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 further from the job location. Fictional résumés randomly assigned to have addresses far from the job location receive 14% fewer callbacks than nearby addresses. Living 5-6 miles away from the job results in a penalty equal to that received by applicants with stereotypically black names. Because commute distances and neighborhood poverty tend to be correlated, this effect can account for two-thirds of discrimination against applicants from poor neighborhoods measured in previous experiments.

JEL Codes: J7; R2; J6

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1. Introduction

The urban poor tend to be concentrated in a small number of neighborhoods (Kneebone, 2014) that tend to be far from employment opportunities. In Washington, DC the proportion of the residents of a census tract with a college degree drops by 13 percentage points for each mile further away from the average job. Proponents of the "spatial mismatch hypothesis" (Kain, 1968; Wilson, 1997) argue that the tendency of the urban poor to live far from jobs makes it difficult for residents to find employment, perpetuating poverty. A large non-experimental literature finds evidence of such effects, but many researchers have discounted neighborhood effects more broadly and spatial mismatch in particular due to null employment effects from subsidized housing moves in the Moving to Opportunity (MTO) experiment (Kling et. al. 2007; Ludwig, et. al. 2012). Recent evidence indicates a stronger role for neighborhood effects among young children in MTO (Chetty, et. al. 2015) and more broadly (Chetty and Hendren, 2015). As stated by Wolfers (2015), "[Chetty et. al. (2015)] transforms what was previously seen as influential evidence that neighborhoods are unimportant into the more nuanced finding that moving *while young* can be tremendously beneficial" (emphasis added). However, this new evidence has not touched the debate on whether urban geography matters for adult employment.

In the present study, I contribute to this debate by testing whether one potential mechanism of spatial mismatch, employer discrimination[2] according to residential location, holds empirical relevance. Employers may discriminate against applicants who live far away because long commutes lower productivity (Zenou, 2002) or because distance statistically correlates with important factors not observed by employers (Phelps, 1972). Whether employer discrimination, out of many possible mechanisms (Gobillon, et. al. 2007), drives spatial

[2] Throughout, I use the term "discrimination" in the general sense defined by Lang (2007): "we will use discrimination to refer to any situation in which [two groups of people] with identical observed characteristics have systematically different outcomes." This includes taste-based, statistical, and other varieties of discrimination.

mismatch effects matters when designing public policy on multi-dimensional "neighborhood quality." For instance, Quigley et. al. (2008) document that MTO moved residents from high poverty public housing complexes to low poverty but nearby neighborhoods. While potentially beneficial in other ways, such a program would not affect employer discrimination based on distance to the job. Thus, the main goal contribution of this paper is to act as a mechanism experiment (Ludwig, et. al., 2011) testing whether employer discrimination based on commute distance represents an empirically relevant mechanism of spatial mismatch.

I use the standard correspondence study methodology[3] to examine the low-wage labor market in Washington, DC, sending 2,260 fictional résumés to actual job vacancies. I confirm that employers discriminate according to the residential location of job applicants. Employers call back résumés listing residential addresses in distant poor neighborhoods less frequently than those listing addresses in nearby affluent neighborhoods. To test for my hypothesis regarding commute distance, though, I need to examine whether distance contributes to employer discrimination separate from other attributes such as neighborhood affluence. I send applications listing addresses in distant and near neighborhoods matched to have similar levels of affluence (education, income, and racial composition). Addresses far from the job location receive 14 percent fewer callbacks than addresses in nearby neighborhoods matched to have similar levels of affluence. Alternatively, I estimate how callback rates relate to commute distance, instrumenting this continuous measure with randomly assigned address types. Every mile an applicant moves away from the job callback rates fall by 1.1 percentage points.

[3] 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-Gomex and Campos-Vazquez, 2014), immigrant status (Oreopoulos, 2011), unemployment duration (Eriksson and Rooth, 2014; Kroft, et. al. 2013), and age (Lahey, 2006).

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 effects, I also measure the standard discount in callback rates for stereotypically black names (Bertrand and Mullainathan, 2004). Applicants living 5-6 miles further from the job face approximately the same discount as applicants with stereotypically black names. Alternatively, I use ACS data to measure that, even within the compact city limits of Washington, the average non-white person lives 1.1 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 24% beyond the direct response to different names. Employers respond to distance in a manner that is empirically relevant.

I find suggestive evidence that employers care about distance not only in the absolute but also more than neighborhood affluence. In the experiment, I also match addresses of different affluence but similar distance to the job. Employers respond more positively to affluent neighborhoods, though these effects are statistically insignificant. Even so, I gauge the relative magnitudes of employer responses to distance and affluence. Consider someone who moves to a more affluent but distant neighborhood. I find that moving an applicant to a neighborhood one standard deviation more affluent but also one standard deviation more distant from the job receives 4 percentage points fewer callbacks, though this difference is only significant at the 10% level. The data provide clear evidence that distance matters and some additional suggestive evidence that distance matters more than affluence.

My results contribute to a small but growing experimental literature on discrimination based on 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.[4] Beside context, these studies differ in how they treat commute distance. Tunstall, et. al. (2013) send applications matched to have similar commute distances. Bertrand and Mullainathan (2004) do not control for distance[5] and thus measure a combination of distance and affluence effects. For comparison, I measure these combined effects in the present study. I find combined effects of the applicant's address of the same magnitude as Bertrand and Mullainathan (2004), and commute distance accounts for two thirds of employers' response.

The main goal of this paper is to measure whether employers discriminate by distance rather than explain why they do; however, I investigate this latter question to the extent possible. First, a straightforward model of statistical discrimination would imply that the distance penalty would depend on the availability of other jobs near the applicant's address, but I find no such evidence. Second, I find that low and high quality applicants experience similar decreases in the level of their callback rates. Thus, low quality applicants face a much larger proportional penalty, which could support a model in which long commutes lower productivity, affecting the viability of marginal applicants. However, I also find that employers respond more sensitively to "straight line" distance than to public transit travel time. Thus, a productivity mechanism consistent with the data requires employers that can only noisily map addresses to commutes.

In what follows, section 2 details the geography of employment in Washington, DC and the relevant literature. Section 3 describes the design of the experiment, and section 4 presents the results. Section 5 investigates possible mechanisms, and section 6 concludes.

[4] 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.
[5] Of course, given that their study focuses on racial discrimination this is not a flaw in their study.

2. Background

2.1. Context

Neighborhood poverty correlates strongly with geographic access to jobs in Washington, DC, as in many other cities. Figure 1 displays the poverty rate for different zip codes across the city. The red line displays the outline of the city itself with the Virginia suburbs beyond the Potomac River to the south and Maryland suburbs to the north and east. Poverty rates display a strong tendency to increase as one travels south and east, as evidenced by the darker shades for those zip codes. The three zip codes just inside the southeast boundary of the city coincide with the part of the city beyond the Anacostia River and exhibit the city's highest poverty rates.

The same neighborhoods tend to be distant from job locations as well. Figure 2 displays a heat map of job locations for the job vacancies used in this experiment (see below for definition of sample). Jobs cluster downtown as evidenced by the large dark circles in this area. Notably, few firms locate jobs east of the Anacostia River. Thus, these areas remain both high poverty and distant from job vacancies. In Figure 3, I summarize this relationship for all census tracts in the city. The data exhibit a strong negative relationship between the fraction of residents with at least a bachelor's degree and the average distance from the tract centroid to the jobs in my experimental sample. On average, being one mile further from the average job is associated with 13 percentage points fewer people with college degrees. Similar results obtain for tract median incomes or fraction white.

