UPPSALA
UNIVERSITET

Uppsala Center for Fiscal Studies
Department of Economics

Working Paper 2013:2

Ethnic Diversity and Preferences for Redistribution: Reply

Matz Dahlberg, Karin Edmark and Heléne Lundqvist

Uppsala Center for Fiscal Studies
Department of Economics
Uppsala University
P.O. Box 513
SE-751 20 Uppsala
Sweden
Fax: +46 18 471 14 78

Working paper 2013:2
February 2013

Ethnic Diversity and Preferences for Redistribution: Reply

Matz Dahlberg, Karin Edmark and Heléne Lundqvist

Ethnic Diversity and Preferences for Redistribution: Reply*

Matz Dahlberg[†] Karin Edmark[‡] Heléne Lundqvist[§]

February 7, 2013

Abstract

In a comment to Dahlberg, Edmark and Lundqvist (2012), Nekby and Pettersson-Lidbom (2012) argue (i) that the refugee placement program should be measured with contracted rather than actually placed refugees, and claim that the correlation between the two measures is insignificant and close to zero; (ii) that instead of using the rotating individual panel, we should have used the full cross-sections in combination with municipality fixed effects; and (iii) that immigrants should be defined based on country of birth rather than citizenship. In this response, we discuss why we (i) do *not* agree that contracted refugees is the preferred measure, and we show that the correlation between the two measures is highly significant and large; (ii) do *not* agree that the full cross-sections can be used; and (iii) *do* agree that defining immigrants according to country of birth is preferred. In a re-analysis, the conclusion from Dahlberg, Edmark and Lundqvist (2012) that ethnic diversity has a statistically and economically significant negative effect on preferences for redistribution is only marginally affected.

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

1 Introduction

Before addressing the issues raised by Nekby and Pettersson-Lidbom (2012)[1] in their comment on Dahlberg, Edmark and Lundqvist (2012)[2], let us state

*We thank Per Johansson as well as participants at the Political Economy Workshop held in Oslo in December 2012 for insightful comments. Edmark and Lundqvist are grateful for financial support from the Jan Wallander and Tom Hedelius foundation.

[†]Uppsala University; CESifo; IEB; IFAU; UCFS; UCLS. matz.dahlberg@nek.uu.se

[‡]IFN; CESifo; IFAU; UCFS; UCLS. karin.edmark@ifn.se

[§]Stockholm University; CESifo; UCFS; UCLS. helene.lundqvist@ne.su.se

[1]From hereon referred to as NP-L.

[2]From hereon referred to as DEL.

what we think are the main contributions of DEL to the literature on ethnic diversity and preferences for redistribution.

First, when estimating the effects of increased immigrant shares (our measure of ethnic diversity), we exploit a refugee placement policy that induces variation in two desirable ways; (i) it provides substantial within-municipality variation in immigrant shares over time, which is typically not the case; (ii) although the placement did not constitute a randomized experiment—and we never claimed it did—we argue that the policy-induced variation in immigrant shares *is likely to be more exogenous to preferences for redistribution than variation used in standard OLS regressions*, where the variation is due to immigrants' sorting into certain types of neighborhoods. This latter argument is backed up by evidence in the data showing that settlement patterns changed distinctively at the time when the placement policy was enacted. In particular, it is clear that one of the aims of the policy—to break the concentration of refugees to the larger cities—was indeed achieved. Furthermore, the OLS estimate differs substantially from the estimate obtained when exploiting the policy-induced variation, and it does so in a way that is consistent with why we think that OLS yields biased estimates.

Second, aside from exploiting variation from the refugee placement program, a novel feature of our paper is that it is the first to have access to panel data on individual preferences for redistribution. Unlike previous studies that have used cross-sectional or repeated cross-sectional data, this allows us to control for all individual level factors that are constant over time.

In their comment to our paper, NP-L mainly criticize and base their re-analysis on (i) the exogeneity of the refugee placement; (ii) the use of individual-level (rotating) panel data; and (iii) our definition of immigrants. In the next three sections of this response, we address these points of critique more thoroughly. Our response can be summarized as follows.

First, regarding the placement policy program, NP-L note that there are two alternative ways of measuring the placement policy in the data; with the data that we use on actual placements, and with data on contracted placements. NP-L argue that contracted placements is the correct measure, and that the use of actual placement does not capture the placement policy and is therefore endogenous.

From NP-L, one gets the impression that the correlation between the two measures—contracted and actual placements—is essentially zero (see Table 1 in NP-L). This is misleading. As we show in Section 2 below, the two measures are in fact highly correlated. Importantly, using the same type of variation as in our original model (where we estimate the effect of increased immigrant shares on changes in preferences for redistribution; see equations (1) and (2) in DEL), the correlation is a statistically significant 0.74.

Replacing actually placed refugees with contracted refugees in the empirical analysis yields qualitatively similar results. The estimates are, however, somewhat smaller in magnitude and less precise as compared to in DEL, so that the second-stage estimate is no longer statistically significant. This is in our opinion not surprising, given that the contracts reflect agreements for intended future placement that were, for reasons to be discussed below, not always exactly fulfilled. The contracts are therefore a weaker instrument for the change in immigrant shares in the municipalities. However, we recognize that the contract data, which was generously provided to us by NP-L, allows for a relevant robustness test of our results.

Second, regarding the rotating panel, NP-L claim that our use of individual-level differences is inappropriate, and that we instead should have used the full cross-sections in combination with municipality fixed effects. We disagree. The reason why we believe that such a cross-sectional analysis is not possible is that the number of observations per municipality is too small to provide informative measures of changes in preferences at the municipality level. We do, however, agree with their point regarding the other, more minor, sample restrictions that we do, but these do not affect the results in DEL.

The third and final main point in the re-analysis in NP-L is that data on country of birth gives a better measure of the share of immigrants from refugee countries residing in the municipalities than what our data on country of citizenship does, since country of birth is a more stable indicator. We agree with this point, and are grateful to Nekby and Pettersson-Lidbom for providing us with these data. But as we show below, defining immigrants according to this alternative measure does not alter the results to any considerable extent.

