent other times.¹⁶ In column (4) of Table 5,

¹⁶ Though unlikely, it is possible for an address to be classified sometimes as poor and sometimes as affluent. For example, consider an address in near southeast DC which has below median affluence. This address will most commonly be classified as near and poor for downtown job locations. On rare occasions, the job location will be in far

exploiting the limited within address variation in the affluent vs. poor treatment generates a large *positive* estimate of being from a poor neighborhood but also an extremely large standard error. More useful results can be obtained using intermediate steps between no address controls and address fixed effects. Discrimination by neighborhood affluence disappears in column (2) controlling only for quadrant fixed effects. The effect of being assigned a poor address is positive and with a small standard error. This more precise zero suggests that any discrimination by neighborhood affluence that does exist is very broad according to large regions of the city. On the other hand, discrimination by distance remains negative and statistically significant. The results with quadrant dummies provide additional evidence that employers discriminate according to commute distance, per se, rather than simply redlining large, distant areas of the city.

4.5. Different Outcome Measures

My interpretation of the main results is consistent with different employer response measures. Table 6 shows these results. In the paper thus far, I have used as my preferred outcome an indicator of receiving either a positive or neutral response from the employer. I replicate the main results in Column (1), demonstrating a statistically significant 2.7 percentage point gap between near and far applicants using my preferred outcome measure. Though 70% of the responses in my preferred measure include explicit requests to setup an interview, the other 30% are more neutral responses that do not state the reason for calling or simply request more information. Column (2) tests my main specification using only specific interview requests as the outcome and obtains nearly identical results. My decision to include more neutral responses does not drive the results. Any correspondence experiment also necessarily separates out clearly negative responses

southeast DC where there may not be any nearby addresses with above median affluence. So, the address may be classified as the most affluent nearby address. However, only 16 addresses representing 18 percent of applications are assigned to both poor and affluent at least once. Only in one case do I assign more than two applications as poor and more than two applications as affluent from the same address.

rejecting the applicant. However, these responses also convey information that can be exploited. Column (3) demonstrates that listing an address far from the job not only decreases positive responses but also increases clear rejections by 0.9 percentage points. Because of this fact, commuting distance does not exhibit a statistically significant relationship with the probability of receiving any response. Commuting distance affects the pattern of positive and negative responses in a manner consistent with my interpretation of the main results.

5. Conclusion

In this study, I demonstrate that employers discriminate against job applicants who list more distant residential addresses. When presented with otherwise similar fictional résumés, hiring managers for real low-wage job vacancies in Washington, DC call back applicants living further away 14 percent less often. This effect is large. Living 5-6 miles further away decreases callback rates by the same discount experienced by applicants with ‘black names’ relative to those with ‘white names.’ Because commuting distance and neighborhood poverty are correlated in DC, discrimination by distance can account for two-thirds of the penalty in callback rates for addresses in poor neighborhoods. On the other hand, the results provide limited support for the notion that the listed address’s neighborhood affluence directly affects employer behavior. To the extent that employer behavior and the correlation between commuting distance and affluence translate to other contexts, commute distance could explain employers’ demonstrated reluctance to interview applicants from poor neighborhoods.

These results provide support for one potential mechanism behind the spatial mismatch hypothesis, the idea that living far from employment opportunities directly diminishes labor market prospects of the urban poor. While I cannot observe actual employment or wage offers, a drawback of all résumé studies, standard random search models predict that a lower arrival rate of contacts with employers should result in lower employment rates and wages in equilibrium (McCall, 1970).

In a labor market with frictions, evidence of employer discrimination provides a causal mechanism running between living in a neighborhood far from jobs and poor labor market outcomes. While a large non-experimental literature has examined spatial mismatch, very few experiments have directly tested such mechanisms of spatial mismatch. The present study confirms that a mechanism on the employer side can contribute to spatial mismatch effects.

The effects of public policy depend on whether a spatial mechanism drives neighborhood effects. The Moving to Opportunity (MTO) project prominently found that providing housing vouchers to public housing residents did not improve adult labor market outcomes (Kling, et. al., 2007; Ludwig, et. al. 2012). However, while MTO participants moved to neighborhoods with lower poverty rates, Quigley, et. al. (2008) note that the voucher recipients tended to move to neighborhoods with similarly poor access to jobs. If neighborhood effects operate through a spatial mechanism, then housing vouchers that facilitate moves to less poor but equally distant neighborhoods will not affect labor market outcomes. Instead, housing interventions moving residents close to jobs or better public transit would matter. The net effects of such policies may be hard to predict. Moving residents from distant to near neighborhoods could have general equilibrium effects on which this study provides no evidence. For example, employers might replace statistical discrimination on distance with statistical discrimination on race. Whatever their overall effect, policies affecting transportation and proximity to jobs influence low-wage labor market outcomes. If low-wage employers respond to an applicant's commute distance, the relative location of workers and jobs matters for anti-poverty policy.