2.2. Spatial Mismatch: The Job Applicant's Decision

The observed negative correlation between distance to employment and neighborhood affluence motivates the spatial mismatch hypothesis. Kain (1968) and Wilson (1996) argue that concentrated poverty directly results from living in neighborhoods that are geographically far from job vacancies. A large empirical literature debates whether and in what contexts spatial mismatch effects actually exist. The large scale Moving To Opportunity experiment found no effect of housing moves on employment (Ludwig, et. al. 2012) while many studies with observational data find negative effects of spatial mismatch (Aslund, et. al. 2010; Sanchis-Guarner, 2014; Andersson, et. al., 2014; Miller, 2015). Others argue that spatial mismatch effects only matter in interaction with race (Hellerstein, et. al. 2008).

Given disagreement regarding the overall effects of spatial mismatch, one useful path forward is to more directly test whether potential mechanisms behind spatial mismatch are operational. Many different mechanisms could generate such an effect (Gobillon, et. al. 2007). From the point of view of the worker, job search could be less effective in distant locations due to transportation costs, lack of information, or more limited job search networks. The ineffectiveness of search could then lead those living in distant neighborhoods to reduce their search intensity. Phillips (2014) and Franklin (2014) both find evidence in field experiments that subsidizing transportation costs can induce greater search intensity for those living in neighborhoods far from jobs. Standard search models also predict that workers will require higher wages to work further from home (Zenou, 2009), and empirical evidence from firm relocations supports this idea (Mulalic, Van Ommeren, and Pilegaard, 2014). If the wage is inflexible job applicants may also reject job offers far from home, never apply in the first place, or quit jobs when the location changes because commuting costs erode net take-home pay. For instance, Zax and Kain (1996) find that firms moving to the suburbs tend to lose black employees. All of these mechanisms match the general empirical finding that workers tend to search for jobs close to home (Manning and Petrongolo, 2013; Marinescu and Rathelot, 2013), and this is especially true for poor minority workers (Holzer and Reaser, 2000). Thus, living in a neighborhood far from job vacancies could limit job prospects by affecting the job applicant's behavior.

2.3. Spatial Mismatch: Employer Discrimination

On the other hand, employer behavior could also generate spatial mismatch effects if employers discriminate based on the residential location of the job applicant (Zenou and Boccard, 2000). I will focus on testing this mechanism. Bertrand and Mullainathan (2004) send matched fictional résumés to real jobs and find that employers are less likely to call back applicants who list addresses in neighborhoods that have lower income/education/fraction white. Employers may not care about neighborhood attributes per se but still engage in statistical discrimination by poverty or any other fixed neighborhood attribute that can be extracted from a residential address. They may use neighborhood poverty to proxy average productivity differences in workers across neighborhoods (Phelps, 1972). However, as documented in the previous section, commuting distance and neighborhood affluence are strongly correlated. Observed employer discrimination against applicants from poor neighborhoods could result from employers discriminating against applicants who live far away from the particular job in question rather than discrimination based on any fixed neighborhood attribute. Supporting this theory, Tunstall et. al. (2013) find no difference in callback rates for job applicants listing addresses in neighborhoods with differing levels of poverty but the same distance from the job.

Employers may wish to account for commuting distance when making hiring decisions for various reasons. First, employers may be concerned that long commutes directly decrease productivity due to fatigue or unreliability of public transit systems (Zenou, 2002). On the other hand, employers may be aware of the effect of long commutes on the employee's behavior, leading to concerns that applicants with high commuting costs will not attend an interview, not accept the job, or quit the job in the future. Both of these mechanisms could generate discrimination by employers based on commute distance. However, these two mechanisms should have differing effects on observably more productive versus less productive applicants. In an environment with a binding minimum wage, employer concerns about direct productivity effects should fall most severely on low quality applicants for whom transit-related productivity losses causes their productivity to fall below the minimum threshold required to be hired. On the other hand, attractive applicants with better outside options should face greater distance-related discrimination if employers are concerned about distant applicants quickly quitting in favor of a new job. Finally, employers may also use observable information about commuting distance to statistically discriminate in either direction (Heckman, 1998), concluding either that distant applicants are very persistent or have already been rejected by local employers.

In sum, employers may wish to discriminate according to an applicant's residential location for many different reasons. They may wish to discriminate against applicants from poor neighborhoods, or they may wish to discriminate against applicants who live far from the job. Economic theories of discrimination provide ample justification for either possibility. Because distance to employment tends to negatively correlate with neighborhood affluence, either of these mechanisms will lead to employers calling back applicants from poor neighborhoods at lower rates. In the present study, I undertake an experiment to disentangle these two effects and isolate whether employers discriminate by commute distance.

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 send 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 according to 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, these will all be generally referred to as "callbacks" and "responses." 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 focus on the address listed at the top of the résumé. The natural occurrence of such addresses on résumés provides a straightforward way to manipulate employer perceptions of the applicant's residential location. Importantly, the residential address provides information to the employer regarding both the affluence of the applicant's neighborhood and the applicant's commute distance. As noted above, these two characteristics tend to be correlated with each other such that employer discrimination based on one cannot be, in general, disentangled from discrimination based on the other. Thus, I adopt a 2x2 research design to separately vary neighborhood affluence and commute distance. I randomly assign each job application to have an address in one of four categories: near and poor (NP), near and affluent (NA), far and poor (FP), or far and affluent (FA). Greater detail for how these addresses are chosen can be found in the Appendix. Figure 4 summarizes my strategy graphically. I choose addresses from a grid so that NA and NP are the same distance from the job. Comparing callback rates for such addresses allows me to measure the effect of neighborhood affluence separately from commuting distance. The same holds for types FA and FP, and greater statistical precision can be obtained by pooling NA and FA types and comparing to NP and FP types. Likewise, I match NP and FP addresses to be of similar affluence as measured by an index of neighborhood income, education, and racial composition. Distance effects can then be measured by comparing callback rates for types NP and FP, which are both addresses in poor neighborhoods but differ in their distance to the job site; likewise for types NA and FA.

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 in fact further away at 5.3 miles and 5.8 miles. The final two columns measure the pooled differences between treatment types. 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 both meaningful and reasonable. For instance, 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 2011 matched employer-employee Longitudinal Employer-Household Dynamics dataset indicate that

22% of workers living in DC southeast of the Anacostia River commute more than 10 miles to work.[6] Finally, I can compute average travel distance to the jobs in this study for the overall white and non-white populations in Washington, DC. I take ACS data on population by census tract and assume that all individuals live at the tract centroid. I compute the average distances to the jobs in my sample for white and non-white populations. Non-white (mostly black) individuals in DC would travel on average 3.8 miles to the jobs in this study while the average white individual would need to travel only 2.7 miles. Thus, the experimental manipulation of commute distance is about twice the black-white job access gap in DC. Overall, the "near" addresses can be thought of as being a similar to the average white person's commute. The "far" addresses in this study can be thought of as a longer than typical commute for an average minority person or a typical commute for a disadvantaged, low-wage, minority worker. Thus, the experimental variation in commute distance meaningfully captures the difference in job access for those living in areas of concentrated poverty.

