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Ethnic Diversity and Preferences for Redistribution*

Matz Dahlberg† Karin Edmark‡ Heléne Lundqvist§

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If people are more altruistic towards their own kind, the increased ethnic heterogeneity resulting from increased immigration to Europe in recent decades will likely lead to reduced support for redistribution among natives. This paper exploits a nationwide program placing refugees in municipalities throughout Sweden during the period 1985–94 to isolate exogenous variation in immigrant shares and can thereby estimate causal effects of increased ethnic heterogeneity on preferences for redistribution, defined as preferred social benefit levels as obtained from panel survey data on inhabitants of the receiving municipalities. The results show that a larger immigrant population leads to less support for redistribution, especially among respondents with high income and wealth. We also establish that OLS estimators that do not properly deal with endogeneity problems—as in earlier studies—are likely to yield positively biased effects of ethnic heterogeneity on preferences for redistribution.

Keywords: Income redistribution, ethnic heterogeneity, immigration
JEL codes: D31, D64, I3, Z13

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1 Introduction

During past decades, the immigration of workers and refugees to the European countries has increased substantially. Immigrants are obviously different in terms of their ethnic background compared to "the average native" and, more generally, are overly represented among welfare dependents. Coupled with the increased immigration, these differences raise the question of how an increasing immigrant population has affected natives' views on redistribution and the size of the welfare state?

In a comparison of the US welfare state versus that of most European countries, Alesina et al. (2001) point to the historically much more ethnically heterogeneous US population as one of the main explanations of its welfare state being of a more limited size. There are at least two main mechanisms through which ethnic diversity may influence the welfare state and the degree of redistribution in such a way. On the one hand, there is the mechanism modeled by Roemer et al. (2007) that operates via political parties. In their model, larger immigrant shares reduce the support for redistribution because parties favoring less immigration often also favor less redistribution. This policy-bundling therefore makes it difficult to distinguish a vote for less immigration from a vote for less redistribution.

A second, more direct, possible explanation to a negative link between heterogeneity and redistribution is that people exhibit so-called in-group bias—that is, that people have a tendency to favor their own kind and are more altruistic toward others in their own group.[1] "One's own group" may (but need not) be defined in terms of ethnicity, implying that altruism would not travel well across ethnic lines. The aim of this paper is to provide new and, compared to what has previously been established, more convincing empirical evidence of the causal link behind this idea.

Our main contribution is to identify the causal effects of increased im-

[1] An extensive theoretical framework for this idea is laid out by Shayo (2009), who, in addition to modeling distaste for cognitive distance to other agents, also endogenizes group identity. The equilibrium level of redistribution in his model decreases with the size of minority groups, and the reason is that the increased distance to other agents in the original group of identity makes identification with a less redistributive group more attractive. See also the model in Lindqvist and Östling (2009).

migrant shares by making use of a nearly nationwide program intervention placing refugees in municipalities throughout Sweden between 1985 and 1994. During this period, the placement program provides exogenous variation in the number of refugees placed in the 288 municipalities. By exploiting the source of variation in immigrant shares in the municipalities induced by the refugee placement program, we can estimate the causal effects on individual preferences for redistribution.[2]

Furthermore, a novel feature of our study is that we match the size of the refugee inflow via the placement program to survey information on individuals living in the receiving municipalities. As part of the Swedish National Election Studies Program, the survey has been carried out every election year since the 1950s and is advantageous for several reasons. It includes questions on the respondent's preferences for redistribution, and most importantly, it is in the form of a rotating panel, with each individual being surveyed twice and with half of the sample changing each wave. This panel structure enables us to control for individual fixed effects as well as for time trends in the preferences for redistribution during this period. This means that, to see how increasing immigrant shares causally affects preferences for redistribution, we link changes in an individual's preferences between two elections/survey waves to the placement program-induced change in immigrants in the individual's municipality over the corresponding period. If individuals exhibit positive in-group bias, we expect this effect to be negative.

The existing empirical literature is suggestive—but not conclusive—of positive in-group bias. Luttmer (2001) uses repeated cross-section survey data from the US (The General Survey) over a period from the mid-1970s to the mid-1990s and finds that increased welfare recipiency among blacks makes non-black respondents prefer less redistribution but has little effect on black respondents' preferences, and vice versa for increased welfare recipiency among non-blacks.[3] Senik et al. (2009) use information from the

[2]Using municipal-level data is advantageous, as a municipality is a rather small jurisdiction, implying that individuals presumably do indeed observe the refugee inflow (which is a prerequisite for this approach to work).

[3]A similar analysis as in Luttmer (2001), on the same type of data, is also conducted by Alesina et al. (2001).

European Social Survey conducted in 22 countries in 2002 and 2003 to study the relationship between attitudes towards immigrants, attitudes towards the welfare state and respondents' perception of immigrant shares (measured as deviations from the national average). Their estimations suggest that negative attitudes towards immigrants are associated with less support for the welfare state but that this correlation is unrelated to the perceived share of immigrants in the population. A third related study is that by Eger (2009), who uses survey data collected by Swedish sociologists and regresses three repeated cross sections from the first half of the 2000s of survey-stated preferences for social welfare expenditures on immigrant shares in Swedish counties, concluding that ethnic heterogeneity has a negative effect. It should however be noted that, since there are only 20 Swedish counties, the aggregation to county-level data poses problems for inference.

As with our study, the aforementioned examples all have access to individual survey data, making it possible to isolate the direct effects on preferences for redistribution of ethnic diversity.[4] However, although existing research reveals interesting relations, the evidence is best described as descriptive rather than causal.[5] To be able to draw causal inference from estimated relations, it is required that the identifying variation is not systematically related to the outcome of interest. There are two main reasons why this exogeneity requirement is unlikely to be fulfilled in earlier studies and why we believe that our empirical approach offers an improvement to existing work.

First, regressing preferences for redistribution on the share of immigrants in a jurisdiction (or on the share of some ethnic group's welfare dependency as in Luttmer (2001)) may capture reverse causality, as it is possible that certain groups of people sort into neighborhoods based on inhabitants' preferences for redistribution. We solve this problem by only using variation in immigrant

[4]In studies that use an aggregate welfare measure as the dependent variable, such as total welfare spending per capita (see, for example, Hjerm (2009)), it is not possible to separate the direct effect that works through a change in preferences for redistribution from the policy-bundling effect that operates via political parties. The same goes for those studies that examine the effect of ethnic heterogeneity on (aggregate measures of) the size of the public sector; see for example Alesina et al. (1999) and Gerdes (2010).

[5]This is also acknowledged by some of the authors. For example, Luttmer (2001) notes that "caution with this causal interpretation remains in order" (p. 507).

shares stemming from what we argue was exogenous placement of refugees via the placement program.

Second, earlier estimates of in-group bias in preferences for redistribution are more likely to capture omitted factors affecting both the left-hand and the right-hand side variables. In Luttmer (2001), for example, a welfare-prone individual is more likely to live in a high welfare-recipiency area and is also likely to prefer higher levels of redistribution. Additionally, in Senik et al. (2009), who estimate the effect of perceptions on attitudes, there is an obvious possibility of some latent variable affecting both and thereby biasing their results. A clear advantage for us in this regard is that, while existing studies have used cross-sectional or repeated cross-sectional data on individual preferences, we are the first to have access to panel data, allowing us to control for all individual factors that are constant over time. In our context, where we match preferences to the refugee placement program, this means that factors affecting preferences that could also have affected the refugee placement do not pose any identification problems, as long as these are time-invariant factors (either on at the individual level or municipality level, as we only study preferences of the non-movers).

In combination with the individual and municipality fixed-effects analysis that our method entails, the placement program has an additional value besides inducing exogenous variation in immigrant shares, namely that it provides substantial within-municipality variation per se. Net of aggregate trends and municipality fixed effects, this is typically not true.

This is not the first study to exploit the exogenous variation that the refugee placement program generated. Two examples, each with a different angle from ours, are Dahlberg and Edmark (2008) and Edin et al. (2003). The former uses the placement program to isolate exogenous variation in neighboring municipalities' welfare benefit levels to test whether there is a "race-to-the-bottom" among local governments, whereas the latter uses the initially exogenous placement of individual refugees to study the effect of segregation on labor market outcomes. These two examples thus require two different identifying assumptions, namely that the placement was exogenous with respect to the receiving municipalities' politicians (Dahlberg and Ed-

mark) and that the placement was exogenous with respect to the refugees themselves (Edin et al.). For our case, however, we need the placement to be exogenous from the point of view of the receiving municipalities' population. We think that our context makes our case for identification, perhaps not more but at least as plausible.

