Ethnic Diversity and Preferences for Redistribution*

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Abstract

During past decades, immigration of workers and refugees to Europe has increased substantially, and the composition of the population in many countries has consequently become much more heterogeneous in terms of ethnic background. If people exhibit in-group bias in the sense of being more altruistic to one's own kind, such increased heterogeneity will lead to reduced support for redistribution among natives. This paper exploits a nation-wide program placing refugees in municipalities throughout Sweden during 1985–94 to isolate exogenous variation in immigrant shares. We match data on refugee placement to survey data on inhabitants' of the receiving municipalities to estimate causal effects of increased immigrant shares on preferences for redistribution. The results show that a larger immigrant population leads to less support for redistribution in the form of preferred levels of social benefits. 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—as in earlier studies—are likely to yield positively biased effects of ethnic heterogeneity on preferences for redistribution.

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1 Introduction

During past decades, immigration of workers and refugees to the European countries has increased substantially. Immigrants are, obviously, different

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in terms of ethnic background compared to "the average native" and, more generally, 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 to its more limited size of welfare state. 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 towards 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 causal effects of increased immigrant shares by making use of a nearly nation-wide program intervention placing refugees in municipalities throughout Sweden between 1985 and 1994. For each of the years when the placement program was in effect, it 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 thus estimate 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

[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.

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

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, where each individual is 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 over a 20-year period, 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. Senik et al. (2008) use information from the European Social Survey conducted in 22 countries in 2002 and 2003 to study the relation 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 Eger (2009), who regresses repeated cross sections of survey-stated preferences for social welfare expenditures on immigrant shares in Swedish counties, and concludes 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.[3]

Like our study, the aforementioned examples all have access to individual survey data, making it is possible to isolate the direct effects on preferences of ethnic diversity rather than the composite effect that operates via political parties. However, although existing research reveals interesting relations, the evidence is best described as descriptive rather than causal.[4] 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.

[3]See also, e.g., Alesina et al., 2001 and Hjerm, 2009

[4]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).

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 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 variable. 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. And in Senik et al. (2008), 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 individual level or municipality level.[5]

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 the case.

This is not the first study to exploit the "natural experiment" that the refugee placement program generated. Two examples, each with a different angle than 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 hence require two different identifying assumptions, namely that the placement was exogenous with respect to the receiving municipalities' politicians (Dahlberg and Edmark), 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, if not more but, at least as plausible.

[5]Note that we only study preferences of the non-movers.

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 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

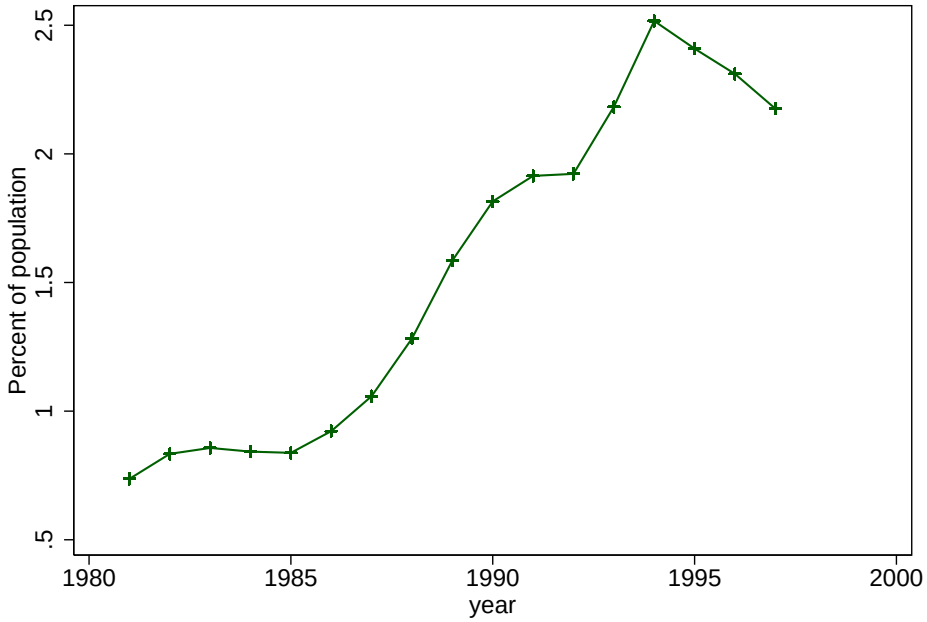
This section provides an overview of Sweden's experience with increased immigration around the turn of the century, and of the refugee placement program that provides a source of exogenous variation in the share of immigrants in Swedish municipalities.