The experiment manipulates affluence a bit more strongly but still within a reasonable range. The $74,000 increase in median income represents a move from the 17th percentile of census tract median income to the 85th percentile. A 50 percentage point increase in college completion moves from the 20th percentile of census tracts to the 71st percentile. A 40 percentage point increase 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.

The research team conducted a small-scale public survey in Washington, DC to confirm that such variation in actual attributes of addresses leads to perceived differences. A sample of

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

52 individuals were each presented with 2 addresses in Washington, DC and prompted to respond to a series of questions regarding their characteristics. Respondents demonstrated knowledge of both location and affluence subject to some noise. Travel time (p-value = 0.02), neighborhood median income (0.23), fraction college educated (0.03), and fraction white (0.01) all correlate positively with actual values.[7] Combined with the documented variation in actual commuting distance and neighborhood affluence, these survey results allow us to reasonably conclude that the experiment shifts perceptions of hiring managers observing résumés.

Table 1 demonstrates one challenge inherent in the process of selecting addresses: the addresses are not perfectly matched. For instance, addresses classified as poor versus near should be the same distance from jobs. In fact, poor addresses are 0.2 miles further away. 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 Appendix for details). The matching process does, though, significantly reduce the correlation between commuting distance and neighborhood affluence. Median income and distance to the job are no longer correlated. Even when correlation between distance and measures of affluence remain, it has been reduced. Recall from Figure 3 that in a representative sample of addresses, an address one mile further from the average job tends to be in a neighborhood with 13 percentage points fewer people with college degrees. In the experimental sample this falls to 3.5 percentage points per mile $\left(\frac{9}{2.6}\right)$. Thus, the confounding relationship between distance and educational attainment has been deflated to at least one quarter of its original magnitude. In any case, I will check the results for

[7] More detailed results available upon request.

robustness to these small differences in address characteristics by including applicant address fixed effects in some specifications and implementing an instrumental variables framework that allows each treatment type to affect both affluence and distance.

3.2. Designing Fictional Job Applications

The research team composes fictional job applications in a manner similar to previous studies (e.g. Bertrand and Mullainathan 2004; Lahey 2008; Oreopoulos, 2011). A detailed experimental protocol defines the process by which research assistants apply to jobs. 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 different job categories and only jobs with lower skill requirements than previous studies.

I generate fictional applicants with only high school education and do not apply to jobs requiring more than high school. Eight different job low-wage job categories (administrative assistant, cook, fast food, janitor, building maintenance, retail, server, and valet driver) are randomly distributed to different research assistants and randomly ordered. 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. 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. Using different job categories and a lower level of education leads to a pool of jobs substantially different from previous studies. Even in situations when the job categories of the present study overlap with previous studies (e.g. retail and administrative), the education requirement leads my team to apply to a different subset of such jobs. Thus, I tailor the sample to fit an urban poverty research question by requiring limited formal education.

Once a job vacancy has been identified, the research assistant sends four separate applications to the job with at least one hour between each application. The four fictional applications include one of each address type (NP, FP, FA, NA) with specific addresses chosen according to the computerized algorithm described in the Appendix and the sending order of the applications sorted randomly. Research assistants insert the four addresses into four different résumé designs drawn from online databases of job applicants and a local employment agency in DC. Occasionally, errors 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.

The templates also require applicant names, phone numbers, e-mail addresses, prior employment information, and education information. 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 list to include a few more last names that have the highest ratio of black to white with at least 160,000 people having the last name in Social Security name data. Similarly, ambiguous last names are chosen to have at least 160,000 people having the name and a black to white ratio of close to 1 to 2.[8] 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 voicemail boxes in total so that an application can be matched both by the sex of the applicant and by each of the four address types. 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

[8] Because 1 to 1 essentially gives the list of black last names. 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.

was determined by randomly drawing a current 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 of the group of low-wage job seekers in the sample of Phillips (2014).

I set education information to fit the low wage labor market. In particular, all résumés list only high school graduation. This differs significantly from the previous literature, which studies college graduates or applicants with some college. I do, though, list high schools that signal high quality by selecting four schools from local parochial schools and public magnet schools and randomly assigning these to each application. 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 experiment 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.

Altogether, the research team sent 2,260 fictional applications to 565 job vacancies.[9] The final two panels of Table 1 present summary statistics of the various résumé characteristics as well as their balance across address treatment types. Panel B shows characteristics of the job location's census tract. Jobs tend to be in high-income, well-educated, and white neighborhoods near downtown. These variables are perfectly balanced by construction because I stratify the address treatments by job vacancy. 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

[9] 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.

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 the 8 different job types. This model and results are discussed in Appendix A.3 and Appendix Table 3. In general, employers respond to these applicant characteristics in a manner consistent with previous studies. The fitted values of this regression measure the overall quality of non-address-related characteristics on the résumé. The final row of Table 1 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 and small. 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 is a dummy for the Far Poor and Far Affluent treatments; $Poor$ is a dummy for the Near Poor and Far Poor treatments, and ϵ 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. Given the 2x2 experimental design, I can also allow for an interaction between the distance and affluence treatments.

For interpretive reasons, consider also continuous measures of treatment:

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

$Distance$ is the great circle distance between address a and job j while $Affluence$ can either be the affluence index described in the appendix or any of its components: log median income, fraction white, or fraction college educated. These continuous treatments are only partially randomly selected. For example, commute distance depends on both the randomly assigned treatment types and the extent to which the employer location and city boundaries limit the maximum distance. Thus, I estimate equation (2) by instrumental variables, using the randomly assigned treatment status to instrument for the 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 instrumental variables setup in equation (2) naturally tests whether my results conflate distance effects with imbalanced neighborhood attributes in the affluence index. In the IV setup, I allow all treatment types to instrument for both commute distance and neighborhood affluence. Thus, the estimates in (2) will, subject to a linearity assumption, parse the extent to which measured treatment effects are robust to accounting for imperfect matching. Second, I revise my main specification to include address fixed effects. I estimate the following equation.

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

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 near downtown can be "near" for downtown jobs and "far" for jobs near city limits. I include job vacancy fixed effects ψ_j to avoid confounding distance with fixed employer attributes and applicant controls X_{aij}. Equation (3) identifies the effects of distance using variation generated by the interaction of applicant and job location. 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 shows the simplest presentation of the experimental results. As expected, near affluent addresses have the highest callback rate at 0.207, and near 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 neighborhood affluence and commute distance measurably contribute to this discrimination. Near poor addresses receive only a slightly lower callback rate of 0.195 compared to the 0.207 rate of near affluent addresses, indicating a 1.2 percentage point decrease in callback rates for an applicant living in a neighborhood with a similar commute time but lower affluence level. 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 neighborhood, holding commute distance constant. Discrimination against applicants listing distant addresses appears roughly three times larger. Applicants from far affluent addresses are called back at a rate 3.0 percentage points lower than those with near affluent addresses, and 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). 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 for 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. Column (2) shows no evidence that the two treatments interact significantly; hence, I focus on the pooled results which have greater statistical power. Column (3) includes 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) and job fixed effects. 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 commuting distance; the evidence for discrimination by neighborhood affluence remains weaker.

