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ABSTRACT

How Important is Access to Jobs? Old Question - Improved Answer*

We study the impact of job proximity on individual employment and earnings. The analysis exploits a Swedish refugee dispersal policy to get exogenous variation in individual locations. Using very detailed data on the exact location of all residences and workplaces in Sweden, we find that having been placed in a location with poor job access in 1990-91 adversely affected employment in 1999. Doubling the number of jobs in the initial location in 1990-91 is associated with 2.9 percentage points higher employment probability in 1999. The analysis suggests that residential sorting leads to underestimation of the impact of job access.

JEL Classification: J15, J18 and R23
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1 Introduction

The recent riots in France in November 2005 combined with the riots in England (in Oldham, Leeds, Burnley and Bradford) in the summer of 2001, and those in the United States in April 1992 (in Los Angeles, referred to as the Rodney King riots) had in common that most of the rioters belonged to ethnic minority groups: children of immigrants from Arab and African countries in France, young British Asian men in England and young black and Latino males in the United States. The common explanation put forward was the high degree of racial segregation and the high unemployment rates experienced by these groups.

It is indeed true that, in most industrialized countries, majority and minority groups have very unequal (labor market) outcomes. For example, most American cities exhibit a high level of racial segregation and stark socioeconomic disparities between neighborhoods (Cutler et al., 1999). Not surprisingly, an important debate has focused on the existence of a possible link between residential segregation and the adverse labor-market outcomes of racial minorities. Empirical studies have shown that such a link exists (see, for instance, Cutler & Glaeser, 1997). It remains, however, unclear which economic mechanisms account for this link.[1] We focus here on one potentially important mechanism: job access in the individual's place of residence. As described below, our analysis combines unusually rich and detailed data with a quasi-experiment in which the location of people was decided by the Swedish government. Thus, our study overcomes many of the econometric problems plaguing previous studies on this topic.

The spatial mismatch hypothesis initiated by Kain (1968) provided an important insight into the debate on the pervasive labor market disadvantages of some minorities. Kain argued that residing in urban segregated areas distant from and poorly connected to major centers of employment growth, minority workers face strong geographic barriers to finding and keeping well-paid jobs. In particular, white city dwellers experience much better labor market outcomes than blacks.

In the US context, where jobs have been decentralized and blacks have stayed in the central part of cities, the main conclusion of the spatial mismatch hypothesis is to put forward the distance to jobs as the main culprit for the high unemployment rates and low earnings among blacks. The spatial mismatch literature has focused on race under the presumption that (inner-city) blacks are not residing close to (suburban) jobs.[2] Since the study of Kain, hundreds of studies have been carried out trying to test the spatial mismatch hypothesis[3] (see, in particular, the literature surveys by Holzer, 1991, Kain, 1992, Ihlanfeldt & Sjoquist, 1998). The usual approach to test the spatial mismatch hypothesis is to relate a measure of labor market outcomes

[1] Cutler & Glaeser (1997) estimate that a 13 percent reduction in residential segregation would eliminate one third of the black/white gap in schooling, employment, earnings, and unwed pregnancy rates. This leads the authors to conclude that segregation is extremely harmful to blacks even though they "do not have an exact understanding of why this is true".

[2] In the United States, it is often argued that blacks are disproportionately affected by spatial mismatch because their residential locations are more severely constrained than those of lower-skilled whites due to racial discrimination in housing and mortgage markets.

[3] Most empirical studies are using US data. Very few are European. Exceptions include Thomas (1998) and Patacchini & Zenou (2005), for the UK, and Dujardin et al. (2005) for Belgium.

(employment or earnings), based on either individual or aggregate data, to a measure of job access, typically some index that captures the distance from residences to centers of employment.

The main problem with this literature is that it is plagued by endogeneity problems. The main econometric problem is that *residential location is endogenous* because families are not randomly assigned a residential location but instead choose it. Indeed, self-selection and unobserved heterogeneity (for example unobserved productivity such as motivation or perseverance) rather than distance to jobs may explain why black workers have adverse labor market outcomes. It may well be that the more (unobserved) productive black workers choose locations close to jobs while the others reside further away. There may also exist reverse causality running from employment to job access (Ihlanfeldt, 2006). It may well be that better labor market outcomes of workers in some neighborhood attract firms into the area, which implies a higher neighborhood job access. As noted by Ihlanfeldt (1992), if the simultaneity between employment and residential location is ignored, the estimated effect of job access on employment will likely be biased toward zero.

Researchers have been dealing with these endogeneity problems e.g. by exploiting inter-city variations in black residential centralization (assuming that sorting across metropolitan areas is not an issue) to estimate the effect of job access on black employment (Cutler & Glaeser, 1997, Weinberg, 2000, 2004). Another way is to focus the analysis on youth who still reside with their parents since residential location is decided by parents for their children (Raphael, 1998). Though probably better than the methods used in many previous studies, there are strong limitations also in these approaches. For example, if parents and children share the same unobserved heterogeneity (in terms of productivity), the youth approach does not solve the selection problem.

Another problem in this literature that was highlighted by Ihlanfeldt & Sjoquist (1998) is that measures of job accessibility often contain measurement errors. For a given worker, the correct accessibility measure is arguably the number of nearby *relevant job vacancies* relative to the competing labor supply. The commonly used number of nearby occupied jobs per worker captures only vacancies that arise from turnover, not those created by job growth. Furthermore, this measure does not allow for the possibility that proximity to certain types of jobs is the relevant indicator (which causes a problem if different types of jobs vary in their distribution across areas).

A final problem of the traditional approach is that omitted variables may bias the results. In particular, in the case of individual-level data, neighborhood variables are generally not available because the individual's neighborhood or census tract is not identified for reasons of confidentiality. As stated by Ihlanfeldt & Sjoquist (1998), "the failure to consider both job accessibility and neighborhood effects together is problematic, because neighborhoods with negative effects are frequently distant from job opportunities for less-educated workers". Also, census tracts are typically not defined to capture aspects of job access.

The aim of the present study based on individual data is to overcome most of the econometric problems described above by (*i*) exploiting a quasi-experiment based on a policy in

Sweden, under which the government assigned refugees to neighborhoods with different degrees of geographic job accessibility and (*ii*) by using a very rich data set with coordinates for the residence and the workplace of all Swedish workers, which enables us to calculate individual based job access measures.

Most importantly, by using the policy experiment we are able to address properly the endogeneity issues discussed above. The refugee was not free to choose his/her preferred location. Also, the officials handling placement only acted on factors observed to us; there was no direct interaction with the refugees. Indeed, in our case, any excluded individual variable should be uncorrelated with the measure of job accessibility, resulting in an unbiased estimate of the effect of job access on labor market outcomes. Given that data on all jobs and individuals in consecutive years are available, we can compute job growth rates and look at jobs of different types. We can also derive measures of neighborhood characteristics at a very disaggregate level because we have information on nearby jobs of different types (and job growth rates) per worker, together with detailed neighborhood characteristics. With the help of the rich data, we avoid much of the measurement error and the omitted variable problems mentioned above. We believe that this is the first study that is able to overcome so many of the problems inherent to the testing of the impact of job access on labor market outcomes.[4]

Let us now summarize our main findings. First, we find that immigrants who in 1990-91 were placed in a location surrounded by few jobs had difficulties to find work also after several years in 1999. Doubling the number of jobs in the initial location in 1990-91 is associated with 2.9 percentage points higher employment probability in 1999. Second, our investigation suggests that residential sorting leads to underestimates of the importance of geographic distance to jobs. OLS regressions relating contemporary job access to individual outcomes shows no significant effect of job access on employment probabilities, neither for the 1990-91 refugee sample nor for a random sample of immigrants to Sweden. If we are willing to generalize the sign of this bias to the overall Swedish population—where we find a positive association between job access and outcomes—our findings imply that job access does in general have an impact on individual labor market outcomes. Finally, we show that immigrants have lower access to jobs than natives but this cannot fully explain the vast employment gap between immigrant and native workers.

