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ABSTRACT

Neighborhood Signaling Effects, Commuting Time, and Employment: Evidence from a Field Experiment*

The question of whether and how living in a deprived neighborhood affects the labor market outcomes of its residents has been a subject of great interest for both policy makers and researchers. Despite this interest, empirical evidence of causal neighborhood effects on labor market outcomes is scant, and causal evidence on the mechanisms involved is even more scant. The mechanism that this study investigates is neighborhood signaling effects. Specifically, we ask whether there is unequal treatment in hiring depending on whether a job applicant signals living in a bad (deprived) neighborhood or in a good (affluent) neighborhood. To this end, we conducted a field experiment where fictitious job applications were sent to employers with an advertised vacancy. Each job application was randomly assigned a residential address in either a bad or a good neighborhood. The measured outcome is the fraction of invitations for a job interview (the callback rate). We find no evidence of general neighborhood signaling effects. However, job applicants with a foreign background have callback rates that are 42 percent lower if they signal living in a bad neighborhood rather than in a good neighborhood. In addition, we find that applicants with commuting times longer than 90 minutes have lower callback rates, and this is unrelated to the neighborhood signaling effect. Apparently, employers view information about residential addresses as important for employment decisions.

JEL Classification: C93, J15, J21, J71

Keywords: neighborhood signaling effects, neighborhood stigma, commuting time, discrimination, field experiment, correspondence study

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1. Introduction

In many major metropolitan cities, there are neighborhoods that consist of clusters of individuals with low incomes and high levels of unemployment (see, e.g., Durlauf, 2004, and Aldén and Hammarstedt, 2016). These neighborhoods are usually further characterized by low education levels, high concentrations of residents with foreign backgrounds, and high crime rates (Aldén and Hammarstedt, 2016; Tammaru, et al., 2016; Andersson et al., 2010; Andersson and Bråmå, 2004; and Grönqvist et al., 2015). An important and policy relevant question is whether living in such neighborhoods *causes* poor individual labor market outcomes, and if so, what the mechanisms are. By conducting a field experiment in which residential addresses for different types of neighborhoods are randomly attached to fictitious job applications, we set out to improve the answer to this question.

Empirically estimating causal neighborhood effects has proven to be difficult. Since residential location is an endogenous choice, there could be negative selection of individuals with lower skills into these neighborhoods (or with higher skills out of the same neighborhoods). In this case, there may be no real neighborhood effect. Instead, the observed differences in labor market outcomes across residential locations could be entirely driven by unobserved individual productivity differences.

A number of potential mechanisms could explain a causal neighborhood effect, and distinguishing among them is a further challenge. A first potential mechanism relates to the labor demand side, where employers assume that individual productivity is lower among job candidates living in deprived neighborhoods. The rationale behind this assumption is that the average citizen in such an area could have lower productivity. As a result, employers may statistically discriminate against workers living in deprived neighborhoods, i.e., there could be a negative neighborhood signaling effect (Hasting and Deaton, 2003; Wacquant, 1993; Atkinson and Kintrea, 2001). A

second set of pathways is related to the amount of social capital available in the neighborhood and the local labor market conditions. For example, the worse economic outcomes of deprived neighborhoods could result from a lack of good role models, negative peer influences, weak labor market networks and/or a weak local labor markets (Wilson 1987; Borjas, 1995; Lucas 1998; Lundberg and Startz 1998). A third possible mechanism, often referred to as *the spatial mismatch effect*, is that deprived neighborhoods may be located further away from available jobs, which could create a barrier to accessing jobs (Ihlanfeldt & Sjoquist, 1998).

The main contribution of this study is to improve our understanding of the role of neighborhood signaling effects for employment opportunities. While signaling effects are emphasized in the theoretical literature, few empirical studies try to estimate signaling effects. A first study related to ours is the *Moving to Opportunity* experiment (Katz et al, 2001), which addresses the potential problem with selection bias in observational studies[1] by conducting a lottery in which some families living in deprived neighborhoods in five US cities (Baltimore, Boston, Chicago, Los Angeles and New York) were randomly given the opportunity to move to a better neighborhood located nearby. This study finds no short-run effects on employment of moving to a better neighborhood. However, in a follow-up study of the long-term effects, Chetty et al (2016) find that adults who moved to a better neighborhood at a young age (below age 13) are more likely to have attended college, have higher income and less likely to be a single parent. Although experiments of this type make the neighborhood exogenous, they do not necessarily help distinguish between signaling effects and potential mechanisms beyond selection.[2]

[1] Most studies use observational data to study neighborhood effects. For example, Atkinson and Kinterea (2001) use survey data to study neighborhood effects, while Galster et al (2015) use longitudinal data to study neighborhood effects in Sweden. However, with observational data, it is often not possible to entirely rule out the potential bias of unobserved individual characteristics. See Durlauf (2004) for an overview of the literature on neighborhood effects.
[2] In the Moving to Opportunity experiment (Katz et al, 2001), spatial mismatch should not be a relevant mechanism, since the better neighborhoods to which the families were moved where located nearby.

The study by Bertrand and Mullainathan (2004) takes a step in the direction of revealing causal pathways. Although the study was designed primarily to measure ethnic discrimination, their field experiment is also relevant for investigating neighborhood signaling effects. They randomly assigned fictitious job applications black or white names and applied for real jobs, ruling out both the selection of individuals into neighborhoods and any mechanism working through social capital or local labor market conditions. They find that job applicants signaling living in more affluent neighborhoods in terms of the fraction of white residents, the level of education and the level of income have higher callback rates for job interviews, and that this neighborhood signaling effect is the same for both whites and blacks. However, they do not control for employers potentially using the distance to work as a sorting factor, that is, for potential systematic correlations between the type of neighborhood and the distance to the job.

We are only aware of two studies that set out to specifically identify neighborhood signaling effects. Both are field experiments with a similar design as in Bertrand and Mullainathan (2004). Tunstall et al (2014) randomly assign neighborhoods with similar distances to the job, thereby controlling for distance to work by design, while Phillips (2016) randomly and independently assign neighborhood and distance to the job, which enables him to identify neighborhood signaling effects. However, these studies do not consider heterogeneity in the effects, such as whether the neighborhood signaling effects vary by the ethnic background of the job applicant. Heterogeneity by ethnicity could be important, as suggested by Edin et al (2003), who use a quasi-experimental design and find that living in an ethnic enclave in Sweden has positive effects on labor market outcomes among refugees. The contribution of the current study is that it adds to the small literature that identifies neighborhood signaling effects and considers an important source of heterogeneity in these effects. It also shows that employers consider commuting distance when deciding which applicant to hire.

To investigate neighborhood signaling effects, we conducted a field experiment where 2,790 job applications were sent to employers with a job vacancy. The measured outcome was callbacks from employers for job interviews. The idea behind a field experiment of this type is that it can identify the causal effects of variables that are randomly assigned to the job applications. In our experiment, we first randomly assign neighborhood (residential address) to the job applications, which gives the causal neighborhood effect, and then add a control variable for the commuting distance, which leaves us with the neighborhood signaling effect. This type of experiment has been used to study ethnic, gender, and age discrimination (e.g., Riach and Rich, 2002; Bertrand and Mullainathan, 2004; Carlsson and Rooth, 2007; Carlsson, 2011; Rich, 2014; Carlsson and Eriksson, 2017) and scaring effects (Eriksson and Rooth, 2014; Kroft et al, 2013). In our experiment, postal addresses were selected to be representative of either a deprived (bad) or an affluent (good) neighborhood and indicated in the resume. In addition to the type of neighborhood, we randomly assigned gender and ethnicity signals (through the applicant's name).