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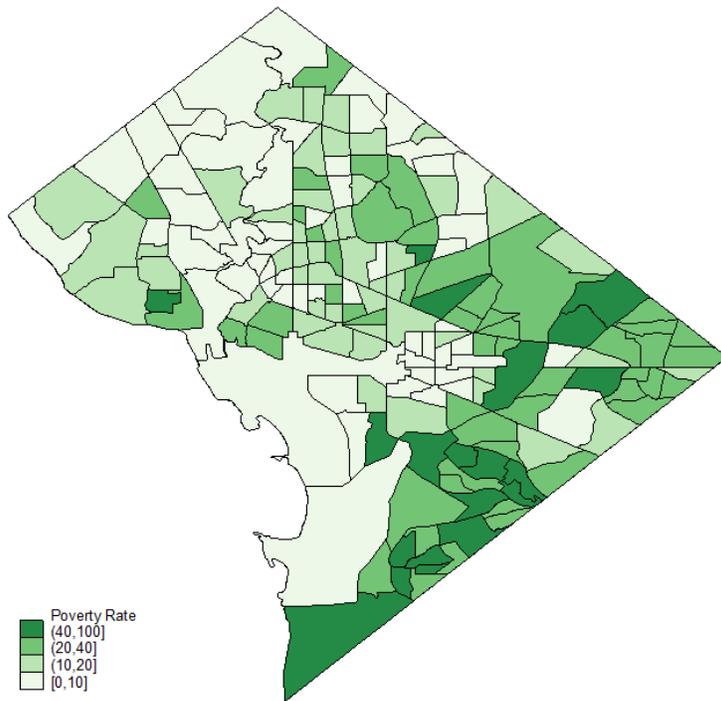
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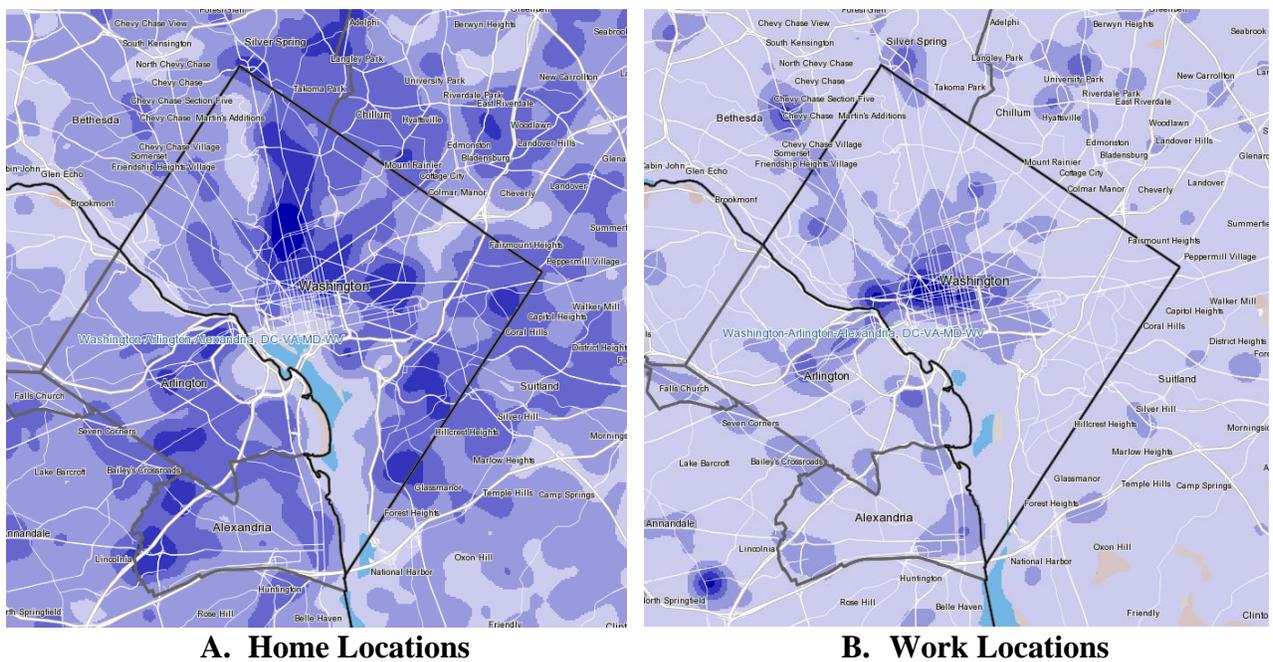
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Figure 1. Poverty Rates across Washington, DC Census Tracts



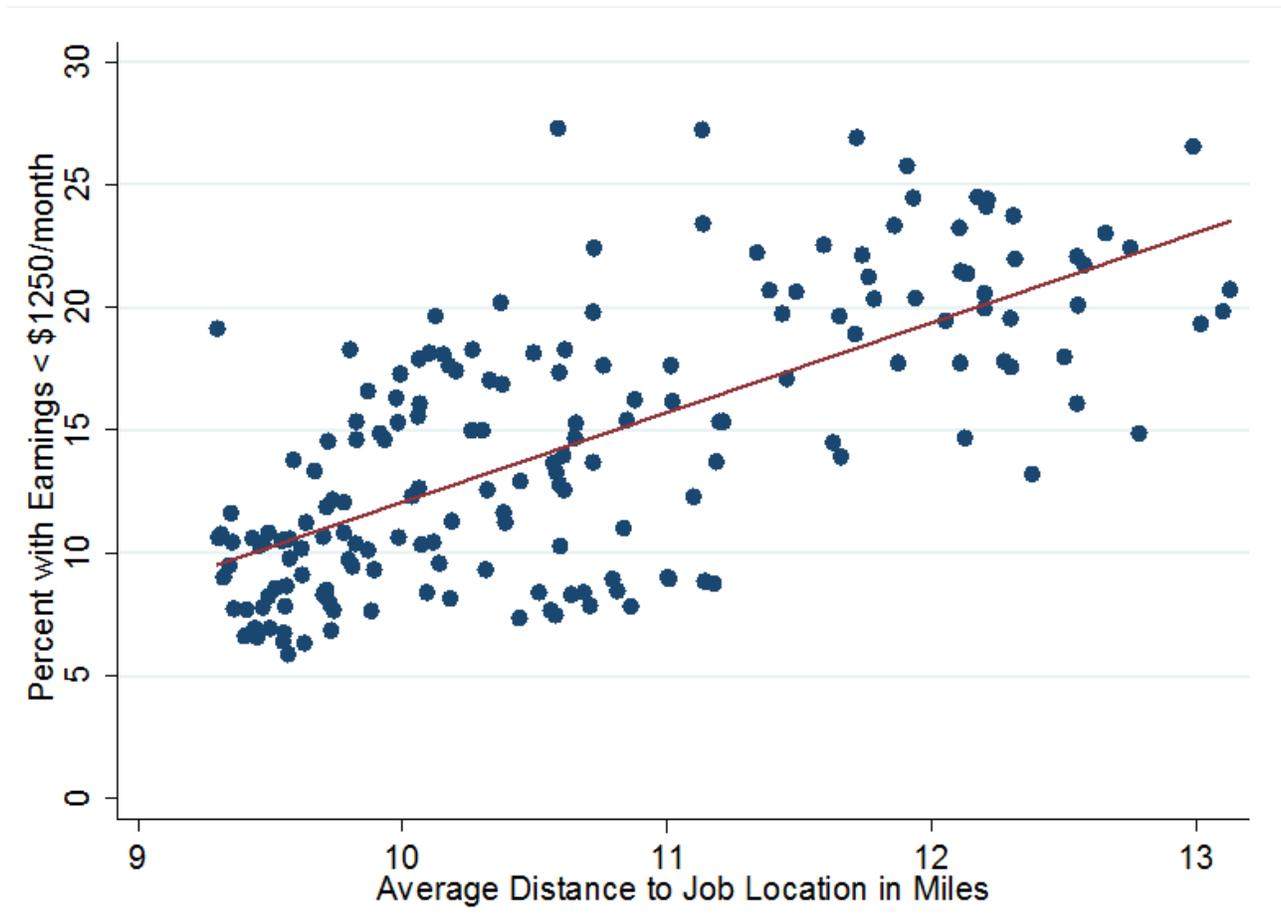
Source: American Community Survey, 2011-2015 5-Year Estimates.