In sum, we argue (i) that the preferred instrument is the one used in DEL based on actually placed refugees, but that the alternative instrument based on contracted refugees allows for a relevant robustness analysis, and we show that the correlation between the number of placed and the number of contracted refugees is highly significant and large; (ii) that using the full cross-section is incorrect and that using the rotating individual panel is the appropriate option, but that we could have avoided some of the other, more minor, sample restrictions; and (iii) that the preferred definition of immigrants is the alternative one based on country of birth rather than citizenship as in DEL. In DEL, we concluded that a one percentage point increase in immigrant shares reduces preferences for redistribution with around 1/3 of a step in the 5-point preference ordering. If we instead define immigrants according to country of birth, and if we estimate the model using all respondents in the rotating individual panel, we instead conclude that preferences are reduced with around 1/4 of a step. In our view, this is merely a marginally different conclusion.

We now proceed with a more thorough response to the main three points of critique put forth by NP-L. In Section 2, we discuss the choice of instrument, in Section 3 we discuss sample attrition and in Section 4 we discuss the definition of immigrants. In Section 5, we conduct a re-analysis on the effects of ethnic heterogeneity on preferences for redistribution to investigate the robustness of the results in DEL to alternative variable definitions and estimation samples. Section 6 concludes our response. In an appendix, we respond to some of the other, what we consider to be more minor, issues raised by NP-L.

2 Choice of instrument

One of the main issues in the comment by NP-L is our choice of instrument. NP-L claim that there is an insignificant correlation between the number of *placed* refugees (the instrument that we use) and the number of *contracted* refugees,[3] and that the placement policy did not affect the settlement patterns of refugees to any considerable extent (i.e., that the refugees could still choose where to locate). They therefore argue that the contracted number of refugees should be used as an instrument instead of the actual placement.

In contrast, we think that it a priori is open to discussion whether contracted or placed refugees should be used as the instrument, and that the description of the relation between the two measures provided by NP-L is flawed.[4] In particular, in this section we will:[5]

1. Show that the correlation between the contracted number of refugees and the number of placed refugees in the municipalities 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.

2. Show that the placement policy certainly changed the settlement pattern of refugees.

3. Discuss likely reasons for the slight differences between the two measures of the placement policy, and the implications for using them as instrument for changes in immigrant shares.

First, there are several possible ways to examine the correlation between contracted and placed refugees. The first, and most obvious, if one is inter-

[3]See Table 1 in NP-L.

[4]We collected the data on placed refugees around 10 years ago, partly from authors of previous studies of the placement program, partly from (physical) yearbooks from the Immigration Board, and partly from (electronic) spreadsheets from the Swedish Integration Board (Integrationsverket). We considered this data to be appropriate for our research question, and do not remember considering that data on the written contracts would also be available.

[5]In Section 5, we analyze the sensitivity of the main results in DEL to the two alternative measures.

ested in examining to what extent the contracted number of refugees was matched by the actual number of placed refugees in the municipalities, is to study the yearly correlation over the period 1986–1994 (the period under study in DEL when the refugee placement program was running). To examine this, we estimate versions of the following equation:

$$placed_{mt} = \alpha contracted_{mt} + \lambda_t + f_m + \epsilon_{mt} \tag{1}$$

where $placed_{mt}$ is the number of placed refugees in municipality m in year t, $contracted_{mt}$ is the number of refugees that the municipality had contracted to take on in year t, λ_t are year fixed effects, f_m are municipality fixed effects and ϵ_{mt} is the error term. We estimate equation (1) both with and without the fixed effects and with the variables in levels as well as normalized with the population in the municipality. Table 1 presents the results and clearly, all correlations are highly significant and close to one. There is hence a clear correspondence between the contracted number of refugees and the placed number of refugees.

Table 1: Correlation between placed and contracted refugees, yearly data for the period 1986–94

	Variables in levels	Variables as share of population
No controls	1.005***	0.927***
	(0.103)	(0.0584)
Year FE:s	0.995***	0.827***
	(0.103)	(0.0542)
Year and municipal FE:s	0.907***	0.785***
	(0.200)	(0.0411)
Observations	2554	2554

Note: Standard errors, clustered on municipality, are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

As a sensitivity analysis in DEL, we drop the last survey panel (i.e., the 1991/94-panel), with the argument that the placement policy was likely to be more exogenous in the first part of the program period. It can therefore be of interest to estimate equation (1) for shorter time periods. Using yearly data, Table 2 examines the correlations for the periods 1986–1990 and 1986–1991. In the majority of the specifications, the correlation is, again, highly statistically significant and close to one. The only exceptions are the specifications with year and municipality fixed effects when contracted/placed refugees are measured in total rather than as population shares; in these specifications, the correlations are insignificant and low.

The variation in the two variables measuring the refugee placement program used to estimate equation (1) does however not correspond to the

Table 2: Correlation between placed and contracted refugees, yearly data for the time periods 1986–90 and 1986–91

	Levels		Share of population	
	1986–90	1986–91	1986–90	1986–91
No controls	0.998***	0.918***	0.991***	0.875***
	(0.0292)	(0.0346)	(0.0435)	(0.0422)
Year FE:s	1.002***	0.924***	0.975***	0.889***
	(0.0301)	(0.0357)	(0.0485)	(0.0449)
Year and municipal FE:s	0.0598	0.00459	0.828***	0.821***
	(0.280)	(0.297)	(0.0488)	(0.0402)
Observations	1413	1697	1413	1697

Note: Standard errors, clustered on municipality, are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

variation used to estimate the empirical model in DEL; see equations (1) and (2) on page 60. To examine the correlation between placed and contracted refugees in the way that most closely relates to this empirical model, we instead estimate the following equation:

$$\begin{aligned} placed_{ms} = \alpha_1 contracted_{ms} + \alpha_2 \bar{H}_{ms} + \alpha_3 \Delta Z_{ms} \\ + \alpha_4 SIZE_{ms} + \alpha_5 POL_{ms} + \alpha_6 SURVEY_s + \epsilon_{ms}, \end{aligned} \quad (2)$$

where $placed_{ms}$ is the total inflow of program refugees to municipality m between survey waves s and $s-1$, and $contracted\ refugees_{ms}$ is the equivalent measure of contracted rather than placed refugees. The resulting estimates of α_1 from equation (2) are given in Table 3. Again, in columns 1–2 *placed* and *contracted* are defined in levels, while in columns 3–4 they are averaged by the size of the population (in particular—as in DEL—by the average population during the 3-year panel period). Columns 1 and 3 are estimated using data from all three survey panels 1985/88, 1988/91 and 1991/94, while columns 2 and 4 exclude the latter. In the three different panels of the table, we include either no controls, only survey panel fixed effects, or the full set of controls as given in equation (2).