We thus believe that our empirical approach allows us to convincingly answer how increased immigration causally affects preferences for redistribution. We find that increased immigrant shares, stemming from inflows of refugees to municipalities via the placement program, lead to less support for redistribution in the form of preferred social benefit levels. This reduction in support is especially pronounced for respondents with high income and wealth. We also establish that OLS estimators that do not properly deal with endogeneity problems are likely to yield positively biased (i.e., less negative) effects of ethnic heterogeneity on preferences for redistribution.

The paper is structured as follows: the next section describes Sweden's immigration experience around the turn of the century and the coinciding refugee placement program, focusing on whether it is likely to yield exogenous variation in the share of immigrants. Section 3 provides a more detailed description of the refugee and other municipal-level data as well as of the survey data from where information on individual preferences for redistribution is obtained. Section 4 specifies the empirical model that uses the refugee placement program to identify effects of increased immigrant shares, which are then estimated and presented in section 5. Included in the result section are also a set of placebo regressions, an investigation of how the overall effects interact with individual characteristics and a sensitivity analysis. Finally, the last section concludes.

2 Immigration and refugee placement

This section provides an overview of Sweden's experience with increased immigration during the last decades of the 20th century, a description of the refugee placement program that we use as an exogenous source of variation in the immigrant share in the municipalities, and a discussion about the

exogeneity of the program.

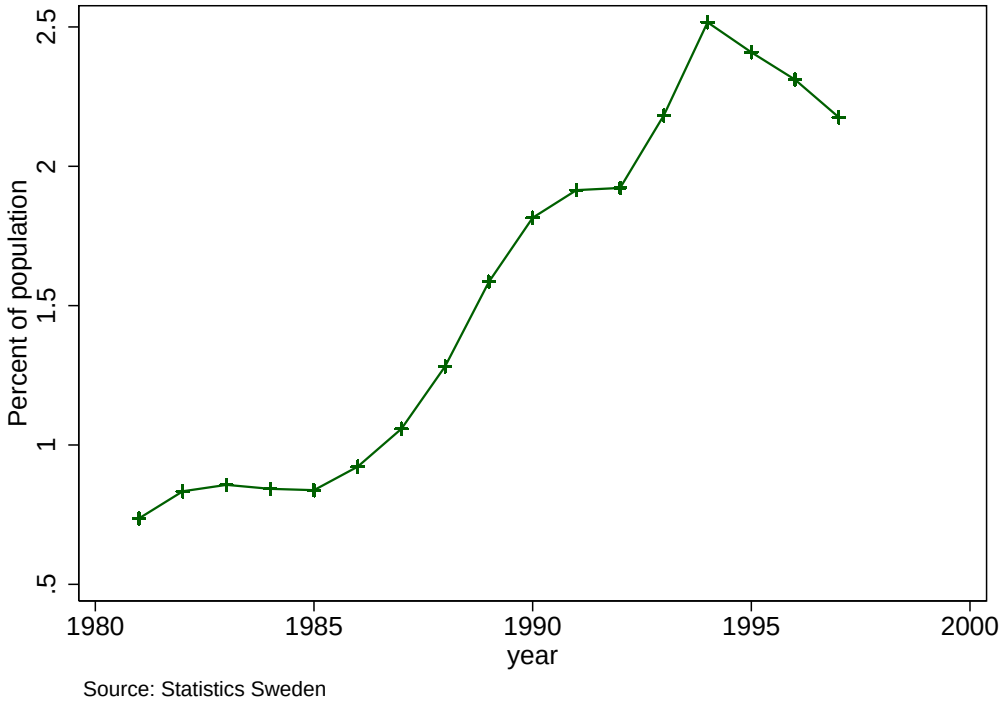
2.1 Immigration to Sweden

In the 1970s, the size of the population living in Sweden with a foreign citizenship was a rather stable five percent. The vast majority of these immigrants had arrived in Sweden in the 1950s and 1960s as labor migrants, primarily from the Nordic countries, with Finland as the prime example, but also from other European countries, such as Hungary. Over the next two decades, however, the situation completely changed, with more immigrants originating from other parts of the world and for political instead of economic reasons (refugees). Economic migration to Sweden more or less completely stopped during the 1970s. The evolution of immigration characterizing the 1980s and the 1990s is illustrated in Figure 1 (Figure 1 covers the years that will be used in the empirical analysis), from which it is clear that Sweden experienced a dramatic increase in the percentage in the population with citizenship from countries not member of the OECD (according to membership before 1994). Starting in 1981 from a mere 0.7 percent, it peaked at 2.5 in 1994—i.e., an amazing increase of 250 percent—before starting to trend back down.

To get a better sense of from what parts of the world the immigrants came from, Figure 2 shows the evolution over time but by region of origin rather than by OECD membership status. Three distinct features emerge: i) the share of Nordic citizenship has slowly declined over the period, which is most likely explained by Finns becoming Swedish citizens after having lived in Sweden for several years, ii) a large inflow of Asians, mainly from Iran and Iraq, from the mid-1980s and onward, and iii) a sharp increase in people from European countries other than the Nordic, explained by a significant influx of refugees from the Balkans in the early 1990s. In other words, the increasing share from non-OECD displayed in Figure 1 is primarily driven by inflows of refugees rather than by outflows of people from OECD countries. It is thus clear that Sweden has become a much more ethnically heterogeneous country, as people with a non-OECD citizenship are arguably more ethnically

different from native Swedes than OECD citizens. For the purpose of this paper, a suitable definition of immigrants is therefore the share of population with a non-OECD citizenship,[6] and—from an econometric point of view—it is promising to see such a large influx of non-OECD immigrants as revealed by Figure 1.

Figure 1: Share of population with non-OECD citizenship

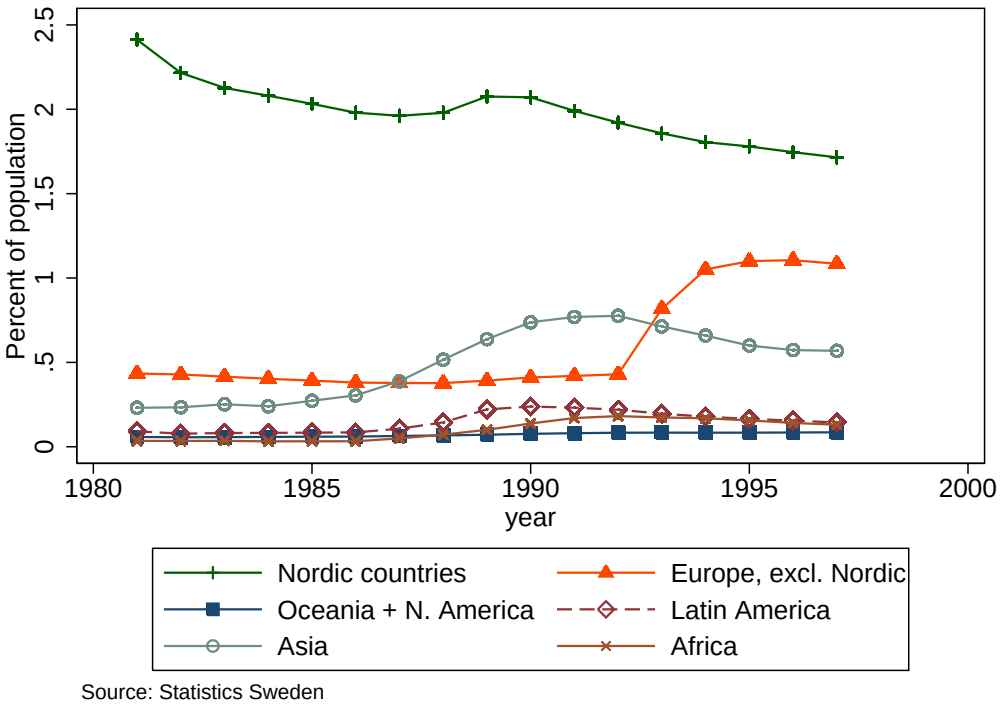


2.2 The refugee placement program

One purpose of the refugee placement program, which was in place between the beginning of 1985 and July 1^{st} 1994, was to achieve a more even distribution of refugees over the country, or more specifically, to break the concentration of immigrants to larger towns. Under the program, refugees arriving to

[6]Our precise definition of immigrants in the empirical analyses will be those with non-OECD citizenship according to OECD membership status before 1994 and those with Turkish citizenship. See, further, section 3.