We find no evidence of a neighborhood signaling effect for typical Swedish names. In contrast, for typical Middle Eastern (male) names, we find a significant neighborhood signaling effect. Job applicants with typical Middle Eastern names who signal living in a bad, as opposed to a good neighborhood, receive 42 percent fewer callbacks for a job interview from employers. The ethnic difference in the neighborhood signaling effect implies that the ethnic callback gap could be reduced by one-third if applicants with typical Middle Eastern names simply moved from a bad to a good neighborhood.

We also find that commuting time is negatively associated with the callback rate. This suggests that moving to a neighborhood less than one and a half hours from the job, as opposed to commuting more than one and a half hours to the job, would raise the callback rate by approximately 13 percentage points. To put the size of this estimate in perspective, it is of about

the same magnitude as the (negative) effect of having a typical Middle Eastern name, as opposed to a native Swedish name, when living in a bad neighborhood. It should also be mentioned that in our case, commuting time and neighborhood type are uncorrelated.

The remainder of this article is organized as follows. Section 2 provides a theoretical discussion of why neighborhood could affect labor market outcomes. Section 3 describes the experimental design and the choice of neighborhoods used in the experiment. Section 4 provides descriptive statistics for the experimental data and presents results of the empirical analysis. Section 5 interprets the results, and section 6 concludes the paper.

2. Why neighborhood could affect labor market outcomes

As stated in the introduction, neighborhood effects could operate through signaling effects of the neighborhood, pathways related to the characteristics of the neighborhood, or spatial mismatch.

Neighborhood signaling effects, which this study focuses on, operate on the labor demand side, where employers may assume that individual productivity is lower among job candidates living in deprived neighborhoods, and this information is then used when deciding who to hire (see, e.g., Hasting and Deaton, 2003; Wacquant, 1993; Atkinson and Kintrea, 2001). The underlying mechanism for the neighborhood signaling effect is that employers statistically discriminate against individuals on the basis of neighborhood reputation (Phelps, 1972; Arrow, 1973). In theories based on statistical discrimination, it is assumed that information about individual productive attributes is incomplete, and employers therefore use easily observable group characteristics to predict unobserved individual productivity. In our case of signaling effects, statistical discrimination implies that employers assume that individual productivity is lower for workers living in deprived neighborhoods. The rationale behind this assumption could be that the productivity of the average

citizen is, in fact, lower, but one can also imagine a case where employers have incorrect or exaggerated beliefs about the average productivity of individuals living in distressed neighborhoods. As previously mentioned, there are only two studies of which we are aware that identify neighborhood signaling effects. Tunstall et al (2014) randomly assign neighborhoods with similar distances to the job, thereby controlling for the distance to work by design, while Phillips (2016) randomly and independently assigns neighborhood and distance to the job, which enables him to identify neighborhood signaling effects.

Beyond signaling effects, one could imagine several pathways related to the characteristics of the neighborhood that could result in a neighborhood effect, including the available amount of social capital in the neighborhood and local labor market conditions. Effects related to social capital stem from social interactions and socialization with peers and depend, among other things, on the characteristics of the peers and access to good role models within the neighborhood (Wilson 1987, 1996; Cutler and Glaeser, 1995; Borjas 1995; Akerlof 1980). It is argued that exposure to a culture where, for example, joblessness, dropping out of school, and committing certain crimes are accepted could influence the process of human capital accumulation by affecting values and expectations regarding labor force participation, education and criminality. A large economic literature, using various fixed effects and quasi-experimental methods, has examined these types of neighborhood effects on individual economic outcomes (Bolster et al., 2007; van Ham and Manley, 2010; Brattbakk and Wessel, 2013; Katz et al., 2001; Clampet-Lundquist and Douglas, 2008; Chetty et al., 2016 and Galster, et al., 2015). However, the empirical evidence is mixed (see Ellen and Turner, 1997; Durluf, 2004; and Galster, 2012 for a review of the literature).

The spatial mismatch hypothesis suggests that spatial isolation of deprived neighborhoods from the parts of cities where most jobs are located leads to poor employment opportunities for individuals living in the deprived areas. Thus, according to the spatial mismatch hypothesis, the

geographical location creates the disadvantage for individuals living in deprived neighborhoods. Gobilon et al (2007) discusses the ways through which spatial mismatch could affect employment opportunities. First, on the supply side, a longer distance to the job could lead to lower search intensity due to higher transportation costs, less access to information about job openings and, therefore, higher search costs. Second, on the demand side, employers could statistically discriminate against workers who commute long distances (see also Wilson, 1997). The reason could be that employers view job candidates with long commutes as less productive if commuting makes a worker inflexible, takes considerable energy, and increases absences from work. A large literature analyzes the spatial mismatch hypothesis.[3] However, it remains a challenge to separate spatial mismatch from other potential neighborhood effects as well as to distinguish between supply- and demand-side explanations. One study that specifically investigates the demand side of the spatial mismatch hypothesis is Phillips (2016). Using a field experiment similar to ours, he randomly attaches residential addresses to fictitious job applicants CVs, and he obtains evidence of employer discrimination against job applicants with long commutes.

To summarize, there are essentially three broader theoretical explanations for how neighborhood may affect the employment opportunities of its residents. We focus on the role of neighborhood signaling effects, which we investigate by conducting a field experiment in the labor market.

3. Experimental design

To investigate neighborhood signaling effects, we conducted a field experiment where job applications were sent to employers with a job vacancy. The measured outcome is callbacks from

[3] See Ihlanfeldt and Sjoquist (1998) for a review of the empirical literature on the spatial mismatch hypothesis.

employers in the form of invitations to job interviews. The idea behind a field experiment of this type is that it can identify the causal effects of variables that are randomly assigned to the job applications. The key variable that is randomly assigned in our experiment is the neighborhood the job applicant resides in, which is signaled by the postal address listed on the resume. This empirical strategy, together with information on the commuting distance, enables us to uniquely identify the neighborhood signaling effect.

This field experiment is part of a larger research project that has an objective of testing for labor market discrimination on the basis of ethnicity, gender, and history of unemployment, as well as other hypotheses. These dimensions are not analyzed in this paper.

Designing the experiment involved selecting appropriate neighborhoods, creating the identities of the job applicants, selecting occupations, constructing fictitious resumes, and finally, creating procedures for sampling, sending out the resumes to the employers, and recording their responses.

3.1 Selecting neighborhoods

In this investigation of neighborhood signaling effects, we focus on two major cities in Sweden – Stockholm and Gothenburg. The reason is that the majority of advertised jobs are found in these cities, and it is only in the larger cities where there is considerable variation neighborhood type. In both cities, the relatively wealthy and those with Swedish backgrounds[4] tend to live in affluent neighborhoods in the central parts of the city, while the relatively poor and those with foreign backgrounds tend to live in deprived neighborhoods outside the central parts of the city. Perhaps surprisingly, there is no ethnic difference in the distance to available jobs, see Åslund, Zenou and Öst (2010). Further, people living in relatively poor neighborhoods are found to have lower

[4] We define a Swedish background as being born in Sweden and having two Swedish born parents. Other individuals are defined as having a foreign background.

employment rates and incomes and to have more difficulties finding employment after a period of unemployment or sickness.

