

Ethnic Diversity and Preferences for Redistribution: Reply*

Matz Dahlberg[†] Karin Edmark[‡] Hélène Berg[§]

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1 Introduction

In this issue of *The Scandinavian Journal of Economics*, Nekby and Pettersson-Lidbom comment on Dahlberg, Edmark and Lundqvist (2012).¹ We welcome this discussion, and in this reply we briefly respond to three of the issues raised: possible measures of preferences for redistribution, sample selection bias, and the use of the refugee placement policy as an instrument for immigration. Lengthier responses to these issues, and also responses to some other points raised by the previous version of this comment in Nekby and Pettersson-Lidbom (2012), can be found in Dahlberg, Edmark and Lundqvist (2013).

2 Measure of preferences for redistribution

In the literature on preferences for redistribution, it is typically not obvious how one should measure these preferences, and the chosen measurement is by necessity guided by the definitions available in the existing survey data.

When starting working on the paper that eventually resulted in DEL, there were three survey questions in the Swedish Election Survey that we considered as potential candidates for measuring preferences for redistribution; the respondents' view on decreasing the public sector,² decreasing

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[†]Uppsala University; CESifo; IEB. matz.dahlberg@ibf.uu.se

[‡]Stockholm University; CESifo; IFN. karin.edmark@sofi.su.se

[§]Previously Heléne Lundqvist. Stockholm University; CESifo. helene.berg@ne.su.se

¹From hereon we refer to the comment by Nekby and Pettersson-Lidbom as NP-L, and we refer to the article by Dahlberg, Edmark and Lundqvist (2012) as DEL.

²Exact question in Swedish: "Vilken är Din åsikt om förslaget att minska den offentliga sektorn?"

social benefits,³ and increasing the support to immigrants so that they can maintain their own culture.⁴ The early drafts of the paper, with results for all these three outcome measures, were presented at several workshops, conferences and seminars.⁵ These presentations often generated a discussion on how one should interpret the three outcome measures, and whether or not they were good measures of “preferences for redistribution”. In general, the view was that the question about decreasing social benefits was the measure for preferences for redistribution that most closely relates to the definition in theoretical models (see, e.g., Shayo, 2009),⁶ and that the questions about the size of the public sector in general and about support to immigrants so that they can maintain their own culture were the least related to that definition. Consequently, we chose to go with the respondents’ view on decreasing social benefits as our measure of preferences for redistribution.

Thus, while it goes without saying that it is highly relevant to think about how to best interpret answers to different survey questions and how to understand why some turn out significantly while others do not in a specific setting, the remark in NP-L that the findings in DEL might be the result of “p-hacking” is not true.

3 Sample selection

NP-L argue that the estimated effect on preferences for redistribution is biased due to inappropriate sample selection. Two categories of tests for an endogenous sample are proposed; one based on weighting group means, and one based on using the full repeated cross sections rather than restricting the sample to the rotating panel. We discuss these tests in reverse order.

3.1 Rotating panel vs. repeated cross sections

NP-L suggest testing for endogenous sample selection by comparing the results from the rotating panel to results from using the full repeated cross sections. Before discussing this, we would like to stress that the reason for the focus on the rotating panel in DEL is that we, as stressed in the introduction of the paper, considered the access to this type of survey data, where each individual is observed twice, a crucial and novel feature of our study; thereby, we were able to study the changes in preferences *within*

³Exact question in Swedish: ”Vilken är Din åsikt om förslaget att minska de sociala bidragen?”

⁴Exact question in Swedish: ”Vilken är Din åsikt om förslaget att öka det ekonomiska stödet till invandrarna så att de kan bevara sin egen kultur?”

⁵That we considered these different outcomes is also clear from the Stata code available online at the *Journal of Political Economy*.

⁶Note that the Swedish word “socialbidrag” is synonymous to welfare benefits, likely leading the thoughts to redistribution to the poor.

individuals over time. Now, following the critique by NP-L, we realize that this choice is a rather complicated matter, and we therefore welcome this part of their comment particularly.

