Economic Studies 128

Heléne Lundqvist
Empirical Essays in Political and Public Economics

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: Despite the key role played by political payoffs in theory, very little is known empirically about the types of payoffs that motivate politicians. The purpose of this paper is to bring some light into this. I estimate causal effects of being elected in a local election on monetary returns. The claim for causality, I argue, can be made thanks to a research design where the income of some candidate who just barely won a seat is compared to that of some other candidate who was close to winning a seat *for the same party*, but ultimately did not. This research design is made possible thanks to a comprehensive, detailed data set covering all Swedish politicians who have run for office in the period 1991–2006. I establish that monetary returns are absent both in the short and long run. In stead, politicians seem to be motivated by non-monetary payoffs that can be realized with a successful political career.

Essay 2 (with Matz Dahlberg and Karin Edmark): In recent decades, the immigration of workers and refugees to Europe has increased substantially, and the composition of the population in many countries has consequently become much more heterogeneous in terms of ethnic background. If people exhibit in-group bias in the sense of being more altruistic to one's own kind, such increased heterogeneity will lead to reduced support for redistribution among natives. This paper exploits a nationwide program placing refugees in municipalities throughout Sweden during the period 1985–94 to isolate exogenous variation in immigrant shares. We match data on refugee placement to panel survey data on inhabitants of the receiving municipalities to estimate the causal effects of increased immigrant shares on preferences for redistribution. The results show that a larger immigrant population leads to less support for redistribution in the form of preferred social benefit levels. This reduction in support is especially pronounced for respondents with high income and wealth. We also establish that OLS estimators that do not properly deal with endogeneity problems—as in earlier studies—are likely to yield positively biased (i.e., less negative) effects of ethnic heterogeneity on preferences for redistribution.

Essay 3: While the literature on how intergovernmental grants affect the budget of receiving jurisdictions is numerous, the very few studies that explicitly deal with likely endogeneity problems focus on grants targeted towards specific sectors or specific type of recipients. The results from these studies are mixed and make it clear that the knowledge about grants effects is to this date still insufficient. This paper contributes to this literature by estimating causal effects on local expenditures and income tax rates of general, non-targeted grants. This is done in a difference-in-difference model utilizing policy-induced increases in grants to a group of remotely populated municipalities in Finland. The robust finding is that increased grants have a negligible effect on local income tax rates, but that there is a substantial positive immediate response in local expenditures. Furthermore, there is no evidence of dynamic crowding-out—i.e., that the immediate response in expenditures is reversed in later years. The flypaper behavior displayed by the treatment group can potentially be explained by "separate mental accounting"—i.e., voters treating the government budget constraint separately from their own.

Essay 4 (with Matz Dahlberg and Eva Mörk): Public employment plays an important role in most countries, as it is closely linked to both the quality of publicly provided welfare services and total employment. Large parts of those employed by the public sector are typically employed by lower-level governments, and one potential instrument with which central decision-makers can affect public employment is thus grants to lower-level governments. This paper investigates the effects of general grants on local public employment. Applying the regression kink design to the Swedish grant system, we are able to estimate causal effects of intergovernmental grants on personnel in different local government sectors. Our robust conclusion is that there was a substantial increase in personnel in the central administration after a marginal increase in grants, but that such an effect was lacking both for total personnel and personnel in child care, schools, elderly care, social welfare and technical services. We suggest several potential reasons for these results, such as heterogeneous treatment effects and bureaucratic influence in the local decision-making process.

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I frequently hear—and used to agree with—people saying "As long as you do your best, the result does not matter". But the more I think about this saying, the less I understand it. For one thing, once you have accomplished something really, really good, how, from that moment on, is it possible to say that you did your best unless future outcomes are also really, really good? But more importantly, an optimality condition that economists know as $MB = MC$ tells us that is *not* worthwhile to foolishly do one's best. And so I am sure I could have written a (marginally) better thesis than this one. But it would just not have been worth it.

Many people have made the past five years enjoyable and, hence, worthwhile. My deepest thanks go to Matz Dahlberg. As my main advisor, his way of teaching me how to be a researcher has fit me perfectly. Matz, thank you for believing in me and for providing the right mix of guidance and freedom to do my own thing. It has made me believe that I really can! Matz has also, with much help from my co-advisor Eva Mörk, kept up my mood in moments of doubt. Eva, the combination of your intelligence, humor and sarcasm makes you a truly special person whom I am happy to have (had) as a colleague and friend.

The co-authorship with both Matz and Eva as well as with Karin Edmark has been very inspiring and has, in different ways, improved all four thesis chapters. I also owe many thanks for the quality of the thesis to my licentiate opponent Tuomas Pekkarinen and to my final seminar opponent Björn Öckert, and for polishing my writing I thank Christina Lönnblad.

Visiting MIT I learned a lot, but the main thing that I brought back home was how important—and fun—doing economic research can—and should—be. I thank Sören Blomquist and Jim Poterba for making that stay possible. I also feel privileged to have spent my final semester as a grad student at the IIES, and for that I thank all those who made that happen.

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Fredhäll, September 2011
Heléne Lundqvist

CONTENTS

INTRODUCTION

This thesis consists of four self-contained essays.

Although the specific research questions are rather diverse, all four thesis chapters are devoted to the functioning of the public sector and its actors in different ways. *Essay 1* focuses on those who are ultimately responsible for the public sector; the elected politicians. The paper investigates what types of payoffs that motivate politicians by estimating the effects of being elected in a local election on income and political career prospects. *Essay 2* focuses on how preferences for the public sector are formed. Specifically, the paper investigates how increased ethnic heterogeneity affects people's preferred level of redistribution—an important task for the public sector. *Essay 3* and *Essay 4* instead focus on the financing of the public sector, in particular the local public sector. These two papers investigate how increased intergovernmental grants to lower-level governments affect local expenditures and tax rates (*Essay 3*) and local public employment (*Essay 4*).

Aside from all four chapters concerning the public sector of the economy, a distinguished unifying theme of the thesis is the effort devoted to separating causal treatment effects from spurious empirical correlations by applying proper identification strategies to different data sources. *Essay 1* applies a regression discontinuity strategy to data on rankings of political candidates where there is a well-defined cut-off at which the probability of being elected increases discontinuously. Under the assumption that the direct effect on the outcome of being ranked higher is continuous, this strategy identifies the causal effect of being elected. *Essay 2* uses data on a nationwide refugee placement program and applies an instrumental variable strategy. The program, placing newly arrived refugees throughout Sweden between 1985 and 1994, induced substantial variation in ethnic heterogeneity across Swedish municipalities. Under the assumption that the placement was uncorrelated with the preferences for redistribution among the municipalities' population, exploiting this variation identifies the causal effect of increased ethnic heterogeneity on preferences for redistribution. *Essay 3* utilizes policy-induced increases in intergovernmental grants in a difference-in-difference strategy. The policy treated a group of municipalities in Finland with increased supplemental grants in 2002, whereas the remaining municipalities serving as controls never received the particular grant supplement. Under the assumption that the counterfactual trends run parallel between the treatment and control municipalities—i.e, that nothing but the supplemental grant increase affected the two groups of municipalities differently—this strategy identifies the causal effect of increased grants. Also *Essay 4* identifies and estimates causal effects of increased grants, but in a regression kink design where municipalities with out-migration rates above a threshold value receive additional grants with every percentage point additional out-migration, whereas municipalities below the threshold do not. This strategy is similar to the regression discontinuity strategy applied in *Essay 1*, except that here, the discontinuity is in the marginal effect rather than in the level.

The identification strategies employed in this thesis can all be characterized as reduced form approaches. What distinguishes such approaches from the other main category of empirical methods—structural approaches—is that only the main relationship of interest is modeled while, in some sense, there is a ceteris paribus assumption on "the rest of the world". Typical for reduced form approaches is that they rely on (often one or only a few) *exogenous* institutional features to identify a treatment effect of interest.

Finding such features requires both a fair amount of creativity as well as expertise about the details of the rules, laws, policies etc. in the economy. The reason is that, almost by definition, most institutions are *endogenous*.[1] Consider, for example, a system of intergovernmental grants—which is studied in *Essay 3* and *Essay 4*. Here, the root of the endogeneity problem is that grant systems are not randomly formed but, rather, that grants are distributed to lower-level governments motivated by some underlying need. Such needs are likely to be directly related to the outcome of interest (e.g., public expenditures as in *Essay 3*), implying that perceived correlations between grants and the outcome partly stem from the *determinants* of the grant distribution rather than the causal effect of grants in itself.[2]

Isolating exogenous variation requires institutions or rules that are, in some sense, unjustified. Sticking to the grant example, in *Essay 4* a causal effect can only be recovered under the assumption that the structure of the kinked out-migration grant used for identification is ad hoc—if the underlying demand for personnel were a kinked function of out-migration just as is the out-migration grant, the causal effect of grants would not be identified. Understandably, policy makers want to conduct policies that are justified and fair. But there is a trade-off here; while randomization might not be the most fair way of implementing a policy, it is ideal if one wants to credibly evaluate the policy.

The four essays in this thesis attempt to credibly estimate causal effects on different outcomes by employing identification strategies that, more or less, mimic randomization of some treatment.

Those responsible for the public sector

Essay 1 focuses on those who are ultimately responsible for the public sector; the elected politicians.

A prevalent feature in political economy models is that there are some benefits or returns that politicians aim for by maximizing their probability of winning elections. More often than not, these objectives simply appear in the model as some parameter B (for benefits) or R (for returns). But what, precisely, are these payoffs that motivate politicians? Considering the key role played by political payoffs in theory, surprisingly

[1] See Besley and Case (2000) for a discussion of endogenous institutions and policies.

[2] See Dahlberg et al. (2008), Gordon (2004) and Knight (2002) for further discussions of the potential endogeneity of grants.

little is known about this empirically. The purpose of *Essay 1* is to bring some light into this.

Political payoffs can be either monetary or non-monetary. The latter can, for example, be in the form of political accomplishments, a sense of actively taking part in the community, the desire to affect society in a certain direction, prestige and power. Even if not impossible, these things are very hard to measure. However, in some sense, one may view the probability of making a successful political career as encompassing all these types of payoffs. Therefore, to investigate what the different payoffs from politics are, in *Essay 1* I estimate causal effects of being elected both on short- and long-run income as well as on future probabilities of being elected to a municipal council and of being nominated to the national parliament.

I argue that the claim for causality in the paper can be made by applying a regression discontinuity design where the outcome (income, say) of some candidate that just barely got elected is compared to that of some other candidate who was close to being elected, but ultimately was not. Implementing this identification strategy is possible thanks to a comprehensive, detailed data set covering all Swedish politicians who have run for office in the period 1991–2006.

Essay 1 shows that monetary returns from politics are absent irrespective of if one considers the period right after the election, up to 15 years later or the period right after exiting politics. This result holds for different income measures such as disposable income, total labor income or labor income from the largest source. It is also true on average as well as when considering heterogeneous effects across various dimensions of parties, councils and candidates. In contrast, there are quite large effects on the chances for a successful political career of—for exogenous reasons—being elected into a local council. For example, the analysis suggests that the probability of being nominated to the national parliament is almost fully explained by variation in experience from a local council.

The combined analysis on income and political career prospects thus shows that, given that they are absent, politicians are likely not motivated by monetary returns. Instead, politicians seem to be motivated by non-monetary payoffs that can be realized with a successful political career.

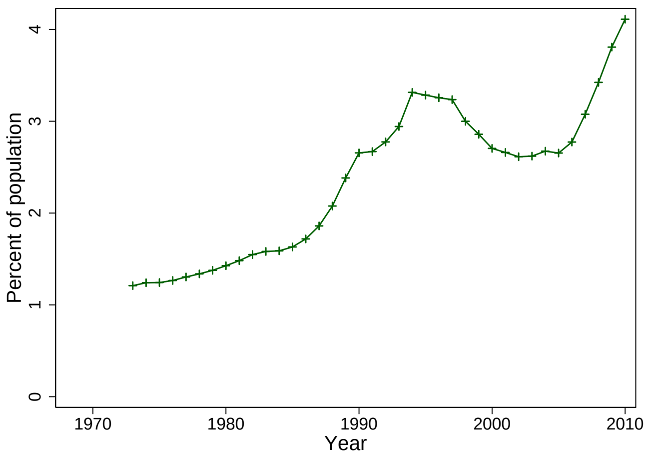
The finding in *Essay 1* of no positive monetary returns from politics stands in contrast to the few previous studies: Diermeier et al. (2005) find that political experience increases earnings for US congressmen upon exiting politics, and Eggers and Hainmueller (2009) estimate substantial effects on wealth (at the time of death) for conservative politicians who were elected into the British parliament with narrow vote margins. These contrasting results can potentially be explained with their focus on national rather than, as in *Essay 1*, local politics. Another interesting potential explanation is the difference in political institutions. Sweden is characterized by a typical multi-party, proportional representation system where the parties are the main political players. Therefore, it is likely that monetary returns are larger in countries like the US and Great Britain where the focus lies (more or less) on individual politicians.

Formation of preferences for the public sector

Essay 2 focuses on how preferences for the public sector are formed, specifically for the redistributive part of the public sector.

The motivation for the paper is the increased immigration of workers and refugees experienced by many countries in recent decades. For example, due to immigration primarily from Latin America and Asia, the share of foreign-born of the US population rose from around 5% in 1970 to 11% in 2000 (Gibson and Jung, 2006). A similar experience is documented for Sweden, where the share of foreign-born was around 6.5% in 1970 and as high as 15% in 2010 (Statistics Sweden). Also quite remarkable is the evolution of the share of the Swedish population with a citizenship from a non-OECD country,[3] which is seen in Figure 1. From a mere 1.5% in the 1970's, the share temporarily peaked at 3.5% in the mid 1990's and is today more than 4%—i.e., an increase of around 170% over 40 years.

Figure 1: Share of population in Sweden with non-OECD citizenship



Source: Statistics Sweden.

A direct consequence of this trend is that, as people from non-OECD countries are arguably rather ethnically different from native Swedes, the population in Sweden (like in many other countries) has become much more heterogeneous in terms of ethnic background. *Essay 2* asks how this increased ethnic diversity has affected preferences for redistribution among the native population. There are several potential mechanisms for why the support for redistribution could be affected by the ethnic diversity of the recipients. A rather direct mechanism is that people exhibit so-called in-group bias, meaning that people have a tendency to be more altruistic towards others in their own group.[4] "One's own group" can be defined in terms of ethnicity, or along some other

[3] According to OECD membership status in 1994.

[4] Shayo (2009) formulates a theoretical model for this idea and, e.g., Alesina et al. (2001), Eger (2010),

dimension—for example, most people are more inclined to spend time and money on friends and family than on strangers. However, if ethnicity defines groups with which people identify, the implication is that altruism does not travel well across ethnic lines.

The contribution of *Essay 2* is to provide new and, compared to what has previously been established, more convincing empirical evidence of the causal link behind the idea of in-group bias. We identify the causal effect of increased immigrant shares by making use of a nearly nationwide program intervention placing refugees in municipalities throughout Sweden between 1985 and 1994. During this period, the placement program provides exogenous variation in the number of refugees placed in the 288 municipalities. By exploiting the source of variation in immigrant shares in the municipalities induced by the refugee placement program and by matching this data to individual panel survey data, we can estimate the causal effect on individual preferences for redistribution.

Essay 2 establishes that increased immigrant shares, stemming from inflows of refugees to municipalities via the placement program, lead to less support for redistribution, defined as preferred levels of social benefits.[5] This reduction in support is especially pronounced among individuals with high income and wealth. The paper also establishes that OLS estimators that do not properly deal with endogeneity problems are likely to yield positively biased (i.e., less negative) effects of ethnic heterogeneity on preferences for redistribution.

Considering the result in *Essay 2*, it is worth highlighting that—just like people being more generous towards their own family and friends does not mean that they are hostile towards others—lower levels of preferred redistribution when the recipients are more ethnically diverse must not necessarily be interpreted as xenophobia.

The finances of the public sector

Essay 3 and *Essay 4* focus on the financing of the public sector, in particular the local public sector.

In many countries, a considerable part of public sector expenditures goes to goods and services that are provided locally, such as child care, schooling and elderly care. But there is often a vertical imbalance between expenditures and revenues, in the sense that local governments usually cannot (or do not have the legal right to) collect enough taxes to cover their costs.[6] A corrective device is then intergovernmental grants—i.e., central funds that are being transfered to local governments.[7] *Essay 3* and *Essay 4* investigate how increased intergovernmental grants to lower-level governments affect local expenditures and tax rates (*Essay 3*) and local public employment (*Essay 4*).

According to a parsimonious theoretical model, an increase in lump-sum grants is

Luttmer (2001) and Senik et al. (2009) investigate the idea empirically.

[5] Specifically, preferences for redistribution are defined as survey respondents' rating of the proposal to "decrease the level of social benefits" on a five-point scale ranging from "very bad" to "very good".

[6] Even when local governments are able to collect taxes so as to be self-supportive, (partly) centralized taxation can be more efficient.

[7] Grant systems can be (and are often) also designed to reduce inequality between local governments and thus to also correct for horizontal imbalances.

equivalent to a tax base increase and is predicted to induce a pure income effect, causing public expenditures to increase by the overall propensity to spend on public goods and services. To see this, consider a representative agent in a local government with preferences over private consumption C and public goods G, $U = (C, G)$, and with income Y which is taxed at rate t. The government uses the tax revenues and central grants B to finance the provision of G. The agent's and the local government's budget constraints are then

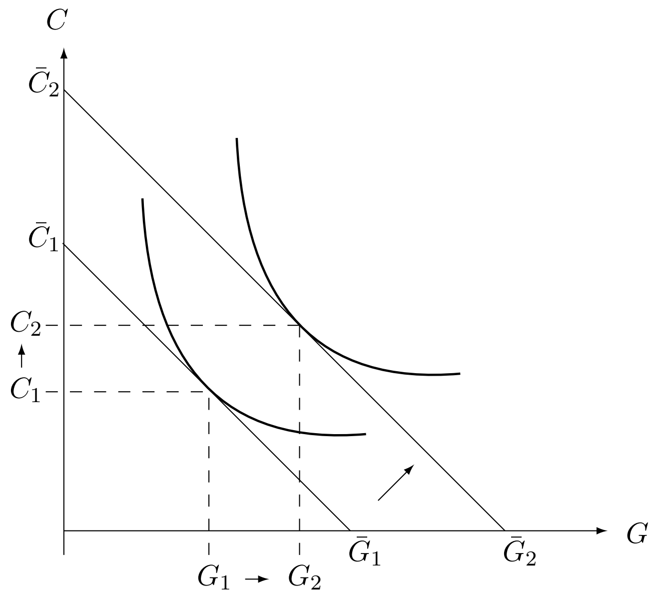
$$C = (1 - t)Y \tag{1}$$

and

$$G = tY + B, \tag{2}$$

respectively. The combination of equations (1) and (2) is portrayed as the budget line $\bar{C}_1\bar{G}_1$ in Figure 2, which has slope $-t$. In the figure, the utility of the agent is maximized at (C_1, G_1). An increase in either Y or B is simply illustrated in the figure as a shift of the budget line to $\bar{C}_2\bar{G}_2$, in which case the agent maximizes the utility at (C_2, G_2).

Figure 2: Lump-sum grant increase



As is illustrated in the figure, with standard properties of the utility function $U = (C, G)$, a grant increase is thus predicted to cause consumption of both the public and the private good to increase.[8] In contrast, most early empirical estimates suggested a larger stimulatory effect on expenditures than what would be predicted by theory.[9] It seemed that the money stuck where it first hit, which is why this empirical anomaly was dubbed the "flypaper effect". However, because of the endogeneity problems inherited in grants alluded to earlier in this introduction, there is reason to believe that the findings in the early studies are simply statistical artifacts.

In *Essay 3*, I contribute to the flypaper literature by estimating causal effects of grants on local expenditures and income tax rates.[10] This is done in a difference-in-difference model, where a policy-induced increase in intergovernmental grants to a group of municipalities in Finland is used to identify the effects. The robust finding is that increased grants have a negligible effect on local income tax rates, but that there is a substantial positive immediate response in local expenditures.[11] Specifically, a 1 euro increase in grants caused expenditures to increase by around 70–80 cents. Or, evaluated at the amount of the policy-induced grant increase, expenditures increased by around 60 euro per capita as a result of the policy, whereas the implied cut in own-source revenues was only 6 euro per capita. Furthermore, there is no evidence that the immediate response in expenditures was reversed in later years. Thus, *Essay 3* concludes that the flypaper effect can indeed be a real economic phenomenon.

Having established that grant increases are almost exclusively used to finance increased public expenditures rather than private consumption (via decreased taxes), *Essay 4* then goes on to look at how grants affect a particular part of the local public sector; namely, local public employment.

Since local public goods and services are typically very labor intensive, policies that influence how many and who to employ can influence the quality of these goods and services considerably. Furthermore, the public sector commonly accounts for large parts of aggregate labor demand; in many countries, as much as 15–20% of the labor force are publicly employed, and the majority of these often have local public sector jobs. This means that policies that stimulate local public employment can also be a way of keeping the overall unemployment level down. The contribution of *Essay 4* is to investigate whether increased intergovernmental grants to local governments is one such potential policy. We do this by estimating causal effect of grants on local public employment—both in total and disaggregated by sector—using a panel of Swedish municipalities covering the period 1996–2004.

Identification of the causal effects of grants in *Essay 4* is achieved by making use of a kinked assignment rule in the Swedish grant system whereby municipalities with a net out-migration above 2% receive grants, whereas those below 2% do not. Because

[8] Note that the predictions from this very simple model survive a variety of extensions such as, like in Bradford and Oates (1971), incorporating political aspects of grants.

[9] For surveys of the literature on intergovernmental grants, see, e.g., Bailey and Connolly (1998), Gramlich (1977) and Hines Jr and Thaler (1995).

[10] Other studies that devote effort to properly solve the endogeneity problem in grants are Dahlberg et al. (2008), Gordon (2004) and Knight (2002).

[11] This is a result well in line with those found by Dahlberg et al. (2008) who conduct a similar study on Swedish municipalities.

any direct effect of out-migration on personnel can be assumed to be smooth, a kinked relationship between out-migration and personnel can be attributed to differences in the amount of grants received.[12] With this empirical framework, we find that an increase in intergovernmental grants has no effect on the total number employed by the municipality. When looking at employment disaggregated by sector, we only find a positive, statistically and economically significant effect on administrative personnel. Personnel in the other sectors—child care, schools, elderly care, social welfare and technical services—are however unaffected (in a statistical as well as an economical sense).

The asymmetric results across sectors as found in *Essay 4* raise the question of what distinguishes administrative bureaucratic personnel from personnel in other sectors. One possibility is that bureaucrats are able to influence the local decision-making process in ways that other types of personnel cannot and that they derive utility from employing more of their own kind. Alternatively, it might be more risky for municipalities from which there has been substantial out-migration to use increased grants to employ more personnel in child care and schools, for example. Labor demand in these sectors is likely to be more sensitive to demographic changes than in the administrative sector, with the implication that risk-averse decision-makers (be they politicians, bureaucrats, or both) are reluctant to hire any personnel at all in the sectors where demand is more volatile and uncertain.

But to end on a more positive note, it is also possible that only employing more administrative personnel improves efficiency. Such improvement would be possible if, in the absence of a grant increase, other personnel are occupied with administrative duties for which they are overqualified due to a lack of enough resources to hire administrative assistants.

[12] The method is labeled "regression kink design" (RKD) by Nielsen et al. (2010). Card et al. (2009) derive formal identifying assumptions and resulting testable predictions for the RKD.

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ESSAY 1: IS IT WORTH IT? ON THE RETURNS TO HOLDING POLITICAL OFFICE

1 Introduction

Politics is just like any economic activity; for it to be worthwhile, the benefits must outweigh the costs. This notion is prevalent in close to all political economy models from Downs (1957), where a politician is "some agent" whose main objective is to maximize votes and win elections in order to reap some (unspecified) benefits from being in office, to the more modern citizen-candidate models (Besley and Coate, 1997; Osborne and Slivinski, 1996), where the benefits explicitly include the possibility of implementing some desired policy. Despite its key theoretical role, empirical evidence of what types of payoffs that motivate politicians is more or less a black box. The purpose of this paper is to bring some light into this.

To this aim, I first look at monetary returns from politics by estimating causal effects of being elected in a local election on income shortly after being elected as well as up to 15 years later. This is made possible thanks to a newly collected extensive data set covering all Swedish politicians who have run for office at any level (local, regional or national) in the period 1991–2006.[1]

To get a first idea of what these monetary returns could be, Figure 1 displays the income profiles for all candidates who ran for a municipal council in the 1998 election, separately by whether or not they were elected. Although those elected clearly have higher income than those who were not, the gap is almost as large before the election as after. These differences can potentially be the result of selection—i.e., that elected candidates would have earned more than non-elected candidates even in the absence of being elected—as well as of different political histories—i.e., that elected candidates in 1998 are more likely to have been elected also in previous elections. While it is possible to partly control for these and other confounding factors, the figure illustrates quite well the difficulty in identifying the causal effect of being elected.

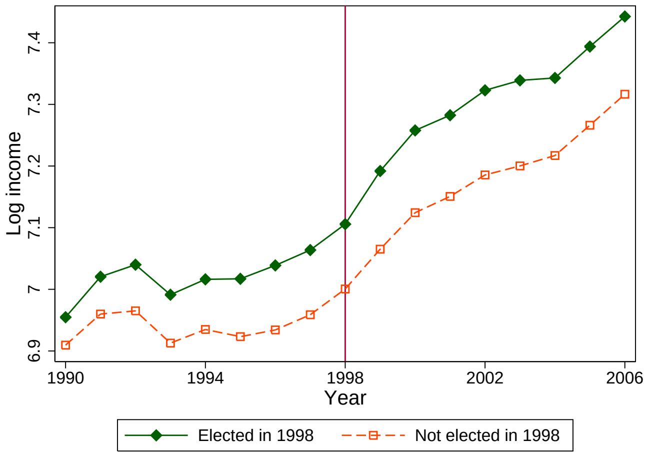
Instead, the claim for causality in this paper, I argue, can be made thanks to a simple yet compelling research design which, to my knowledge, has never before been applied. It fits into the class of identification strategies that rely on stochastic features of close elections (e.g., Lee et al., 2004 and Folke, 2011), but differs in that identification comes from within-party discontinuities rather than between. The idea is to compare the income of some candidate who just barely won a seat to that of some other candidate who was close to winning a seat *for the same party*, but ultimately did not. Because elections result in a fixed final ranking of each party's candidates,[2] the discontinuity between these candidates—whom I refer to as the *borderline elected* and *borderline defeated*—is well-defined. Moreover, other candidates than these two can be used to detect and control for any possible direct effects of being more highly ranked on income.[3]

[1] The majority of local politicians in Sweden hold regular jobs and, at least partly, devote their spare time to politics. This means that monetary returns from politics can stem both directly from official perquisites and remuneration as well as from a better paid private job, even in the short run.

[2] Which to a large extent corresponds to the party's own ballot paper rankings of candidates; see Section 3.1.

[3] As already noted, the identification strategy is clearly related to the regression discontinuity designs that rely on discontinuities in vote shares and focus on elections where some party won with a small

Figure 1: Disposable income among candidates running for a municipal council in 1998



Note: The figure plots average disposable income among candidates who were elected into a municipal council in 1998 and among candidates running for a municipal council in 1998 without getting elected. Income is measured in logs of 100 SEK deflated to 2000 year values.

Source: Statistics Sweden & The Swedish Election Authority.

Applying this identification strategy, I show graphically and econometrically that monetary returns from politics are absent irrespective if one considers the period right after the election, up to 15 years later or the period right after exiting politics. This result holds for different income measures such as disposable income, total labor income or labor income from the largest source. It is also true on average as well as when considering heterogeneous effects across various dimensions of parties, councils and candidates.

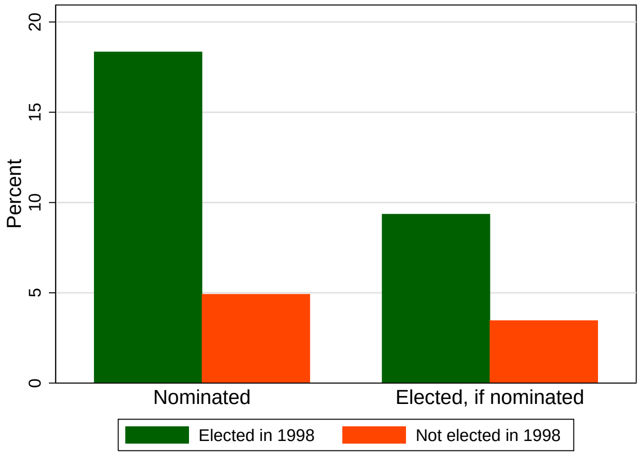
Thus, given that there are no positive monetary returns, politicians are likely *not* motivated by such returns. Rather, it seems that there must be some non-monetary returns that politicians pursue. These can, for example, be political accomplishments, a sense of actively taking part in the community, the desire to affect society in a certain direction, prestige and power—things that are hard if not impossible to measure. However, if such non-monetary returns are what motivates politicians, their objective should be to make a successful political career. Therefore, I proceed to investigate if being elected locally improves future political career prospects.