The rest of the paper is outlined as follows. Section 2 briefly presents some theories on why distance to jobs may matter for individual labor market outcomes. Section 3 gives an overview of ethnic minorities in Sweden and the governmental refugee placement policy utilized in the empirical analysis. The data are described in section 4, beginning with the construction of the dataset and then turning to the characteristics of the different samples studied. Section 5 contains the empirical analysis. We first show how job access is generally related to

[4] Other experiments have been used in the literature, such as the Moving to Opportunity (MTO) programs, which relocate families from high- to low-poverty neighborhoods (Ludwig et al., 2001, and Kling et al., 2005), and the Toronto housing program where adults were assigned as children to different residential housing projects in Toronto (Oreopoulos, 2003). However, in these studies, the main objective is to analyze the impact of peer effects rather than job access on different outcomes of workers.

employment and earnings in the Sweden. Then we perform the analysis on the refugees who were subjected to the municipal placement policy. Section 6 concludes.

2 Theories

Despite an abundant empirical literature, theoretical models have emerged only recently, which probably explains why the mechanisms of spatial mismatch has long remained unclear and not properly tested (Gobillon et al., 2005). In this section, we present some mechanisms that have been proposed to explain the impact of spatial mismatch. Even though we do not test a particular mechanism, it would help us to understand and to interpret some of our results obtained below. Among the possible mechanisms are:

(*i*) Workers' job search efficiency may decrease with distance to jobs and, in particular, workers residing far away from jobs may have few incentives to search intensively (Smith & Zenou, 2003). Also, for a given search effort, workers who live far away from jobs have few chances to find a job because, for instance, they get little information on distant job opportunities (Ihlanfeldt, 1997, Wasmer & Zenou, 2002). Based on search-matching models, these theories state that distance to jobs can be harmful because it implies low search intensities. Indeed, locations near jobs are costly in the short run (both in terms of high rents and low housing consumption), but allow higher search intensities, which in turn increase the long-run prospects of reemployment. Conversely, locations far from jobs are more desirable in the short run (low rents and high housing consumption) but allow only infrequent trips to jobs and hence reduce the long-run prospects of reemployment. Therefore, for the workers who reside far away from jobs, it will then be optimal to spend a minimal amount of time in searching for jobs, and thus their chance of leaving unemployment will be quite low.

(*ii*) Workers may refuse jobs that involve commutes that are too long because commuting to that job would be too costly in view of the proposed wage (Coulson et al., 2001; Brueckner & Zenou, 2003). This will cause them to restrict their spatial search horizon at the vicinity of their neighborhood. If, for some reason, workers are skewed towards the Central Business District (CBD) and thus have their residences remote from the suburbs, then, because of higher commuting costs, few of them will accept Suburban Business District (SBD) jobs and will therefore search for jobs at the vicinity of the CBD, thus restricting their area of search. This makes the CBD labor pool large relative to the SBD pool. Under either a minimum-wage or an efficiency wage model, this enlargement of the CBD pool leads to a high unemployment rate among CBD workers and lower wages.

(*iii*) If workers' productivity negatively depends on distance to jobs then workers may refuse jobs that involve commutes that are too long and employers may be less willing to hire people living far away from the workplace. Because of the lack of good public transportation in large US metropolitan areas, especially from the central city to the suburbs, workers have relatively low productivity at suburban jobs because they arrive late to work due to the unreliability of the mass transit system that causes them to frequently miss transfers. If this is true, then firms may draw a red line beyond which they will not hire workers (Wilson, 1996, Zenou, 2002,).

All these mechanisms are equally valid for the majority group and ethnic minorities. However, in the US, (inner-city) blacks are not in general residing close to (suburban) jobs, either because they are discriminated against in the (suburban) housing market or because they want to live near members of their own race. So these different mechanisms are particularly relevant to explain the high unemployment rates experienced by black workers in the US.

Though these models have been constructed with the American situation in mind, they can easily be reinterpreted for European and, in particular, Swedish cities. It suffices to "flip" the city so that ethnic minorities live predominantly in the suburbs and most jobs are in the CBD. We will return to the issue of residential segregation in Sweden in the next section.

3 Some facts about Sweden

3.1 Ethnic Minorities and Residential Patterns in Swedish Cities

To an even larger degree than many other European countries, Sweden has experienced a dramatic change in its population composition during the last five decades. In 1960, there were about 300,000 immigrants in Sweden. Today, there are over 1,000,000 foreign-born, constituting twelve percent of Sweden's population of nine million. Most of the ethnic variation in Sweden comes from recent immigration. The immigrant population of non-European descent has grown from virtually zero to substantial numbers since the 1960s. For example, the Asian-born amounted to 300,000 people in 2003. The corresponding figure for Africa (South America) was 62,000 (55,000).

Like in most Western countries, immigrants are concentrated in large cities. Sweden is a small country in terms of population, and has very few areas that would be considered metropolitan in an international perspective. The primary candidate is the greater Stockholm area, which has a population of 1.7 million. In official Swedish statistics, the areas of Gothenburg and Malmö are also classified as metropolitan (populations of 800,000 and 500,000 respectively). The three metropolitan areas host half of the immigrant population but only one third of the overall population. The residential concentration is even more pronounced for many groups born in Africa, Asia, and South America.

The difference in the residential distribution coincides with frequent problems in the Swedish labor market. In 2002, the employment rate among those born outside Europe was as low as 53.5 percent, to be compared with 76.8 percent for the Swedish-born and 69.3 percent for immigrants from EU/EES countries. Wage differences are in general much lower than the employment disparities, but follow the same pattern in terms of disadvantaged groups. The average monthly (full-time) wage among the Swedish-born was SEK 22,250 in 2002; for immigrants from non-European countries it was SEK 19,050.

Larger Swedish cities typically have a "European" urban structure with a rich city center where most jobs are concentrated. The immigrant populations—particularly those of non-European descent—are concentrated in the suburbs with predominantly rental housing (Andersson, 2000). With very few exceptions, immigrant neighborhoods contain a mix of people from many parts of the world. The common denominator is that few ethnic Swedes live in these areas.

Figure 1 presents the patterns of job location and immigrant density in Stockholm.[5] Clearly, the jobs (left map) are in the central parts of the city, and the very immigrant dense areas (right map) are scattered in the suburban areas. So, there seems to be a spatial mismatch between where ethnic minorities live and where jobs are. Observe, however, that most of the strongly immigrant-dominated neighborhoods were built within the so-called "Million-housing-program" in the late 1960s and early 1970s. Many natives have left these locations in the last two decades, which has increased the immigrant concentration (Andersson, 2000). Despite poor amenities in many dimensions, the areas are relatively well-connected to the public transportation system. In other words, high (time or monetary) costs for commuting to the central business district may not be an important explanation as to why these areas are poorer than other suburbs.[6]

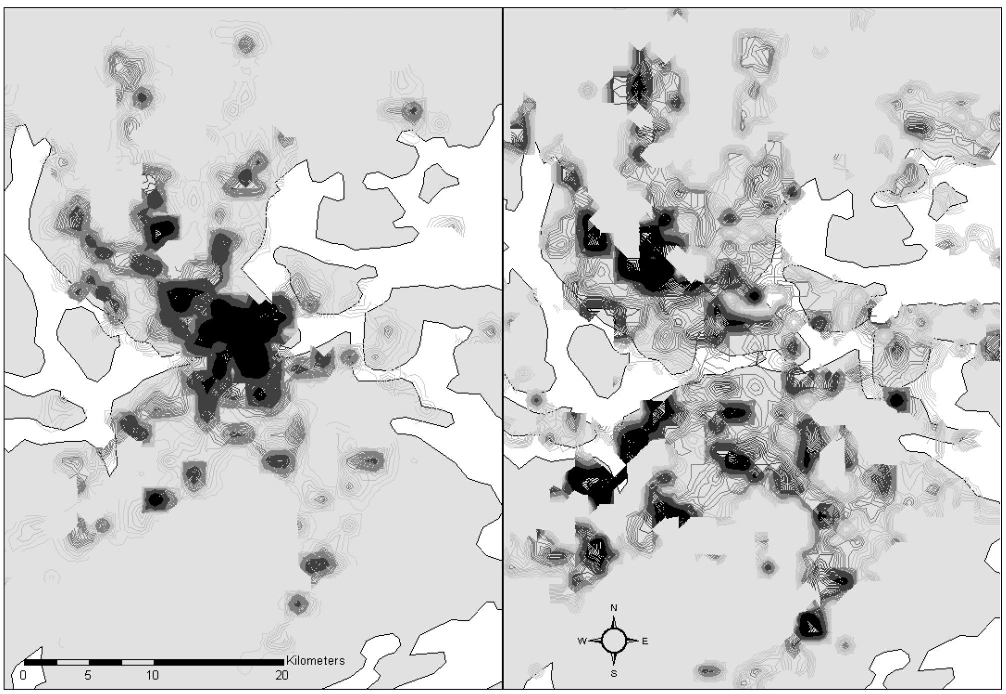

Figure 1 Job density and foreign-born population in the Stockholm area.