For comparison, I also display the coefficient on a dummy for having a stereotypically black name. The coefficient of -0.060 indicates that individuals with black names receive 6.0 percentage points fewer callbacks than those with ambiguous or white names. Since there is no difference between white and ambiguous name callback rates, this can also be interpreted as the standard white/black difference. An applicant living 2.6 miles further from the job receives at least 2.4 percentage points fewer callbacks, i.e. 40% of the penalty received by stereotypically black names.

4.2. Continuous Measures of Distance and Affluence

To draw a more direct comparison with the previous literature and to provide a robustness check against imperfect matching of treatment types, I can replace the 2x2 design and treatment dummies with an instrumental variables approach using continuous measures of distance and affluence. I measure commuting distance as the great circle distance between the employer's listed address and the generated applicant address. Figure 5 demonstrates that callback rates correlate with distance to the job. Interestingly, callback rates show a generally monotonically decreasing trend for distances out to about 7 miles, only showing an uptick for the furthest 5% of addresses. Lowess-smoothed callback rates fall from about 0.24 in the immediate neighborhood of the job to about 0.17 at 7 miles away. This drop approximately matches the 6.0 percentage point callback penalty incurred by having a stereotypically black name.

Table 3 examines these continuous measures of treatment within the instrumental variables framework as in equation (2). I use the treatment categories to instrument for continuous measures of distance and affluence because some variation in the continuous measures depends on the job location. The first two columns of Table 3 show the first stages of the IV setup. 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 previously documented imperfect matching of distance across affluent and poor types. Similarly, the second column shows that the poor dummy provides the stronger instrument for affluence though the far dummy also matters. Allowing both treatment dummies to be instruments for both distance and affluence provides an additional check that imperfect matching does not drive our results. The IV estimation will account for the extent to which the randomized applicant types crossover and affect the other continuous treatment measure.

Using the instrumental variable framework, I find that continuous measures of treatment provide similar results to the simple analysis of treatment categories above. As shown in Column (3), a 1-mile increase in distance to the job decreases callback rates by 1.1 percentage points. This difference is statistically significant at the 5% level. Distance still matters allowing for an IV framework in which all treatment types may affect both distance and affluence. The affluence measure again shows no statistically significant relationship with callback rates. These results hold constant across different specifications. Column (4) shows similar results using log median income instead of the affluence index.[10] Column (5) 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. Finally, Column (6) 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.

The continuous measure of distance provides a natural way gauge the magnitude of the estimated effects. Commute distances for far and near types differ on average by 2.6 miles. This difference implies a gap in callback rates of 0.029 (2.6*0.011=.029) between far and near types, which matches the results from above. 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,

[10] Results are also similar for fraction white and fraction college-educated.

DC could contribute noticeably to racial differences in the labor market, not just at the lower tail but even at the mean. As documented above, the average non-white person in Washington, DC lives 1.1 miles further from the jobs in this study than the average white person does. According to the model in column (3) of Table 3, the average black person would thus receive about 1.2 percentage point fewer callbacks than the average white person if only residential location mattered. This gap represents 24% 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 (3) of 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 with Washington, DC, and a move to the suburbs would likely involve similar changes in affluence but a much larger change in distance to jobs. Nonetheless, changing this combination of attributes would decrease callback rates by 0.04 with statistical significance at the 10% level. While the comparison is somewhat noisy, it suggests that distance matters significantly 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:

$$Y_{aij} = \alpha_0 + \alpha_1 Log\ Median\ Income_a + u_{aij}$$

They estimate α_1 at 0.018, indicating that doubling median income drops 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:

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

I draw the first two values from column (4) of Table 3. I compute the distance-income gradient using a simple regression across all census tracts in DC as in Figure 3 and find that doubling neighborhood income is associated with being on average 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 of the coefficient on log median income of which 0.012 results from commute distance. The overall effect matches exactly their results from an earlier time period in Boston and Chicago. The distance and affluence effects I measure can explain the previously documented relationship between callback rates and neighborhood income, and commute distance contributes roughly 2/3 of this effect.

4.3. Controlling for Fixed, Address-Specific Attributes

As noted above, I can use applicant address fixed effects 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. Such an issue faces 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. Due to variation in the location of the employer, 987 out of 2,260 applications are assigned to applicant addresses that are sometimes classified as "far" and other times as "near." The 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. Using only variation in commute distance within the same listed address, I can measure a pure distance effect separately from all fixed attributes of the address.

Column (4) of Table 2 displays the results for the regression including address fixed affects. The results allay concerns that distance discrimination has been conflated with imperfect matching, either due to observed or unobserved variables. The measured effect of distance actually gets much stronger. Even with a much larger standard error, it 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 if anything underestimate discrimination by commute distance.

The results for affluence in column (4) provide less helpful information. While assignment to far versus near types can vary with the location of the job leading to substantial within address variation in treatment assignment, very few addresses are assigned as poor sometimes and affluent at others.[11] Exploiting the limited within address variation in the affluent

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[11] 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

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 by an intermediate step between no address controls and address fixed effects. Washington, DC addresses are divided into four quadrants (defined by location relative to the US Capitol Building) with differing reputations regarding neighborhood affluence. I remove the address fixed effects and instead control for quadrant fixed effects, testing how controlling for neighborhood attributes at the level of the quadrant affects the results. As shown in column (5), discrimination by neighborhood affluence disappears. The effect of being assigned a poor address is actually positive but now with a smaller 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. Employers do appear to discriminate against job applicants from distant neighborhoods even conditional on quadrant of the city, while the evidence for discrimination by neighborhood affluence is both statistically weak and not evident after controlling for broad regions of the city.

4.4. Different Outcome Measures

My interpretation of the main results is consistent with different employer response measures. Table 4 shows these results. My preferred outcome includes all responses except clearly negative responses. I replicate my main result 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

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commonly be classified as near and poor for downtown job locations. On rare occasions, the job location will be in far 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, this occurs infrequently.

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 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. Overall, commuting distances affect the pattern of positive and negative responses in a manner consistent with my interpretation of the main results.

5. Potential Mechanisms

5.1. A Theoretical Framework for Potential Mechanisms

Thus far, the results establish that employers call back applicants less frequently when the applicant lists an address far from the job location. This result is interesting in and of itself, indicating that employers take distance into consideration when considering candidates. De-facto segregation and sprawl in housing markets have implications for low wage, urban labor markets, and employer perceptions may be improved more readily by moving a person to a closer rather than less poor neighborhood.