Figure 2. Home and Work Locations of Workers Earning \$1,250 or Less per Month



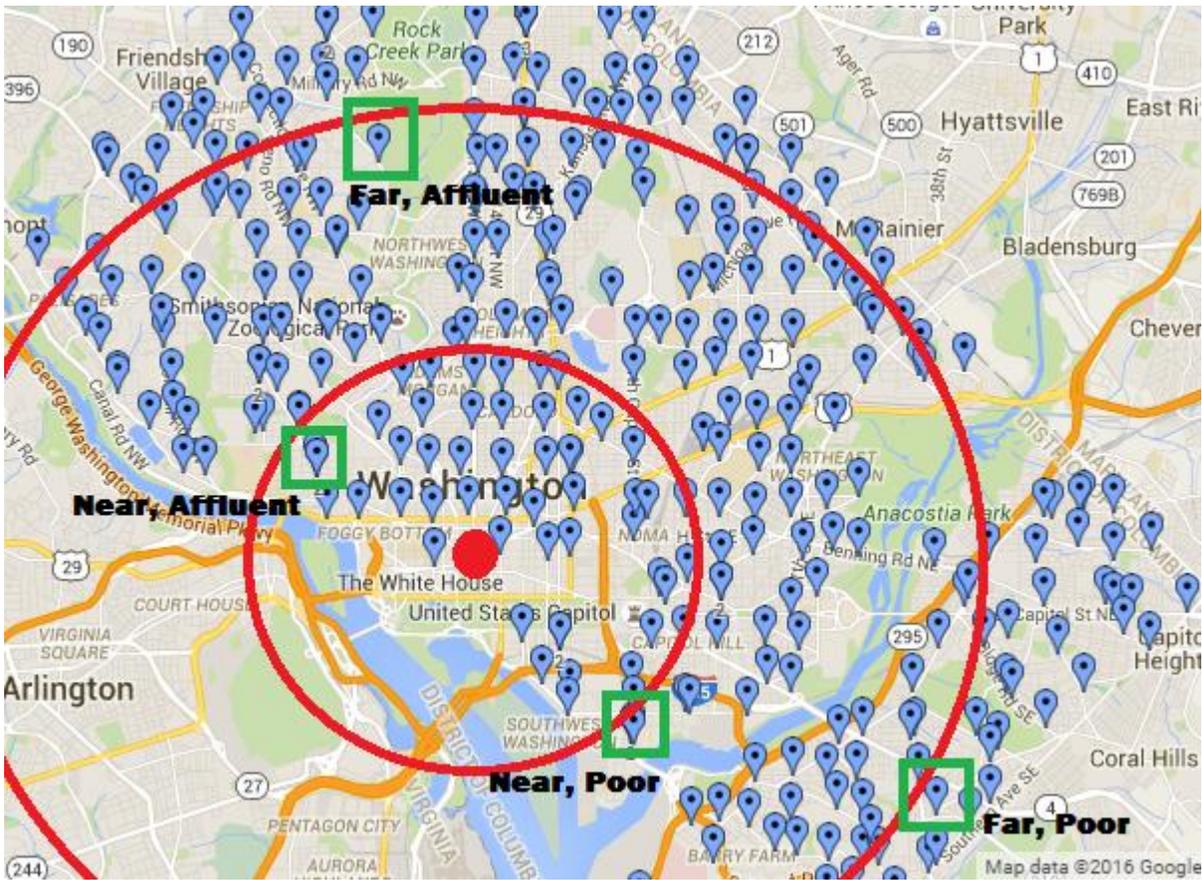
Source: 2014 Longitudinal Employer-Household Dynamics Data. Images from <http://onthemap.ces.census.gov/>

Figure 3. Low Wage Job Access and Earnings in Washington, DC Census Tracts



Source: Percent with earnings less than \$1,250 per month is calculated using 2014 LEHD data on primary jobs with workers whose residence is in the tract. The average distance between a tract and low wage jobs is calculated as a weighted average of distances between the tract of interest and other tract centroids, weighting each destination tract by the number of primary jobs with earnings less than \$1,250 per month with the employment location in the tract.