Notes: Tight contours (dark shades) indicate high job density (left map) and high fraction foreign-born (right map) respectively. The maps originate from two raster images, where each cell/pixel in the images contains information on the number of jobs or shares of immigrant residents. To improve visual ability, the raster maps have been converted into contours.

[5] The data used to generate the graphs are described in the next section.

[6] Note that our primary aim is not to test whether differing job proximity is an explanation to differences in average group outcomes, but to see whether job access is related to outcomes at the individual level.

3.2 The refugee placement policy in the 1990s[7]

In 1985, the Swedish Immigration Board was given the responsibility of handling refugee reception. A first step was to implement a refugee dispersal policy, where recently arrived immigrants where assigned to an initial place of residence. The placement policy was a reaction to immigrant concentration in large cities. The idea was to distribute asylum seekers over a larger number of municipalities that had suitable characteristics for reception, such as educational and labor market opportunities. Initially, the plan was to focus on 60 reception locations, but due to the increasing number of asylum seekers in the late 1980s, a larger number became involved; in 1989, 277 out of Sweden's (then) 284 municipalities participated to the policy. Instead of the labor market criteria that initially were supposed to govern the policy, the availability of housing came to determine placement.[8]

The policy of assigning refugees to municipalities was formally in place from 1985 to 1994. During 1987–91, the placement rate, i.e., the fraction of refugee immigrants assigned an initial municipality of residence by the Immigration Board, was close to 90 percent. For our purposes, this is the most attractive time period, since there were few degrees of freedom for the individual immigrant to choose the initial place of residence. From 1992, the placement system gradually eroded due to a large inflow of asylum seekers from former Yugoslavia.

Several studies have used the settlement policy as an exogenous source of variation that identifies the causal effect of neighborhood characteristics (Edin et al. 2003, Åslund & Fredriksson, 2005, Åslund & Rooth, 2006). The basic arguments for the exogeneity of the initial location with respect to unobserved individual characteristics are the following: (*i*) the placement rate was high (in particular during 1987–91), (*ii*) the housing market was booming (making it difficult to find vacant housing in attractive areas), and (*iii*) there was no interaction between local officers and the refugee in question.

The handling of a typical asylum seeker from the border to the final placement was as follows. After applying for asylum, the individual was placed in a refugee center pending a decision from the immigration authorities. There was no correlation between the port of entry and which center the person was put in. However, immigrants were sorted by native language when placed in centers. After receiving asylum and a permanent residence permit, the refugee was placed in a municipality.[9] When the refugee left the center, it was already decided in which apartment he or she would live. Thus, there was no direct interaction with the local authorities before the individual was assigned to a specific apartment. This is particularly important for this

[7] This section builds upon The Committee on Immigration Policy (1996) and The Immigration Board (1997). We also draw on Edin et al. (2003) who present a more thorough discussion on the placement policy, partly based on interviews with government officials involved in different parts of the system at the time of implementation.

[8] Edin et al. (2004) evaluate the consequences for the refugees of the policy shift occurring in 1985. The policy shift had two components: (i) dispersal of refugees across the country; and (ii) increased reliance on income support. They show that the overall effect of the policy shift was negative for the refugees subjected to the policy and that the increased focus on income support contributed mostly to this negative effect.

[9] There was no formal restriction against relocating. The cost of doing so was basically that the refugee lost access to some introductory activities supplied by the assigned municipality, and had to wait for a slot in a language class in the new location. Åslund (2005) studies secondary migration among refugees subjected to the dispersal policy, and finds that 38 percent of the refugees had left the initial municipality within four years. However, this mobility rate was nearly as high before the implementation of the dispersal policy.

study, since we use the exact coordinates of the initial place of residence to calculate individual-based measures of job access (see section 4).

The refugees could state preferences for different locations. Most immigrants then applied for residence in the major immigrant cities of Stockholm, Gothenburg and Malmö. However, it was very hard to find housing in these cities. Also, vacancies in different locations opened up at different times. Therefore, most individuals could not realize their preferred option when it was their turn to be placed.

The policy did not imply an unconditional randomization across locations. Placement was influenced by observed characteristics of the individual. First, there were practical reasons for this. Some local administrations had better resources for dealing with people coming from a particular country or speaking a certain language. Certain areas contained housing that was more suitable for families, whereas others were richer in small apartments for singles. Also, when the number of applicants exceeded the number of available slots, municipal officers may have selected the "best" immigrants (e.g. the highly educated). There was no interaction between municipal officers and refugees, so the selection was purely in terms of observed characteristics. Given the richness of the data, it seems plausible that the municipal officers did not base their actions on factors unobserved to us. We therefore believe that is justified to think of initial placement as random, *conditional* on observed characteristics. We discuss this issue further in the next section.

4 Data and empirical strategy

4.1 The data

We wish to measure the impact of individual job access on individual labor market outcomes. To this end we extract two samples of Swedish residents: (i) refugees arriving in 1990-91 (for whom we can acquire causal estimates since they were subjected to the governmental dispersal policy); (ii) a random sample of the entire Swedish population (for which we can retrieve results that can be related to previous findings showing the apparent impact of job access). For both samples we combine register data on earnings, employment and individual characteristics with information on job access in the area surrounding each person's place of residence. Details follow below.

All data used come from the Uppsala University geographical database PLACE (compiled by Statistics Sweden). PLACE is based on register data and contains a complete record of individual residents in Sweden between 1990 and 2002. A strong emphasis in this database is on variables describing individuals' financial situation, education, work status, family status and the geography of home and work. Since the variables available throughout the years differ, the study cannot make use of data after 1999. The analysis is therefore primarily based on observations made in this year.

As mentioned above, the first sample consists of immigrants arriving in Sweden in the years 1990 and 1991. To capture refugees of working age we keep only individuals who (*i*) were born in one of the countries listed in Table A1; (*ii*) did not have a spouse living in Sweden prior to their arrival; (*iii*) were in an employable age (18–64 years[10]) for a period stretching from the year of arrival until the end of 1999. Given these restrictions, our refugee sample comprises 21,745 individuals. We also use a random sample of Swedish residents in employable age 1999, initially containing 500,000 thousand individuals. After applying the age restrictions used in the refugee population, the second population contained 424,462 individuals.

We compute job access variables with the help of geographical coordinates listing all individuals' place of residence, and the working population's workplace coordinates. Our baseline econometric model (see the next subsection) uses the following variables to measure job access:

(i) the log of the number of jobs within a 5 km radius from the individual's place of living.

(ii) the log of the number of people living within a 5 km radius from the individual's place of living.

We are primarily interested in the effects of the number of jobs, which is probably closest to a "pure" job access variable. It seems reasonable to study effects of the number of jobs surrounding the individual, conditional on the size of the population in the same area. The population variable captures both competing labor supply and a potential effect of urban density.[11] The 5 km radius is of course arbitrary, but it is close to the median commute both among refugees and in the overall population sample (*Table 1*).[12]

The calculation of these variables is built on so-called floating catchment areas . Technically, this means that coordinates are first aggregated at the square kilometer level. Then, a geometrical shape, in this case a circle with a radius of 5 km, is placed over a grid containing the number of jobs or residents per square kilometer (counting the entire Swedish population). All values encompassed by the circle are summed and saved with the coordinates of the central-most square. The circle is thereafter moved to the neighboring square, repeating the procedure until the catchment area of every square has been calculated. Since the 5 km radius encompasses the sum of jobs or people within 73 square kilometers, a rugged circle makes up the measured delineation. The procedure itself is performed using a GIS-program. [13]

Furthermore, the geographical coordinates map each individual to a neighborhood—SAMS area. There are about 9,200 SAMS areas in Sweden (with an average population of less than

[10] The official Swedish age of retirement is 65.

[11] Including the ratio of number of jobs divided to the number of residents rather than the two variables separately is an alternative. Note, however, that we get the same estimate for the ratio entered in logarithmic form as for the log number of jobs, as long as the population variable is included (which it should be given that it may also capture e.g. effects of urban density).

[12] Section 5 discusses sensitivity checks using job access within other distances from the individual than 5 kilometers.