We study neighborhood effects at the SAMS (Small Areas for Market Statistics) level. Statistics Sweden divides Sweden into approximately 9,000 small areas that are intended to be homogenous neighborhoods with respect to socioeconomic characteristics. SAMS have an average population of approximately 1,000 residents. This division facilitates the study of neighborhood effects; see Berggren (2010) for a review of Swedish studies using SAMS to study various neighborhood issues.

In the experiment, we include two categories of clearly distinct SAMS. Areas in each category were selected to be representative of a typical affluent (henceforth, good) or deprived (henceforth, bad) SAMS. We choose SAMS based on a geographic index of economic segregation at the SAMS level (The National Board of Health and Welfare, 2006).[5] For both cities, we selected the three most affluent and the three most deprived SAMS. For each SAMS, we then chose a postal address to be used in the experiment with a housing type that is typical for the SAMS.[6] Thus, we used postal addresses with apartments in bad neighborhoods and postal addresses with houses and duplexes in good neighborhoods. In the end, twelve different addresses (2 cities x 2 types of neighborhood x 3 addresses per type of neighborhood) were used in the experiment. The specific addresses are reported in Appendix Table A1.

[5] The segregation index used is an entropy index, which is normalized so that it ranges between zero and unity. A value of zero means complete integration, where all neighborhoods have the same composition as the overall population. A value of unity means complete segregation, where the groups in question are completely separated from each other. The neighborhoods included in the experiment are segregated areas where individuals with low incomes are overrepresented (bad areas) or individuals with high incomes are overrepresented (good areas). Integrated areas with an index close to zero are not included in the experiment. See the National Board of Health and Welfare (2006) for further details.

[6] There is also a subset of job applications with other arbitrary addresses, i.e., not in a clearly good or bad SAMS. These are excluded from the analysis. We did not randomize good/bad neighborhoods for all job applications, since strong neighborhood signals could reduce the external validity of the results for the other issues (i.e., ethnic and gender discrimination) that the project was designed to investigate.

Since SAMS are, by definition, homogenous areas, our strategy of including two types of areas, affluent and deprived neighborhoods, should result in areas that are similar in terms of other characteristics within neighborhood types but are different between neighborhood types. Not surprisingly, the bad neighborhoods are characterized by low socioeconomic status, high immigrant density, and reputations for gang-related crime and violence. In contrast, good neighborhoods are characterized by high socioeconomic status, predominantly native Swedish residents and good reputation for neighborhood safety and security. Figure 1 presents the neighborhood characteristics for the good and bad neighborhood types, which confirm this picture.[7] Panels A-D show average annual income, share of foreign-born residents, employment rate, and share of residents with a university education. For all four variables, there is a clear difference in the averages/shares by area type, although they are similar for areas of the same type.

*** Figure 1 ***

Another clear difference between good and bad neighborhoods is their reputation for safety and security. According to a Swedish police study on criminal networks, five of six bad neighborhoods are classified as less safe areas, while none of the good neighborhoods are classified as less safe (Rikskriminalpolisen, 2014). See Appendix Figure A1 for the geographical distribution of criminal networks with great influence in the local community.

A relevant question is whether employers know which residential addresses are located in bad and good neighborhoods. If the signal is not strong, or clear, enough, we will not be able to detect the true neighborhood signaling effect. However, as we show below, we find clear evidence that

[7] These statistics are obtained from Statistics Sweden.

employers act on the neighborhood signal. The residential address contains three parts – a street name (and number), a postal code, and a municipality name. If employers are unfamiliar with the street name and postal code, they may easily locate them using the Internet. Since we find that employers act on commuting time, this seems rather realistic. Having pinpointed the residential address on a map, most employers are likely to have a perception of whether the neighborhood is good or bad.[8] However, given uncertainty about the signal's strength, one should interpret our estimates as lower bounds.

Although residential addresses are randomly assigned to job applications, we also have to control for the commute distance in order to identify the neighborhood signaling effect. For example, if bad neighborhoods are, on average, farther away from jobs and employers avoid hiring job applicants with long commutes, then we cannot separate the neighborhood signaling effect from the confounding effect of commuting time.

To separate the neighborhood signaling effect from the confounding effect of commuting time, we extracted the data on travel time to work using public transportation between the addresses we used for the fictitious job applicants and the actual addresses of the firms to which we applied for jobs in the experiment. This enables us to examine whether the bad neighborhood addresses selected for the experiment have longer commuting distances compared to the good neighborhoods. It also enables us to analyze how commuting time itself is associated with callback rates. We extracted information on commuting time from the Stockholm and Gothenburg public transport websites (www.resplanerare.sl.se and www.vasttrafik.se, respectively) when leaving home at 7 am on a Monday. For each sent job application, we entered the address stated in the job application

[8] Some employers may also have used information provided by the name of the municipality. Most employers are likely to be familiar with the municipalities we use, and the characteristics of the SAMS and the larger municipalities are correlated.

and the address of the firm to which we applied for a job and then recorded the website's estimates commuting time. In cases where more than one means of public transport was available – such as bus, tram and underground metro – we selected the type of public transport with the shortest commuting time. Of the 2,790 job applications, we were able to retrieve commuting time data for 2,049 job applications.[9]

Panel A in Figure 2 shows the average commuting times for good and bad neighborhoods. There is no strong evidence that commuting time differs between the two types of neighborhoods. The average commuting time for good neighborhoods is approximately 50 minutes, while the average commuting time for bad neighborhoods is approximately 45 minutes, a difference of only 5 minutes that favors the bad neighborhoods. This suggests there is no strong correlation between the type of neighborhood and the commuting time in these two major cities of Sweden – at least not for the neighborhoods included in the experiment. However, we still control for commuting time in the empirical analysis since we are interested in the association between the commuting time itself and the callback rate.

Panels B and C in Figure 2 present the distributions of commuting times in minutes for Stockholm and Gothenburg, respectively. It is interesting to note that although Gothenburg is smaller than Stockholm, the average commuting time for Gothenburg (60 minutes) is higher than the average for Stockholm (42 minutes). The shorter commuting time is probably explained by Stockholm's underground metro system, which is likely to be faster than the trams and buses that are the two main options for public transportation in Gothenburg.

[9] The addresses of the firms were obtained via the unique workplace identifier (a CFAR number used in administrative registers for firms by Statistics Sweden) linked to the firms' addresses. For 445 job applications, we were not able to calculate the commuting time because the firm address is missing in the Statistics Sweden data. For another 296 job applications, the address of the firm is missing because the firm is located outside the transport zone covered by the public transportation websites.

*** Figure 2 ***

In summary, the SAMS in good and bad neighborhoods are distinct in terms of important socioeconomic and other dimensions, while the areas are similar in these dimensions within neighborhood type. Bad neighborhoods have reputations of low socioeconomic status, high immigrant density, and histories of violence. Good neighborhoods have reputations of high socioeconomic status, low immigrant density, and safety.