Most of the correlations in Table 3 are precisely estimated and very close to one, and in columns 3–4 where the number of refugees are normalized by population size, the inclusion of covariates has very little impact. Note that the variation used in the bottom panel in column 3, where the correlation is estimated to 0.738, is the same variation as is used to estimate the main model in DEL.

All in all, from the results in Tables 1–3 we conclude that the correlation

Table 3: Correlation between placed and contracted refugees, 3-year aggregates over three/two survey panels

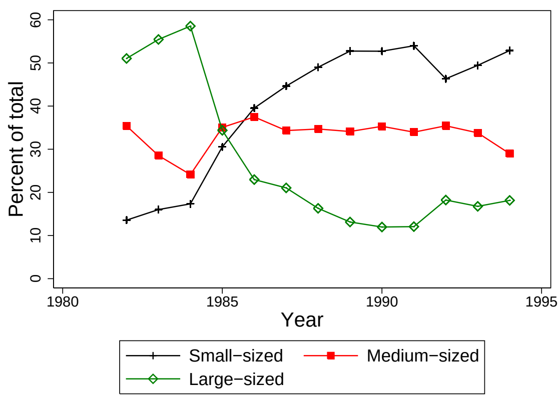
	Levels		Share of population	
	Full sample	1991/94 excluded	Full sample	1991/94 excluded
No controls	1.009***	0.935***	0.843***	0.945***
	(0.0859)	(0.0366)	(0.0883)	(0.0464)
Panel FE:s	1.010***	0.940***	0.812***	0.960***
	(0.0872)	(0.0375)	(0.100)	(0.0506)
Panel FE:s and municipal X:s	0.731***	0.670***	0.738***	0.900***
	(0.111)	(0.130)	(0.0961)	(0.0500)
Observations	839	558	839	558

Note: Standard errors, clustered on municipality, are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

between placed and contracted refugees is high and statistically significant. This is a completely different conclusion than the one drawn by NP-L. It is only in two specifications—the ones in levels with year and municipality fixed effects for the shorter time periods 1986–1990 or 1986–1991—that we get an insignificant and low correlation. The latter of these is the only correlation reported by NP-L, and in view of all the estimations in Tables 1–3, we do not think that this is a good representation of the variation in the data—and it is definitely not a good representation of the variation in the data used in DEL.

Second, regarding whether the placement program indeed affected the refugee settlement patterns, with the descriptive statistics in DEL and in earlier papers using the placement policy for exogenous variation, it is very hard to argue that settlement patterns were unaffected. On the contrary, the descriptive evidence suggests that the placement program affected the placement of refugees in a way that one would expect given the objectives of the program; see for example Figure 1, replicated from DEL (page 50, Figure 3B). This figure illustrates how the total inflow of refugees were distributed across small-sized (population$<$50,000), medium-sized (50,000$\geq$population$<$200,000) and large-sized (population$\geq$200,000) municipalities. The sharp trend break in 1985 clearly indicates that the program changed the location pattern of refugees by fulfilling its purpose of breaking the segregation by redirecting refugees from large to smaller municipalities. Evidence along the same lines is also shown in, e.g., Table 1 in Edin et al. (2004).

Figure 1: Received share of total increase/inflow of refugees



Source: Dahlberg, Edmark and Lundqvist (2012)

Third, a priori, it is in our view open to discussion which of the two measures (contracted vs. placed refugees) that is best suited as an instrument. In general, an instrument is valid if it is *relevant* (i.e., is correlated with the endogenous variable) and *exogenous* (i.e., only affects the outcome through its correlation with the endogenous variable). Both contracted and actually placed refugees potentially fulfill these conditions and are thus candidates for a valid instrument for the endogenous variable changes in immigrant shares. But which one is to be preferred?

Let us first discuss instrument relevance. Even though the two potential instruments are highly correlated, they differ to the extent that the contracts reflect the intentions of a municipality to take on a certain number of refugees, and these intentions were not always perfectly fulfilled. Unfulfilled contracts cannot be expected to be a good predictor of the actual change in the share of immigrants in the municipalities. That is, to the extent that they differ, contracted refugees is a weaker instrument than placed refugees.[6] This notion is confirmed in Section 5 below, which shows that, throughout the various specifications, the first-stage point estimates are smaller and the standard errors are larger when instrumenting with contracted rather than actually placed refugees.[7] On relevance grounds, actually placed refugees is therefore the preferred instrument.

We then turn to the exogeneity condition. NP-L conclude (page 1) "We ... find that there is little or no correlation between contracted levels and actual refugee settlement. This implies that Dahlberg et al. use one endogenous variable, actual refugee settlement, to instrument for another endogenous variable, share of immigrants." We have already shown above that the conclusion about the correlation is wrong. Neither do we agree with the deduction in NP-L that if the correlation between contracted and actually placed refugees is low, then the number of contracted refugees is exogenous while the number of placed refugees is endogenous. The correctness of this argument—and consequently which is the preferred instrument—depends on *why* the two instruments differ. In other words, *why* were not all contracts of intention exactly realized in terms of the number of refugees placed in a specific year?

We interpret NP-L as presuming that if the number of contracted and actually placed refugees differ, it is due to self-selection of refugees into municipalities. This would mean that the exogeneity assumption is violated when instrumenting immigrants with placed refugees. In contrast, our interpretation of the evidence in the data is that self-selection is not the main reason to why they differ. In particular:

- As shown in Figure 1 above, as well as in Table 1 in Edin et al. (2004), there is good reason to believe that the settlement policy did affect the

[6]In addition, unfulfilled contracts cannot be expected to have any behavioral effect on the preferences for redistribution among the municipality's inhabitants, as they were never "treated" with the refugees who were only contracted but were ultimately never placed in their municipality.

[7]See Table 4.

actual placement of refugees in the way that one would expect, given the objectives of the program. That is, the placement of refugees indeed seems to be an informative measure of the placement policy.

- As seen in Table 3 above, when normalized by population size (as in DEL), the correlation between the two measures is very robust to the inclusion of municipal covariates. This says that the difference between the two measures is at least not due to self-selection based on the included covariates.

- Self-selection of refugees would yield second-stage estimates biased towards the OLS-estimate, which is clearly not the case (see Table 2 in DEL). If anything, the second-stage estimates when instrumenting with contracted refugees are closer to the OLS-estimate.