[13] The coordinates listed in PLACE express positions in the Swedish reference system, RT90. The RT90 grid is based on the right angle distance from the equator and is fixed at the location that insures the longest path through Sweden. To ensure that all values in the grid are positive, the meridian is pushed westwards with its origin located at 2.5 gon west of Stockholm's old observatory. The RT90 coordinates used in the dataset are aggregated at the square kilometer level. Since the grid only possesses positive values within Sweden and through its right angle alignment, the calculation of Cartesian distances and floating catchment areas are feasible.

1,000). For each SAMS, we compute characteristics such as commuting rates[14], fraction highly educated, and the fraction foreign-born (see the appendix for definition of the variables). In other words, we use different techniques to calculate the primary job access variables and the supplementary neighborhood characteristics. We think it is reasonable to assume that people consider jobs based on physical distance, but that other contextual effects are determined by people living in one's neighborhood.

In the presentation of the results we discuss alternative specifications with varying sets of job access variables, e.g. including the squares of the number of jobs and the size of the population. We also present results with measures of job growth and a richer parameterization of neighborhood (SAMS) characteristics. Further details are given in the presentation of the results in section 5.

4.2 Description of the samples

Table 1 presents some descriptive statistics on the refugee population and the overall sample of the Swedish population. Clearly, earnings and employment are much lower among the refugees. It is striking that only 43 percent of the refugees are classified as employed in 1999 using the "official" employment definition (which is based on employment in the month of November). The corresponding figure in the random sample is 78 percent. Turning to the job access measures, we see that refugees live in more populated and job-dense areas. The average (and median) individual lives in a neighborhood where about half the workers commute more than five kilometers from their home to the workplace. Five kilometers is also close to the median individual commute in both the samples. Note that mean commutes are substantially higher; outliers with very long distances between home and work are the source of this difference.

The refugees are on average younger than people in the random sample (note that both samples are restricted to those 26–64 years of age in 1999). In terms of education, the refugees have a higher percentage with little education, but also a somewhat larger fraction with higher university degrees.

[14] We use the coordinates to calculate Cartesian distances between an individual's residence and workplace (i.e. the length of the commute), see the appendix for a description.

Table 1 Descriptive statistics.

Variable	1990–91 Refugees Mean (sd)	Median	Random population sample Mean (sd)	Median
Ann. Earn. (1,000 SEK) (cond. on y>0)	125.6 (121.4)	114.1	211.9 (138.0)	201.9
Fraction earnings>0	.58		.85	
Employment	.43		.78	
ln # jobs within 5 km	10.39 (1.37)	10.55	8.92 (2.32)	9.22
ln # people within 5 km	10.44 (1.19)	10.63	9.17 (1.90)	9.32
Commuting rate in SAMS (>5 km)	.49 (.19)	.48	.53 (.22)	.53
Female	.43		.49	
Age	38.83 (8.2)	37	44.96 (10.6)	45
Education				
Missing	.06			
<9 years	.16		.12	
9-10 yrs	.18		.13	
Secondary	.31		.47	
Tertiary <2 yrs	.04		.06	
Tertiary >=2 yrs	.23		.21	
Graduate	.02		.01	
Civil status				
Married male	.29		.25	
Married female	.23		.26	
Cohabiting male	.03		.05	
Cohabiting female	.02		.05	
Single	.43		.39	
Commuting distance	17.4 (57.4)	4.7	19.4 (61.8)	5.4
# observations	21,745		424,462	

Notes: All variables measured in 1999. Earnings is conditional on earnings>0. The variables are defined in the appendix

4.3 Empirical strategy

Our empirical analysis is based on estimating models of the following form:

$$Y_i = \alpha + \beta X_i + \gamma \, job_{it} + \delta D_j + \varepsilon_{it} \tag{1}$$

where Y_i is the outcome of individual i in year 1999. The outcome variables used are: (*i*) employment, and (*ii*) log annual earnings. X_i is a set of standard characteristics for individual i (age, age squared, gender, family status, level of education, and country of origin). job_{it} contains the job access variables (measured at time t (1999 or year of immigration, see below)) and D_j is a set of municipal dummy variables. We estimate these models both for the random population sample and for the 1990–91 refugee cohorts. Note that the specifications include municipal fixed effects, meaning that we utilize only variation in job access within Sweden's (then) 289 municipalities. Considering also the fact that the models include country of birth dummies, the specifications are quite demanding.

As mentioned in the introduction, there are several problems with estimating a causal relationship using the specification above. First, we may have omitted variable bias due to the endogenous location of workers. If workers with higher unobserved skills locate in job-dense areas, there will be a spurious positive relationship between job access and individual outcomes. Second, in the longer run it may be that jobs enter an area as a result of the presence of successful workers in the neighborhood. These problems more or less plague all previous studies of spatial mismatch. This is also true for our analysis of the overall Swedish population, which should be seen as a regression description.

To get a better estimate of the effects of job access we study the 1990–91 refugee cohorts. As discussed in section 3.2, we exploit the fact that these individuals were not free to choose their initial place of residence in Sweden. This approach has also been used in previous studies (e.g. Edin et al. 2003, Åslund & Fredriksson, 2005, Åslund & Rooth, 2006). Conditional on observed characteristics, the initial location of the refugees can be regarded as exogenous. Our strategy is to use job access variables measured in the year of immigration, which alleviates both omitted variable bias and the problem of reversed causality. We present both reduced form specifications (where 1999 outcomes are regressed on immigration year job access) and IV specifications (where 1999 job access is instrumented by immigration year job access). Both types of models build on the conditional exogeneity of the initial location. As discussed in section 5.2 below, the IV approach also requires an exclusion restriction that may be questioned. This is why we to some extent focus on the reduced form results.[15]

Can we believe in the conditional exogeneity assumption? A basic argument in favor of the assumption is the major change in the distribution of the refugees brought by the dispersal policy. After the introduction of the municipal placement, substantially larger fractions of the refugees started out in Northern Sweden, and fewer people came directly to the Stockholm region (Edin et al., 2003). Still, as described in section 3.2, the placement of refugees was not a totally random process. People of a certain national origin were more likely to end up in some locations than others. Municipal officers also considered e.g. the level of education of the refugees. *Table 2* presents results from regressions of the number of jobs within 5 km from the individual on individual characteristics. The first column contains results for the full random population sample in 1999. The second column restricts the estimations to immigrants in the random sample. Columns three and four present estimates for the 1990–91 refugees, in the year of immigration and in 1999 respectively.[16]

The coefficients in the first column reveal that people less than 30 years of age live in more job-dense areas. Singles on average have more jobs near their homes, and the same is true for

[15] Due to the use of IV (in the refugee analysis) and the large number of dummies included, we use linear probability models for the employment outcome. The baseline (reduced form) results are very similar with a probit model.

[16] Note that the models include country of birth and municipality dummies to be in correspondence with the analysis in section 5. We thus use variation in job access within regions. If we exclude the municipal dummies, the estimates generally increase in magnitude. In other words, it seems that the labor market sorting between and within regions goes in the same direction.

immigrants (compared to the Swedish-born). This is most likely a reflection of these groups tendency to live in dense urban areas. Further analysis shows the difference between immigrants and natives is mostly due to sorting *across* regions. We develop this issue further in section 5.4, where we ask whether differences in job access can explain the ethnic employment gap in Sweden.

Note in the second column that the sorting pattern differs somewhat between the overall and the immigrant population. The positive correlation between job proximity and education is not as strong, and the sign of the "female" coefficient differs across the two columns.

Obviously, refugee placement was not random with respect to observed individual characteristics (column three). However, the case we are making is that the placement was not systematically related to any factor unobserved to us (e.g. "ability"). An argument in favor of the conditional exogeneity assumption is the difference between columns three and four in *Table 2*. The initial location was not related to age, and the coefficients on gender and marital status were different from the ones in the random sample of immigrants.[17] Over time, the sorting pattern changed and became more similar to that in the random sample of immigrants. This can be taken to suggest that individuals were not sorted into their preferred location right after immigration.