3.2 Identities of the applicants

Since the broader project of which this paper is a part was also designed to investigate gender and ethnic discrimination, it includes both typical Swedish names for both males and females as well as typical Middle Eastern names for males. Because of statistical power considerations, we limited the number of dimensions of the broader project and therefore did not include typical Middle Eastern names for females. In the remainder of the paper, typical Middle Eastern names refer to male names. In each of the three groups of applicants, we used names that are among the most common in Sweden according to Statistics Sweden's name register. One-third of job applications had a typical Swedish male name, one-third a typical Swedish female name, and one-third a typical Middle Eastern male name. In a previous experiment on gender discrimination (Carlsson, 2011), we used three typical Swedish names for males and females. We did not find any name effects for these names, and therefore, we use only one of these names for male and female applicants. For typical Middle Eastern male names, we do not to use exactly the same names as in any previous study. To be able to investigate potential name effects in this case, we included three typical Middle

Eastern male names. The names used are Anna Nilsson, Erik Johansson, Ali Mustafa, Mohammed Ismail, and Hassan Said.

3.3 Occupations

In total, thirteen of the most common occupations in the Swedish labor market were included in the field experiment. The occupations include skilled, semiskilled and unskilled occupations. The included occupations were shop sales assistants, construction workers, computer professionals, motor vehicle drivers, business sales assistants, teachers (math/science and language teachers in compulsory school and a general category of secondary school teachers), accountants, nurses, restaurant workers, janitors/cleaners and maintenance/repair. Appendix Table A2 presents descriptive statistics for the occupations.

3.4 Constructing resumes

We aimed to construct resumes that represented a typical worker in each occupation, i.e., the goal was to make the resumes realistic while not referring to real persons. The first step was to construct a resume template that only contained general content. This was achieved by using real resumes that were available from a Swedish Employment Service database where job applicants can record their job applications. We calibrated and adjusted these resumes for our purpose. At this stage, we also used our previous experience conducting similar experiments and designing resumes.

In the next step, a computer program generated the final job applications by completing the resumes content depending, for example, on the randomization of neighborhood, ethnic background, gender, resume layout, the order in which the resumes were to be sent out and the positions to apply for. In the end, the resume consisted of a personal letter on one page and a detailed CV on another page (see Appendix B for a sample resume).

3.5 Procedures for sampling and recording responses

The field experiment was conducted from March to November 2007, and during this period, all employment advertisements for the included occupations that were posted on the Swedish Employment Service webpage were collected. A small fraction of employers did not accept job applications by e-mail, and these employers were not included in the experiment. We also applied to jobs in other parts of Sweden, but these observations are excluded, since we did not assign bad/good addresses for those regions. The reason is that strong residential segregation mainly occurs in the largest cities in Sweden. A clear majority of the jobs we applied for were in the two major cities of Sweden, Stockholm and Gothenburg.

We sent three applications to the same employer. This increased efficiency since more observations were obtained with a similar amount of resources. However, there is a trade-off between increased efficiency and the higher risk of being detected when sending multiple job applications. Therefore, we decided to not send more than three job applications to the same employer. To avoid suspicion, we generated three resume versions that had different typefaces and layouts.

We created email addresses and registered telephone numbers (with voicemail) to enable employers to contact the fictitious applicants. We have learned from previous field experiments that employers do not use regular mail to contact candidates. Therefore, the postal addresses used to signal neighborhoods are fictitious in the sense that the street numbers do not exist, although the street names are real. After recording the responses, we immediately declined any invitations for job interviews to avoid unnecessarily inconveniencing the employers.

4. Results

4.1 Descriptive Statistics

Table 1 presents the descriptive results for the callback rate by neighborhood type and commuting time. As seen in panel A, 2,790 job applications were sent to employers with advertised positions. The callback rate is approximately .26, meaning that around ¼ of the applications resulted in an invitation to a job interview. Roughly one-half of the job applications (1,373) signaled living in a bad neighborhood, and the callback rate for these job applications is approximately .24. The other half of the job applications (1,417) signaled living in a good neighborhood, and the callback rate for these job applications is approximately .27, a difference of approximately twelve percent. These raw numbers reveal no evidence of a strong negative signaling effect of living in a bad neighborhood.

*** Table 1 ***

A factor that could conceal a general neighborhood signaling effect, which is interesting in itself, is the commuting time to the job. Panel B in Table 1 presents the callback rates for different commuting times. The majority of job applications (1,598) have commuting times of less than 60 minutes. A total of 352 and 97 job applications have commutes of 61–90 minutes and more than 90 minutes, respectively. The callback rate for the two categories of job applications with commutes of less than 90 minutes are similar (.26 and .28, respectively), while job applications with commutes of more than 90 minutes deviate, with a much lower callback rate of .19. Thus, employers appear reluctant to interview job applicants with long commutes.

4.2. Estimating the neighborhood signaling effect

To investigate neighborhood signaling effects more formally, we need to conduct an analysis that controls for commuting time, since this is a potential confounding factor for the neighborhood signaling effect. Additionally, we are interested in the commuting time variable itself and its association with the callback rate. This motivates the estimation of the following specification:

$$Callback_i = \alpha + \beta_1 Bad\ NB_i + \beta_2 Minutes^{61-90} + \beta_3 Minutes^{>90} + X_i\beta_4 + \varepsilon_i \quad (1)$$

where the dependent variable, $Callback_i$, is a dummy variable that equals 1 if application *i* received a callback for an interview and 0 otherwise. $Bad\ NB_i$ is a dummy variable that equals 1 if job application *i* has a postal address in a bad neighborhood and 0 otherwise (the reference category being living in a good neighborhood). $Minutes^{61-90}$ and $Minutes^{>90}$ are commuting time dummies that take the value 1 if application *i* has a commuting time of 61-90 and more than 90 minutes, respectively. The reference category for commuting time is a commute of 60 minutes or less. The parameter α is the intercept, which is the callback rate when living in a good neighborhood; β_1 is the main parameter of interest, which gives the effect of the callback rate of *changing* ones residential neighborhood from a good one to a bad one; β_2 and β_3 estimate the association between commutes of 61-90 minutes and more than 90 minutes, respectively (both relative to less than 61 minutes), and the callback dummy.

The random assignment of neighborhood type facilitates the estimation of a causal neighborhood signaling effect, at least after controlling for commuting time. For precision, we also control for other randomly assigned characteristics of the job application, city, and occupation, denoted by the vector X_i. The vector X_i includes the following characteristics of the job application:

work experience, education, personal attributes, unemployment duration, leisure activity, city, occupation, order and format of the application. Excluding these characteristics makes little difference in the point estimates of β_1, β_2, and β_3.

4.3 Main results

We start by estimating the neighborhood signaling effect using equation (1) without controls for commuting time. Overall, we find evidence of a weak negative neighborhood signaling effect in this specification. The first column of Table 2 shows a point estimate of -.027, which is significant at the ten percent level. The interpretation is that the callback rate is 2.7 percentage points (or 14 percent) lower for job applicants who signaling living in a bad neighborhood as opposed to a good one.

*** Table 2 ***

The neighborhood signaling effect may differ by ethnicity. To investigate this possibility, we estimate a specification that allows for different neighborhood signaling effects for job applicants with typical native Swedish and Middle Eastern sounding names. The first two rows of column 2 report the results of this regression model. Interestingly, we only find evidence of a negative neighborhood signaling effect for job applicants with Middle Eastern names. While the neighborhood signaling effect is close to zero (-.003) and statistically non-significant for job applicants with typical Swedish names, there is a substantial negative effect (-.057) for job applicants with Middle Eastern names.[10] This implies that job applicants with typical Middle

[10] For applicants with typical Swedish names, the 95% confidence interval includes point estimates in the interval from -.043 to .037. Thus, we can rule out a negative neighborhood signaling effect larger than approximately 4

Eastern names have a 42 percent lower probability of receiving a callback for a job interview if their resume signals that they live in a bad neighborhood as opposed to a good one. Note that the general effect of -.027 for all job applicants, shown in the first column, is simply the weighted average of the effect for ethnic majority applicants (-.003) and that for ethnic minority applicants (-.057).