Turning to the point of critique put forth by N-PL on this matter, they argue that using the full repeated cross sections is superior to using the more restrictive rotating panel. And since the results between the two samples differ, they conclude that the approach with most observations is to be preferred.

A potential problem with this conclusion is that the groups that make up the repeated cross sections are based on a very small number of observations. In other words, there are very few respondents per municipality and year; the distribution of municipal sample sizes on our survey question of interest is such that, in a given survey, there is at most 1/3/5 respondents in 5/25/50 percent of the municipalities, with an average of around 9. This implies that the mean preferences in the municipality—which, as highlighted by NP-L, is what we ultimately are interested in since treatment is at the municipality level—is estimated with a lot of uncertainty. Note that this uncertainty is different from the typical uncertainty that is the basis for doing inference on mean comparisons, where it is implicitly assumed that the means are indeed the correct means thanks to a large group size. Thus, for correct inference in cases of small groups when there are relatively few of them, this additional uncertainty needs to be taken into account.

We do not know of any studies of the properties of these types of estimators on cross sections with very small group sizes. In contrast, related methodological studies concern how to do inference when the number of groups is small, and then commonly assume that the group size is large or even very large. For example, Donald and Lang (2007) focus in their Monte Carlo simulations on the difference-in-difference case with four groups each of size 250 or 2500, and find that group size does matter when the average group outcome is not normally distributed (page 231). Even though the setting in Donald and Lang (2007), with only four group of moderate to large size, is different from ours, with moderately many groups but with very small group sizes (with an average of 9), their results indicate that inference with cluster-based sampling likely depends on the size of the groups. By, as in DEL, utilizing the rotating panel and taking within-individual differences, the sampling variability is reduced, and hence inference in a situation with very small group sizes might be improved. Whether or not this is the case is a question we leave for future methodological research.

It is worth noting, though, that the resulting estimate from using the smaller sample with the rotating panel should be interpreted as the effect on the preferences of the sampled population (i.e., those individuals who state their preferences in two consecutive surveys). We argue that the internal validity is high for this sample, but without additional assumptions, this effect cannot be generalized to the full population. Admittedly, this distinc-

tion between the internal and external validity could have been discussed more thoroughly in DEL. This brings us to the other proposed test for an endogenous sample; the one based on weighting group means.

3.2 Weighting group means

The other test for endogenous sample selection proposed by NP-L is based on aggregating the individual data into group (municipality) means, and then test whether the results are sensitive to different weights. The idea is that if the sample is truly a random draw from the population, weighting by the group sample size (as is implicitly done in the individual-based regression) or weighting by the actual population group size should yield the same results.

However, NP-L show that the results *do* change; going from column 2 to column 3 in Table 1 of their comment cuts the second-stage estimate by half. We find this rather interesting. As noted above, the result in DEL that increased immigration leads to decreased preferences for redistribution refer to the average effect in the sampled population. The fact that this result changes when the estimate is reweighted tells us two things; (i) there are heterogeneous treatment effects; and (ii) the respondents do not—as pointed out by NP-L—constitute a random sample from the population.

Regarding (i), another result in DEL is that the effect within the sampled population indeed is quite heterogeneous, in the sense that it is much more pronounced for high-income individuals. And regarding (ii), non-random non-response is, while important to point out, not very surprising. In fact, combining (i) and (ii), a potential explanation to the sensitivity of the results to the different weighting schemes is that non-response is higher among low-income individuals where the effect of increased immigration is less pronounced, and when attempting to compensate for their non-response with alternative weights, the resulting average effect therefore becomes smaller.

It turns out that this indeed is a plausible explanation; among all individuals who stated their income in the three survey panels used for the main analysis in DEL, non-response to any of the three questions studied or to two questions that we originally planned to also study and therefore based our sample on is higher among the bottom two than among the top two income classes (41 versus 28 percent).

All in all, the re-analysis in NP-L suggests that our, what we argue are internally valid, estimates of the negative effect of increased immigration on people’s preferences for redistribution, especially among high-income earners, do not necessarily imply that there is an average negative effect in the overall population.⁷ In times of increased immigration, we find this distinction to be rather important; although increased immigration might have

⁷Note that, if non-response is non-random and effects are heterogeneous, weighting the group means with actual population shares does not necessarily recover the true population mean (Solon et al., 2015).

interesting impacts on high-income individuals (which one may want to dig deeper into), there is no evidence that this poses a threat to the welfare state by decreasing the overall support for redistribution.

4 Refugee placement program

The information that we, as well as authors of earlier papers (see, e.g., Edin et al., 2003 and Edin et al., 2004), used to measure the number of placed refugees is based on the number of individuals for whom the municipalities received a fixed, standard amount grant from the central government as a reimbursement for costs due to initial integration activities (learning Swedish etc.) that the municipalities were to provide for new refugees (“*schablonersättning*” in Swedish).

In their comment, NP-L claim that not only do these individuals include the municipality-placed refugees, but also “tied-movers, refugees who have migrated internally within Sweden and asylum seekers, none of which are placed in the municipality via the placement program” (see footnote 19).⁸

While it is true that the municipalities were entitled to various types of reimbursement from the central government for accommodating both refugees with a residence permit as well as asylum seekers, the type of standard amount grant that our data is based on was, to our knowledge, given only to refugees or asylum seekers that were placed according to the contracts with the Immigration Board (see Regulation 1984:683,⁹ §§10-12, for the early period of our data, and Regulation 1990:927,¹⁰ §3 and §§10-13, for the later period). This means that asylum seekers are in general not included in our data, the exception being when they were actually part of the placement policy. Furthermore, the data that we use does not double count refugees who have migrated internally (between Swedish municipalities), but is based only on the first payment of the grant.

However, NP-L are correct in pointing out that tied movers (immigrants with close family ties to a refugee that has already been granted residence permit) are to some extent included in our measure. Specifically, the fixed reimbursement was also given for family members who arrived *some time* after the initial placement of a refugee, or who were themselves granted refugee status. What is meant by “some time” has varied over the years; the regulation from 1984 (1984:683) states that, in order for the municipality to receive additional grants for tied family members who were not themselves given refugee status, they had to arrive within six months after the initial

⁸In addition, in section 4 it is claimed that the grants were paid with a considerable lag. This information is new to us.

⁹In Swedish: “Förordning (1984:683) om statlig ersättning för mottagande av flyktingar och vissa andra utlänningar”.

¹⁰In Swedish: “Förordning (1990:927) om statlig ersättning för flyktingmottagande m.m.”.

placement of the refugee, while this time span was increased to two years in 1991¹¹.

We acknowledge that it is a disadvantage that tied movers—under certain conditions—are counted in our data. This is only a problem for our analysis if the placed initial refugee had moved to another municipality than originally placed in, but since moving was quite common we take this as a valid point of critique.¹²

NP-L suggest in their comment that, instead of actual placement of refugees as measured with the data on the fixed reimbursements, a preferred instrument is data on the refugee placement *agreements* between the Board of Immigration and the municipalities. As we discuss in our longer reply to a previous version of the comment in NP-L (see Dahlberg, Edmark and Lundqvist, 2013), we think that the agreements constitute an interesting *alternative* instrument, but that there are pros and cons with both alternatives.

In sum, we would like to point out that, in our view, the placement program clearly changed the geographical distribution of immigrants' locations in Sweden, and this makes it an interesting policy to study.¹³ Furthermore, both the currently available measures of this policy have their drawbacks, and they thus constitute useful complements. Digging deeper into the policy, and finding better measures, might be a valuable task for future research.

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¹¹According to a report from the Swedish National Audit Office; “Riksrevisionens styrelses framställning angående statliga insatser för nyanlända invandrare, Framställning till riksdagen 2006/07:RRS13”.

¹²That is, it might cause endogeneity problems if the first-arriving refugee had moved from the initial municipality of placement before the arrival of the family members. In such cases, our data would count the new family members as “placed refugees” in the new municipality, even though really, this new municipality was *chosen* by the first-arriving refugee.

¹³In DEL as well as in earlier papers, it is shown that the settlement patterns changed distinctively at the time when the placement policy was enacted, see, e.g., Figure 3B in DEL.

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