It is very hard to get a strict test of the conditional exogeneity assumption. What we need is a skill-related variable that was not observed (or considered) by those who handled the placement. Most easily observed skill-related variables (e.g. education) potentially affected also placement through the actions of the authorities. Åslund & Fredriksson (2005) use a different database to study welfare dependence with essentially the same group of refugees. Their data include month of birth, which is sometimes claimed to be related to skills (see e.g. Bound et al., 2000), but was arguably not a criterion determining placement. If month of birth is related to skill and there was sorting on unobserved skills, one would then expect a correlation between placement and month of birth. The authors find no evidence in favor of this hypothesis, which strengthens the argument for the conditional exogeneity of the initial location.[18]

[17] One should be cautious in interpreting the estimates for education in the year of immigration. The education variable is often missing and its quality can be questioned.

[18] In section 5.3 we present some sensitivity checks suggesting that violations of the conditional exogeneity assumption are not likely to explain our empirical findings.

Table 2 Regressions of job proximity on individual characteristics.

	Full random sample (1999)	Immigrants in random sample (1999)	Refugees (year of immigration)	Refugees (1999)
Age (<30 ref.)				
30–39	–.174**	–.158**	.011	–.031
	(.010)	(.023)	(.036)	(.016)
40–49	–.239**	–.241**	.003	–.056**
	(.011)	(.025)	(.038)	(.018)
50–59	–.193**	–.282**	–.011	–.057*
	(.011)	(.026)	(.042)	(.022)
60<	–.144**	–.300**	.026	–.079*
	(.014)	(.030)	(.053)	(.034)
Female	.042**	–.024*	.036*	–.039**
	(.005)	(.011)	(.016)	(.010)
Married	–.341**	–.195**	.054**	–.091**
	(.012)	(.015)	(.017)	(.010)
<9 years	Ref.	Ref.	.007	–.029
			(.035)	(.023)
9-10 yrs	.038**	–.054**	.041	–.026
	(.011)	(.018)	(.034)	(.022)
Secondary	.111**	–.066**	.069*	–.034
	(.010)	(.018)	(.032)	(.022)
Tertiary <2 yrs	.345**	.068*	.172**	.046
	(.016)	(.031)	(.055)	(.032)
Tertiary >=2 yrs	.352**	.053*	.148**	–.002
	(.015)	(.025)	(.038)	(.023)
Graduate	.511**	.100	.074	.076
	(.035)	(.069)	(.156)	(.042)
Immigrant	.215**			
	(.017)			
Country of birth dummies	Yes	Yes	Yes	Yes
Mun. dummies	Yes	Yes	Yes	Yes
Observations	424,462	45,366	21,745	21,745
R-squared	.58	.66	.69	.74

Notes: The table presents estimates (standard errors) from linear regressions of (the log of) the number of jobs within 5 km from the individual on individual variables. “1999” and “year of immigration” denotes when job access and the covariates are measured.

5 Empirical results

The purpose of this paper is to investigate the importance of job proximity as a determinant of individual labor market outcomes. The aim is to get causal estimates, but we begin by showing how job access is correlated with individual labor market outcomes in the overall population, using the random sample of the overall Swedish population (section 5.1). This section provides a link to previous research, addressing the following question: does a (potentially erroneous) standard analysis using Swedish data give results similar to those retrieved in other countries? We then turn to the study of the 1990–91 refugee migrants who were subjected to the governmental placement policy (sections 5.2 and 5.3). In this last section, we use the exogeneity of the initial location to get causal estimates of the importance of job access. We conclude the section with a brief discussion on whether differing job access can explain the immigrant-native differential in labor market performance.

5.1 The apparent importance of job access

Table 3 shows results from specifications relating employment and annual earnings (excluding those without earnings) to job access. Columns 1 and 4 present the baseline estimates. Employment is positively related to job access, but limited in the quantitative sense. According to the estimates, doubling the number of jobs within 5 kilometers from the individual is associated with 0.3 percentage points higher employment; the earnings estimate is insignificant.[19] The population variable is negative in the employment models. This is expected: given the number of jobs, more people mean higher competition. The positive estimates given in the earnings specifications probably reflect the fact that inner cities in Sweden host many high-wage people.

[19] The average “within municipality” standard deviation in the (log of the) number of jobs is 1.43. Sensitivity checks including the squares of the number of jobs and the size of the population, suggest that the relationship between earnings and job proximity is positive at low job access levels but decreasing with higher values of job access.

Table 3 Job access, employment and annual earnings, population sample.

	Employment			Log earnings (given y>0)		
	(1)	(2)	(3)	(4)	(5)	(6)
ln # jobs (5 km)	.003**	.003**	.003*	.006	.006	.014**
	(.001)	(.001)	(.001)	(.004)	(.003)	(.004)
ln # people (5 km)	–.004*	–.004*	.004*	.021**	.021**	.029**
	(.002)	(.002)	(.002)	(.005)	(.005)	(.005)
Age	.058**	.058**	.058**	.121**	.121**	.121**
	(.001)	(.001)	(.001)	(.002)	(.002)	(.002)
Age squared	–.072**	–.072**	–.072**	–.136**	–.136**	–.136**
	(.001)	(.001)	(.001)	(.002)	(.002)	(.002)
Female	–.012**	–.012**	–.013**	–.196**	–.196**	–.197**
	(.002)	(.002)	(.002)	(.006)	(.006)	(.006)
9–10 yrs	.006*	.006*	.005	–.010	–.010	–.011
	(.003)	(.003)	(.003)	(.007)	(.007)	(.007)
Secondary	.094**	.094**	.089**	.127**	.127**	.121**
	(.002)	(.002)	(.002)	(.006)	(.006)	(.006)
Tertiary <2 yrs	.079**	.079**	.071**	.135**	.135**	.123**
	(.003)	(.003)	(.003)	(.009)	(.009)	(.009)
Tertiary >=2 yrs	.181**	.181**	.172**	.440**	.440**	.427**
	(.003)	(.003)	(.003)	(.007)	(.007)	(.007)
Graduate	.209**	.209**	.199**	.728**	.728**	.711**
	(.005)	(.005)	(.005)	(.015)	(.015)	(.015)
Job growth (98–99)		.011			.003	
		(.007)			(.018)	
Commute rate			.010			.084**
			(.006)			(.014)
Fr. highly educated			–.049**			.014
			(.008)			(.020)
Fr. foreign–born			–.355**			–.473**
			(.009)			(.023)
Civil status	yes	yes	yes	yes	yes	yes
Municipality	yes	yes	yes	yes	yes	yes
Country of birth	yes	yes	yes	yes	yes	yes
Observations	424,462	424,462	424,461	362,514	362,514	362,514
R–squared	.14	.14	.15	.13	.13	.13

Notes: Estimates (robust standard errors in parentheses) from regressions of individual employment and annual earnings (in 1999) on job access and individual variables. * (**) denotes significance at the 5-(1-)percent level. The variables are explained in the appendix.

It is quite likely that the effects of job proximity vary across groups. *Table A2* shows results for different subgroups of the population sample. The estimate for the job proximity measure in the employment specification is significant for women, but small and insignificant for men. In the earnings model, the estimate is larger for men. Local access to jobs exhibits stronger correlation with both earnings and employment among the low-educated than among people with at least some tertiary education. The outcomes of immigrants are not significantly connected to the job access variable. We will return to this observation in the refugee analysis. When the sample is split up according to region of residence, it turns out that the jobs and residents in the nearby area are closer linked to employment in large cities, whereas the opposite is true for earnings.

Apart from the problem of endogenous location (which is addressed in the next subsection), the introduction mentioned two problems frequently encountered in spatial mismatch studies: (*i*) the failure to control for neighborhood characteristics and (*ii*) the difficulty of measuring job vacancies as opposed to the stock of jobs. *Table 3* presents specifications addressing these problems. In columns (2) and (5), the rate of job growth has been added to the baseline specifications. Job growth is measured as the change in the log of the number of jobs around the individual between 1998 and 1999. Including both the stock of jobs and job growth proxies the number of vacancies. Job growth appears to be related to employment but not to earnings. The estimate for employment suggests that a difference of 10 percentage points in the local job growth rate (close to a standard deviation), only means a 0.11 percentage points difference in the probability of employment. The marginal impact of including job growth signals that—in this context—the stock of jobs measures job access in an acceptable way.

Columns (3) and (6) show employment and earnings models where three additional neighborhood (SAMS) variables are included: the commute rate (i.e. the fraction of resident workers whose workplace is more than 5 km away from home), the fraction of highly educated residents and the fraction foreign-born.[20] The employment estimates for the job density variable remains unchanged, but the population variable switches sign compared to the baseline model. The commute rate enters positively and marginally significant in the employment model, but highly significant in the earnings model. In the latter specification, the estimate for the job proximity variable is positive and significant, thus suggesting a negative correlation between job proximity and the commute rate. The coefficient for the fraction highly educated is negative in the employment model. One interpretation is that this variable captures the characteristics of the competing labor: given my own level of education, having many high-skilled people around means more competition.[21] The average level of education in the neighborhood is not correlated with individual earnings (conditional on the other covariates). Living in areas with high immigrant representation is negatively related to earnings and employment. A standard deviation (within municipalities) in immigrant density amounts to eight percentage points. Such

[20] This type of parameterization is the best we can do in controlling for neighborhood effects. Including very low-level fixed effects, e.g., would eliminate virtually all variation in the job access variable.
[21] Of course, it may also capture e.g. areas with many students.

a variation is associated with 4.7 percent lower earnings and a 3.5 percentage points reduction in employment.

We have now established a positive but limited correlation between job access and individual employment and (to some degree) earnings. The relationship is stronger in some groups usually believed to be more affected by spatial mismatch, such as the low-educated. Furthermore, the estimated relationship between labor market outcomes and the number of jobs surrounding the individual is not sensitive to the inclusion of additional neighborhood variables or measures of job growth.

The patterns found in this section are important for generalizing the results presented in the next section concerning the question of real interest: the causal effects of job access.

5.2 Causal effects of job access

This section presents estimates of the importance of job access for the 1990–91 refugee sample only. As discussed above, studying this group enables us to obtain estimates of the causal effects of interest. We follow the same approach as above and relate earnings and employment to the number of jobs and the size of the population within 5 km around the individual.

Table 4 below shows three specifications for earnings and employment respectively. The "OLS" model is the same as in the analysis above, i.e. outcomes in 1999 are regressed on job access in 1999. The "OLS" estimates suffer from the same sorting problems as most analyses of spatial mismatch. These problems of self-selection are eliminated in the "Reduced form" specifications. They relate 1999 outcomes to job access (i.e. both the job and the population variable) in the year of immigration (1990 or 1991). The reduced form estimates arguably capture at least the direction of the impact of contemporary job access. They also answer an interesting policy question: what is the long-run effect of exposing an individual to a certain type of environment?

To get a quantitative estimate of the impact of current job access, we estimate 2SLS "IV" models where 1999 job access is instrumented by immigration year job access in the first stage, and in the second stage outcomes in 1999 are regressed on the first stage predictions.[22] Note, however, that in addition to the conditional exogeneity assumption, the IV models require the exclusion restriction that the *only* link between immigration year job access and employment in 1999 is through local job access in 1999. We will return to this issue below.

The OLS models do not suggest any significant correlation between job access and labor market outcomes. However, the pattern changes when we control for residential sorting in the "Reduced form" specifications. They show that employment is clearly affected by job access. Doubling the number of jobs in the initial location is associated with 2.9 percentage points higher employment probability in 1999. In other words, having been placed in a location badly connected to jobs in 1990–91 leaves traces on employment for at least 8 years.[23] This means that

[22] In the employment model, the first stage estimate (s.e.) for ln # jobs 5 km is .154 (.026).

[23] In the context of refugee integration in the Swedish labor market, 8 years is not such a long time considering the low employment rate among the refugees in 1999 (less than 50 percent).

job access has a lasting effect on employment outcomes for refugees. This impact could work via a number of mechanisms, two of which are state dependence ("scarring", i.e. past outcomes affects current outcomes)[24] and an increased probability of living in a location with poor job access also in 1999. Åslund & Rooth (2006) analyze long-term effects of facing high local unemployment rates after immigration, and find support for both these mechanisms.

As discussed above, the IV procedure rests on the assumption that the only link between immigration year job access and employment in 1999 is through local job access in 1999. If proximity to jobs in the year of immigration affected early employment, which in turn had an impact on later outcomes, the IV estimates are upward biased. If, however, we are willing to assume no scarring in this particular context, the IV specifications can be used to identify the effect of contemporary job access.[25] At face value, the IV employment estimate suggests a huge effect of job proximity. Living in an area with twice the number of jobs (ceteris paribus) increases the individual employment probability by 25 percentage points. There are, however, reasons to be skeptical about such a large effect given the assumptions regarding the exclusion restriction.

[24] There are of course several possible causes for state dependence: skill loss during unemployment, signalling to employers, and poor peer connections as in the framework of Calvó-Armengol & Jackson (2004). Hansen & Löfstrom (2001) suggests that state dependence in employment is a factor of importance for immigrants to Sweden. Swedish studies also indicate the importance of contacts and informal methods for finding a job, especially for low-qualified workers and ethnic minorities (see e.g. Olli Segendorf, 2005). Duration dependence is also a well-known feature of the US labor market. See e.g. Flinn & Heckman (1982) or Lynch (1989).

[25] For IV to capture average treatment effects, additional assumptions are of course required.

Table 4 The effects of job access on refugee earnings and employment

	Employment			log annual earnings		
	OLS	Reduced form	IV	OLS	Reduced form	IV
ln # jobs (5 km)	.019	.029**	.255**	.009	.028	.244
	(.014)	(.010)	(.095)	(.047)	(.035)	(.293)
ln # people (5 km)	–.043*	–.049**	–.480**	–.029	–.070	–.642
	(.020)	(.015)	(.165)	(.069)	(.051)	(.457)
Age	.022**	.023**	.020**	.031*	.036*	.030
	(.003)	(.003)	(.003)	(.015)	(.015)	(.016)
Age squared	–.034**	–.035**	–.032**	–.038*	–.044*	–.039*
	(.003)	(.003)	(.004)	(.018)	(.019)	(.019)
Female	–.022	–.022	–.017	–.031	–.031	.011
	(.013)	(.013)	(.015)	(.053)	(.053)	(.058)
Education <9 years	.077**	.079**	.071**	–.063	–.049	–.076
	(.013)	(.013)	(.015)	(.079)	(.081)	(.086)
9–10 yrs	.115**	.117**	.114**	–.070	–.069	–.072
	(.013)	(.014)	(.015)	(.077)	(.080)	(.083)
Secondary	.184**	.187**	.190**	.053	.051	.065
	(.013)	(.013)	(.015)	(.076)	(.078)	(.081)
Tertiary <2 yrs	.164**	.166**	.167**	–.177	–.172	–.139
	(.020)	(.020)	(.022)	(.092)	(.094)	(.101)
Tertiary >=2 yrs	.253**	.255**	.258**	.231**	.227**	.254**
	(.014)	(.014)	(.015)	(.077)	(.079)	(.083)
Graduate	.308**	.318**	.322**	.699**	.711**	.753**
	(.025)	(.026)	(.028)	(.103)	(.105)	(.111)
Civil status	yes	yes	Yes	yes	yes	yes
Municipality dummies	yes	yes	Yes	yes	yes	yes
Country of birth	yes	yes	Yes	yes	yes	yes
Observations	12,655	12,655	12,655	21,745	21,745	21,745
R–squared	.15	.13	.02	.10	.09	.03

Notes: Estimates (robust standard errors in parentheses) from regressions of individual employment and annual earnings (in 1999) on job access and individual variables. The number of jobs and residents is measured in 1999 (the year of immigration) in the OLS (Reduced form) models. In the IV models, 1999 values are instrumented by immigration year values. * (**) denotes significance at the 5-(1-)percent level.

It is worth noting that just like the OLS employment estimate in *Table 4*, the employment estimate for immigrants in the population sample in *Table A2* is statistically insignificant. The difference between the reduced form (or IV) estimates and the OLS estimates seems to imply that immigrants with poor unobserved characteristics move into job-dense areas in Sweden, which blurs the impact of job access on employment.[26]

Another interesting result in the table concerns the impact on employment of the number of people living within a 5-km radius from the individual's residence. The estimates are always negative and significant for any (employment) specification considered. The similarity across

[26] A similar sorting pattern is found in Åslund & Fredriksson (2005).

the specifications suggests that self-sorting based on the size of the local population density is less of an issue than job-related sorting. In terms of interpretation, a negative sign indicates that a large pool of competing labor supply seems to hamper refugees in the labor market. Of course, keeping the number of jobs constant but increasing the number of people means a decrease in local job access.

The annual earnings equations show that job access has no significant impact on earnings. This is quite standard in the spatial mismatch literature (Ihlanfeldt & Sjoquist, 1998) because the wage setting is complex and captures different aspects; for example, wages can compensate for distance to jobs and/or housing quality (see e.g. Zax, 1991, Gabriel & Rosenthal, 1996, Manning, 2003). This should be particularly true in the case of Sweden since the employment rate among the studied refugees was as low as 43 percent in 1999. It is indeed plausible that local labor market properties would then be a determinant of who finds a job rather than who obtains a good salary.

The main lesson that can be drawn from *Table 4* is that there is an impact of job access on employment, and that we understate this effect unless we control for endogeneity of location. The OLS estimates are insignificant while the "Reduced form" and the IV estimates show a significant impact of job access. This is a crucial result, which shows the importance of handling endogeneity issues in this type of studies. Thus, for refugees, distance to jobs does matter for getting a job, and this result is *not* due to any unobserved heterogeneity.

Can we generalize these results to other contexts? In the refugee data, a simple regression understates the importance of job access as a determinant of labor market outcomes. If we are willing to apply the sign of this bias to (e.g.) the findings of section 5.1, they would indeed suggest that job access affects outcomes. We can of course not be sure that the sorting patterns are similar across groups (and contexts), but the fact that exposure to jobs many years ago is so clearly related to employment among the refugees arguably favors the hypothesis that access to jobs is generally a determinant of individual employment.

5.3 Extensions and robustness checks

We will now discuss some extensions and robustness checks using the refugee sample. We focus on the reduced form specification, since this is the most robust model in terms of reliability.

In the introduction we mentioned two other econometric problems that often confound empirical analysis of the impact of job access on labor market outcomes: measurement errors in the job access variable and omitted neighborhood characteristics. The first two columns in *Table 5* below present results where the jobs within five kilometers from the individual have been split according to the level of education of the workers holding them. Given that immigrants to Sweden frequently experience difficulties in finding jobs matching their level of education, it is not surprising to find that it is only proximity to low-skilled jobs that has a positive impact on employment.

A second type of variation is to include the additional neighborhood characteristics discussed in section 5.1 (now for the initial location). As shown in columns three and four, this has basically no impact on the estimates for the number of jobs within 5 km. Furthermore, most of the estimates for the additional neighborhood characteristics are insignificant. The other variation made in *Table 3*—including job growth 1998–99—is not appropriate in these models where we look at local conditions in the year of immigration.[27] Estimating OLS specifications using 1999 job access including job growth, however, yields insignificant estimates for the job growth variable (not in the table but available upon request).

Table 5 Robustness checks: jobs by skill, additional neighborhood characteristics. Reduced form estimates.

	Jobs by skill level		Neighborhood chars.	
	Empl.	Log earnings	Empl.	Log earnings
ln # jobs (5 km)			.028*	.036
			(.012)	(.041)
ln # no tert.edu. jobs (5 km)	.050*	.013		
	(.020)	(.071)		
ln # tert.edu. jobs (5 km)	–.019	.013		
	(.017)	(.059)		
ln # people (5 km)	–.047**	–.070	–.047**	–.075
	(.015)	(.052)	(.015)	(.052)
Commute rate			.000	.107
			(.035)	(.129)
Fr. Highly educated			–.000	.005*
			(.001)	(.002)
Fr. Foreign–born			–.000	.001
			(.000)	(.001)
Civil status dummies	yes	yes	yes	yes
Municipality dummies	yes	yes	yes	yes
Country of birth dummies	yes	yes	yes	yes
Observations	21,745	12,655	21,745	12,655
R–squared	.13	.09	.13	.09

Notes: Reduced form estimates (robust standard errors in parentheses) from regressions of individual employment and annual earnings (in 1999) on job access in the year of immigration and individual variables. * (**) denotes significance at the 5-(1-)percent level. “(no) tert. edu, jobs” means that the holder has some (no) tertiary education.

We now move on to other robustness checks. As discussed in section 3, not all refugees were in fact assigned to their first location; about 10 percent found housing on their own. To investigate the possibility that these individuals are driving the results, we tried dropping observations according to different criteria. First, we excluded everybody who lived in a metropolitan area in the year of immigration, assuming that the remaining group hardly chose for themselves. The point estimates changed very little. Under the assumption that it is those with high ability that opt out of the placement scheme and sort into their optimal location, we

[27] In an IV context, one could argue that we could use job growth 1998–99 in the assigned location as an instrument for job growth in the observed 1999 location. This would require not only the assumption on the exclusion restriction discussed in the text, but also that the instrument (measured after immigration) was not somehow affected by the refugee inflow.

then (respectively) tried dropping: (i) everybody who had any earnings in their year of immigration; (ii) the top ten percent 1999 earners; (iii) the self-employed (in 1999). All variations confirmed the baseline results.

We also split the sample and ran the regressions by groups; see *Table A3*. The estimates were relatively stable across groups—in no dimension are the estimated coefficients significantly different. At face value, however, the effects of job proximity are stronger among males than among females. The point estimate is also larger for the highly educated. This is perhaps not surprising given the poor labor market position of the studied refugees. It may be that it is only the normally stronger groups that are affected by general local labor market conditions. Ihlanfeldt (2006) points out that a shortcoming of the spatial mismatch literature is its strong focus on large metropolitan areas. It is therefore interesting to note that we get similar point estimates for metropolitan and non-metropolitan areas.

While observed median commuting distances give some a priori reasons for the 5 km radius, we have experimented with the distance within which we measure the number of jobs and the resident population. Since the computation is very computer-intensive, we restricted the variations to 2 and 10 km respectively. The 10 km radius yields results that are similar to the ones presented above. With the 2 km radius, the estimates are insignificant. Probably, the 2 km radius is too short to capture the relevant job search area for most individuals.[28] We also tested the functional form of the job access variable by adding the square of the log of the number of jobs (and residents) surrounding the individual. The coefficients of the quadratic terms were statistically insignificant, and the linear coefficients were largely unaltered.

5.4 Can differences in job access explain employment differences in Sweden?

The explanation to majority-minority differences in the labor market offered by SMH builds on two assumptions: (i) job access matters for individual outcomes; (ii) minorities have lower job access. The results above clearly suggest that job access matters for employment in Sweden. The question is then whether it differs across ethnic groups. To investigate this issue we regressed the log of the number of jobs on the log of the number of residents and a set of dummies for region of birth, using the random population sample. Conditioning on the size of the population corresponds with the employment specifications presented above.

According to estimates presented in *Table 6*, immigrants have fewer jobs in their surroundings (conditional on the number of people living there). For those originating outside the Western world, the difference is about 7 percent compared to natives. Column (2) shows that the pattern is quite similar within metropolitan areas as in the country as a whole. Furthermore, column (3) shows that part of the differences remains also when we condition on municipality of residence.

[28] Note two things regarding the alternative radii. The approximated “job search circle” is poorer the smaller the radius. For 2 km it looks more like a rhombus. The larger the radius, the more the circle enters other municipalities, which questions the plausibility of regional fixed effects in the models.

Table 6 Job access by group: regression estimates using the random population sample.

	(1) All	(2) Metropolitan	(3) All, municipal dummies
Foreign–born “western”	–.057**	–.040**	–.012**
	(.004)	(.004)	(.003)
Foreign–born “other countries”	–.073**	–.077**	–.033**
	(.004)	(.004)	(.004)
ln # people (5 km)	1.192**	1.287**	1.278**
	(.000)	(.001)	(.001)
Observations	424,462	156,617	424,462
R–squared	.94	.95	.96

Notes: Regressions of “ln # jobs (5 km)” on dummies for region of birth (natives reference) and the “ln people (5 km)”, using the random population sample.

The results thus suggest that immigrants have somewhat lower job access than natives (with our admittedly limited way of measuring it). The question is then if these differences combined with our estimates can explain a substantial part of the immigrant-native employment gap in Sweden? The answer is no, which is hardly surprising given that the employment difference between natives and people born outside Europe amounts to 23 percentage points. Even if we would believe in the implausibly large IV estimates of *Table 4*, they would still require that natives have almost twice the job access of non-European immigrants to fully explain the employment difference.[29] In a more general sense, however, one could claim that spatial mismatch is a contributing factor to employment differences in Sweden: job access matters and it is lowest in the group with the poorest performance.

6 Concluding remarks

In this paper, we investigate the role of job proximity as a determinant of individual labor market outcomes for the case of Sweden. Using very detailed data on the exact location of all residences and workplaces in Sweden, we find that local job proximity is positively correlated with individual outcomes in the overall population. This pattern is in line with previous studies from other countries, but does not necessarily imply a causal effect of job access. Indeed, one of the most severe critiques that have been addressed to this literature is that residential location is not exogenous but a rational choice. As a result, the weight of the evidence in the United States that suggests that job access is partly responsible for the adverse labor-market outcomes experienced by ethnic minorities could be interpreted in a different way. It may well be that the more (unobserved) productive black workers choose locations close to jobs while the others

[29] The point estimate of .255 suggests that doubling the number of jobs (keeping the population constant) increases employment with about 25 percentage points, i.e. close to the difference in the employment rates.

reside further away. This has crucial implications in terms of policy since, if the latter is true, one should not blame job access but rather some intrinsic characteristics of workers.

We therefore exploit a Swedish refugee dispersal policy to overcome this central methodological problem. Using the exogenous variation in the location of individuals, we show a strong positive employment effect of job access. To be more precise, we find that refugees who in 1990-91 were placed in a location surrounded by few jobs, had employment disadvantages that remained in 1999. Doubling the number of jobs in the initial location in 1990-91 is associated with 2.9 percentage points higher employment probability in 1999.

Our results also suggest that residential sorting leads to underestimation of the importance of geographic distance to jobs. Even though Sweden and the United States have experienced different patterns of segregation (Hårsman & Quigley, 1995), we believe that our analysis can shed some light on the nearly exclusively American debate on whether job access affects labor market outcomes of ethnic minorities. First, the results suggest that Sweden is similar to other countries in the sense that ethnic minorities have lower spatial job access and that there is an apparent general connection between job access and individual outcomes. Second, and more importantly, our analysis confirms that job access is causally related to obtaining a job in a minority with poor average labor market status.

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Appendix

Variable definitions

Earnings	Annual earnings (including self-employment and employer's income)
Fraction earnings>0	= 1 if earnings>0, 0 otherwise
Employment	=1 if classified as employed in the official annual employment statistics (based on status during measurement week in November 1999).
ln # jobs within 5 km	Number of occupied jobs within 5 km from the individual's place of residence
ln # people within 5 km	Number of resident individuals within 5 km from the individual's place of residence
Commuting rate in SAMS (>5 km)	Share of working individuals resident in SAMS with commuting distance exceeding 5 km
Commuting distance	Cartesian distance between home and workplace, calculated using Pythagoras theorem: $d_{ij} = \sqrt{(x_i - x_j)^2 (y_i - y_j)^2}$, where d_{ij} is the straight-line distance between home and work.
Job growth	The change "ln # jobs within 5 km" between 1998 and 1999, based on the individuals 1999 location.
Fraction highly educated	Share of population in SAMS area with at least some tertiary education.
Fraction foreign-born	Share of population in SAMS area born outside of Sweden.
ln # tert edu jobs 5 km	Number of jobs within 5 km from the individual's place of residence occupied by people with tertiary education.
ln # no tert edu jobs 5 km	Number of jobs within 5 km from the individual's place of residence occupied by people without tertiary education.
Female	1 if female, 0 if male
Age	Age on Dec 31
Education	Highest completed education (dummies for six levels): <9 years, 9-10 yrs, Secondary, Tertiary <2 yrs, Tertiary >=2 yrs, Graduate, Missing
Civil status	Dummies for the following categories: married (wo-) man, cohabiting (wo-) man, (wo-) man in partnership, single (wo-) man with kids<(>=)18 years, singles, grown-ups living with their parents.
Country of birth	Dummies for each country / group of countries listed in Table A1.
Municipality	Dummies for residing in a particular municipality

Table A1 Countries of origin in the refugee sample.

Country of birth	Freq.	Percent	Cum.
Romania	687	3.16	3.16
Czechoslovakia	148	0.68	3.84
Hungary	261	1.20	5.04
Bulgaria	536	2.46	7.51
Estonia	100	0.46	7.97
Latvia, Lithuania	25	0.11	8.08
Fm Soviet republics	682	3.14	11.22
Russia	9	0.04	11.26
Ethiopia	1,345	6.19	17.44
Somalia	1,343	6.18	23.62
Gambia	156	0.72	24.34
Tunisia	230	1.06	25.39
Morocco	239	1.10	26.49
Uganda	114	0.52	27.02
Algeria	101	0.46	27.48
Egypt	62	0.29	27.77
Eritrea	383	1.76	29.53
Other Africa	566	2.60	32.13
Lebanon	1,874	8.62	40.75
Syria	1,333	6.13	46.88
Turkey	881	4.05	50.93
Iraq	2,231	10.26	61.19
Iran	2,998	13.79	74.98
Other Middle East	322	1.48	76.46
Cambodia, Vietnam	955	4.39	80.85
Thailand	579	2.66	83.51
China, Taiwan	349	1.60	85.12
The Philippines	354	1.63	86.75
Afghanistan	152	0.70	87.45
Bangladesh	195	0.90	88.34
India	135	0.62	88.96
Pakistan	74	0.34	89.30
Sri Lanka	241	1.11	90.41
Other Asia	193	0.89	91.30
Central America	468	2.15	93.45
Chile	624	2.87	96.32
Bolivia	32	0.15	96.47
Peru	242	1.11	97.58
Brazil	165	0.76	98.34
Argentina	72	0.33	98.67
Colombia	173	0.80	99.47
Other South America	116	0.53	100.00
Total	21,745	100.00	

Table A2 Job access by group—variations on Table 3

	Gender		Tertiary Education		Age		Foreign-born		Metropolitan areas	
	M	F	No	Yes	>=40	<40	No	Yes	No	Yes
					Employment					
ln # jobs (5 km)	.002	.005*	.004**	–.002	.003*	.002	.003*	.008	.003	.008**
	(.002)	(.002)	(.001)	(.002)	(.001)	(.002)	(.001)	(.006)	(.001)	(.003)
ln # people (5 km)	–.005*	–.003	–.005**	.005	–.002	–.009**	–.003	–.012	–.002	–.015**
	(.002)	(.003)	(.002)	(.003)	(.002)	(.003)	(.002)	(.008)	(.002)	(.004)
Observations	215,070	209,392	303,847	120,615	274,996	149,466	379,096	45,366	267,845	156,617
R–squared	.15	.14	.14	.11	.18	.11	.12	.16	.14	.15
					Log earnings					
ln # jobs (5 km)	.009	.004	.008*	.001	.012**	–.004	.007*	–.007	.012**	–.012
	(.004)	(.005)	(.004)	(.007)	(.004)	(.006)	(.003)	(.016)	(.004)	(.008)
ln # people (5 km)	.018**	.023**	.021**	.022*	.024**	.010	.020**	.018	.017**	.028*
	(.006)	(.007)	(.005)	(.010)	(.005)	(.008)	(.005)	(.022)	(.005)	(.011)
Observations	185,931	176,583	250,251	112,263	228,557	133,957	330,674	31,840	228,834	133,680
R–squared	.11	.10	.09	.14	.13	.14	.13	.11	.12	.13

Notes: Specifications also include individual variables and municipality fixed effects.

Table A3 The impact of job access by group: reduced form employment estimates for the 1990-91 refugees.

	Baseline	Gender		Age		Tertiary education		Metropolitan area	
		Male	Female	>=40	<40	No	Yes	No	Yes
ln # jobs (5 km)	.029**	.038**	.018	.035*	.024	.027*	.036	.032**	.026
	(.010)	(.015)	(.014)	(.015)	(.013)	(.012)	(.019)	(.012)	(.019)
ln # people (5 km)	–.049**	–.057**	–.040	–.061**	–.038	–.050**	–.051	–.050**	–.053
	(0.015)	(.022)	(.021)	(.023)	(.020)	(.019)	(.028)	(.018)	(.031)
Table 4 ind. vars.	Yes	yes	yes	yes	yes	yes	yes	yes	yes
Civil status	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mun. dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country of birth	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	21,745	12,325	9,420	8,726	13,019	15,378	6,367	14,036	7,709
R–squared	.13	.12	.19	.19	.12	.14	.11	.14	.14

Notes: Reduced form estimates (robust standard errors in parentheses) from regressions of individual employment (in 1999) on job access in the year of immigration and individual variables. * (**) denotes significance at the 5-(1-)percent level.