Next, we turn to the effect of commuting time on hiring. Commuting time is a potential cofounding factor for the neighborhood signaling effect. The spatial mismatch hypothesis suggests that employers treat workers with long commuting unfavorably. If deprived neighborhoods are located farther away from areas where jobs are located, then neighborhood signaling effects could capture the effects of commuting time. Although the descriptive statistics indicate that in our sample there is not a strong correlation between the type of neighborhood and commuting time, we cannot entirely rule out the possibility that the commuting time results in some bias in the estimated neighborhood signaling effect. To investigate this issue, we repeat the regressions in Table 2 adding controls for commuting time using dummy indicators for commute times of 0-60, 61-90, or more than 90 minutes; see Table 3. We find no evidence that commuting time is an important control variable in this context; the estimates of the neighborhood signaling effect are essentially unchanged. This holds if we include commuting time as a continuous variable (see Table A3).

*** Table 3 ***

percentage points at the 95% confidence level. If we exclude applicants with typical Swedish female names, the estimated coefficients in columns 1 and 2 are very similar in terms of magnitude (only marginally stronger) and statistical significance.

However, we are also interested in whether commuting time in itself is associated with callbacks for job interviews. The hypothesis is that employers are reluctant to hire job candidates with long commutes. Although commuting times of 0-60 and 61-90 minutes are not significantly related to the callback rate, job applicants with commutes longer than 90 minutes have substantially (and significantly) lower callback rates. The interpretation of the coefficient of this latter variable is that the callback rate is approximately 12 percentage points lower for job applicants that have to commute more than 90 minutes compared to job applicants with commutes shorter than 60 minutes.

We close this section by investigating heterogeneity in the neighborhood signaling effect with respect to city and gender. The results comparing the effects in Stockholm and Gothenburg are presented in Table A4. Column 2 reveals that in both Stockholm and Gothenburg, general neighborhood signaling effects are weak, close to zero and not different from each other in a statistical sense. In column 4, we investigate whether the neighborhood signaling effect for typical native Swedish and Middle Eastern names differs between Stockholm and Gothenburg. For job applicants with typical native Swedish names, the effect is close to zero and not statistically significant for both Stockholm and Gothenburg. For job applicants with typical Middle Eastern names, the neighborhood signaling effect is also very similar in both cities at -.057 for Stockholm and -.058 for Gothenburg. However, the precision of the estimates decreases when we estimate separate effects by city, which motivated us to merge the data in the main analysis and analyze the weighted average effect for the two cities.[11] Regarding heterogeneity by gender, there is no evidence of a difference in the callback rates of job applicants with native Swedish male and female names when living in either good or bad neighborhoods; see Column 3 of Table A5. Power

[11] The standard errors increase by 20 percent for Stockholm and more than doubles for Gothenburg compared to the estimate of the weighted average effect.

considerations again motivate the estimation of a weighted average effect of these two groups of job applicants in the main analysis.

5. Interpretations

How can we interpret the ethnic difference in the neighborhood signaling effect? In light of statistical discrimination theories (e.g., Aigner and Cain, 1977), the callback rate of minority job applicants living in worse neighborhoods could be lower if recruiters view their productivity as more uncertain. In these models, signaling a good neighborhood then removes more uncertainty about the productivity of job applicants with typical Middle Eastern (male) names when changing from a bad neighborhood to a good one. As a result, the callback rate increases relatively more for job applicants with typical Middle Eastern names compared to those with typical Swedish names when changing from a bad neighborhood to a good one.

However, it is also possible to imagine a modified model of preference-based discrimination (see Becker, 1957, for the original model). In such a model, the ethnic difference in the neighborhood signaling effect could be due to preference-based discrimination, with such preferences varying by neighborhood type. In other words, distaste for an ethnic minority may vary by the type of neighborhood in which the ethnic minority job applicant lives. In general, it is very difficult to distinguish among theories of preference and statistical discrimination using the kind of correspondence field experiment that we have conducted; see Carlsson et al. (2012) for a formal explanation.

A fascinating result is how much ethnic discrimination changes by moving to a neighborhood with a better reputation. The point estimates in Table 2 reveal ethnic callback gaps of approximately 15 and 9.4 percentage points in bad and good neighborhoods, respectively. This implies that ethnic

discrimination is reduced by almost 40 percent if a job applicant with a Middle Eastern name moves from a bad neighborhood to a good one, which is a substantial effect.

The results for the commuting time indicate that employers consider long commutes to be strong negative productivity signals, and they decide to statistically discriminate against job applicants with long commutes. Potential reasons for this include that employers expect workers with longer commuting times to be less flexible in terms of working hours, to be less able to work particular hours, to put less effort into work because of tiresome commuting and to be more likely to quit if they find a job closer to their neighborhood.

6. Conclusion

A common pattern in many metropolitan cities is the existence of neighborhoods where individuals who have low incomes and high levels of unemployment are concentrated. An important question is whether living in such deprived neighborhoods *causes* poor individual labor market outcomes. To investigate this question, we conducted a field experiment in the Swedish labor market in which 2,790 job applications were sent to employers with job vacancies. In our experiment, we first randomly assigned neighborhoods (residential addresses) to the job applications, which gives a causal neighborhood effect, and then add a control variable for commuting distance, which leaves us with the neighborhood signaling effect.

We find no evidence of a neighborhood signaling effect for typical native Swedish names. In contrast, for typical Middle Eastern names, we find a significant neighborhood signaling effect. Job applicants with typical Middle Eastern names who signal living in a bad neighborhood rather than a good one receive 42 percent fewer callbacks for job interviews. Moreover, adding a control for commuting time does not change the results, meaning that this is not a confounding factor of

the neighborhood signaling effect. However, we find that a long commute is itself negatively associated with callback rates. Apparently, employers consider information about residential addresses important to their employment decisions. Finally, we find no evidence of heterogeneity across the two cities included in the experiment or between male and female applicants with native Swedish names.

Why do we find a strong neighborhood signaling effect only for job applicants with typical Middle Eastern names? The theory of statistical discrimination provides a plausible explanation. Theoretical arguments suggest that there could be a greater uncertainty in terms of unobserved productive skills for minority job applicants, i.e., in our case, those with typical Middle Eastern names. In this case, the theory suggests that sending a positive signal regarding unobserved productive skills, such as living in a good neighborhood, could have a larger impact for a minority candidate and, as a result, raise the callback rate. However, we cannot rule out the possibility that preferences for hiring ethnic minorities vary by the neighborhood in which they reside. Given the large differences in ethnic discrimination we find depending on what kind of neighborhood the applicants reside in, future studies should measure whether uncertainty over productivity varies by residential location and/or whether attitudes toward minorities differ by residential location.

An important observation is that ethnic discrimination is reduced by almost 40 percent if a job applicant with a Middle Eastern name changes his residential address from a bad neighborhood to a good one. This means that residential segregation is a serious problem beyond obvious issues, such as a lack of good role models, negative peer influences, weak labor market networks and/or weak local labor markets. Residential segregation results in more ethnic discrimination, potentially because it reinforces issues of uncertainty regarding the unobserved productive skills of minority job applicants.