As a motivation for this, consider the stylized picture in Figure 2 showing the percentages among all elected and all non-elected candidates from the 1998 municipal council elections that went on to national politics in the 2002 and/or the 2006 election. The figure shows that locally elected politicians are 3.5 times more likely to be nominated

margin. Instead, I rely on the discontinuity in candidate ranks induced by the fact that each party will assign only as many seats as were won in the election. To check the robustness of the results I can, however, also use the more traditional vote share discontinuities generated by the seat assignments *between* parties by only focusing on the borderline elected and defeated in those parties that were close to winning/losing an extra seat.

for parliament (left bars) and, conditional on being nominated, 2.5 times more likely to actually be elected (right bars). Now, a causal interpretation of this picture is, of course, as problematic as the income comparison between elected and non-elected candidates in Figure 1. For evidence of how being elected locally for *exogenous* reasons affects political careers, I therefore apply the same identification strategy as for income and compare the borderline elected and the borderline defeated with respect to their future probabilities of being nominated for parliament as well as their future probabilities of being elected in local elections.[4]

Figure 2: Percentages among municipal council candidates in 1998 nominated for and elected into the national parliament in 2002 and/or 2006



Note: The figure shows the percentage that was nominated for and elected into the national parliament in the 2002 and/or 2006 election among candidates who were elected into a municipal council in 1998 and among candidates who ran for a municipal council in 1998 but did not get elected.

Source: Statistics Sweden & The Swedish Election Authority.

The main conclusion from this analysis is that being borderline elected into a municipal council improves political career prospects, especially through increased chances of advancing to national politics, but also of being elected in future local elections—at least in the short run. Hence, if the goal of politicians is to enjoy non-monetary payoffs such as political accomplishments, prestige and power from a successful political career, the local arena is a possible platform from which to start off.

With the caveat that some of the subgroup-specific effects are estimated with poor precision, the positive effects are especially pronounced for candidates running for smaller parties and to smaller councils. One possible mechanism behind this pattern is that the borderline elected can be a quite prominent figure in small parties and councils, whereas the borderline elected in large parties and councils is just a marginal guy who is less

[4] Note that because the national parliament only has 349 seats, getting elected is a very rare event. For this reason, the analysis on advancing nationally will be restricted to nominations.

visible. Another explanation which, to some extent, is supported by the data is that there is not sufficient variation in actual council experience between the borderline elected and the borderline defeated candidates in the larger parties. The reason is that a fair share of the latter are council replacements with a high probability of taking over a permanent council seat.

The method in the paper is applicable thanks to high-quality data. Lack of proper data is probably the main reason why there is very scant causal evidence of what the returns to politics are. However, one recent study by Eggers and Hainmueller (2009) has overcome the data limitations by collecting estates of deceased members of the British House of Commons. This data together with their empirical approach make this the perhaps most credible study so far. They estimate the effect on wealth (at the time of death) of being elected into parliament using a regression discontinuity design (RDD) where they compare candidates who won/lost with narrow vote margins—a research design similar to that in this paper. The resulting estimates point to substantial wealth effects for Conservative members of parliament but no effects for Labour members.[5]

With an entirely different approach, Diermeier et al. (2005) also aim at quantifying the returns to holding political office. They formulate a comprehensive dynamic structural model of career decisions of politicians, and test the model with data on US congressmen that includes pre-election characteristics as well as post-congressional employment information. However, a problem is that their data is restricted to actual congressmen, implying that the results can only be interpreted *conditional on being elected*. In a sample selection-correction model à la Heckman (1979) with local, regional and national trends in the Democratic/Republican support as an exclusion restriction, they estimate that being *re*-elected once has a positive effect on post-congressional earnings, but that the positive effect vanishes rather quickly with additional experience. Another interesting finding is that non-pecuniary returns from policy accomplishments and realized political ambition are seemingly large.

This paper provides new evidence on what types of returns that motivate politicians in two main ways. First, it is the only study to focus on the local rather than the national political arena. I argue that local politics is the relevant context for studying politicians' motivations, since this is where most political careers start off. For example, among the 349 members of the Swedish parliament in 2006, 75% had previously held a municipal council seat during at least one election period. Furthermore, local politics deals with issues affecting the everyday life of citizens, making its actors an important group to study.

Second, unlike Great Britain and the US, Swedish politics is characterized by a typical multi-party, proportional representation system with less focus on the individual candidate and more on the party as such. As a result, there is a great deal of interesting party dynamics to be explored. Moreover, these political institutions introduce a new dimension of electoral gain/defeat; a candidate can win or lose a seat in a party that wins or loses political power. This raises questions like whether the returns to being elected into a large party that might be part of the governing majority differ from the

[5] Aside from RDD they also use a matching framework, and the results are the same.

returns of being elected into a small opposition party.

Another merit of the paper is its high-quality data. It covers all candidates who have run for office at any level (local, regional or national) in any of the five elections held during the period 1991–2006. Two crucially important features are, first, that it contains the same information on all candidates irrespective of whether or not they were elected. Second, for most of the elections, it contains sufficiently detailed information to reproduce the final ranking of candidates resulting from the election, which makes it possible to determine who is the borderline elected. These two features, alone, make the data unique in its kind. Furthermore, rich register-based information on characteristics such as age, sex, foreign background, educational attainment, labor market status, occupation and various income measures is matched to all these candidates using a unique person identifier. The registers are in annual form and cover the years 1990–2006 for all candidates, which makes it possible to (i) follow candidates over a long time period; (ii) verify the identifying assumption with many pre-determined candidate characteristics; and (iii) study heterogeneous treatment effects across candidate characteristics.

Evidence of the types of payoffs that motivate politicians is an important piece to understanding the wider scheme of how politics work. The natural follow-up questions are then if payoffs matter for the selection of politicians and, ultimately, if the selection of politicians matters for policy. Above, I discussed studies that, like the present study, focus on the question of what the payoffs are. In the next section, I review the existing research on these other two related aspects. After that brief literature review, the paper is structured as follows: Section 3 describes the key features of local politics in Sweden and the procedure for ranking candidates within parties. Section 4 states the general assumptions for identifying the effect of being elected, as well as some additional parametric assumptions needed for estimation and inference. The data is described in Section 5 along with a motivation of the choice of outcome variables. Section 6 discusses what the treatment—being elected into a municipal council vs. being close to being elected—is likely to capture. In terms of main results, monetary returns constitute the focus in Section 7 and political careers in Section 8. The paper is concluded in Section 9.

2 *Related literature*

In his discussion of recent developments in political economics, Merlo (2006) recognizes the following two questions as important (p. 26): (i) Who chooses to become a politician? (ii) What are the payoffs from becoming a politician?

Like Eggers and Hainmueller (2009) and Diermeier et al. (2005) that were discussed in the introduction, this paper focuses on the second question. To put things in perspective, below I briefly go over the evidence on the first question regarding the selection of politicians.[6]

[6] The natural follow-up questions are then whether politicians' types and characteristics matter for their voting decisions (Lott and Kenny, 1999; Washington, 2008), resulting policies (Chattopadhyay and Duflo, 2004; Pande, 2003; Svaleryd, 2009) and, ultimately, for economic outcomes such as growth (Besley

Theoretical models of the selection effect of rewards reach different conclusions (Besley, 2004; Caselli and Morelli, 2004; Mattozzi and Merlo, 2008; Messner and Polborn, 2004).[7] On the empirical part, two studies with similar focus yield the same results: Ferraz and Finan (2009) and Gagliarducci and Nannicini (2011) both estimate positive effects of increased wages on performance and selection—the former for local politicians in Brazil and the latter for Italian mayors. For Finland, Kotakorpi and Poutvaara (2010) find that a policy-induced salary increase among members of parliament raised the average level of education among female candidates but not among males. Finally, Keane and Merlo (2010) use the framework and data from Diermeier et al. (2005) to simulate a variety of policy changes and study whether the effects are disproportionate across different types of politicians. Their model has two dimensions of ability: (i) "skill", defined as the ability to win elections; and (ii) "desire for legislative accomplishment". According to their simulations, congressional wage decreases induce politicians with high ability of type one to exit congress relatively more quickly, but do not affect politicians with high ability of type two.

3 *Swedish local politics*

This section provides an overview of key features of Swedish local politics and municipal elections. There are 290 municipalities in total, each governed by a municipal council elected every fourth year (every third year before 1994) in proportional elections held on the same day as elections to the national parliament and the county councils. Voter turnout is high from an international perspective; usually around 80%.

Around two thirds are single-constituency municipalities, but municipalities with a larger electorate have multiple constituencies. In the case of two constituencies or more, candidates are elected separately from each constituency. The municipal council decides on the total number of council seats, subject to minimum restrictions set by the Municipal Law ranging between 31 for municipalities with up to 12,000 eligible voters to 101 for the municipality of Stockholm. The median council size is 41. Seats are distributed between parties based on vote shares via the so-called "modified odd-number method", and there is no formal vote threshold for a seat.[8] All seven major parties in the national parliament (eight after the 2010 election) operate and have separate organizations at the national, regional and local level.[9] In some municipalities, there are additional local parties.

The municipal council is the highest decision-making body in the municipality and

et al., 2011; Jones and Olken, 2005).

[7] See also Besley (2005) on how political selection is affected by institutions in general.

[8] These and other regulations surrounding elections are mainly stipulated in the Municipal Law and the Elections Act.

[9] Since the founding of the Green Party in 1981, national politics has been dominated by seven parties; besides the Green, there is the Left Party, the Social Democrats, the Center Party, the Liberal Party, the Moderate Party and the Christian Democratic Party. In the 1991 election, the populist party the New Democrats made a short appearance, and in the 2010 election the right-wing extremist party the Sweden Democrats—which had so far only been locally successful–entered the national parliament.

its tasks are regulated in the Municipal Law; it *must* appoint members and replacements for committees, the most important of which is the executive board[10] (i.e., the "government" of the municipality); it *must* decide on issues that are of first-order relevance to the municipality such as the budget, the rate of the proportional income tax, organizational forms for the executive branch, remunerations to elected representatives and local referenda; it *can* delegate decisions on issues that are of second-order relevance to the executive board and to working committees.

Hence, the power of the council as stated in the Municipal Law is quite high. However, a parliamentary report with the purpose of considering measures for improving local democracy suggested, among other things, that the council's power over the agenda and its overall participation in preparations and decisions of political decisions be increased (Swedish Ministry of Integration and Equality, 2001). This suggestion was motivated by an increasing trend in delegations of decisions to the executive board and to the chairmanships of major working committees, and a more pronounced view of the council as merely being a formal decision-making institution on issues that have in practice been settled much earlier in the political process.

Part of the explanation for the more widespread delegations is the fact that the majority of local politicians have other occupations and devote their spare time to politics—less than 3% of all elected representatives (Öhrvall, 2004; Öhrvall and Persson, 2008) and around 8% of the politicians elected into the council (own data) receive full-time or part-time compensation.[11] According to a survey of local politicians conducted in 1999, the hours per week devoted to politics are 17.8 among chairs, 8.3 among regular council members and 5.3 among council replacements (Hagevi, 2000). But even though this system implies that time constraints can be significant obstacles, it is generally viewed as desirable because it also has the benefit of sustaining close connections between politicians and voters.

Section 6 returns to the question of what being elected into a municipal council really entails. Now, however, follows a description of the process of actually getting there, which forms the basis for the identification strategy of the paper.

3.1 Assignment of seats within parties

Candidates can only be elected to the municipal council via parties. Parties running for election nominate and subsequently rank candidates on ballot papers, somewhat generalized, according to the following procedure (Bäck and Möller, 2003):

1. All party members can nominate candidates. At this stage, special-interest politics plays a role in that youth organizations, women's organizations, unions etc. nominate their preferred candidates. Anyone who has the right to vote in the municipal

[10] The executive board is appointed such that the resulting distribution of seats between parties mirrors the seat distribution in the council.

[11] At least 40 but less than 100% of full-time pay are classified as part-time, although this is a rough classification since it is not always clear what constitutes a full-time assignment.

election can be nominated for their municipality's council.[12]

2. An appointed election committee ranks the nominated candidates who have agreed to run. Naturally, overall popularity plays a role in the ranking but also representativity in terms of gender, age, experience and political standpoints. Some parties hold internal trial elections to assist in the ranking.

3. The ballot paper rankings are fixed. This normally occurs around six months before the election.

A party can run with several ballot papers in a single constituency and/or with one ballot paper in several constituencies, meaning that there can be several *ballot paper rankings* in a single constituency and/or one *ballot paper ranking* for several constituencies. Because the seats are assigned separately for each constituency, there is, however, always one single *final ranking* per constituency. Given the total number of seats that each party has won in the constituency, it is according to this final ranking that seats are distributed within parties.

Starting with the 1998 election, voters can mark *one* preferred candidate on the ballot paper (so-called preference voting). When determining the final ranking, the top is set based on the ranking of such preference votes. The threshold for being elected via preference votes is 5% of the party's votes in the constituency, though this must be at least 50 votes. For candidates who do not reach this threshold, so-called comparison numbers are calculated, which are then ranked.

How the ballot paper ranking translates into the final ranking can be a complicated matter, for example when there are multiple ballot papers per constituency or when candidates run in several constituencies. These complications only arise in a minority of cases, and the details of the procedure are described in the Appendix. For the majority of cases, however, the final ranking mirrors the ballot paper ranking, except that candidates who have reached the preference vote threshold are put at the top.[13] The following section describes how the final candidate ranking is used for identification of the effect of being elected into a municipal council.

4 Identification strategy

The potential outcome framework introduced by Rubin (1974, 1990) is useful for conceptually thinking about identification of the effect of being an elected politician on some outcome Y. Let $Y_i(1)$ be the potential outcome of individual i if being treated (i.e.,

[12] There are some minor exceptions to this rule, such as municipal employees in charge of personnel (Municipal Law 4 Ch. 6§).

[13] In the three elections since the introduction of the preference vote covered by the data, around 15–20% of the candidates reached this threshold. However, considerably fewer were elected *because of* their preference votes, as the majority of those who reached the threshold were also sufficiently highly ranked on their party's ballot paper. Thus, the difference between the ballot paper ranking and the final ranking induced by moving candidates elected via preference votes to the top is, in practice, very small.

being elected to the municipal council), and $Y_i(0)$ the potential outcome of the same individual if not treated. The difference between the two potential outcomes, $Y_i(1)-Y_i(0)$, is then the treatment effect. While this definition of a treatment effect is intuitive, it is fundamentally impossible to measure. The reason—i.e., the identification problem—is that $Y_i(1)$ and $Y_i(0)$ are both *potential* outcomes of which only one can be observed.

Consider the outcome disposable income. Assume that we observe $Y_i(1)$—that is, we observe the disposable income of an elected politician.[14] The challenge in determining the treatment effect is then to find the best counterfactual outcome, meaning that one should look for the income that this individual would have earned, had he not been elected. A number of possible counterfactuals can be considered. First, it is possible to exploit time variation and compare the income of the same individual before and after he was elected. However, this will fail to identify the treatment effect if other things affecting his income changed during this period besides becoming elected (either directly for the politician or indirectly due to some aggregate shock), an event that seems highly plausible. Second, one could exploit cross-sectional variation and compare the income of the politician with that of other individuals at the same point in time. Unfortunately, this will most likely bias the estimated treatment effect even more, because the politician and "other individuals" differ along numerous other dimensions of which some are likely to be correlated with income.

Ideally, one would like the treatment of being elected into a municipal council to be random, since randomization ensures zero correlation with any outcome. And as elections have stochastic features, for some politicians it is indeed a matter of chance whether or not they are elected. Thus generally, under the assumption that election outcomes cannot be perfectly controlled, close elections induce random variation in who does and who does not get elected.[15]

Specifically, I will use the variation in treatment status between candidates running for the same party, given the number of seats won by that party. The idea is to reproduce the final ranking of candidates, as laid out in Section 3.1 and the Appendix, of a party that won n seats in some constituency and then compare the outcome (income, say) of the treated n^{th} candidate to that of the untreated $(n+1)^{th}$ candidate. Because the n^{th} ranked candidate just barely got elected by being assigned his party's last seat and the $(n+1)^{th}$ ranked candidate was close to being elected but was ultimately not, in what follows I refer to the former as the *borderline elected* and to the latter as the *borderline defeated*.

It is possible that the final ranking is systematically related to the outcome of interest. Or, put differently, it is possible and even likely that there is a systematic difference between the innate "quality" of the borderline elected and the borderline defeated. Other candidates than the borderline elected and defeated can help detect such direct effects. To this aim, visual inspection of the data is particularly illustrative; the treatment effect

[14] I abstract from time indices here but, as will soon be clear, outcomes will be measured in three different periods from the time of election.

[15] Following Lee et al. (2004), this idea has been exploited in numerous papers estimating "party effects".

will be seen graphically as the difference between the borderline elected and defeated that is above and beyond differences between any other two candidates.

Technically, the identification strategy is a regression discontinuity design (RDD) where the forcing variable is the difference between a candidate's (final) rank and the (final) rank of the borderline elected, $rank^*$.[16] The identifying assumption is that parties cannot perfectly anticipate how many seats they will win and thereby rank their candidates accordingly.[17] That is, parties cannot be absolutely certain which n candidates will be elected so that the quality of the $(n+1)^{th}$ candidate is irrelevant. Rather, the direct effect of rank on the outcome must be smooth for ranks around the borderline elected.

Because the forcing variable is discrete, assuming some parametric functional form is necessary in order to estimate the magnitude and standard error of the treatment effect. This is different from an RDD with a continuous forcing variable, which allows for non-parametric identification if there is a sufficiently large number of data points "infinitely close" to the discontinuity point. Lee and Card (2008) discuss identification and inference in RDD in the discrete case. They show that when the assumed parametric form differs from the true parametric form by some error that is identical irrespective of treatment status, the treatment effect is still identified, although the confidence intervals need to be inflated. Inflating the confidence intervals is then done by clustering at the level of the discrete values of the forcing variable. However, this procedure is not feasible in this application, because the forcing variable, $rank^*$, can only take a limited number of values.

Instead, underlying the preferred regression specification will be the parametric assumption that the direct effect of $rank^*$ is linear for a limited sample consisting of the n^{th}, $(n+1)^{th}$ and $(n+2)^{th}$ ranked candidates. I refer to such a set of candidates per party and constituency as the *borderline group*.[18] By limiting the estimation sample to three candidates per borderline group, the error from assuming linearity is likely to be smaller.

The regression to be estimated on the sample of candidates ranked n^{th}–$(n+2)^{th}$ is then:[19]

---

[16] The first application using RDD was Thistlethwaite and Campbell (1960), which, like in this paper, was based on a discrete forcing variable. Since the formal conditions for identification in the continuous case were derived by Hahn et al. (2001), the applications in economics have been numerous (see Lee and Lemieux (2010)).

[17] Recall from above that the ballot paper rankings are normally set around six months before the election, implying that this is not a very strong assumption.

[18] The reason for including the $(n+2)^{th}$ rather than the $(n-1)^{th}$ candidate is to have the sample as representative as possible. In the latter case, the sample needs to be restricted to parties where at least two candidates were elected via comparison numbers. Now, instead, the only restriction is that there is at least one candidate elected via comparison numbers. This is explained in more detail in the Appendix.

[19] For the continuous income outcomes, the estimated model will be a log-linear. For the binary future election outcomes, a linear probability model will be estimated.

$$Y_{ig,t+j} = \beta_0 + \beta_1 elected_{ig,t} + \beta_2 rank^*_{ig,t} \; (+\mathbf{\Gamma}'\mathbf{X}_{ig,t-1}) + \varepsilon_{ig,t+j}, \tag{1}$$

where $Y_{ig,t+j}$ is the outcome for candidate i in borderline group g running in election year t, j periods ahead. The forcing variable $rank^*_{ig,t}$—the difference between the rank of candidate i in group g and the rank of the borderline elected in group g—is defined such that it equals 0 for the borderline elected and -1 and -2 for the candidates who would have been elected had the party gained one or two more seats, respectively. The term in parenthesis represents effects of a vector of individual characteristics measured one year prior to the election that will be controlled for in most of the estimations and the graphical counterparts (although they should be redundant for identification purposes). Finally, $\varepsilon_{ig,t+j}$ is an error term that is allowed to be arbitrarily correlated within municipality.[20]

Both the graphical analysis and the estimations of equation (1) will consider short-, medium- and long-run outcomes, which for income outcomes translate into the time index $t+j$ being the average over 1–3, 6–8 and 13–15 years after election t, respectively. For short-, medium- and long-run election outcomes, $t+j$ will be the first, second and fourth subsequent election, respectively.[21]

The treatment parameter of interest is β_1 and the condition for the causal effect to be identified in equation (1) is that the direct effect of rank relative to the borderline elected is captured by β_2, meaning, once more, that it must be (at most) of order one for candidates ranked n^{th}–$(n+2)^{th}$.

More than three candidates per borderline group (i.e., per party and constituency)[22] are required for the treatment effect to be identified if the direct effect of $rank^*$ is of higher order than one.[23] As a complement to the main specification in (1), a set of results from running the following regression on the borderline elected and several defeated candidates will therefore also be presented:

$$Y_{ig,t+j} = \beta_0 + \beta_1 elected_{ig,t} + \sum_{p=1}^{\bar{p}} \beta_{2p} (rank^*_{ig,t})^p + \varepsilon_{ig,t+j}, \tag{2}$$

where the term summing over order of polynomial p represents the direct effect of $rank^*$ and $\bar{p}$ is the highest order of polynomial included in the regression. Several versions of

[20] This variance-covariance matrix may seem too restrictive. However, it turns out that clustering the standard errors at smaller units than municipality—as is done now—does, in fact, not improve the precision of the estimates (the results are available upon request).

[21] Four elections ahead is as far as the data allows the analysis to go. The reason for not studying the third subsequent outcome is simply to keep the number of outcomes down.

[22] The majority of borderline groups are at the constituency level. However, when a ballot paper overlaps several constituencies, the group is at the municipality level; see the Appendix.

[23] Analogously, a simple mean comparison of the borderline elected and defeated identifies the treatment effect if there is no direct effect of $rank^*$.

equation (2) will be estimated by varying $\bar{p}$ between 1 and 3 and the number of defeated candidates included (i.e, the bandwidth) between 5 and 10.

With the empirical setup represented by equations (1) and (2), controlling for group-specific characteristics or a group fixed-effect (or some other more aggregate fixed-effect) is, for identification purposes, more or less redundant. To see this, note that the estimation samples consist of a nearly-balanced panel with borderline groups of candidates with the same $rank^*$ values. The only exceptions are those groups where there are too few defeated candidates so that it is not possible to assign low values of $rank^*$ to anyone (cf. Figure A1 in the Appendix). Therefore, unless these exceptions are systematic, any group characteristics must be uncorrelated with $rank^*_{ig,t}$ and hence, also with the treatment variable $elected_{ig,t}$ since this is simply an indicator variable $1(rank^*_{ig,t} = 0)$.[24]

The identifying assumption that parties cannot perfectly anticipate which candidates that will be elected may be more likely to hold for some groups than for others. Specifically, parties that have repeatedly won n seats may anticipate that they will do so also in the next election and, consequently, may not care about the quality of the $(n + 1)^{th}$ candidate. Figure 3 assesses whether this is likely to be a problem. Separately by party size, it shows the variability of seats for a given party in a given council over elections 1982–2002, measured as the deviation in the number of seats in a particular election from the mean number of seats over the entire period.

Reassuringly, Figure 3 shows substantial variation even for parties that on average have two seats or less (top left plot).[25] To further investigate the validity of the identifying assumption, the empirical analysis will contain robustness checks where I mimic a group-specific unanticipated shock that affects who the borderline elected is. Specifically, the estimation sample will be restricted to only include (i) groups whose total number of seats changed from the previous election; (ii) groups that won their n^{th} seat or lost their $(n+1)^{th}$ seat with narrow vote margins; and (iii) the combination of (i) and (ii). For this exercise, the definition and calculation of minimum changes in votes to win or lose an additional seat in proportional elections as developed by Folke (2011)[26] will be used.

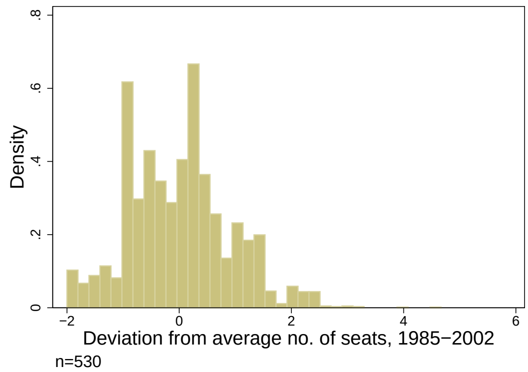
Moreover, to strengthen the notion that β_1 really captures the effect of being elected, placebo regressions in which each group is assigned one or two additional seats so that the $(n + 1)^{th}$ or the $(n + 2)^{th}$ candidate is the "borderline elected" will be estimated. These estimations will serve as complements to the graphical analysis where such placebo effects can be directly detected.

[24] One may still want to include group fixed-effects to increase the precision of the estimates. However, it turns out that doing this neither affects the point estimates nor the standard errors (the results are available upon request).

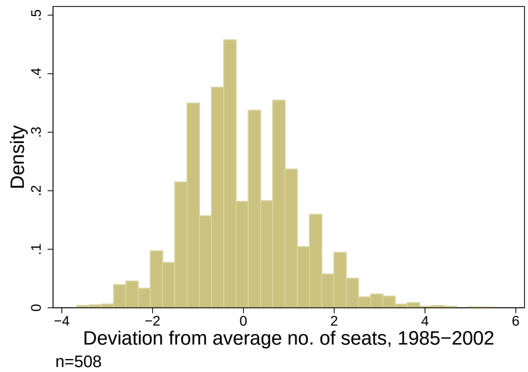
[25] As should be clear from Section 3.1, there is a considerable amount of internal democracy within the parties in setting the ranking, suggesting that the quality of the (borderline) defeated candidates matters even when there is little uncertainty about how many seats the party will win.

[26] I sincerely thank him for generously sharing his STATA code.

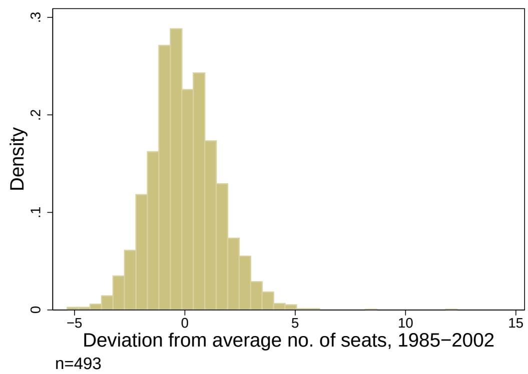
Figure 3: Variability in parties' number of seats



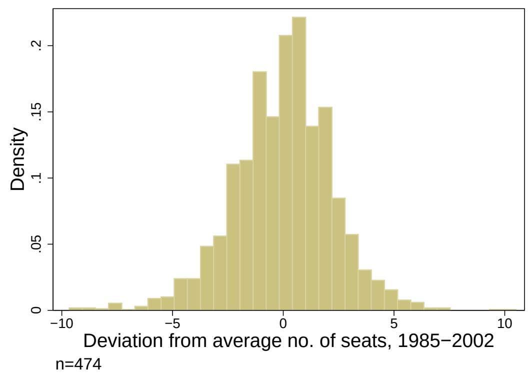
(a) $0 < \overline{seats} \leq 2$



(b) $2 < \overline{seats} \leq 4$



(c) $4 < \overline{seats} \leq 9$



(d) $9 < \overline{seats}$

Note: The figures show the distribution of the deviation in the number of seats in a particular election between 1982 and 2002 from the mean number of seats over the entire period, $\overline{seats}$.
Source: Statistics Sweden.

5 *Data*

Detailed data over political candidates is a necessity for applying the above described research design. The data used in this paper, obtained from Statistics Sweden and The Swedish Election Authority, covers all candidates who have run for office to a Swedish municipal council or to the national parliament in any of the five elections held during the period 1991–2006.[27] The elections to municipal councils in 1991, 1998 and 2002/in 1991 and 1998/in 1991 define the population under study for short-/medium-/long-run outcomes. The number of borderline groups is around 1800–1900 in each of these three elections. Data from the 1994 election is of poorer quality and could not be used to define borderline groups. However, data from all elections between 1994 and 2006 will be used for outcome purposes (see below for details), and the 2006 data additionally contains some useful information that will be used for descriptive purposes. The analysis will not cover local parties but is restricted to the seven parties that have traditionally dominated national politics.[28]

Two crucially important features of the data are, first, that it contains the same information on all candidates irrespective of whether they were elected or not. Second, except for the 1994 election, it contains all ballot paper rankings so that the final ranking that identifies the borderline groups can be calculated.[29] These two features, alone, make the data unique in its kind. Furthermore, rich register-based information on characteristics such as age, sex, foreign background, educational attainment, labor market status, occupation and various income measures is matched to all candidates using a unique person identifier. The registers are in annual form and cover the years 1990–2006 for all candidates, which enables an empirical analysis that (i) follows candidates over a relatively long time period; (ii) can verify the identifying assumptions using pre-determined covariates; and (iii) looks at heterogeneous treatment effects across characteristics such as age and level of education.

5.1 *Outcome variables*

The effects of being elected into a municipal council will be considered on a short-, medium- and long-run basis which, as described in connection with the identification strategy, for income outcomes translate into the time index $t + j$ denoting the average over 1–3, 6–8 and 13–15 years after the election in year t, respectively. For short-, medium- and long-run election outcomes, $t + j$ denotes the first, second and fourth subsequent election, respectively. Descriptive statistics of all outcomes in the sample of candidates in borderline groups with $rank^* = \{-2, -1, 0\}$ are provided in Table A1 in the Appendix. Below follows a description and motivation of the choice of variables.