Figure 2: Shares of population with foreign citizenship



Source: Statistics Sweden

Sweden were consequently not allowed to decide themselves where to settle but were assigned to a municipality through municipality-wise contracts, coordinated by the Immigration Board (the refugees were, however, allowed to move after the initial placement). At the start of the program, only a fraction of the municipalities were contracted, but as the number of refugees soared in the late 1980s and early 1990s, so did the number of receiving municipalities. By 1991, as many as 277 out of the then 286 Swedish municipalities had agreed to participate.

Via the Immigration Board, the central government compensated the municipalities for running expenses on their received refugees. The compensation was paid out gradually in the year of placement and in the three following years. After that period, the centrally financed compensation ended. In 1991, this system of transfers was replaced by one where the municipalities received a lump-sum grant for each refugee, paid out only in the year of placement but estimated to cover the expenses for about 3.5 years.

As indicated in Figures 1 and 2, the number of refugees arriving to Sweden increased dramatically during our period in focus. Between 1986 and 1991, on average over 19,000 refugees arrived each year, compared to an annual average of just below 5,000 during the previous four years. Additionally, during the last three years in our data, 1992–1994, the situation was even more exceptional, with an annual arrival of 35,000 (peaking in 1994 at 62,853), to a large extent driven by refugees from the Balkans.

This evolution is illustrated in Figure 3 along with an illustration of how the total inflow of refugees were distributed across small-sized (population$<$ 50,000), medium-sized (50,000$\geq$population$<$200,000) and large-sized (population$\geq$200,000) municipalities.[7] These time series are constructed using two slightly different data sources: for the years 1986–1994, the variable measures the number of refugees placed via the placement program and thus captures the gross inflow of refugees, whereas for the years 1982–1985, when the placement program had not yet started (apart from 1985), the variable instead captures the net increase in the sense that it measures the annual

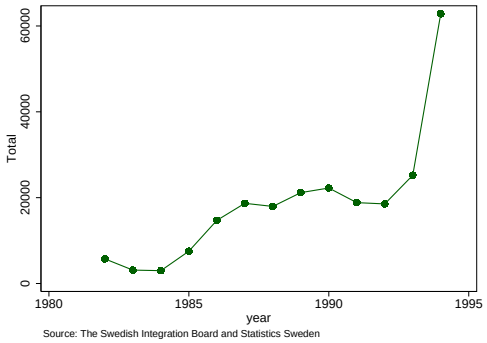
[7]In a given year, around 85 percent of the municipalities are categorized as small, whereas only Stockholm, Göteborg and Malmö are categorized as large (in all years).

change in the number of residences with a citizenship from typical refugee countries[8].

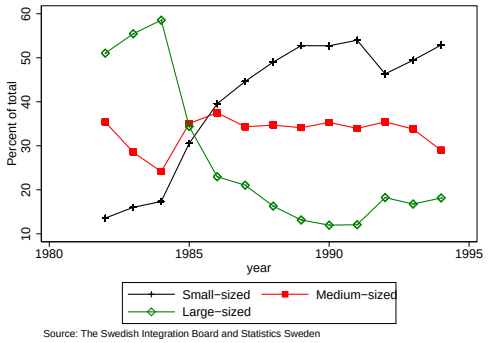
By inspection of the graph to the right in Figure 3, we learn several things. First, from the sharp trend break in 1985, it is clear that the program successfully fulfilled its purpose of breaking the segregation by redirecting refugees from large to smaller municipalities. Second, the graph reveals that the program yielded substantial variation in refugee placement over time within the three groups.[9] Both of these features are promising for our identification strategy. Third, not only did the program break the refugee settlement trend, it even reversed it. This illustrates not only that the placement program did not randomly allocate refugees to municipalities but also that the placement was correlated with a set of municipality characteristics, among them the size of municipalities. As will be further discussed in section 2.3, our identification strategy thus hinges on the exogeneity of refugee placement *conditional on* this set of municipality factors.

Figure 3: Annual increase/inflow of refugees

(a) Total increase/inflow



(b) Received share of total increase/inflow



[8]According to what statistics from the Swedish Migration Board (previously the Integration Board) say are typical refugee countries.

[9]As will be clear later on when we discuss the instrument, there is also substantial variation in treatment across municipalities within the three groups shown in Figure 3.

2.3 Exogeneity of the placement program

The differential refugee treatment across municipalities and over time seen in Figure 3 is closely related to the variation in immigrant shares, which we will use to identify causal effects of increased ethnic heterogeneity on changes in preferences for redistribution. The difference is that we exploit program-induced variation across *all* municipalities as opposed to variation only across the three groups according to population size. Therefore, our identifying assumption is that the placement of refugees was exogenous with respect to the inhabitants' of the municipalities preferences for redistribution. We claim that this assumption is indeed plausible. By construction, the placement program eliminates problems with the refugees themselves sorting into municipalities based on their characteristics (including the preferences of the inhabitants). We argue below that the placement can also be characterized as exogenous, conditional on a couple of observable municipal characteristics, with respect to the preferences of the municipalities' inhabitants.

The original idea of the placement program was to place refugees in municipalities with an advantageous labor market, education and housing situation and in municipalities that had previous experience with immigration. However, as the implementation of the program coincided not only with a dramatic increase in the number of refugees but also with a tightening of the housing market, housing availability seems to have become the more important factor.[10] Especially labor market but perhaps also the housing situation may matter for individual preferences for redistribution, in which case they will confound our analysis if not properly dealt with. Fortunately, with access to municipal-level data on both vacant housing and unemployment we are able to control for them in the regression analysis and thus use the conditional variation in refugee placement.

However, it is also important to recognize that the refugee placement

[10]This is according to Bengtsson (2002) and our own interviews with program officials. These claims are supported by various studies arguing that the high unemployment rates among immigrants from 1980 and onwards are partially due to the fact that housing, instead of factors such as labor market prospects, has determined the refugee placement (see, for example, Edin et al. (2003)).

was not forced on the municipalities and that they could have some say in whether they wished to sign a contract. For our empirical approach to work, it is thus required that the decision of the municipality to allow/accept refugees is not correlated with our outcome variable, changes in preferences for redistribution among the inhabitants.

A number of circumstances suggest this requirement to be fulfilled. First, as discussed in the previous section, the number of refugees arriving in Sweden increased dramatically during the period of study. This made it harder for the municipalities to dismiss the refugee placement proposal from the Immigration Board: the refugees had to be placed somewhere, and it became necessary that all municipalities shared the responsibility.[11] Second, refusals of refugee placement were in fact very rare,[12] and those that at first did refuse received a lot of negative publicity. Third, the panel structure of our data allows us to control for individual fixed effects, implying that it is okay for the refugee placement to be correlated with preferences in levels. We only require that the placement is exogenous with respect to individual *changes* in preferences, which arguably is much more likely to hold.[13]

Bengtsson (2002) and our own interviews with program placement officials confirm that most municipalities accepted the idea that all should participate in a manner of solidarity and that most municipalities did so, especially during the early years of the program. This furthermore created a peer pressure, which made it harder to refuse placement.[14]

We therefore claim that, conditional on the housing and perhaps also the labor market situation, the variation over time in immigrant shares within

[11] In 1988, the national authorities explicitly asked all municipalities to accommodate their share of refugees, that year corresponding to 0.28 percent of the population.

[12] Only 3 out of the 286 municipalities in our data did not receive any refugees at all via the program during 1986-1994.

[13] Correlation with the level of preferences could pose a problem in case of mean reversion. However, adding the respondent's initial preference levels to the regressions does not alter the results (results are available upon request), which suggests that this is not a problem in our case.

[14] This suggests that the variation in immigrant shares induced by the refugee placement program is more likely to be exogenous during the initial years of the program. We will, therefore, present results using data from the entire period 1986–94, as well as from only the initial period 1986–91.

municipalities induced by the placement program is exogenous to individual changes in preferences for redistribution. Still, to eliminate the risk of any remaining bias, we will, in addition to housing vacancies and unemployment, control for a set of municipal characteristics that may matter for preferences and that may have influenced the refugee placement. As the description above hinted, population size is one such characteristic, and section 4 discusses this and others in more detail. Additionally, in the empirical section, we will examine the plausibility of this claim by, for example, conducting placebo analyses.