The fact that we find lower callback rates for job applicants with long commuting times suggests that employers view this as a signal of lower productivity. Potential reasons for this include that employers expect workers with longer commutes to be less flexible in terms of working hours, to be less able to work particular hours, to put less effort into work because of tiresome commuting, and to be more likely to quit if they find a job closer to their neighborhood. To put the size of this effect into perspective, it is of the same magnitude as the (negative) effect of having a typical Middle Eastern name (in a bad neighborhood). Moving to a neighborhood with a commute shorter than one and a half hours, as opposed to living more than one and a half hours from the job, would raise the callback rate by approximately 12 percentage points.

While our experimental approach is an effective way of identifying unequal treatment in hiring, it still has some weaknesses. First, it measures unequal treatment only at the initial stage of the hiring process, but unequal treatment may also exist in promotions and wage setting. On the other hand, discrimination may be most widespread at the initial stage during which it is easy for employers to avoid certain job applicants and uncertainty about job applicant qualifications is likely to be higher. Second, our findings apply only to job applicants that use a formal search method to find a job, whereas many job applicants use informal search methods or become self-employed.

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Tables

Table 1. Descriptive results of field experiment.

	Callback rate	Number of job applications
A) Bad and good neighborhoods		
All Applications	.26	2,790
Bad Neighborhood	.24	1,373
Good Neighborhood	.27	1,417
B) Commuting time		
All Applications	.26	2,790
< 60 minutes	.26	1,598
60-90 minutes	.28	352
> 90 minutes	.19	97
Commuting time missing	.26	743

Table 2. Neighborhood signaling effect and ethnic background.

	(1)	(2)
Neighborhood signaling effect	-.027*	
	(.016)	
Neighborhood signaling effect, native Swedish name		-.003
		(.021)
Neighborhood signaling effect, Middle Eastern name		-.057**
		(.024)
Middle Eastern name (good neighborhood)		-.094***
		(.023)
Constant	.192**	.231***
	(.078)	(.078)
Number of applications	2,790	2,790
p-value (test of equal NH coeff. for native Swedish and Middle Eastern names)		.091

Notes: The dependent variable is a dummy variable that equals one if the applicant was invited to a job interview, otherwise zero. The neighborhood signaling effect is the effect of changing the CV from signaling living in a good to living in a bad neighborhood. The regression specification in column (2) is $Callback_i = \alpha + \beta_1 Bad\ NB_i\ x\ Swedish\ name + \beta_2 Bad\ NB_i\ x\ Middle\ Eastern\ name + \beta_3 Middle\ Eastern\ name + X_i\beta_4 + \varepsilon_i$. The reference category in column 1 is a job applicant living in a good neighborhood, while the reference category in column 2 is a job applicant with a Swedish name living in a good neighborhood. All model specifications are linear probability models and controls for individual attributes on work experience, education, personal attributes, unemployment spell, leisure activity, city, occupation, order and format of the application (see appendix D for details). The standard errors are clustered at the job level. *, ** and *** denote 10, 5 and 1 percent significance level.

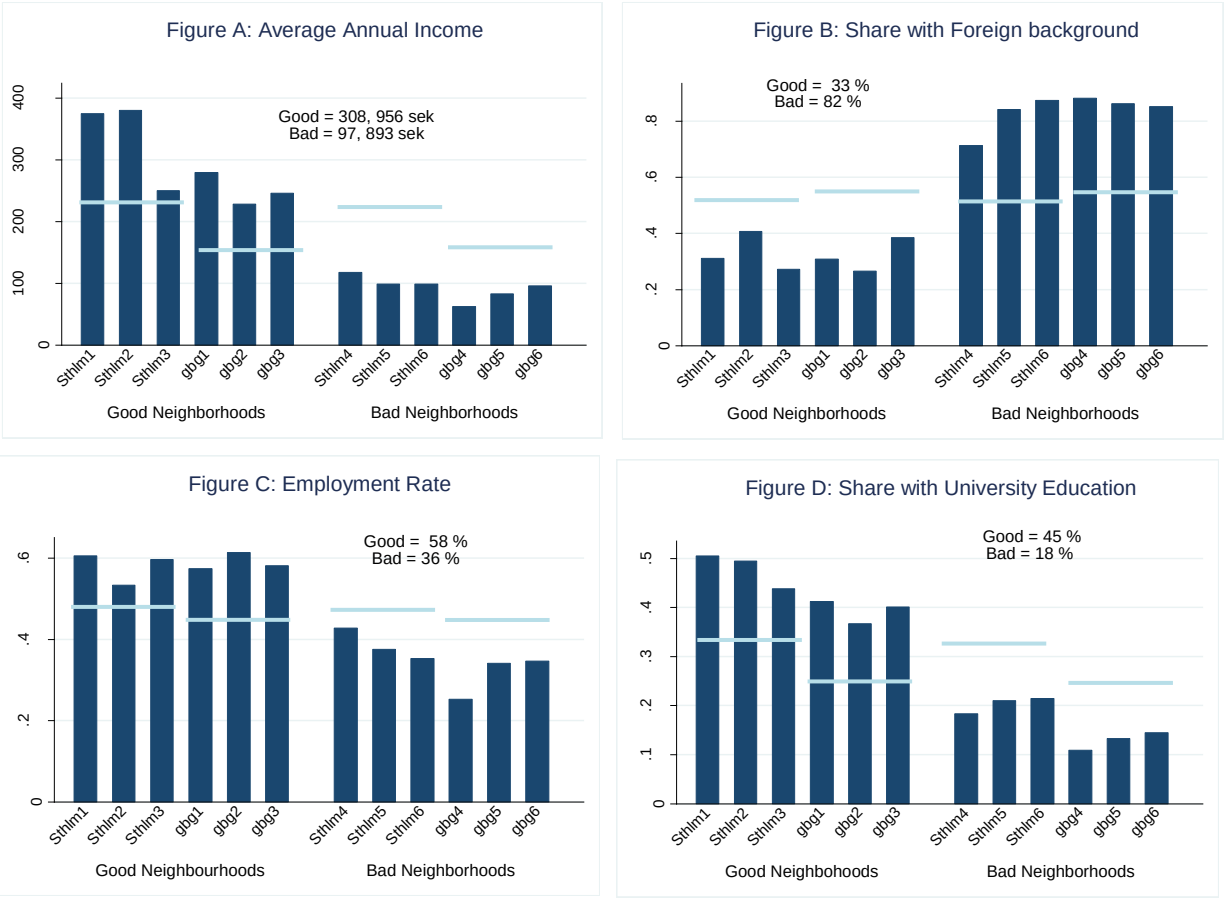
Table 3. Neighborhood signaling effect, ethnic background, and commuting time.

	(1)	(2)	(3)	(4)
Neighborhood signaling effect	-.027*	-.028*		
	(.016)	(.016)		
Neighborhood signaling effect, native Swedish name			-.003	-0.004
			(.021)	(.021)
Neighborhood signaling effect, Middle Eastern name			-.057**	-.059**
			(.024)	(.024)
Middle Eastern name (good neighborhood)			-.094***	-.095***
			(.023)	(.023)
61-90 minutes		-.005		-.005
		(.027)		(.027)
> 90 minutes		-.118***		-.124***
		(.045)		(.045)
Constant	.192**	.191**	.231***	.231***
	(.078)	(.078)	(.078)	(.078)
Number of job applications	2,790	2,790	2,790	2,790
p-value (test of equal NH coeff. for Swedish and Middle Eastern names)			.091	.087

Notes: Column 1 and 3 repeat the regressions in Table 1. Column 2 and 4 adds controls for commuting time. The reference category for commuting time is commuting time less than or equal to 60 minutes. In the specifications of columns 2 and 4, we also add a dummy for the missing observations on commuting time (which is equal to 1 if commuting time is missing and zero otherwise). See also the notes below Table 2. The standard errors are clustered at the job level. *, ** and *** denote 10, 5 and 1 percent significance level.