Figure 4. Identification Strategy



The large number of markers show the potential addresses that can be chosen for the experimental applicants. Job refers to a particular job location. The boxes near the indications for Near Affluent, Near Poor, Far Poor, and Far Affluent provide examples of addresses in the four treatment categories for this job location.

Figure 5. Locations of Residential Addresses in Experiment, by Treatment Type

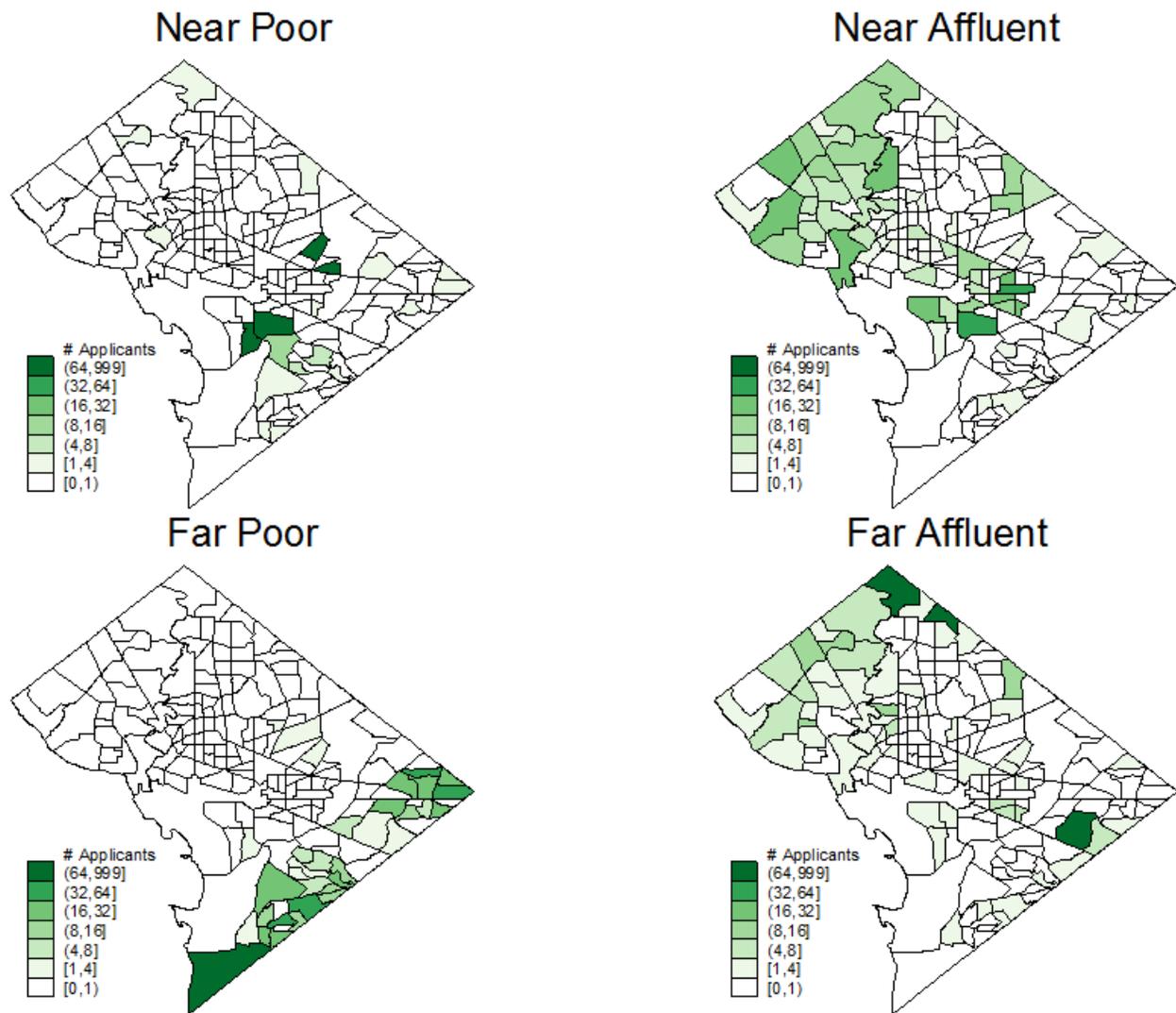


Figure 6. Locations of Jobs in Experiment

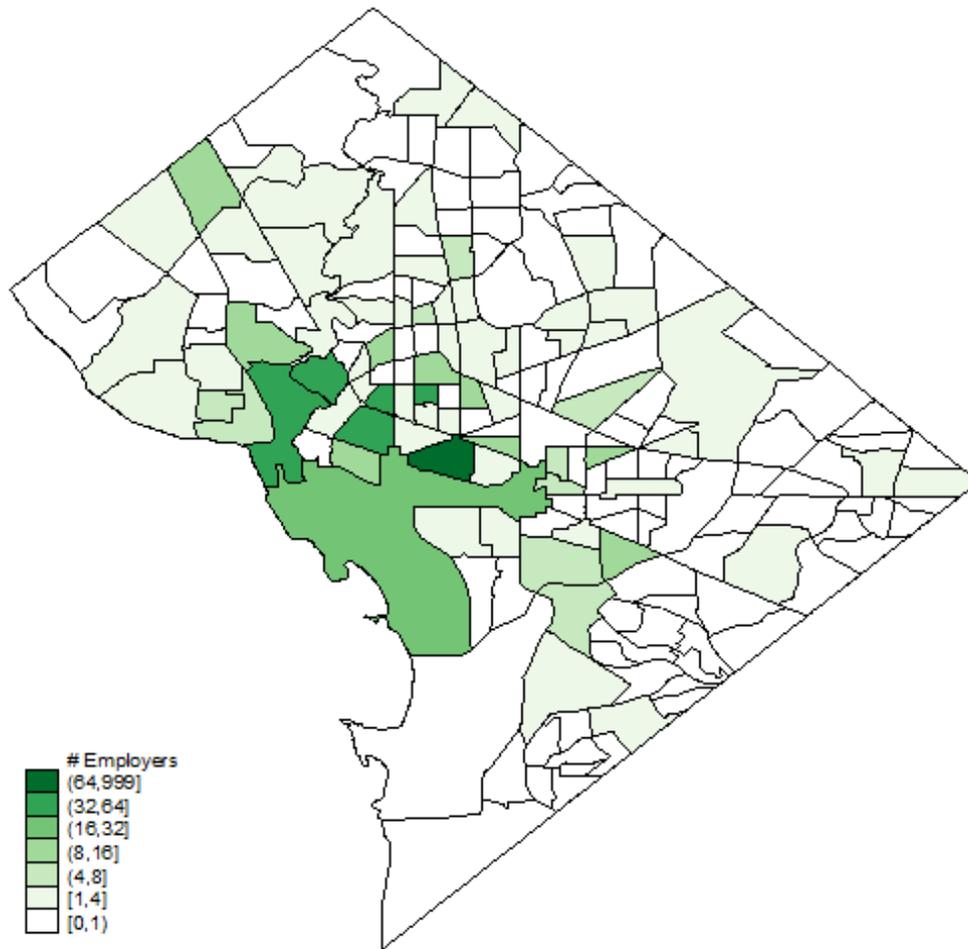
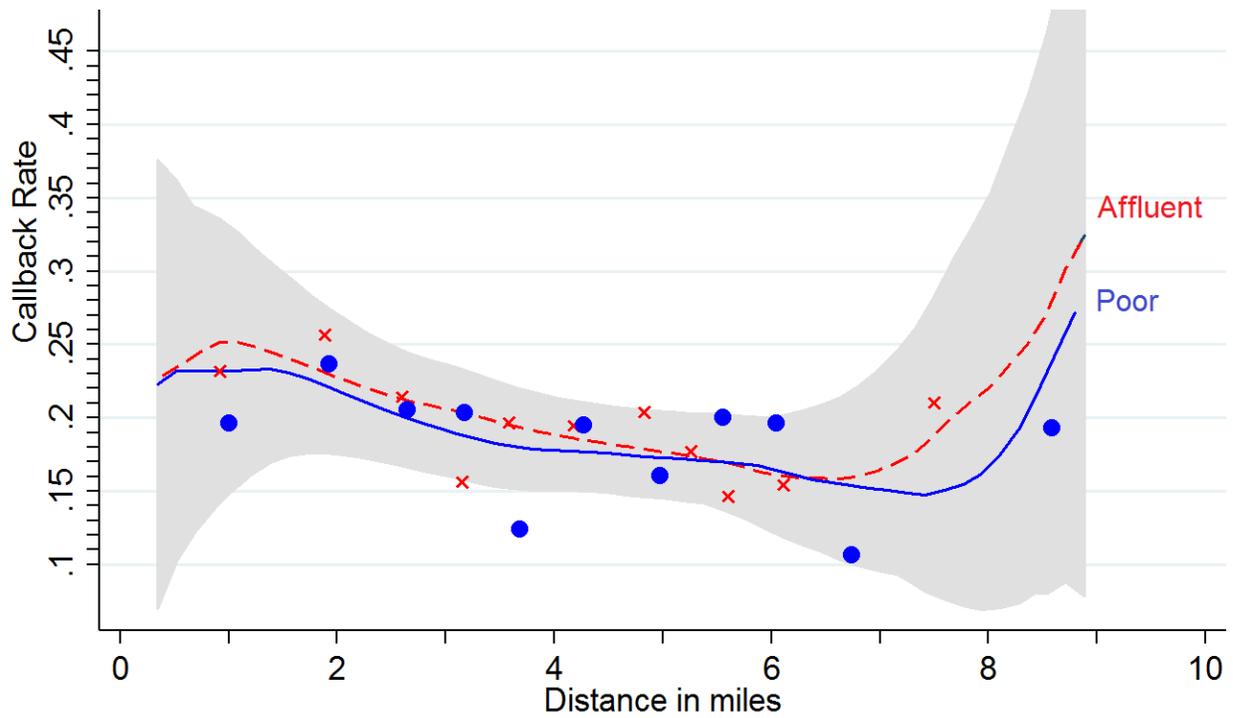


Figure 7. Callback Rates and Commuting Distance, by Affluence Level



Distance is measured as great circle distance between the employer's listed location and the residential location generated for the fictional applicant. Within affluent versus poor addresses, applicants are grouped into 20 equal ventiles by distance with callback rates within the group displayed as circles for poor addresses and X's for affluent addresses. The solid and dashed curves shows local linear fits for poor and affluent addresses, respectively. 95% confidence intervals for both lines are shown (overlapping) in gray.