[27] Candidates running for a county council are also covered, but this data will not be used in this paper.

[28] The main reason for excluding local parties is that they are very diverse and would therefore be likely to introduce unnecessary noise.

[29] Because the 1991 and 1998 election data contains somewhat less information than the 2002 election data, some assumptions were needed to find borderline groups in these two elections. See the Appendix for details.

Disposable income—This variable is meant to capture all monetary returns from politics. It is individualized but measured at the household level, and is the sum of numerous types of after-tax income of the family, including, e.g., labor income, capital income, pensions and unemployment and sickness benefits. To the extent that there is intra-household bargaining—so that also the income of the politician's spouse could be affected—this is a proper measure of total monetary returns. Note, though, that with the available data it is also possible to check the sensitivity of the results to alternative income measures.

To reduce the noise that often plagues income data, disposable income is measured in three-year averages. For a candidate in the 1991 election, for example, short-run income is the average income over years 1992–1994, medium-run income is the average over years 1997–1999 and long-run income is the average over years 2004–2006. To avoid results that are driven by outliers, the three-year averages are censored at the 1^{st} and 99^{th} percentiles. The analysis will be performed on logs of the three-year averages.

Monetary returns from politics will be positive if individuals acquire certain skills that are rewarded in the labor market, if there is a positive signaling effect or if the individuals develop closer ties to certain firms or organizations. Note that such returns could be retained while still in politics, since the majority of local politicians hold regular jobs and, at least partly, devote their spare time politics. While still in politics, there is also the direct effect of official perquisites and remunerations. There is, however, also the possibility of mechanisms operating in the opposite direction: political engagement may require foregone earnings because of time and effort constraints.[30]

Monetary returns in the form of outright bribes will obviously be close to impossible to measure, as these are unlikely to show up in official income registers. But to the extent that politicians attempt to hide parts of their (illegitimate) income by transferring *official* income within the household, such returns will show up in their disposable income.

Being nominated for/elected into a municipal council—These are indicator variables measuring the probability of a candidate being nominated to a municipal council in subsequent elections and the probability of being elected into the council in subsequent elections. These outcomes will capture if being randomly elected into a council improves future political career prospects locally.

As for potential effects on the probability of running, one can imagine that being elected establishes closer connections to the local party organization which would increase the likelihood of future nominations, or that being elected has a positive encouragement effect on continuing in politics which would increase the likelihood of accepting a nomination. For some individuals, on the other hand, being elected may imply learning and being disappointed by what local politics really is about which would then discourage future political engagement.

The effects on being elected in future elections, or incumbency effects, may in part operate via similar channels. Parties may reward "good politicians" that, for example, stick to the party line by promoting them and ranking them higher in subsequent elec-

[30] The Municipal Law (4 Ch. 12§) states that elected representatives have the right to be "reasonably compensated" for foregone earnings due to their political assignments.

tions. If such abilities are better revealed in the council, being elected would thus affect the chances of being reelected. But reelection probabilities may also be affected through more traditional incumbency effects that operate via voters.

Being nominated for the national parliament—This is an indicator variable measuring the probability of a candidate being nominated to the national parliament in subsequent elections. Advancing from the local to the national arena is a likely goal among candidates who are motivated by political accomplishments and prestige and who want to pursue a political career.

Because the parliament only has 349 seats, actually getting elected is a very rare event, which is the reason why the analysis on national politics is restricted to nominations. So, what does it mean to be nominated for the national parliament? Naturally, the probability of actually being elected is infinitely greater for those running than for those who do not. But, to some extent, even non-elected parliamentary candidates have advanced from their local political careers, since not all party members that wish to be nominated actually are.

Although there is very little research on the vertical structure of political parties in Sweden (Erlingsson, 2008), one can imagine that the mechanisms operating locally to some degree extend to the national level. According to Bäck and Möller (2003), the local organizations constitute the basis for the political parties as they are platforms for member recruitment and for most meetings, and as they handle nominations of candidates to numerous political assignments. However, although the local party organizations operate separately from their central counterparts, there is arguably still some degree of vertical interdependence.

5.2 Control variables

The register data includes numerous variables measuring the candidate's characteristics. Table 1 shows the mean and standard deviation of a set of these variables (measured one year before the election) for three different samples taken from the 1991, 1998 and 2002 election data that is the focus of the paper; (i) column 1 includes all non-elected candidates; (ii) column 2 includes all elected candidates; and (iii) column 3 includes candidates with $rank^* = \{-2, -1, 0\}$ in the borderline groups that constitute the sample for the main econometric analysis. Comparing columns 1–2 with column 3 shows how representative the candidates in the borderline groups are (ignore column 4 for now). For example, in terms of age and marital status, the borderline groups are more similar to the non-elected sample, whereas in terms of education they are more like the elected sample. Hence, the representativity is in general quite good.

Since all time-variant covariates are set at one year before the election, all variables in Table 1 are pre-determined and should hence not be affected by the treatment. Therefore, one implication of the identifying assumption (that the direct effect of rank is the same for ranks around the borderline elected) is that the treatment effect conditional on these variables should not differ from the unconditional treatment effect. This will be explored

Table 1: Representativity and balance in pre-determined characteristics of candidates in borderline groups with $rank^* = \{-2, -1, 0\}$

	Sample			β_1
	All non-elected	All elected	$rank^* = \{-2, -1, 0\}$	t-stat.
Disposable income	1189.4	1345.9	1204.6	0.88
	(514.9)	(574.0)	(522.8)	
Age	47.9	49.3	47.7	0.22
	(12.9)	(10.8)	(12.1)	
Children under 18	0.81	0.75	0.88	-0.46
	(1.14)	(1.10)	(1.18)	
Female	0.40	0.40	0.41	-1.08
	(0.49)	(0.49)	(0.49)	
Married	0.66	0.71	0.66	1.50
	(0.47)	(0.45)	(0.47)	
Less than high school	0.20	0.16	0.15	0.57
	(0.40)	(0.37)	(0.36)	
High school graduate	0.43	0.40	0.40	-1.13
	(0.49)	(0.49)	(0.49)	
$<$ 2 years university	0.061	0.072	0.070	1.41
	(0.24)	(0.26)	(0.26)	
$\geq$ 2 years university	0.30	0.36	0.37	0.15
	(0.46)	(0.48)	(0.48)	
Graduate studies	0.0083	0.0094	0.0093	-0.92
	(0.091)	(0.097)	(0.096)	
Born in Sweden	0.94	0.95	0.94	-0.47
	(0.25)	(0.22)	(0.25)	
Born in other Nordic country	0.029	0.026	0.030	-0.79
	(0.17)	(0.16)	(0.17)	
Born in non-Nordic Europe	0.020	0.017	0.018	1.09
	(0.14)	(0.13)	(0.13)	
Born in North America	0.0021	0.0011	0.0023	-0.08
	(0.045)	(0.033)	(0.048)	
Born elsewhere	0.014	0.0091	0.014	0.94
	(0.12)	(0.095)	(0.12)	
Both parents foreign-born	0.0087	0.0068	0.010	-0.15
	(0.093)	(0.082)	(0.100)	
Observations	109369	38229	16738	16738

Note: Columns 1–3 report the mean and standard deviation (in parentheses) of variables measured one year before the election. Column 4 reports the t-statistic of the estimate of β_1 from running equation (1) on each of the variables on the sample of candidates with $rank^* = \{-2, -1, 0\}$ in the borderline groups. Income is measured in 100 SEK deflated to 2000 year values (6.50 SEK$\approx$1 USD). The education variables indicate highest completed level. Born elsewhere equals one for individuals born in Africa, Asia, Oceania, Russia or S. America. Both parents foreign-born equals one for individuals born in Sweden but with both parents foreign-born. All variables but Disposable income, Age and Children under 18 are binary.
Source: Statistics Sweden.

in the result section.[31]

A mirror implication of the identifying assumption can be tested by running the main equation (1) on pre-determined covariates. If the direct effect of rank is linear among the candidates in the borderline groups, non-linearities in pre-determined covariates should not be expected. In other words, the estimate of β_1 should not differ from zero. The rightmost column of Table 1 provides the t-statistics of the β_1 estimate from running these regressions, which indeed are small enough to confirm that there are no non-linearities in the direct effect of $rank^*$.[32]

Aside from the variables in Table 1, individual controls will further include a set of dummies capturing past political experience by indicating whether the candidate ran for/was elected into a municipal council in the past three elections. Because the earliest election covered by the data is 1991, these dummies are censored or partly censored (set to zero) for borderline groups in the 1991 and 1998 elections.

6 *Characterizing the treatment*

The treatment group and the control group consist of candidates who got their party's last seat and those who were next in line to get a seat had their party won enough additional votes, respectively. The idea is that a comparison of these two groups will capture exogenous differences along dimensions such as political experience, power, success and representation. While Section 4 laid out the assumptions under which the exogeneity requirement is fulfilled, I now discuss what the treatment—being elected into a municipal council vs. being close to being elected—is likely to capture.

An important aspect is the appointment of council replacements to stand in for regular council members in the case of defection or absence from a meeting. Based on the ranking on the ballot paper from which each of the regular council members were elected, non-elected candidates are appointed replacements. A replacement can stand in for several regular members, and the total number of replacements to be appointed is decided by the council prior to the election (as a share below half of the total seats won).

Thus, it is quite likely that candidates in the control group (in particular the borderline defeated) serve as council replacements. If actual political experience is what matters for income and political career prospects, it is thus sensible to define treatment as actually having served in the council, rather than being elected into the council on election day. If any regular council member resigns early in the election period and a candidate in the control group thereby gets a permanent seat in the council, and/or if the borderline elected is the one who resigns, the variation in treatment status—defined in this way—will, therefore, be fuzzy at the threshold at $rank^* = 0$.

[31] Disposable income will be controlled for with quantile dummies, age with dummies for 10-year intervals and number of children linearly. All other control variables are binary.

[32] An analogous test is to run a regression of the binary variable *elected* on $rank^*$ and all covariates in Table 1 and test for joint significance of the covariates. Doing this, the obtained F-statistic is 0.80 (p-value 0.71), thus strengthening the confirmation of no non-linearities.

Fortunately, at least for the 2002 and 2006 elections, there is information on early resignations and effective replacements that can tell the extent to which the treatment effects obtained from running the regression in (1) underestimate effects of being de facto treated (i.e., actually having served in the council). If borderline elected candidates are defined as having de facto been treated if they did not resign during the first year after the election date, and if defeated candidates are defined as having been de facto treated if they overtook someone's permanent council seat at least 300 days before the next election,[33] then, according to the 2002 and 2006 data, 95% and 40% of all borderline elected and defeated were de facto treated, respectively. The corresponding percentage among candidates ranked -2 is around 20%.

If this information were available for all elections, a fuzzy RDD with the probability of being de facto treated as a discontinuous function of $rank^*$ as the first stage would be ideal. As revealed by the percentages just stated, running such a first stage on the 2002 and 2006 data on candidates in the borderline groups with $rank^* = \{-2, -1, 0\}$ yields an estimate of around 0.30 (with a t-statistic of 18.5). Thus, although the treatment of having actually served in the council is not deterministically determined by $rank^*$, there is still substantial discontinuous variation at the threshold at $rank^* = 0$.

Another aspect is that committee work outside of the council provides alternative forums for political engagement. Only politicians in the municipal council are directly elected by the voters. However, when the council subsequently appoints members to working committees (and committee replacements), they can do so both from within as well as from outside the council. The term "elected representative" in the Municipal Law refers both to regular council members directly elected by the voters, municipal council replacements as well as to those appointed to committees by the council. With this definition, the number of locally elected representatives exceeds the number of municipal council members by far.

However, we know that exerting the formal power as placed on the municipal council by the Municipal Law is reserved to council members, and this should be considered as an important part of the treatment. This means that, if—as has been expressed—substantial de facto power is concentrated to the executive board and major committees, council members can influence the composition of committees in a way that is favorable to themselves by, e.g., appointing themselves or fellow council members. That 90% of the executive board are also members of the council (Bäck, 1993; Bäck and Öhrvall, 2004) suggests this to be the case. Information on the number and type of positions held by the politicians in the data available here (unfortunately only for the 2006 election) also supports this argument; 8% of the borderline elected in 2006 are members of the executive board, whereas the corresponding percentage is merely around 1.5–2.5 among candidates ranked -1 or -2. Furthermore, also according to the 2006 data, the borderline defeated are not compensated with positions in other committees, in the sense that the borderline elected hold, on average, one more regular position than the borderline defeated (1.6 compared to 0.7).

Thus, it is clear that being borderline elected into a municipal council vs. being close

[33] Note that the new council is not formally in place immediately after the next election.

to being elected induces differences in dimensions such as political representation and power. The remainder of the paper will show if and how these differences affect income and political career prospects.

7 *Monetary returns from being elected*

To start investigating what types of payoffs that motivate politicians, this section looks at the monetary returns from politics by analyzing the effect of being elected into a municipal council on short-, medium- and long-run income as measured by the log of disposable income 1–3, 6–8 and 13–15 years after being elected, respectively. The analysis combines graphical presentations with econometric methods as described in Section 4.

Let us first look at the graphics in Figure 4. It plots the $rank^*$-specific means of disposable income in the three different periods. The plot to the left shows raw means, whereas the plot to the right shows conditional means obtained from a regression of the outcome variable on a set of individual controls measured one year before the election; the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Recall that the variable $rank^*$ is defined as the difference between a candidate's final rank and the final rank of the borderline elected, so that it takes the value zero for the borderline elected and negative values for non-elected candidates.

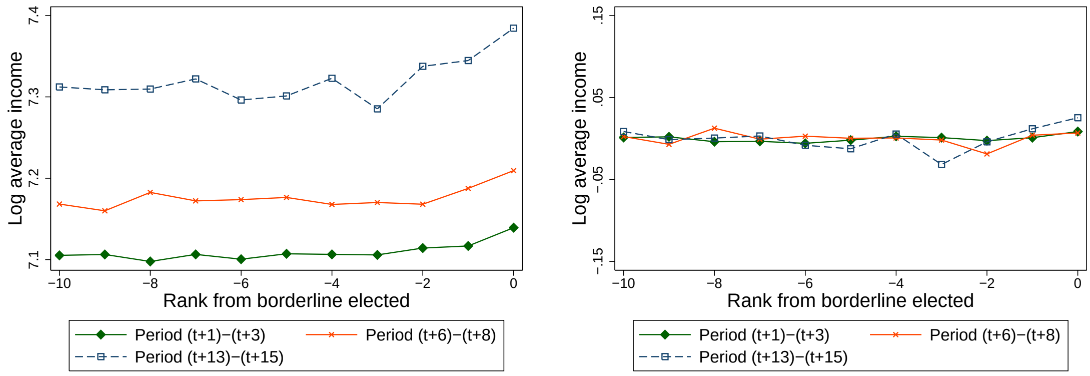
Direct effects of $rank^*$ on the outcome are represented by the overall slope of the lines connecting the $rank^*$-specific means. Conceptually, the treatment effect is the difference between the borderline elected ($rank^* = 0$) and the borderline defeated ($rank^* = -1$) that is above and beyond the difference between any other two candidates. Visually, a treatment effect therefore corresponds to a kink in the slope at $rank^* = -1$. The raw means to the left thus reveal small or zero effects on income from being elected.[34] This is particularly clear for medium-run income, where any kink at $rank^* = -1$ is completely absent. For short- and long-run income, a slight kink can be detected. For the latter, however, as there is a considerably more distinct kink at $rank^* = -3$, this is more likely to be due to random variation than to a treatment effect.

Comparing the left and the right plots, the main difference is that there is a mean-adjustment to zero for all income periods (as these are residuals). Although this adjustment makes the plot less clear, it is suggestive of the same pattern as in the raw means, which thus suggests that to extensively control for pre-determined characteristics would not alter the results.

The econometric counterpart to the plots in Figure 4 is given in Table 2, providing the results from estimating equation (1) on candidates with $rank^* = \{-2, -1, 0\}$. Note that the parameter β_2 is the marginal effect of $rank^*$ and thus corresponds to the overall slopes in the plots, whereas β_1 is the main parameter of interest that captures the

[34] Not only are the treatment effects absent, but what might be somewhat surprising is that also the direct effects of $rank^*$ are negligible. Thus, to the extent that income is a proxy for ability (in some broader sense), candidates around the borderline elected are not ranked according to this.

Figure 4: Short-, medium- and long-run disposable income



(a) Raw means (b) Conditional means

Note: The figures plot means of disposable income by rank from borderline elected in election year t. Income is deflated to 2000 year values and measured as logs of three-year averages in the short run (years t+1 to t+3), medium run (years t+6 to t+8) and long run (years t+13 to t+15). Conditional means are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.
Source: Statistics Sweden & The Swedish Election Authority.

additional effect of having $rank^* = 0$, or the effect of being elected.

The results in column 1 are for short-run income without any further controls while column 2 controls for the same set of individual controls as in the right plot with the conditional means. Equivalent results for medium- and long-run income are given in columns 3–4 and 5–6, respectively. As seen in the table, none of the estimated treatment effects are statistically significant and the point estimates are very close to zero either with controls (for the short and long run) or without controls (for the medium run). Note especially that the suspected kinks in short- and long-run income seen graphically are not statistically significant. Qualitatively, the inclusion of controls makes no difference, and—although the size of the estimates changes when controls are included—estimates with and without controls are within the 95% confidence interval of one another. In all regards, the econometric results thus confirm the graphical inspection.

7.1 Returns while in vs. after exiting politics

Some candidates elected in a particular election are still active politicians 6–8 and 13–15 years later, while others are not. The candidates' medium- and long-run income should be seen as the result of optimizing behavior, which may lead to political careers of different length for different people. But it is also of interest to see whether returns to politics kick in after leaving politics. In general, however, looking at income conditional on exiting politics is problematic since exit is endogenous. For example, some politicians may exit because they expect it to be profitable, and others may exit because they were

Table 2: Effects of being elected on disposable income

	Period (t+1)–(t+3)		Period (t+6)–(t+8)		Period (t+13)–(t+15)	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0199	0.00391	0.00271	-0.0187	0.0327	-0.00659
	(0.0124)	(0.00890)	(0.0176)	(0.0154)	(0.0282)	(0.0245)
rank*	0.00260	0.00402	0.0192*	0.0226**	0.00713	0.0183
	(0.00714)	(0.00497)	(0.0105)	(0.00887)	(0.0165)	(0.0145)
Observations	16673	16673	10915	10915	5283	5283
Individual controls	no	yes	no	yes	no	yes

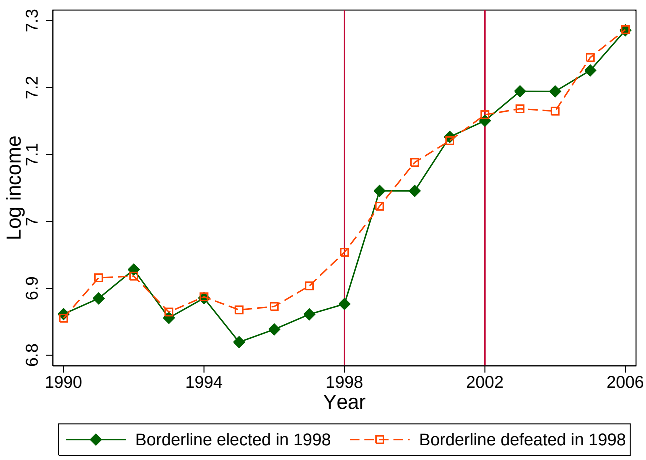
Note: The table reports effects of being elected into a municipal council on disposable income measured as logs of three-year averages 1–3 (columns 1–2), 6–8 (columns 3–4) and 13–15 (columns 5–6) years after the election. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

unsuccessful incumbents.

A way of circumventing this problem is to look at the income profile of candidates who were not only elected by chance, but who also left politics for exogenous reasons. An exogenous source of variation in exit rates that lies close at hand is being borderline defeated. To this end, Figure 5 plots the income profile of candidates who were borderline elected in 1998 *and* borderline defeated in 2002. This is to be compared with the income profile of candidates who were borderline defeated in 1998 and not elected in 2002 either, whose income profile is also seen in the figure. These candidates were neither elected in previous elections in the data (1991 or 1994), nor in the next election (2006). 975 candidates of the borderline defeated in 1998 satisfy these conditions, but only 59 of the borderline elected in 1998.

As seen in the figure, the income levels are very similar several years before the 1998 election, while being in office as well as after exiting. The exceptions are the few years preceding the 1998 election, when the income of those who get elected later is lower. Although, because of the small sample size, one should perhaps be careful about reading too much into this pattern. That there is no income gain following the exogenous exit from politics in 2002 is, however, clear.

Figure 5: Disposable income while in vs. after exiting politics



Note: The figure plots average disposable income among candidates who were borderline elected in 1998 *and* borderline defeated in 2002, and among candidates who were borderline defeated in 1998. Income is measured in logs of 100 SEK deflated to 2000 year values.

Source: Statistics Sweden & The Swedish Election Authority.

The above results all lead to the conclusion that monetary returns from politics are, on average, absent irrespective of if one considers the period right after the election, up to 15 years later or the period right after exiting politics. To support this conclusion, I have done further analyses on (i) total labor income and labor income from the largest source in stead of disposable income; and (ii) heterogeneous effects across parties, council size, party size, ruling status of the party and candidate's age, political experience, education level and pre-election income. None of these analyses show any systematic effects of being elected, thus strengthening the conclusion that there is no monetary payoff from politics.

8 Effects on future political careers

Given that monetary returns from politics are absent, politicians are likely *not* motivated by that. Rather, it seems that there are some non-monetary returns that politicians pursue. These can be in the form of political accomplishments, a sense of actively taking part in the community, the desire to affect society in a certain direction, prestige and power—types of returns that are hard if not impossible to measure. However, if such non-monetary returns are what motivates politicians, their objective should be to make a successful political career. In the remainder of the paper, I therefore investigate if being elected into a municipal council improves future political career prospects. I begin by studying if, for exogenous reasons, being elected improves the chances of being elected also in future local elections, and then move on to see whether it increases the chances of advancing to national politics. Analogously to the previous section on monetary returns, most of the analysis on political careers is carried out graphically as well as econometrically.

8.1 Local politics

I start by assessing if being elected into a municipal council in election year t has an effect on the probability of running in future elections to a municipal council in the short, medium and long run, corresponding to the first, second and fourth subsequent election, respectively. The results are presented in Figure 6 and Table 3, and are to be read in the same way as above except that the outcome is now the probability of being nominated for election instead of income.

Like those above, these graphs are quite illustrative. Not surprisingly, there is a positive direct relationship between $rank^*$ and the probability of running in future elections to a municipal council, as seen from the overall positive slopes. However, there is little evidence of any treatment effect of being elected, as there is no kink in the slope between the borderline elected and defeated (at $rank^* = -1$), except maybe in the second subsequent election. This pattern is confirmed by Table 3, where all estimates are statistically insignificant except the one for the election in $t + 2$ (at the 10% level and only without individual controls). Controlling for individual characteristics barely affects the point estimates and—just like for income—the only graphical differences are in the intercepts.

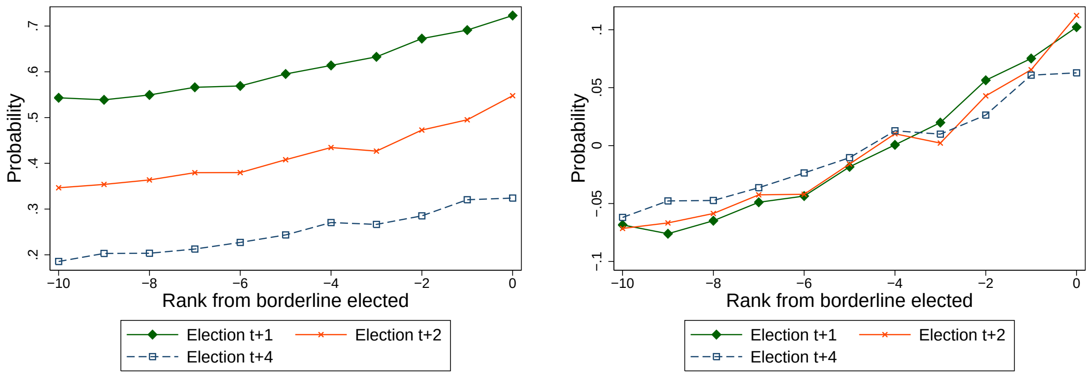
The large average probabilities of running in future elections as seen from the left plot in Figure 6 show that there is a high degree of persistence in who runs for elections, especially in the short run. One possibility is that candidates who are not ranked sufficiently high to be elected in election t to a large extent also run in subsequent elections because they perceive their chances of being elected to increase, perhaps if they are compensated by being ranked higher. If that is the case, one should expect no effect of being elected at time t on also being elected in future elections.

Table 3: Effects of being elected on the probability of being nominated in future elections to a municipal council

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0133	0.0104	0.0302*	0.0251	-0.0316	-0.0322
	(0.0133)	(0.0133)	(0.0183)	(0.0181)	(0.0264)	(0.0267)
rank*	0.0186**	0.0193**	0.0224**	0.0231**	0.0353**	0.0345**
	(0.00778)	(0.00782)	(0.0109)	(0.0108)	(0.0157)	(0.0159)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

Note: The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 6: Probabilities of being nominated in future elections to a municipal council



(a) Raw probabilities

(b) Conditional probabilities

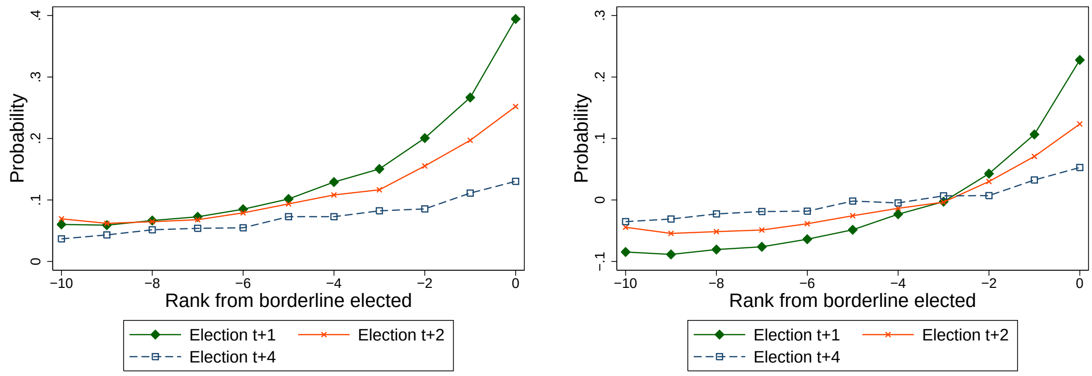
Note: The figures plot the probability of being nominated in future elections to a municipal council by rank from borderline elected in election year t. Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.
Source: Statistics Sweden & The Swedish Election Authority.

These future election probabilities are assessed in Figure 7 and Table 4.[35] Focusing on candidates with $rank^* = \{-2, -1, 0\}$, from the graphics one can detect a positive short-run treatment effect (i.e., being elected in the first subsequent election), as there is a kink between the borderline elected and defeated. According to columns 1–2 in Table 4, this effect is a statistically significant 6 percentage points and it is unaffected when controlling for individual characteristics. As suggested by Figure 7 and as confirmed in columns 3–6 in Table 4, there are, however, no effects of being elected in election t on also being elected in elections $t+2$ and $t+4$.

In terms of magnitude, 6 percentage points amount to about the same size as the direct effect of $rank^*$, and around 20% of the mean election rate in election $t+1$ for the sets of three candidates with $rank^* = \{-2, -1, 0\}$ in the borderline groups (see the descriptive statistics in the Appendix). However, recall from the discussion in Section 6 that a fair share of the borderline defeated who initially were council replacements in fact overtook a permanent council seat, so that—if treatment is defined as actually having served in the council—treatment status is fuzzy at the threshold at $rank^* = 0$. Evaluating the magnitude of such a treatment effect requires scaling up the coefficient by around three (since the first stage is estimated to around 0.30). Thus, the obtained

[35] Note that these are unconditional election probabilities, in the sense that they are not conditional on running. The reason for this is that the decision to run in future elections can conceptually be an outcome due to the treatment, which means that a causal interpretation of the conditional effects on being elected would not be valid. In practice, because of the previous result that there are no large effects on running probabilities, conditioning on running only scales up the future election probabilities without making a qualitative difference.

Figure 7: Probabilities of being elected in future elections to a municipal council



(a) Raw probabilities

(b) Conditional probabilities

Note: The figures plot the probability of being elected in future elections to a municipal council by rank from borderline elected in election year t. Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.
Source: Statistics Sweden & The Swedish Election Authority.

result means that having served in the council in the previous election period explains as much as 60% of the probability of being elected in the next election.[36]

For the borderline elected's 6 percentage points higher probability of being elected in the next election obtained above to be interpreted as a causal treatment effect, the direct effect of $rank^*$ must be at most of order one for $rank^* = \{-2, -1, 0\}$. Since this is an identifying assumption, it is not possible to test it directly. But there are several ways of indirectly investigating whether the obtained effects are likely to be causal. First, it is more likely that the direct effect is linear between this set of three candidates if it is also linear for another set of three candidates close by—i.e., if there are no kinks in the slope of $rank^*$ of similar magnitude between any other two candidates further down the ranking. This can be more or less inferred from the graphics, but it can also be formally tested with placebo regressions that, falsely, assign the borderline elected status to candidates with $rank^* = -1$ or $rank^* = -2$. Doing this on the probability of being elected in the first subsequent election to a municipal council results in estimated coefficients that are about 30–40% of the size of the effect for the true borderline elected (i.e., around 2–2.5 percentage points) and that are also statistically significant.[37]

One possible interpretation of these placebo estimates is that there is a non-linear direct effect of $rank^*$ (which is in fact suggested by Figure 7). But if the direct effect of rank is of higher order than one for the set of three candidates with $rank^* = \{-2, -1, 0\}$ *and* for candidates further down the ranking, the effect of being elected can still be

[36] Although by the same token, if they become council replacements, some of those who are not elected in the next election can also end up serving in the council.
[37] The placebo estimates are found in columns 1–2 of Table A4 in the Appendix.