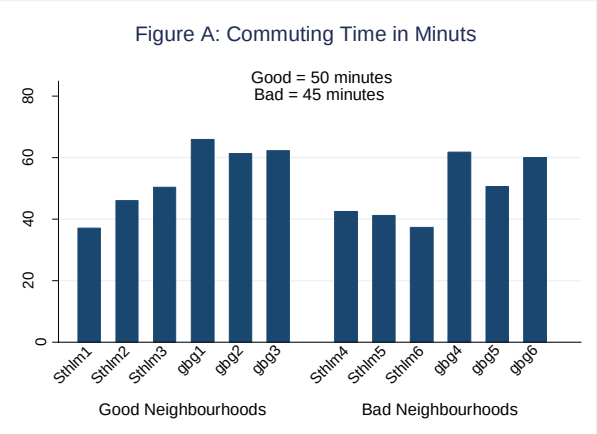
Figures

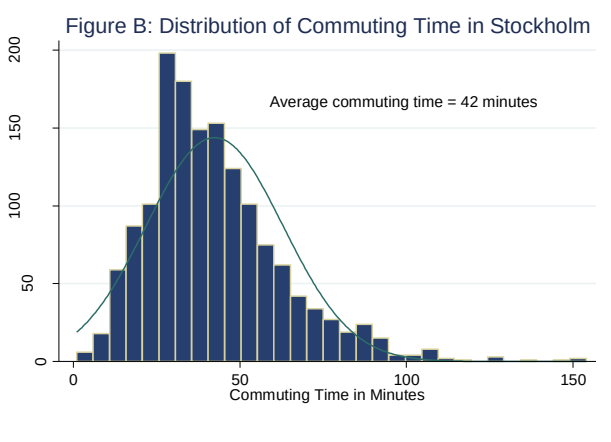
Figure 1: Socio-economic Characteristics of Good and Bad Neighborhoods.

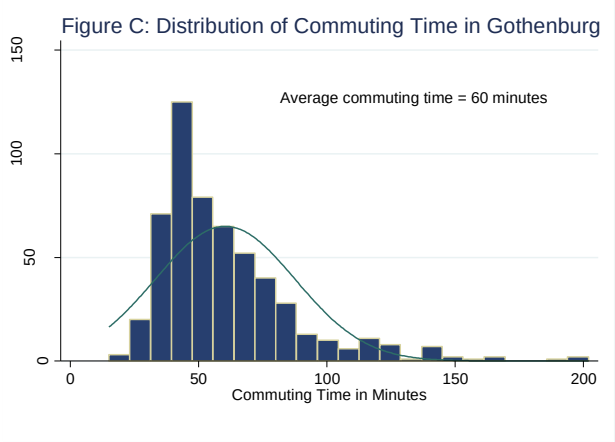


Notes: The data is from Statistics Sweden, measured in 2007 (the year the experiment was conducted). The data used to calculate the neighborhood characteristics includes all individuals above the age of 16. The neighborhood area of each postal address is defined at Small Area Market Statistics (SAMS) level. The labels on the x-axis represent the 12 neighborhood addresses used for the experiment. The label Sthlm1-3 and gbg1-3 represent three good neighborhood types in Stockholm and Gothenburg countries, respectively. Sthlm4-6 and gbg4-6 represents the bad neighborhood types in Stockholm and Gothenburg, respectively. For each variable in Figure A-D the horizontal line shows the mean value across all neighborhood areas in Stockholm and Gothenburg, respectively.

Figure 2: The distribution of commuting time.







Appendix A

Table A1. Postal address used in the experiment.

	Address 1	Address 2	Address 3
Bad neighborhoods			
Stockholm	Spårfinnargränd xx 124 64 Bandhagen	Oslogatan xx 164 32 Kista	Ortopedvägen xx 141 53 Huddinge
Gothenburg	Merkuriusgatan xx 415 19 Göteborg	Salviagatan xx 424 40 Angered	Dimvädersgatan xx 418 37 Göteborg
Good neighborhoods			
Stockholm	Sunnerdahlsvägen xx 167 62 Bromma	Eiravägen xx 18260 Djursholm	Allmogevägen xx 18730 Täby
Gothenburg	Vanadisgatan xx 426 76 Västra Frölunda	Hovåsvägen xx 43650 Hovås	Triangelstigen xx 436 42 Askim

Notes: The actual street numbers used in the experiment are not shown in the table.

Table A2: Descriptive Statistics of the Occupations

Occupations	Number of applications	Fraction applications
High skill jobs		
Computer professionals	352	.13
Accountants and auditors	227	.08
Compulsory school teachers (math/natural Science)	102	.04
Compulsory school teachers (Swedish/social science)	107	.04
Secondary school teachers	121	.04
Registered nurses	140	.05
Low and medium skill jobs		
Janitors and cleaners	139	.05
Installation, maintenance, and repair occupations	101	.04
Shop sales assistant	372	.13
Construction workers	153	.05
Motor vehicle driver	211	.08
Business sales assistant	574	.21
Restaurant workers	191	.07

Table A3. Neighborhood signaling effect, ethnic background, and commuting time continuously.

	(1)	(2)	(3)	(4)
Neighborhood signaling effect	-.027*	-.031*		
	(.016)	(.016)		
Neighborhood signaling effect, native Swedish name			-.003	-.007
			(.021)	(.021)
Neighborhood signaling effect, Middle Eastern name			-.057**	-.063**
			(.024)	(.024)
Middle Eastern name (good neighborhood)			-.094***	-.094***
			(.023)	(.023)
Commuting time (minutes)		-.0013***		-.0013***
		(.0004)		(.0004)
Constant	.192**	.249***	.231***	.288***
	(.078)	(.080)	(.078)	(.080)
Number of job applications	2,790	2,790	2,790	2,790
p-value (test of equal NH coeff. for Swedish and Middle Eastern names)			.091	.084

Notes: This table use commuting time as a continuous variable. Otherwise it mirrors Table 3. The standard errors are clustered at the job level. *, ** and *** denote 10, 5 and 1 percent significance level.

Table A4. Neighborhood signaling effect and city.