3 Data

As explained in the introduction, we are fortunate to be able to match individual survey information to municipal-level data on refugee placement, immigrant shares and various other municipal covariates. In this section we discuss these two types of data sources, starting with the survey data.

3.1 Survey data

Survey data on individual preferences for redistribution is obtained from the Swedish National Election Studies Program[15]. The survey has been carried out every election year since the 1950s, and is in the form of a rotating panel, where each individual is surveyed twice and with half of the sample changing each wave. The survey contains information on political preferences and voting habits, as well as on several background characteristics of the respondent. This study uses information from waves 1982, 1985, 1988, 1991 and 1994, when roughly 3,700 individuals were surveyed each wave.[16] Based on

[15]See http://www.valforskning.pol.gu.se/ for more information. The survey data has partly been made available by the Swedish Social Science Data Service (SSD). The data was originally collected within a research project at the Department of Political Science, Göteborg University. The principal investigators were Sören Holmberg (in 1982) and Sören Holmberg and Mikael Gilljam (in 1985, 1988, 1991 and 1994). Neither SSD nor the principal investigators bear responsibility for the analyses in this paper.

[16]The vast majority were interviewed in their homes, whereas a few people who were "busy and difficult to get in touch with" were interviewed over the phone.

the panel feature of the survey, with these waves we construct three survey panels for the baseline analysis; 85/88, 88/91 and 91/94. For the placebo analysis we also construct a survey panel for the years 82/85. Each survey panel thus includes individuals who were surveyed in both of the two respective election years.

Our measure of individuals' preferences for redistribution is extracted from a survey question on whether the respondents were "in favor of decreasing the level of social benefits". The respondents were asked to rate this proposal according to the following 5-point scale:[17]

1. Very good
2. Fairly good
3. Does not matter much
4. Rather bad
5. Very bad

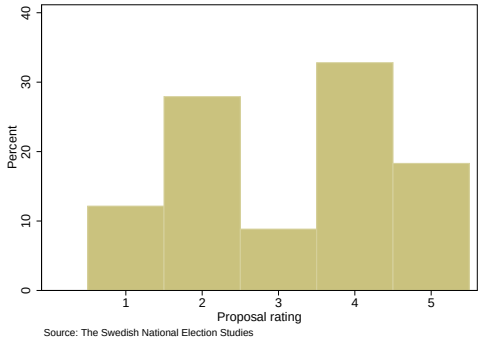
For each of the four surveys studied in the main analysis, Figure 4 displays the distribution of proposal ratings of the respondents included in our estimation sample. A few features stand out; for example, that few respondents in 1985 did not care much about the benefit levels and that the 1991 and 1994 distributions are very similar. Notable is also the smaller percentage who thought it was a very bad idea to decrease the level of social benefits in the two latest surveys, thus indicating a negative trend in the support for redistribution.

By taking the difference in response between the two survey waves (starting with the latter value), the proposal rating is used to construct a variable measuring the change in individual support for redistribution in the form of preferred social benefit levels. This means that individuals who become more positive to the proposal to decrease social benefits over time (i.e., move up in the preference ordering) are given a negative number, and vice versa.

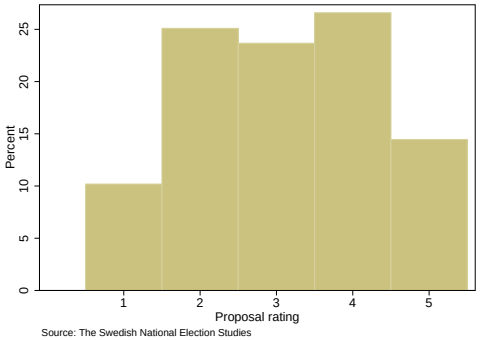
[17]The additional category "Do not know/Do not want to answer" is dropped from the analysis.

Figure 4: Proposal ratings by survey

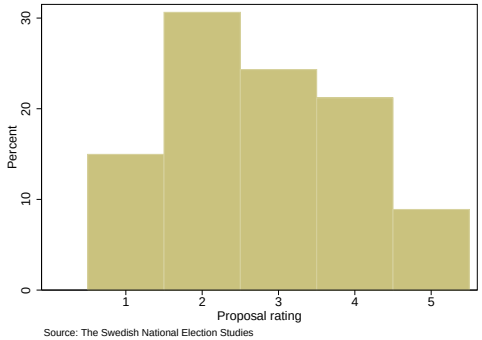
(a) 1985



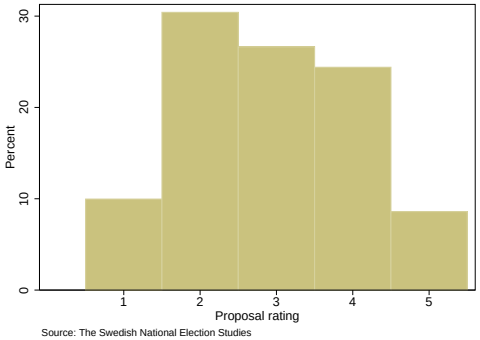
(b) 1988



(c) 1991



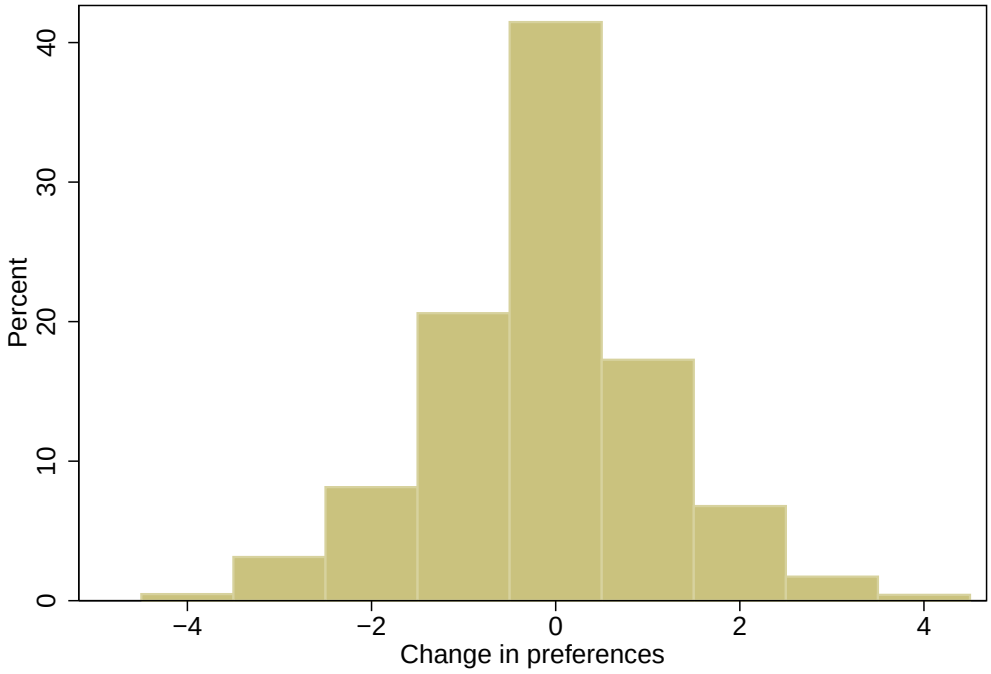
(d) 1994



A negative value for the change in preferences thus characterizes a situation where the support for social benefits decreases between two consecutive survey waves.

Figure 5 shows the distribution of this constructed variable measuring the change in preferences for redistribution in the form of social benefits. This will be the dependent variable in the empirical analysis. As can be seen in the figure, around 40 percent of the individuals in the sample do not change preferences between the survey waves. The distribution around zero is fairly symmetric, perhaps with a tilt towards the negative side. Very few individuals changed their ranking from "very good" to "very bad", or vice versa.