Table 1. Summary Statistics and Baseline Balance		Near, Affluent	Far, Affluent	Near, Poor	Far, Poor	Far - Near	Poor - Affluent
A. Applicant Address							
Distance to Job (miles)	3.0 (1.3)	5.3 (1.1)	3.0 (1.3)	5.8 (1.3)	2.6 [0.000]	0.2 [0.001]	
Tract Median Income (\$)	101,698 (29,066)	106,371 (26,392)	32,429 (19,822)	27,812 (4,832)	28 [0.98]	-73,914 [0.000]	
Tract Percent Bachelor's or Higher	70 (16)	62 (18)	20 (18)	11 (5)	-9 [0.000]	-50 [0.000]	
Tract Percent White	60 (24)	31 (27)	9 (18)	1 (1)	-19 [0.000]	-40 [0.000]	
Tract Affluence Index	0.146 (0.018)	0.142 (0.019)	0.083 (0.021)	0.077 (0.005)	-0.005 [0.000]	-0.064 [0.000]	
B. Job Address							
Tract Median Income (\$)	95,672 (34,951)	95,672 (34,951)	95,672 (34,951)	95,672 (34,951)	0 [1.00]	0 [1.00]	
Tract Percent Bachelor's or Higher	74 (20)	74 (20)	74 (20)	74 (20)	0 [1.00]	0 [1.00]	
Tract Percent White	65 (22)	65 (22)	65 (22)	65 (22)	0 [1.00]	0 [1.00]	
C. Other Applicant Characteristics							
White	0.35 (0.48)	0.35 (0.48)	0.40 (0.49)	0.34 (0.48)	-0.03 [0.20]	0.02 [0.42]	
Black	0.36 (0.48)	0.42 (0.49)	0.35 (0.48)	0.41 (0.49)	0.06 [0.01]	-0.01 [0.65]	
Age	41 (11)	41 (12)	41 (11)	41 (12)	0 [0.56]	-1 [0.27]	
Work Gap (days)	88 (52)	91 (53)	89 (55)	95 (52)	5 [0.04]	2 [0.31]	
Work Experience (years)	8.1 (5.5)	7.7 (4.7)	8.0 (5.4)	8.2 (5.2)	0.0 [0.82]	0.2 [0.32]	
Female	0.49 (0.50)	0.49 (0.50)	0.54 (0.50)	0.48 (0.50)	-0.03 [0.11]	0.02 [0.31]	
Overall Quality (Predicted Callback Rate)	0.186 (0.107)	0.183 (0.109)	0.192 (0.105)	0.188 (0.111)	-0.003 [0.22]	0.005 [0.053]	
D. Outcome							
Callback Rate	0.207	0.177	0.195	0.170	-0.027 [0.03]	-0.010 [0.46]	
Sample Size	565	565	565	565			

The first four columns display means for each characteristic by treatment group. The final two columns measure differences in characteristics by regressing the variable of interest on a dummy variable for a poor address or a dummy variable for a far address, respectively. P-values are reported in brackets. Standard deviations are in parentheses. Standard errors for the final two columns are clustered by job. The overall quality variable predicts a callback dummy using a female name dummy, racial name dummies, age, years of listed work experience, length of work gap, and job profileXjob category dummies.

Table 2. Effect of Address Treatments on Employer Response

Dependent Variable:	Callback Dummy				
	(1)	(2)	(3)	(4)	(5)
Far	-0.027** (0.013) [0.03]	-0.030* (0.018) [0.09]	-0.027** (0.013) [0.03]	-0.024* (0.013) [0.06]	0.009 (0.033) [0.79]
Poor	-0.010 (0.013) [0.46]	-0.012 (0.018) [0.50]	-0.010 (0.013) [0.46]	-0.015 (0.013) [0.24]	-0.016 (0.013) [0.23]
Far X Poor	--	0.005 (0.024) [0.83]	--	--	--
Black	--	--	--	-0.060*** (0.016) [0.00]	-0.046 (0.028) [0.10]
White	--	--	--	0.011 (0.013) [0.54]	0.039 (0.029) [0.18]
Far X Black	--	--	--	--	-0.029 (0.043) [0.49]
Far X White	--	--	--	--	-0.061 (0.045) [0.18]
Job Fixed Effects	N	N	Y	Y	Y
Applicant Controls	N	N	N	Y	Y
Sample size	2,260	2,260	2,260	2,260	2,260

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Applicant controls include a female name dummy, racial name dummies, age, years of listed work experience, length of work gap, and job profileXjob category dummies. "Ambiguous" is the omitted racial name category. Standard errors are clustered at the job vacancy level. Selected p-values are in brackets.

Table 3. OLS and IV Estimates for Continuous Measures of Distance and Affluence

Dependent Variable	OLS	OLS	First Stage	First Stage	IV	IV
	Callback (1)	Callback (2)	Distance (3)	Affluence (4)	Callback (5)	Callback (6)
Distance to Job (miles)	-0.0085 (0.0053)	-0.0089* (0.0048)	--	--	-0.011** (0.005)	-0.011** (0.005)
Affluence Index	0.11 (0.19)	0.19 (0.19)	--	--	0.11 (0.21)	0.11 (0.21)
Far	--	--	2.56*** (0.05)	-0.005*** (0.001)	--	--
Poor	--	--	0.24*** (0.05)	-0.064*** (0.001)	--	--
Job Fixed Effects	N	Y	N	N	N	Y
Instruments	--	--	--	--	Far, Poor	Far, Poor
Kleibergen-Paap F- Statistic	--	--	--	--	3,742	3,211
Sample size	2,260	2,260	2,260	2,260	2,260	2,260

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Standard errors are clustered at the job vacancy level.