Table 4: Effects of being elected on the probability of being elected in future elections to a municipal council

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0619***	0.0587***	0.0131	0.0114	-0.00664	-0.00573
	(0.0139)	(0.0138)	(0.0157)	(0.0155)	(0.0179)	(0.0177)
rank*	0.0659***	0.0662***	0.0418***	0.0417***	0.0257**	0.0262**
	(0.00728)	(0.00732)	(0.00858)	(0.00855)	(0.0102)	(0.0102)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 5: Effects of being elected on the probability of being elected in future elections to a municipal council; allowing non-linear effects of $rank^*$

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected, $\bar{p} = 1$	0.156***	0.104***	0.0744***	0.0409***	0.0232***	0.0178*
	(0.00774)	(0.00898)	(0.00810)	(0.00981)	(0.00824)	(0.0102)
elected, $\bar{p} = 2$	0.0827***	**0.0511*****	0.0240**	**-0.000853**	**0.0130**	**-0.00437**
	(0.00948)	**(0.0136)**	(0.0104)	**(0.0151)**	**(0.0107)**	**(0.0165)**
elected, $\bar{p} = 3$	**0.0532*****	0.0210	**0.00972**	-0.0132	0.00233	-0.0227
	(0.0124)	(0.0257)	**(0.0143)**	(0.0297)	(0.0156)	(0.0361)
Observations	54798	32504	36430	21620	18239	10888
rank* $\geq$	-10	-5	-10	-5	-10	-5

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

recovered by—as explained in Section 4—running equation (2) on more than three candidates per borderline group. The resulting estimates from this exercise are found in Table 5. Equation (2) is estimated on the sample of borderline elected candidates plus ten defeated candidates in columns 1, 3 and 5, for the first, second and fourth election, respectively. In columns 2, 4 and 6, five instead of ten defeated candidates are included in the estimations. Each column contains results from three different regressions with a linear, quadratic or qubic function of $rank^*$, of which the one preferred by the Akaike information criterion (AIC) is in bold.

The point estimates as well as the significance levels seen in Table 5 are somewhat sensitive to different bandwidths (the number of defeated candidates included) and order of polynomial. But restricting the attention to the AIC-preferred specifications, the previous result in Table 4 from estimating the baseline regression (1) is quite robust; being borderline elected in election t increases the chances of being elected in election $t + 1$ by around 5 percentage points (compared to 6 in Table 3), but does not affect the election probabilities in later elections.

An alternative way of investigating the linearity assumption underlying the baseline results in Table 4 is to test whether the estimates differ when it is unlikely that the parties could have known who would be the borderline elected to win the last seat. The idea is that the ranking of candidates would be different if it was a priori certain who would actually be elected. I propose a number of instances when there was presumably more uncertainty regarding this, and present the results in Table 6 (where column 1 reproduces the baseline results with controls in column 2 of Table 4): (i) the party's number of seats changed from the previous election, cf. column 2; (ii) the party won their last seat or were close to winning an additional seat with a vote margin of less than 1 and 0.5%, cf. columns 3–4; and (iii) a combination of (i) and (ii), cf. column

5.[38] The table shows estimates that are quite robust across the different specifications. For example, the estimate hardly changes even when the sample size is cut in half as the vote margin of the last seat is restricted to 0.5%. This is reassuring evidence that parties in general cannot perfectly anticipate how many votes they will win and rank their candidates accordingly.

Table 6: Robustness checks of the effects on being elected in the first subsequent election to a municipal council

	(1)	(2)	(3)	(4)	(5)
elected	0.0587***	0.0592***	0.0475***	0.0586***	0.0421**
	(0.0138)	(0.0163)	(0.0158)	(0.0202)	(0.0186)
rank*	0.0662***	0.0612***	0.0696***	0.0589***	0.0675***
	(0.00732)	(0.00872)	(0.00839)	(0.0113)	(0.00994)
Observations	16754	12692	13283	7737	10080
Vote margin (%)	no restr.	no restr.	1	0.5	1
$\lvert\Delta\text{seats}\rvert \geq 1$	no	yes	no	no	yes
Individual controls	yes	yes	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first subsequent election to a municipal council. Column 1 reproduces the baseline results in column 2 of Table 4. All regressions include individual controls (cf. Table 4). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Heterogeneous effects in the local arena

The previous analysis shows that, on average, candidates who are borderline elected into a municipal council are 5–6 percentage points more likely to be elected also in the next election. In this section, I analyze whether this effect differs depending on, first, the political environment and, second, on candidate characteristics.

To see how this is done, let $D = \{D_1, ..., D_{\bar{d}}\}$ be the dimension of heterogeneous effects to be explored. The regression to be estimated (on the set of three candidates with $rank^* = \{-2, -1, 0\}$) is then

$$\begin{aligned} Y_{ig,t+j} = \sum_{d=1}^{\bar{d}} \big[& \beta_{0,d} \times 1(D = D_d) + \beta_{1,d} elected_{ig,t} \times 1(D = D_d) \\ & + \beta_{2,d} rank^*_{ig,t} \times 1(D = D_d) \big] + \mathbf{\Gamma}' \mathbf{X}_{ig,t-1} + \varepsilon_{ig,t+j}, \end{aligned} \quad (3)$$

[38] The robustness checks are performed on the probability of being elected in the first subsequent election to a municipal council where the baseline estimates were significant. As vote margins, I use the minimum changes in votes to win or lose an additional seat in proportional elections as defined and calculated by Folke (2011).

where $1(\cdot)$ is the indicator function. That is, the effect of being elected, the direct effect of $rank^*$ and the intercepts are all allowed to vary with D.

Table 7 presents the resulting estimates of $\beta_{1,d}$ for three different dimensions of the political environment, namely (i) the ruling status of the party that the candidate ran for in election t ; (ii) how many seats the party had during that election period; and (iii) the size of the council that the candidate ran for, where a small/small-medium/medium-large/large council is defined to have 31–39/41–49/51–59/61+ seats in total (which needs to be an odd number).[39] Note that council size may also be seen as a proxy for the population size of the municipality.

First looking at the upper panel and ruling status, the effect on being elected in the first subsequent election is more or less the same for candidates who are borderline elected into an opposition party as for candidates who are borderline elected into a governing party, and the point estimates are close to the 5–6 percentage points to which the average effect was estimated.

Moving along to the mid panel and party size, the point estimates in election $t+1$ are somewhat larger for candidates running for the very small parties that only have 1–2 seats and perhaps also for parties with 5–9 seats, and it is only for these two groups that the estimates are statistically significant (although none of the four estimates are statistically significant from one another). For candidates in the smallest parties, the effect is still there in the $t+2$ election.

Finally, looking at the bottom panel and council size, there are larger effects in election $t+1$ in small and medium-large councils and no statistically significant effect in the largest councils with more than 61 seats. The point estimates in election $t+2$ for the small and medium-large councils are also quite large (around 5 percentage points), but are estimated with poor precision.

Next, I analyze how the effects differ across candidate characteristics—specifically, across their level of education, their age at election and whether they run for office for the first time or if they have previous political experience. For this purpose, similar but not entirely identical regressions as above are estimated (cf. equation (3)). Recall the note that the research design almost mechanically ensures that characteristics of the borderline group (or any other more aggregate entity) are uncorrelated with the treatment, since it uses a nearly-balanced panel. This is no longer the case when estimating effects interacted with individual characteristics, since that would require each of the different subgroups defined along a particular dimension D to constitute a balanced panel. When D_d in equation (3) varies within borderline groups (i.e., between individuals), borderline group-specific fixed-effects would have to be included in order to identify off the same type of within-borderline group variation as in the baseline analysis. Unfortunately, this leaves too little variation, so instead I estimate a similar but less restrictive model that includes separate fixed effects for election, municipality and party.[40]

[39] Table A2 in the Appendix shows the size of the various subgroups along with subgroup-specific mean probabilities of being elected in future elections to a municipal council.

[40] In practice, it turns out that the estimates of the $\beta_{1,d}$:s are very similar with and without these fixed

Table 7: Effects of being elected on the probability of being elected in future elections to a municipal council; by parties' ruling status and size and by council size

	Election		
	t+1	t+2	t+4
elected× opposition party	0.0660***	0.0174	0.0118
	(0.0187)	(0.0221)	(0.0247)
elected× governing party	0.0499**	0.00471	-0.0211
	(0.0216)	(0.0229)	(0.0237)
Observations	16754	11208	5710
elected× 1–2 seats	0.0949***	0.0599**	0.00334
	(0.0273)	(0.0285)	(0.0288)
elected× 3–4 seats	0.0393	-0.00103	0.0215
	(0.0280)	(0.0323)	(0.0341)
elected× 5–9 seats	0.0670**	-0.0259	-0.0614
	(0.0296)	(0.0316)	(0.0379)
elected× 10+ seats	0.0262	0.00675	0.00437
	(0.0289)	(0.0307)	(0.0410)
Observations	16754	11208	5710
elected× small council	0.0950***	0.0461	-0.0140
	(0.0292)	(0.0330)	(0.0364)
elected× small-medium council	0.0464**	-0.0109	-0.0377
	(0.0201)	(0.0224)	(0.0247)
elected× medium-large council	0.0952**	0.0579	0.0991*
	(0.0428)	(0.0463)	(0.0541)
elected× large council	0.0299	0.0109	0.0503
	(0.0310)	(0.0356)	(0.0437)
Observations	16754	11208	5710
Individual controls	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first (column 1), second (column 2) and fourth (column 3) subsequent election to a municipal council. Effects are allowed to differ by the parties' ruling status (top panel) and size (mid panel) and by council size (bottom panel). The intercept and the direct effect of *rank** are also allowed to vary by party ruling status (top) and size (mid) and by council size (bottom). All regressions include individual controls (cf. Table 4). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 8: Effects of being elected on the probability of being elected in future elections to a municipal council; by politicians' level of education, age at election and previous political experience

	Election		
	t+1	t+2	t+4
elected× < high school	0.0364	0.0228	0.0329
	(0.0357)	(0.0356)	(0.0315)
elected× < 2 years university	0.0461**	-0.0107	-0.0393
	(0.0212)	(0.0238)	(0.0280)
elected× ≥ 2 years university	0.0788***	0.0307	0.0109
	(0.0244)	(0.0241)	(0.0289)
Observations	16688	11168	5699
elected× age 18–39	0.0724**	-0.00639	-0.0577
	(0.0298)	(0.0344)	(0.0370)
elected× age 40–49	0.00822	-0.00943	-0.00139
	(0.0274)	(0.0308)	(0.0351)
elected× age 50–59	0.0906***	0.0462	0.0342
	(0.0284)	(0.0291)	(0.0329)
elected× age 60+	0.0660**	0.0166	-0.000110
	(0.0274)	(0.0273)	(0.0167)
Observations	16754	11208	5710
elected× no prev. experience	0.0458	0.0636*	
	(0.0312)	(0.0376)	
elected× with prev. experience	0.0672***	0.00356	
	(0.0222)	(0.0264)	
Observations	11044	5498	
Individual controls	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first (column 1), second (column 2) and fourth (column 3) subsequent election to a municipal council. Effects are allowed to differ by politicians' level of education (top panel), age at election (mid panel) and previous political experience (bottom panel). The intercept and the direct effect of *rank** are also allowed to vary by politicians' level of education (top), age at election (mid) and previous political experience (bottom). All regressions include individual controls (cf. Table 4) and election, municipality and party fixed-effects. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

The upper panel of Table 8 gives the estimates of the effect of being elected for politicians with different levels of education. Interestingly, the effect in election $t + 1$ seems to be increasing with the level of education. The interpretation of the coefficient for at least two years of university, for example, is that such candidates who are borderline elected in election t are 8 percentage points more likely to be elected also in the next election, as compared to borderline defeated candidates with the same level of education. The point estimate for candidates who have not completed high school is not statistically significant and smaller in magnitude, but due to the rather large standard errors, it cannot be rejected from that of the more highly educated candidates. The mid panel instead gives estimates by the candidate's age at election and shows there to be a positive effect on being elected in the first subsequent election for all ages but 40–49.

Finally, consider the separate effects of being elected for candidates who run for office for the first time against those with previous political experience (from local, regional or national politics). Since the data starts at the 1991 election and ends with the 2006 election, these effects can only be estimated on the 1998 and 2002 sample and not for effects in the $t + 4$ elections. This also means that candidates' political experience from before 1991 is not observed, so that "running for the first time" may be measured with error. With that in mind, the conclusion from the bottom panel of Table 8 is that there are no or only minor short-run heterogeneous effects across this dimension; even though, for election $t + 1$, only the estimate for candidates with previous experience is statistically significant, the size of the point estimates is quite similar (and statistically not distinguishable from one another). In the medium run, however, the estimate is as high as 6 percentage points for unexperienced candidates (significant at the 10% level) but close to zero for those with previous experience. A possible explanation for this null effect—as well as for that on 40–49-year olds—is that these groups leave local politics and instead advance to the national arena; see Section 8.2.

Overall, the heterogeneity analysis is, first, suggestive of larger short-run effects for well-educated candidates. A general caveat, however, is the relatively large standard errors of some of the estimates, which hinders precise comparisons of estimates across political environments and individual characteristics. Moreover, it is interesting that medium- and long-run effects are absent for the most part (exceptions being medium-run effects among candidates running for the smallest parties and with no previous political experience). As noted above, this suggests that many of the borderline defeated in election t are, in fact, elected in later elections.

The heterogeneity analysis also suggests that being borderline elected into a council improves the chances of being elected also in the next election for candidates who do not run for the largest parties or the largest councils. In part, council and party size probably capture the same thing, as it is more common to only have one or two seats in a council that has few seats in total. One possible mechanism behind these patterns is that the borderline elected can be a quite prominent figure in small parties and councils, whereas the borderline elected in large parties and councils is just a marginal guy who is less visible.

effects, which is reassuring.

Recall that running the regression in (1) with actually having served in the council as the dependent variable yields an estimate of β_1 of around 0.30 (which can only be done with data from the 2002 and 2006 election). Similar estimates are obtained if the sample is split according to the various subgroups in Tables 7 and 8—except across party size; then, the estimate ranges from around 0.55 for the smallest parties to around 0.10 for the largest parties (both statistically significant at the 1% level). In other words, treatment is more fuzzy in the larger parties. The reason is that because a replacement can stand in for several regular members, defeated candidates from larger parties who are council replacements are more likely to take over a permanent seat, as compared to defeated candidates from smaller parties. Another, perhaps more likely, explanation for the stronger results for the smaller parties is therefore that there is not sufficient variation in de facto treatment—i.e., of actually having served in the council—between the borderline elected and defeated candidates from the larger parties.

8.2 National politics

If politicians are motivated by politics-specific non-monetary payoffs such as power or prestige, their goal should be to pursue a successful political career. So far, the analysis has focused on local political careers. This section investigates whether being a local politician can be a path to national politics. Specifically, I investigate whether being elected into a municipal council in election t affects the probability of being nominated to the national parliament in election $t + 1$, $t + 2$ and $t + 4$. The outline is the same as in the previous section; I begin by presenting graphical and econometric results on the average effect, then test the robustness of the baseline results and end the section with a heterogeneity analysis.

The average effects are presented graphically in Figure 8 with associated estimates in Table 9, which reveal a clear and significant positive effect in the short and medium run (first and second subsequent elections). The magnitude is around 2–3 percentage points in both elections (although somewhat higher in the short run), which is around twice the size of the estimated direct effect of $rank^*$ and around 30% of the overall mean probability of running for the national parliament among candidates with $rank^* = \{-2, -1, 0\}$ in the borderline groups. Considering, instead, the treatment of actually having served in the council (of which the average probability jumps by around 0.30 for the borderline elected)[41], an alternative interpretation of these results is that short- and medium-run chances of being nominated to the national parliament are almost fully explained by municipal council experience.

Compared to the graphical analysis on local election probabilities above (cf. Figure 7), the linearity assumption in the direct effect of $rank^*$ seems less restrictive here. Moreover, supporting the interpretation of the kinks at $rank^* = -1$ as representing an effect of being elected is that placebo regressions of the short- and medium-run probabilities of being nominated to the national parliament that assign the borderline

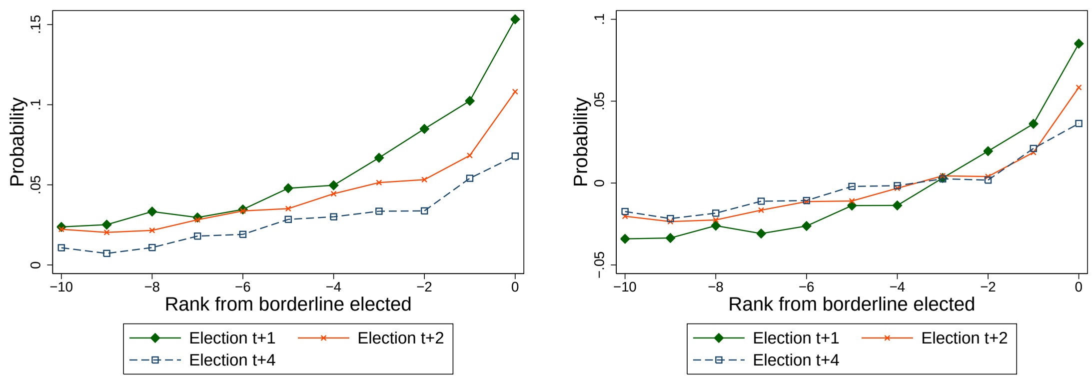
[41] Recall from Section 6 that this figure is obtained by using the 2002 and 2006 election data to estimate a first stage with the probability of actually having served in the council as a discontinuous function of $rank^*$.

Table 9: Effects of being elected on the probability of being nominated in future elections to the national parliament

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0334***	0.0330***	0.0247**	0.0259***	-0.00647	-0.00235
	(0.0102)	(0.0100)	(0.00977)	(0.00964)	(0.0125)	(0.0125)
rank*	0.0175***	0.0172***	0.0151***	0.0146***	0.0203***	0.0187***
	(0.00510)	(0.00495)	(0.00554)	(0.00549)	(0.00672)	(0.00678)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

Note: The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Figure 8: Probabilities of being nominated in future elections to the national parliament



(a) Raw probabilities (b) Conditional probabilities

Note: The figures plot the probability of being nominated in future elections to the national parliament by rank from borderline elected in election year t. Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.
Source: Statistics Sweden & The Swedish Election Authority.

elected status to candidates with $rank^* = -1$ or $rank^* = -2$ yield small and insignificant estimates—a result in accordance with what is suggested by the graphs.[42]

For long-run effects—that is, for effects on being nominated in election $t + 4$—the graphs are suggestive of a kink at $rank^* = -2$. In a placebo regression, this "effect" is estimated to 2 percentage points and is significant at the 10% level. Because this is a long-run outcome, a possible explanation is that the borderline defeated in election t ran successfully for the municipal council in election $t + 1$, which increased the chances of running for parliament in election $t + 4$.

As in the previous section, the baseline results in Table 9 from estimating equation (1) on the set of three candidates per borderline group are complemented with results from estimating equation (2), expanding the bandwidth to include five or ten defeated candidates. The results are given in Table 10. These estimates are more robust across the different bandwidths and polynomials, and thus give little reason to doubt the effect of a 2–3 percentage point increase in the probability of being nominated in the first and second subsequent election to the national parliament.

To further support this conclusion, Table 11 tests the robustness of the statistically significant effects on being nominated for parliament in elections $t+1$ and $t+2$. Column 1 reproduces the baseline estimates with controls in columns 2 and 4 of Table 9, and columns 2–4 are the result from the same set of robustness checks as in Table 6 for local politics above. As can be seen, the effects in the first subsequent election are very

[42] Placebo estimates for the probability of being nominated for the national parliament are provided in Table A5 in the Appendix.

Table 10: Effects of being elected on the probability of being nominated in future elections to the national parliament; allowing non-linear effects of $rank^*$

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected, $\bar{p} = 1$	0.0566***	0.0395***	0.0415***	**0.0350******	0.0182***	0.0149**
	(0.00560)	(0.00648)	(0.00541)	**(0.00645)**	(0.00612)	(0.00717)
elected, $\bar{p} = 2$	0.0318***	**0.0238****	**0.0318*****	0.0316***	**0.0108**	**-0.00208**
	(0.00688)	**(0.0102)**	**(0.00687)**	(0.00876)	**(0.00776)**	**(0.0117)**
elected, $\bar{p} = 3$	**0.0240****	0.0458**	0.0327***	0.00986	0.00689	-0.0274
	(0.00934)	(0.0181)	(0.00861)	(0.0180)	(0.0104)	(0.0234)
Observations	54798	32504	36430	21620	18239	10888
rank* ≥	-10	-5	-10	-5	-10	-5

Note: The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

robust, while the effects in the election thereafter only partly survive the various sample restrictions. For the latter, the drop in the coefficient is especially large when the sample is restricted to parties that were 0.5% votes from winning or losing an additional seat (column 4, bottom panel). This drop is most likely due to a combination of the sample size reduction of 50% and—as will be seen in the following section—the fact that effects are quite heterogeneous across party and council size.

Heterogeneous effects in the national arena

According to the above estimates, candidates who are borderline elected into a municipal council are on average 2–3 percentage points more likely to be nominated to the national parliament in the first and second subsequent elections. As in Section 8.1 on the local arena, this section considers how these average effects differ across the political environment and across candidate characteristics.

Consider, first, heterogeneous effects across the political environment as provided in Table 12. As shown in the top panel, only candidates who ran for an opposition party in election t go on to be nominated for the national parliament in election $t + 1$ and $t + 2$ (although the difference is only statistically significant in election $t + 1$). This is an interesting result which may be explained by local politicians in governing majority being content with their position, while local politicians in opposition feel that they could accomplish more if they were to advance to the national arena.

Next, consider party size in the mid panel, which shows that candidates borderline elected into the smallest parties that only have 1–2 seats in the council are around 9 percentage points more likely to be nominated for parliament in the first subsequent election

Table 11: Robustness checks of the effects on being nominated in the first and second subsequent elections to the national parliament

	(1)	(2)	(3)	(4)	(5)
		Election $t+1$			
elected	0.0330***	0.0246**	0.0448***	0.0331**	0.0355***
	(0.00990)	(0.0110)	(0.0107)	(0.0140)	(0.0117)
rank*	0.0152***	0.0146**	0.00839	0.00866	0.00710
	(0.00489)	(0.00565)	(0.00543)	(0.00703)	(0.00621)
Observations	16754	12692	13283	7737	10080
		Election $t+2$			
elected	0.0259***	0.0172	0.0278***	0.0136	0.0221*
	(0.00964)	(0.0107)	(0.0107)	(0.0139)	(0.0117)
rank*	0.0146***	0.0161**	0.0130**	0.0153*	0.0118*
	(0.00549)	(0.00656)	(0.00586)	(0.00790)	(0.00673)
Observations	11208	8662	8947	5276	6934
Vote margin (%)	no restr.	no restr.	1	0.5	1
$\lvert\Delta\text{seats}\rvert \geq 1$	no	yes	no	no	yes
Individual controls	yes	yes	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being elected in the first (top panel) and second (bottom panel) subsequent election to the national parliament. Column 1 reproduces the baseline results in columns 2 and 4 of Table 9. All regressions include individual controls (cf. Table 9). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

and around 6 percentage points in the second subsequent election.[43] In terms of magnitude, these effects are large—they correspond to around 50% of the overall probabilities of running for parliament among this subgroup of candidates.[44] Analogously to the average treatment effects in Table 9, the estimated effects for this subgroup imply that the chances of being nominated for the national parliament are almost fully explained by the de facto treatment of having served in a local council. Also those borderline elected into parties with 3–4 seats are more likely to be nominated for the parliament, albeit only in the second subsequent election.[45] This point estimate of around 5 percentage points also corresponds to around 50% of the overall mean for this subgroup and thus, again, is a large effect.

Being borderline elected into a larger party does not increase the probability of later being nominated to the national parliament, however. And a similar picture pertains to council size; it is only to the small and small-medium councils (up to 49 seats) that being borderline elected increases the probability of future nominations to the parliament.[46] In terms of magnitude, also these point estimates correspond to around half of the overall mean among these subgroups. Just like the more pronounced effects on local election probabilities for candidates in smaller parties and councils, these null effects are most likely explained by the lack of sufficient variation in de facto treatment between the borderline elected and defeated candidates from the larger parties.

Moving along to see for which types of candidates future nomination probabilities increase after they have been borderline elected into a council, the top panel of Table 13 reveals that the strongest short-run effects are estimated for the most educated. However, also the point estimate for the least educated is quite large and significant at the 10% level, so it is hard to draw any strong conclusion from this. In the medium run (election $t + 2$), the estimates across the different levels of education are very similar.

The mid panel instead provides separate estimates by age at election and shows that, in a statistical sense, all but the youngest are more likely to be nominated to the parliament in election $t + 1$ as an effect of being elected locally in election t (although the point estimate for the youngest is not statistically significantly different from the estimates for the older subgroups). Expect for the oldest group (aged 60 or older at election), the effects in election $t + 2$ are also quite homogeneous, most likely because the oldest group has retired by then.

Recall from the analysis in Section 8.1, first, that there was a positive short-run effect for the youngest group on being elected in subsequent local elections, whereas there was no effect for the group of 40–49-year olds and, second, that there was a positive medium-run effect for unexperienced candidates but no such effect for those

[43] For election $t+1$, the difference from all other three subgroups is statistically significant. For election $t + 2$, the difference is statistically significant from the groups with 5–9 or 10+ seats.

[44] Table A3 in the Appendix shows the subgroup-specific mean probabilities of being nominated in future elections to the national parliament.

[45] Although this estimate is only statistically significant from the 5–9-seats subgroup.

[46] For election $t + 1$, the difference between the estimates for small/small-medium councils on the one hand and those for medium-large/large councils on the other hand are statistically significant. For election $t + 1$, only the difference between small-medium and large councils is statistically significant.

Table 12: Effects of being elected on the probability of being nominated in future elections to the national parliament; by parties' ruling status and size and by council size

	Election		
	t+1	t+2	t+4
elected× opposition party	0.0522***	0.0360***	0.00957
	(0.0131)	(0.0132)	(0.0168)
elected× governing party	0.0101	0.0149	-0.0127
	(0.0157)	(0.0135)	(0.0174)
Observations	16754	11208	5710
elected× 1–2 seats	0.0892***	0.0610**	0.0168
	(0.0226)	(0.0268)	(0.0271)
elected× 3–4 seats	0.00941	0.0491**	0.000647
	(0.0234)	(0.0215)	(0.0297)
elected× 5–9 seats	0.0249	-0.0164	-0.0154
	(0.0166)	(0.0184)	(0.0203)
elected× 10+ seats	0.00962	0.0100	-0.0128
	(0.0124)	(0.0134)	(0.0183)
Observations	16754	11208	5710
elected× small council	0.0394**	0.0131	-0.0159
	(0.0176)	(0.0238)	(0.0286)
elected× small-medium council	0.0640***	0.0462***	0.00375
	(0.0135)	(0.0122)	(0.0162)
elected× medium-large council	-0.0361	0.00982	0.0185
	(0.0341)	(0.0328)	(0.0368)
elected× large council	-0.0105	0.00221	-0.0119
	(0.0246)	(0.0215)	(0.0345)
Observations	16754	11208	5710
Individual controls	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being nominated in the first (column 1), second (column 2) and fourth (column 3) subsequent election to the national parliament. Effects are allowed to differ by the parties' ruling status (top panel) and size (mid panel) and by council size (bottom panel). The intercept and the direct effect of *rank** are also allowed to vary by party ruling status (top) and size (mid) and by council size (bottom). All regressions include individual controls (cf. Table 9). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 13: Effects of being elected on the probability of being nominated in future elections to the national parliament; by politicians' level of education, age at election and previous political experience

	Election		
	t+1	t+2	t+4
elected× < high school	0.0353*	0.0285*	0.0177
	(0.0186)	(0.0150)	(0.0146)
elected× < 2 years university	0.0183	0.0280**	-0.0151
	(0.0144)	(0.0138)	(0.0205)
elected× ≥ 2 years university	0.0516***	0.0251	0.00420
	(0.0184)	(0.0210)	(0.0224)
Observations	16688	11168	5699
elected× age 18–39	0.0214	0.0388	-0.0175
	(0.0253)	(0.0239)	(0.0334)
elected× age 40–49	0.0439**	0.0293	0.00227
	(0.0197)	(0.0189)	(0.0216)
elected× age 50–59	0.0395**	0.0362**	0.000622
	(0.0161)	(0.0168)	(0.0212)
elected× age 60+	0.0278*	-0.00485	0.0179
	(0.0160)	(0.0163)	(0.0132)
Observations	16754	11208	5710
elected× no prev. experience	0.0298	0.0115	
	(0.0203)	(0.0260)	
elected× with prev. experience	0.0286*	0.0337**	
	(0.0149)	(0.0158)	
Observations	11044	5498	
Individual controls	yes	yes	yes

Note: The table reports effects of being elected into a municipal council on the probability of being nominated in the first (column 1), second (column 2) and fourth (column 3) subsequent election to the national parliament. Effects are allowed to differ by politicians' level of education (top panel), age at election (mid panel) and previous political experience (bottom panel). The intercept and the direct effect of *rank** are also allowed to vary by politicians' level of education (top), age at election (mid) and previous political experience (bottom). All regressions include individual controls (cf. Table 9) and election, municipality and party fixed-effects. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

with previous political experience. From the effects over age seen in Table 13 along with the results in the bottom panel over political experience—showing a statistically significant effect on advancing to national politics of having been elected locally only for the experienced group (though not significantly different from the unexperienced)—it may be concluded that the absence of positive effects on local election probabilities for these subgroups is explained by them leaving the local arena for national politics.