In the 1970s, the size of the population living in Sweden with a foreign citizenship was a rather stable five percent. Over the next two decades, however, the situation completely changed. The evolution of immigration characterizing the 80s and the 90s is illustrated in Figure 1, from where 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 in 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.

So where did all these people come from? Figure 2 shows the same evolution, but by region of origin rather than by OECD membership status. Three distinct features emerge: i) the share with Nordic citizenship has slowly declined over the period, which is most likely explained by Finns becoming Swedish citizens after having lived there several years, ii) a large inflow of Asians, mainly from Iran and Iraq, from the mid 80s and onwards,

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 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. In the empirical analysis to follow, we will therefore define immigrant shares as the share of population with a non-OECD citizenship (the variable seen in Figure 1).

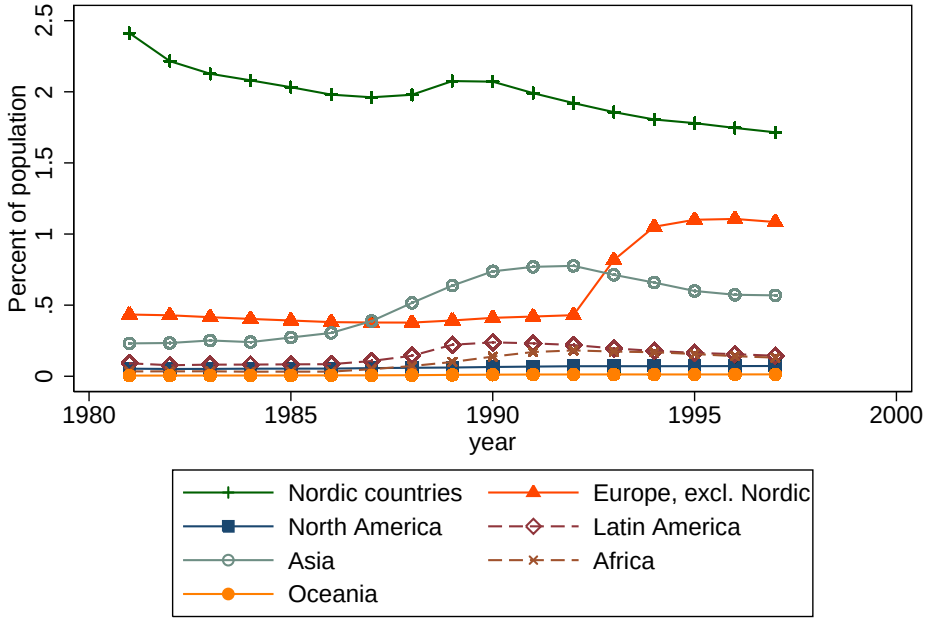
Figure 1: Share of population with non-OECD citizenship



2.1 Description of the refugee placement program

One purpose of the refugee placement program, which was in place between 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 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

Figure 2: Shares of population with foreign citizenship



Source: Statistics Sweden

agreed to participate.