Figure 5: Change in preferences for social benefits between surveys



Source: The Swedish National Election Studies

3.2 Municipality data

To relate the changes in preferences between survey waves displayed in Figure 5 to the inflow of refugees during the corresponding period, we construct a variable for the cumulative number of refugees placed in each municipality during each election period (86–88, 89–91, 92–94), measured as a percentage of the average size of the population in the municipality during the respective election periods. Figure 6 shows how this variable is distributed over all municipalities and all three election periods. As is seen from the figure, the mass of the distribution is around or just below one percent; that is, during an election period of three years, most municipalities received refugees amounting to around one percent of the population. It is also relatively common with figures around two percent. The data contains one extreme value at 7.7 percent. This observation is excluded from the analysis (although it is not entirely unreasonable: the observation comes from a municipality with a small population, implying that relatively few refugees translate into a large percentage share.)[18]

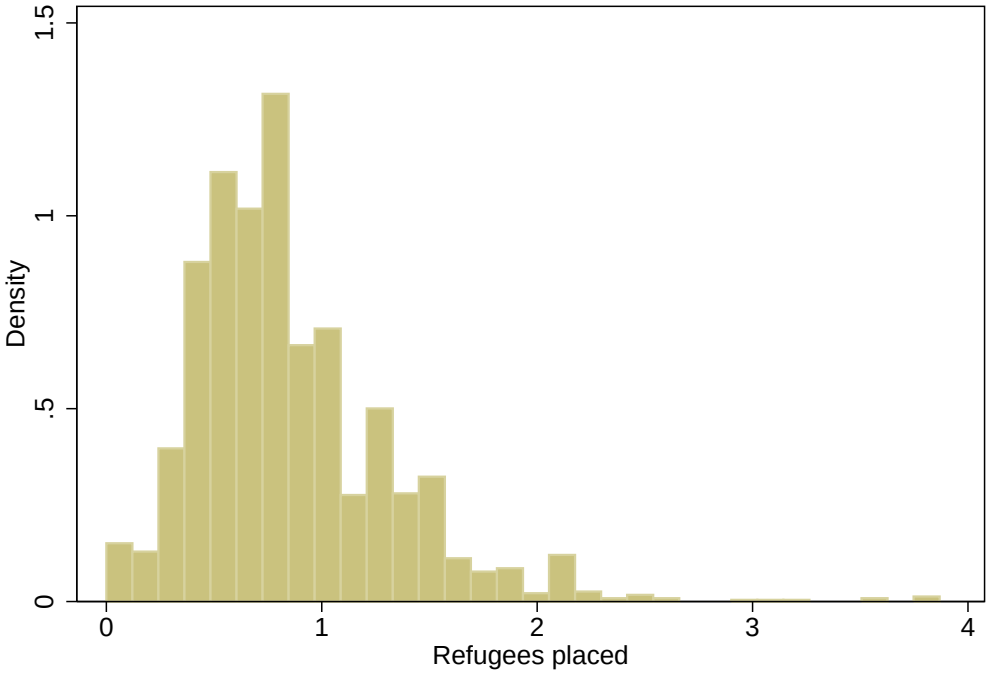
The refugee placement as displayed in Figure 6 will be used as an instrument to capture exogenous variation in the share of immigrants living in the municipality. As noted above, our working definition of immigrants is people with a non-OECD citizenship (according to membership status before 1994), or with a Turkish citizenship. With this definition, we hope to capture variation in ethnic background, as citizens from non-OECD countries are arguably more ethnically different from native Swedes than citizens from OECD countries are—except for maybe Turkey, which is probably the one OECD country whose citizens are ethnically least similar to native Swedes.[19,20] Note that with this definition, a person is an immigrant only until he or she obtains a Swedish citizenship, implying that negative changes in immigrant shares

[18]It can also be noted that the results do not change if we include it.

[19]The Turkish exception is also likely to be important for the analysis, as refugee migration to Sweden from Turkey was relatively frequent during the period under study.

[20]Apart from Sweden and Turkey, the OECD members before 1994 were Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Switzerland, the UK and the US.

Figure 6: Distribution of refugee placement between surveys



Source: The Swedish Integration Board

can stem either from individuals emigrating or from them obtaining Swedish citizenship.

Table 1 provides summary statistics of the immigration variable along with the other variables used in the empirical analysis. All variables defined as population shares are given in percentage. Because our identifying variation is within-municipality changes between two consecutive election/survey periods, the main variables are presented as such: the immigrant share *IM* (the independent variable of interest), the size of the refugee inflow defined as the share of total population *Refugee inflow* (the instrument used to isolate exogenous variation in immigrant shares) and preferences for redistribution in the form of preferred social benefit levels *PREF* (the outcome variable). Note that the variable *Refugee inflow* refers to refugees placed within the placement program—hence the minimum value of zero. The rest of the variables in the table, starting with *Welfare spending*, will be included as controls: see the following section.

4 Estimation method

To be able to identify whether a larger share of immigrants in a municipality causally affects the preferred level of redistribution among the municipality's population, we need to isolate the variation in the share of immigrants that is exogenous to preferences. That is, we require that our exploited variation in the change in immigrant shares is not systematically related to differences in the change in individuals' preferences for redistribution, neither directly via reverse causality nor indirectly via some omitted variable(s) affecting both preferences and the location choice of immigrants.

Because this exogeneity requirement is generally not fulfilled, OLS estimation of the relationship between immigrant shares and preferences will most likely fail to identify the causal effect. Although one can think of circumstances causing the OLS estimate to be biased in either direction, a positive bias seems more probable. It is, for example, likely that immigrant families with a typical high probability of welfare dependence prefer to live in municipalities whose population is more positive towards redistribution. It is

Table 1: Descriptive statistics; levels and changes between surveys

	mean	std.dev	min	max
IM	2.77	2.03	0.14	12.9
Refugee inflow	0.85	0.46	0	3.87
Δ IM	0.61	0.44	-1.47	3.26
Δ PREF	-0.10	1.24	-4	4
Control variables:				
Welfare spending	8.33	5.25	0	29.3
Vacant housing	1.85	2.63	0	19.0
Unemployment	3.54	2.69	0.19	11.7
Tax base	964.4	129.2	717.5	1738.7
Population	112.0	175.6	2.94	698.3
Population$<$50,000	0.51	0.50	0	1
Population$\geq$200,000	0.13	0.34	0	1
Socialist majority	0.40	0.49	0	1
Green party	0.78	0.42	0	1
New democrats	0.44	0.50	0	1

The number of observations is 1917

All variables in shares are given in percentage points

Tax base and Welfare spending are given in 100 SEK per capita, and Population is given in 1000s

The variables Population$<$50,000, Population$\geq$200,000, Socialist majority, Green party and New democrats are binary

Source: Statistic Sweden and the Swedish Integration Board

also likely that municipalities where preferences for redistribution are higher thanks to, for example, a more well-functioning welfare system in terms of assisting beneficiaries in becoming self-supported, attract more immigrants.

One way of attacking these types of biases is to only use the within-variation by differencing the variables (or, equivalently, including municipal fixed effects). There are, however, two major problems with such an approach: first, net of the aggregate trends, there is typically not enough variation in the population share of immigrants over time. Second, although differencing can reduce the bias, it will probably not eliminate it.

In contrast, this paper employs an IV-approach which exploits the within-variation in the share of immigrants induced by the refugee placement program. To the extent that the number of refugees that the program placed in different municipalities during the period between waves of the election survey is exogenous to the corresponding change in preferences for redistribution, this approach identifies the causal effect of an increased immigrant population on such preferences. To increase the likelihood that this is fulfilled, we will not only include measures of housing and local unemployment, which were suggested as important covariates to include according to section 2.3, but also include an additional set of local characteristics that could potentially have affected refugee placement while also being correlated with changes in preferences. We believe it likely that, conditional on the included covariates, the refugee placement was exogenous from the municipalities' (and thus from their population's) point of view, as well as from the refugees' point of view. Therefore, the variation induced by the program enables us to solve problems both with reverse causality and with unobserved factors simultaneously related to the share of immigrants and to preferences.

Motivated by the above considerations, the first and second stages of the 2SLS model are specified as follows (with $\widehat{\ }$ indicating predicted values from the first stage):

$$\Delta IM_{ms} = \alpha_1 Refugee\ inflow_{ms} + \alpha_2 \bar{H}_{ms} + \alpha_3 \Delta Z_{ms} + \alpha_4 SIZE_{ms} + \alpha_5 POL_{ms} + \alpha_6 SURVEY_s + \epsilon_{ms} \quad (1)$$

$$\Delta PREF_{ims} = \beta_1 \widehat{\Delta IM}_{ms} + \beta_2 \bar{H}_{ms} + \beta_3 \Delta Z_{ms} + \beta_4 SIZE_{ms} + \beta_5 POL_{ms} + \beta_6 SURVEY_s + \varepsilon_{ims} \quad (2)$$

Our instrument in the first-stage equation (1), $Refugee\ inflow_{ms}$, is defined as the total inflow of program refugees to municipality m between survey waves s and $s-1$, normalized by the average population size during the same period. The main parameter of interest in the second-stage equation (2) is β_1, representing the effect of a one percentage point change in the share of immigrants, ΔIM_{ms}, on the change in preferences for redistribution in the form of social benefits, $\Delta PREF_{ims}$ (for variable definitions, see section 3). Note that all differences are taken between survey waves s and $s-1$.

The municipal unemployment rate and the rate of vacant housing (in public housing/rental flats), which we believe affected the refugee placement are contained in the vector $\bar{H}_{ms}$, are both averaged over the panel periods. Because the *change* in unemployment rate but presumably not in the housing vacancy rate is likely to affect changes in preferences for redistribution, the former is also included in the Z_{ms} vector. This vector additionally contains per capita social welfare expenditures, per capita tax base and population size of the respondent's municipality. The reason for including per capita social welfare expenditures is to accommodate the possibility that these expenditures have changed between two consecutive elections (i.e., by conditioning on social welfare expenditures we make sure that a given change in preferences for redistribution do not simply reflect that a change in social welfare expenditures has occurred).

Equations (1) and (2) also include three sets of dummy variables. First, given the aims of the policy program and the pattern seen in Figure 3, we

allow the population size to be non-linear by also including an indicator for large-sized municipalities (population$\geq$ 200,000) and one for small-sized municipalities (population<50,000); these variables are contained in $SIZE_{ms}$. Second, we include a vector of political variables, POL_{ms}, to control for the possibility that the political views of certain parties might be correlated with both placement policy and preferences for redistribution. POL_{ms} therefore contains a dummy for a socialist majority in the municipal council (defined as the Social Democrats and Left Party together having at least 50 percent of seats), and two separate dummies for council representation by the Green Party and by the populist right-wing party "the New Democrats". Third, $SURVEY_s$ denotes survey panel fixed effects that capture nation-wide trends in changes in preferences between panels 85/88, 88/91 and 91/94.

Finally, ϵ and ε are error terms that we allow to be arbitrarily correlated within municipalities (i.e., when estimating the standard errors, we cluster the residuals at the municipality level).

5 Results

This section presents the results from estimating equations (1) and (2) on preferences for redistribution in terms of changes in preferred levels of social benefits.

5.1 Baseline results

Before turning to the IV-estimations, where we make use of the refugee placement program as an instrument for the share of immigrants living in the municipalities, we estimate equation (2) but with the actual share of immigrants instead of the predicted share, with OLS. The results, given in the first column of Table 2, show no evidence of an effect of the share of immigrants on individuals' preferred levels of social benefits. As discussed above, however, the OLS-estimator is likely to be biased. First, although the estimation equation controls for a set of municipal characteristics, time trends and, through first-differencing, individual fixed effects, it is still possible that unobserv-

ables correlated both with immigrants' choice of location and preferences for redistribution confound the estimates. Second, the estimated relation may reflect reverse causality—that is, we cannot rule out that immigrants' choice of residency is affected by the inhabitants' preferences for social benefits.

We therefore turn to see how the results change when we deal with these endogeneity problems by employing our instrument. Note that an instrument is valid only if it is exogenous as well as a strong predictor of the endogenous variable. We have already argued that the former criterion, exogeneity, is fulfilled, and we will examine it through placebo analyses in the next section. The latter criterion, the relevance of the instrument, can easily be tested by estimating the first stage in equation (1)—that is, by regressing the change over survey panels in the municipality's share of immigrants on the inflow of refugees as a share of the average population during the same period, including the full set of controls as motivated in section 4. The results, presented in the middle column of Table 2, show that the refugee placement explains roughly half of the variation in the change in the share of immigrants and that the effect is significant at the one-percent level (see the bottom of the table). We conclude that the correlation of our instrument (the program placement of refugees) with the share of immigrants is strong, even after conditioning on a set of municipal characteristics as well as survey fixed effects. With this reassuring first stage, we now turn our focus to the relation of interest in equation (2).

The results from estimating equation (2) using the program placement of refugees as an instrument for the immigrant share in the municipality are given in the rightmost column of Table 2. In contrast to the insignificant coefficients that were obtained in the first column with OLS, this column reveals a negative and statistically significant coefficient for the effect of changing the share of immigrants in the municipality on preferred levels of social benefits. An increase in the share of immigrants is hence estimated to reduce the support for redistribution, as measured by the preferences for social benefits. The size of the effect implicates that a one percentage point increase in the immigrant share in the municipality makes the average individual move up roughly 1/3 of a point in the preference ordering for social benefits (which

Table 2: Baseline results

	OLS	IV 1^{st} stage	IV 2^{nd} stage
Δ IM	-0.0438		-0.347**
	(0.0675)		(0.155)
Δ Welfare spending	-0.0200	0.00912	-0.00768
	(0.0138)	(0.0105)	(0.0147)
Vacant housing	0.00315	-0.000486	0.00967
	(0.0140)	(0.00759)	(0.0144)
Unemployment	-0.0354	-0.0292	-0.0482
	(0.0336)	(0.0229)	(0.0343)
Δ Unemployment	0.0255	0.0102	0.0320
	(0.0424)	(0.0309)	(0.0416)
Δ Tax base	-0.00171*	-0.000564	-0.00183*
	(0.00101)	(0.000782)	(0.00101)
Δ Population	-0.00431	-0.0175**	-0.00919
	(0.00807)	(0.00758)	(0.00841)
Population≥200,000	-0.0282	-0.0737*	-0.0494
	(0.0602)	(0.0437)	(0.0633)
Population<50,000	0.0966	0.414***	0.222
	(0.138)	(0.0963)	(0.144)
Socialist majority	0.0683	0.0392	0.0952
	(0.0668)	(0.0441)	(0.0714)
Green party	0.0935	0.00972	0.0910
	(0.0829)	(0.0387)	(0.0822)
New democrats	0.0498	0.0574	0.0630
	(0.0778)	(0.0564)	(0.0770)
Panel 88/91	-0.417***	0.181**	-0.342***
	(0.121)	(0.0848)	(0.132)
Panel 91/94	0.0634	-0.218	0.0393
	(0.300)	(0.207)	(0.301)
Refugee inflow		0.497***	
		(0.0616)	
Constant	0.140	0.258**	0.303
	(0.187)	(0.127)	(0.197)
R^2	0.026	0.410	0.017
Observations	1917	1917	1917

Standard errors clustered on municipality in parentheses
The dependent variable is Δ PREF in columns 1 and 3, and Δ IM in column 2
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

was given on page 15). Considering that preferences are measured along a 5-point scale, this is a considerably large effect.

It is interesting to note that the presumption that OLS would be positively biased is verified; compared to the more convincing IV-strategy, OLS estimation yields, in addition to statistical insignificance, coefficients much closer to zero.

5.2 Placebo analyses

We have argued that, conditional on municipal characteristics such as housing vacancies and the unemployment rate, our instrument is exogenous with respect to changes in preferences for redistribution among the municipalities' inhabitants. To ascertain the validity of this claim and to give more credibility to the causal interpretation of the results in the previous section, we conduct two types of placebo analyses in this section; the first analysis is related to placebo in treatment, the other to placebo in outcome.

Regarding placebo in treatment, we will run a placebo regression to test for a correlation between refugee placement and pre-placement trends in preferences for redistribution. If our assumption that the refugee placement was exogenous with respect to changes in preferences for redistribution holds, then we expect no correlation between the pre-placement preference trends and the subsequent refugee placement. Therefore, we run a regression equation of pre-placement preference trends, measured as the change in preferences for redistribution between 1982 and 1985 (i.e., the panel period preceding the placement program), on changes in immigration as predicted by the refugee placement in the three subsequent panel periods. The regression includes the same set of covariates, measured for the period 1982–85, as the baseline regressions in equations (1) and (2).[21,22]

The first three columns in Table 3 show the first-stage estimates of the instruments (refugee placement during the three panel periods) for each of the

[21]For the sake of brevity, the covariates are suppressed in the tables for the remaining analyses.