Isolating why employers discriminate based on distance is not the main focus of the present study; however, isolating why employers discriminate may be of interest. Neumark (2012), Heckman (1998), and Heckman and Siegelman (1993) provide a simple theory of employer discrimination which provides some guidance in interpreting the results of correspondence experiments. Following their framework, suppose that the productivity of worker i with address a at job j is as follows:

$$P_{aij} = \beta_1 Far_{aj} + \delta' X_{ai} + \epsilon_{aij}$$

Far_{aj} is a dummy for a distant address, X_{ai} is a vector of worker and address attributes observable on the job application (including a constant), and ϵ_{aij} is the unobservable component of worker i's productivity. Suppose that the firm uses a cutoff rule (at zero, without loss of generality) to determine interviews:

$$Y_{aij} = I\{\beta_1 Far_i + \delta' X_{ai} + \epsilon_i > 0\}$$

If ϵ_i is normally distributed, then we can write the probability of a callback as:

$$\Pr[Y_{aij} = 1] = 1 - \mathrm{F}(-[\beta_0 + \beta_1 Far_{aj} + \delta' X_{ai}]) = \Phi\left(\frac{\beta_1 Far_{aj} + \delta' X_{ai} - \mu_{aij}}{\sigma_{aij}}\right)$$

where $F(\cdot)$ is the distribution of ϵ_{aij}, $\Phi(\cdot)$ is the standard normal distribution, and I have allowed the mean μ_{aij} and standard deviation σ_{aij} of the unobservables to vary across people, jobs, and neighborhoods. Suppose that unobservables only vary between near and poor addresses. Then, callback rates by group can be written as:

$$\Pr[Y_{aij} = 1 | Far_{aj} = 1] = \Phi\left(\frac{\beta_1 + \delta' X_{ai} - \mu_F}{\sigma_F}\right) \quad (4)$$

$$\Pr[Y_{aij} = 1 | Far_{aj} = 0] = \Phi\left(\frac{\delta' X_{ai} - \mu_N}{\sigma_N}\right) \quad (5)$$

The difference between these two expressions is the population analog of the difference in callback rates between near and far addresses that can be measured empirically. The theory thus indicates that callback rates for those with addresses far from the job may be lower for four reasons. First, callback rates may differ if living further from the job causes lower productivity ($\beta_1 < 0$), either directly through exhaustion or indirectly by increasing turnover rates. Second, callback rates will differ if near and far addresses are associated with different observable characteristics X_i. The correspondence study explicitly eliminates this possibility by controlling and randomly assigning characteristics on the job application. Third, callback rates may be lower if employers use classic statistical discrimination, believing that characteristics not listed on the résumé are worse for workers living far from the job than those of workers living near the job ($\mu_F < \mu_N$). Finally, employers may be less likely to call back applicants with distant addresses if the unobservables have different variances, even if the unobservables have similar means.

5.2. Direct Effect of Distance on Productivity

The productivity mechanism ($\beta_1 < 0$) has received most of the attention in the theory of urban labor markets. For example, Zenou (2002) argues that employers may avoid applicants from distant locations who will tend to be tired or more frequently late for work. Presumably this channel would more significantly affect observably low-quality workers who would be more likely either to shirk or to require the use of public transit. On the other hand, distant job applicants may be likely to obtain similar job offers close to home and thus be prone to higher turnover or interview cancellation. This channel presumably affects observably high quality applicants who have better outside options. Thus, I can provide some insight on potential mechanisms by testing whether employers' discounting of distant applicants differs with overall observable applicant quality. As in Table 1, I measure overall applicant quality by regressing the response dummy on observable characteristics of the résumé and obtaining the fitted values. I split the sample into high and low quality halves using these fitted values. I can then measure whether near-far differences in employer response differ with this measure of applicant quality. Splitting the sample according to the calculated quality measure would generate bias as it uses information on the outcome variable; however, I follow Abadie, Chingos, and West (2013) to remove this bias. This method involves randomly splitting the sample in half, using one half to compute the coefficients for the quality index, using the other half to run the main regression, and then averaging over several repetitions.

The first two columns of Table 5 display the results of this estimation. Distant addresses receive a similar 2.5 percentage point discount to the level of employer callbacks, whether high or low quality. This result is surprising, though, because it represents a much larger proportional decrease for the low quality group, which receives callbacks only 10 percent of the time as opposed to 27 percent for the high quality group. A simple cutoff theory as described in equations (4) and (5) would predict that the high quality group would experience a larger decrease in callback rates because they are further up the slope of the probability density function. However, I observe similarly large drops in the level of callback rates for both high and low quality applicants, indicating a larger effect on perceptions of productivity for low quality applicants. This result suggests that employers disproportionately discriminate on distance when faced with low quality applicants. Interpreted through the productivity lens, these results suggest employers are more concerned about direct productivity loss from, e.g., public transit delays rather than the potential loss of distant but high-quality applicants due to turnover.

I can also more directly test whether employers respond sensitively to commute times rather than just linear distance. While I use great circle distance for the experiment, I can also measure public transit travel time between job and home locations using the Washington Metropolitan Transit Authority's "Trip Planner."[12] Surprisingly, as shown in column (7) of Table 3, employers do not respond strongly to travel time. When I include both great circle distance and public transit travel time, the coefficient on travel time is nearly zero and

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

statistically insignificant. Employers respond more to simple "straight line" distances than to actual travel times. This result does not fully eliminate a mechanism related to how commuting affects productivity. Employers may only have noisy knowledge of the bus system and use linear distance as an approximation. However, a mechanism based on how commuting distance affects productivity requires some complication to fit this result.

5.3. Using Distance to Statistically Discriminate

Of course, statistical discrimination regarding unobserved attributes of the applicant may also drive measured discrimination in correspondence experiments (Heckman, 1998). The present study is not designed to distinguish between direct effects of distance on productivity and statistical discrimination. However, I can test a small number of plausible hypotheses regarding statistical discrimination.

Employers may view willingness to search far from home as a positive signal indicating high productivity (e.g. persistence). Of course, such channels would only serve to make the present results overestimates of the pure productivity channel. On the other hand, employers may view the choice to search far from home as a negative signal that other more proximate employers have evaluated and rejected the applicant. In a theoretically parallel case Kroft, Lange, and Notowidigdo (2013) use a correspondence study to document that employers discriminate against the long-term unemployed but are less likely to do so in cities with weak labor markets in which the negative signal is weaker. I test the possibility that employers statistically discriminate in this manner using a similar strategy. For each applicant, I measure the average distance from that applicant's address to *all jobs* in the experiment. If employers statistically discriminate in this manner, then they should discriminate by distance more when the applicant is from a job-rich neighborhood (signaling multiple previous rejections) and less when the applicant is from a job-poor neighborhood. Such a theory would imply a positive coefficient on the interaction between the applicant living far from the job itself and the applicant living far from jobs more generally. Column (3) of Table 5 provides the results of this estimation. The coefficient is positive as predicted but very small. Most addresses vary between 2 to 4 miles from the average job, meaning that the effect of living at a far address would only vary $\pm$ 0.002. The power of this test is limited by a strong correlation between the distance to the job itself and distance to other jobs, but to the extent that I can test for it I find no evidence of this most plausible form of statistical discrimination.

Finally, as discussed by Heckman (1998) and Neumark (2012), differences in the variance of unobservables for distant and near applicants can actually drive changes in callback rates. If callbacks are relatively unlikely (small X_i relative to $\mu = \mu_F = \mu_N$) then only candidates with very good unobservables are worth interviewing. So, if distant applicants have a lower variance of unobservables ($\sigma_F < \sigma_N$), then they will be called back less frequently. However, this story should reverse for observably good candidates (large X_i) who have a high likelihood of being called back. In that situation, distant applicants with a lower σ should be called back more often than those living nearby (or at least the gap should narrow). As documented above, we do not observe heterogeneous effects by overall application quality. Thus, employers do not appear to be responding to differing variances of unobservable characteristics.