	(1)	(2)	(3)	(4)
Neighborhood signaling effect	-.027*			
	(.016)			
Stockholm		-.032*		
		(.020)		
Gothenburg		-.015		
		(.028)		
Neighborhood signaling effect, native Swedish name			-.003	
			(.021)	
Stockholm				-.011
				(.025)
Gothenburg				.015
				(.036)
Neighborhood signaling effect, Middle Eastern name			-.057**	
			(.024)	
Stockholm				-.057**
				(.029)
Gothenburg				-.058
				(.045)
Middle Eastern name (good neighborhood)			-.094***	
			(.023)	
Stockholm				-.105***
				(.028)
Gothenburg				-.069*
				(.042)
Gothenburg		-.029		-.046
		(.026)		(.032)
Constant	.192**	.195**	.231***	.235***
	(.078)	(.078)	(.078)	(.078)
Number of job applications	2,790	2,790	2,790	2,790

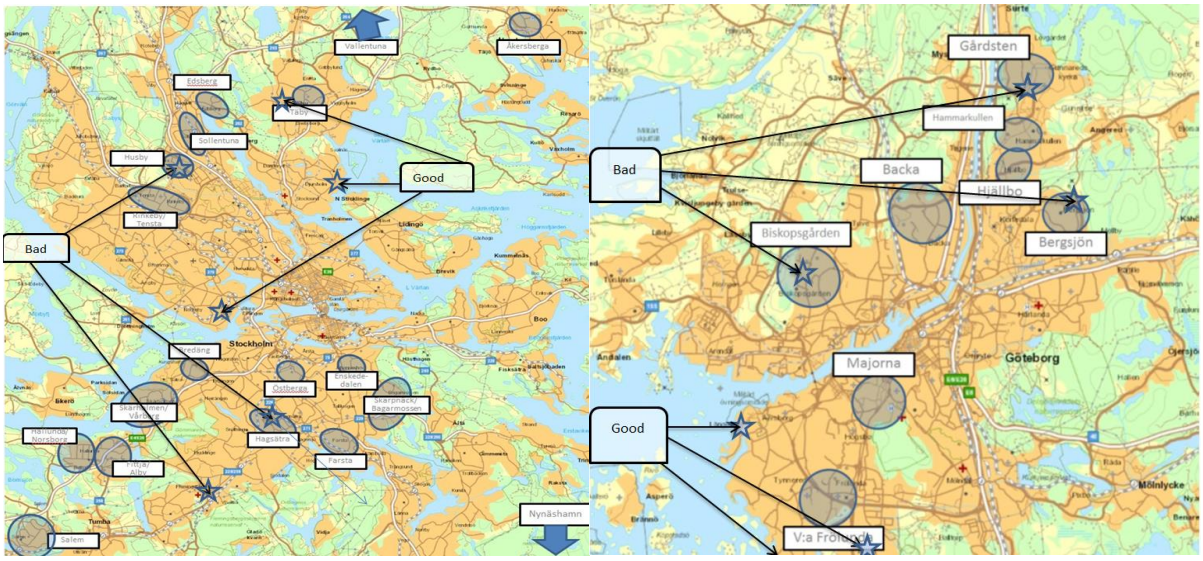
Notes: Column 1 repeats column 1 in Table 2. Column 2 allows for different neighborhood effects for Stockholm and Gothenburg, i.e. the estimates are a weighted average of the estimates in the previous column. Column 3 repeats column 2 of Table 2. Column 4 allow for different neighborhood effects for Stockholm and Gothenburg for job applicants with typical Swedish and Middle Eastern names, i.e. the estimates are weighted averages of the estimates in the previous column. The reference category in column 2 is a job applicant living in a good neighborhood in Stockholm, while the reference category in column 4 is a job applicant with a Swedish sounding name living in a good neighborhood in Stockholm. See also notes blow Tables 2. The standard errors are clustered at the job level. *, ** and *** denote 10, 5 and 1 percent significance level.

Table A5. Neighborhood signaling effect and gender.

	(1)	(2)	(3)
Neighborhood signaling effect	-.027*		
	(.016)		
Neighborhood signaling effect, native Swedish name		-.003	
		(.021)	
Male name			-.012
			(.029)
Female name			.003
			(.030)
Neighborhood signaling effect, Middle Eastern name		-.057**	-.057**
		(.024)	(.024)
Middle Eastern name (good neighborhood)		-.094***	-.080***
		(.023)	(.026)
Female name (good neighborhood)			.031
			(.028)
Constant	.192**	.231***	.212
	(.078)	(.078)	(.079)
Number of job applications	2,790	2,790	2,790

Notes: Columns 1 and 2 repeat the same columns in Table 2. Column 3 allows for different neighborhood signaling effects for men and women with Swedish sounding names, i.e. the estimates are a weighted average of the estimates in the previous column. The reference category in column 3 is a male job applicant with a Swedish sounding name who lives in a good neighborhood. See also notes blow Tables 2. The standard errors are clustered at the job level. *, ** and *** denote 10, 5 and 1 percent significance level.

Figure A1. Criminal networks in Stockholm (left) and Gothenburg (right)



Notes: The source is Rikskriminalpolisen (2014). The circles in the above Figures show the neighbourhood areas identified by the Swedish National Police study as areas with local criminal networks. The stars in the above Figures show the neighborhood addresses selected for our study. Among the selected 6 bad neighborhood types, five of them are identified as areas with local criminal networks. Among the selected 6 good neighbourhood types, none are identified as areas with local criminal network.

Appendix B: Example of a resume

Ansökan till den lediga tjänsten

Hej!

Jag läste den utannonserade tjänsten och tyckte att det lät mycket intressant. Här följer en kort beskrivning av mina tidigare erfarenheter och mig som person.

På min tidigare arbetsplats, The Stadium, arbetade jag som avdelningsansvarig på junioravdelningen. Detta arbete gav mig erfarenhet både i att exponera varor, bemöta kunder och tillsammans med mina medarbetare hålla ordning på min avdelning. Under min tid där fick jag självklart också god kassavana. Arbetet på Stadium lärde mig även att se till att såväl kunden som de anställda trivdes i butiken.

Mina vänner och före detta kollegor tycker att jag är en varm och social person som funkar bra tillsammans med andra både på jobbet och annars. Dessutom så tycker jag att det är viktigt att se till människors behov och inte bara på ekonomin. Jag har en stark medkänsla med människor som är mindre lyckligt lottade än jag själv och är lite aktiv i Röda korsets hjälpverksamhet.

På kvällar och helger njuter jag helst av att bara vara ledig. Vi bjuder gärna hem bekanta på middag då vi tycker om att laga mat och ha sköna hemmakvällar.

Beträffande jobbet gillar jag att arbeta men tycker samtidigt att det är viktigt att det finns en balans mellan jobb och fritid. Bäst är de dagar där jag känner att jag gjort mitt på jobbet men ändå har ork att vara aktiv på fritiden. Det är inte viktigt för mig att vara bäst utan jobbkollegorna skulle nog beskriva mig som ganska avspänd.

Jag bor för tillfället med min sambo i Stockholm.
Med hopp om ett personligt möte

Vänligen,
Anna Nilsson

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Bakgrund (intyg och betyg finns)

ANSTÄLLNINGAR
Butikssäljare

Arbetsgivare:	The Stadium AB, Stockholm
Tidsperiod:	200409 - 200609
Beskrivning:	Övergripande ansvar för junioravdelningen. Kassavana, exponering av kläder samt kundkontakt.

Brevbärare

Arbetsgivare:	Posten, Stockholm
Tidsperiod:	Sommarjobb under studietiden
Beskrivning:	Utdelning av post

UTBILDNINGAR
Andreasgymnasiet, Stockholm

Utbildningsnivå:	Gymnasial utbildning
Inriktning:	Samhälle
Tidsperiod:	2001 - 2004
Beskrivning:	3-årig naturvetenskaplig linje

ANDRA KVALIFIKATIONER
Körkort – B

Språk
Svenska - Flytande
Engelska - Flytande

Dataprogram, allmänna kompetenser
Excel, kalkylprogram
Word, ordbehandlingsprogram