[22]Housing vacancies are not available until 1985. We therefore use the 1985 value as a proxy for average vacant housing in years 1983–85.

three endogenous variables (the change in immigrant shares over the three respective periods), and the fourth column shows the second-stage placebo regression result. Note first from columns 1–3 that refugee placement is strongly correlated with the change in immigrant shares during each corresponding panel period. However, importantly, as can be seen from column 4 in the table, the instrumented changes in immigrants are non-significantly related to the pre-placement preference-trends in all periods, and a test of joint significance for all three periods yields a p-value of 0.76. This strengthens our assumption that the refugee placement was exogenous conditional on the included covariates.[23]

Regarding placebo in outcome, we will estimate the model in (1) and (2), but on preferences for issues that ought to be unrelated to the size of the immigrant population; preferences for private health care and for nuclear power. Accordingly, the respondent's rate of the proposals (on the same 5-point scale as for redistribution) a) to increase privatization of health care and b) to keep nuclear power as an energy source are used to construct measures of changes in preferences equivalent to those for redistribution. Because the respondents now were asked whether these things should increase rather than decrease, we multiply these changes with -1 to maintain the interpretation that a negative sign means reduced support.

The resulting placebo estimates of β_1, which are obtained from the same set of respondents as in the original sample with three panel periods, are found in Table 4.[24] As expected, no effects of increased immigrant shares are found neither on attitudes towards privatizing health care, nor towards nuclear power. This strengthens the notion that the estimated effects on preferences for redistribution indeed have a causal economic interpretation.

[23]It can be noted that the coefficient in the last panel period, 91/94, is closer to being significant than the former periods. This is also the period when we expect the placement program to be less strictly enforced (see section 5.4). As is reported in Table 7, the negative effect of refugee placement on preferences for redistribution is however present also if we exclude period 91/94 from the analysis.

[24]Note that the first-stage placebo estimates are identical to those in Table 2.

Table 3: IV-regressions of ΔPREF in 82/85 on placebo treatments

	Δ IM 85/88	Δ IM 88/91	Δ IM 91/94	Δ PREF 82/85
Refugee inflow 85/88	0.483***	0.0151	-0.491***	
	(0.0781)	(0.119)	(0.132)	
Refugee inflow 88/91	-0.0691	0.508***	-0.477***	
	(0.0716)	(0.0989)	(0.165)	
Refugee inflow 91/94	0.105***	-0.0188	0.943***	
	(0.0388)	(0.0503)	(0.0938)	
Δ IM 85/88				0.330
				(0.541)
Δ IM 88/91				-0.0492
				(0.546)
Δ IM 91/94				-0.152
				(0.164)
R^2	0.456	0.620	0.595	0.021
F-statistic	27.97	12.79	43.09	
χ^2-statistic				1.168
Municipal covariates	yes	yes	yes	yes
Panel effects	yes	yes	yes	yes
Observations	759	759	759	759

Standard errors clustered on municipality in parentheses

The reported F-statistics correspond to a joint test of the three excluded instruments in the first-stage regressions (columns 1–3), and the reported χ^2-statistic corresponds to a joint test of the three placebo treatments in the second-stage regression (column 4)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: IV-regressions of ΔPREF for placebo outcomes

	Private health care	Nuclear power
Δ IM	-0.0438	-0.0211
	(0.132)	(0.125)
R^2	0.157	0.054
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1917	1917

Standard errors clustered on municipality in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3 Do responses vary with individual characteristics?

According to the coefficients in Table 2, the causal effect of a one percentage-point increase in immigrant shares is that the support for redistribution is reduced by 1/3 in the 5-point preference ordering. This result pertains to the "average respondent", but it is of course possible that the effect varies depending on individual characteristics. For example, it could be that respondents who are large contributors to the redistribution scheme are more sensitive to the ethnic diversity of the recipients, compared to respondents who are themselves more likely to be net receivers in the redistribution scheme.

To investigate this, we use three questions contained in the survey to categorize the respondents as being a likely net contributor or receiver; questions on individual income (y), individual wealth (w) and worker type (blue-/white-collar). For individual income. the question is in which one of five intervals their previous year's income belongs. With this information we construct three dummy variables indicating whether the individual belongs a) to the lowest of five income classes (which is around 15 percent of individuals; we thus call this variable y <p15); b) to the two lowest of five income classes (y <p40); and c) to the highest of five income classes (y >p80).

The question on individual wealth is posed identically, so we proceed in the same way with this information. That is, we construct three additional dummy variables indicating whether the individual belongs a) to the lowest of five wealth classes (containing the 40 percent of respondents that have

zero wealth, w <p40); b) to the two lowest of five wealth classes (w <p60); and c) to the top wealth class (w >p85).

The third question, on type of worker, asks the respondent to categorize himself/herself as either blue-collar, white-collar, self-employed or a farmer. From this information we construct a) a dummy that equals one if the respondent states blue-collar and zero otherwise and b) a dummy that equals one if the respondent states white-collar and zero otherwise.

To see how the effect of increased immigrant shares on preferences for redistribution differs across these individual characteristics, we then run the model in (1) and (2) three times in the income dimension, three times in the wealth dimension and twice for worker type, each time interacting the variables ΔIM and $Refugeeinflow$ with one of the class/worker type dummies. The resulting second-stage IV estimates are displayed in Table 5 for income (left column) and wealth (right column), and in Table 6 for worker type.[25]

Looking first at Table 5 showing how effects vary over the income and wealth dimension, it is clear that respondents in the top percentiles express the largest reduction in support for redistribution as the population becomes more ethnically heterogeneous. The negative effect of a one percentage-point increase in immigrant shares is 0.8 larger among the top 20^{th} income percentiles compared to the rest, and the corresponding figure for the top 15^{th} wealth percentiles is as large as 1.3. On the contrary, respondents in the two lowest income and wealth groups do not change their preferences for social benefits as the immigrant share increases (the sums of the coefficients in the two top panels in the two respective columns are not significantly different from zero).

We finally estimate how effects vary with worker type, and the results from this are found in Table 6. These estimates are in line with those found above; we see the negative effect on preferred levels of social benefits for white-collar workers (who presumably are also the high-income earners). Overall, these sets of results clearly reveal that the respondents who

[25]Note that the interaction terms of ΔIM and the class/worker type indicators are also endogenous. We therefore use as additional instruments the interaction of $Refugeeinflow$ and the respective indicators. As can be seen from Tables 8–10 in the Appendix, all instruments are strong and the joint F-tests are within conventional significance levels.

Table 5: Differential effects for income groups y <p15, y <p40, y >p80 and wealth groups w <p40, w <p60, w >p85

	ΔPREF	ΔPREF
Δ IM	-0.337**	-0.717***
	(0.170)	(0.243)
Δ IM*($y <$ p15)	-0.198	
	(0.575)	
Δ IM*($w <$ p40)		0.936**
		(0.409)
Δ IM	-0.422**	-0.758***
	(0.192)	(0.290)
Δ IM*($y <$ p40)	0.239	
	(0.370)	
Δ IM*($w <$ p60)		0.686*
		(0.380)
Δ IM	-0.114	-0.225
	(0.182)	(0.168)
Δ IM*($y >$ p80)	-0.804**	
	(0.380)	
Δ IM*($w >$ p85)		-1.253**
		(0.610)
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1917	1917

Standard errors clustered on municipality in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Differential effects for blue-collar and white-collar workers

	ΔPREF
Δ IM	-0.721***
	(0.253)
Δ IM*Blue-collar	0.660**
	(0.337)
Δ IM	-0.0934
	(0.202)
Δ IM*White-collar	-0.804**
	(0.374)
Municipal covariates	yes
Panel effects	yes
Observations	1899

Standard errors clustered on municipality in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

contribute more extensively to the redistribution scheme are those whose support for redistribution is reduced as the group of likely recipients become more ethnically diverse.[26]

5.4 Sensitivity analysis

We claim above that the refugee placement program generates exogenous variation in immigrant shares across municipalities, and this of course needs to be true if the results can be given a causal interpretation. However, we cannot claim that our research design corresponds to a perfectly controlled experiment. For example, whereas the program dictated where newly arrived refugees were to settle initially, it could not force them to stay there indefinitely. If many refugees ended up in a different municipality than where

[26]We have also studied interactions with numerous other individual characteristics, such as gender, whether the respondent is publicly employed, whether the initial support for redistribution was high or low and whether the respondent's private economic situation has improved over the past 2–3 years. However, none of these interactions was statistically significantly different from zero.

they were initially placed, our instrument measuring the number of refugees placed in the municipality within the program would be poorly defined.

Dahlberg and Edmark (2008) investigate the extent of refugee migration and come to the conclusion that around 40 percent indeed lived in a different municipality than where they were initially placed four years later, and of these the vast majority had moved to one of the three large cities (Stockholm, Göteborg and Malmö) and their surrounding areas. As a robustness check of the baseline results presented in Table 2, we therefore estimate the model while excluding the 250 respondents living in these three municipalities. If anything, we would expect effects to be smaller among the respondents from the remaining municipalities where the true increase in immigrants perhaps was smaller than what is being measured. However, the results presented in the first and second columns of Table 7 show no evidence of a reduction in estimates—both the first- and second-stage estimates are reassuringly the same as when estimating on the full sample.

Another aspect with the placement program that differs from most randomized experiments is that it lasted for as long as ten years. It is therefore likely that it functioned somewhat differently in the beginning than in the end. Specifically, Bengtsson (2002) reports that more municipalities willingly participated during the initial years. This suggests that the variation in immigrant shares induced by the refugee placement program is more likely to be exogenous in the earlier time periods than towards the end, when participating municipalities comprise a more selected sample. To investigate this we therefore exclude the last survey panel (covering years 1991–1994) from the estimation sample. Recalling the above discussion of likely directions of the bias of the OLS estimator, if the placement program was "more exogenous" early on, we thus expect the estimate on this limited sample to differ even more from the OLS estimate than the baseline IV estimate in Table 2. In other words, if anything, we expect a more pronounced negative effect in the early period.

Columns three and four of Table 7 present these estimates and indeed confirm our priors. In particular, the second-stage estimate increases in an absolute sense and is now essentially as large as -1. That is, increases in

Table 7: Sensitivity analysis

	Big cities excluded		91/94 excluded	
	IV 1^{st} stage	IV 2^{nd} stage	IV 1^{st} stage	IV 2^{nd} stage
Refugee inflow	0.550***		0.310***	
	(0.0530)		(0.0621)	
Δ IM		-0.389**		-0.958**
		(0.151)		(0.380)
R^2	0.370	0.017	0.537	-0.013
Municipal covariates	yes	yes	yes	yes
Panel effects	yes	yes	yes	yes
Observations	1667	1667	1335	1335

Standard errors clustered on municipality in parentheses

The dependent variable is ΔIM in columns 1 and 3, and ΔPREF in columns 3 and 4

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

immigrant shares of one percentage point during the periods 1985–1988 and 1988–1991 caused the support for redistribution in the form of preferred levels of social benefits to decrease with an amount corresponding to a full step along the 5-point rating scale. If this estimate can be interpreted causally with higher confidence, it thus means that increased ethnic heterogeneity has a very large, negative effect on preferences for redistribution. It also means that the above estimated effects on the full sample should be viewed as lower bounds. There is, however, no reason to doubt the overall pattern of effects across different individual characteristics from section 5.3. Unfortunately, the 30 percent drop in the number of observations resulting from excluding the later survey panel leaves a too small sample to study interaction effects with any reasonable precision.

6 Conclusions

In this paper, we have examined whether an increased ethnic heterogeneity in society affects natives' preferences for redistribution. We use data from Sweden, a country that has experienced a dramatic increase since the 1970s in the share of the population originating from a non-OECD country. By combining two data sources covering the period 1985-1994, we improve upon the earlier literature on in-group bias and argue that we are able to estimate causal effects. The first data source includes information on a nation-wide policy intervention program that exogenously placed refugees coming to Sweden between 1985 and 1994 among the Swedish municipalities. We use this policy intervention as an instrument for the municipalities' share of immigrants (defined as the share of non-OECD citizens). The second data source is individual panel survey data, which is matched on the respondent's municipality of residence to the first data and in which each respondent in two consecutive elections is asked questions about, among many other things, his or her preferences for redistribution (specifically, his or her preferred level of social benefits). By exploiting the exogenous source of variation in immigrants shares in the municipalities induced by the refugee placement program between two consecutive elections, we are able to causally estimate the effect

of increased ethnic heterogeneity on the individuals' change in preferences for redistribution between the two elections.

We have found that an increasing share of immigrants leads to lower preferred levels of social benefits. This negative effect on preferences for redistribution is especially pronounced for individuals in the upper tail of the income and wealth distributions. Placebo analyses support a causal interpretation of the obtained results. Sensitivity analyses with different alterations of the baseline model (such as a shorter time period in which the policy intervention was arguably more exogenous and an exclusion of the three large cities from the estimation sample to avoid potential problems with migration of refugees within Sweden after the initial placement) also support the validity of the empirical approach. The conclusion is thus that people exhibit in-group bias in the sense that native Swedes become less altruistic when the share of non-OECD citizens increases.

Comparing OLS and IV estimates reveals that the OLS estimates are upward biased, implying that OLS yield less negative estimates of increased ethnic heterogeneity on natives' preferences for redistribution. Because it is quite likely that this result can be generalized to other contexts, results in previous studies—such as in Luttmer (2001)—may be interpreted as lower bounds of the true effects.

This paper has shed further light on the direct effect on natives' preferences for redistribution of an increased ethnic diversity, following the theoretical argument as laid out in, e.g., Shayo (2009). How the changing preferences translate into actual redistribution policies is however an open question. It also remains to be explained to what extent increased ethnic heterogeneity can explain the increased support for anti-immigrant parties seen in many countries (including Sweden), which via policy-bundling can lead to less redistribution. To get a more complete picture on how overall redistribution is affected by an increased ethnic heterogeneity, an interesting task for future research is to tease out the relative importance of the direct and the indirect channels.

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A First-stage estimates

This section presents first-stage estimates corresponding to tables 5–6 in section 5.3.

Table 8: First-stage estimates; y <p15, y <p40, y >p80

	Δ IM	Δ IM*I(y< (>) p#)
Refugee inflow	0.490***	0.0162***
	(0.0618)	(0.00587)
Refugee inflow*($y <$ p15)	0.0962	0.432***
	(0.101)	(0.123)
F-statistic	34.53	9.601
Refugee inflow	0.497***	0.0385***
	(0.0663)	(0.0142)
Refugee inflow*($y <$ p40)	-0.000711	0.378***
	(0.0672)	(0.0704)
F-statistic	32.61	17.26
Refugee inflow	0.513***	0.0410***
	(0.0588)	(0.0156)
Refugee inflow*($y >$ p80)	-0.0533	0.339***
	(0.0623)	(0.0868)
F-statistic	38.50	10.11
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1917	1917

Standard errors clustered on municipality in parentheses

The reported F-statistics correspond to a joint test of the two excluded instruments

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: First-stage estimates; w <p40, w <p60, w >p85

	Δ IM	Δ IM*I(w< (>) p#)
Refugee inflow	0.474***	0.0429**
	(0.0683)	(0.0173)
Refugee inflow*($w <$ p40)	0.0594	0.401***
	(0.0628)	(0.0729)
F-statistic	34.84	22.48
Refugee inflow	0.476***	0.0555**
	(0.0675)	(0.0236)
Refugee inflow*($w <$ p60)	0.0352	0.398***
	(0.0456)	(0.0634)
F-statistic	32.74	28.46
Refugee inflow	0.498***	0.0145**
	(0.0642)	(0.00646)
Refugee inflow*($w >$ p85)	-0.0148	0.384***
	(0.0773)	(0.0861)
F-statistic	35.19	11.36
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1917	1917

Standard errors clustered on municipality in parentheses

The reported F-statistics correspond to a joint test of the two excluded instruments

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: First-stage estimates; blue-collar and white-collar workers

	Δ IM	Δ IM*X-collar
Refugee inflow	0.465***	0.0810***
	(0.0668)	(0.0207)
Refugee inflow*Blue-collar	0.0354	0.384***
	(0.0528)	(0.0600)
F-statistic	33.74	35.43
Refugee inflow	0.492***	0.0253
	(0.0589)	(0.0189)
Refugee inflow*White-collar	-0.0262	0.342***
	(0.0579)	(0.0753)
F-statistic	35.27	11.18
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1899	1899

Standard errors clustered on municipality in parentheses

The reported F-statistics correspond to a joint test of the two excluded instruments

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: First-stage estimates; blue-collar and white-collar workers

	Δ IM	Δ IM*X-collar
Refugee inflow	0.465***	0.0810***
	(0.0668)	(0.0207)
Refugee inflow*Blue-collar	0.0354	0.384***
	(0.0528)	(0.0600)
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Refugee inflow*White-collar	-0.0262	0.342***
	(0.0579)	(0.0753)
F-statistic	35.27	11.18
Municipal covariates	yes	yes
Panel effects	yes	yes
Observations	1899	1899

Standard errors clustered on municipality in parentheses

The reported F-statistics correspond to a joint test of the two excluded instruments

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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