6. Conclusion

In this study, I have demonstrated that employers discriminate against job applicants who list more distant residential addresses. When presented with otherwise similar fictional résumés, hiring managers for actual low-wage job vacancies call back applicants living further away 14 percent less often. This effect is large. Living 5-6 miles further away decreases callback rates by an amount approximately equal to the discount experienced by applicants with ‘black names’ relative to those with ‘white names.’ Because commuting distance and neighborhood poverty are correlated, discrimination by distance can account for roughly two thirds of the previously documented discount in callback rates experience by applicants from poor neighborhoods. On the other hand, the evidence provides only mixed support for the notion that the listed address’s neighborhood affluence directly affects employer behavior.

These results provide support for the spatial mismatch hypothesis, the idea that living far from employment opportunities has a direct negative impact on 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.

Understanding the mechanisms behind spatial mismatch helps to guide policy responses. For instance, the Moving to Opportunity (MTO) project prominently found that providing housing vouchers to public housing residents did not improve their 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) point out that the voucher recipients tended to move to neighborhoods with similarly poor geographic access to jobs. If neighborhood effects operate through spatial mechanisms relating specifically to commuting distance, then it would not be surprising for a housing voucher that facilitates moving to a less poor but equally distant neighborhood to provide no improvement in labor market outcomes. Eliminating the negative employment effects of living in poor neighborhoods would instead require housing interventions moving residents further from home and closer to jobs. Perhaps more practically, the effective distance between areas of concentrated poverty and job vacancies could be shortened by improving public transit. Of course, exact policy prescriptions may depend on whether employers discriminate on distance because commuting lowers productivity or because distance proxies statistically for another variable of interest. However, the present study demonstrates that employer discrimination by commuting distance exists. Thus, employer behavior can translate disparate housing market outcomes into disparate labor market outcomes. The fact that employers consider commute distance in the hiring process can help interpret previous attempts to address spatial mismatch and inform future public policy responses.

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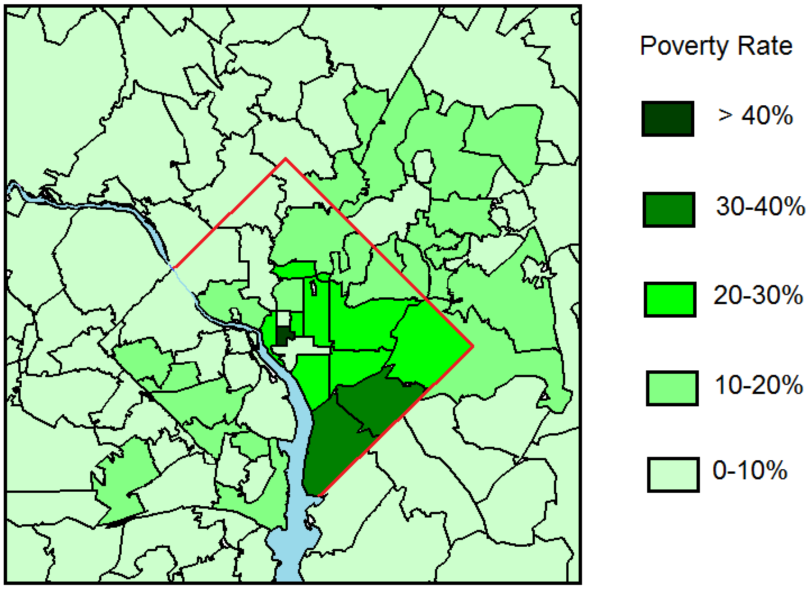
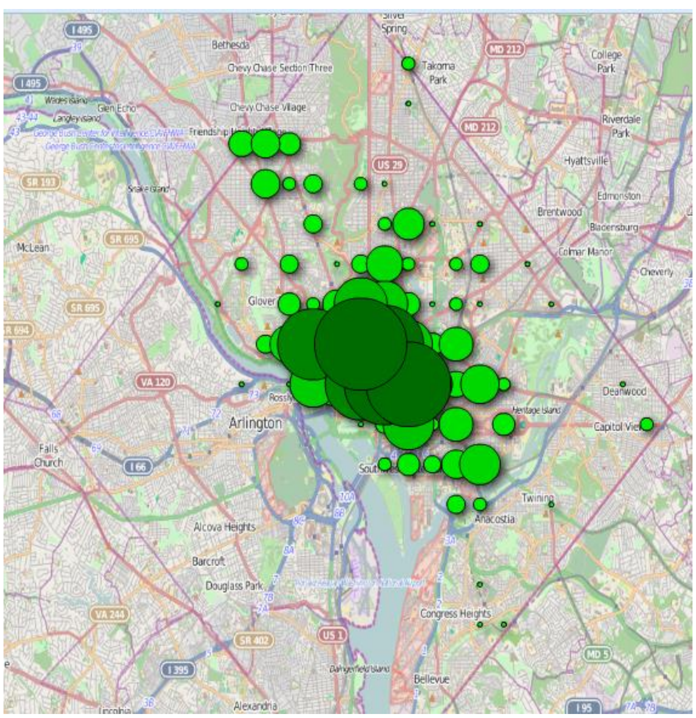
Figure 1. Poverty Rates across Washington, DC Area Zip Codes



Source: US Census of Population, 2000

Figure 2. Location of Job Vacancies Included in Experiment

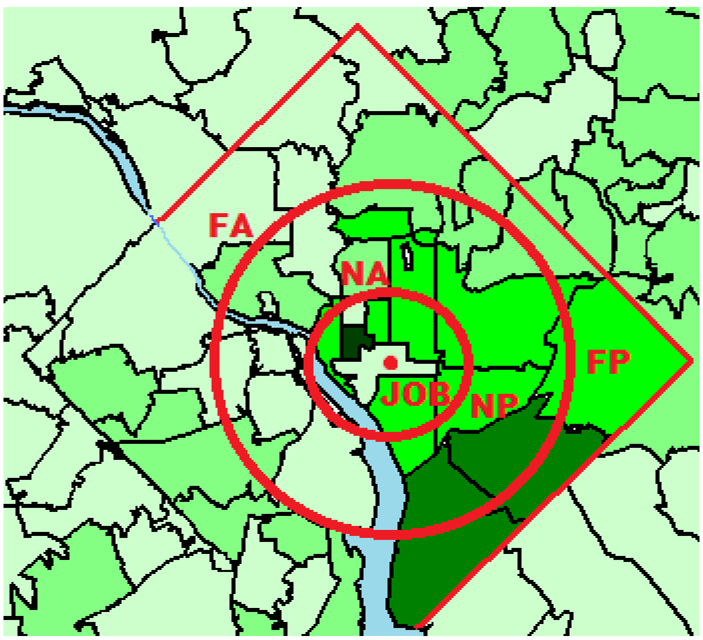


Source: Data from experiment. Larger/darker circles indicate more job vacancies in that location.

Figure 3. Job Access and Education Levels in Washington, DC Census Tracts

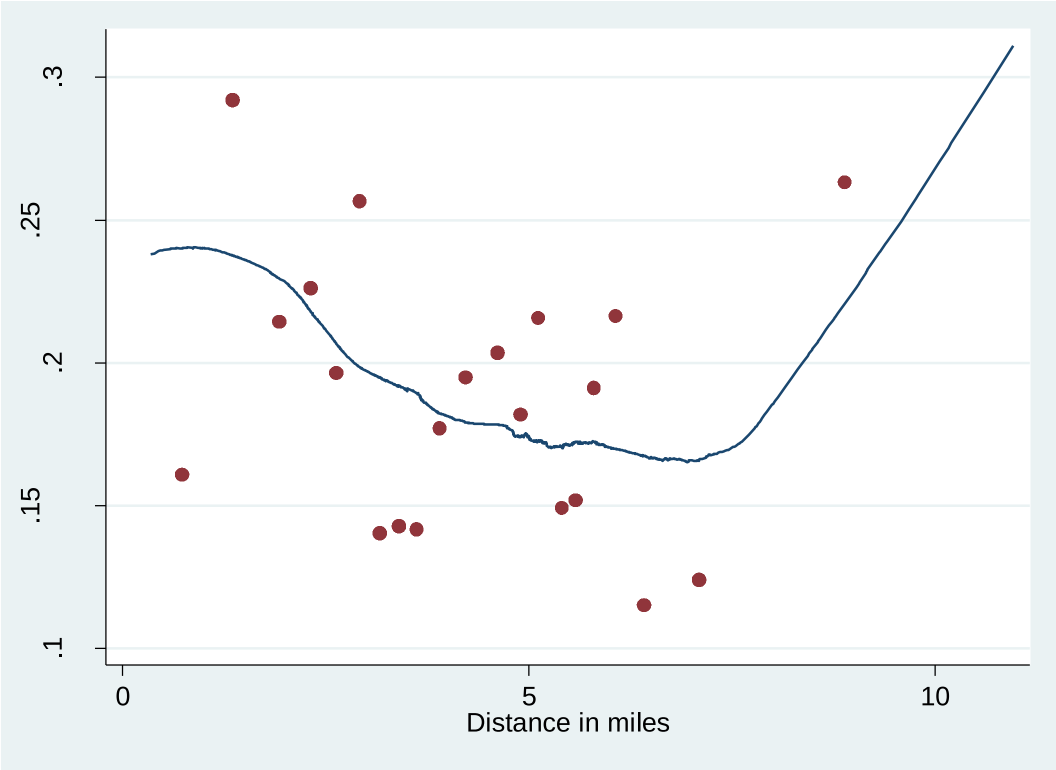
Source: Average distance to jobs is computed as average great circle distance from the tract centroid to the job vacancies used in the correspondence experiment. Fraction of the population with a Bachelor's or More comes from the American Community Survey 2011 5-Year estimates.

Figure 4. Identification Strategy



Shading reflects poverty rates from the US Census of Population, 2000. Darker indicates higher poverty rates. JOB refers to the job location. NA (near affluent), NP (near poor), FA (far affluent), and FP (far poor) refer to the four treatment categories for addresses.

Figure 5. Callback Rates and Commuting Distance



Source: Authors calculations using experimental data. Distance is measured as great circle distance between the employer's listed location and the residential location generated for the fictional applicant. Applicants are grouped into 20 equal ventiles by distance with callback rates within the group displayed. The curve shows a quadratic fit of the individual-level data with 95% confidence interval in gray.

Table 1. Summary Statistics and Baseline Balance	Near, Affluent	Far, Affluent	Near, Poor	Far, Poor	Poor - Affluent	Far - Near
A. Applicant Address Characteristics						
Distance to Job (miles)	3.0	5.3	3.0	5.8	0.2	2.6
	(1.3)	(1.1)	(1.3)	(1.3)	[0.001]	[0.000]
Tract Median Income ($)	101,698	106,371	32,429	27,812	-73,914	28
	(29,066)	(26,392)	(19,822)	(4,832)	[0.000]	[0.98]
Tract Percent Bachelor's or Higher	70	62	20	11	-50	-9
	(16)	(18)	(18)	(5)	[0.000]	[0.000]
Tract Percent White	60	31	9	1	-40	-19
	(24)	(27)	(18)	(1)	[0.000]	[0.000]
Tract Affluence Index	0.146	0.142	0.083	0.077	-0.064	-0.005
	(0.018)	(0.019	(0.021)	(0.005)	[0.000]	[0.000]
B. Job Address Characteristics						
Tract Median Income ($)	95,672	95,672	95,672	95,672	0	0
	(34,951)	(34,951)	(34,951)	(34,951)	[1.00]	[1.00]
Tract Percent Bachelor's or Higher	74	74	74	74	0	0
	(20)	(20)	(20)	(20)	[1.00]	[1.00]
Tract Percent White	65	65	65	65	0	0
	(22)	(22)	(22)	(22)	[1.00]	[1.00]
C. Applicant Characteristics						
White	0.35	0.35	0.40	0.34	0.02	-0.03
	(0.48)	(0.48)	(0.49)	(0.48)	[0.42]	[0.20]
Black	0.36	0.42	0.35	0.41	-0.01	0.06
	(0.48)	(0.49)	(0.48)	(0.49)	[0.65]	[0.01]
Age	41	41	41	41	-1	0
	(11)	(12)	(11)	(12)	[0.27]	[0.56]
Work Gap (days)	88	91	89	95	2	5
	(52)	(53)	(55)	(52)	[0.31]	[0.04]
Work Experience (years)	8.1	7.7	8.0	8.2	0.2	0.0
	(5.5)	(4.7)	(5.4)	(5.2)	[0.32]	[0.82]
Female	0.49	0.49	0.54	0.48	0.02	-0.03
	(0.50)	(0.50)	(0.50)	(0.50)	[0.31]	[0.11]
Overall Quality (Predicted Callback Rate)	0.186	0.183	0.192	0.188	0.005	-0.003
	(0.107)	(0.109)	(0.105)	(0.111)	[0.053]	[0.22]
D. Outcome						
Callback Rate	0.207	0.177	0.195	0.170	-0.010	-0.027
					[0.46]	[0.03]
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**	-0.030*	-0.024*	-0.074**	-0.030**
	(0.013)	(0.018)	(0.013)	(0.030)	(0.014)
	[0.03]	[0.09]	[0.06]	[0.02]	[0.03]
Poor	-0.010	-0.012	-0.015	0.092	0.006
	(0.013)	(0.018)	(0.013)	(0.065)	(0.019)
	[0.46]	[0.50]	[0.24]	[0.16]	[0.74]
Far X Poor	--	0.005	--	--	--
		(0.024)			
		[0.83]			
Black	--	--	-0.060***	-0.064***	-0.061***
			(0.016)	(0.018)	(0.016)
			[0.00]	[0.00]	[0.00]
Applicant Controls	N	N	Y	Y	Y
Job Fixed Effects	N	N	Y	Y	Y
Applicant Address Controls	N	N	N	Address Fixed Effects	Quadrant Dummies
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. Standard errors are clustered at the job vacancy level. Selected p-values are in brackets.

Table 3. Instrumental Variable Estimates for Continuous Measures of Distance and Affluence

Dependent Variable	Distance (1)	Affluence (2)	Callback (3)	Callback (4)	Callback (5)	Callback (6)	Callback (7)
Far	2.56***	-0.005***	--	--	--	--	--
	(0.05)	(0.001)					
Poor	0.24***	-0.064***	--	--	--	--	--
	(0.05)	(0.001)					
Distance to Job (miles)	--	--	-0.011**	-0.011**	-0.010**	-0.06	-0.013
			(0.005)	(0.005)	(0.005)	(0.12)	(0.023)
Distance Sq.	--	--	--	--	--	0.006	--
						(0.014)	
Affluence Index	--	--	0.11	--	0.11	0.19	0.09
			(0.21)		(0.21)	(0.29)	(0.37)
Log Med. Inc.	--	--	--	0.006	--	--	--
				(0.01)			
Public Transit Travel Time (min.)	--	--	--	--	--	--	0.000
							(0.005)
Instruments	N/A	N/A	Far, Poor	Far, Poor	Far, Poor, Far*Poor	Far, Poor, Far*Poor	Far, Poor, Far*Poor
Sample size	2,260	2,260	2,260	2,260	2,260	2,260	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 4. Different Outcome Measures

Dependent Variable:	Preferred Measure (1)	Interview Only (2)	Rejected Only (3)	Any Response (4)
Far	-0.027**	-0.027**	0.009**	-0.014
	(0.013)	(0.012)	(0.004)	(0.013)
	[0.03]	[0.03]	[0.02]	[0.27]
Poor	-0.010	-0.004	0.004	0.005
	(0.013)	(0.011)	(0.003)	(0.013)
	[0.46]	[0.75]	[0.29]	[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.

Table 5. Heterogeneous Effects

Dependent Variable:	Callback Dummy		
	(1)	(2)	(3)
Sample:	High Quality	Low Quality	All
Far	-0.024	-0.025	-0.030
	(0.019)	(0.025)	(0.071)
Far X Avg. Distance	--	--	0.001
			(0.017)
Applicant Controls	Y	Y	Y
Job Fixed Effects	N	N	Y
Applicant Address Controls	N	N	N
Callback rate	0.27	0.10	0.19
Sample size	1,130	1,130	2,260

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Applicant controls include a dummy for poor address, female name dummy, racial name dummies, age, years of listed work experience, length of work gap, and job profileXjob category dummies. Column (3) also includes avg. distance to job and a poor address dummy interacted with avg. distance to job. Standard errors are clustered at the job vacancy level. Selected p-values are in brackets.

Appendix

A.1. Measuring Distance and Affluence

The 2x2 research design described above requires measuring both commuting distance from an applicant's address to a job location and measuring an index of affluence for any address. I measure distance using great circle distance in miles. This can be easily measured by geo-coding the address of the job vacancy and the address listed on the job application. To measure affluence, I draw on publicly available data from the American Community Survey (2011 5-Year Estimates) and previous work by Bertrand and Mullainathan (2004). The challenge is to summarize all fixed (i.e. not dependent on the location of the employer, such as distance) neighborhood attributes such as poverty, racial composition and educational attainment into an index describing employer perception of that neighborhood. I use propensity-score matching techniques to this end. Using the Bertrand and Mullainathan (2004) experimental data, 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) data. The three variables are jointly significant (F-test p-value of 0.005).

I extrapolate these results to the new setting by combining the results with ACS data for Washington, DC. I calculate expected callback rates for DC census tracts as:

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

This is my measure of affluence. This process combines census tract income, racial composition, and educational attainment into one measure where different attributes are weighted depending on the observed importance placed on these characteristics by employers in the Bertrand and Mullainathan (2004) data. More specifically, the index is the propensity score that can then be used to match census tracts by how their characteristics are viewed by employers. Two addresses with similar propensity scores should be treated similarly by employers if neighborhood income, racial composition, and educational attainment sufficiently characterize the information contained in an address.

Of course, 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 with college attainment but still positively. Altogether, this index provides a reasonable measure of affluence. As described in the main text, I will also be able to control for potential errors in measuring affluence using address fixed effects.

A.2. Choosing Addresses

To choose specific addresses, I list addresses in an 18x18 equally spaced grid with borders formed by the points of the Washington, DC diamond. I then eliminate points outside of Washington, DC. I also eliminate addresses in census tracts dominated by universities (at least 30% college students), military bases (at least 30% in armed forces), and parks/water. 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 level of zoom). I use main streets because a public survey (described above) 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 alphabetical/numeric system of streets in DC. The result is a grid of addresses across Washington, DC entirely composed of addresses on main streets. The distance of each address to the location of a particular job vacancy can be measured easily. I also attach ACS data, and thus an affluence index, to each address according to its census tract.

Given the location of the job vacancy, I first define “Near, Poor” addresses by requiring that they be below the 10th percentile of the affluence index among all addresses on the grid. Then, I select addresses that are no more than 1 mile further from the job than the closest such address. From this group of potential addresses, I choose one at random. I require that the “Near, Affluent” address be the same distance from the job as “Near, Poor” ($\pm$ 0.15 miles) and select one address at random from those that have an index above median affluence.[13] For “Far, Poor” addresses I choose an address at random from among those that have the same affluence as the NP address ($\pm$ 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 is the most difficult as it requires matching both the affluence of the NA address and the distance of the FP address. Sometimes these two goals trade off against each other. In practice, I balance these two concerns by choosing the address that minimizes the following:

[13] If there is no such address, I choose the most affluent address.

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

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. In practice I come close to this ideal, as demonstrated in Table 1, though the tradeoff between matching distance and matching affluence leads the FA addresses to be somewhat nearer than FP addresses and somewhat less affluent than NA addresses. As discussed above, though, I can control for imperfect matching using address fixed effects.

A.3. Applicant Quality Index

I use an index of applicant quality in addressing both baseline balance and heterogeneous effects. Formally, I run a regression of a callback dummy on some observable characteristics X:

$$Y_{aij} = \beta_0 + \delta X_{aij} + \epsilon_{aij}$$

I include a white name dummy, a black name dummy, a female name dummy, work experience, age, employment gap, and 32 dummies for each of 4 employment histories in 8 different occupations. My index of applicant quality consists of the fitted values from this regression computed from the coefficients shown in Appendix Table 3.

All of these characteristics are randomly assigned. Thus, Appendix Table 3 also shows the causal effect of different applicant characteristics on callback rates, which can be compared to existing studies. As in Bertrand and Mullainathan (2004), I find that stereotypically black names lead to lower callback rates. I find no difference between white and new "ambiguous" names that I introduce. Interestingly, I also find marginally statistically significant evidence that employers prefer female workers, though this may reflect the particular labor market and jobs chosen. I find no average effect of work experience, which is not surprising given jobs that require only limited training. Similar to Lahey (2008) I find evidence of age discrimination with older workers receiving lower callback rates. Finally, I find no evidence for discrimination against the long-term unemployed. Response rates do not respond to greater work gaps. In the previous literature, Kroft, et. al. (2013) and Eriksson and Rooth (2014) both find evidence in correspondence experiments that employers discount long-term unemployed. However, both previous studies would predict no effects in the context of the present study. Eriksson and Rooth (2014) only find an effect beyond 9 months of contemporaneous unemployment. Kroft, et. al. (2013) find stronger evidence that employers respond to unemployment durations but still mainly beyond 6 months. In the present study, I randomize employment gaps between 0 and 6 months and thus find no effect. Overall, treatment effects for other characteristics generally match the existing literature, confirming similarity between my experimental design and previous work.

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.