Table 4. Robustness of Instrumental Variables Results

Dependent Variable	Callback (1)	Callback (2)	Callback (3)	Callback (4)	Callback (5)	Callback (6)
Distance to Job (miles)	-0.011** (0.005)	-0.011** (0.005)	-0.010** (0.005)	-0.06 (0.12)	--	-0.013 (0.023)
Distance Sq.	--	--	--	0.006 (0.014)	--	--
Affluence Index	0.11 (0.21)	--	0.11 (0.21)	0.19 (0.29)	-0.09 (0.24)	0.09 (0.37)
Log Med. Inc.	--	0.006 (0.01)	--	--	--	--
Public Transit Travel Time (min.)	--	--	--	--	-0.003** (0.001)	0.000 (0.005)
Instruments	Far, Poor	Far, Poor	Far, Poor, Far*Poor	Far, Poor, Far*Poor	Far, Poor, Far*Poor	Far, Poor, Far*Poor
Kleibergen-Paap F- Statistic	3,742	3,978	2,609	29	171	10
Sample size	2,260	2,260	2,260	2,260	2,116	2,116

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively.

Standard errors are clustered at the job vacancy level. Sample sizes are lower when using the travel time measure because some addresses cannot be assigned a transit time by WMATA trip planner.

Table 5. Effect of Address Treatments on Employer Response, Applicant Address Controls

Dependent Variable: Callback Dummy	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Far	-0.024*	-0.030**	-0.052*	-0.074**	-0.058**	--	-0.025
	(0.013)	(0.014)	(0.028)	(0.030)	(0.025)		(0.22)
	[0.06]	[0.03]	[0.06]	[0.02]	[0.02]		[0.27]
Poor	-0.015	0.006	0.024	0.092	-0.006	0.094	-0.015
	(0.013)	(0.019)	(0.047)	(0.065)	(0.027)	(0.080)	(0.014)
	[0.24]	[0.74]	[0.61]	[0.16]	[0.84]	[0.24]	[0.25]
Far X Poor	--	--	--	--	--	-0.078	--
						(0.076)	
						[0.30]	
Far X Affluent	--	--	--	--	--	-0.073**	--
						(0.032)	
						[0.02]	
Average Distance to All Jobs	--	--	--	--	--	--	0.000
							(0.009)
							[0.96]
Job Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Applicant Controls	Y	Y	Y	Y	Y	Y	N
Applicant Address Controls	N	Quadrant FE	Tract FE	Address FE	N	Address FE	N
Sample	Full	Full	Full	Full	Far Varies within Address	Full	Full
Sample size	2,260	2,260	2,260	2,260	987	2,260	2,260

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Applicant controls include a female name dummy, racial name dummies, age, years of listed work experience, length of work gap, and job profileXjob category dummies. Standard errors are clustered at the job vacancy level. Selected p-values are in brackets.

Table 6. Different Outcome Measures

Dependent Variable:	Preferred Measure (1)	Interview Only (2)	Rejected Only (3)	Any Response (4)
Far	-0.027** (0.013) [0.03]	-0.027** (0.012) [0.03]	0.009** (0.004) [0.02]	-0.014 (0.013) [0.27]
Poor	-0.010 (0.013) [0.46]	-0.004 (0.011) [0.75]	0.004 (0.003) [0.29]	0.005 (0.013) [0.69]
Applicant Controls	N	N	N	N
Job Fixed Effects	N	N	N	N
Applicant Address Controls	N	N	N	N
Overall response rate	0.19	0.13	0.02	0.21
Sample size	2,260	2,260	2,260	2,260

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Applicant controls include a female name dummy, racial name dummies, age, years of listed work experience, length of work gap, and job profileXjob category dummies. Standard errors are clustered at the job vacancy level. Selected p-values are in brackets.

APPENDIX

A. Details of Experimental Design

A.1. Selection of Job Vacancies

A detailed experimental protocol defines the process by which research assistants apply to jobs. I apply to jobs requiring no more than high school education. I include eight different job low-wage job categories: administrative assistant (17%), cook (20%), fast food (13%), janitor (2%), building maintenance (6%), retail (14%), server (26%), and valet driver (2%). Each day, I randomly distribute a randomly ordered list of categories to each research assistant. Each research assistant identifies the most recent advertisement in their first assigned category on a popular website for posting job vacancies. Jobs must be located within the District of Columbia (not the suburbs), must request an e-mailed résumé or online application (not in-person application), must have an identifiable location, must not require more than high school education, and must not have been the subject of an application within the previous two weeks.¹⁷ If no new appropriate jobs have been posted in the job category, the research assistant moves onto their next category. Each research assistant continues through their list until meeting a daily quota of 2-4 new jobs. Altogether, this leads to a sample which is similar to other studies (e.g. Bertrand and Mullainathan, 2004) in that I focus on entry-level jobs but different from some studies to the extent that I filter out jobs with higher education requirements. Thus, I tailor the sample to fit an urban poverty research question by requiring limited formal education.

A.2. Designing Fictional Job Applications

For a given job, the research assistant generates fictional job applications in a manner similar to previous studies (e.g. Bertrand and Mullainathan 2004; Lahey 2008; Oreopoulos, 2011).

¹⁷ Eliminating the small number of repeat employers from the sample does not change the main results.

The research assistant sends four separate applications to the job, leaving at least one hour¹⁸ between applications. The four fictional applications include one of each address type (NP, FP, FA, NA) with specific addresses chosen according to the computerized algorithm described above and the sending order of the applications sorted randomly. Research assistants insert the four addresses into four different base résumé templates (e.g. font type, organization) drawn from online databases of job applicants and a local employment agency in DC. Occasionally, errors by research assistants in entering the inputs of the address selection algorithm result in incorrect address assignment. However, since address selection was completed correctly for 98% of applications, I will measure intent-to-treat effects using the intended address type.