9 Concluding remarks

By estimating causal effects of being elected in local elections on income and political career prospects, this paper has looked for empirical evidence of what types of payoffs that motivate politicians. I argue that local politics is the relevant context for studying politicians' motivations, since this is where the majority of political careers start off.

Using a regression discontinuity design where the income of elected candidates who just barely won a seat (*the borderline elected*) is compared to that of non-elected candidates who were close to winning a seat (*the borderline defeated*), the paper has shown that monetary returns from politics are absent. This seems to be true irrespective of if one considers the period right after the election, up to 15 years later or the period right after exiting politics. It is also true on average as well as when considering heterogeneous effects across various dimensions of parties, councils and candidates.

The result of no positive monetary returns from politics stands in contrast to findings from the few previous studies: Diermeier et al. (2005) find that political experience increases earnings for US congressmen upon exiting politics, and Eggers and Hainmueller (2009) estimate substantial effects on wealth (at the time of death) for conservative politicians who were elected into the British parliament with narrow vote margins. These contrasting results can potentially be explained with their focus on national rather than local politics. Another interesting potential explanation is the difference in political institutions. Sweden is characterized by a typical multi-party, proportional representation system where the parties are the main political players. Therefore, it is likely that monetary returns are larger in countries like the US and Great Britain where the focus lies (more or less) on individual politicians.

Given that there are no positive monetary returns, is it then worth it? If politicians are motivated by politics-specific non-monetary payoffs such as power or prestige, their goal should be to pursue a successful political career. Hence, the paper looked at whether—for exogenous reasons—being elected in a local election improves future political career prospects. The main conclusion from this analysis is that being borderline elected into a municipal council improves political career prospects, especially through increased chances of being nominated in future elections to the national parliament, but also of being elected in future local elections—at least in the short run. If the goal of politicians is to make a successful political career, the local arena can thus be a platform from which to start off.

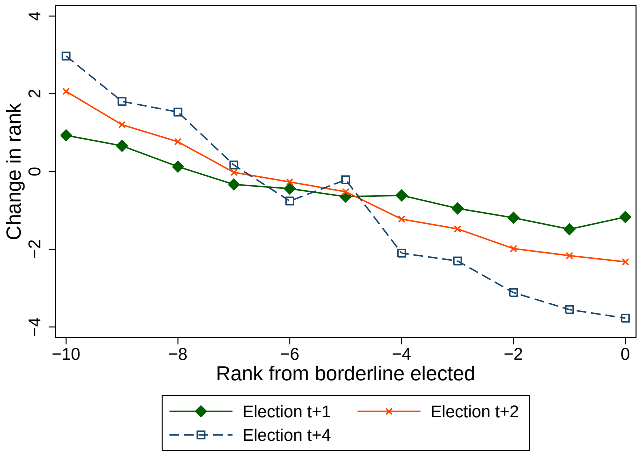
Interestingly, there is no strong evidence that the effects on political career prospects differ across candidates with different levels of education (at least not when considering the chances of advancing to the national arena), or across candidates with or without previous political experience. If anything, it seems as if the absence of positive effects on local election probabilities for some subgroups is explained by them leaving the local arena for national politics.

A caveat with the comparisons of some of the subgroup-specific effects, however, is that they are estimated with rather poor precision, which hinders precise comparisons of estimates across political environments and individual characteristics. But taken together, the results show that the positive effects of being borderline elected on future local election probabilities and national nominations are especially pronounced for candidates running for smaller parties and smaller councils. One possible mechanism behind these patterns is that those borderline elected to larger parties are marginal politicians who are less visible, whereas those borderline elected into smaller parties can be quite prominent figures. Under this mechanism, the interpretation of the results is that what is of importance for political career prospects is being a prominent local representative, and not simply being elected. An alternative but related explanation for these patterns is that what matters for political career prospects is actual council experience, rather than being elected into the council on election day. This would imply that the null effects among the larger parties are explained by a lack of sufficient variation in council experience between the borderline elected and defeated candidates. The reason for this—i.e., why the de facto treatment of actually having served in the council is more "fuzzy" between elected and defeated candidates in the larger parties—is that, because a replacement can stand in for several regular members, defeated candidates from larger parties who are council replacements are more likely to take over a permanent seat (as compared to defeated candidates from smaller parties).

A key, robust finding is that being borderline elected into a municipal council increases the probability of being nominated for the national parliament in subsequent elections by as much as 50% (for some subgroups). Considering, instead, the treatment of actually having served in the council, these results imply that the chances for nominations are almost fully determined by municipal council experience. In this paper, the analysis of national politics stopped at nominations. The reason is that the parliament only has 349 seats, which makes actually being elected a very rare event which, in turn, hinders a full-scale econometric analysis of election probabilities. However, a follow-up of these candidates is an important future task—if only descriptively.

Another finding is that being borderline elected into a municipal council also seems to increase the chances of being elected in the next local election. This, together with the fact that the overall probabilities of running in many subsequent municipal council elections is quite high, suggests that there are interesting dynamics within the political parties to be explored. For example, are those elected rewarded for serving a term with a higher rank in the next election, and/or are those not elected compensated in the next election? Merely for suggestive purposes, Figure 9 plots the change in rank for candidates who ran in several consecutive elections, and reveals an interesting picture: Candidates who start out way down advance in the ranking with each election, while candidates who are already quite high in the ranking show the opposite pattern. This picture may be the result of selection, in the sense that candidates who start out low in the ranking are not willing to run in subsequent elections unless they receive a higher rank. But it may also reveal something more fundamental about the internal processes within the political parties. Digging deeper into these and other related mechanism is an interesting avenue for future research.

Figure 9: Change in rank between election t and future elections to a municipal council



Note: By rank from borderline elected in election year t, the figure plots the change in rank between election t and future elections to a municipal council.

Source: Statistics Sweden & The Swedish Election Authority.

Appendix

Determining the final ranking

This Appendix describes some of the complications in determining the order in which candidates for a party with a given number of seats is elected—i.e., how the ballot paper rankings translate into the final ranking. The full procedure is stipulated in the Elections Act.

Starting with the 1998 election, voters can mark *one* preferred candidate on the ballot paper (so-called preference voting). The top of the final ranking is set based on the ranking of such preference votes, given that a candidate has reached the preference vote threshold of 5% of the party's votes in the constituency, which must be at least 50 votes.

For candidates who do not reach the preference vote threshold (or for all candidates prior to the 1998 election), comparison numbers are calculated and ranked. The comparison numbers are calculated based on votes per ballot paper and the so-called "whole-number method". In the case of one ballot paper per constituency, the ranking of comparison numbers simply boils down to the party's ballot paper ranking of candidates who did not reach the preference vote threshold. These relatively simple cases constitute around 90%. Matters become much more complex in the case of multiple ballot papers per constituency, where comparison numbers and the associated final ranking depend on a combination of the number of votes per ballot paper and the number of ballot papers and how high each candidate was ranked on the various ballot papers.

Additional complications in determining the final ranking arise when candidates are sufficiently highly ranked in several constituencies (or for several parties, although this rarely happens), for example as a consequence of their party running with the same ballot paper in several constituencies. This happens in around 30% of the cases. A candidate can only fill one seat, which leaves the remaining seats to be assigned to someone else—a procedure known as "double-election replacement".

Finding the borderline groups

When the final ranking is completely known, it is quite straightforward to determine which candidates constitute the borderline groups. However, not all data is in sufficient detail to allow for completely determining the final ranking and hence, to find the borderline groups without making some assumptions.

Due to the lack of ballot paper rankings, it is not possible to determine any borderline groups in the 1994 election. Also the 1991, 1998 and 2002 election data is in different levels of detail—the later the election, the more detailed the data.

For the 2002 election, data is sufficiently detailed to reproduce nearly the exact final ranking. The exception is preference votes, where the information is limited to whether or not a candidate reached the threshold but not by how much, hindering ranking of such candidates and implying that identifying a borderline elected is only possible when at least one candidate is elected via comparison numbers. This also implies that the borderline elected is never elected via preference votes but always via comparison numbers.

Determining the final ranking in the 2002 election by applying the rules as stipulated in the Elections Act to the various combinations of ballot paper rankings and ballot paper votes results in the error event that a candidate is labeled as elected in a particular *constituency* when in fact he is not, or vice versa, that amounts to 0.8%. The corresponding percentage at the *council* level is as low as 0.03.[47]

For the 1991 and 1998 elections, some assumptions were needed about the interdependence of ballot papers in the case of multiple-constituency municipalities and/or constituency-overlapping ballot papers to identify the borderline groups. Applying the assumptions used for the 1998 election to the 2002 election results in about 90% identical borderline groups consisting of the sets of three candidates with $rank^* = \{-2, -1, 0\}$.

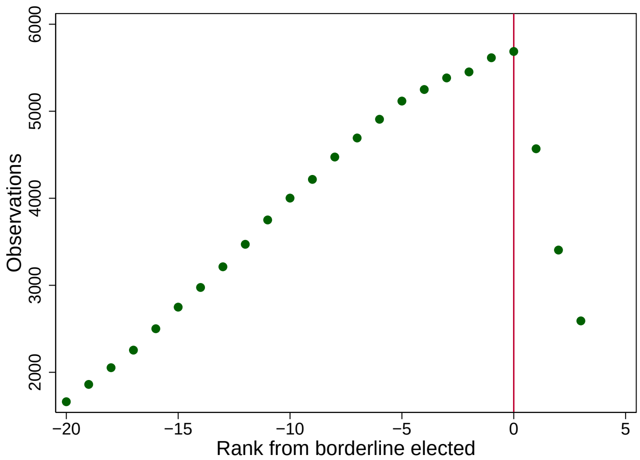
The majority of borderline groups are at the constituency level. However, when a ballot paper overlaps several constituencies, the group is at the municipality level. The reason is that it is hard to think of a candidate as being borderline elected in

[47] These error events can be calculated using an indicator contained in the data for whether or not a candidate was elected.

some constituency if other candidates on the same ballot paper were also elected, but in a different constituency. This can happen as a consequence of the double-election replacement procedure.

Candidates with missing values on either of the control variables are dropped in all estimations. Furthermore, only candidates from borderline groups that have a borderline elected are included. Groups missing a borderline elected mainly occur because the candidate is dropped due to missing values on control variables, or because no candidate within the group was elected via preference votes.

Figure A1: Number of observations by rank from borderline elected



Source: Statistics Sweden & The Swedish Election Authority.

The final number of borderline elected candidates amounts to 1917, 1838 and 1837 from the 1991, 1998 and 2002 election, respectively. Because the preference votes were only introduced in 1998 and as only candidates elected via comparison numbers can be borderline elected, the 1991 number is somewhat larger. Figure A1 shows the corresponding number of candidates at each $rank^*$ in the range $-20 \leq rank^* \leq 3$, but summed over all three elections. The reason why the number of observations decreases to the left of the borderline elected (at $rank^* = 0$) is that some groups lack a sufficiently large number of defeated candidates to assign low values of $rank^*$ to anyone.[48] Analogously, the main reason why the number of observations decreases to the right of the borderline elected is that many parties (and hence groups) only have a few seats in the council, so that being ranked several positions higher than the borderline elected is not possible. Compared to the lack of a sufficiently large number of defeated candidates, it is much more likely that the lack of candidates ranked higher than the borderline elected that follows from being a small party is systematically related to the outcome. This is the rationale for estimating the direct effect of $rank^*$ using additional defeated candidates rather than additional elected candidates.

[48] Note that with the largest bandwidth used in the paper, the sample is restricted to $rank^* \geq -10$.

Table A1: Descriptive statistics of outcome variables for candidates in the borderline groups with $rank^* = \{-2, -1, 0\}$

	Short run: 1–3 years/1 election later				
	mean	std.dev	min	max	obs
Average disposable income	1358.7	597.4	361.3	4059.3	16673
Run for municipal council	0.70	0.46	0	1	16754
Elected into municipal council	0.29	0.45	0	1	16754
Run for national parliament	0.11	0.32	0	1	16754
	Medium run: 6–8 years/2 elections later				
	mean	std.dev	min	max	obs
Average disposable income	1477.7	736.5	377	5163.3	10915
Run for municipal council	0.51	0.50	0	1	11208
Elected into municipal council	0.20	0.40	0	1	11208
Run for national parliament	0.077	0.27	0	1	11208
	Long run: 13–15 years/4 elections later				
	mean	std.dev	min	max	obs
Average disposable income	1764.1	925.6	436.3	6099	5283
Run for municipal council	0.31	0.46	0	1	5710
Elected into municipal council	0.11	0.31	0	1	5710
Run for national parliament	0.053	0.22	0	1	5710

Note: The sample for short-run outcomes includes borderline groups from the 1991, 1998 and 2002 elections, the sample for medium-run outcomes includes borderline groups from the 1991 and 1998 elections and the sample for long-run outcomes includes borderline groups from the 1991 election. Income is measured in 100 SEK deflated to 2000 year values (6.50 SEK≈1 USD), all other variables are indicator variables.

Source: Statistics Sweden & The Swedish Election Authority.

Table A2: Subgroup-specific probabilities of being elected in future elections to a municipal council

	Election		
	t+1	t+2	t+4
Opposition party, $\bar{D} = 0.54$	0.28	0.19	0.10
Governing party, $\bar{D} = 0.46$	0.30	0.21	0.12
1–2 seats, $\bar{D} = 0.24$	0.25	0.18	0.081
3–4 seats, $\bar{D} = 0.24$	0.24	0.18	0.10
5–9 seats, $\bar{D} = 0.25$	0.28	0.19	0.11
10+ seats, $\bar{D} = 0.27$	0.37	0.25	0.14
Small council, $\bar{D} = 0.22$	0.29	0.20	0.10
Small-medium council, $\bar{D} = 0.48$	0.28	0.19	0.11
Medium-large council, $\bar{D} = 0.11$	0.29	0.21	0.12
Large council, $\bar{D} = 0.19$	0.31	0.23	0.12
$<$ high school, $\bar{D} = 0.15$	0.27	0.16	0.074
$<$ 2 years university, $\bar{D} = 0.47$	0.30	0.21	0.13
$\geq$ 2 years university, $\bar{D} = 0.38$	0.28	0.21	0.11
Age 18–39, $\bar{D} = 0.23$	0.29	0.21	0.14
Age 40–49, $\bar{D} = 0.28$	0.32	0.25	0.15
Age 50–59, $\bar{D} = 0.29$	0.32	0.21	0.091
Age 60+, $\bar{D} = 0.21$	0.21	0.088	0.0067
No prev. experience, $\bar{D} = 0.34$	0.28	0.18	
With prev. experience, $\bar{D} = 0.66$	0.28	0.19	

Note: For subgroups of candidates in the borderline groups with $rank^* = \{-2, -1, 0\}$, the table reports the average probability of being elected in the first (column 1), second (column 2) and fourth (column 3) subsequent election to a municipal council. $\bar{D}$ denotes the fraction of the sample belonging to the specific subgroup.

Source: Statistics Sweden & The Swedish Election Authority.

Table A3: Subgroup-specific probabilities of being nominated in future elections to the national parliament

	Election		
	t+1	t+2	t+4
Opposition party, $\bar{D} = 0.54$	0.12	0.081	0.053
Governing party, $\bar{D} = 0.46$	0.10	0.073	0.052
1–2 seats, $\bar{D} = 0.24$	0.18	0.12	0.085
3–4 seats, $\bar{D} = 0.24$	0.14	0.089	0.072
5–9 seats, $\bar{D} = 0.25$	0.090	0.068	0.035
10+ seats, $\bar{D} = 0.27$	0.045	0.035	0.020
Small council, $\bar{D} = 0.22$	0.085	0.058	0.041
Small-medium council, $\bar{D} = 0.48$	0.11	0.069	0.050
Medium-large council, $\bar{D} = 0.11$	0.12	0.083	0.052
Large council, $\bar{D} = 0.19$	0.15	0.12	0.071
$<$ high school, $\bar{D} = 0.15$	0.055	0.032	0.016
$<$ 2 years university, $\bar{D} = 0.47$	0.11	0.076	0.055
$\geq$ 2 years university, $\bar{D} = 0.38$	0.14	0.098	0.067
Age 18–39, $\bar{D} = 0.23$	0.16	0.11	0.090
Age 40–49, $\bar{D} = 0.27$	0.13	0.099	0.056
Age 50–59, $\bar{D} = 0.29$	0.093	0.058	0.039
Age 60+, $\bar{D} = 0.21$	0.062	0.029	0.0089
No prev. experience, $\bar{D} = 0.34$	0.11	0.073	
With prev. experience, $\bar{D} = 0.66$	0.10	0.068	

Note: For subgroups of candidates in the borderline groups with $rank^* = \{-2, -1, 0\}$, the table reports the average probability of being nominated in the first (column 1), second (column 2) and fourth (column 3) subsequent election to the national parliament. $\bar{D}$ denotes the fraction of the sample belonging to the specific subgroup.

Source: Statistics Sweden & The Swedish Election Authority.

Placebo estimates

Table A4: Placebo estimates on the probability of being elected in future elections to a municipal council

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected^{placebo}	0.0195*	0.0264**	0.00946	0.0229*	0.0266	-0.0127
	(0.0117)	(0.0118)	(0.0138)	(0.0132)	(0.0167)	(0.0150)
rank*	0.0469***	0.0209***	0.0326***	0.0109	-0.000392	0.0125
	(0.00666)	(0.00689)	(0.00768)	(0.00756)	(0.00907)	(0.00870)
Observations	16450	16085	10958	10633	5498	5268
Cut-off at rank*:	-1	-2	-1	-2	-1	-2
Individual controls	yes	yes	yes	yes	yes	yes

Note: The table reports placebo estimates of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. All regressions include individual controls (cf. Table 4). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table A5: Placebo estimates on the probability of being nominated in future elections to the national parliament

	Election t+1		Election t+2		Election t+4	
	(1)	(2)	(3)	(4)	(5)	(6)
elected^{placebo}	0.000748	-0.0000453	0.0154	-0.00817	0.0200*	-0.00566
	(0.00811)	(0.00775)	(0.0100)	(0.00957)	(0.0113)	(0.0100)
rank*	0.0167***	0.0171***	-0.000597	0.00788	-0.000817	0.00475
	(0.00476)	(0.00439)	(0.00554)	(0.00537)	(0.00616)	(0.00549)
Observations	16450	16085	10958	10633	5498	5268
Cut-off at rank*:	-1	-2	-1	-2	-1	-2
Individual controls	yes	yes	yes	yes	yes	yes

Note: The table reports placebo estimates of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. All regressions include individual controls (cf. Table 9). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

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ESSAY 2: ETHNIC DIVERSITY AND PREFERENCES FOR REDISTRIBUTION

1 Introduction

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

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

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

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

Furthermore, a novel feature of our study is that we match the size of the refugee inflow via the placement program to survey information on individuals living in the receiving municipalities. As part of the Swedish National Election Studies Program, the survey has been carried out every election year since the 1950's and is advantageous for

[1] An extensive theoretical framework for this idea is laid out by Shayo (2009), who, in addition to modeling distaste for cognitive distance to other agents, also endogenizes group identity. The equilibrium level of redistribution in his model decreases with the size of minority groups, and the reason is that the increased distance to other agents in the original group of identity makes identification with a less redistributive group more attractive. See also the model in Lindqvist and Östling (2011).

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

several reasons. It includes questions on the respondent's preferences for redistribution, and most importantly, it is in the form of a rotating panel, with each individual being surveyed twice and with half of the sample changing each wave. This panel structure enables us to control for individual fixed effects as well as for time trends in the preferences for redistribution during this period. This means that, to see how increasing immigrant shares causally affects preferences for redistribution, we link changes in an individual's preferences between two elections/survey waves to the placement program-induced change in immigrants in the individual's municipality over the corresponding period. If individuals exhibit positive in-group bias, we expect this effect to be negative.

The existing empirical literature is suggestive—but not conclusive—of positive in-group bias. Luttmer (2001) uses repeated cross-section survey data from the US (The General Social Survey) over a period from the mid-1970's to the mid-1990's and finds that increased welfare recipiency among blacks makes non-black respondents prefer less redistribution but has little effect on black respondents' preferences, and vice versa for increased welfare recipiency among non-blacks.[3] Senik et al. (2009) use information from the European Social Survey conducted in 22 countries in 2002 and 2003 to study the relationship between attitudes towards immigrants, attitudes towards the welfare state and respondents' perception of immigrant shares (measured as deviations from the national average). Their estimations suggest that negative attitudes towards immigrants are associated with less support for the welfare state but that this correlation is unrelated to the perceived share of immigrants in the population. A third related study is that by Eger (2010), who uses survey data collected by Swedish sociologists and regresses three repeated cross sections from the first half of the 2000's of survey-stated preferences for social welfare expenditures on immigrant shares in Swedish counties, concluding that ethnic heterogeneity has a negative effect. It should however be noted that, since there are only 20 Swedish counties, the aggregation to county-level data poses problems for inference.

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

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

[4] In studies that use an aggregate welfare measure as the dependent variable, such as total welfare spending per capita (see, for example, Hjerm (2009)), it is not possible to separate the direct effect that works through a change in preferences for redistribution from the policy-bundling effect that operates via political parties. The same goes for those studies that examine the effect of ethnic heterogeneity on (aggregate measures of) the size of the public sector; see for example Alesina et al. (1999) and Gerdes (2011).

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

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

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

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

This is not the first study to exploit the exogenous variation that the refugee placement program generated. Two examples, each with a different angle from ours, are Dahlberg and Edmark (2008) and Edin et al. (2003). The former uses the placement program to isolate exogenous variation in neighboring municipalities' welfare benefit levels to test whether there is a "race-to-the-bottom" among local governments, whereas the latter uses the initially exogenous placement of refugees to study the effect of segregation on the refugees' labor market outcomes. These two examples thus require two different identifying assumptions, namely that the placement was exogenous with respect to the receiving municipalities' politicians setting the welfare benefit levels (Dahlberg and Edmark) and that the placement was exogenous with respect to the refugees themselves (Edin et al.). For our case, however, we need the placement to be exogenous from the point of view of the receiving municipalities' population. We think that our context makes our case for identification, perhaps not more but at least as plausible.

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

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

2 Immigration and refugee placement

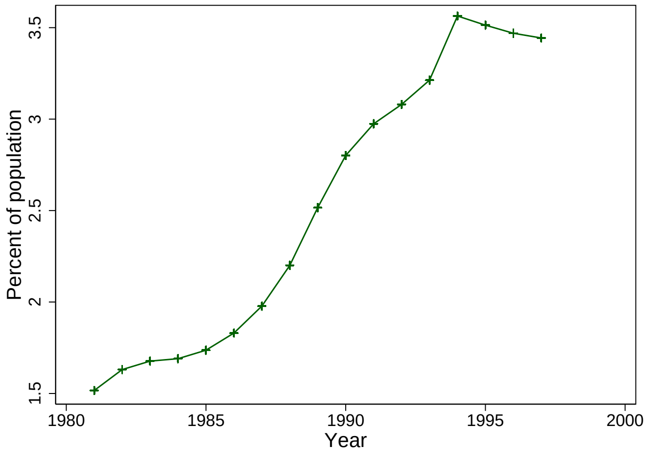
This section provides an overview of Sweden's experience with increased immigration during the last decades of the 20th century, a description of the refugee placement program that we use as an exogenous source of variation in the immigrant share in the municipalities, and a discussion about the exogeneity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

2.1 Immigration to Sweden

In the 1970's, the size of the population living in Sweden with a foreign citizenship was a rather stable 5%. The vast majority of these immigrants had arrived in Sweden in the 1950's and 1960's as labor migrants, primarily from the Nordic countries, with Finland as the prime example, but also from other European countries, such as Hungary. Over the next two decades, however, the situation completely changed, with more immigrants originating from other parts of the world and for political instead of economic reasons (refugees). Economic migration to Sweden more or less completely stopped during the 1970's. The evolution of immigration characterizing the 1980's and the 1990's is illustrated in Figure 1 (Figure 1 covers the years that will be used in the empirical analysis), from which it is clear that Sweden experienced a dramatic increase in the percentage in the population with citizenship from countries not member of the OECD (according to membership before 1994). Starting in 1981 from a mere 1.5%, it peaked at 3.5% in 1994—i.e., an increase of around 130%—before starting to trend back down.

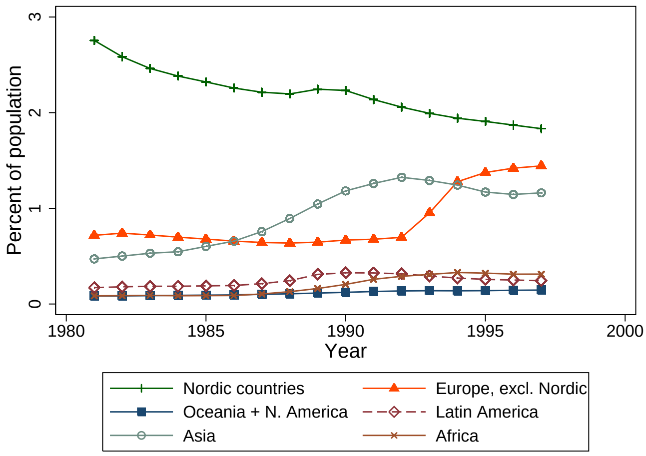
To get a better sense of from what parts of the world the immigrants came from, Figure 2 shows the evolution over time but by region of origin rather than by OECD membership status. Three distinct features emerge: (i) the share of Nordic citizenship has slowly declined over the period, which is most likely explained by Finns becoming Swedish citizens after having lived in Sweden for several years; (ii) a large inflow of Asians, mainly from Iran and Iraq, from the mid-1980's and onward; and (iii) a sharp increase in people from European countries other than the Nordic, explained by a significant influx of refugees from the Balkans in the early 1990's. In other words, the

Figure 1: Share of population with non-OECD citizenship



Source: Statistics Sweden.

Figure 2: Shares of population with foreign citizenship



Source: Statistics Sweden.

increasing share from non-OECD displayed in Figure 1 is primarily driven by inflows of refugees rather than by outflows of people from OECD countries. It is thus clear that Sweden has become a much more ethnically heterogeneous country, as people with a non-OECD citizenship are arguably more ethnically different from native Swedes than OECD citizens. For the purpose of this paper, a suitable definition of immigrants is therefore the share of population with a non-OECD citizenship,[6] and—from an econometric point of view—it is promising to see such a large influx of non-OECD immigrants as revealed by Figure 1.

2.2 The refugee placement program

One purpose of the refugee placement program, which was in place between the beginning of 1985 and July 1^{st} 1994, was to achieve a more even distribution of refugees over the country, or more specifically, to break the concentration of immigrants to larger towns. Under the program, refugees arriving to Sweden were consequently not allowed to decide themselves where to settle but were assigned to a municipality through municipality-wise contracts, coordinated by The Immigration Board (the refugees were, however, allowed to move after the initial placement). At the start of the program, only a fraction of the municipalities were contracted, but as the number of refugees soared in the late 1980's and early 1990's, so did the number of receiving municipalities. By 1991, as many as 277 out of the then 286 Swedish municipalities had agreed to participate.

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

As indicated in Figures 1 and 2, the number of refugees arriving to Sweden increased dramatically during our period in focus. Between 1986 and 1991, on average over 19,000 refugees arrived each year, compared to an annual average of just below 5,000 during the previous four years. Additionally, during the last three years in our data, 1992–94, the situation was even more exceptional, with an annual arrival of 35,000 (peaking in 1994 at 62,853), to a large extent driven by refugees from the Balkans.

This evolution is illustrated in Figure 3 along with an illustration of how the total inflow of refugees were distributed across small-sized (population<50,000), medium-sized (50,000≥population<200,000) and large-sized (population≥200,000) municipalities.[7] These time series are constructed using two slightly different data sources: for the years 1986–94, the variable measures the number of refugees placed via the placement program and thus captures the gross inflow of refugees, whereas for the years 1982–85,

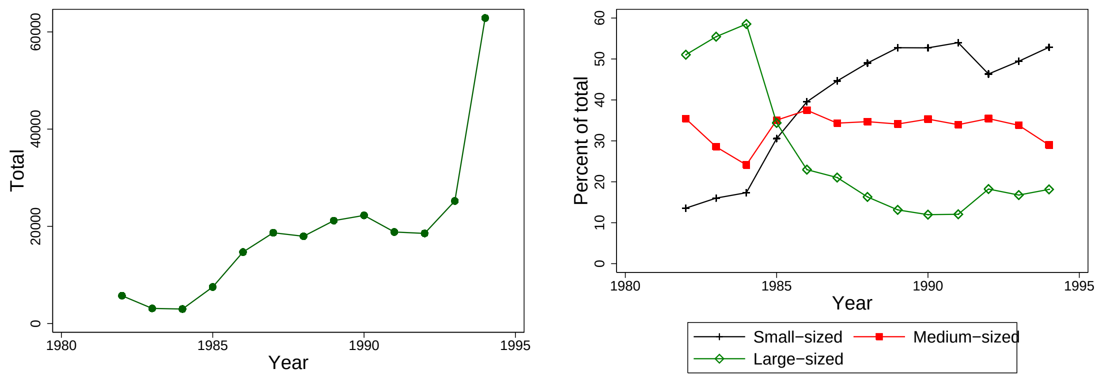
[6] Our precise definition of immigrants in the empirical analyses will be those with non-OECD citizenship according to OECD membership status before 1994 and those with Turkish citizenship. See, further, Section 3.

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

when the placement program had not yet started (apart from 1985), the variable instead captures the net increase in the sense that it measures the annual change in the number of residences with a citizenship from typical refugee countries[8].

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

Figure 3: Annual increase/inflow of refugees



(a) Total increase/inflow (b) Received share of total increase/inflow

Source: Statistics Sweden & The Swedish Integration Board.

2.3 Exogeneity of the placement program

The differential refugee treatment across municipalities and over time seen in Figure 3 is closely related to the variation in immigrant shares that we will use to identify causal effects of increased ethnic heterogeneity on changes in preferences for redistribution. The difference is that we exploit program-induced variation across *all* municipalities as opposed to variation only across the three groups according to population size.[10]

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

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

[10] These three groups are constructed in Figure 3 merely for illustrative purposes.

Therefore, our identifying assumption is that the placement of refugees was exogenous with respect to the inhabitants' of the municipalities preferences for redistribution. We claim that this assumption is indeed plausible. By construction, the placement program eliminates problems with the refugees themselves sorting into municipalities based on their characteristics (including the preferences of the inhabitants). We argue below that the placement can also be characterized as exogenous, conditional on a couple of observable municipal characteristics, with respect to the preferences of the municipalities' inhabitants.

The original idea of the placement program was to place refugees in municipalities with an advantageous labor market, education and housing situation and in municipalities that had previous experience with immigration. However, as the implementation of the program coincided not only with a dramatic increase in the number of refugees but also with a tightening of the housing market, housing availability seems to have become the more important factor.[11] Especially labor market but perhaps also the housing situation may matter for individual preferences for redistribution, in which case they will confound our analysis if not properly dealt with. Fortunately, with access to municipal-level data on both vacant housing and unemployment we are able to control for them in the regression analysis and thus use the conditional variation in refugee placement.

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

A number of circumstances suggest this requirement to be fulfilled. First, as discussed in the previous section, the number of refugees arriving in Sweden increased dramatically during the period of study. This made it harder for the municipalities to dismiss the refugee placement proposal from The Immigration Board; the refugees had to be placed somewhere, and it became necessary that all municipalities shared the responsibility.[12] Second, refusals of refugee placement were in fact very rare,[13] and those that at first did refuse got a lot of negative publicity. Third, the panel structure of our data allows us to control for individual fixed effects, implying that it is okay for the refugee placement to be correlated with preferences in levels. We only require that the placement is exogenous with respect to individual *changes* in preferences, which arguably is much more likely to hold.[14]

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

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

[13] Only 3 out of the 286 municipalities in our data did not receive any refugees at all via the program during 1986–94.

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

Bengtsson (2002) and our own interviews with program placement officials confirm that most municipalities accepted the idea that all should participate in a manner of solidarity and that most municipalities did so, especially during the early years of the program. This furthermore created a peer pressure, which made it harder to refuse placement.[15]

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

3 Data

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

3.1 Survey data

Survey data on individual preferences for redistribution is obtained from the Swedish National Election Studies Program[16]. The survey has been carried out every election year since the 1950's, and is in the form of a rotating panel, where each individual is surveyed twice and with half of the sample changing each wave. The survey contains information on political preferences and voting habits, as well as on several background characteristics of the respondent. This study uses information from waves 1982, 1985, 1988, 1991 and 1994, when roughly 3,700 individuals were surveyed each wave.[17] Based on the panel feature of the survey, with these waves we construct three survey panels for the baseline analysis; 85/88, 88/91 and 91/94. For the placebo analysis we also construct a survey panel for the years 82/85. Each survey panel thus includes individuals who were surveyed in both of the two respective election years.

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

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

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

Our measure of individuals' preferences for redistribution is extracted from a survey question on whether the respondents were "in favor of decreasing the level of social benefits". The respondents were asked to rate this proposal according to the following five-point scale:[18]

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

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

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

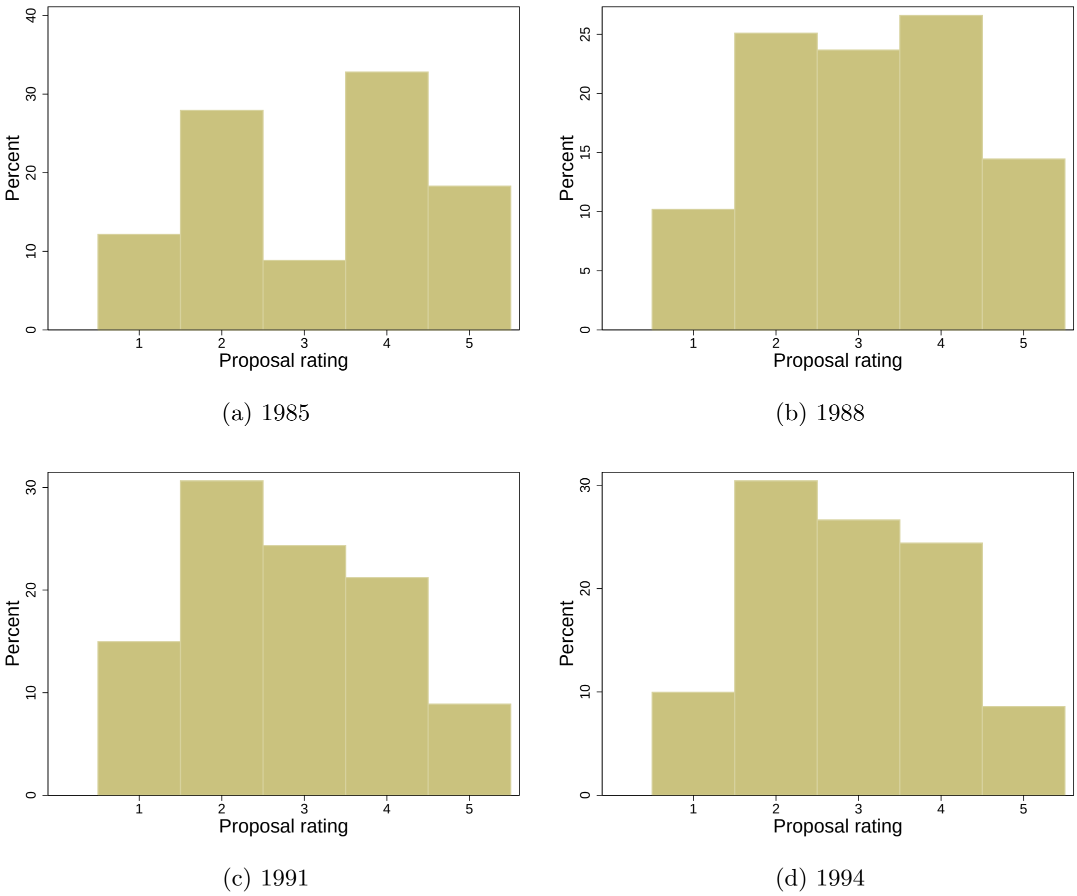
Figure 5 shows the distribution of this constructed variable measuring the change in preferences for redistribution in the form of social benefits. This will be the dependent variable in the empirical analysis. As can be seen in the figure, around 40% of the individuals in the sample do not change preferences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

3.2 Municipality data

To relate the changes in preferences between survey waves displayed in Figure 5 to the inflow of refugees during the corresponding period, we construct a variable for the cumulative number of refugees placed in each municipality during each election period (86–88, 89–91, 92–94), measured as a percentage of the average size of the population

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

Figure 4: Proposal ratings by survey

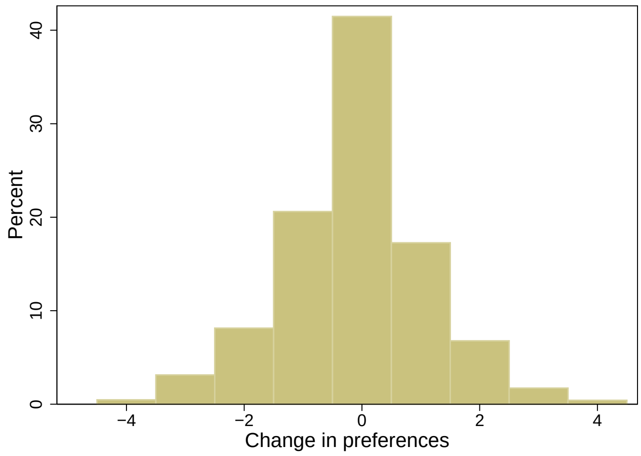


(a) 1985 (b) 1988

(c) 1991 (d) 1994

Source: The Swedish National Election Studies.

Figure 5: Change in preferences for social benefits between surveys



Source: The Swedish National Election Studies.

in the municipality during the respective election periods. Figure 6 shows how this variable is distributed over all municipalities and all three election periods. As is seen from the figure, the mass of the distribution is around or just below 1%; that is, during an election period of three years, most municipalities received refugees amounting to around 1% of the population. It is also relatively common with figures around 2%. The data contains one extreme value at 7.7%. This observation is excluded from the analysis (although it is not entirely unreasonable: the observation comes from a municipality with a small population, implying that relatively few refugees translate into a large percentage share).[19]

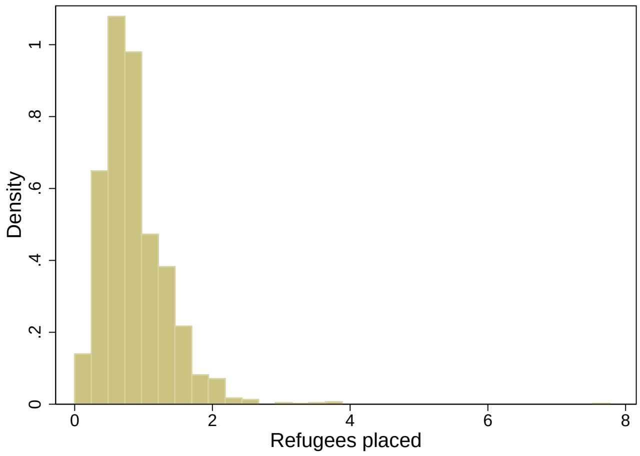
The refugee placement as displayed in Figure 6 will be used as an instrument to capture exogenous variation in the share of immigrants living in the municipality. As noted above, our working definition of immigrants is people with a non-OECD citizenship (according to membership status before 1994), or with a Turkish citizenship. With this definition, we hope to capture variation in ethnic background, as citizens from non-OECD countries are arguably more ethnically different from native Swedes than citizens from OECD countries are—except for maybe Turkey, which is probably the one OECD country whose citizens are ethnically least similar to native Swedes.[20,21] Note that with this definition, a person is an immigrant only until he or she obtains a Swedish citizenship, implying that negative changes in immigrant shares can stem either from individuals emigrating or from them obtaining Swedish citizenship.

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

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

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

Figure 6: Distribution of refugee placement between surveys



Source: The Swedish Integration Board.

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

4 Estimation method

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

Because this exogeneity requirement is generally not fulfilled, OLS estimation of the relationship between immigrant shares and preferences will most likely fail to identify the causal effect. Although one can think of circumstances causing the OLS estimate to be biased in either direction, a positive bias seems more probable. It is, for example, likely

Table 1: Descriptive statistics; levels and changes between surveys

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

Note: The number of observations is 1,917. All variables in shares are given in percentage points. Tax base and Welfare spending are given in 100 SEK per capita deflated to 1994 year values (6.50 SEK≈1 USD), and Population is given in 1000s. The variables Population<50,000, Population≥200,000, Socialist majority, Green Party and New Democrats are binary.

Source: Statistics Sweden & The Swedish Integration Board.

that immigrant families with a typical high probability of welfare dependence prefer to live in municipalities whose population is more positive towards redistribution. It is also likely that municipalities where preferences for redistribution are higher thanks to, for example, a more well-functioning welfare system in terms of assisting beneficiaries in becoming self-supported, attract more immigrants.

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

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

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

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

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

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

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

Equations (1) and (2) also include three sets of dummy variables. First, given the aims of the policy program and the pattern seen in Figure 3, we allow the effect of population size to be non-linear by also including an indicator for large-sized municipalities (population$\geq$ 200,000) and one for small-sized municipalities (population$<$50,000); these variables are contained in $SIZE_{ms}$. Second, we include a vector of political variables, POL_{ms}, to control for the possibility that the political views of certain parties might be correlated with both placement policy and preferences for redistribution. POL_{ms} therefore contains a dummy for a socialist majority in the municipal council (defined as the Social Democrats and Left Party together having at least 50% of seats), and two separate dummies for council representation by the Green Party and by the populist right-wing party "the New Democrats". Third, $SURVEY_s$ denotes survey panel fixed effects that capture nation-wide trends in changes in preferences between panels 85/88, 88/91 and 91/94.

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

5 *Results*

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

5.1 Baseline results

Before turning to the IV estimations, where we make use of the refugee placement program as an instrument for the share of immigrants living in the municipalities, we estimate equation (2) but with the actual share of immigrants instead of the predicted share, with OLS. The results, given in column 1 of Table 2, show no evidence of an effect of the share of immigrants on individuals' preferred levels of social benefits. As discussed above, however, the OLS estimator is likely to be biased. First, although the estimation equation controls for a set of municipal characteristics, time trends and, through first-differencing, individual fixed effects, it is still possible that unobservables correlated both with immigrants' choice of location and preferences for redistribution confound the estimates. Second, the estimated relation may reflect reverse causality—that is, we cannot rule out that immigrants' choice of residency is affected by the inhabitants' preferences for social benefits.

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

Table 2: Baseline results

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

Note: The dependent variable is ΔPREF in columns 1 and 3, and ΔIM in column 2. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

on a set of municipal characteristics as well as survey fixed effects. With this reassuring first stage, we now turn our focus to the relation of interest in equation (2).

The results from estimating equation (2) using the program placement of refugees as an instrument for the immigrant share in the municipality are given in column 3 of Table 2. In contrast to the insignificant coefficients that were obtained in column 1 with OLS, this column reveals a negative and statistically significant coefficient for the effect of changing the share of immigrants in the municipality on preferred levels of social benefits. An increase in the share of immigrants is hence estimated to reduce the support for redistribution, as measured by the preferences for social benefits. The size of the effect implicates that a one percentage point increase in the immigrant share in the municipality makes the average individual move up roughly 1/3 of a point in the preference ordering for social benefits (which was given on page 77). Considering that preferences are measured along a five-point scale, this is a considerably large effect.

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

5.2 Placebo analyses

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

Regarding placebo in treatment, we will run a placebo regression to test for a correlation between refugee placement and pre-placement trends in preferences for redistribution. If our assumption that the refugee placement was exogenous with respect to changes in preferences for redistribution holds, then we expect no correlation between the pre-placement preference trends and the subsequent refugee placement. Therefore, we run a regression of pre-placement preference trends, measured as the change in preferences for redistribution between 1982 and 1985 (i.e., the panel period preceding the placement program), on changes in immigration as predicted by the refugee placement in the three subsequent panel periods. The regression includes the same set of covariates, measured for the period 1982–85, as the baseline regressions in equations (1) and (2).[22,23]

Columns 1–3 in Table 3 show the first-stage estimates of the instruments (refugee placement during the three panel periods) for each of the three endogenous variables (the change in immigrant shares over the three respective periods), and column 4 shows the second-stage placebo regression result. Note first from columns 1–3 that refugee

[22] For the sake of brevity, the covariates are suppressed in the tables for the remaining analyses.
[23] Housing vacancies are not available until 1985. We therefore use the 1985 value as a proxy for average vacant housing in years 1983–85.

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

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

Note: The reported F-statistics correspond to a joint test of the three excluded instruments in the first-stage regressions (columns 1–3), and the reported χ^2-statistic corresponds to a joint test of the three placebo treatments in the second-stage regression (column 4). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

placement is strongly correlated with the change in immigrant shares during each corresponding panel period. However, importantly, as can be seen from column 4 in the table, the instrumented changes in immigrants are non-significantly related to the pre-placement preference trends in all periods, and a test of joint significance for all three periods yields a p-value of 0.76. This strengthens our assumption that the refugee placement was exogenous conditional on the included covariates.[24]

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

Table 4: IV regressions of ΔPREF for placebo outcomes

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

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

The resulting placebo estimates of β_1, which are obtained from the same set of respondents as in the original sample with three panel periods, are found in Table 4.[25] As expected, no effects of increased immigrant shares are found neither on attitudes towards privatizing health care, nor towards nuclear power. This strengthens the notion that the estimated effects on preferences for redistribution indeed have a causal economic interpretation.

5.3 Do responses vary with individual characteristics?

According to the coefficients in Table 2, the causal effect of a one percentage-point increase in immigrant shares is that the support for redistribution is reduced by 1/3 in the

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

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

five-point preference ordering. This result pertains to the "average respondent", but it is of course possible that the effect varies depending on individual characteristics. For example, it could be that respondents who are large contributors to the redistribution scheme are more sensitive to the ethnic diversity of the recipients, compared to respondents who are themselves more likely to be net receivers in the redistribution scheme.

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

The question on individual wealth is posed identically, so we proceed in the same way with this information. That is, we construct three additional dummy variables indicating whether the individual belongs (i) to the lowest of five wealth classes (containing the 40% of respondents who have no wealth, $w <$ p40); (ii) to the two lowest of five wealth classes ($w <$ p60); and (iii) to the top wealth class ($w >$ p85).

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

To see how the effect of increased immigrant shares on preferences for redistribution differs across these individual characteristics, we then run the model in (1) and (2) three times in the income dimension, three times in the wealth dimension and twice for worker type, each time interacting the variables ΔIM and $Refugee\ inflow_{ms}$ with one of the class/worker type dummies. The resulting second-stage IV estimates are displayed in Table 5 for income (left column) and wealth (right column), and in Table 6 for worker type.[26]

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

[26] Note that the interaction terms of ΔIM and the class/worker type indicators are also endogenous. We therefore use as additional instruments the interaction of $Refugee\ inflow$ and the respective indicators. As can be seen from Tables A1–A3 in the Appendix, all instruments are strong and the joint F-tests are within conventional significance levels.

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

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

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

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

	ΔPREF
ΔIM	-0.721***
	(0.253)
ΔIM$\times$ blue-collar	0.660**
	(0.337)
ΔIM	-0.0934
	(0.202)
ΔIM$\times$ white-collar	-0.804**
	(0.374)
Observations	1899
Municipal covariates	yes
Panel effects	yes

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

We finally estimate how effects vary with worker type, and the results from this are found in Table 6. These estimates are in line with those found above; we see the negative effect on preferred levels of social benefits for white-collar workers (who presumably are also the high-income earners). Overall, this set of results clearly reveals that the respondents who contribute more extensively to the redistribution scheme are those whose support for redistribution is reduced as the group of likely recipients become more ethnically diverse.[27]

5.4 *Sensitivity analysis*

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

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

Dahlberg and Edmark (2008) investigate the extent of refugee migration and come to the conclusion that around 40% indeed lived in a different municipality than where they were initially placed four years later, and of these the vast majority had moved to one of the three large cities (Stockholm, Göteborg and Malmö) and their surrounding areas. As a robustness check of the baseline results presented in Table 2, we therefore estimate the model while excluding the 250 respondents living in these three municipalities. If anything, we would expect effects to be smaller among the respondents from the remaining municipalities where the true increase in immigrants perhaps was smaller than what is being measured. However, the results presented in columns 1 and 2 of Table 7 show no evidence of a reduction in estimates—both the first- and second-stage estimates are reassuringly the same as when estimating on the full sample.

Table 7: Sensitivity analysis

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

Note: The dependent variable is ΔIM in columns 1 and 3, and ΔPREF in columns 2 and 4. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Another aspect with the placement program that differs from most randomized experiments is that it lasted for as long as ten years. It is therefore likely that it functioned somewhat differently in the beginning than in the end. Specifically, Bengtsson (2002) reports that more municipalities willingly participated during the initial years. This suggests that the variation in immigrant shares induced by the refugee placement program is more likely to be exogenous in the earlier time periods than towards the end, when participating municipalities comprise a more selected sample. To investigate this we therefore exclude the last survey panel (covering years 1991–94) from the estimation sample. Recalling the above discussion of likely directions of the bias of the OLS estimator, if the placement program was "more exogenous" early on, we thus expect the estimate on this limited sample to differ even more from the OLS estimate than the baseline IV estimate in Table 2. In other words, if anything, we expect a more pronounced negative effect in the early period.

Columns 3 and 4 of Table 7 present these estimates and indeed confirm our priors. In particular, the second-stage estimate increases in an absolute sense and is now essentially as large as -1. That is, increases in immigrant shares of one percentage point during the periods 1985–88 and 1988–91 caused the support for redistribution in the form of preferred levels of social benefits to decrease with an amount corresponding to a full step along the five-point rating scale. If this estimate can be interpreted causally with higher confidence, it thus means that increased ethnic heterogeneity has a very large, negative effect on preferences for redistribution. It also means that the above estimated effects on the full sample should be viewed as lower bounds. There is, however, no reason to doubt the overall pattern of effects across different individual characteristics from Section 5.3. Unfortunately, the 30% drop in the number of observations resulting from excluding the later survey panel leaves a too small sample to study interaction effects with any reasonable precision.

6 Conclusions

In this paper, we have examined whether an increased ethnic heterogeneity in society affects natives' preferences for redistribution. We use data from Sweden, a country that has experienced a dramatic increase since the 1970's in the share of the population originating from a non-OECD country. By combining two data sources covering the period 1985-94, we improve upon the earlier literature on in-group bias and argue that we are able to estimate causal effects. The first data source includes information on a nation-wide policy intervention program that exogenously placed refugees coming to Sweden between 1985 and 1994 among the Swedish municipalities. We use this policy intervention as an instrument for the municipalities' share of immigrants (defined as the share of non-OECD citizens). The second data source is individual panel survey data, which is matched on the respondent's municipality of residence to the first data and in which each respondent in two consecutive elections is asked questions about, among many other things, his or her preferences for redistribution (specifically, his or her preferred level of social benefits). By exploiting the exogenous source of variation in immigrants shares in the municipalities induced by the refugee placement program between two consecutive elections, we are able to causally estimate the effect of increased ethnic heterogeneity on the individuals' change in preferences for redistribution between the two elections.

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

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

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

Appendix

First-stage estimates

Table A1: First-stage estimates; $y <$ p15, $y <$ p40, $y >$ p80

	ΔIM	ΔIM$\times 1(y <$ (>) p#)
Refugee inflow	0.490***	0.0162***
	(0.0618)	(0.00587)
Refugee inflow$\times$ ($y <$ p15)	0.0962	0.432***
	(0.101)	(0.123)
F-statistic	34.53	9.601
Refugee inflow	0.497***	0.0385***
	(0.0663)	(0.0142)
Refugee inflow$\times$ ($y <$ p40)	-0.000711	0.378***
	(0.0672)	(0.0704)
F-statistic	32.61	17.26
Refugee inflow	0.513***	0.0410***
	(0.0588)	(0.0156)
Refugee inflow$\times$ ($y >$ p80)	-0.0533	0.339***
	(0.0623)	(0.0868)
F-statistic	38.50	10.11
Observations	1917	1917
Municipal covariates	yes	yes
Panel effects	yes	yes

Note: The reported F-statistics correspond to a joint test of the two excluded instruments. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table A2: First-stage estimates; $w <$ p40, $w <$ p60, $w >$ p85

	ΔIM	ΔIM$\times 1(y <$ (>) p#)
Refugee inflow	0.474***	0.0429**
	(0.0683)	(0.0173)
Refugee inflow$\times$ ($w <$ p40)	0.0594	0.401***
	(0.0628)	(0.0729)
F-statistic	34.84	22.48
Refugee inflow	0.476***	0.0555**
	(0.0675)	(0.0236)
Refugee inflow$\times$ ($w <$ p60)	0.0352	0.398***
	(0.0456)	(0.0634)
F-statistic	32.74	28.46
Refugee inflow	0.498***	0.0145**
	(0.0642)	(0.00646)
Refugee inflow$\times$ ($w >$ p85)	-0.0148	0.384***
	(0.0773)	(0.0861)
F-statistic	35.19	11.36
Observations	1917	1917
Municipal covariates	yes	yes
Panel effects	yes	yes

Note: The reported F-statistics correspond to a joint test of the two excluded instruments. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table A3: First-stage estimates; blue-collar and white-collar workers

	ΔIM	ΔIM$\times$ x-collar
Refugee inflow	0.465***	0.0810***
	(0.0668)	(0.0207)
Refugee inflow$\times$ blue-collar	0.0354	0.384***
	(0.0528)	(0.0600)
F-statistic	33.74	35.43
Refugee inflow	0.492***	0.0253
	(0.0589)	(0.0189)
Refugee inflow$\times$ white-collar	-0.0262	0.342***
	(0.0579)	(0.0753)
F-statistic	35.27	11.18
Observations	1899	1899
Municipal covariates	yes	yes
Panel effects	yes	yes

Note: The reported F-statistics correspond to a joint test of the two excluded instruments. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

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ESSAY 3: GRANTING PUBLIC OR PRIVATE CONSUMPTION? EFFECTS OF GRANTS ON LOCAL PUBLIC SPENDING AND INCOME TAXES

1 Introduction

Most fiscally decentralized economies rely heavily on transfers from upper to lower-level governments as well as on equalizing transfers between lower-level governments. Knowledge about how and to what extent these intergovernmental grants are spent is therefore crucial for designing public policies that relate to the federal structure. In the end, whether or not grants have the intended effect will serve as strong arguments regarding the optimal level of decentralization.

One cannot expect to answer such broad economic questions in one single paper. As has been long understood and was explicitly articulated by Besley and Case (2000), economic policies can generally not be seen as exogenous events. Because this problem is likely to be more pronounced with broader policies, the path to knowledge about deep economic issues instead often goes through a careful evaluation of many different policies that are more narrowly targeted. However, while the literature on the effects of intergovernmental grants has a long history,[1] so far the studies that are truly convincing thanks to such an approach are too few for the puzzle on grants effects to be complete. The aim of this paper is to add a piece thereto. Utilizing policy-induced increases in intergovernmental grants to a group of municipalities in Finland, I identify and estimate causal effects of grants on local expenditures and income tax rates in a difference-in-difference (DID) model. Because the grant increase lasted for several years, I can also analyze the dynamics in the response to investigate whether it occurs with a lag or, alternatively, if immediate responses are reversed in later years.

The policy under consideration increased a grant supplement to a group of remotely populated municipalities in 2002 whereas the remaining municipalities serving as controls never received the grant supplement. While the setup identifies the effect of increases in this particular grant, the results can easily be extended to other types of grants. The reason is that over the period studied (1998–2004), all grants were distributed to the municipalities as a general sum with no strings attached, meaning that increases in the particular supplement are exactly equivalent to increases in any other broader grant category.

There is a lack of studies that credibly estimate causal effects of intergovernmental grants on the receiving jurisdictions, and even among the few that do so, the focus is mostly on more or less narrowly targeted grants. To my knowledge, aside from a paper by Dahlberg et al. (2008), this is the only study to focus on the effects of *general* grants on overall expenditures and tax rates, which, in turn, are two highly broad and general—and therefore relevant—economic outcomes.

Dahlberg et al. (2008) utilize a non-linearity in the distribution of grants to Swedish municipalities with a diminishing population to estimate causal effects in a regression kink design (Nielsen et al., 2010; Card et al., 2009). I, instead, estimate effects of grants in a DID model. The fact that treatment in DID models occurs at a distinct point in time makes it possible to investigate the dynamics in the responses to grant

[1] Surveys of the field include, e.g., Gramlich (1977); Bailey and Connolly (1998); and Hines Jr and Thaler (1995).

increases. Especially since earlier work has highlighted such dynamics (see Gordon (2004)), this is an advantage compared to the regression kink framework where a treated municipality is likely to receive similar grant increases at several consecutive points in time, considerably complicating a dynamic analysis. These differences aside, in two seemingly similar contexts with general grants and broad economic outcomes like total spending and taxes, it is also interesting to see whether data from two different countries tell similar stories.

Although the empirics in many of the earlier studies can be questioned, the literature on the effects of grants can be summarized as being somewhat puzzling. This is related to the fact that it is not obvious even what the starting point should be when studying the behavior of local governments. Is each jurisdiction to be viewed as a single entity just as any other decision-maker, or is a more complex framework required? A parsimonious theoretical model predicts that increased lump-sum grants will, equivalently to a tax base increase, induce a pure income effect and should therefore affect expenditures according to the overall marginal propensity to spend on public goods and services, i.e. with around 15–20% for most countries (grants targeted to specific sectors or projects where the propensity to spend is considerably lower are naturally predicted to have an even smaller effect). The analysis in Bradford and Oates (1971), who were among the first to incorporate political aspects of grants, by and large sticks to this prediction. Since this implies that the majority of a grant increase is either spent in other than the intended area or substituted for other sources of revenue, grants according to these models are said to have a crowding-out effect on spending. However, most early empirical estimates suggested otherwise, namely a larger stimulatory effect on expenditures than what would be predicted by theory. It seemed that the money stuck where it first hit, which is why this apparent crowding-in effect was dubbed the "flypaper effect". A large literature has offered various explanations for this empirical anomaly; either as, e.g., Becker (1996) by hypothesizing that the estimated flypaper effects are simply statistical artifacts that disappear with a correctly specified model and proper instruments; or by acknowledging the anomaly as real and focusing on possible mechanisms behind the phenomenon. For example, Filimon et al. (1982) further stress the political aspects of grant distributions and explain the flypaper effect with poorly informed voters that enable budget-maximizing policy makers to pursue their own objective. Hamilton (1986) offers a different explanation that is instead good news for the voters: since income tax revenues involve deadweight losses that intergovernmental grants do not, more extensive use of the latter to finance expenditures is optimal.[2]

Since one possible explanation for the apparent flypaper effect is simply that it is not real but a mere statistical artifact, it is imperative that the identification problem is properly solved. This means that researchers are required to isolate exogenous variation in grants, and although grants do often vary considerably, most of the variation is endogenous in the sense that it is due to structures that are themselves directly related to

[2] Revenue raising at the federal level may also involve deadweight losses, but these are assumed to either not be internalized by lower-level governments or to be substantially smaller (which is indeed the rationale behind federal systems with intergovernmental grants).

the outcome of interest. The problem is particularly evident for the case of expenditures: jurisdictions with characteristics associated with high expenditures, such as a large share of elderly, typically receive more grants exactly because they need to be spending more. Therefore, it is highly likely that perceived relations between grants and expenditures simply reflect such needs. A tempting remedy for this inherent endogeneity problem is to control for all characteristics that determine expenditures in a regression analysis. However, depending on the design of the grant system, such an approach would typically kill all variation in grants. A more promising remedy is therefore to closely study how grants are determined and search for experimental-type features where the amount of grants varies but the underlying needs do not—that is, the strategy aimed for here.

It might be argued that this study of the effects of un-earmarked, general grants on total expenditures and tax rates fits well into the flypaper literature since it is closely linked to parsimonious theoretical models. In contrast, as noted above, most existing studies that convincingly deal with the likely endogeneity problem in grants concern grants that are targeted towards specific sectors or projects. For example, Knight (2002) incorporates the legislative bargaining process behind the distribution of federal grants to state highway constructions and estimates the effects on state spending. He shows that when accounting for differences in bargaining power that are correlated with the demand for road construction across states, the effects are small, thus suggesting that grants crowd out state spending. Knight's paper is an excellent example of how institutional knowledge about narrowly targeted grants enables identification. Another such example is the study by Gordon (2004) (although her focus is on school spending which one may consider to be less narrow than highway spending). She recognizes that the basis for Title I grants[3] is updated only every tenth year, whereas the factors determining the demand for school spending change continuously—a structure suitable for a regression discontinuity design. She estimates the effects of federal grants on state and local education revenue and how it affects school spending, and finds that the immediate effects are large but that they disappear after three years, suggesting dynamic crowding-out effects.

The robust finding of this paper—in line with the results in Dahlberg et al. (2008)—is that increased grants have a negligible effect on local income tax rates, but that there is a substantial positive immediate response in local expenditures. Specifically, a 1 euro increase in grants causes expenditures to increase by around 70–80 cents. Or, evaluated at the grant increases to one of the groups of treated municipalities, expenditures increased by around 60 euro per capita as a result of the reform, whereas the implied cut in own-source revenues was only 6 euro per capita. Furthermore, there is no evidence that the immediate response in expenditures was reversed in later years.

These large stimulatory effects on expenditure can be interpreted as crowding-in effects. Despite contradicting the results found by, e.g., Knight (2002) for targeted grants, it is likely that the common effects of general grants to Finnish municipalities as found here and to Swedish municipalities as found in Dahlberg et al. are externally

[3] Title I is a US federal program that allocates extra funds to elementary and secondary education based on child poverty.

valid at least to other federations characterized by comprehensive local independence. Indeed, the scope for targeted grants to crowd out spending on specific projects seems much larger than that for general grants to crowd out total expenditures.

As far as I am aware, this is the first paper to estimate effects of intergovernmental grants on Finnish data while taking explicit account of potential endogeneity problems, but a few descriptive-type papers also study Finland: Moisio (2002) studies determinants of expenditures in Finnish municipalities and finds larger effects of grants than of taxable income—i.e., results supporting the flypaper effect. Oulasvirta (1997) also finds evidence of the flypaper effect when looking at a grant reform in 1993 that changed the majority of grants from matching to general type grants. His results suggest that both types of grants stimulated spending more than taxable income, and even more so during the early period with matching grants.[4]

The remainder of the paper goes as follows: The next section describes the particular grant supplement subject to the policy reform in 2002 and how the reform makes it possible to circumvent the grant endogeneity problem. Section 3 describes the data and its variables. Section 4 presents the baseline results accompanied by a thorough robustness check to investigate the validity of the identifying assumption of parallel trends by (i) controlling for, among other things, other simultaneous policy implementations; and (ii) by exploiting the discontinuous structure of the supplemental grant. Section 5 proposes a two-stage procedure as an alternative to the baseline DID that can help to understand the dynamic effects. Section 6 concludes the paper with a discussion of the results.

2 *Identifying causal effects of grants: A difference-in-difference approach*

This section describes the structure of the supplemental grant given to remotely populated municipalities and the policy in 2002 that enables identification of causal effects of intergovernmental grants in a DID approach. The supplemental grant is given to municipalities where few inhabitants live close to the city center but rather have their population remotely located. In order to decide which municipalities that qualify for the grant supplement, every fifth year starting in 1997, Statistics Finland has assigned a remote index to each municipality according to the formula:[5]

$$\text{remote index}_i = \frac{15,000 - pop_i^{25km}}{15,000} + \frac{60,000 - pop_i^{50km}}{60,000}, \tag{1}$$

where pop^{25km} and pop^{50km} are the population within a 25 and 50 kilometer radius from the municipal center, respectively. As is apparent from (1), the remote index can range from negative values to +2, where +2 corresponds to a situation where the entire population lives outside the 50 kilometer radius. In 1997–2005, the supplemental grant

[4] Since matching grants induce both an income and a positive price effect, theoretically matching grants should stimulate expenditures more than general grants. In practice, however, matching occurs in most cases only up to a certain amount of expenditures above which receiving jurisdictions are often spending. This implies that also matching grants effectively induce a pure income effect.

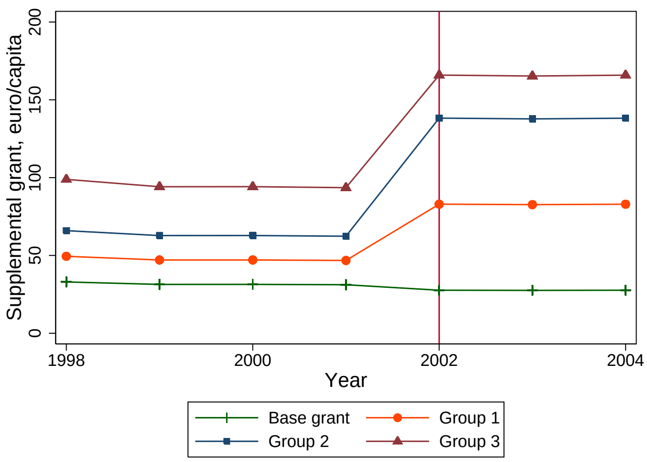
[5] The remote index assignment in the period studied here took place in 2002.

was distributed based on this index as described in Table 1 and illustrated in Figure 1.[6] Ever since the supplement was introduced in 1997, the structure of the grant in terms of which municipalities get the largest supplement has been the same; municipalities with a remote index smaller than 0.50 never received any grant supplement, while municipalities with a remote index in the range 0.50–1 (group 1 in the figure), 1–1.50 (group 2), or 1.50–2 (group 3) received a grant supplement equal to a fixed multiplier of a base grant, the multiplier being larger the larger the remote index. The base grant is a euro per capita amount that is given to all municipalities and is decided annually by the central government. As seen in Figure 1, during 1998–2004 the size of the base grant varied around 30 euro per capita.[7]

Table 1: Distribution of the supplemental grant

	Remote index	Supplemental grant 1997–2001	Supplemental grant 2002–05
Control group	<0.50	0	0
Group 1	0.50–0.99	1.5×base grant	3×base grant
Group 2	1.00–1.49	2×base grant	5×base grant
Group 3	1.50–2	3×base grant	6×base grant

Figure 1: The supplemental grant



Source: Government Institute for Economic Research.

The sharp increase in the supplemental grant in 2002 seen in Figure 1 is due to a

[6] In 2006, a new grant system came into place where this as well as many other grant types were changed considerably, but due to lack of data, the figure only illustrates how the supplemental grant was distributed during 1998–2004.

[7] For the years prior to 2002 (when the euro was introduced), the exchange rate 1 euro = 5.94573 Finnish marks is used.

policy reform.[8] Relative to the base grant, the reform doubled the supplemental grant for groups 1 and 3, and more than doubled the grant for group 2. To finance these supplemental increases, the base grant decreased from around 31 to 28 euro, meaning that effectively the supplemental grant increased somewhat less, but still enough so that the net positive change was substantial.

The supplemental grant increase was part of a group of policy reforms implemented in 2002 motivated by the fact that economic conditions varied across municipalities despite rather stable finances for the country in general. Of these policies, the two most significant ones were the abolishment of a system with repayments of value added taxes from the municipalities to the state, and a decrease in the municipalities' share of revenue from corporate taxation from 37.25 to 24.09%. The result section returns to these and related reforms (described in more detail in the Appendix), but note for now that the general aim was to stabilize the local government sector and increase fiscal independence for those municipalities that were struggling the most. For example, the idea was to avoid continuous dependence of a discretionary aid from the state that could (and still can) be granted municipalities with extraordinary financial difficulties through a special application procedure. The intention was, however, that the fiscal relation between the state and the municipalities was, on the whole, not to be altered due to these changes.

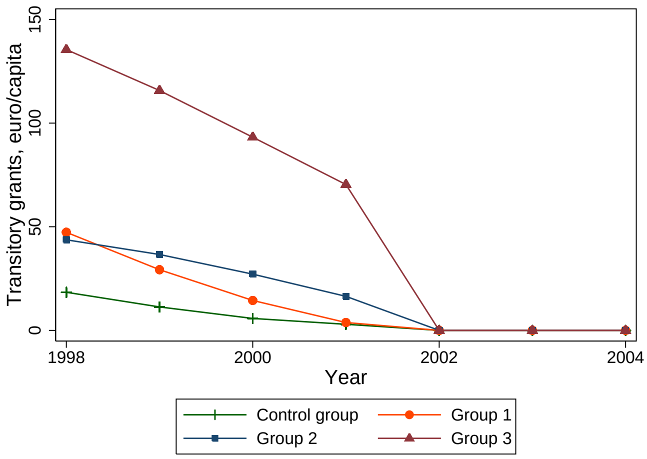
As part of an overall reform of the grant system, the launch of the supplemental grant in 1997 coincided with other changes in the grant distribution. Municipalities that were highly affected by this grant reform were compensated with transitory grants that were gradually phased out through 2001 and were entirely removed in 2002. Among other things, the previous grant system had put more weight on large areas than did the 1997 system and thus, large municipalities received larger amounts of transitory grants. Because having a remotely located population is correlated with a large area, the coinciding removal of the transitory grants reduces any potential effects of the supplemental grant increase in 2002 for the most remotely populated municipalities. As can be seen in Figure 2, plotting transitory grants separately for the same three groups as in Figure 1 along with a control group consisting of municipalities with remote indices below 0.50, the problem is especially apparent for group 3. In fact, for the 13 municipalities in this group, the average decrease in transitory grants just about equals their supplemental grant increase. For groups 1 and 2, however, the size of the transitory grant decrease is more modest. Motivated by this, the empirical analysis will focus on municipalities in these two groups.[9]

The particular policy-induced increases displayed for group 1 and 2 in Figure 1 will be used in a DID model to identify causal effects of grants on municipal expenditures and on local income tax rates. The treatment group is comprised of municipalities i with remote indices in the range 0.5–1.5 and treatment is defined as changes (increases)

[8] The reform is proposed by the government in Bill 128/2001 and legislated in Law 1360/2001.

[9] Combining Figures 1 and 2 suggests that, because of the counteracting effect from decreased transitory grants, for group 3 the supplemental grant increase was not associated with an overall grant increase, and could thus not have caused any behavioral response. An analysis of the municipalities in this group—from which results are available upon request—indeed shows this to be case.

Figure 2: Transitory grants



Source: The Association of Finnish Local and Regional Authorities.

in supplemental grants, ΔSG_i, that occurred in 2002. The control group accordingly consists of municipalities with a remote index smaller than 0.50 that never received this particular grant. A straightforward DID model that identifies the effect of ΔSG_i on changes between year t and $t-1$ in either of the outcome variables per capita expenditures or tax rates, ΔY_{it}, is then

$$\Delta Y_{it} = \bar{\tau}\Delta SG_i + T_t + \varepsilon_{it}, \tag{2}$$

with T_t denoting year fixed-effects and ε_{it} being the error term. The parameter $\bar{\tau}$ captures how much a euro per capita increase in SG caused the average value of Y to change pre and post treatment.

However, it is also of interest to see in which year(s) the effect took place.[10] The supplemental grant increase in 2002 was not a temporary increase. That means that municipalities that, say, used the extra grants to increase spending did not have to cut back in the following years. On the contrary, one possibility is that adjustment to a larger budget is not immediate but that it takes time to decide where to spend, suggesting positive effects should also be expected in subsequent years. Or, alternatively as in Gordon (2004), jurisdictions may over time substitute increased grants with own-source revenues, which would imply negative effects in later years. In order to investigate these dynamics, the following model allows the supplemental grant increase to have differential effects in different years:

$$\Delta Y_{it} = \tau_{2001}\Delta SG_i + \tau_{2002}\Delta SG_i + \tau_{2003}\Delta SG_i + \tau_{2004}\Delta SG_i + T_t + \varepsilon_{it} \tag{3}$$

[10] Note that Finnish municipalities do not have a balanced budget requirement and are allowed to take up loans.

For $t \in [2001, 2004]$, each of the parameters τ_t represents the effect of ΔSG_i between year t and year $t-1$. Because the supplemental grant increase ΔSG_i took place in year 2002, τ_{2002} thus represents the immediate effect on ΔY_{it}, whereas τ_{2003} and τ_{2004} represent the additional effects one and two years later. Finally, τ_{2001} captures the "effect" of ΔSG_i one year before the treatment actually took place, whose estimate is a test of the identifying assumption (its expectation is zero if the assumption holds).

For the treatment effects in equation (3) to be identified, it is required that, conditioning on the differences prior to the grant increase in 2002, the outcome of the control group represents the potential outcome of the treatment group had there been no treatment.[11] In other words, there can be no other factor except the supplemental grant increase that causes the pre-treatment difference between the control group and the treatment group to change at the time of treatment (or within two years after treatment for the dynamic effects τ_{2003} and τ_{2004}). This is the maintained identifying assumption of parallel trends.

Since the treatment was targeted towards remotely populated municipalities and hence was not random, it is a priori not obvious that the assumption of parallel trends should hold. As mentioned above, an insignificant estimate of τ_{2001} capturing differences in pre-treatment trends strengthens the assumption that also the counterfactual post-treatment trends would be the same.[12] But to further investigate the validity of the identifying assumption, the empirical section also conducts numerous robustness checks of the baseline results. For example, included in the parallel trend assumption is the requirement that all other policies implemented in 2002 (like those mentioned above and described in the Appendix) on average affected the treated and control municipalities equally. To test this, the changes and pre-treatment levels of other types of grants (including the transitional grant that was removed in 2002) as well as of corporate tax revenues are included as controls in a sensitivity analysis. Furthermore, as seen in Table 1, the pre- and post-treatment level as well as the policy increase in the supplemental grant are discontinuous functions of the remote index with discrete jumps at 0.50 and 1. These discontinuities are exploited in a second sensitivity analysis.

While equation (3) identifies the effect of increased supplemental grants, the interpretation of the estimated effects can easily be extended to other types of grants. The reason is that, as described in the following section, over the period studied all grants were distributed to the municipalities with no strings attached. This means that increases in the particular supplement are exactly equivalent to increases in any other broader grant category. Section 5 returns to this by estimating a two-stage least squares model as an alternative to the baseline DID model.

[11] It may be worth noting that the specification in (3) identifies the average treatment effects (ATE) *on the treated* if responses to treatment are heterogeneous. That is, even though the outcome of the control group serves as the potential outcome of the treatment group had it not been treated, the opposite cannot be assumed to hold unless treatment effects are constant. This is always the case in standard DID models. In contrast, Athey and Imbens (2006) develop an approach that also identifies the ATE on the untreated (and consequently the overall ATE) even in the presence of heterogeneous effects.

[12] Although non-parallel pre-treatment trends do, in principle, not completely rule out parallel counterfactual post-treatment trends (and vice versa).

3 Descriptive data

To explain the surrounding context with the Finnish grant system and other relevant institutional details, this section provides summary statistics of the data and a description of its variables.

The original data consists of a seven-year panel between 1998 and 2004 of all Finnish municipalities. From this data, the main sample restrictions are that 52 municipalities that were consolidated with another around this period are dropped,[13] as are 16 municipalities belonging to the autonomous island of Åland, and 11 municipalities with discrepancies concerning entitlement to the supplemental grant. For reasons discussed above, the 13 municipalities belonging to group 3 (cf. Table 1) are also dropped. This leaves a balanced panel of 367 municipalities amounting to 2569 observations for the full sample period 1998–2004, or to 2202 observations after taking first-differences.

Summary statistics of the variables used in the empirical analysis are presented in Table 2 for different subsamples—for the treatment and control group, separately pre and post treatment. With 330 municipalities in the pre-treatment period, the control group constitutes the majority of observations. Among the treatment group, around 2/3 are classified into group 1 (i.e., have a remote index of 0.50–1) and the remaining 1/3 consequently into group 2 that got the largest grant increase (those with a remote index of 1–1.50). Most of the treated municipalities are located in the mid and especially mid-eastern parts of the country. As can be seen from the table, three of the municipalities in the pre-treatment control group belong to the treatment group (group 1) after treatment took place. In addition, two municipalities in group 1 pre treatment switched into group 2 post treatment (not seen in the tables). Thus, with only 5 out of 367 municipalities changing groups, selection into treatment is hardly a severe problem.

The expenditure variable at the top of Table 2 is defined per capita net of investments, and the largest shares are devoted to social services and health care (on average around 50%) and education and culture (around 25%). The largest single item of expenditure is wages to municipal employees (around 30%).[14] On the revenue side, the main source is taxation, mainly of private income but also of property and corporate income. In 2002, proportionate taxation of private income—i.e., the tax studied here—amounted to around 45% of total revenue, while the corresponding percentage for property and corporate income taxation was merely around 3 and 6, respectively. Tax rates on private income and properties are set locally whereas the level of taxation of corporate income is centralized.

Not too surprisingly, Table 2 reveals differences between treated and controls in many of the variables. Of the outcome variables, especially expenditures are higher in the

[13] Statistics Finland has an awkward way of dealing with consolidated municipalities. For example, if municipality A joined municipality B in year 2001, in new data sets A's population will be added to that of B even in earlier years than 2001. For some variables, this procedure makes more or less sense, while for others (e.g., tax rates or political majority) it makes no sense at all. Consequently, there is no good option but to drop all consolidated municipalities from the data.

[14] Most municipalities operate independently, but some cooperate with one another and provide services through so-called joint authorities, an arrangement most common in the health sector.

treatment group, whereas tax rates do not seem to vary to any great extent. Given how the groups are defined and how the remote index is constructed (cf. the formula in (1)), the fact that the municipality area is considerably larger for those treated with the grant supplement makes sense since larger municipalities naturally have more people living far from the city center. The overall population is also notably smaller. Despite these cross-sectional differences, it is comforting that—aside from the outcome and grants variables—there are no large differential changes over time. For example, the table shows similar decreases in corporate tax revenues in the post-treatment period among treated and control municipalities (explained by the decrease in the share accruing to the municipalities from 37.25 to 24.09%). Still, to ascertain that the effects of increased supplemental grants are not confounded by other factors, the empirical analysis presents regressions that control for relevant variables from Table 2.

The descriptive table includes two grants variables, namely generic grants and total grants. Total grants consist of three main components, and generic grants is the component that includes the supplemental grant to remotely populated municipalities. In addition to this supplement, generic grants include supplements to archipelago municipalities, urban municipalities, and bilingual municipalities as well as a general per capita grant given to all municipalities (above referred to as the base grant). For the municipalities that received a positive supplement of the kind considered here (i.e. those with a remote index larger than 0.50), that supplement was around 70–80% of the generic grants, which, in turn, was around 10% of total grants. Due to a rather uneven distribution of grants across municipalities this figure is, however, closer to 5% overall. Aside from generic grants, the two remaining components of total grants are the so-called sector grants to social services and health care (around 68%) and to education and culture (around 27%). For the average municipality, all these grants amount to around 15–20% of total revenue.[15]

In addition to the three grant components, there is a revenue equalization system where tax revenues are (partly) equalized between municipalities. A fixed percentage of the revenue equalization grant or fee is added to or subtracted from each of the three grant components before the final grant is payed to the municipality as a general, non-earmarked sum. Whenever there are any major reforms in the grant system, municipalities that are largely affected also get a grant (or pay a fee) that is gradually decreased in order to ease the transition. As mentioned above, such transitory grants were used between 1997 and 2001 after the implementation of a new grant system in 1997. Finally, within the grant system, municipalities can also apply for and get additional financial aid due to extraordinary circumstances.

Table 2 shows a slight increase in both outcome variables between the pre- and post-treatment period, both for treated and control municipalities. In order to get a more complete view of the evolution over time, Figure 3 plots yearly averages of expenditures and tax rates in the treatment and control group. The overall picture is a positive but

[15] As shown in Section 5, the policy-induced increase in the supplemental grant indeed induced corresponding increases in the broader grant categories. Whether or not the increase was sufficiently large to yield any behavioral response is then, of course, an empirical question.

Table 2: Summary statistics

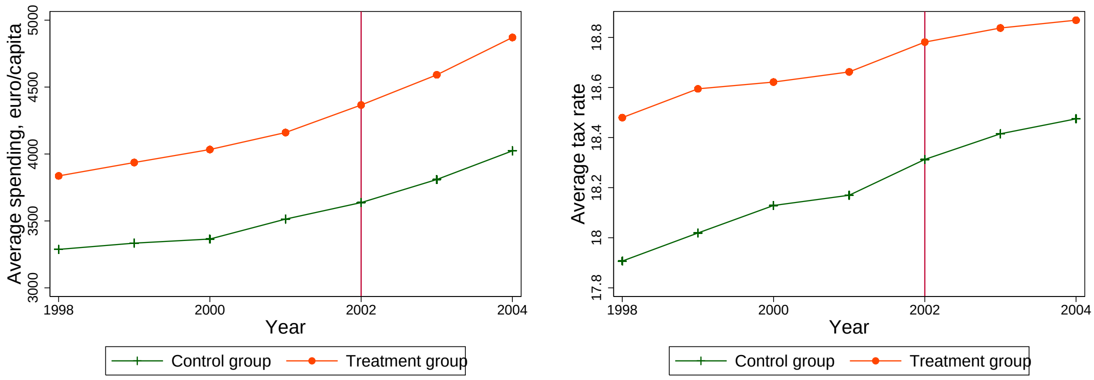
	Treatment group		Control group	
	1998–2001 mean/sd	2002–04 mean/sd	1998–2001 mean/sd	2002–04 mean/sd
Expenditures	3991.6	4609.4	3374.8	3823.4
	(337.8)	(521.1)	(478.7)	(537.1)
Tax rate	18.59	18.83	18.06	18.40
	(0.428)	(0.327)	(0.689)	(0.626)
Generic grants	81.50	121.9	28.42	24.61
	(9.382)	(27.14)	(14.00)	(22.43)
Total grants	1536.4	1907.8	923.4	1167.9
	(240.5)	(271.7)	(389.7)	(480.1)
Population	5288.9	4839.7	13010.0	13269.2
	(4140.3)	(3832.1)	(38034.3)	(39024.6)
Area	1864.3	1799.5	422.3	417.0
	(2240.4)	(2172.8)	(316.3)	(309.5)
Remote index	0.901	0.948	-7.007	-7.231
	(0.267)	(0.267)	(12.12)	(12.57)
School-aged children	0.129	0.120	0.115	0.113
	(0.0204)	(0.0199)	(0.0259)	(0.0256)
Elderly	0.192	0.215	0.180	0.188
	(0.0300)	(0.0325)	(0.0447)	(0.0461)
Welfare recipients	0.0994	0.0818	0.0753	0.0649
	(0.0263)	(0.0251)	(0.0270)	(0.0232)
Income tax base	7478.6	8013.1	9217.1	9822.4
	(819.5)	(723.3)	(2036.9)	(1954.2)
Corp. tax revenues	338.6	224.5	324.1	243.8
	(118.7)	(145.9)	(171.2)	(208.5)
Observations	148	120	1320	981
Municipalities	37	40	330	327

Note: Expenditures, grants, tax base and tax revenues are in euro per capita deflated to 2000 year values, school-aged children, elderly and welfare recipients are in shares of overall population and area is in square kilometers. Elderly and welfare recipients contain 105 and 14 missing values, respectively. Corporate tax revenues are not available for the years 1998–2000.

Source: Government Institute for Economic Research & The Association of Finnish Local and Regional Authorities.

rather stable and parallel trend in both variables prior to the reform, thus showing no evident violation of the identifying assumption of parallel trends. Moreover, it is difficult to visually detect any aggregate effects of increased grants to the treatment group in year 2002, but the econometric analysis in the following section explores these effects in detail.

Figure 3: Average per capita expenditures and tax rates



(a) Expenditures (b) Tax rate

Source: Government Institute for Economic Research.

4 Results

Baseline results are obtained from the estimation of the treatment effects in equation (3) and are presented in Table 3. The four respective rows of the table show the estimated effects of a 1 euro per capita increase in supplemental grants on changes in the outcome between two consecutive years for the period 2001–04, with associated standard errors that allow for clustering within municipality. The first column presents effects on total per capita expenditures, and the second column effects on income tax rates. Because the grant increase occurred in 2002, τ_{2002} represents the immediate treatment effect, whereas τ_{2003} and τ_{2004} represent the dynamic incremental effects one and two years later. Finally, τ_{2001} is an estimate of the difference in pre-treatment trends displayed by the control group and the treatment group, and is thus as such a test of the identifying assumption.

Looking at the left column, the results show both economically and statistically significant effects of increased grants on expenditures: τ_{2002} is estimated to around 0.80, meaning that as grants increase by 1 euro, total expenditures increase by as much as 80 cents (in the same year). Furthermore, one and perhaps even two years after the grant increase, expenditures continue to increase by an additional euro, although the dynamic estimates for the two later years are obtained with much less precision (especially for the year 2004). There are at least two possible interpretations of this pattern: Taken at face value, the estimates reveal a total cumulative response that in fact exceeds the grant increase in 2002, suggesting a path-dependence in the sense that expanding municipalities do not only get accustomed to a larger size of the budget but also to a faster growth rate. Alternatively, it is possible that the assumption of parallel trends is too strong as more and more years pass after the supplemental grant policy-reform, in which case the estimates of τ_{2003} and τ_{2004} cannot be causally interpreted. Although this would, in principle, shed some doubt also on the interpretation of τ_{2002}, the causality of this immediate response is strengthened by the fact that the "effect" in year 2001 is much smaller and not statistically indistinguishable from zero—thus suggesting that there is no difference in trends one year before the policy reform.

To the extent that the large estimated treatment effects on expenditures in the left column are causal, little room is left for grant increases to be used for tax cuts—a notion that is confirmed to be correct in the right column of Table 3. Although the treatment effect in all years is negative, the statistical significance is, at best, weak. More importantly, the size of the point estimates implies limited economic relevance; the immediate effect of -0.001 means that an increase in grants of 100 euro per capita causes the tax rate to decrease by a mere 0.10 percentage points. Or, evaluated at the supplemental grant increase for the group of municipalities with a remote index of 1–1.50 (a 75 euro increase) and holding their pre-treatment tax base constant, the immediate tax response implies that own-source revenues were cut by only 6 euro. This is thus in sharp contrast to the positive implied immediate expenditure response of around $0.80 * 75 = 60$ euro for this group of municipalities.

Table 3: Baseline results

	Expenditures	Tax rates
τ_{2001}	-0.172	-0.000145
	(0.322)	(0.000347)
τ_{2002}	0.829***	-0.000997*
	(0.298)	(0.000531)
τ_{2003}	0.974*	-0.000774
	(0.551)	(0.000489)
τ_{2004}	1.361	-0.000493
	(1.053)	(0.000312)
Observations	2202	2202

Note: The table reports estimated effects of a 1 euro per capita increase in *SG* on per capita expenditures and tax rates over the years 2001–04. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

It is comforting that also the estimate of differences in pre-treatment trends in tax rates supports the identifying assumption, as seen by the insignificant estimate of τ_{2001} in the right column. Hence, the conclusion so far is that increases in grants leave tax rates virtually unchanged but cause expenditures in the same year to increase substantially. Moreover, so far, there is no evidence of dynamic crowding-out—i.e., that the immediate response in expenditures is reversed in later years. The interpretation of the dynamic estimates is not yet clear, however, and therefore, Section 5 returns to this.