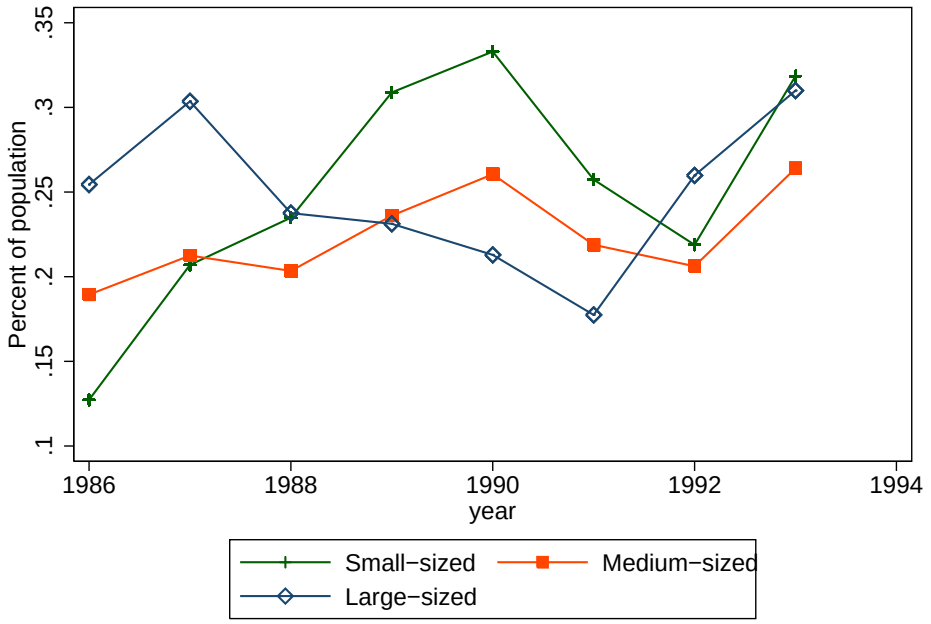
Via the Immigration Board, the central government compensated the municipalities for running expenses on their received refugees. The compensation was paid out gradually in the year of placement and in the three following years. After that period, the centrally financed compensation ended. In 1991, this 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.

Figure 3 illustrates the size of the refugee inflow via the program, in terms of percentages of population, separately for small-sized (population<50,000), medium-sized (50,000≥population<200,000) and large-sized (population≥ 200,000) municipalities.[6] It is this variation in refugees induced by the placement program that we will exploit to identify causal effects of increased immigrant shares on changes in preferences for redistribution. Thus, in order to achieve identification, we require that the placement was exogenous with respect to the inhabitants of the municipalities and, specifically, their attitudes towards redistribution. We claim that this requirement indeed is fulfilled. By construction, the placement program eliminates problems with the refugees themselves sorting into municipalities based on their character-

[6]In a given year, around 80 percent of the municipalities are categorized as small, whereas only Stockholm, Göteborg and Malmö are categorized as large (in all years).

istics (including the preferences of the inhabitants). We argue below that the placement can also be characterized as exogenous from the point of view of the preferences of the municipalities' inhabitants.

Figure 3: Refugees to municipalities with different population size



Source: the Swedish Integration Board

The original idea with the placement program was to place refugees in municipalities with an advantageous labour market, education and housing situation, and in municipalities that had previous experience of 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.[7] Especially labor market but perhaps also the housing situation may matter for individual preferences, 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, hence, use the conditional variation in refugee placement.

It is however also important to recognize that the refugee placement was

[7]Bengtsson (2002) and 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 been determining the refugee placement (see for example Edin et al., 2003).

not forced on the municipalities, but that they had some say in whether or not they wished to cooperate in the program or not. For our empirical approach to work, it is hence crucial that the decision to accept refugees is not correlated with our outcome variable; changes in preferences for redistribution.

We argue that a number of circumstances make this assumption plausible: First, in accordance with the previous interpretation of Figures 1 and 2, the number of refugees arriving to Sweden increased dramatically during the period under study. Between 1986 and 1991, on average over 16,000 refugees arrived each year (peaking in 1989 at 24,879), compared to a yearly average of just above 5,000 during the previous six years. And during the last three-year period in our data, 1992–1994, the situation was even more exceptional with an annual arrival of 35,000, to a large extent driven by refugees from the Balkans. 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.[8] Second, refusals of refugee placement were in fact very rare,[9] 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.[10]

Bengtsson (2002) and own interviews with program placement officials confirm that most municipalities accepted the idea that all should participate in a manner of solidarity, and most municipalities did so, especially during the early years of the program. This furthermore created a peer pressure, which made it harder to refuse to accept placement.[11]

We thus claim that, conditional on the housing and perhaps also the labor market situation, the variation over time in immigrant shares within municipalities induced by the placement program is exogenous to individual changes in preferences for redistribution.[12] Still, to eliminate the risk of any

[8] 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.

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

[10] Correlation with the level of preferences could pose a problem in case of mean reversion. However, adding initial preference levels in each election period to the regression specification does not alter the results (results are available upon request), which suggests that this is not a problem in our case.

[11] 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 from only the initial period 1986–91.

[12] This conclusion is also supported by Figure 3, from which one can deduce that the program was successful in redirecting the flow of immigrants from larger to smaller mu-

bias remaining 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. Section 4 discusses these characteristics in more detail.

3 Data

As explained in the introduction, we are fortunate to be able to match municipal-level data on refugee placement, immigrant shares and various other municipal covariates, to individual survey information.

Survey data on individual preferences for redistribution is obtained from the Swedish National Election Studies Program[13]. 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 1985, 1988, 1991 and 1994, when roughly 3700 individuals were surveyed each wave.[14] Based on the panel feature of the survey, with these waves we construct three survey panels; 85/88, 88/91 and 91/94. 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 or not 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:[15]

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

For each of the four surveys, Figure 4 displays the distribution of proposal ratings of the respondents who will be part of 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

nicipalities.

[13]See http://www.valforskning.pol.gu.se/ for more information.

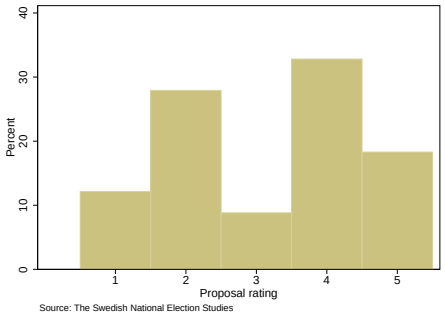
[14]The vast majority were interviewed in their homes, whereas a few people who were "busy and difficult to get in touch with" were interviewed over the phone.

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

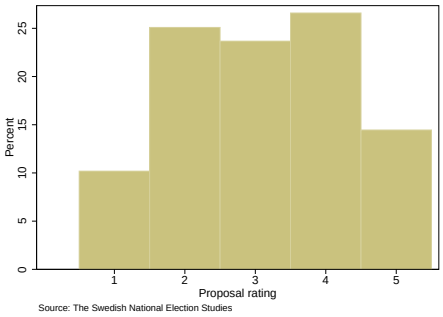
very similar. Notable is also the smaller percentage who thought it was a very bad idea to decrease the level of social benefits in the two latest surveys, hence indicating a negative trend in the support for redistribution.

Figure 4: Proposal ratings by survey

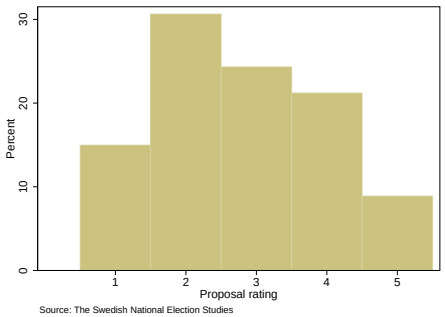
(a) 1985



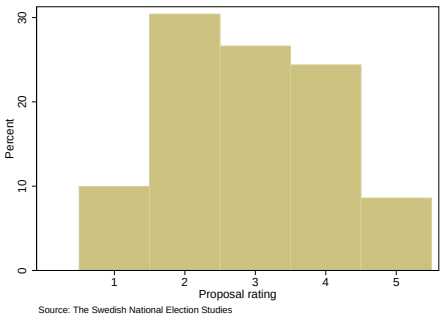
(b) 1988



(c) 1991



(d) 1994

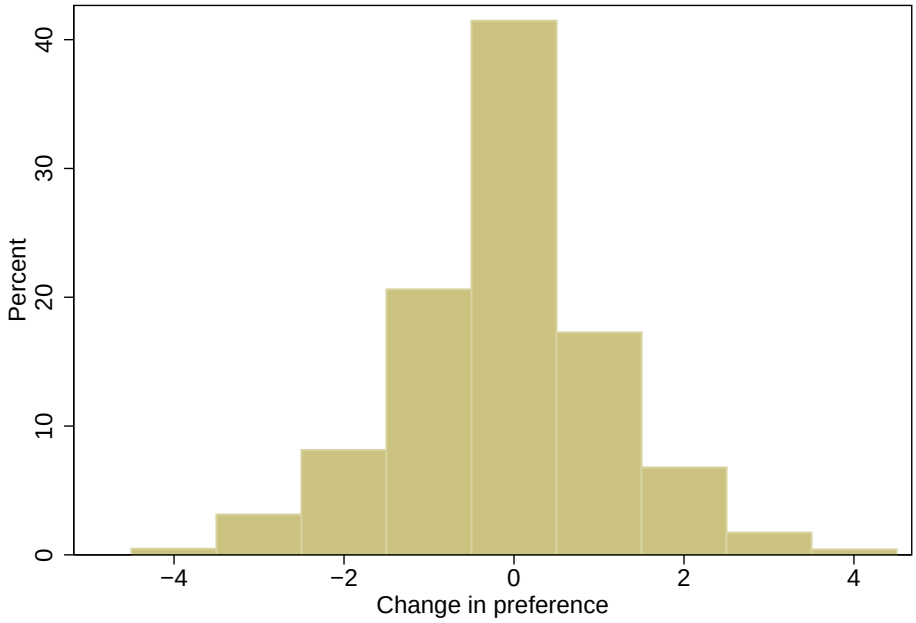


By taking the difference in response between the two survey waves (starting with the latter value), the proposal rating is used to construct a variable measuring the change in individual support for redistribution in the form of preferred social benefit levels. This means that individuals who become 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. A negative value for the change in preferences hence characterizes a situation where the support for social benefits decreases between two consecutive survey waves.

Figures 5 shows the distribution of this constructed variable for the change in preferences for redistribution in the form of social benefits. 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 the 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 between surveys; social benefits



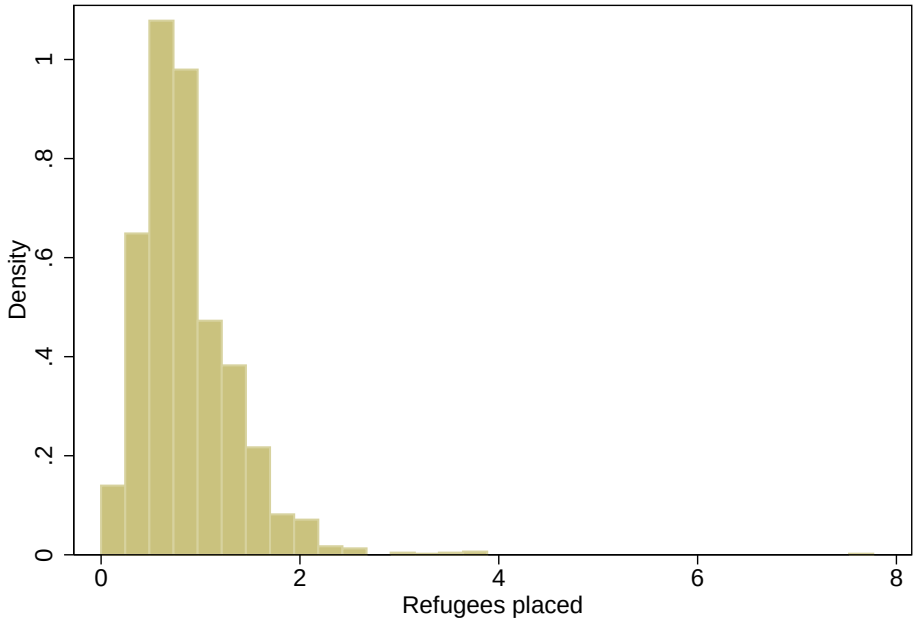
Source: The Swedish National Election Studies

In order 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 measure of the cumulative number of refugees placed in each municipality during each election period (86–88, 89–91, 92–94) as a percentage of the population in the municipality (averaged over the respective election periods). Figure 6 shows the distribution of this measure, including all 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 can have a large effect on the percentage share.)[16]

The refugee placement will be used as an exogenous source of variation in the share of immigrants living in the municipality. Our working definition of immigrants are people with citizenship from a country not a member of the OECD (according to membership status before 1994), an exception being

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

Figure 6: Distribution of refugee placement between surveys



Source: the Swedish Integration Board

Turkish citizens who are defined to be immigrants despite Turkey being an OECD-country.[17] 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 can stem either from individuals emigrating, or from them obtaining Swedish citizenship.

Table 1 displays 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. Since our identifying variation is within-municipality changes between two consecutive survey periods, the main variables are presented as such: the immigrant share (the independent variable of interest), the refugee share (used for exogenous variation in immigrants) and the measure of preferences for redistribution (the outcome variable). Note that the variable Δ*Refugee share* refers to the refugees placed within the placement program—hence the minimum value of zero. The variables in the bottom part of the table, starting with *Welfare spending*, will be included as controls: see the following section.

[17]The reason for the Turkish exception is that refugee immigration to Sweden from Turkey was relatively common during the period under study.

Table 1: Descriptive statistics; levels and changes between surveys

	mean	std.dev	min	max
Immigrant share	2.77	2.03	0.14	12.9
Δ Immigrant share	0.61	0.44	-1.47	3.26
Δ Refugee share	0.85	0.46	0	3.87
Δ PREF	-0.10	1.24	-4	4
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≥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≥200,000, Socialist majority,
Green party and New democrats are binary

Source: Statistic Sweden and the Swedish Integration Board

4 Estimation method

Our aim is to investigate whether a larger share of immigrants in a municipality causally affects the preferred level of redistribution among the municipality's population. To be able to identify this effect we need to isolate variation in the share of immigrants that is exogenous to preferences. That is, we require that our exploited variation in immigrant shares is not systematically related to differences in individuals' preferences for redistribution—neither directly via reverse causality, nor indirectly via some omitted variable(s) affecting both preferences as well as the residence 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 also likely that municipalities where preferences for redistribution are higher thanks to, e.g., a more well-functioning welfare system in terms of assisting beneficiaries in becoming self-supported, attract more immigrants.

4.1 Using exogenous variation from the refugee placement program

One way of attacking these types of bias 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. In order 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 2.1, but we will also include an additional set of local characteristics which 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. Thus, 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 stage of the 2SLS model is specified as follows (with $\widehat{\ }$ indicating predicted values from the first stage):

$$\begin{aligned}\Delta IM_{ms} = \alpha_1 \Delta REF_{ms} + \alpha_2 \bar{H}_{ms} + \alpha_3 \Delta Z_{ms} + \alpha_4 SIZE_{ms} \\ + \alpha_5 POL_{ms} + \alpha_6 SURVEY_s + \epsilon_{ms}\end{aligned} \tag{1}$$

$$\begin{aligned}\Delta PREF_{ims} = \beta_1 \widehat{\Delta IM}_{ms} + \beta_2 \bar{H}_{ms} + \beta_3 \Delta Z_{ms} + \beta_4 SIZE_{ms} \\ + \beta_5 POL_{ms} + \beta_6 SURVEY_s + \varepsilon_{ims}\end{aligned} \tag{2}$$

Our instrument in the first-stage equation (1), ΔREF_{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 $s-1$.

Municipal unemployment rate and rate of vacant housing (in public rental flats) which we believe affected the refugee placement are contained in the vector $\bar{H}_{ms}$, both averaged over the panel periods. Because also 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. Of these three variables, the reason for including the former is to take into account that municipalities may have changed their level of welfare expenditures, possibly as a consequence of receiving many refugees. By including this variable a given change in preferences is interpreted as a given change in support for redistribution rather than as a change from the prevailing level.

Equations (1) and (2) also include three sets of dummy variables: First, $SIZE_{ms}$ contains an indicator for large-sized municipalities (population$\geq$ 200,000) and one for small-sized municipalities (population$<$50,000). Second, the vector POL_{ms} 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 which we allow to be arbitrarily correlated within municipalities (i.e., when estimating the standard errors we cluster the residuals on 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 regressions

Before turning to the IV-estimations, where we will make use of the refugee placement program as an instrument for the share of immigrants living in the municipalities, we estimate equation (2) 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 unobservables correlated both with immigrants' choice of location and preferences for redistribution confound the estimates. Second, the estimated relation may reflect reverse causality, i.e. 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. The latter criterion, the so-called relevance, can easily be tested by estimating the first stage in equation (1)—i.e., 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 change in refugee share explains roughly half of the variation 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 change in refugee shares stemming from the program placement) with

Table 2: Baseline results

	OLS	IV 1^{st} stage	IV 2^{nd} stage
Δ Immigrant share	-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)
Δ Refugee share		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 in parentheses
The dependent variable is Δ PREF in columns 1 and 3,
and Δ IM in column 2
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 effects 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 was given on page 10). Considering that preferences are measures 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 effects

Before digging deeper into the overall results just presented and investigating whether the effects differ for different types of individuals, we need to ascertain that what we have estimated is not a mere spurious correlation. We do this by running 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.

Hence, 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 the predicted changes in immigration due to 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).[18,19]

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

[18]The covariates are suppressed in the table for the sake of brevity.

[19]Housing vacancies not available until 1985. We therefore use the 1985 variable 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. But importantly, as can be seen from column 4 in the table, the change in immigrants (as instrumented by the refugee placement) is 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.[20]

A second type of placebo-test is to 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 in order 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.[21] 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 an economic interpretation.

5.3 Do responses vary with individual characteristics?

According to the coefficients in Table 2, the causal effect of a one percent increase in immigrant shares is that the support for redistribution is reduced with 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, as compared to respondents who are themselves more likely to be net receivers in the redistribution scheme.

[20] 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.

[21] Because we are primarily not interested in the controls they are left out from the tables for the remaining of the analysis. Note, also, 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
Δ REF 85/88	0.483***	0.0151	-0.491***	
	(0.0781)	(0.119)	(0.132)	
Δ REF 88/91	-0.0691	0.508***	-0.477***	
	(0.0716)	(0.0989)	(0.165)	
Δ REF 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 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 Immigrant share	-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 in parentheses

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

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 fit. 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 and, hence, we 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 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 ΔREF 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.[22]

[22]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 ΔREF and the respective indicators. As can be seen from Tables 8-10 in the Appendix, all instruments

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 agents in 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 statistically different from zero).

We finally estimate how effects vary with worker type, and results on 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 also are the high-income earners). Overall, these sets of results clearly reveal that it is those respondents who contribute more extensively to the redistribution scheme whose support for redistribution is reduced as the group of likely recipients become more ethnically diverse.[23]

5.4 Sensitivity analysis

We claim 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. We can however not claim that our research design corresponds to a perfectly controlled experiment. For example, while 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 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 big 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

are strong and the joint F-tests are within conventional significance levels.

[23] We have also studied interactions with numerous other individual characteristics, such as gender, whether or not 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. None of these interactions were however statistically significantly different from zero.

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

	Social benefits	Social benefits
Δ Immigrant share	-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 Immigrant share	-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 Immigrant share	-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 in parentheses

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

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

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

Standard errors in parentheses

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

the remaining municipalities where the true increase in immigrants perhaps was smaller than what is being measured. But 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. Specially, Bengtsson (2002) report 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. In order 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 for the early period.

Columns three and four of Table 7 present these estimates, and indeed confirm our priors. Especially the second-stage estimate is reduced and is now essentially as large as -1. That is, increases in immigrant shares of one percentage point during the periods 1985–1988 and 1988–1991 caused the

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
Δ Refugee share	0.550***		0.310***	
	(0.0530)		(0.0621)	
Δ Immigrant share		-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 in parentheses

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

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

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

To be written.

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

	Δ IM	Δ IM*I(y< (>) p#)
Δ Refugee share	0.490***	0.0162***
	(0.0618)	(0.00587)
Δ REF*($y <$ p15)	0.0962	0.432***
	(0.101)	(0.123)
F-statistic	34.53	9.601
Δ Refugee share	0.497***	0.0385***
	(0.0663)	(0.0142)
Δ REF*($y <$ p40)	-0.000711	0.378***
	(0.0672)	(0.0704)
F-statistic	32.61	17.26
Δ Refugee share	0.513***	0.0410***
	(0.0588)	(0.0156)
Δ REF*($y >$ 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 in parentheses

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

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

A First-stage estimates

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

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

	Δ IM	Δ IM*I(w< (>) p#)
Δ Refugee share	0.474***	0.0429**
	(0.0683)	(0.0173)
Δ REF*(w < p40)	0.0594	0.401***
	(0.0628)	(0.0729)
F-statistic	34.84	22.48
Δ Refugee share	0.476***	0.0555**
	(0.0675)	(0.0236)
Δ REF*(w < p60)	0.0352	0.398***
	(0.0456)	(0.0634)
F-statistic	32.74	28.46
Δ Refugee share	0.498***	0.0145**
	(0.0642)	(0.00646)
Δ REF*(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 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
Δ Refugee share	0.465***	0.0810***
	(0.0668)	(0.0207)
Δ REF*Blue-collar	0.0354	0.384***
	(0.0528)	(0.0600)
F-statistic	33.74	35.43
Δ Refugee share	0.492***	0.0253
	(0.0589)	(0.0189)
Δ REF*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 in parentheses

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

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