Revisiting the Relationship between Ethnic Diversity and Preferences for Redistribution*

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Abstract

In this paper, we return to the question raised in Dahlberg *et al.* (2012) concerning a causal relationship between ethnic diversity and preferences for redistribution. A re-analysis of their study indicates that results are based on an endogenous instrument and severe sample attrition bias. Correcting for either of these two problems reveals that there is no relationship between ethnic diversity and preferences for redistribution. More generally, we provide results that put into question the conventional description of the Swedish refugee placement policy.

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1. Introduction

Numerous papers have analyzed the relationship between increased ethnic heterogeneity and the size of the welfare state or preferences for redistribution (e.g. Alesina *et al.*, 2001 and Alesina and Glaeser, 2004). In a recent study, Dahlberg, Edmark and Lundqvist (2012a) add to this literature by using a refugee placement policy, which they argue, provides exogenous variation in the number of refugees placed in Swedish municipalities from 1985 to 1994.

Here, we revisit this question and show (i) that their instrument (actual refugee settlement) does not provide exogenous variation in refugee settlement and (ii) that their results are compromised by extensive sample attrition bias stemming from unwarranted sample restrictions not discussed in the paper.

The Swedish refugee placement policy, discussed in greater detail below, was in place from 1985 to 1994. During this time, the Swedish Board of Immigration (SIV) and local municipal governments negotiated and signed *contracts* concerning the number of refugees any given municipality committed to annually for a given period of time. However, Dahlberg *et al*. (2012a) do *not* use the contracted levels as their instrument variable for their main (endogenous) explanatory variable of interest, the share of immigrants in a municipality.[1] Instead their instrument is based on a variable, which they label as "the number of refugees placed within the placement program".[2] We discovered that this variable actually measures payments of grants from SIV to local governments for immigrants that are defined as "refugees" in the municipality and that refugee status is *not* conditional on being part of the placement policy.[3] As a result, this measure includes a significant number of people that were not part of the placement policy such as tied movers (family members), asylum seekers, and re-settlers.[4] According to the statistics provided by the Swedish Migration Board (the successor of SIV), the average share of tied movers constituted 27% of all refuges during the period 1986-1994, but is as high as 40% in some years. Note that tied movers are not placed in municipalities via the placement program. Since the re-settlements of refugees to other municipalities is high, about 40% move within a 4-year period (Dahlberg *et al.,* 2012a), and considering that these resettled migrants, as well as tied movers, are counted as refugees in the

[1] See also Dahlberg and Edmark (2008) for a similar use of this instrument as an exogenous source of variation in welfare benefit levels.

[2] Despite this, they still misleadingly refer to their variable as "the number of *placed* refugees" in Dalberg *et al* (2012b)

[3] The rules governing the payments to municipalities are laid out in Swedish law ("Förordning (1984:683) om statlig ersättning för mottagande av flyktingar och vissa andra utlänningar 1984:683 and Förordning (1990:927) om statlig ersättning för flyktingmottagande m.m.) See links https://lagen.nu/1984:683 and https://lagen.nu/1990:927 for more information.

[4] See link: http://www.migrationsverket.se/download/18.478d06a31358f98884580007980/tabs1.pdf)

new municipality of residence for the first three years after initial placement, they will be included in the variable used by Dahlberg *et al.* (actual refugee placement). Thus, the instrumental variable used by Dahlberg *et al.* to a large extent includes individuals that were not part of the placement policy and may therefore be endogenous to municipal characteristics. To further investigate this problem, we have collected data on the contracts established between SIV and municipalities and find that there is a large discrepancy between contracted levels and actual refugee placement. This finding, consequently, strongly confirms that the Dahlberg *et al*. instrument is not likely to be exogenous since it contains a significant share of refugees that are not part of the placement program. In addition, it seems clear that an identifying source of variation that is "clearly identified and understood",[5] such as that given by the written contracts, provides a more credible analysis than a study based on an incomprehensible source of variation, such as the actual settlements of refugees.[6]

Another equally serious problem with the Dahlberg *et al*. study concerns attrition bias resulting from unwarranted sample restriction choices. We show that this leads to a very large sample attrition rate of 66 percent leading to a large bias (more negative) in their estimated coefficients. In contrast, when estimation is based on samples with considerably less sample attrition, the estimated coefficients are reduced considerably and are no longer significantly different from zero in the sample with the smallest attrition rate (33 percent). Most importantly, when the regressions are re-weighted with population weights to reflect the population regression of interest, then there is no effect even in the sample with the largest attrition rate, i.e., the sample used by Dahlberg *et al*. Equally importantly, when IV estimation is based on the arguably more correct definition of the policy (contracted number of refugees), there is no statistical relationship, regardless of estimation sample used, or whether the regressions are weighted or not.

In this study, a critical analysis of the Swedish refugee placement policy is undertaken in order to provide new information concerning the degree to which this policy can be described as providing (conditionally) random placement of refugees to municipalities. Thereafter, the robustness of results reported in Dahlberg *et al.* (2012) to use of the contracted levels of refugee assignment as an instrument variable in the analysis is tested. In addition,

[5] The natural or quasi-experimental approach emphasizes the importance of understanding the source of variation used to estimate key parameters. In the words of Meyer (1995) "researcher should seek to find variation that is driven by factors that are clearly identified and understood. One can then make an informed decision about the exogeneity of the variation and rule out other explanations." Thus, if the variation is not well understood then it is hard to make a compelling case for exogeneity.

[6] This is related to the distinction between discretionary and rule-based policies. Discretionary policies, by definition, can rarely be used an exogenous source of variation while rule-based policies, under certain conditions, can (Besley and Case, 2000).

numerous sample restrictions made in Dahlberg *et al*. are discussed and the sensitivity of results to these restrictions tested. Finally, we document a large number of inconsistencies in the Dahlberg *et al* data in the Appendix.[7]

It is noteworthy that we had to collect the data ourselves from the original sources since the data was not made publicly available on the JPE website due to claims of propriety by Dahlberg *et al*.[8] However, we managed to secure permission to post these data on the JPE Web site directly from the Swedish National Election Studies.[9]

The rest of this paper is organized as follows. In Section 2, the exogeneity of the Swedish refugee placement policy is discussed and tested. In Section 3, the Dahlberg *et al* study is re-analyzed focusing particularly on instrument endogeneity and sample selection issues. Results are presented in Section 4 and concluding remarks are presented in Section 5.

2. Exogeneity of the refugee placement policy

In this section, we analyze whether the Swedish refugee placement policy provides an exogenous source of variation in refugee shares allowing it to be used as an instrumental variable to identify the causal impact of immigrant shares on preferences for redistribution. More formally, Dahlberg *et al.* (2012) define their first-stage relationship as

$$x_{jt} = \alpha_j + \lambda_t + \beta z_{jt} + e_{jt} \tag{1}$$

where x_{jt} is the share of immigrants residing in municipality j at time period t, z_{jt} is the instrument, i.e., the Swedish refugee placement policy, α_j is a municipality fixed-effect and λ_t is a time fixed-effect. In other words, only the within-municipality variation in the policy instrument is used to identify the causal relationship of interest—the effect of immigration on preferences for redistribution.

As noted above, Dahlberg *et al.* define the instrument as *actual* refugee settlement in municipalities rather than the *contracted* levels determined by the formal agreements between

[7] We note that the authors continue to use the same data in their response to our replication, Dahlberg (2012b), despite numerous inconsistencies in these data. They could easily have discovered these inconsistencies since we have provided both our data and do-files. However, in footnote 10 of their response, they acknowledge that the two data sets differ stating that they "believe this is due to a few data typing errors resulting in missing values in some of our variables."

[8] The data used by Dahlberg *et al* is posted at the Swedish National Data Service (SND) as study SND 0906. In order to access this data, a formal request at SND is required, subject to the approval of the authors. SND cannot give permission for access to data since they have no property rights over the data from the Swedish National Election Studies. We would like to stress here, that the material posted by Dahlberg et *al*. at SND only includes the data necessary to re-produce their reported results. This implies that it is not possible to perform the analysis we conduct in this paper without a significant data collection effort.

[9] Formal permission was granted by Henrik Oscarsson Ekegren (Henrik.Oscarsson@pol.gu.se) at the Swedish National Election Studies, Department of Political Science, University of Gothenburg, Box 711, SE 40530, Gothenburg, Sweden

municipalities and the government authority in charge of refugee settlements, the Swedish Immigration Board (SIV). Surprisingly, the issue of how to correctly characterize the Swedish refugee policy has not been properly discussed, or empirically investigated, in the literature to date. Edin *el al.* (2003), the first study to exploit the refugee placement policy, write "Unfortunately, there is very little documentation about the practical implementation of the placement policy. Therefore, part of the information is based on interviews with placement officers and other officials of the Immigration Board." Similarly, Dahlberg *et al.* also largely rely on interviews with program officials.[10]

There are however a number of studies that have now documented how the Swedish refugee placement policy was implemented, many of which have not earlier been cited in the economics literature. This body of work provides a more varied and nuanced view on the workings of the refugee placement policy.

2.1 Description of the Swedish refugee placement policy

Dahlberg *et al.* (2012) describes the refugee placement policy as follows: "Under the program, refugees arriving to Sweden were consequently not allowed to decide themselves where to settle but were assigned to a municipality through municipal 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." They also write that it became "harder for 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. Second, refusals of refugee placement were in fact very rare, and those who at first did refuse got a lot of negative publicity." Finally they write "housing availability seems to have become the more important factor" governing refugee placement.[11] Given this description of the placement policy, and note that their description is based on the *contracts* signed between SIV and the municipalities, they argue that "the program is quite likely to provide exogenous variation conditional on a set of municipality-specific covariates."

This description, however, differs in important respects from the ones provided by for example Andersson (2003), Andersson and Solid (2003), Hammar (1993) and Soininen

[10] Dahlberg *et al.* also base their description of the placement policy on information provided in Bengtsson (2002) and Edin *et al.* (2003).
[11] Here they cite Edin *et al.* (2003) and Bengtsson (2003), see footnote 13 in Dahlberg *et al.* (2012).

(1992, 1993).[12] Specifically, these studies raise doubts about the following claims; (i) that refugees had little or no ability to decide themselves where to settle (ii) that the formal agreements, the contracts, between SIV and the local governments were binding and (iii) that the availability of housing vacancies was the key factor in determining refugee placement in municipalities.

Regarding the first claim, that refugees had no say on where they could settle, Hammar (1993) notes that all refugee placement should be voluntary and that the wishes and demands of the refugees themselves should be taken into consideration. Moreover, Hammar argues that refugees could refuse an offer of municipal placement. Those that did either stayed in the asylum centers until offered a new placement or made their own arrangements. Andersson (2003) argues along the same lines that "it was nevertheless obvious that the State's control [of refugee settlement] was only temporary, and perhaps illusionary, as the refugee could choose to migrate whenever (s)he wanted." The only cost of doing so was delayed enrolment in language courses (or other introduction programs) (Åslund *et al.,* 2011).

Regarding the claim that the Swedish Immigration Board (SIV) could largely determine the terms and conditions in the written contracts, Andersson (2003) writes "At the beginning of 1989, the SIV staff faced ever growing problems in their attempts to find enough municipal places for refugees, and despite the fact that almost all municipalities were engaged in the 1989 reception programme, the number of places was reduced to 17,800", from a previous level of 22,000-24,000. Likewise, Andersson writes, "The autumn of 1989 saw an even greater influx of new asylum seekers. Refugee camps were crowded, the municipalities became more reluctant to further increase receptions."

Finally, regarding the claim that municipal (public) housing vacancies was the key factor determining the placement of refugees, Soininen (1992, 1993) notes that a lack of housing vacancies was not considered a valid argument for a municipality to refuse signing contracts for refugee settlement during this period. At the beginning of 1989, SIV tried to convince some municipalities to establish temporary housing in institutional buildings that were not currently in use. This strategy was not initially successful but as open unemployment dropped to below two percent in 1990, municipalities became more interested in temporary solutions implying that refugees were housed in for example, hotels, military tents and cruise ships (Andersson, 2008).

[12] For other studies on the Swedish refugee placement policy, see for example; Sarstrand (2011), Similä (1992) Ålund and Schierup (1991) and Wickström (2008).

These studies on the Swedish refugee placement policy suggest that the characteristics determining both contracted and actual refugee settlement varied over time and across municipalities. We now turn to an empirical assessment of the refugee placement policy and the degree to which it can be considered to generate conditionally random placement of refugees across municipalities.

2.2 An empirical analysis of the refugee placement policy

In this section, we perform an empirical analysis of the Swedish refugee placement policy and its determinants and provide new evidence showing that the instrument used by Dahlberg *et al.* is not likely to be exogenous. Again, Dahlberg *et al.* define their instrument as *actual* refugee settlement, which is determined after contracts between municipalities and the government authority SIV have been negotiated. Using actual refugee settlement may be of little concern if the formal agreements correspond closely to actual settlement. However, to date no one has empirically analyzed if this is the case. We therefore compiled information on the formal written agreements between SIV and the municipalities for the time period 1986-1994 from the Archives of the Swedish Immigration Board.

By studying the written contracts, we see that the negotiated contracts varied across municipalities in both contract length and stipulations. Almost all contracts were formulated such that the municipality in question committed to *preparing* for a given number of refugee settlements per year during the contract period. Most contracts included clauses stating that SIV must consult with the municipality before each refugee placement on factors such as date of arrival, nationality, language skills, family composition, education and labor market experience of the refugee(s) in question. Contract length typically varied from one to three years and most contracts had a stipulation citing how and when contracts could be terminated. Contracts were effective only after local municipal government ratification. Most contracts included a clause stating that both partners in the agreement were to follow up refugee settlements in order to secure that settlement was in accordance with the goals stated in the national policy. Contracts were negotiated and re-negotiated during the program period and some terminated during the contract period. The municipality of Ragunda, for example, broke its contract of April 1991 in November 1991. Our reading of the contracts strongly suggests that municipals had large degrees of freedom in deciding the terms and conditions of these written agreements.

Empirically, we also find that there is a large discrepancy between actual refugee settlement and contracted levels. The percentage difference between actual and contracted refugee migration for the period 1986-1991 when the refugee settlement policy was considered to be most exogenous is plotted in Figure 1.[13] Interestingly there are large, and both negative and positive deviations, in percentage terms in actual refugee settlement from the contracted levels. A more formal test of this relation is to estimate a fixed-effect regression of the form:

(2) $$actual\_settlements_{jt} = \alpha_j + \lambda_t + \pi written\_agreements_{jt} + v_{jt}$$

where α_j is a municipality fixed-effect and λ_t is a time fixed-effect. If the written agreements completely determined actual refugee settlements we would expect that $\pi = 1$. On the other hand, if $\pi \neq 1$, this would imply either greater leeway among refugees in determining where to settle or greater self-determination among municipalities in whether or not to abide by written agreements concerning potential placement. Thus, with equation (2) it is possible to make a joint test of the two claims made by Dahlberg *et al.*; that is to say (i) that refugees arriving to Sweden were not allowed to decide themselves where to settle and (ii) that municipals could not affect the terms and conditions in the written contracts.

Table 1 presents results from estimation of Equation (2). In Columns 1 and 2, the two variables, *actual_settlements* and *written_agreements*, are defined in terms of levels (number of individuals) since the written agreements - which are arguably the theoretically correct measure of the placement policy - are stated in levels. All regressions include a full set of control variables (e.g., population size, welfare spending, income tax base, housing vacancies, and political variables). We note that Dahlberg et al (2012a,b) prefer to specify the placement policy in terms of population shares rather than levels. This is questionable since this not only re-defines the policy but also makes the re-scaled measure of the policy endogenous if there is migration, i.e., "'white flight' - that is, if native Swedes move out of a municipality in between two surveys as a result of an influx of refugees" which is also noted by Dahlberg *et al.* (2012a). Columns 3 and 4 report results from specifications when the variables are defined as population shares. Another issue raised by Dahlberg *et al.* (2012b) is that the model specification in Equation (2) does not reflect the micro data relationship being estimated in Dahlberg *et al.* (2012a). This can easily be fixed by weighing the (grouped-data) regression (Equation 2) with population weights (number of eligible voters) as discussed further below. The weighted regressions are displayed in Columns 2 and 4. Finally, to check the sensitivity

[13] Dahlberg *et al.* (2012) argue that variation in immigrant shares induced by the refugee placement policy is more likely to be exogenous during the initial years of the program, acknowledging that over time negotiations between municipalities and SIV concerning the possibility of settling fewer refugees increased. Åslund *et al.* (2011) also state that the policy formally was in place between 1985 and 1994 but that implementation was strictest between 1987 and 1991.

of the relationship across time, we add one year at a time implying that Row 1 shows the results for 1986-87, Row 2 for 1986-88, Row 3 for 1986-89 and so on.

Starting with the level regression in Columns 1 and 2 of Table 1, the estimates of π in Equation (2) are typically smaller than 0.4 but with one outlier value of 0.84 when the year 1994 is added to the specification.[14] When the regression is weighted in Column 2, some of the estimates are now also negative and the largest estimated coefficient is 0.61. Thus, from the specifications in levels, we conclude that the relationship between actual and contracted refugee immigration is highly non-robust and most of the estimates are imprecisely estimated. Turning to the specification in share of population, the estimates of π are in the range of 0.50-0.72. Since all the estimates in Columns 3 and 4 are statistically significantly different from $\pi = 1$, this suggests that a considerable part of the variation in actual refugee settlement is not driven by the written agreements. Consequently, actual refugee settlement is not a credible instrument. Taken together, the results in Table 1 clearly illustrate that the relationship between actual refugee settlement and contracts are highly non-robust: depending on time period, specification of the population regression (weighting scheme) and re-definition of the policy measure by normalizing it with population shares.[15] It is noteworthy that Dahlberg *et al.* (2012b) come to a completely different conclusion when they investigate this relationship, namely that relationship "is somewhat dependent on the time period and the type of variation used to correlate the two, but is in most specifications highly statistically significant and close to one."

Another way of empirically assessing to what degree the placement policy can be considered to generate a credible source of exogenous variation in immigrant shares is to analyze if the determinates of the policy are "clearly identified and understood". Several studies have argued that housing availability was the key determinant behind both contracted and actual refugee settlement during the policy period (Dahlberg *et al*., 2012a, Åslund *et al.,* 2011; *Edin et al.,* 2003).[16] The idea is that if the relationship turns out to be weak or negative

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[14] The finding that year 1994 is an outlier is not surprising since there was a large inflow (more than a 300 percent increase relative to 1993) of immigrants from the Balkan countries during this year due to the crisis in Bosnia-Herzegovina. This large inflow caused *all* municipal contracts to be re-negotiated, which makes the contracted levels an endogenous response to refugee settlements, making it is highly questionable to draw any inference on the relationship between written agreements and actual settlements based on the year 1994. In addition, the placement policy was abolished on July 1st, 1994, and was replaced with a law allowing refugees to choose their own place of residence.

[15] Excluding the control variables in estimation does not change reported conclusions.

[16] Dahlberg *et al.* (2012a) claim that "the program is quite likely to provide exogenous variation conditional on a set of municipality-specific covariates". Those mentioned include vacant public housing and local unemployment rates. Edin *et al*. (2003) state "Government authorities placed refugees in localities that were deemed suitable according to certain criteria". In practice, the availability of housing was the all-important

(contrary to expectation) then this would put into question the degree to which the policy can be characterized as conditionally random which would undermine the credibility of using the refugee placement policy as an exogenous source of variation.

Data on annual public housing vacancies, available at two different dates, March 1st and September 1st, were therefore collected. Table 2 displays the simple means for actual refugee settlement and the formal contracts for two groups of municipalities; those with no housing vacancies and those with available housing vacancies.[17] Panel A in Table 2 shows means for the September data while Panel B displays the means for the March data. The comparison in Table 2 reveals that around 40 percent (e.g., 628/1704 or 682/1704) of contracted agreements were made with municipalities with no public housing vacancies during the years 1986-1991. On average, slightly more than 50 refugees where contracted to municipalities with no housing vacancies while, on average, about 70 refugees where contracted to municipalities with available housing vacancies.[18] Moreover, the corresponding numbers for actual settlements is 53 and 76 in municipalities with zero housing vacancies and with positive vacancies, respectively. Thus, the results from Table 2 reveal that the difference in contracted agreements or actual refugee placements is not very different between municipalities with no housing vacancies and those with positive vacancies. Put differently, municipalities with zero housing vacancies received a total of about 35,000 refugees during the period 1986-1991 while those with positive housing vacancies received about 77,000 refugees.

To conduct a more formal test of the correspondence, we run regressions of the following form:

(3) $$w_{jt} = \alpha_j + \lambda_t + \delta housing\_vacancies_{jt} + n_{jt}$$

where w_{jt} is either actual refugee placement or contracted levels through the written agreements, α_j is a municipality fixed-effect and λ_t is a time fixed-effect. If housing vacancies completely determine the placement policy, we would expect that δ=1.

Results from estimation of Equation (3) are reported in Table 3. Panel A shows results for the written agreements (contracts) while Panel B shows results for actual refugee settlement. The first thing to note is that all estimates are negative (significant for contracted

factor. Finally, Åslund *et al.* (2011) argue "Available public housing essentially determined the placement." Åslund *et al.* also stress that as there was no direct interaction between placement officers and refugees, any selection must have been on observable characteristics.

[17] The average number of available rental apartments is close to 40 in municipalities with positive housing vacancies.

[18] The results that on average more than 50 refugees are placed in municipalities with zero vacancies are at odds with claims made by Åslund *et al*. (2011) that "Assigning a refugee to a municipality was conditional on having found a vacant apartment within that particular municipality."

levels) which is contrary to expectation given the presumption that housing vacancies should essentially determine the refugee placement policy (both contracted levels and actual placements of refugees). A negative correlation suggests that it is the urban areas with more limited public housing availability that continued to attract refugee migration, once fixed effects are taken into account.

In short, our empirical analysis of the Swedish refugee placement policy suggests that (i) there is little or no relationship between contracted agreements and actual refugee settlement and (ii) that there is a negative relationship between the refugee placement policy and housing vacancies, irrespective of whether the policy is defined by actual refugee settlement or contracted agreements. These results are at odds with arguments made in previous studies suggesting that the refugee placement policy can be considered exogenous conditional on observable factors such as housing availability although such claims have not earlier been empirically tested.[19]

We now turn to a re-analysis of Dahlberg *et al*. (2012a) in order to determine if the results presented in their paper are sensitive to choice of policy instrument and sample attrition bias (they have an attrition rate of 66%). We also investigate the sensitivity of results to how the variable immigrants is measured, i.e., according to citizenship, as is preferred by Dahlberg *et al*., or according to country of birth, which may arguably be a better measure of ethnicity (e.g Borjas 1992, 1995).

3. Re-analysis of the Dahlberg *et al*. study

Dahlberg *et al*. (2012) study the relationship between preferences for redistribution and ethnic diversity where Swedish local governments are used as a testing ground. They exploit survey data from the *Swedish National Election Studies* to measure preferences for redistribution. The following population regression is estimated:

$$Preferences_{ijt} = \alpha_j + \lambda_t + \beta Immigrants_{jt} + u_{ijt} \tag{4}$$

where the index i denotes individuals, j municipalities and t time period. The variable *Immigrants* is measured as the share of immigrants with non-OECD citizenship residing in municipality j at time period t. Time periods correspond to election years: 1985, 1988, 1991 and 1994, during which the survey data were collected. The parameter of interest is β which

[19] Folke (2011) also investigates the determinants of the refugee placement policy and finds that actual settlement is correlated with the political make-up of the local government. This finding is, in turn, related to the more general question concerning the use of policies as a source of exogenous variation when policies are created by policy makers, who among other factors take into account the preferences of the electorate in their policy making, as discussed by Besley and Case (2000).

measures the causal effect of immigration on preferences for redistribution under the assumption that E[u|*Immigrants*]=0.

3.1 Choice of Instrument

The major contribution of Dahlberg *et al.* is that they claim to avoid the endogeneity problem that immigrants self-select into municipalities based on, among other attributes, community attitudes towards redistribution, by using an instrument generated by the Swedish refugee placement policy. The instrument used by Dahlberg *et al*., however, is arguably not the refugee placement policy, that is to say the number of *contracted* refugees via written agreements between the municipality and SIV, but rather the *actual* number of refugee settlements in respective municipality. Below, we show that their results of a negative causal impact of ethnic diversity on preferences for redistribution do not hold when the contracted number of refugees is used as the instrumental variable rather than the actual number of refugees.[20]

3.2 Sample attrition bias

Another problem with the Dahlberg *et al*. study is the very large sample attrition due to unnecessary and unmotivated sample restrictions not discussed in the paper. Dahlberg *et al.* measure preferences for redistribution using the *Swedish National Election Studies,* a survey conducted each election year consisting of a representative sample of the eligible voters in Sweden. Thus, the eligible voters in Sweden define the relevant population of interest since the quasi-experimental design, the placement policy, is potentially affecting this population.

The survey has a rotating panel design in which half of the sample has been interviewed in connection with the previous election and the other half in connection with the succeeding election. Dahlberg *et al*. base their estimation on data from respondents in the rotating panel only. Specifically, they exploit the rotating panel feature of the survey, i.e., that the same individual is surveyed at two consecutive points in time, e.g., t and t-3. The following difference transformation of equation (4) is estimated:

$$(5) \qquad (Preferences_{ijt} - Preferences_{ijt-3}) = \beta(Immigrants_{jt} - Immigrants_{jt-3}) + (u_{ijt} - u_{ijt-3})$$

[20] In their response, Dahlberg *et al*. (2012b) argue that the contracted number of refugees is a weaker instrument than actual number of refugees and therefore more biased. This argument is wrong since in a just identified model, the estimate is approximately (median) unbiased and therefore one cannot assess which of the two IV estimates is more or less biased. In addition, the reduced form estimate is unrelated to the strength of the first-stage estimates. Thus, if the reduced form effect is not statistically different from zero, then there does not exist a causal relationship between immigrants and preferences for redistribution.

where the difference is taken over a three year-period (which corresponds to the date of election surveys). Dahlberg *et al.* then instrument the variable, ($Immigrants_{jt} – Immigrants_{jt-3}$), by actual refugee inflows between *t*-3 and *t*.[21]

From an identification point of view, access to panel data, or in this case, a rotating panel is not necessarily useful in a difference-in-difference set-up such as that of Equations 4 and 5. As the placement policy generates variation at the municipal level, what matters for identification is that group (municipal) means are consistently estimated (Angrist and Pischke, 2008 and Blundell and MaCurdy, 1999). That panel data are not required for identification in a difference-in-difference set-up is also illustrated by noting that a grouped-data regression weighted by the cell size is identical to OLS estimation on micro data.[22] Thus, repeated-cross sections or panel data are equally good as long as these data are representative of the underlying population (via random sampling) since otherwise the sample mean will not be consistent estimators of the population means.[23] However, repeated cross-sections typically have fewer problems with attrition than panel or rotating panel data.[24] Thus, if attrition rates are high in panel data, repeated cross-sectional data are preferred since representativeness is then maintained.

To illustrate the sample attrition problem in the Dahlberg *et al.* study, Table 4 provides information about the number of survey respondents in the 1985, 1988, 1991 and 1994 elections. Panel A shows information for the repeated cross-section while Panel B shows information for the rotating panel. Due to non-response on the question of interest, only 9,620 individuals out of total of 14,297 can potentially be used in the analysis. Thus, the attrition

[21] Note that they do not transform all of their control variables, such as the political variables and housing vacancies. As a result, any changes in these variables are not controlled for and can result in omitted variable bias.

[22] In their response, Dahlberg *et al* (2000b) argue that the repeated cross-section data cannot be used since cell sizes are too small, with an average of about 9 observations. This argument is clearly wrong since in their rotating panel data specification the number of observations per cell is only three. In other words, they fail to understand that their panel data specification is also a grouped-data regression as their regressor varies only at the municipality level.

[23] Dahlberg *et al.* seem to confuse panel data with rotating panel data with regards to random sampling. Panel data without attrition only requires random sampling at a single point in time to maintain its representativeness with the underlying parent population. However, a rotating panel requires random sampling across rotation periods in order to maintain representativeness. For example, the set of individuals in the rotating panel of 1985/1988 are a completely different set of individuals than those in the rotating panel of 1988/1991. To pool these two rotations together therefore requires random sampling at each rotation. In other words, the rotating panel does not solve any identification problems that cannot be solved by a pure repeated cross-section analysis. Indeed, given a large attrition rate across time periods within a rotation, repeated cross-section is preferable to a rotating panel since it maintains representativeness over time thus providing an unbiased estimate of the population treatment effect.

[24] Deaton (1997, p.19) writes in his book, *Analysis of Household Surveys,* that “panel data have a number of specific problems. One of the most serious is attrition”. Another problem with rotating or panel data is that survey respondents may change their behavior after being surveyed, as discussed by Peterson *et al.* (2001) and Bartels (2000). In sharp contrast, independent cross-sections are much less affected by such problems.

rate is 33 percent in the repeated cross-section. In the rotating panel, the maximum sample size is 5,571 while the available number of respondents is 2,703 leading to an attrition rate of 51 percent. Estimation in Dahlberg *et al*, however, is based on even fewer observations at 1,917 individuals. They therefore have an attrition rate of 66 percent from the original rotating panel, which is likely to exacerbate any sample attrition bias.

The average number of individuals per municipality and year in the Dahlberg *et al.* sample is less than three. There are also about 70 municipalities out of 284 in each survey panel that have no observations meaning that, on average, 25% of all municipalities are dropped from their analysis. In contrast, in the repeated-cross section, the average cell size (municipal-year) is almost nine and, on average, only eight municipalities have no observations. As a result, treating the survey data as a repeated cross-section rather than a rotating panel is superior with regards to sample attrition bias. In addition, since repeated cross-section data provides us with 9,620 useable observations, it is likely to be preferred on efficiency grounds as well.[25]

The reason Dahlberg *et al.* have only 1,917 observations instead of the available 2,703 observations, is partly due to the fact that a number of sample restrictions are made which are not reported in the paper.[26] First and foremost, many sample restrictions were made based on responses to survey questions not used in the final analysis of the study. In the 1985 and 1988 election surveys, only individuals that answered *all* of the following questions and answered according to the first five options on a six point scale, (i) very good proposal, (ii) fairly good proposal, (iii) neither good nor bad proposal, (iv) fairly bad proposal, and (v) very bad proposal, were included in the analysis:[27]

1. Retain nuclear power, also after 2010
2. Reduce third world aid
3. Increase the proportion of health care run by private interest
4. Reduce defense spending
5. Reduce the public sector
6. Increase economic support to immigrant so they can maintain their own culture

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[25] The type of data (panel, rotating panel, or independent cross-section) that will be most statistically efficient depends on the degree of temporal autocorrelation in the quantity being estimated. The formulas are given in Hansen *et al.* (1953, p. 268-72) and are discussed in the context of developing countries by Ashenfelter *et al.* (1986).

[26] The authors kindly provided us with their STATA do-files documenting the sample restrictions made.

[27] We have used the original English translation of the survey questions as provided by the *Swedish National Election Studies*.

In the 1991 and 1994 surveys, two additional restrictions were made based on the following questions:

7. Reduce income differences in society
8. Accept fewer refugees into Sweden

Individuals that responded to any of these survey questions according to the sixth option (6) "do not know/do not want to answer" are dropped from the analysis as well as all responses coded as (i) a missing answer or (ii) a missing observation.[28] These unnecessary sample restrictions reduce the number of individuals used in estimation by 581.

Another questionable sample restriction made by Dahlberg *et al.* is to exclude all individuals that moved to a different municipality between survey periods. In total, there are 205 individuals out of 2,703 that change municipality of residence. Such attrition naturally induces sample selection problems. This type of migration problem can, however, easily be solved by using information on previous municipality of residence (as discussed in Angrist and Pischke, 2008). In other words, the problem of survey respondents who migrate can be eliminated by defining comparison groups based on prior (to survey response) place of residence, as this cannot be affected by the treatment.

In total, these unwarranted sample restrictions reduce the sample to 1,927 observations (excluding one observation dropped due to an extreme value on their instrument). The Dahlberg *et al.* sample, however, is even smaller at only 1,917 individuals. It is unclear in the paper or the do-files what other sample restrictions are made to reduce the sample size further. One possibility is, of course, that there are missing values on other control variables of interest. The data we compiled for this replication do not reveal any such missing values in other control variables.

3.3 Definition of immigrants

Dahlberg *et al*. measure the municipal share of immigrants as the proportion with non-OECD citizenship (according to OECD membership prior to 1994). Given the question of interest, the impact of ethnic heterogeneity on preferences for redistribution, it seems more natural to define immigrants according to country of birth.[29] A definition based on citizenship is likely to mask part of the heterogeneity Dahlberg *et al.* aim to measure as a proportion of individuals with non-OECD origin are likely to have changed citizenship and will not be counted as

[28] When the authors use the 1982 survey for the placebo analysis, they use four survey questions (i.e., questions 2, 4, 5 and 6) to make their sample restrictions, implying that sample restrictions are inconsistent over time.

[29] In the response, Dahlberg et al (2012b) state at "the time of writing the paper, we only had access to citizenship, so the alternative definition was not an option." We note that these data are available at Statistics Sweden both then and now.

immigrants. Indeed, in a European perspective, Sweden is characterized by a relatively high naturalization rate, 65 percent of the foreign born are Swedish nationals (OECD, 2006). As citizenship is not an observable characteristic, the correlation between native preferences for redistribution and ethnic diversity is more likely to be based on the number and composition of the foreign born in any given municipality. At the very least, both series should be shown in order to determine to what degree results are sensitive to choice of definition.

4. Results

In this section, we provide results from our re-analysis of Dahlberg *et al.* (2012) regarding (i) choice of instrument, (ii) sample attrition bias and (iii) definition of immigrants.

We start by analyzing the reduced form relationship between preferences for redistribution and the choice of instrument as we do not want to rely on there being a first stage relationship between the instrument and the endogenous variable, share of immigrants (regardless of definition). Estimation of the reduced form equation is, however, still informative since in a just-identified instrument variable model, the *P*-value for the reduced form effect of the instrument is approximately the *P*-value from the second-stage (e.g., Chernozhukov and Hansen, 2008). To use the words of Angrist (2012), "if you can't see the relationship you're after in the reduced form, it ain't there!"

Results are estimated for three samples: (i) the selected sample used by Dahlberg *et al,* (ii) the full sample available in the rotating panel of the survey (without unnecessary sample restrictions) and (iii) the cross-sectional survey data. In addition, two model specifications are estimated and reported; (i) No controls except for panel (time) effects and municipality-fixed effects, (ii) all the control variables used in Dahlberg *et al*. We report two sets of standard errors: cluster-robust and homoscedasticity-only standard errors since the robust standard errors may be more biased than the homoscedasticity-only standard errors (Angrist and Pischke, 2008)

The results reported in Column 1 and 2 of Table 5 using the Dahlberg *et al.* instrument, actual refugee settlement, illustrate a number of important points. Notice, again, that our replication results are based on a slightly larger sample than the Dahlberg *et al.* sample (1,927 observations rather than 1,917) as discussed earlier. We do, however, come close to replicating their reduced form estimate of -0.172, as can be seen in Panel A (Columns 1 and 2).[30]

[30] Results in Table 2 of Dahlberg *et al.* (2012a) indicate that the first stage coefficient is 0.497 and the second stage coefficient -0.347 implying a reduced form estimate equal to -0.172 (0.497*-0.347).

A comparison of the different samples used in estimation indicates that there is a large bias due to attrition. The reduced form estimate is most biased in the Dahlberg *et al.* sample where the attrition rate is the largest at 66 percent (Panel A). Coefficient estimates are in the range [-0.15, -0.19]. The bias gets smaller when the number of observations is increased in the rotating panel (Panel B). Here the attrition rate is 51 percent and the corresponding coefficient estimates are in the range [-0.10, -0.14]. The bias gets smaller still when estimation is based on the repeated cross-section where the attrition rate is the smallest at 33 percent. Now the estimates are in the range [-0.05, -0.07] and no longer significant. Our results therefore reveal a clear pattern showing that the estimated reduced form effects are reduced by two-thirds when sample attrition bias is corrected for. In the specification without any control variables (Column 1), the point estimate is reduced from -0.15 to -0.05. Note that the bias also seems to depend on the control variables. The (absolute) size of the coefficient estimate tends to be 25 to 40 percent higher when control variables are added to the specifications in Column 2.

In Columns 3 and 4 of Table 5, we present results from estimation of the reduced form equation when the number of contracted refugees is instead used as the instrument rather than actual refugee settlements. The first thing to note is that all the reduced form estimates are much smaller than the corresponding estimates in Columns 1 and 2. For example, the reduced form estimates in the limited rotating sample (Panel A) in Columns 3 and 4 are in the range [-0.097, -0.106] while the instrument used by Dahlberg *et al*. produces results in the range [-0.15, -0.19]. The reduced form effects in both the extended rotating panel and the cross-section sample are smaller still and not significant at conventional levels. Moreover, the control variables no longer seem to introduce any bias in the reduced form estimates since estimates are quite insensitive to inclusion of covariates. This suggests that the placement policy (contracted refugees) is orthogonal to municipality characteristics whereas actual refugee migration is not. A comparison of results in Table 5 indicates that the significant result reported in Dahlberg *et al.* can only be reproduced by a combination of their (endogenous) instrument and the limited rotating sample. Results also suggest that a causal relationship between preferences for redistribution and the share of immigrants does not exist as we find no reduced-form relationship between the refugee placement policy (contracted refugees) and preferences for redistribution.

Turning to the corresponding instrumental variable estimates of ethnic diversity on preferences for redistribution, results (displayed in Table 6) show an even stronger case for no causal relationship. This is especially noticeable in a comparison between the preferred specification of Dahlberg *et al.* using actual refugee inflows as the IV and the limited rotating sample, which produces a coefficient estimate of –0.385 (Column 2, Panel A), and our preferred specification using contracted refugees as the IV and the largest possible sample, which produces an insignificant and small (and positive!) point estimate of 0.065 (Column 4, Panel C).

Another way of testing whether sample attrition leads to a biased estimate of the population regression coefficient is to compare results from the sample-weighted regressions, i.e., those displayed in Tables 5 and 6, with results based on the true population weights.[31] If sample attrition is random then the two weighting schemes should yield similar estimates. A Hausman *t* test can be used to formally test whether these estimates differ from each other. To conduct such a test, we exploit the fact that a grouped-data regression, such as the difference-in-difference specifications of Equations (4) and (5), weighted by cell size is identical to OLS on micro data (as discussed by Angrist and Pischke, 2008). As a result, one can therefore instead estimate the micro data specification (Equation 5) used by Dahlberg *et al.* by a grouped-data regression weighted by cell size. Similarly, we can estimate the population regression function using the same grouped-data specification but where the weights now correspond to the population weights, i.e., the number of eligible voters, instead of the sample weights, which are likely to be affected by attrition.

Tables 7 and 8 present the results from grouped-data regressions with population weights: the reduced form estimates in Table 7 and the IV estimates in Table 8. These estimates should be compared with the corresponding estimates in Tables 5 and 6 since these estimates are implicitly weighted by the size of the sample in each cell. The impact of using population weights has a dramatic impact on all the estimates in the rotating panel sample (shown in Panel A) since these estimates are now much smaller - about half the size - and no longer significantly different from zero. For example, the baseline reduced-form effect -0.189 in Table 5 (Column 2, in Panel A) is reduced to -0.099 in Table 7 (Column 2, in Panel A). Similarly, the baseline IV estimate of -0.385 in Table 6 (Column 2, in Panel A), is reduced to -0.188 in Table 8 when the IV regression is based on population weights. A Hausman *t*-test clearly rejects that the two estimates are similar. These results strongly suggest that attrition in the rotating panel data is non-random. Importantly, the estimates for the repeated cross-section are little affected by the weighting scheme, suggesting that non-random attrition or non-response in the cross-section data is not an issue.

[31] See Deaton (1997) for a discussion about using weighted regressions with survey data.

Next we analyze to what degree the definition of the variable *immigrants* matters for the results presented in Dahlberg *et al.* As noted above, they define their independent variable of interest, the (municipal) immigrant share, as the share of people with non-OECD citizenship. Table 9 shows the first-stage estimates for the two definitions of immigrant share: citizenship (Columns 1 and 2) and foreign birth (Columns 3 and 4) and the two definitions of the instrument: actual refugee settlement (Columns 1 and 3) and contracted refugees (Columns 2 and 4). The first thing to note from Table 9 is that there is large variation in the first-stage estimates. For example, comparing the first-stage estimates based on citizenship with those based on foreign birth reveals that that the first-stage estimate increases by almost 40 percent in the limited rotating sample from 0.49 to 0.68 (in the specification with the full set of control variables).[32]

This implies that Dahlberg *et al*. overstate the economic magnitude of their instrumental variable estimate with 40 percent, since the IV estimate is the ratio between the reduced-form result and the first-stage relationship. Moreover, such a large change in their IV estimate due to a re-definition of their key endogenous variable of interest also raises concerns about a causal interpretation of their findings. A perhaps even more disturbing fact is that there is *no* statistical first-stage relationship between the share of immigrants and the instrument when immigrants are defined according to country of birth and the instrument is defined as contracted refugees (Column 4, Panel C), i.e., the specification we argue to be most correct.

Finally, regarding statistical efficiency, the repeated cross-section analysis is more efficient than the rotating panel data approach as can be seen by comparing the standard errors in Panel A and Panel B of Tables 5 and 6 with those in Panel C of Tables 5 and 6. The standard error of the rotating panel is at least 40 percent larger than the corresponding standard errors of the repeated cross-section. Another noteworthy feature is that the cluster-robust standard errors (within parenthesis) are likely to be biased since they are typically smaller than the homoskedasticity-only standard errors (within brackets). This issue is discussed in Angrist and Pischke (2008) who suggest that in such cases, one should base inference on the largest standard errors. Dahlberg *et al.* base inference on the (smaller) clustered standard errors.

In summary, our re-analysis of Dahlberg *et al.* reveals that their results are highly non-robust due to choices concerning (i) instrument, (ii) estimation sample, (iii) definition of immigrants and (iv) standard errors, all of which are biased towards finding an economically

[32] The first-stage estimate reported in Dahlberg *et al.* is 0.497.

significant relationship between the share of immigrants and preferences for redistribution when no such relationship appears to exist.

5. Conclusions

In this paper, the results presented in Dahlberg *et al.* (2012a) concerning the causal relationship between ethnic diversity and preferences for redistribution are re-analyzed. Our results show that the previous characterization of the placement policy is inaccurate. First, there is no stable relationship between the formal agreements concerning refugee placement in municipalities and actual refugee settlement in municipalities. Second, we find that observable characteristics such as housing vacancies are *not* the key determinant of refugee placement, as previously argued in the literature. These new results put into question the extent to which the Swedish refugee placement policy can be characterized as an exogenous source of variation in immigrant placement given key observable characteristics.

A re-analysis of the results reported in Dahlberg *et al.* (2012a) indicates that their results are not robust to a correct characterization of the placement policy. In addition, we find that their study is plagued by severe sample attrition bias. Correcting for either of these two problems reveals that there is no relationship between ethnic diversity and preferences for redistribution.

Our results on the practical implementation of the Swedish refugee placement policy also speaks to the literature using the refugee placement policy as an exogenous source of variation in refugee sorting across municipalities (e.g., Edin *et al*., 2003; Edin *et al*., 2004; Åslund, 2005; Åslund and Rooth, 2007; Åslund and Fredriksson, 2009; Åslund *et al*., 2010; Åslund *et al*., 2011 and Grönqvist *et al.,* 2012). Specifically, our results question the underlying assumption in these studies that housing vacancies essentially determined the placement of refugees in municipalities during the era of the refugee placement policy.

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Figure 1: Percentage difference between actual and contracted immigration 1986-1991

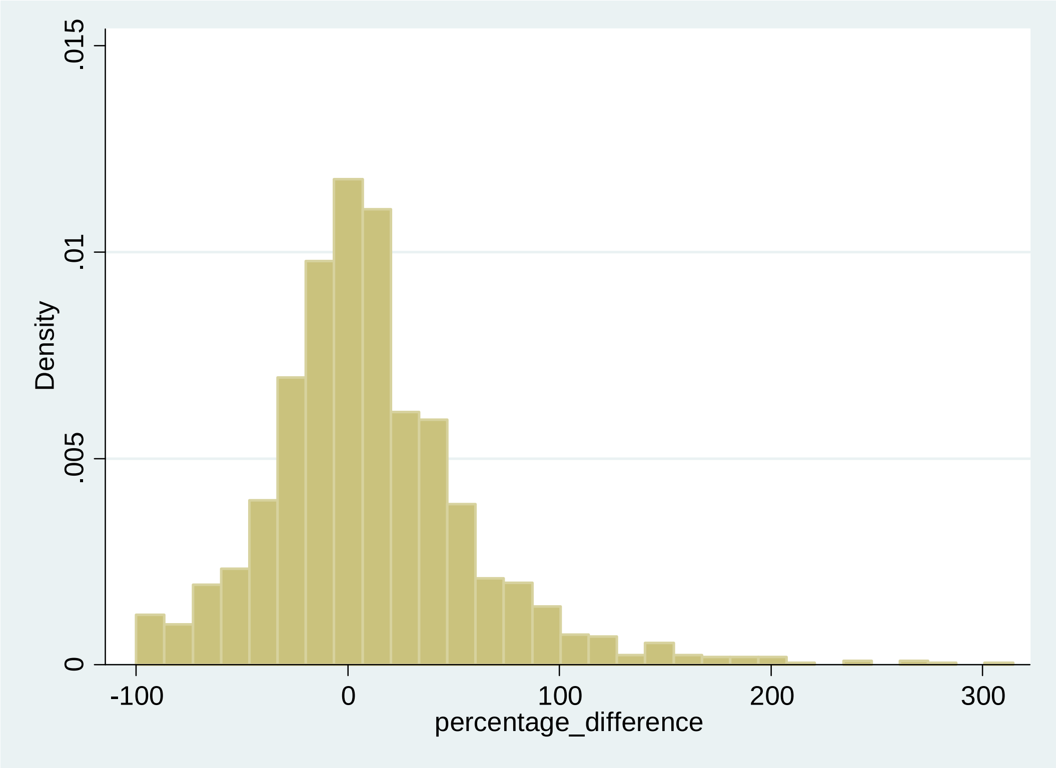

Table 1. The relationship between actual refugee settlement and contracted refugees for the period 1986-1994

	Levels		Share of population	
	Unweighted (1)	Weighted (2)	Unweighted (3)	Weighted (4)
Estimate for period:	0.364	0.131	0.600	0.556
86-87	(0.188)	(0.407)	(0.091)	(0.085)
Estimate for period:	0.399	0.325	0.623	0.586
86-88	(0.207)	(0.358)	(0.067)	(0.062)
Estimate for period:	0.530	0.422	0.690	0.647
86-89	(0.236)	(0.422)	(0.048)	(0.047)
Estimate for period:	0.302	0.049	0.691	0.650
86-90	(0.159)	(0.141)	(0.057)	(0.042)
Estimate for period:	0.168	0.022	0.648	0.614
86-91	(0.200)	(0.107)	(0.055)	(0.039)
Estimate for period:	0.128	-0.132	0.514	0.495
86-92	(0.167)	(0.075)	(0.131)	(0.086)
Estimate for period:	0.227	-0.211	0.504	0.501
86-93	(0.149)	(0.226)	(0.132)	(0.086)
Estimate for period:	0.839	0.612	0.727	0.640
86-94	(0.184)	(0.160)	(0.044)	(0.086)

Note: A full set of municipality and time fixed effects are included in the specifications as well as a full set of control variables: population size, welfare spending, income tax base, housing vacancies, and three political variables. Standard errors are reported in the parentheses and clustered at the municipality level.

Table 2. Available housing vacancies and the refugee policy: 1986-1991

	Housing vacancies = 0	Housing vacancies > 0
Panel A: Housing vacancies September 1st		
Number of contracted refugees	51	71
Number of actual refugee settlements	53	75
Number of observations	628	1,076
Panel B: Housing vacancies March 1st		
Number of contracted refugees	52	71
Number of actual refugee settlements	53	76
Number of observations	682	1,022

Table 3. Test of whether housing vacancies determine refugee placement

	Housing vacancies: September 1st (1)	Housing vacancies: March 1st (2)
Panel A: Dependent variable: actual refugee settlements		
Vacancies	-0.062	-0.037
	(0.043)	(0.028)
Observations	1,704	1,704
Panel B: Dependent variable: Number of contracted refugees		
Vacancies	-0.084	-0.089
	(0.029)	(0.020)
Observations	1,704	1,704

Note: A full set of municipality and time fixed effects are included in the specifications. Standard errors are reported in the parentheses and clustered at the municipality level.

Table 4. Sample definitions

Data set	Sample sizes
Panel A: Repeated cross-section data 1985-1994	
Total repeated cross-section survey data	14,297 (85=3704, 88= 3694, 91= 3558, 94=3341)
Available sample	9,620 (85=2598, 88=2383 , 91= 2382, 94=2259)
Panel B: Rotating panel data 1985-1994	
Total rotating panel survey data	5,571 (85/88=1901, 88/91=1956, 91/94=1714)
Available sample	2,703 (85/88=898, 88/91=989, 91/94=815)

Table 5. Reduced form estimates

	Instrument = refugee settlements (Dahlberg *et al*)		Instrument = contracted refugees	
	(1)	(2)	(3)	(4)
Panel A: Limited rotating panel sample. N=1,927 individuals: Attrition rate = 65%				
Reduced form effect	-0.152	-0.189	-0.106	-0.097
	0.069	(0.072)	(0.084)	(0.087)
	[0.071]	[0.079]	[0.090]	[0.096]
P-value: *cluster-robust*	0.028	0.009	0.209	0.257
P-value: *homoscedasticity*	0.032	0.016	0.239	0.300
Panel B: Available rotating panel sample: N=2,702 individuals: Attrition rate = 51%				
Reduced form effect	-0.105	-0.137	-0.062	-0.060
	(0.059)	(0.062)	(0.077)	(0.083)
	[0.062]	[0.069]	[0.081]	[0.087]
P-value: *cluster-robust*	0.075	0.029	0.421	0.469
P-value: *homoscedasticity*	0.093	0.048	0.443	0.487
Panel C: Available repeated cross-section sample: N=9,620 individuals: Attrition rate = 33%				
Reduced form effect	-0.052	-0.072	0.033	0.029
	(0.043)	(0.048)	(0.035)	(0.036)
	[0.047]	[0.054]	[0.050]	[0.052]
P-value: *cluster-robust*	0.225	0.135	0.355	0.416
P-value: *homoscedasticity*	0.263	0.186	0.508	0.577
Control variables	No	Yes	No	Yes

Note. The dependent variable is preferences for redistribution ("decrease social welfare spending") measured on a five point scale. Standard errors clustered at the municipality level are within parentheses and homoskedasticity-only standard errors within brackets. The regressions are estimated on micro data. All regressions are based on difference-in-difference set ups whereby all municipality specific factors and time fixed factors are controlled for (see text for more information). The control variables include population size, welfare spending, income tax base, housing vacancies, and three political variables.

Table 6. Instrumental variable estimates

	Instrument = refugee settlements (Dahlberg *et al*)		Instrument = contracted refugees	
	(1)	(2)	(3)	(4)
	Panel A: Limited rotating panel sample. N=1,927 individuals: Attrition rate = 65%			
Share of immigrants	-0.294	-0.385	-0.230	-0.229
	(0.141)	(0.156)	(0.192)	(0.212)
	[0.138]	[0.161]	[0.196]	[0.227]
P-value: *cluster-robust*	0.038	0.014	0.231	0.280
P-value: *homoscedasticity*	0.033	0.017	0.240	0.313
	Panel B: Available rotating panel sample. N=2,702 individuals: Attrition rate = 51%			
Share of immigrants	-0.202	-0.291	-0.134	-0.149
	(0.118)	(0.140)	(0.172)	(0.208)
	[0.121]	[0.148]	[0.175]	[0.214]
P-value: *cluster-robust*	0.089	0.038	0.435	0.475
P-value: *homoscedasticity*	0.094	0.049	0.444	0.488
	Panel C: Available repeated cross-section sample: N=9,620 individuals: Attrition rate = 33%			
Share of immigrants	-0.067	-0.099	0.061	0.065
	(0.058)	(0.069)	(0.067)	(0.082)
	[0.060]	[0.075]	[0.092]	[0.116]
P-value: *cluster-robust*	0.247	0.150	0.366	0.429
P-value: *homoscedasticity*	0.263	0.186	0.508	0.557
Control variables	No	Yes	No	Yes

Note. The dependent variable is preferences for redistribution ("decrease social welfare spending") measured on a five point scale. Standard errors clustered at the municipality level are within parentheses and homoskedasticity-only standard errors within brackets. The regressions are estimated on micro data. All regressions are based on difference-in-difference set ups whereby all municipality specific factors and time fixed factors are controlled for (see text for more information). The control variables include population size, welfare spending, income tax base, housing vacancies, and three political variables.

Table 7. Reduced form estimates with population weights

	Instrument = refugee settlements (Dahlberg *et al*)		Instrument = contracted refugees	
	(1)	(2)	(3)	(4)
Panel A: Limited rotating panel sample: N=1,927 individuals: Attrition rate = 65%				
Share of immigrants	-0.075	-0.099	-0.082	-0.054
	(0.076)	(0.085)	(0.094)	(0.102)
	[0.080]	[0.089]	[0.112]	[0.119]
P-value: *cluster-robust*	0.324	0.246	0.384	0.601
P-value: *homoscedasticity*	0.344	0.269	0.463	0.652
Panel B: Available rotating panel sample: N=2,702 individuals: Attrition rate = 51%				
Share of immigrants	-0.042	-0.063	-0.009	0.020
	(0.064)	(0.073)	(0.090)	(0.097)
	[0.069]	[0.077]	[0.097]	[0.103]
P-value: *cluster-robust*	0.510	0.388	0.923	0.833
P-value: *homoscedasticity*	0.542	0.411	0.928	0.843
Panel C: Available repeated cross-section sample: N=9,620 individuals: Attrition rate = 33%				
Share of immigrants	-0.060	-0.080	0.025	0.023
	(0.050)	(0.056)	(0.050)	(0.051)
	[0.047]	[0.054]	[0.054]	[0.057]
P-value: *cluster-robust*	0.224	0.158	0.619	0.654
P-value: *homoscedasticity*	0.195	0.139	0.646	0.688
Control variables	No	Yes	No	Yes

Note. The dependent variable is preferences for redistribution ("decrease social welfare spending") measured on a five point scale. Standard errors clustered at the municipality level are within parentheses and homoskedasticity-only standard errors within brackets. The regressions are estimated on grouped data and weighted by the number of eligible voters in each survey-panel/municipality cell. There are 647, 728 and 1106 cells in Panel A, Panel B and Panel C, respectively. All regressions are based on difference-in-difference set ups whereby all municipality specific factors and time fixed factors are controlled for (see text for more information). The control variables include population size, welfare spending, income tax base, housing vacancies, and three political variables.

Table 8. Instrumental variable estimates with population weights

	Instrument = refugee settlements (Dahlberg *et al*)		Instrument = contracted refugees	
	(1)	(2)	(3)	(4)
	Panel A: Limited rotating panel sample: N=1,927 individuals: Attrition rate = 65%			
Share of immigrants	-0.131	-0.188	-0.148	-0.112
	(0.134)	(0.167)	(0.173)	(0.213)
	[0.139]	[0.171]	[0.203]	[0.248]
P-value: *cluster-robust*	0.332	0.260	0.391	0.600
P-value: *homoscedasticity*	0.347	0.273	0.465	0.653
	Panel B: Available rotating panel sample: N=2,702 individuals: Attrition rate = 51%			
Share of immigrants	-0.074	-0.125	-0.016	0.046
	(0.114)	(0.148)	(0.171)	(0.217)
	[0.122]	[0.153]	[0.183]	[0.230]
P-value: *cluster-robust*	0.514	0.399	0.923	0.833
P-value: *homoscedasticity*	0.543	0.413	0.928	0.843
	Panel C: Available repeated cross-section sample: N=9,620 individuals: Attrition rate = 33%			
Share of immigrants	-0.076	-0.109	0.052	0.056
	(0.067)	(0.081)	(0.094)	(0.114)
	[0.059]	[0.074]	[0.101]	[0.125]
P-value: *cluster-robust*	0.256	0.177	0.582	0.622
P-value: *homoscedasticity*	0.201	0.142	0.609	0.653
Control variables	No	Yes	No	Yes

Note. The dependent variable is preferences for redistribution ("decrease social welfare spending") measured on a five point scale. Standard errors clustered at the municipality level are within parentheses and homoskedasticity-only standard errors within brackets. The regressions are estimated on grouped data and weighted by the number of eligible voters in each survey-panel/municipality cell. There are 647, 728 and 1106 cells in Panel A, Panel B and Panel C, respectively. All regressions are based on difference-in-difference set ups whereby all municipality specific factors and time fixed factors are controlled for (see text for more information). The control variables include population size, welfare spending, income tax base, housing vacancies, and three political variables.

Table 9. First-stage relationship according to definition of immigrants

	Share of immigrants according to citizenship		Share of immigrants according to country of birth	
	Instrument=refugee settlements	Instrument=contracted refugees	Instrument=refugee settlements	Instrument=contracted refugees
	(1)	(2)	(3)	(4)
	Panel A: Limited rotating panel sample of 1,927 individuals: Attrition rate = 65%			
First-stage effect	0.492	0.424	0.677	0.396
	(0.059)	(0.078)	(0.058)	(0.083)
	Panel B: Available rotating panel sample of 2,702 individuals: Attrition rate = 51%			
First-stage effect	0.472	0.406	0.652	0.371
	(0.055)	(0.067)	(0.058)	(0.080)
	Panel C: Available repeated cross-section sample of 9,620 individuals: Attrition rate = 33%			
First-stage effect	0.724	0.450	0.668	0.162
	(0.011)	(0.052)	(0.117)	(0.143)
Control variables	Yes	Yes	Yes	Yes

Note. Standard errors clustered at the municipality level are within parentheses. The regressions are estimated on micro data. All regressions are based on a difference-in-difference set up whereby all municipality specific factors and time-fixed factors are controlled for (see text for more information). The control variables include population size, welfare spending, income tax base, housing vacancies, and three political variables.

Web appendix (Not to be published)

1. Replication issues before having access to Dahlberg's *et al.'s* data

Here we describe our attempt to replicate the results in Dahlberg et al. (2012a) before we gained access to their data (with the exception of information on their instrument). We independently gathered our data based on the sources provided by Dahlberg *et al* (2012a), that is to say, the Swedish Board of Immigration (SIV) and Statistics Sweden.