Aside from self-selection, a second potential reason behind unfulfilled contracts that would cast doubts on the exogeneity assumption of placed refugees is that some municipalities chose to break their contracts, or to accept fewer (or more) refugees than agreed. More precisely, if such contract deviations are correlated with our outcome variable, changes in individual preferences for social benefits, this could bias our results. But although the contract data is new to us, in some sense we have already discussed this type of bias, since it is much related to the potential violations of the exogeneity assumption discussed in DEL (see the discussion on potential bias due to municipalities refusing to take on refugees, pages 52–54 in DEL). As explained there, the claim for exogeneity hinges on the assumption that the refugee placement was exogenous *conditional* on the included observable characteristics, among them variables for the local political situation. In addition, the bias from such contract violations would probably work against us finding an effect of increased ethnic heterogeneity (see, again, pages 52–54 in DEL).

One can also think of more practical reasons for contract deviations—reasons that do not pose any evident threat to the exogeneity of actually placed refugees. It could for example be that:

- The situation in the source country changed, which would affect the number of refugees from different countries that needed placement. Since municipalities in general received refugees from one or a small set of countries—so that they could more easily provide language-specific institutions such as interpreters—this could affect the number of refugees that actually needed placement in different municipalities.

- There was some uncertainty regarding exactly when a refugee got a residence permit and in which municipalities there was housing available when a residence permit was granted.

To sum up this section, we interpret the comparison of the data on contracted vs. actual refugee placement as evidence of a strong positive correlation between the two measures of the placement policy, and we show that the actual placement of refugees indeed changed their location patterns. Further, since contracted refugees is a weaker instrument than actually placed refugees, and since the evidence in the data does not suggest that the difference between the two violates the exogeneity assumption of the latter, we argue that actually placed refugees is the preferred instrument. Nevertheless, the additional data on contracted refugees provides an interesting robustness test of our results; see Section 5 below.

3 Sample attrition bias

The second main point of critique in NP-L is that instead of using the rotating individual panel, we should have used the full repeated cross-sectional data in combination with municipality fixed effects. Additionally, they consider a few other, more minor, sample restrictions to be unwarranted.

NP-L refer to Angrist and Pischke (2008) and Blundell and MaCurdy (1999) to note that using the pooled cross-sections instead of the rotating panels requires that the sample mean is a consistent estimator of the population mean. For our application, this means that the sample from each municipality should not only be random but also large enough to enable consistent estimation of the municipality fixed effects. But, although the respondents in our survey data on preferences are drawn by random sampling, for the majority of municipalities, the number of respondents is far too low for the sample mean to be a reliable estimator of the population mean; 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 a mean of around 9. Consequently, by attempting to construct municipality-level differences in preferences for redistribution by using the repeated cross-sections, what one ends up with is a variable containing mostly noise stemming from the fact that the set of (few) respondents' changes between surveys s and $s + 1$.

Instead—as described in Section V. in DEL—thanks to the rotating panel format of the survey data, we can take individual-level differences and thereby net out the individual-specific fixed effects. This controls for all time-invariant unobserved individual-level heterogeneity but, due to the limited sample from many of the municipalities, it does not follow that we are able to consistently estimate the municipality fixed effects.[8]

[8]The difference between using the rotating individual panel and the full cross-sections in combination with municipality fixed effects can be illustrated with the following two different scenarios. Scenario A: Individual i in municipality m rates a proposal with a 3 in survey s, and with a 4 in survey $s+1$. It thus follows that individual i has changed his/her

We thus believe that using the full cross-section in combination with municipality fixed effects is incorrect, and that using the rotating individual panel is the appropriate option. Note that the resulting estimate is then interpreted as the effect on the preferences of the sampled population (i.e., those individuals who state their preferences in two consecutive surveys), and cannot without additional assumptions be generalized to the full population. Admittedly, this distinction between the internal and external validity could have been discussed more thoroughly in DEL.[9]

We thus disagree with NP-L that the restrictions put on the sample by using the rotating individual panel are "unnecessary and unmotivated" (page 9). They also note that we put additional restrictions on our estimation sample by dropping individuals with non-responses also to other survey questions, some of which are not studied in the paper. While we agree that these, more minor, sample restrictions may be unnecessary, the explanation is that when we started the research project, we studied a few additional outcomes and placebo outcomes, and we preferred to have the same sample throughout the analysis. Then, as the project proceeded and following comments and advice in seminars and conferences, we decided to focus on the questions that now appear in DEL, but we did not then make any changes to the sample. Again, one can argue that once we knew that we would not study a particular question, we should have added the non-respondents back to our estimation sample.[10] Therefore, we now do this in Section 5 below—and find that the results in DEL are not sensitive to alternative estimation samples; using the original specification in terms of instrument and immigrant definition, the second-stage estimate of the effect of increased immigrant shares on preferences for redistribution is -0.332 (standard error 0.147) when the maximum 2446 individuals in the rotating panel are included, as compared to -0.347 (standard error 0.155) with 1917 individuals as in DEL.[11]

preferences with 1 step. Scenario B: Individual i in municipality m rates a proposal with a 3 in survey s, and individual $j \neq i$ also in municipality m rates the proposal with a 4 in survey $s+1$. From this it does *not* follow that the average preference in municipality m has changed with 1 step, since we do not know individual j's rating at the time of survey s. And although there should be no difference in the ratings of individual i and j at the time of survey s *on average*, the very few respondents per municipality do not make for a reliable average.

[9]Concerning the external validity of the results in DEL, we highlight a few circumstances that are specific to the period studied in the Appendix.

[10]Additionally, from our STATA-dofiles, NP-L found inconsistencies in these sample restrictions in the sense that they differ in the 1982 survey as compared to in the 1985–94 surveys. In DEL, the 1982 survey is not used in the main analysis but in a placebo analysis (see Section IV.B.) which was added to the paper at a later stage when it was clear which questions we would include in the study. Even so, NP-L are correct that these are inconsistent restrictions.

[11]See the three different panels of Tables 4 and 5. NP-L also analyze the robustness of the results in this dimension, but the number of observations in their various samples

4 Definition of immigrants

In Section 3.3 of their comment, NP-L criticize our measure of the municipal share of immigrants on the grounds that a large share of the immigrants to Sweden are naturalized Swedes, i.e. have acquired Swedish citizenship, and argue that defining immigrants according to country of birth is therefore better.

At the time of writing the paper, we only had access to citizenship, so the alternative definition was not an option. We acknowledge that the variable based on country of birth, which was generously provided to us by NP-L, at the very least provides a good robustness test of the results in DEL and is probably even the preferred variable. Now, it turns out that the results are only marginally affected when we use country of birth rather than citizenship to define immigrants, and that the qualitative conclusion remains; the second-stage estimate of the effect of increased immigrant shares on preferences for redistribution is still negative and statistically significant, although around 3/4 of the size of the original estimate in DEL.[12]

Although defining immigrants according to country of birth rather than citizenship does not change the qualitative results, we discuss below in which respect the identifying variation differs between the two alternative definitions.

An important circumstance in this respect is the rule that refugees are required to live in Sweden for four years before they are eligible to apply for a Swedish citizenship.[13] For our analysis, it is particularly important that the newly arrived refugees (our instrument) were not eligible to become Swedish citizens during the 3-year period when we measure their effect on the share of immigrants from typical refugee countries living in the municipality (the dependent variable in our first-stage regression). This means that, as long as they do not re-migrate to a different municipality (an issue that is discussed in Section III. in DEL), they will show up in the data on foreign citizens during the 3-year period that makes up the "treatment period" in our analysis.

However, if the rate of naturalization among the immigrants already living in a municipality—that is, the extent to which refugees who migrated earlier and who fulfill the settlement requirements to be eligible for Swedish citizenship—is correlated with the number of newly placed refugees in the

differs somewhat from ours. We believe this is due to a few data typing errors resulting in missing values in some of our variables.

[12] We return to these results in Section 5.

[13] See information from the Migration Board: http://www.migrationsverket.se/info/499.html. The 4-year rule was written into the Law of Citizenship (Medborgarskapslagen) in 2001 and, according to Bo Lundberg, expert of issues of citizenship at the Migration Board, was prior to that implemented according to praxis (at least since 1976). A shorter period of three years applies to refugees who have been married to or lived as common-law spouse with a Swedish citizen during two years prior to the application.

municipality, then the measure based on country of birth is to be preferred to avoid bias due to measurement error. Judging from the comparison of the first-stage estimates when immigrants are defined in the two alternative ways (see Table 4 below), using citizenship instead of country of birth gives a slightly smaller first stage coefficient, which, as is also rightly pointed out by NP-L, resulted in the somewhat larger (in absolute terms) second-stage estimate as referred to above.

5 Re-analysis

In this section, we conduct a re-analysis of the model in DEL where we check the sensitivity of the main results to the issues raised by NP-L and discussed above. In particular, in Tables 4 and 5 we show how sensitive the results are to: (i) the use of contracted rather than actually placed refugees as instrument; (ii) the alternative definition of immigrants based on country of birth rather than citizenship; and (iii) to alternative estimation samples. Most of the results presented have been referred to in previous sections, and are gathered here to provide an overall, comprehensive picture of the practical importance of the points raised by NP-L (most of the results presented here are also presented and discussed in their comment).

The first- and second-stage equations of the two-stage least square model that is estimated in DEL are specified as follows (with $\widehat{}$ indicating predicted values from the first stage):[14]

$$\begin{aligned}\Delta IM_{ms} = \alpha_1 Refugee\ inflow_{ms} + \alpha_2 \bar{H}_{ms} + \alpha_3 \Delta Z_{ms} + \alpha_4 SIZE_{ms} \\ + \alpha_5 POL_{ms} + \alpha_6 SURVEY_s + \epsilon_{ms} \qquad (3)\end{aligned}$$

$$\begin{aligned}\Delta PREF_{ims} = \beta_1 \widehat{\Delta IM}_{ms} + \beta_2 \bar{H}_{ms} + \beta_3 \Delta Z_{ms} + \beta_4 SIZE_{ms} \\ + \beta_5 POL_{ms} + \beta_6 SURVEY_s + \varepsilon_{ims} \qquad (4)\end{aligned}$$

Table 4 displays estimates of α_1 in the first-stage equation (3), while second-stage estimates of β_1 in equation (4) are displayed in Table 5. In both of the tables, columns 1–2 are the results when instrumenting immigrants as in DEL (i.e., when the placement program is measured with actually placed refugees), while columns 3–4 instead use contracts of intended placement as instrument. In columns 1 and 3, immigrants are defined as in DEL (i.e., according to citizenship), and in columns 2 and 4 according to country of birth. The upper panel of the two respective tables uses the original DEL estimation sample, while the second and third panels use two

[14]For definitions and discussion of these equations, see Section V. in DEL.

alternative, extended samples; Extended sample *A* where we add back non-respondents on other survey questions than the three questions studied in DEL (the main outcome preferences for social benefits, and the two placebo outcomes preferences for private health care and nuclear power); and Extended sample *B* where we additionally add back non-respondents on the two placebo preference outcomes so that only non-respondents on the main outcome are dropped.[15] Note that the cells in the upper panel, column 1, row 1 of Tables 4 and 5 reproduce the baseline first- and second-stage estimates, respectively, in DEL (see columns 2 and 3 of Table 2, page 64).

Tables 4 and 5 show, first, that the results are robust to the different samples, as the point estimates are very similar across all panels throughout the columns. This holds for the first-stage results in Table 4 as well as for the second-stage results in Table 5. Hence, bias due to sample attrition does not seem to be an issue.

Neither does the alternative definition of immigrants alter the qualitative conclusion, as seen from pairwise comparisons of columns 1–4. With the original DEL sample of 1917 observations, the first stage is somewhat larger when country of birth is used instead of citizenship, which leads to a slightly smaller effect in the second stage. The differences are however rather small; the effect of interest in the second stage is -0.347 with the definition of immigrants base on citizenship as compared to -0.263 when based on country of birth, and the two are not statistically different from one another. All the estimates are statistically significant when using placed refugees as the instrument, irrespectively of the sample used for estimation.

Next, consider the results when we, as a robustness test, use as instrument contracts of intended refugee placement instead of actual refugee placement. Throughout the specifications, the first-stage point estimates in Table 4 are smaller and the standard errors are larger when using this alternative instrument. Thus, along the lines laid out above in Section 2, contracted refugees is a weaker instrument than actually placed refugees. And as seen in columns 3–4 of Table 5, also the second-stage point estimates are smaller and less precise, so that the effect of increased immigrant shares on preferences for redistribution is no longer significantly different from zero.

Looking at the various second-stage specifications across Table 5, the pattern that emerges is that of point estimates that are in most cases fairly similar; they vary between 0.16 to 0.35, but most of them are around 0.25. The standard errors are around 3/4 in size with the alternative definition of immigrant based on country of birth rather than citizenship, and around twice the size with the alternative instrument based on contracted rather than actually placed refugees. Again, as a result, the effect is no longer

[15]Because, as discussed in Section 3 above, we believe that to use the full cross-section is wrong, we do not find it meaningful to estimate a model using this alternative sample.

Table 4: Robustness of first-stage estimates to alternative samples, instruments and immigrant definitions

	Placed refugees		Contracted refugees	
	(1)	(2)	(3)	(4)
Original sample; n=1917	0.497***	0.657***	0.442***	0.402***
	(0.0616)	(0.0620)	(0.0789)	(0.0767)
Extended sample *A*; n=2177	0.489***	0.651***	0.434***	0.390***
	(0.0619)	(0.0635)	(0.0771)	(0.0774)
Extended sample *B*; n=2446	0.485***	0.637***	0.435***	0.390***
	(0.0597)	(0.0621)	(0.0743)	(0.0763)
Immigrant definition:	citizenship	country of birth	citizenship	country of birth

Note: The first-stage estimate from the baseline specification in DEL is given in the upper panel, column 1. The first-stage estimate from the preferred specification in the re-analysis is given in the bottom panel, column 2. Standard errors, clustered on municipality, are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 5: Robustness of second-stage estimates to alternative samples, instruments and immigrant definitions

	Placed refugees		Contracted refugees	
	(1)	(2)	(3)	(4)
Original sample; n=1917	-0.347**	-0.263**	-0.221	-0.244
	(0.155)	(0.119)	(0.208)	(0.232)
Extended sample *A*; n=2177	-0.285*	-0.214*	-0.198	-0.220
	(0.150)	(0.113)	(0.208)	(0.234)
Extended sample *B*; n=2446	-0.332**	-0.252**	-0.157	-0.175
	(0.147)	(0.112)	(0.200)	(0.225)
Immigrant definition:	citizenship	country of birth	citizenship	country of birth

Note: The second-stage estimate from the baseline specification in DEL is given in the upper panel, column 1. The second-stage estimate from the preferred specification in the re-analysis is given in the bottom panel, column 2. Standard errors, clustered on municipality, are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

statistically significant when placement contracts are used as instrument.

What should one conclude from this re-analysis? Instrumenting immigrants with contracted rather than placed refugees yields second-stage estimates that are somewhat smaller and that are not statistically significant from zero due to larger standard errors. This leads NP-L to conclude that, contrary to our findings in DEL, there is no relationship between ethnic diversity and preferences for redistribution (page 16). We take a different view. In particular, the less negative and less precise estimates resulting from instrumenting immigrants with contracts of intended placement (that were not always fulfilled) are consistent with what one would expect; contracted refugees is a weaker instrument and, just as here, weak instruments typically yield 2SLS estimates that are biased towards OLS (see the discussion in, e.g., Angrist and Pischke, 2008).[16]

Furthermore, as argued above, to the extent that the two measures of the placement policy differ,[17] the evidence in the data suggests that it is not due to reasons that would violate the exogeneity assumption of actually placed refugees. In light of this, we argue that the preferred instrument is the one based on actually placed refugees—i.e., the one used in DEL. And while the alternative instrument based on the contracts is weaker, we find it reassuring that the estimates point in the same direction in all specifications.

Regarding the definition of immigrants, we agree with NP-L that the one based on country of birth is preferred over the one based on citizenship. And regarding the estimation sample, while we do not think that it is appropriate to use the full cross-section in combination with municipality fixed effects, one may argue in favor of using all individuals in the rotating panel who responded to our preference question (i.e., extended sample B).

In DEL, we concluded that a one percentage point increase in immigrant shares reduces preferences for redistribution with around 1/3 of a step in the 5-point preference ordering. If we instead define immigrants according to country of birth, and if we estimate the model using the extended sample B, we instead conclude that preferences are reduced with around 1/4 of a step. In our view, this is merely a marginally different conclusion.

6 Conclusion

In a comment to Dahlberg, Edmark and Lundqvist (2012)[18], Nekby and Pettersson-Lidbom (2012) argue (i) that the refugee placement program should be measured with contracted rather than actually placed refugees,

[16] The baseline OLS estimate (standard error) in DEL is -0.04 (0.07).

[17] Recall, however, from Section 2 that the two measures of the placement program are in fact highly correlated. In particular, when using the same variation to correlate the two measures as when estimating the model in equations (3) and (4), the correlation is approximately 0.74.

[18] DEL.

and claim that the correlation between the two measures is insignificant and close to zero; (ii) that instead of using the rotating individual panel, we should have used the full cross-sections in combination with municipality fixed effects; and (iii) that immigrants should be defined based on country of birth rather than citizenship.

The conclusions from the results and discussions in this response is (i) that, in our view, the preferred instrument is the one used in DEL based on actually placed refugees, but that the alternative instrument based on contracted refugees allows for a relevant robustness analysis, and that the correlation between the number of placed and the number of contracted refugees is highly significant and large; (ii) that using the full cross-section is incorrect and that using the rotating individual panel is the appropriate option, but that we could have avoided some of the other, more minor, sample restrictions; and (iii) that the preferred definition of immigrants is the alternative one based on country of birth rather than citizenship as in DEL.

In DEL, we concluded that a one percentage point increase in immigrant shares reduces preferences for redistribution with around 1/3 of a step in the 5-point preference ordering. If we instead define immigrants according to country of birth, and if we estimate the model using all respondents in the rotating individual panel, we instead conclude that preferences are reduced with around 1/4 of a step. In our view, the main conclusion remains.

In closing, we wish to comment on the general suitability of using the refugee placement policy in place in Sweden between 1985 and 1994 in research. As argued in this response, in DEL as well as in earlier paper, the policy certainly seems to have induced the refugees to settle in places that were not their preferred choice. This feature of the placement policy—that the refugees were not able to self-select into certain municipalities—is interesting and attractive, and indeed has the potential of being exploited in various research designs. However, given that it was not a randomized experiment in which refugees were allocated to different municipalities through lotteries, whether or not the placement policy induces exogenous variation should be carefully considered in each specific application.

References

Angrist, J. and J. Pischke (2008): *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press.

Blundell, R. and T. MaCurdy (1999): "Labor Supply: A Review of Alternative Approaches," in *Handbook of Labor Economics*, ed. by O. Ashenfelter and D. Card, vol. 3.

Dahlberg, M., K. Edmark, and H. Lundqvist (2012): "Ethnic Diversity and Preferences for Redistribution," *Journal of Political Economy*, 120, 41–76.

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

Folke, O. (2011): "Shades of Brown and Green: Party Effects in Proportional Election Systems," mimeo, Columbia University.

Nekby, L. and P. Pettersson-Lidbom (2012): "Revisiting the Relationship between Ethnic Diversity and Preferences for Redistribution," Research Papers in Economics 2012:9, Stockholm University, Department of Economics.

Nilsson, A. (2004): "Immigration and Emigration in the Postwar Period," Demographic Reports 2004:5, Statistics Sweden.

A Miscellaneous

In this Appendix, we respond to some of the other, what we consider to be more minor, issues raised by NP-L. We also extend the discussion of external validity that we touch upon in Section 3 on sample attrition, by highlighting a few circumstances that are specific to the period studied in DEL.

Housing vacancies

We start with the argument made by NP-L in Section 2.2, that the fact that housing vacancies have little explanatory power in a regression of refugee placement implies that the placement policy is not, contrary to what we argue in DEL, exogenous conditional on a set of municipal characteristics.

The reason why we chose to include housing vacancies in public rentals as one (of several) covariates is that this is a factor that was pointed out as relevant by several of the program officials that we contacted—with this information, it would have been inappropriate *not* to control for housing vacancies. And we do not agree with the conjuncture that, just because it ex post turns out that housing vacancies did not in fact determine refugee placement, then refugee placement is endogenous. Furthermore, as with the analysis of how the two measures of the placement policy—contracts vs. actual placement—correlates with one another, there are several possible ways to examine the correlation between placement and housing vacancies. Going through the same set of specifications as we did for contracted/actual placed refugees in Section 2 above, we find positive and statistically significant correlations between placement and housing vacancies in several cases (but not in all). Thus, it is indeed possible to confirm in the data the information from the officials whom we spoke to. However, that is not to say that there is a causal effect of housing vacancies on refugee placement—which we never claimed.

Refugee re-migration

In connection to their discussion on sample attrition bias, NP-L writes (page 11): "Another questionable sample restriction made by Dahlberg *et al.* is to exclude all individuals that moved to a different municipality between survey periods." We do not think that this is a questionable restriction, but rather, a necessary one; the first-stage equation where we isolate the change in immigrant shares between survey s and $s-1$ in the individual's municipality that is due to the refugee inflow to the municipality is not defined for individuals who live in different municipalities at the time of survey s and $s-1$. Note, also, that DEL discuss potential bias from "white flight"—i.e., from natives moving out of a municipality as a response to an influx of refugees (see page 55).

Politics

In footnote 11, NP-L refer to Folke (2011), who "finds that actual settlement is correlated with the political make-up of the local government" (page 8). Note that we acknowledge this when discussing the empirical model in DEL, where we write (page 61): "Second, we include a vector of political variables, POL_{ms}, to control for the possibility that the political views of certain parties might be correlated with both placement policy and preferences for redistribution."

Standard errors

NP-L report both cluster-robust standard errors (as in DEL) as well as homoskedasticity-only standard errors, and refer to Angrist and Pischke (2008) to motivate this with the fact that the former may be more biased. In DEL, we report standard errors that are robust to clustering at the municipality level, since this is the level of the treatment. Following recommendations received in the review process, as noted in footnote 23, we also estimated the model with standard errors robust to clustering at the more aggregate level of counties, and found the results to be unaffected. Usually, homoskedasticity-only standard errors are less conservative (i.e., smaller) than their clustered counterparts. This is also what we find for our preferred specification with the new data; measuring the placement policy with actual placements, defining immigrants according to country of birth and using all respondents in the rotating individual panel, the municipality-clustered standard error of the first-stage estimate is 0.062 while the homoskedasticity-only one is 0.018, and the two are almost identical for the second-stage estimate (0.112 vs. 0.115).

Data availability

Information on how to get access to the data and do-files used in DEL is given on the homepage of the *JPE*. In footnote 12, NP-L comment on the fact that the data is not posted directly at the *JPE* web, but is instead available from the Swedish National Data Service (SND). The reason is that we were informed by the SND that it was not possible to post the data on the *JPE* web, and they instead suggested that the data would be deposited at the SND, and there be available for interested researchers upon request.[19] We found this suggestion a good way to proceed, and the *JPE* agreed. The SND then asked us to choose whether or not we wished to be notified and to approve any requests for access of these data. Since the data contains information from various sources, and in part consists of data that we ourselves coded, we preferred to be informed about how it would be distributed, and therefore stated that we wished to be notified. In other words, our intention was *not* to limit the access to the data. On the contrary, the data can be accessed by anyone interested by following the instructions on the *JPE* web.[20]

External validity

In addition to responding to the issues raised by NP-L, we wish to extend the discussion of external validity that we touch upon in Section 3 above on sample attrition by highlighting a few circumstances that are specific to the period studied in DEL.

First, we analyze a time period when refugee immigration to Sweden took off. Previously, in the 1950s and 1960s, immigration had mainly consisted of labor migrants, primarily from the other Nordic countries but also from other European countries. Immigration from countries outside Europe was very low. During the 1970s, refugee immigration increased at a slow rate, while work immigration slowly decreased. Then, from around 1985 and onwards, the refugee immigration increased more rapidly, peaking in the early 1990s mainly because of the Balkan conflict.[21]

Second, the integration policy during our studied period—in the form of municipal introductory programs—has been criticized for poorly channeling

[19] Note that the normal procedure for getting access to the data on the election surveys used in DEL is to make a formal request to the SND. Additionally, our project needed approval from the Central Ethical Review Board before we were granted access.

[20] At the time of writing, two such requests have been received and granted.

[21] See Nilsson (2004) for an overview of the immigration to Sweden after World War 2.

individuals into the labor force. A number of changes have since then been undertaken (see, for example, Government Proposition 2009/10:60).

Thus, the period studied can be characterized as one during which the share of refugee immigrants increased from an initial very low rate, and where, according to some, the public policy did not facilitate entry into the labor force. Therefore, we wish to emphasize the possibility that the negative effect of increased immigrant shares on the preferences for redistribution as found in DEL is specific to these circumstances.

WORKING PAPERS
Uppsala Center for Fiscal Studies
Editor: Håkan Selin

2010:1 Sören Blomquist and Laurent Simula, Marginal Deadweight Loss when the Income Tax is Nonlinear. 21 pp.

2010:2 Marcus Eliason and Henry Ohlsson, Timing of death and the repeal of the Swedish inheritance tax. 29 pp.

2010:3 Mikael Elinder, Oscar Erixson and Henry Ohlsson, The Effect of Inheritance Receipt on Labor and Capital Income: Evidence from Swedish Panel Data. 28 pp.

2010:4 Jan Södersten and Tobias Lindhe, The Norwegian Shareholder Tax Reconsidered. 21 pp.

2010:5 Anna Persson and Ulrika Vikman, Dynamic effects of mandatory activation of welfare participants. 37 pp.

2010:6 Ulrika Vikman, Does Providing Childcare to Unemployed Affect Unemployment Duration? 43 pp.

2010:7 Per Engström, Bling Bling Taxation and the Fiscal Virtues of Hip Hop. 12 pp.

2010:8 Niclas Berggren and Mikael Elinder, Is tolerance good or bad for growth? 34 pp.

2010:9 Heléne Lundqvist, Matz Dahlberg and Eva Mörk, Stimulating Local Public Employment: Do General Grants Work? 35pp.

2010:10 Marcus Jacob and Martin Jacob, Taxation, Dividends, and Share Repurchases: Taking Evidence Global. 42 pp.

2010:11 Thomas Aronsson and Sören Blomquist, The Standard Deviation of Life-Length, Retirement Incentives, and Optimal Pension Design. 24 pp.

2010:12 Spencer Bastani, Sören Blomquist and Luca Micheletto, The Welfare Gains of Age Related Optimal Income Taxation. 48 pp.

2010:13 Adrian Adermon and Che-Yuan Liang, Piracy, Music, and Movies: A Natural Experiment. 23 pp.

2010:14 Spencer Bastani, Sören Blomquist and Luca Micheletto, Public Provision of Private Goods, Tagging and Optimal Income Taxation with Heterogeneity in Needs. 47 pp.

2010:15 Mattias Nordin, Do voters vote in line with their policy preferences? The role of information. 33 pp.

2011:1 Matz Dahlberg, Karin Edmark and Heléne Lundqvist, Ethnic Diversity and Preferences for Redistribution. 43 pp.

2011:2 Tax Policy and Employment: How Does the Swedish System Fare? Jukka Pirttilä and Håkan Selin. 73 pp.

2011:3 Alessandra Casarico, Luca Micheletto and Alessandro Sommacal, Intergenerational transmission of skills during childhood and optimal public policy. 48 pp.

2011:4 Jesper Roine and Daniel Waldenström, On the Role of Capital Gains in Swedish Income Inequality. 32 pp.

2011:5 Matz Dahlberg, Eva Mörk and Pilar Sorribas-Navarro, Do Politicians' Preferences Matter for Voters' Voting Decisions?

2011:6 Sören Blomquist, Vidar Christiansen and Luca Micheletto, Public provision of private goods, self-selection and income tax avoidance. 27 pp.

2011:7 Bas Jacobs, The Marginal Cost of Public Funds is One. 36 pp.

2011:8 Håkan Selin, What happens to the husband's retirement decision when the wife's retirement incentives change? 37 pp.

2011:9 Martin Jacob, Tax Regimes and Capital Gains Realizations. 42 pp.

2011:10 Katarina Nordblom and Jovan Zamac, Endogenous Norm Formation Over the Life Cycle – The Case of Tax Evasion. 30 pp.

2011:11 Per Engström, Katarina Nordblom, Henry Ohlsson and Annika Persson, Loss evasion and tax aversion. 43 pp.

2011:12 Spencer Bastani and Håkan Selin, Bunching and Non-Bunching at Kink Points of the Swedish Tax Schedule. 39 pp.

2011:13 Svetlana Pashchenko and Ponpoje Porapakkarm, Welfare costs of reclassification risk in the health insurance market. 41 pp.

2011:14 Hans A Holter, Accounting for Cross-Country Differences in Intergenerational Earnings Persistence: The Impact of Taxation and Public Education Expenditure. 56 pp.

2012:1 Stefan Hochguertel and Henry Ohlsson, Who is at the top? Wealth mobility over the life cycle. 52 pp.

2012:2 Karin Edmark, Che-Yuan Liang, Eva Mörk and Håkan Selin, Evaluation of the Swedish earned income tax credit. 39 pp.

2012:3 Martin Jacob and Jan Södersten, Mitigating shareholder taxation in small open economies? 15 pp.

2012:6 Spencer Bastani, Gender-Based and Couple-Based Taxation. 55 pp.

2012:7 Indraneel Chakraborty, Hans A. Holter and Serhiy Stepanchuk, Marriage Stability, Taxation and Aggregate Labor Supply in the U.S. vs. Europe. 64 pp.

2012:8 Niklas Bengtsson, Bertil Holmlund and Daniel Waldenström, Lifetime versus Annual Tax Progressivity: Sweden, 1968–2009. 56 pp.

2012:9 Firouz Gahvari and Luca Micheletto, The Friedman rule in a model with nonlinear taxation and income misreporting. 36 pp.

2012:10 Che-Yuan Liang and Mattias Nordin, The Internet, News Consumption, and Political Attitudes. 29 pp.

2012:11 Gunnar Du Rietz, Magnus Henrekson and Daniel Waldenström, The Swedish Inheritance and Gift Taxation, 1885–2004. 50 pp.

2012:12 Thomas Aronsson, Luca Micheletto and Tomas Sjögren, Public Goods in a Voluntary Federal Union: Implications of a Participation Constraint. 7 pp.

2013:1 Markus Jäntti, Jukka Pirttilä and Håkan Selin, Estimating labour supply elasticities based on cross-country micro data: A bridge between micro and macro estimates? 35pp.

2013:2 Matz Dahlberg, Karin Edmark and Heléne Lundqvist, Ethnic Diversity and Preferences for Redistribution: Reply. 23 pp.