I also randomly and independently assign applicant names, phone numbers, e-mail addresses, prior employment information, and education information to the four templates. Listed applicant names fall in three categories: white, black, or ambiguous. In each category there are male and female names. Each job vacancy receives applications evenly split between male and female. Each vacancy receives one name from all three racial categories with the fourth randomly selected from white or black. I use the same first names Bertrand and Mullainathan (2004) use to indicate stereotypically white or black first names. Ambiguous first names were drawn using data on baby names in New York City (NYC Open Data, 2009) and chosen to be common (at least 1,000 babies per year) and have as close to equal distribution as possible between black and white. White last names come from Bertrand and Mullainathan (2004) as do most black last names. Since they use fewer black last names, I supplement their last names with names from Social Security name data with the highest black-white ratio for names held by at least 160,000 people. Similarly, ambiguous last names are chosen from those held by at least 160,000 people with a black-white

¹⁸ I do not find any evidence that applications are too close together, raising suspicion about later applicants. I find no evidence that that measured treatment effects are different for earlier versus later applicants.

ratio near 1 to 2.¹⁹ Finally, I randomly assign first names to last names within the same ethnic group. E-mail addresses then correspond to the name on the résumé, and phone numbers are matched to a voicemail box with a generic message recorded by a person of the appropriate sex. I use eight phone numbers, one for each address type-sex combination. This ensures that all callbacks will be matched to the appropriate address type.

I design prior employment information to fit the low-wage jobs that are the subject of this study but also to indicate highly qualified applicants who should receive non-negligible callback rates. For each job category, the research team designed four separate work history profiles which are randomly assigned to the four different applications. Following the previous literature, we drew actual work histories from an online job applicant database (Indeed.com) from cities other than Washington, DC. Work histories were selected to include positive features such as experience relevant to the job category, promotion within the same organization, and increasing level of responsibility. We modified these work histories if necessary to reflect actual employers in Washington, DC and sometimes shortened job responsibility descriptions to fit our four templates. Work dates were chosen at random. First, the end date of the most recent job was determined by randomly drawing an ongoing unemployment duration of zero to six months from a uniform distribution. Then, the applicant shows continuous employment over three separate jobs. The length of each job was set to be at least 6 months and then randomly drawn from the empirical distribution of job lengths from the sample of low-wage job seekers in Phillips (2014).

¹⁹ The distinctively black last names used here and in other studies have roughly a 1 to 1 ratio. Altogether the names are: Black male: Tremayne Jones, Leroy Thomas, Rasheed Jackson, Jamal Coleman, Kareem Robinson, Darnell Washington, Hakim Harris, Jermaine James, and Tyrone Williams. Black female: Aisha Washington, Ebony Jackson, Keisha Robinson, Kenya James, Lakisha Harris, Latonya Thomas, Latoya Williams, Tamika Jones, and Tanisha Coleman. White male: Geoffrey Kelly, Jay Sullivan, Neil Baker, Todd O'Brien, Brett McCarthy, Brendan Murphy, Matthew Ryan, Brad Walsh, and Greg Murray. White female: Allison Sullivan, Anne Walsh, Carrie Ryan, Emily Murray, Jill Murphy, Laurie McCarthy, Kristen Kelly, Meredith O'Brien, and Sarah Baker. Ambiguous male: Tyler Richardson, Jason Brooks, Eric Scott, Antonio Sanders, Raymond Bell, Brian Mitchell, Richard Ford, Joel Butler, Kyle Davis. Ambiguous female: Alyssa Richardson, Ashley Brooks, Danielle Scott, Amanda Sanders, Morgan Bell, Brianna Mitchell, Erin Ford, Christina Butler, and Paige Davis.

I set education information to fit the low wage labor market. In particular, all résumés list only high school graduation. I list high schools that signal high quality by selecting four local parochial and public magnet schools and randomly assigning these to each application independent of all other characteristics. I also list a GPA selected from a random uniform distribution from 3 to 4 when the template requires it. The date of graduation communicates information about age, and I select it at random to match the distribution of ages in Phillips (2014). If the graduation date and work history conflict such that the person would be working as a child, I truncate the work history at age 16.

This process for setting names, contact information, work history, and education history encapsulates all information displayed on the fictional résumés. Beyond this information, jobs requesting an e-mailed résumé also require a cover letter. We compose four standard cover letters based on publicly available templates and randomly assign these to job applications. Some job vacancies require more extensive online applications asking further information. To meet this need, each work history profile also includes wage information (based on estimates from glassdoor.com) and reasons for leaving each job. Each fictional applicant is also assigned three references from the names not used for other applicants to the job. These applications often require either an IQ/skills test or personality questionnaire which is completed by the research assistants in a manner communicating a high quality applicant (i.e. to the best of their ability). For any other question idiosyncratic to the specific job application, the research assistant composes four different answers and randomly assigns them to the different applications.

Appendix Table 1. Probit Predicting Callback Dummy Using Bertrand and Mullainathan (2004) Data

	(1)
Tract Median Income (\$)	0.17 (0.12)
Tract Percent Bachelor's or Higher	0.25 (0.20)
Tract Percent White	0.01 (0.11)
Sample size	4,784

Probit index coefficients reported. Standard errors in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively.

Appendix Table 2. Correlation Matrix for Different Affluence Measures

	All Three Factors	Race Only	Income Only	Schooling Only
All Three Factors	1.00			
Race Only	0.92	1.00		
Income Only	0.99	0.89	1.00	
Schooling Only	0.25	0.24	0.24	1.00

Appendix Table 3. Coefficients for Quality Index – Full Sample

	(1)
White	0.009 (0.021)
Black	-0.061*** (0.021)
Female	0.029* (0.016)
Work Experience (Years)	-0.001 (0.002)
Age (Years)	-0.0016** (0.0007)
Work Gap (Days)	-0.0000 (0.0002)
Occupation X Profile Dummies	YES
R^2	0.08
Sample size	2,260

OLS Regression of response dummy on the listed variables. Standard errors in parentheses. Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively.