Centre for Research
and Analysis
of Migration

CReAM

Discussion Paper Series

CDP No 25/09

How Important is Access to Jobs? Old Question — Improved Answer.

Olof Åslund, John Östh and Yves Zenou

Centre for Research and Analysis of Migration
Department of Economics, University College London
Drayton House, 30 Gordon Street, London WC1H 0AX

CReAM Discussion Paper No 25/09

How Important is Access to Jobs? Old Question — Improved Answer.

Olof Åslund†, John Östh‡ and Yves Zenou*

† Uppsala University, Institute for Labour Market Policy Evaluation (IFAU) and Center for Business and Policy Studies (SNS)
‡ Uppsala University
* Stockholm University, Research Institute of Industrial Economics (IFN) and CREAM

Non-Technical Abstract

We study the impact of job proximity on individual employment and earnings. The analysis exploits a Swedish refugee dispersal policy to obtain 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. Considering that the 1999 employment rate was 43 percent among the refugees, this is a considerable effect. The analysis suggests that residential sorting leads to underestimation of the impact of job access.

Keywords: Job access, endogenous location, natural experiment.

JEL Classification: J15, J18, R23.

Centre for Research and Analysis of Migration
Department of Economics, Drayton House, 30 Gordon Street, London WC1H 0AX
Telephone Number: +44 (0)20 7679 5888
Facsimile Number: +44 (0)20 7916 2775

How Important is Access to Jobs?

Old Question — Improved Answer[•]

by

Olof Åslund[♣], John Östh[♦] and Yves Zenou[*]

October 21, 2009

Abstract

We study the impact of job proximity on individual employment and earnings. The analysis exploits a Swedish refugee dispersal policy to obtain 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. Considering that the 1999 employment rate was 43 percent among the refugees, this is a considerable effect. The analysis suggests that residential sorting leads to underestimation of the impact of job access.

Key words: job access, endogenous location, natural experiment.
JEL Classification: J15, J18, R23.

[•] We thank Henry Overman, two anonymous referees, as well as Keith Ihlanfeldt, Eva Mörk and Oskar Skans for helpful comments. We are also grateful to the seminar participants at IFAU and at the Department of Economics, Uppsala University, for their useful comments, in particular Per-Anders Edin and Peter Fredriksson.

[♣] IFAU, Institute for Labour Market Policy Evaluation adn Uppsala University, Box 513, S-751 20 Uppsala, Sweden. E-mail: olof.aslund@ifau.uu.se. Also affiliated with Center for Business and Policy Studies, SNS.

[♦] Uppsala University, Department of Social and Economic Geography, Box 513, S-751 20 Uppsala, Sweden. E-mail: John.Osth@kultgeog.uu.se

[*] Corresponding author. Stockholm University, Department of Economics, S-106 91, Stockholm, Sweden. E-mail: yves.zenou@ne.su.se. Also affiliated with the Research Institute of Industrial Economics (IFN) and CREAM.

1 Introduction

In most industrialized countries, majority and minority groups have very unequal labor market outcomes. There are also substantial differences in the residential distribution of these groups. Most American cities, for example, 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 and 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*.[2] [3]

The main problem with testing this mechanism 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 ethnic minority workers have adverse labor market outcomes. It may well be that the more (unobserved) productive workers choose locations close to jobs while the others reside further away. Of course, residential sorting can also lead researchers to understate the impact of job access, e.g. if "residential amenities" are better in less job-dense areas, or if the low-skilled are forced to live close to jobs due to transportation restrictions. There may also be 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. Finally, another source of endogeneity is that workers with jobs or higher earnings may actually choose residential locations with poor job access in order to consume larger amounts of housing at a lower price, as hypothesized by the standard urban model.

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 and Glaeser, 1997, Weinberg, 2000, 2004). Another way is to focus the analysis on youth who still reside

[1] Cutler and 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] See, for example, Allard and Danziger (2002) for an interesting study on Detroit Metropolitan area which shows that, after controlling for individual characteristics, greater proximity to employment opportunities is associated with both a higher probability of working and of leaving welfare.

[3] This mechanism is related to the so-called spatial-mismatch hypothesis (Kain, 1968), even though the latter requires more than just bad job access. We discuss in more detail the relationship between these two mechanisms in Section 5.4 below.

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.

There are also three more recent papers where the authors control for neighborhood selection in a different way than those mentioned above. Holzer et al. (2003) exploit a natural experiment (the opening of a new transit line) to evaluate the impact of job access on minority workers. Their findings suggest that employment effects are greatest for those residing nearest to the origin of the new transit road. Weinberg et al. (2004) do not have an experiment, but they make an effort to control for neighborhood selection and other characteristics of neighborhoods. They find that neighborhoods have a significant impact on individual employment outcomes; in particular, both social influences and job proximity are found to be important determinants of work. Finally, using a unique panel of the quarterly employment experience of individual (Temporary Assistance for Needy Families) TANF recipients that contains both individual and place level variables, Gurmu et al. (2008) estimate the relationship between job access and employment probability on a sub-sample of TANF recipients living in *public housing*, claiming that their residential locations can be considered exogenously determined. Contrary to the previous papers, they find that location does not seem to be important to the employment probability of welfare recipients.

Another problem highlighted by Ihlanfeldt and 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.[4] 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 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 and 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

[4] See Shen (2001) who also argues that job vacancies should be captured both by the stock of jobs and job growth.

the residence and the workplace of all Swedish workers, which enables us to calculate individual based job access measures.[5]

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. With the help of the rich data, we avoid much of the measurement error and the omitted variable problems mentioned above. We thus believe that this study is better able than most previous research to overcome the problems inherent to the testing of the impact of job access on labor market outcomes.[6]

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 finding 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 as follows. Section 2 briefly presents some theories on why access 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 employment and

[5] Åslund and Rooth (2007) use a similar strategy to study how regional (and national) labor market conditions at the time of arrival affect economic integration. But while Åslund and Rooth use unemployment rates for broader regions (and thus come close to the wage curve literature), we investigate whether the distribution of jobs and residences within a location matters for individual outcomes. This analysis is made possible by superior data.

[6] 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 and neighborhood poverty rate effects rather than job access on different outcomes of workers (Quigley and Raphael, 2008).

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

In this section, we present some mechanisms that explain why bad job access can negatively affect labor-market outcomes. Even though we do not test a particular mechanism, the presentation helps us to understand and to interpret some of our results obtained below. Among the possible mechanisms are:[7]

(*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 and 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 and 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 and Zenou, 2003). This will cause them to restrict their spatial search horizon at the vicinity of their neighborhood (Gautier and Zenou, 2008). 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

[7] See Gobillon et al. (2007) for a more general overview of these theories.

draw a red line beyond which they will not hire workers (Wilson, 1996; Zenou and Boccard, 2000; 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.[8] 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 thirteen percent of Sweden's nine million population. 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. Figure 1 provides a map of Sweden with the major cities and the location of refugees, foreign-born and the overall population.

[8] To the best of our knowledge, the only paper that tests explicitly one of the theoretical mechanisms mentioned above is that of Patacchini and Zenou (2005). They test mechanism (*i*) using English data.

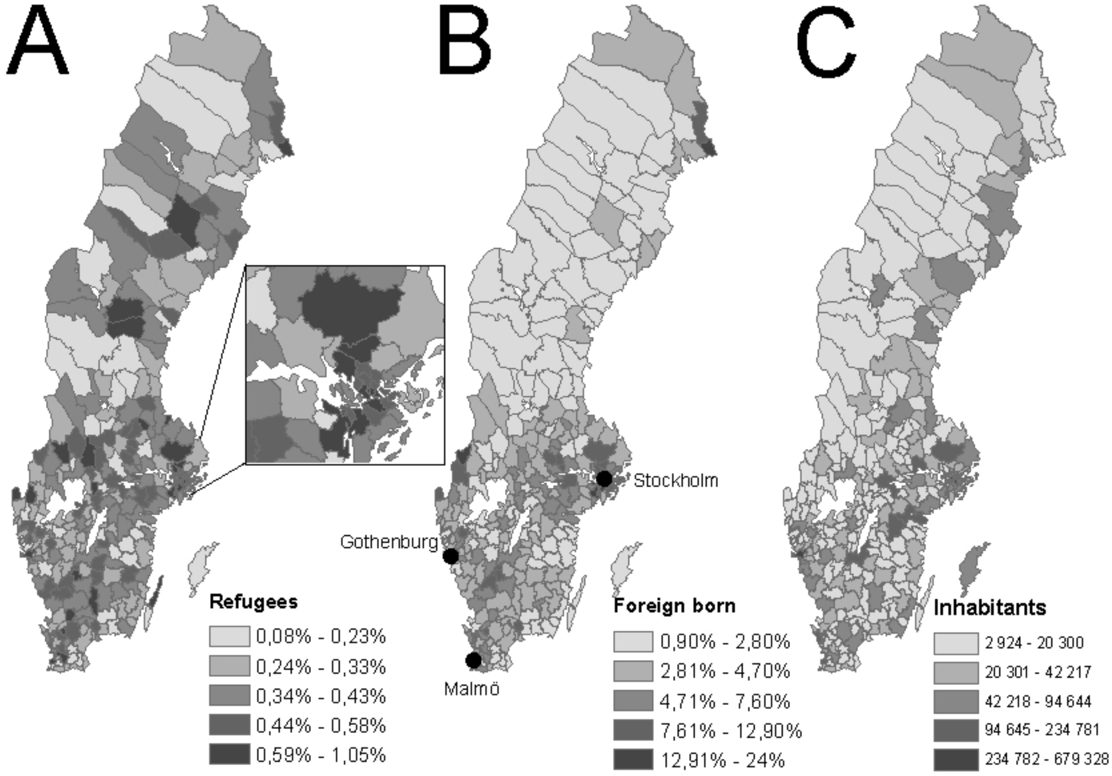

Figure 1 Swedish demographics, 1991.

Notes: Part A illustrates the geographical distribution of refugees arriving in 1990 and 1991, percentage values describe the 1990/1991 refugees to entire population ratio. Part B illustrates the distribution of foreign-born in Sweden. Part C illustrates the distribution of the overall population.

As illustrated in Figure 1, the distribution of refugees arriving in Sweden either in 1990 or in 1991 (part A) differs significantly from the distribution of all foreign born (part B). There is a strong geographical correlation between the number of inhabitants (part C) and the share of immigrants (part B). The figure illustrates the visually detectable effect of the refugee placement policy.

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 22,250 Swedish kronor in 2002 while, for immigrants from non-European countries it was 19,050. As for the median wage, the figures were 17,160 and 19,800 Swedish kronor for Swedish-born and immigrants, respectively.

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 2 presents the patterns of job location and immigrant density in Stockholm. Clearly, the very immigrant dense areas (left map) are scattered in the suburban areas, while the majority of jobs (right map) are located in the central parts of the city. So, there seems to be a 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 of commuting to the central business district may not be an important explanation as to why these areas are poorer than other suburbs.[9]

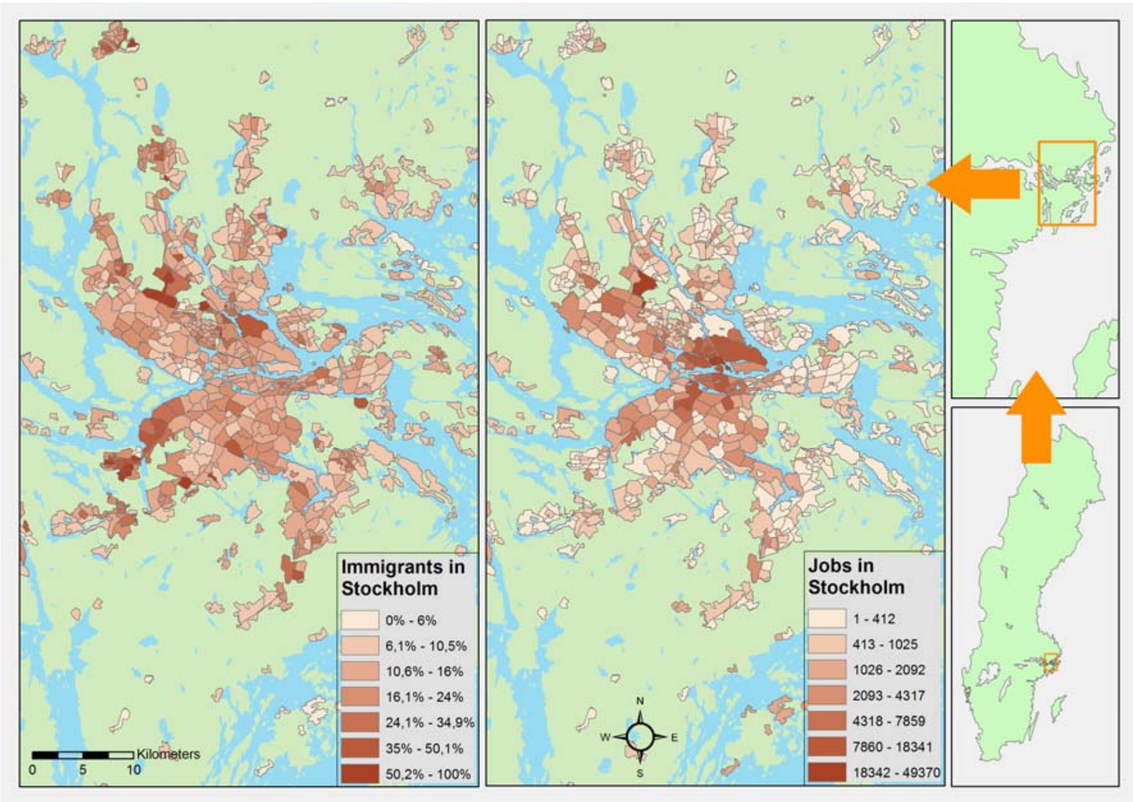

Figure 2 Job density and foreign-born population in the Stockholm area.

Notes: The left map show the percentage of immigrants residing in different parts of the greater Stockholm area. In the adjacent map on the right the absolute number of jobs is displayed. The two maps show that while a greater share of the jobs are located in the central parts of the city, the immigrant population is predominately located in the suburbs. The two maps at the extreme right illustrate the location of the greater Stockholm area in Sweden.

[9] 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[10]

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 were 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.[11] This is also clear from our data showing that the inflow was to neighborhoods with low employment rates: 75.9 percent compared to 80.0 percent in the overall population.[12]

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 and Fredriksson, 2009, Åslund and Rooth, 2007).[13] 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

---

[10] 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.

[11] 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.

[12] As will be discussed below, the neighborhood is defined as so-called SAMS areas. Over time the difference in local employment between the studied refugees and the overall population was accentuated. In 1999, the average refugee lived in an area with an employment rate of 66.2 percent, compared to 77.6 percent in the overall population.

[13] In addition to the differences in the questions and topics addressed, the data differ across the studies. In particular, these three papers contain no information on the location of jobs, and only regional information on the residents. By contrast, the current study has access to information on the exact locations of both jobs and residents, which is of course at the heart of our analysis since we are testing how access to jobs affects the labor-market outcomes of ethnic minorities.

when placed in centers. After receiving asylum and a permanent residence permit, the refugee was placed in a municipality.[14] 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 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. Previous studies show that the policy had an impact on the distribution of the refugee inflow and implied a shift away from the major cities to smaller locations, often in the Northern part of Sweden (Edin et al 2003, Åslund 2005).

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. We therefore believe that it is plausible to think of the 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

[14] 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.

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 people arriving in Sweden in the years 1990 and 1991. To capture the 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[15]) 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 individuals. After applying the age restrictions used in the refugee population, the second population contained 424,462 individuals.

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 working age people living within a 5 km radius from the individual's place of living.

We compute job access variables (i.e. both the number of jobs and working age people living within a 5 km radius) with the help of geographical coordinates listing all individuals' place of residence and the working population's workplace coordinates. The population variable captures both competing labor supply and a potential effect of urban density.[16] The job access variables used in the baseline model are designed to estimate the surrounding competition for jobs as well as the number of surrounding jobs, thus forming job housing balance measures of potential accessibility (Cervero, 1989; 1996). It is worth pointing out that the job access measures are not constrained by administrative borders (between e.g. municipalities).

Since all of Sweden is included in the study, accessibility values must be estimated for every place of habitat and work. This vast amount of locations restrict the use of more complex accessibility measures, such as singly or doubly constrained measures of potential accessibility

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

[16] 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).

(Joseph and Bantock, 1982; Shen, 1998; Östh, 2007). In order to keep computations at a reasonable level, a crude but accomplishable accessibility measure is used.

The calculation of the job access and population variables is built on *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 working age residents per square kilometer. All values encompassed by the circle are summed and saved with the coordinates of the centralmost 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.[17]

The choice of radius is of course open to debate. If too small catchment areas are used, the estimated values may represent accessibility poorly. With too large catchment areas, the measures become uninformative (see Östh, 2007 for further discussions).[18] Figure 3 shows the cumulative distribution of commutes (straight-line distance between home and work) in the random population sample.[19] The median commute is very close to 5 km, which supports the use of this radius to represent observed behavior in a reasonable way.[20] Moreover, in section 5, we will also use, as a robustness check, different radii.

[17] 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.

[18] This is particularly true since, as will be discussed below, the empirical models include relatively low-level regional fixed effects.

[19] In Table 1, the median commute is also around 5 km in the refugee sample.

[20] The median distance between residence and jobs are thus shorter in Sweden than in, for example, the United States. Remember, however, that our measure is a straight line, which is not directly comparable to the travel distance reported in the typical survey on commuting.

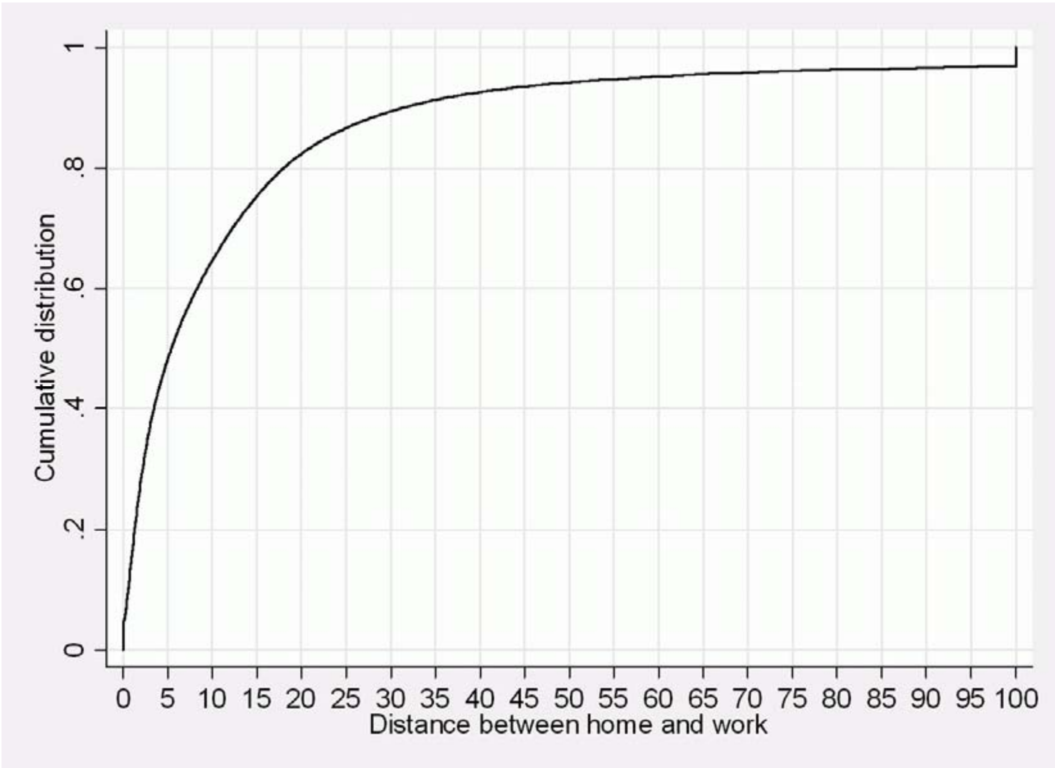

Figure 3 Commuting distance, 1999.

Notes: The figure shows the cumulative distribution of commuting (distance of a straight line between residence and workplace) in the representative sample of the population in 1999. The workers who commute more than 100 km have been given the value of 100, which explains the ”jump” in the curve.

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 1,000). For each SAMS, we compute characteristics such as commuting rates, fraction highly educated, the fraction foreign-born, and the extent of welfare receipt (see the Appendix for the 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, the overall sample of the Swedish population and the foreign-born in the population sample. Clearly, earnings and employment are much lower among the refugees compared to the overall population. 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, and for the foreign-born in this sample it is 61 percent. Turning to the job access measures, we see that refugees live in more populated and job-dense areas. Note, however, that the empirical analysis measures proximity to jobs conditional on the surrounding residential population and municipality fixed effects. As will be discussed below, job access then tends to be lower among the foreign-born than among natives. 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 (which is expected given the pattern in Figure 3). Note that mean commutes are substantially longer than the median; 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. The level of education among the foreign-born in the population sample tends to be lower, particularly due to a smaller fraction of highly educated individuals.

Table 1 Descriptive statistics.

	1990–91 Refugees		Random population sample			
			Full		Immigrants	
Variable	Mean (sd)	Median	Mean (sd)	Median	Mean (sd)	Median
Ann. Earn. (1,000 SEK)	125.6	114.1	211.9	201.9	185.4	183.9
(cond. on y>0)	(121.4)		(138.0)		(119.5)	
Fraction earnings>0	.58		.85		.70	
Employment	.43		.78		.61	
ln # jobs within 5 km	10.39	10.55	8.92	9.22	9.79	10.20
	(1.37)		(2.32)		(2.02)	
ln # working age people	10.44	10.63	9.17	9.32	9.94	10.34
within 5 km	(1.19)		(1.90)		(1.67)	
Commuting rate in SAMS (>5 km)	.49 (.19)	.48	.53 (.22)	.53	.52	
Female	.43		.49		.51	
Age	38.83	37	44.96	45	46.37	47
	(8.2)		(10.6)			
Education						
Missing	.06					
<9 years	.16		.12		.19	
9-10 yrs	.18		.13		.15	
Secondary	.31		.47		.43	
Tertiary <2 yrs	.04		.06		.05	
Tertiary >=2 yrs	.23		.21		.17	
Graduate	.02		.01		.01	
Civil status						
Married male	.29		.25		.25	
Married female	.23		.26		.27	
Cohabiting male	.03		.05		.04	
Cohabiting female	.02		.05		.03	
Single	.43		.39		.42	
Commuting distance	17.4	4.7	19.4	5.4	16.8	4.9
	(57.4)		(61.8)		(56.4)	
# observations	21,745		424,462		45,366	

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:

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

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 on job access. 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 and Fredriksson, forthcoming, Åslund and Rooth, 2007). 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.

Our baseline model is a reduced-form specification where 1999 outcomes are regressed on immigration year job access. This model allows the impact of initial job access on later outcomes to work via any number of mechanisms; e.g. state dependence ("scarring", i.e. past outcomes affects current outcomes) and an increased probability of living in a location with poor job access also in 1999.[21]

It is clearly not obvious which structural model that best captures the impact of job access on individual employment. One may or may not wish to consider e.g. the history of exposure to certain environments (see Åslund and Fredriksson (2009) for a discussion). We estimate 2SLS specifications where 1999 job access is instrumented by immigration year job access to illustrate the impact of contemporary job access. In addition to the the conditional exogeneity of the initial location, this approach requires also the exclusion restriction that the *only* link between immigration year job access and employment in 1999 is through local job access in 1999. We believe that there are reasons to question this assumption, which is why we focus more on the reduced-form results.[22]

[21] There are of course several possible causes for state dependence: skill loss during unemployment, signalling to employers, and poor peer connections as in Calvó-Armengol and Jackson (2004). Hansen and Löfstrom (2009) suggest 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 and Heckman (1982) or Lynch (1989). Åslund and Rooth (2007) analyze long-term effects of facing high local unemployment rates after immigration, and find support for both scarring and geographical lock-in mechanisms.

[22] Throughout, we use linear probability models for the employment outcome. The baseline (reduced form) results in the refugee analysis are very similar with a probit model. A two-step IV probit yields qualitatively similar, although not directly comparable (see Statacorp 2005) estimates as in the 2SLS specifications.

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.[23]

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 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.[24] 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 and Fredriksson (2009) use a different database to study welfare dependence with essentially the same group of refugees. Their data include month

[23] 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.
[24] 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.

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.[25]

[25] 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)			
Years since migration		23.12		
		(11.69)		
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.[26] 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.

[26] 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**	.004*	.006	.006	.015**
	(.001)	(.001)	(.001)	(.004)	(.003)	(.004)
ln # working age 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**	.088**	.127**	.127**	.119**
	(.002)	(.002)	(.002)	(.006)	(.006)	(.006)
Tertiary <2 yrs	.079**	.079**	.070**	.135**	.135**	.122**
	(.003)	(.003)	(.003)	(.009)	(.009)	(.009)
Tertiary >=2 yrs	.181**	.181**	.172**	.440**	.440**	.427**
	(.003)	(.003)	(.003)	(.007)	(.007)	(.007)
Graduate	.209**	.209**	.198**	.728**	.728**	.709**
	(.005)	(.005)	(.005)	(.015)	(.015)	(.015)
Job growth (98–99)		.011			.003	
		(.007)			(.018)	
Commute rate			.010			.083**
			(.006)			(.014)
Fr. highly educated			–.079**			–.044*
			(.008)			(.020)
Fr. foreign–born			–.047**			.074*
			(.014)			(.036)
Fr. Welfare Recipients			–.574**			–1.061**
			(.021)			(0.056)
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	424,462	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 A3 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 studies on job-access: (*i*) the failure to control for other 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 somewhat related to employment (significant at the 10 pct level) 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 four 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, the fraction foreign-born, and the fraction of welfare recipient.[27] 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.[28] The average level of education in the neighborhood is not correlated with individual earnings (conditional on the other covariates). Living in areas with a high fraction of welfare recipient is strongly negatively related to earnings and employment. A standard deviation (within municipalities) in the fraction

[27] 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.
[28] Of course, it may also capture e.g. areas with many students.

of welfare recipient amounts to 5.4 percentage points. Such a variation is associated with 5.7 percent lower earnings and a 3.1 percentage points reduction in employment. The fraction of foreign-born is less strongly correlated with the outcomes. However, this variable is strongly correlated with the fraction of welfare recipient; excluding the latter variable yields much stronger estimates for the fraction of foreign-born in the neighborhood.

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, such as the low-educated ones. 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 on job access. 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?

Assuming that the correct model is that only contemporary job access matters, we can instrument 1999 job access by immigration year job access. For the 2SLS model to be meaningful, the instrument must have a predictive power in the first stage equations.[29] One concern is the high geographical mobility of the refugees (see Åslund, 2005, for details). Our data show that only 15–19 percent of the studied refugees remained in the placement neighborhood in 1999. However, about 50 percent were still in the same municipality and, among those who moved within municipalities, the median relocation distance was 2,800 metres. In other words, the initial placement influenced the 1999 local job access for a larger group than those who remained at the exact same address.

Whether the instrument is strong enough must therefore be tested empirically. Table A2 in the Appendix displays the first stage regressions of the 2SLS “IV” model. As can be seen in the table, the R-squares are relatively high. The added R-squares are indeed relatively (but not

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

unusually) low. The number of jobs in the assignment location is strongly correlated with the number of jobs in the 1999 location. In the baseline specification, this is not the case for the population variables. It can be shown that this is due to the fact that we include relatively fine-level geographical dummies in the baseline model.[30] However, in all specifications, the two instruments are jointly significant (as given by the *F* tests). Observe that, in Table A2, in some regression, two individually insignificant variables appear jointly significant with a *F*-test. The reason is presumably the strong correlation (0.97) between the population and the number of jobs variables, which makes it hard to separate the coefficients. The difference between the regressions for the number of jobs and the population size can be attributed to the fact that the municipal fixed effects pick up different parts of the variation in the different specifications.

Let us now interpret the different columns of Table 4. 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.[31] This means that job access has a lasting effect on employment outcomes for refugees.[32]

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.[33] The IV employment estimate has a large standard error and the confidence interval stretches from 0.06 to 0.44. At face value it suggests that living in an area with twice the number of jobs (ceteris paribus) increases the individual employment probability by 25 percentage points, which is a huge effect of job proximity. Besides the statistical uncertainty, there are reasons to be skeptical about such a large effect given the assumptions regarding the exclusion restriction.

[30] If we instead of 287 municipal dummies include 21 county dummies, the population variable instrument is strongly correlated with the endogenous regressor. One interpretation of this is that while jobs are unequally distributed also within municipalities, there is not so much variation in population density once we eliminate the across-municipal variation.

[31] 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).

[32] We have also included segregation variables (i.e. proportions of highly educated and foreign-born) and the empirical results displayed in Table 4 are qualitatively unchanged.

[33] For IV to capture average treatment effects rather than local average treatment effects (LATE), 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 # working age 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	21,745	21,745	21,745	12,655	12,655	12,655
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 A3 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.[34] Given that Swedish city centers are typically rich, this pattern may be surprising if one has in mind a standard model where residential areas are located at different distances from a central business district. However, in

[34] A similar sorting pattern is found in Åslund and Fredriksson (2009).

our setting, job access will also be high for those living close to e.g. industrial areas, hospitals etc, i.e. locations that may not be so attractive.

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 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 job access literature (Ihlanfeldt and 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 and 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.[35] 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 # working age people (5 km)	–.047**	–.070	–.045**	–.071
	(.015)	(.052)	(.015)	(.052)
Commute rate			.007	.114
			(.035)	(.129)
Fr. Highly educated			–.000	.004*
			(.001)	(.002)
Fr. Foreign–born			.000	.002
			(.000)	(.001)
Fr. Welfare recipients			–.126**	–.237
			(.038)	(.175)
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.

[35] 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.

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 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. It is also possible that the baseline specification is not flexible enough to account for the selection on observables in the placement. We therefore tried including five-year dummies for age, interacted with 16 categories of family status and level of education, respectively. The estimated impact of job access was insensitive to this variation.

We also split the sample and ran the regressions by groups; see Table A4. 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 job access 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.[36] 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? Job access versus spatial mismatch

So far, we have mainly tested the effect of job access on labor-market outcomes of refugees in Sweden. As stated in footnote 3 in the Introduction, there is an important literature based on the Spatial-Mismatch Hypothesis (SMH), which also focuses on the impact of job access (and distance to jobs) on outcomes. Indeed, Kain (1968), who initiated the SMH, argued that residing

[36] 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.

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. Three conditions must be satisfied in order for a spatial mismatch between workers and jobs to play a role in explaining differences in employment or earning among different groups: (*a*) distance must be an impediment, (*b*) there must be barriers in place that prevent the disadvantaged group from overcoming the distance impediment, and (*c*) the disadvantaged group must have worse access to jobs (i.e., be located farther away from jobs) than the advantaged group.

The SMH has most frequently been offered as a possible explanation for the relatively low employment rates of black in the U.S. Blacks are concentrated within central cities and low-skilled job growth is occurring in the suburbs. They cannot overcome the distance impediment because they have poor transportation options for reverse commuting and face racial discrimination in the housing market if they attempt to move to the suburbs. They also are usually shown to be located farther from available jobs than less educated whites and therefore a portion of the employment difference between the races can be attributed to spatial mismatch. Since the study of Kain, hundreds of studies have been carried out trying to test the spatial mismatch hypothesis;[37] most of these studies have shown that indeed distance to jobs negatively affects the labor-market outcomes of ethnic minorities (see, in particular, the literature surveys by Holzer, 1991, Kain, 1992, Ihlanfeldt and Sjoquist, 1998).

The present paper shows that distance (i.e. job access) matters, confirming condition (*a*) of the SMH. As in the U.S., condition (*b*) is likely to be satisfied in Sweden since there are barriers that prevent ethnic minorities from overcoming the distance impediment. Immigrants are typically credit constrained, available public housing is not available at all places and housing discrimination is widespread (see, in particular, Ahmed and Hammarstedt, 2008, who provide strong evidence of housing discrimination for ethnic minorities in Sweden). Concerning condition (*c*), in the raw data, refugees live in more populated and job-dense areas. However, to be in line with the empirical analysis above, we need to measure job access *conditional* on the surrounding residential population and municipality fixed effects. To investigate this issue, we regress 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.

According to the estimates in Table 6, we find that immigrants have fewer jobs in their surroundings (conditional on the number of people living there). For those coming from 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. This indicates that condition (*c*) of the SMH is also satisfied in our Swedish study. As a result, even though the focus on this paper is on the impact of *job access* on ethnic minorities' outcomes, most features of the SMH are present here.

[37] Most empirical studies are using US data. Very few are European. Exceptions include Thomas (1998) and Patacchini and Zenou (2005), for the UK, and Dujardin et al. (2008) for Belgium.

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 # working age 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 working age people (5 km)”, using the random population sample.

The results displayed in Table 6 thus suggest that immigrants have somewhat lower job access than natives. 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.[38] Using the “other countries” estimate from column (2) of Table 6, would allow us to explain 8.5 percent (i.e. (0.073*0.255)/0.23) of the employment gap. Using instead the reduced-form coefficient of 0.029 from Table 4, would imply that only 1 percent (i.e. (0.073*0.029)/0.23) of the gap can be explained by differences in job access.

More generally, however, one could claim that bad job access and 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 in 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

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

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 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. Of course, the bias due to residential sorting could also go the other way around.

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.

As with any natural experiment, our analysis is quite specific and is in a strict sense only informative about the refugees who obtained their permits in 1990/1991. Also, Sweden and the United States have experienced different patterns of segregation (Hårsman and Quigley, 1995) and have different histories, cultures, public transportation systems, etc. Still, one could argue that our analysis can shed *some* light on the nearly exclusively American debate on whether job access affects labor market outcomes of ethnic minorities. As in the US, ethnic minorities have lower spatial job access, reduced and constrained mobility, and there is an apparent general connection between job access and individual outcomes. Our analysis shows that job access is causally related to the probability of obtaining a job among minority workers with poor average labor market status, and that sorting (if anything) gives a negative bias in the estimates. It seems reasonable to argue that our findings give support to studies suggesting that job access is a factor of importance also in the US.

References

Andersson, R. (2000), Etnisk och Socioekonomisk Segregation i Sverige 1990–1998, In: *Välfärden Förutsättningar SOU 2000:37*, Fritzell, J. (Ed.) Stockholm, Fritzes.

Ahmed, A.M. and Hammarstedt, M. (2008), Discrimination in the Rental Housing Market: A Field Experiment on the Internet, *Journal of Urban Economics*, 64, pp. 362–372.

Allard, S.W. and Danziger, S. (2002), Proximity and Opportunity: How Residence and Race Affect the Employment of Welfare Recipients, *Housing Policy Debate*, 13, pp. 675–700.

Åslund, O. (2005), "Now and Forever? Initial and Subsequent Location Choices of Immigrants", *Regional Science and Urban Economics*, 35, pp. 141–165.

Åslund, O. and Fredriksson, P. (2009), Peer Effects in Welfare Cultures—Quasi-Experimental Evidence, *Journal of Human Resources*, forthcoming.

Åslund, O. and Rooth, D-O. (2007), Do When and Where Matter? Initial Labour Market Conditions and Immigrant Earnings, *Economic Journal*, 117, pp. 422–448.

Bound, J., Jaeger, D.A. and Baker, R.M. (2000), Problems with Instrumental Variables Estimation when the Correlation between the Instruments and the Endogenous Explanatory Variable is Weak, *Journal of the American Statistical Association*, 90, pp. 443–450.

Brueckner, J. and Zenou, Y. (2003), Space and Unemployment: The Labor market Effects of Spatial Mismatch, *Journal of Labor Economics*, 21, pp. 242-266.

Calvó-Armengol, A. and Jackson, M.O. (2004), The Effects of Social Networks on Employment and Inequality, *American Economic Review*, 94, pp. 426-454.

Cervero, R. (1989), Job-Housing Balance and Regional Mobility, *Journal of the American Planning Association*, 55, pp. 136-150.

Cervero, R. (1996), Job-Housing Balance Revisited, *Journal of the American Planning Association*, 62, pp. 492-511.

Coulson, E., Laing, D. and Wang, P. (2001), Spatial Mismatch in Search Equilibrium, *Journal of Labor Economics*, 19, pp. 949-972.

Cutler, D. and Glaeser, E. (1997), Are Ghettos Good or Bad?, *Quarterly Journal of Economics*, 112, pp. 827-872.

Cutler, D., Glaeser, E. and Vigdor, J. (1999), The Rise and Decline of the American Ghetto, *Journal of Political Economy*, 107, pp. 455-506.

Dujardin, C., Selod, H. and Thomas, I. (2008), Residential Segregation and Unemployment: the Case of Brussels, *Urban Studies*, 45, pp. 89-113.

Edin, P.-A., Fredriksson, P. and Åslund, O. (2003), Ethnic Enclaves and the Economic Success of Immigrants – Evidence from a Natural Experiment, *Quarterly Journal of Economics*, 118, pp. 329-357.

Edin, P.-A., Fredriksson, P. and Åslund, O. (2004), Settlement Policies and the Economic Success of Immigrants, *Journal of Population Economics*, 17, pp. 133–155.

Flinn, C. and Heckman, J. (1982), New Methods for Analyzing Structural Models of Labor Force Dynamics, *Journal of Econometrics*, 18, pp. 115-168.

Gabriel, S.A. and Rosenthal, S.S. (1996), Commutes, Neighborhood Effects, and Earnings: An Analysis of Racial Discrimination and Compensating Differentials, *Journal of Urban Economics*, 40, pp. 61-83.

Gautier, P.A. and Zenou, Y. (2008), Car Ownership and the Labor Market of Ethnic Minorities, CEPR Discussion Paper No. 7061.

Gobillon, L., Selod, H. and Zenou, Y. (2007), The Mechanisms of Spatial Mismatch, *Urban Studies*, 44, pp. 2401-2427.

Gurmu, S., Ihlanfeldt, K.R. and Smith, W.J. (2008), Does Residential Location Matter to the Employment of TANF Recipients. Evidence from a Dynamic Discrete Choice Model with Unobserved Effects, *Journal of Urban Economics*, 63, pp. 325-351.

Hansen, J. and Löfstrom, M. (2009), The Dynamics of Immigrant Welfare and Labor Market Behavior, *Journal of Population Economics*, forthcoming.

Holzer, H.J. (1991), The Spatial Mismatch Hypothesis: What has the Evidence shown?, *Urban Studies*, 28, pp. 105-122.

Holzer, H.J., Quigley, J.M. and S. Raphael (2003), Public Transit and the Spatial Distribution of Minority Employment: Evidence from a Natural Experiment, *Journal of Policy Analysis and Management*, 22, pp. 415-442.

Hårsman, B. and Quigley, J.M. (1995), The Spatial Segregation of Ethnic and Demographic Groups: Comparative Evidence from Stockholm and San Francisco, *Journal of Urban Economics*, 37, pp. 1-16.

Ihlanfeldt, K.R. (1992), *Job Accessibility and the Employment and School Enrolment of Teenagers*, Kalamazoo (MI): W.E. Upjohn Institute for Employment Research.

Ihlanfeldt, K.R. (1997), Information on the Spatial Distribution of Job Opportunities within Metropolitan Areas, *Journal of Urban Economics,* 41, pp. 218-242.

Ihlanfeldt, K.R. (2006), A Primer on Spatial Mismatch within Urban Labor Markets, In: *A Companion to Urban Economics*, R. Arnott and D. McMillen (Eds.), Boston: Blackwell Publishing, pp. 404-417.

Ihlanfeldt, K. R. and Sjoquist, D. (1998), The Spatial Mismatch Hypothesis: A Review of Recent Studies and their Implications for Welfare Reform, *Housing Policy Debate*, 9, pp. 849-892.

Joseph, A. and Bantock, P. (1982), Measuring Potential Physical accessibility to General Practitioners in Rural Areas: A Method and Case Study, *Social Science and Medicine,* 16, 85-90.

Kain, J. (1968), Housing Segregation, Negro Employment, and Metropolitan Decentralization, *Quarterly Journal of Economics*, 82, pp. 175-197.

Kain, J. (1992), The Spatial Mismatch Hypothesis: Three Decades Later, *Housing Policy Debate*, 3, pp. 371-460.

Kling, J.R., Ludwig, J., and Katz, L.F. (2005), Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment, *Quarterly Journal of Economics*, 120, pp. 87-130.

Ludwig, J., Duncan, G.J. and Hirschfield, P. (2001), Urban Poverty and Juvenile crime: Evidence from a Randomized Housing-Mobility Experiment, *Quarterly Journal of Economics*, 116, pp. 655-679.

Lynch, L.M. (1989), The Youth Labor Market in the Eighties: Determinants of Reemployment Probabilities for Young Men and Women, *Review of Economics and Statistics*, 71, pp. 37-54.

Manning, A. (2003), The Real Thin Theory: Monopsony in Modern Labour Markets, *Labour Economics*, 10, pp. 105-131.

Olli Segendorf, Å. (2005), *Job Search Strategies and Wage Effects for Immigrants*, PhD Thesis, SOFI, Stockholm University.

Oreopoulos, P. (2003), The Long-Run Consequences of Living in a Poor Neighborhood, *Quarterly Journal of Economics*, 4, pp. 1533-1575.

Östh, J. (2007), *Home, Job and Space. Mapping and Modeling the Labor Market*, PhD Dissertation, Uppsala University, Uppsala, Sweden.

Patacchini, E. and Zenou, Y. (2005), Spatial Mismatch, Transport Mode and Search Decisions in England, *Journal of Urban Economics*, 58, pp. 62-90.

Quigley, J.M. and Raphael, S. (2008), Neighborhoods, Economic Self-Sufficiency, and the MTO program, *Brookings-Wharton Papers on Urban Affairs*, 7, pp. 1-46

Raphael, S. (1998), The Spatial Mismatch Hypothesis and Black Youth Joblessness: Evidence from the San Francisco Bay Area, *Journal of Urban Economics*, 43, pp. 79-111.

Shen, Q. (1998), Location Characteristics of Inner-City Neighborhoods and Employment Accessibility of Low-Income Workers, *Environment and Planning B: Planning and Design*, 25, pp. 345-365.

Shen, Q. (2001), A Spatial Analysis of Job Openings and Access in a US Metropolitan Area, *Journal of the American Planning Association*, 67, pp. 53-68.

Smith, T. and Zenou, Y. (2003), Spatial Mismatch, Search Effort and Urban Spatial Structure, *Journal of Urban Economics*, 54, pp. 129-156.

Statacorp (2005), *Stata Base Reference Manual Volume 1 A–J Release 9*.

Thomas, J.M. (1998), Ethnic Variation in Commuting Propensity and Unemployment Spells: Some UK Evidence, *Journal of Urban Economics*, 43, pp. 385-400.

The Committee on Immigration Policy (1996), *Sverige, Framtiden och Mångfalden. Slutbetänkande från Invandrarpolitiska Kommittén*, SOU 1996:55, Stockholm: Fritzes.

The Immigration Board (1997), *Individuell Mångfald: Invandrarverkets Utvärdering och Analys av det Samordnade Flyktingmottagandet 1991–1996*, Norrköping: Statens invandrarverk.

Wasmer, E., and Zenou, Y. (2002), Does City Structure Affect Search and Welfare? *Journal of Urban Economics*, 51, pp. 515-541.

Weinberg, B.A. (2000), Black Residential Centralization and the Spatial Mismatch Hypothesis, *Journal of Urban Economics*, 48, pp. 110-134.

Weinberg, B.A. (2004), Testing the Spatial Mismatch Hypothesis using Inter-City Variations in Industrial Composition, *Regional Science and Urban Economics*, 34, pp. 505-532.

Weinberg, B.A., Yankow, J.J. and Reagan, P.B. (2004), Do Neighborhoods Affect Hours Worked? Evidence from Longitudinal Data, *Journal of Labor Economics*, 22, pp. 891-924.

Wilson, J. (1996), *When Work Disappears: The World of the New Urban Poor*. New York: Alfred A. Knopf.

Zax, J.S. (1991), Compensation for Commutes in Labor and Housing Markets, *Journal of Urban Economics*, 30, pp. 192-207.

Zenou, Y. (2002), How do Firms Redline Workers? *Journal of Urban Economics*, 52, pp. 391-408.

Zenou, Y. and N. Boccard (2000), Racial Discrimination and Redlining in Cities, *Journal of Urban Economics*, 48, pp. 260-285.

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 # working age people within 5 km	Number of resident working age 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.
Fraction of welfare recipients	Share of population in SAMS area receiving social assistance.
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 First stage estimates for 2SLS specifications in Table 4.

	First stage estimates for 2SLS specifications in Table 4			
Dependent var:	Empl.	Empl.	Log earn	Log earn
Instrumented variable	Ln # jobs 5 km	Ln pop. 5 km	Ln # jobs 5 km	Ln pop. 5 km
Instrument				
Ln # jobs	.155**	.022	.139**	.010
	(.038)	(.030)	(.046)	(.036)
Ln population	–.091	.053	–.040	.095
	(.059)	(.046)	(.070)	(.055)
Observations	21,745	21,745	12,655	12,655
R-squared	.25	.30	.29	.34
F test:	45.77	22.99	35.95	21.75
Prob > F	0.000	0.000	0.000	0.000
Added R2	0.006	0.003	0.007	0.004

Notes: The instruments are measured in the year of immigration, the instrumented variables are measured in 1999. The first stage regressions include all covariates listed in Table 4. The F-statistics is performed for the joint significance of the excluded instruments.

Table A3 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*	.007	.003	.008**
	(.002)	(.002)	(.001)	(.002)	(.001)	(.002)	(.001)	(.006)	(.001)	(.003)
ln # working age people (5 km)	–.005*	–.003	–.005**	.005	–.002	–.009**	–.003	–.011	–.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*	–.008	.012**	–.012
	(.004)	(.005)	(.004)	(.007)	(.004)	(.006)	(.003)	(.015)	(.004)	(.008)
ln # working age people (5 km)	.018**	.023**	.021**	.022*	.024**	.010	.020**	.018	.017**	.028*
	(.006)	(.007)	(.005)	(.010)	(.005)	(.008)	(.005)	(.021)	(.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. The specifications for the foreign-born include quadratic controls for years since migration.

Table A4 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 # working age 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.