We collected data on their instrument, actual refugee settlements in municipalities, directly from the published material kept in the Archives of the Swedish Immigration Board (SIV). When comparing our data from SIV with those provided by the authors, 24 inconsistencies were discovered. It is particularly noteworthy that of these 24 wrongly coded observations, four are based on the contracted number of refugees rather than actual refugee settlement while six observations are coded as missing when the correct number is zero. Thus, data used by Dahlberg *et al.* does not correspond well to the data from the Swedish Board of Immigration, which is the source they cite in their paper. In their response to our study (2012), the authors acknowledge in a footnote that data partly "comes from authors of previous studies of the placement program."

Regarding the share of immigrants in the municipality (IM), the endogenous variable of interest, we received this data electronically from Statistics Sweden for the years 1985, 1988, 1991 and 1994. We then discovered from the do-files provided by the authors that information for the year 1985 is missing in the Dahlberg *et al.* (2012a) study. Instead, the authors impute 1985 values by a linear extrapolation between 1984 and 1986 data. It is noteworthy that this issue is not discussed in the published paper.

Other replication issues have to do with the measurement and definition of the control variables used in the Dahlberg *et al*. analysis since these variables are *not* defined in their paper.

Starting with housing vacancies, there are two available annual series on vacancies in March and September and it is unclear which series is used in Dahlberg *et al.* (2012a).[33] We collected these data from the Statistics Sweden's publications "Statistiska meddelanden" ((i) Bo 34 SM8801, (ii) Bo 35 SM 8801, (iii) Bo 35 SM 9401, and (iv) Bo 34 SM 9401). As such, we discovered that it is wrongly stated in the paper that information on housing vacancies is

[33] Here it is important to stress that the data on municipal housing vacancies also used by Dahlberg et al. covers public entities only, which constitute about 40% of the total aggregate housing supply while private housing and housing co-operatives (tenant ownership) are not covered.

not available before 1985 (see footnote 25 in Dahlberg *et al.* (2012a). Data on housing vacancies for the years 1983-1988 can easily be collected from the readily available Statistics Sweden's publications "Statistiska meddelanden":Bo 34 SM8801 or Bo 35 SM 8801.

Turning to the local unemployment data, unemployment can be measured in two different ways, one which includes a significant change in the definition of unemployment that occurred in 1990 and another that uses the same (old definition) of unemployment for the entire time period, 1985-1994. Again, it is not clear in the paper which series is used in the analysis.

Similarly, there are two potential sources of information on social welfare spending; one published by the National Board of Health and Welfare and the other from local government budget records. The latter data contain seven missing values for the period 1985-1994 while the former have no missing values. Again, there is no discussion in the paper concerning which source is used in the analysis.

With these issues in mind, the following choices were made in our replication of the Dahlberg *et al.* results: (i) we use corrected data (24 observations) in estimation based on their instrument (actual refugee settlement) from information gathered directly at the SIV Archives (ii) we use the correct, and not extrapolated, values for share of immigrants per municipality in 1985, (iii) we use the measures of social welfare spending with no missing values, (iv) we use information on housing vacancies from the September surveys, and (v) we use the definition of unemployment which is consistent over time.

2. Replication issues after having access to data

We eventually received data from Dahlberg *et al*, but these data still did not include information on their outcome variable.[34] We then discovered that the cited data sources (i.e., Statistics Sweden and the Swedish Board of Immigration) do not correspond to the data used by Dahlberg *et al.* Below we document these inconsistencies.

Housing vacancies (cited data source: Statistics Sweden)

We discovered that Dahlberg *et al.* use data from the September 1st survey. We also discovered that data from the municipalities Haparanda, Pajala, Valdemarsvik, Borgholm, Lomma, Grästorp, Gnesta, Trosa are coded as missing observations when they should be coded as zero. The observations for Mullsjö and Habo are coded as missing observations in 1994 when information for these municipalities and this year is available.

[34] We request data on their control variables and information on immigrants (which are not propriety) on February 18. We received this data on September 3, only after we provided our data and do-files to the authors.

Welfare spending (cited data source: Statistics Sweden)
We discovered that Dahlberg *et al.* use data on welfare spending published by the National Board of Health and Welfare which has a number of missing values. Data from the municipalities Alingsås, Burlöv, Gävle, Hudiksvall, Hultsfred, Härnösand, Härryda, Mariestad, Sotenäs, Stenungsund, Södertälje, Trosa, Täby and Örnsköldsvik are coded as zero when they should be coded as missing observations.

Immigrants (cited data source: Statistics Sweden)
We discovered that there are systematic inconsistencies in most of data on immigrants defined by citizenship. The number of individuals with unknown citizenship differs for the data used by Dahlberg *et al.* The data provided directly by Statistics Sweden has more immigrants classified with unknown citizenship.

Unemployment
We discovered that Dahlberg *et al.* use the unemployment rate that includes a significant change in the definition of unemployment during the time period. We also discovered that these data include a number of typos. These typos originate from a data-file compiled by the Labor Market Board. However, the correct numbers are also included in the same file.

Population
We discovered that they use the average population size, i.e., the mean of population size at t and t-1, which means that two municipalities (Gnesta and Trosa) are defined to have a population size the year before they were created in January 1st 1992.

3. Description of our data and its sources

Data on the outcome variable of interest (preferences for redistribution):
The survey data from Swedish election studies are taken from:

1. **SND 0217 - Swedish election study 1985**
2. **SND 0227 - Swedish election study 1988**
3. **SND 0391 - Swedish election study 1991**
4. **SND 0570 - Swedish election study 1994**

This data were provided by SND (Swedish National Data Service)
http://snd.gu.se/en

Data on the refugee placement program

1. Yearly data on the number of flat-rate payments ("utbetalda schablonbidrag") for the period 1985-1994
2. Yearly data on the number of contracted slots for refugees ("kommunplatser för flyktingar enligt avtal") for the period 1985-1994

The data was collected from published material in the Archives of the Immigration Board

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Data on immigration
Data on foreign citizenship and country of birth for the years 1985, 1988, 1991 and 1994
The data was provided by Statistics Sweden (e-mail contact: befolkning@scb.se).

Data on housing vacancies
There are two available data series on housing vacancies in semi-public bodies
1. Yearly data on available dwellings in multi-dwelling buildings per March 1st
2. Yearly data on available dwellings in multi-dwelling buildings per September 1st

These data were collected from the publications "Statistiska meddelanden" : (i) Bo 34 SM8801, (ii) Bo 35 SM 8801, (iii) Bo 35 SM 9401, and (iv) Bo 34 SM 9401.

Population
Local government population per December 31st. These data were provided by Statistics Sweden (e-mail contact: befolkning@scb.se).

Welfare spending
There are two available data series:

1. Social welfare spending 1, downloaded from the following link: http://www.ssd.scb.se/databaser/makro/Visavar.asp?yp=tansss&xu=C9233001&huvudtabell=BidragshushBarn&deltabell=K&deltabellnamn=Bist%E5ndshush%E5ll+och+utgivet+ekonomiskt+bist%E5nd+%28socialbidrag%29+efter+kommun%2C+hush%E5llstyp+och+antal+barn%2E+%C5r&omradekod=SO&omradetext=Socialtj%E4nst&preskat=O&innehall=Bidragshushall&starttid=1983&stopptid=2009&Prodid=SO0203&fromSok=&Fromwhere=S&lang=1&langdb=1

The data are collected by The National Board of Health and Welfare ("Socialstyrelsen") and are provided by Statistics Sweden (e-mail contact: bo.thyden@scb.se). There are 7 missing values for the period 1985-1994.

2. Social welfare spending 2 was taken from the local governments budget records: ("Räkenskapssammandrag för kommuner") (see http://www.scb.se/RSkommuner/). These data was provided by Statistics Sweden (e-mail contact: offentlig.ekonomi@scb.se).

Unemployment
There are two available data series:

1. Unemployment series 1. These data were provided by the Swedish Public Employment Service ("arbetsförmedlingen"). e-mail contact: birgitta.i.andersson@arbetsformedlingen.se

2. Unemployment series 2. These data were provided by Anders Forslund at IFAU (Institute for Evaluation of Labor Market and Education Policy). e-mail contact: anders.forslund@ifau.uu.se

The difference between the two unemployment series for the period 1985-1994 is that there is a new definition of unemployment after 1989. The second series has the same (old) definition of unemployment during the whole period 1985-1994.

Tax base
The local government income tax base in period *t* (i.e., taxable personal income as reported in t-2). The data were provided by Statistics Sweden.

Political characteristics
Data on Socialist majority status (the Social Democrats and the Left Party), the Green Party and the New Democrats was taken from the municipal elections1985, 1988, 1991 and 1994. These data were provided by Statistics Sweden. e-mail contact: valstatistik@scb.se