4.1 Sensitivity analysis

Before further exploring possible interpretations of the above results, this section conducts sensitivity analyses that investigate the validity of the identifying assumption of parallel trends. In a first set of robustness checks, presented in the different columns of Tables 4 and 5 for expenditures and tax rates, respectively, various control variables are added to the baseline specification in equation (3).

The supplemental grant increase was not the only policy implemented in 2002, and for the identifying assumption to hold, it is required that other policies on average affected treated and control municipalities equally. As mentioned in Section 2 and described in more detail in the Appendix, there were additional reforms in the grant system—for example, the removal of the transitory grants. To test whether the estimated effects of increased supplemental grants are confounded by these changes, column 1 of Tables 4 and 5 adds first-differenced total per capita grants (net of the supplemental grant) as well as first-differenced transitory grants. Column 2 also adds the 2001 level of total and transitory grants to allow for trends in expenditures and tax rates that differ depending on pre-treatment amounts of grants received.

Motivated by the reform also implemented in 2002 that decreased the share of corporate tax revenues accruing to the municipalities, columns 3–4 instead add the first-difference and the 2001 level of per capita corporate tax revenues.[16] To investigates whether the estimated effects are confounded by differences in trends in other variables that are key determinants of expenditures and taxes, columns 5–6 instead add the first-difference and the 2001 level of per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Finally, column 7 combines all of the above.

The overall conclusion across the columns of Tables 4 and 5 is that the baseline results are quite robust to the inclusion of these controls. For expenditures as well as for tax rates, the only reduction in the point estimates seems to be induced by the inclusion of the pre-treatment level of total and transitory grants (columns 2 and 7), but the immediate effect on expenditures is still (weakly) significant. Note that as the absolute size of the estimates decreases both for expenditures and tax rates when these variables are included, the relative response between spending increases and tax cuts is similar to the baseline estimates obtained without further controls. Note, also, that the estimate of τ_{2001} capturing differences in pre-treatment trends is statistically insignificant across all seven columns for both outcome variables.

Moving along to a second, different type of robustness check of the baseline results,

[16] Corporate tax revenues are not available for the years 1998–2000, so for these years the 2001 year value is set.

Table 4: Sensitivity analysis for expenditures; adding controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
τ_{2001}	-0.150	-0.451	-0.172	-0.174	-0.181	-0.357	-0.492
	(0.321)	(0.316)	(0.322)	(0.323)	(0.337)	(0.335)	(0.329)
τ_{2002}	0.833***	0.571*	0.830***	0.828***	0.850***	0.668**	0.548*
	(0.296)	(0.298)	(0.298)	(0.299)	(0.313)	(0.313)	(0.317)
τ_{2003}	0.904	0.656	0.990*	0.990*	1.079*	0.906	0.729
	(0.550)	(0.574)	(0.552)	(0.552)	(0.565)	(0.576)	(0.600)
τ_{2004}	1.375	1.057	1.355	1.352	1.326	1.147	0.936
	(1.056)	(1.020)	(1.054)	(1.055)	(1.079)	(1.067)	(1.025)
Observations	2202	2202	2202	2202	2100	2100	2100
ΔGrants	yes	yes	no	no	no	no	yes
Grants$_{2001}$	no	yes	no	no	no	no	yes
ΔCorp. tax rev.	no	no	yes	yes	no	no	yes
Corp. tax rev.$_{2001}$	no	no	no	yes	no	no	yes
$\Delta\mathbf{X}$	no	no	no	no	yes	yes	yes
$\mathbf{X}_{2001}$	no	no	no	no	no	yes	yes

Note: The table reports estimated effects of a 1 euro per capita increase in *SG* on per capita expenditure over the years 2001–04. **X** is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 5: Sensitivity analysis for tax rates; adding controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
τ_{2001}	-0.000181	0.000119	-0.000145	-0.000153	-0.0000289	0.0000300	0.000312
	(0.000354)	(0.000370)	(0.000347)	(0.000347)	(0.000354)	(0.000372)	(0.000401)
τ_{2002}	-0.00109**	-0.000824	-0.00101*	-0.00102*	-0.00106**	-0.000995*	-0.000776
	(0.000534)	(0.000536)	(0.000526)	(0.000526)	(0.000513)	(0.000517)	(0.000532)
τ_{2003}	-0.000949*	-0.000722	-0.000898*	-0.000897*	-0.000778	-0.000711	-0.000684
	(0.000503)	(0.000513)	(0.000502)	(0.000503)	(0.000507)	(0.000520)	(0.000549)
τ_{2004}	-0.000455	-0.000157	-0.000444	-0.000456	-0.000460	-0.000397	-0.0000946
	(0.000312)	(0.000335)	(0.000313)	(0.000315)	(0.000324)	(0.000347)	(0.000367)
Observations	2202	2202	2202	2202	2100	2100	2100
ΔGrants	yes	yes	no	no	no	no	yes
Grants$_{2001}$	no	yes	no	no	no	no	yes
ΔCorp. tax rev.	no	no	yes	yes	no	no	yes
Corp. tax rev.$_{2001}$	no	no	no	yes	no	no	yes
$\Delta\mathbf{X}$	no	no	no	no	yes	yes	yes
$\mathbf{X}_{2001}$	no	no	no	no	no	yes	yes

Note: The table reports estimated effects of a 1 euro per capita increase in *SG* on tax rates over the years 2001–04. **X** is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

the structure of the supplemental grant is exploited in Table 6 (for expenditures) and Table 7 (for tax rates). Recall, first, that to be treated with supplemental grants, the remote index had to be larger than +0.50 and that characteristics such as size of population and area varied quite substantially with the remote index. Thus, a potential concern is that the original control group makes for a poor counterfactual. Municipalities with remote indices substantially smaller than in the treatment group are therefore excluded from the control group; in columns 1 and 2 the control group is restricted to only include those with a remote index larger than -10 and -5, respectively. Second, recall that the pre- and post-treatment level as well as the policy-induced increase in the supplemental grant are discontinuous functions of the remote index with discrete jumps at 0.50 and 1 (cf. Table 1). Although the by regression discontinuity (RD) standards small sample hinders a full-fledged non-parametric RD analysis, it is still possible to exploit these discontinuities parametrically. This is done in columns 3–5 in Tables 6 and 7; columns 3–4 control for a linear/quadratic direct effect of the remote index, and column 5 controls for a linear effect while also restricting the control group to municipalities with a remote index larger than -10.

Table 6: Sensitivity analysis for expenditures; restricting the control group and exploiting discontinuities

	(1)	(2)	(3)	(4)	(5)
τ_{2001}	-0.235	-0.296	-0.211	-0.352	-0.511
	(0.326)	(0.330)	(0.324)	(0.326)	(0.337)
τ_{2002}	0.804***	0.790**	0.791***	0.659**	0.546*
	(0.302)	(0.308)	(0.300)	(0.302)	(0.309)
τ_{2003}	0.919*	0.863	0.939*	0.814	0.677
	(0.555)	(0.562)	(0.553)	(0.557)	(0.566)
τ_{2004}	1.334	1.297	1.325	1.195	1.082
	(1.062)	(1.074)	(1.054)	(1.055)	(1.064)
Observations	1860	1464	2202	2202	1860
Remote index>	-10	-5	no restr.	no restr.	-10
f(Remote index)	−−	−−	linear	quadratic	linear

Note: The table reports estimated effects of a 1 euro per capita increase in SG on per capita expenditures over the years 2001–04. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

As seen in Tables 6–7, the picture from the first set of sensitivity analyses also pertains here; most specifications essentially leave the point estimates unaffected—this is true even when 1/3 of the sample is excluded, as seen from column 2. The size and significance of the immediate effect on expenditures (i.e. of τ_{2002}) are, however, reduced somewhat in the perhaps most restrictive regression in column 5. And, as above, none of the estimates of differences in pre-treatment trends (i.e. of τ_{2001}) are statistically significant.

Table 7: Sensitivity analysis for tax rates; restricting the control group and exploiting discontinuities

	(1)	(2)	(3)	(4)	(5)
τ_{2001}	-0.0000861	-0.0000945	-0.0000724	-0.00000249	0.0000589
	(0.000349)	(0.000365)	(0.000350)	(0.000353)	(0.000369)
τ_{2002}	-0.000978*	-0.000909*	-0.000927*	-0.000861	-0.000843
	(0.000544)	(0.000550)	(0.000536)	(0.000543)	(0.000561)
τ_{2003}	-0.000732	-0.000711	-0.000708	-0.000647	-0.000605
	(0.000497)	(0.000511)	(0.000492)	(0.000499)	(0.000513)
τ_{2004}	-0.000389	-0.000488	-0.000424	-0.000360	-0.000257
	(0.000311)	(0.000323)	(0.000316)	(0.000324)	(0.000339)
Observations	1860	1464	2202	2202	1860
Remote index>	-10	-5	no restr.	no restr.	-10
f(Remote index)	−−	−−	linear	quadratic	linear

Note: The table reports estimated effects of a 1 euro per capita increase in *SG* on tax rates over the years 2001–04. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

To sum up the sensitivity analysis, most of the estimated effects of a 1 euro increase in grants on the immediate response in expenditures are in the range 70–80 cents, although the response is reduced to around 55 cents in a few specifications, and all immediate expenditure effects are statistically significant at least at the 10% level but in most cases also at the 5% level. The estimated effects on the tax response from a grant increase are, however, both economically and statistically much weaker. Furthermore, the overall robustness of the baseline results to various alternative specifications together with the test of the identifying assumption of parallel trends validates the claim that the policy-induced increase in the supplemental grant is exogenous and hence, supports a causal interpretation of these results.

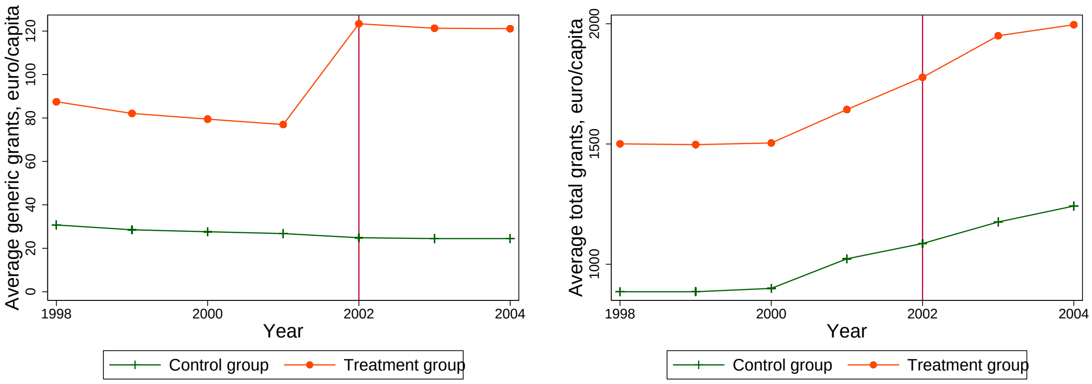
5 *Using 2SLS to understand the dynamics*

Note that not only were the estimated immediate responses robust across specifications in the previous section, but so were the effects one and two years later. If causal, this would imply that there is no dynamic crowding-out of the supplemental grant increase but rather that the cumulative effects are very large. But, as noted above, an alternative explanation is that treated and control municipalities differ also in other respects in later years so that the dynamic effects are biased.

One may be particularly concerned that municipalities treated with supplemental grant increases in 2002 received larger grant increases than control municipalities also in 2003 and 2004, which could cause the large positive estimates of τ_{2003} and τ_{2004} for expenditures. To this aim, Figure 4 illustrates how generic grants (i.e. the type of grant that the supplement is part of) and total grants (of which generic grants subsequently are part) have evolved over the sample period.

Since the supplemental grant constitutes as much as 80% of generic grants to municipalities in the treatment group, it is not surprising that Figure 4 displays an increase in generic grants in 2002 of a similar magnitude as the increase in the particular supplement. In total grants, however, the relative size of the supplemental grant increase is too small and/or there is too much noise for visual inspection to clearly reveal any sharp changes.

Figure 4: Average generic grants and total grants



(a) Generic grants (b) Total grants

Source: The Association of Finnish Local and Regional Authorities.

The extent to which the supplemental grant increase correlates with increases in generic and total grants over time can also be estimated by running the following regression for $t = \{2002, 2003, 2004\}$:

$$G_{it} - G_{i2001} = \gamma_t (SG_{2002} - SG_{2001}) + T_{2001} + (e_{it} - e_{i2001}), \tag{4}$$

where G_{it} is the amount of generic or total grants received by municipality i in year t. With this specification, the parameter γ_t measures how much generic or total grants increased between 2001 and year t for each euro that the supplemental grant increased between 2001 and 2002. If the changes in other types of grants are not systematically different between the treatment and the control group, neither in the same year as the supplemental grant increase (i.e. for $t = 2002$) nor in later years (for $t = \{2003, 2004\}$), then γ_t should be 1 for all t.

In previous sections, treatment was defined as increased supplemental grants. If treatment is instead defined as increased generic grants or increased total grants, equation (4) is the first stage in a two-stage least squares (2SLS) estimation. Using the predicted values from (4), $\widehat{G_{it} - G_{i2001}}$, estimates of the effect of increased grants over a one-, two- and three-year period due to the policy-induced increase in supplemental grants between 2001 and 2002, τ_t^{IV}, are then recovered from the second stage:

$$Y_{it} - Y_{i2001} = \tau_t^{IV} (\widehat{G_{it} - G_{i2001}}) + T_{2001} + (\varepsilon_{it} - \varepsilon_{i2001}) \tag{5}$$

Just like the first stage, equation (5) is estimated for $t = \{2002, 2003, 2004\}$ separately.

In the context of 2SLS, the estimates of τ_t in the previous section are thus the reduced form results. Note that τ^{IV}_{2002} in equation (5) is directly comparable to τ_{2002} from the original equation (3) above, while τ^{IV}_{2003} and τ^{IV}_{2004} are the *cumulative* effects of a 1 euro grant increase whereas τ_{2003} and τ_{2004} are the *incremental* effects. But aside from these technical differences, if the first-stage estimate of γ_t equals 1, the 2SLS results should be the same as the reduced form results. The reason is that the municipalities receive all grants as a non-earmarked general sum, implying that a euro increase is always a euro increase irrespectively of the type of grant. If, one the other hand, γ_t differs from 1, the interpretation of the second-stage estimate of τ^{IV}_t is, in principle, still the effect of a euro grant increase, but the problem is then that part of the variation is most likely not exogenous in which case the effect cannot be causally interpreted.

The first set of results from this 2SLS estimation of equations (4) and (5) is provided in Table 8. Here, G_{it} is defined as generic grants and the upper panel contains the first-stage estimates of γ_t while the mid and bottom panel contain the second-stage estimates of τ^{IV}_t for expenditures and tax rates, respectively. In columns 1–3, $t = 2002$ and the differences are thus over one year; in columns 4–6, $t = 2003$ with differences over two years; and in columns 7–9, $t = 2004$ with differences over three years. For each period, the left column gives the baseline 2SLS results, while the mid column controls for the one-, two- and three-year difference in per capita corporate tax revenues, per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients and the right column additionally controls for the pre-treatment level of these variables. In other words, the mid and right columns contain a similar robustness check as in Tables 4 and 5 above.

The upper panel of Table 8 shows first-stage estimates of γ_t that are essentially 1 for the one-year period with $t = 2002$. These estimates are rather precise (i.e., the standard errors are small), quite insensitive to the inclusion of control variables and do not change to any considerable extent for the longer periods. This says that, aside from the supplemental grant increase, there were no systematic differences in the changes of generic grants to municipalities in the treatment group as compared to those in the control group over the period 2001–04.

Moving along to the mid and bottom panel, as expected from the size of the first stage, the second-stage estimates for the one-year period are very similar to the corresponding reduced form estimates in the previous section. The interpretation of these coefficients is that a 1 euro increase in generic grants stemming from the policy-induced increase in supplemental grants causes expenditures to increase by around 70-90 cents and tax rates to decrease by around 0.001 percentage points (in the same year). Also as expected, the second-stage estimates for longer periods are similar in magnitude to the sum of the incremental effects estimated above.

Table 9 repeats the sensitivity check from the previous section where the discontinuities in the distribution of the supplemental grant were exploited. For the three different periods, the left column reproduces the baseline 2SLS estimates, the mid column controls for a quadratic direct effect of the remote index and the right column controls for

Table 8: Instrumenting generic grants with the supplemental grant; baseline estimates and adding controls

	Period 2001–02			Period 2001–03			Period 2001–04		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
γ_t : 1^{st} stage	0.953***	0.955***	0.966***	0.917***	0.860***	0.830***	0.914***	0.837***	0.811***
	(0.00970)	(0.0110)	(0.00974)	(0.00965)	(0.0243)	(0.0241)	(0.00946)	(0.0273)	(0.0282)
τ_t^{IV}: expenditures	0.910***	0.875***	0.690**	2.087***	2.328***	2.111**	3.629***	3.894**	3.553**
	(0.314)	(0.328)	(0.334)	(0.709)	(0.790)	(0.823)	(1.354)	(1.533)	(1.558)
τ_t^{IV}: tax rate	-0.00104*	-0.00115**	-0.00108**	-0.00197***	-0.00299***	-0.00282***	-0.00257***	-0.00361***	-0.00311***
	(0.000543)	(0.000536)	(0.000542)	(0.000718)	(0.000801)	(0.000841)	(0.000754)	(0.000866)	(0.000884)
Observations	734	700	700	734	700	700	734	700	700
ΔCorp. tax rev.	no	yes	yes	no	yes	yes	no	yes	yes
Corp. tax rev.$_{2001}$	no	no	yes	no	no	yes	no	no	yes
$\Delta\mathbf{X}$	no	yes	yes	no	yes	yes	no	yes	yes
$\mathbf{X}_{2001}$	no	no	yes	no	no	yes	no	no	yes

Note: In the upper panel, the table reports first-stage estimates of a 1 euro per capita increase in SG on per capita generic grants. In the mid and bottom panel, the table reports second-stage estimates of a 1 euro per capita increase in generic grants on per capita expenditures (mid) and tax rates (bottom). $\mathbf{X}$ is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 9: Instrumenting generic grants with the supplemental grant; baseline estimates, exploiting discontinuities

	Period 2001–02			Period 2001–03			Period 2001–04		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
γ_t : 1^{st} stage	0.953***	0.949***	0.944***	0.917***	0.851***	0.796***	0.914***	0.850***	0.796***
	(0.00970)	(0.0119)	(0.0141)	(0.00965)	(0.0160)	(0.0218)	(0.00946)	(0.0158)	(0.0216)
τ_t^{IV}: expenditures	0.910***	0.716**	0.728**	2.087***	1.777**	1.711**	3.629***	3.162**	3.013*
	(0.314)	(0.321)	(0.336)	(0.709)	(0.770)	(0.836)	(1.354)	(1.467)	(1.585)
τ_t^{IV}: tax rate	-0.00104*	-0.000765	-0.00102*	-0.00197***	-0.00165**	-0.00208**	-0.00257***	-0.00231***	-0.00285***
	(0.000543)	(0.000562)	(0.000571)	(0.000718)	(0.000806)	(0.000881)	(0.000754)	(0.000840)	(0.000928)
Observations	734	734	620	734	734	620	734	734	620
Remote index>	no restr.	no restr.	-10	no restr.	no restr.	-10	no restr.	no restr.	-10
f(Remote index)	−−	quadratic	linear	−−	quadratic	linear	−−	quadratic	linear

Note: In the upper panel, the table reports first-stage estimates of a 1 euro per capita increase in *SG* on per capita generic grants. In the mid and bottom panel, the table reports second-stage estimates of a 1 euro per capita increase in generic grants on per capita expenditures (mid) and tax rates (bottom). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

a linear effect while restricting the control group to municipalities with a remote index larger than -10. These results lead to the same conclusion as in the previous table; all first-stage estimates are close to 1, the one-year period second-stage estimates are around 0.70-0.90 for expenditures and around -0.001 for tax rates, and the estimates are larger in absolute terms the longer is the time period.

Thus, on the one hand, it is comforting that there are no systematic differences between the treatment and the control group in the amount of generic grants received even over the longer period. On the other hand, the very large cumulative effects remain somewhat puzzling. To further investigate this, Tables 10–11 reproduce the analysis in Tables 8–9, but instrument total grants rather than generic grants. Looking first at the results for the one-year period in columns 1–3 in both tables, also these first-stage estimates are close to 1 even though they are estimated with less precision (the standard errors are 15 times those for generic grants). Consequently, also the one-year period second-stage estimates are in the same range as before. For longer time-periods, however, the first-stage estimates tend to exceed 1 considerably, and the second-stage estimates are more unstable across time and alternative specifications.

All in all, because there are no systematic differences in the changes in neither generic nor total grants between the treatment and control group over the 2001–02 period (aside from the supplemental grant increase), the analysis in this section supports a causal interpretation of the immediate effect of increases in grants; be they supplemental, generic or total grants, a 1 euro increase causes expenditures to increase approximately by as much as 70–90 cents but causes taxes to decrease by a mere 0.001 percentage points (at most). On the contrary, since there appear to be systematic differences in the amount of total grants received over longer periods, a causal interpretation of the dynamic effects of grant increases is more problematic. But if anything, the results seem to suggest that the stimulatory effects on expenditures remain 2–3 years after the grant increase rather than that the grant increase is crowded out by decreases in own-sources revenues.

6 *Concluding discussion*

Intergovernmental grants are widely used in fiscally decentralized countries. Knowledge about the effects of these grants on the receiving jurisdiction is therefore of considerable policy relevance. To this date, however, there are very few studies that convincingly estimate causal effects of grants and only one that focuses on general, non-targeted grants, which has been the aim in this paper. I estimate the effect on local expenditures and taxes of a policy that treated a group of remotely populated municipalities in Finland with increased grants while leaving another group serving as controls untreated.

The robust finding—in line with the results in Dahlberg et al. (2008)—is that increased grants have a negligible effect on local income tax rates, but that there is a substantial positive immediate response in local expenditures. Specifically, according to most specifications, a 1 euro increase in grants causes expenditures to increase by around 70–80 cents. Or, evaluated at the grant increase to one of the groups of treated municipalities, expenditures increased by around 60 euro per capita whereas the implied

Table 10: Instrumenting total grants with the supplemental grant; baseline estimates and adding controls

	Period 2001–02			Period 2001–03			Period 2001–04		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\gamma_t : 1^{st}$ stage	1.435***	1.227***	0.853***	2.801***	1.742***	1.257***	2.514***	1.393***	0.746***
	(0.169)	(0.171)	(0.162)	(0.320)	(0.242)	(0.249)	(0.330)	(0.233)	(0.249)
τ_t^{IV}: expenditures	0.604***	0.681***	0.782**	0.683***	1.149***	1.394***	1.319**	2.340**	3.863*
	(0.212)	(0.259)	(0.397)	(0.226)	(0.383)	(0.534)	(0.545)	(1.017)	(2.311)
τ_t^{IV}: tax rate	-0.000689*	-0.000895**	-0.00123*	-0.000644***	-0.00148***	-0.00186***	-0.000934***	-0.00217***	-0.00338**
	(0.000361)	(0.000407)	(0.000651)	(0.000247)	(0.000449)	(0.000677)	(0.000292)	(0.000644)	(0.00155)
Observations	734	700	700	734	700	700	734	700	700
ΔCorp. tax rev.	no	yes	yes	no	yes	yes	no	yes	yes
Corp. tax rev.$_{2001}$	no	no	yes	no	no	yes	no	no	yes
$\Delta\mathbf{X}$	no	yes	yes	no	yes	yes	no	yes	yes
$\mathbf{X}_{2001}$	no	no	yes	no	no	yes	no	no	yes

Note: In the upper panel, the table reports first-stage estimates of a 1 euro per capita increase in SG on per capita total grants. In the mid and bottom panel, the table reports second-stage estimates of a 1 euro per capita increase in total grants on per capita expenditures (mid) and tax rates (bottom). **X** is a vector including the per capita corporate tax revenues, per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 11: Instrumenting total grants with the supplemental grant; baseline estimates, exploiting discontinuities

	Period 2001–02			Period 2001–03			Period 2001–04		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
γ_t : 1^{st} stage	1.435***	1.099***	1.101***	2.801***	1.905***	1.674***	2.514***	1.125***	0.795**
	(0.169)	(0.173)	(0.180)	(0.320)	(0.340)	(0.366)	(0.330)	(0.358)	(0.393)
τ_t^{IV}: expenditures	0.604***	0.618**	0.624**	0.683***	0.794**	0.814**	1.319**	2.388*	3.017
	(0.212)	(0.286)	(0.300)	(0.226)	(0.343)	(0.403)	(0.545)	(1.432)	(2.332)
τ_t^{IV}: tax rate	-0.000689*	-0.000661	-0.000873*	-0.000644***	-0.000739*	-0.000987**	-0.000934***	-0.00175**	-0.00286*
	(0.000361)	(0.000487)	(0.000503)	(0.000247)	(0.000387)	(0.000479)	(0.000292)	(0.000828)	(0.00170)
Observations	734	734	620	734	734	620	734	734	620
Remote index>	no restr.	no restr.	-10	no restr.	no restr.	-10	no restr.	no restr.	-10
f(Remote index)	−−	quadratic	linear	−−	quadratic	linear	−−	quadratic	linear

Note: In the upper panel, the table reports first-stage estimates of a 1 euro per capita increase in SG on per capita total grants. In the mid and bottom panel, the table reports second-stage estimates of a 1 euro per capita increase in total grants on per capita expenditures (mid) and tax rates (bottom). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

cut in own-source revenues was only 6 euro per capita. While a few specifications result in somewhat smaller effects, the absolute size of the estimates decreases both for expenditures and tax rates so that the relative response between spending increases and tax cuts is robust.

A glance at a balance of payment sheet for Finnish finances shows that, on aggregate, total consumption is around 50% of GDP. Out of total consumption, only 30% are public consumption and, hence, 70% are private consumption. The large stimulatory effects on public expenditures can thus be interpreted as crowding-in effects, opposite to the crowding-out effects as found by, for example, Knight (2002). But recall that he studied grants aimed at supporting state highway construction, whereas this study and the Dahlberg et al. study concern general, non-earmarked grants and how they affect taxes and overall spending. Thus, it is likely that the common effects of general grants to Finnish municipalities as found here and to Swedish municipalities as found in Dahlberg et al. are externally valid to other federations characterized by considerable local independence. Indeed, the scope for targeted grants to crowd out spending on specific projects seems much larger than for general grants to crowd out total expenditures.

Furthermore, contrary to the results in Gordon (2004), there is no evidence of dynamic crowding-out—i.e., that the immediate response in expenditures is reversed in later years. However, unlike the immediate effects, the dynamic effects seem to be partly driven by variation in grants that is not exogenous, hindering a causal interpretation of the dynamic effects. Future work that further investigates the dynamics in the grant response is therefore called for.

The focus of this paper has been to give a convincing answer to *how* local governments respond to increases in grants. But in concluding, let me propose an answer to *why* these municipalities apparently display flypaper behavior—if only to steer the way for future research. "Separate mental accounting", i.e. that voters treat the government budget constraint separately from their own, is an explanation that can be attributed to Tversky and Kahneman (1984) and Thaler (1985) but that is often dismissed as unlikely to fully explain the empirical flypaper anomaly. In contrast, I believe it to be quite likely. In Finland in particular, the labeling of grants as "grants to social services and health care" and "grants to education and culture" (despite the fact that all grants are in fact non-targeted) may very well trigger such mental accounting.

A different yet related explanation is that, with these labels, the central government signals that its intention in distributing grants is first and foremost to finance expenditures rather than tax cuts. Possibly, this can further encourage increased local spending if the municipalities fear that by instead responding with tax cuts, they may be disqualified from future grants.

An interesting aspect is that there is no obvious reason why the state should be unwilling to finance local tax cuts. One of the main motivations behind a federal system where revenue accumulation is centralized whereas expenditures are decentralized and financed via grants is that local taxation is assumed to have higher deadweight costs. The policy recommendation that emerges from all this would thus not have followed trivially: federal governments that wish to increase disposable income should do so directly by lowering federal tax rates rather than relying on local governments to use increased grants to finance tax cuts, and federal governments that to some extent irrationally wish to induce increased local spending by distributing general grants can succeed in doing so, even though the induced behavior may in itself also be irrational.

Appendix

Other policies implemented in 2002

In this appendix, policies implemented in 2002 other than the one that increased the supplemental grant to remotely populated municipalities are reviewed. This is by no means a complete description of all implementations, but rather the attention is restricted to what is related to the specific policy reform studied in the paper. Specifically, for identification purposes, the simultaneous implementations require that treated and control municipalities were on average equally affected by these other policies. Fortunately—as is done in Section 4.1—most of this can be tested.

The policy reform that increased the supplemental grant to remotely populated municipalities is proposed in Government Bill 128/2001 and legislated in Law 1360/2001. These documents are also concerned with the following changes and reforms:

- There was a change in the amount of the grant supplement to archipelago municipalities. According to law 494/1981, the development of a group of municipalities located in the archipelago is to be promoted. Before (after) 2002, such municipalities where at least 50% of the population lacked access to a solid connection to the mainland got a per capita supplement equal to 3 (6) times the base grant, and those where less than 50% lacked access to a solid connection to the mainland got a per capita supplement equal to 1.5 (3) times the base grant. In addition, municipalities not belonging to this particular group but that also had some share of their population in the archipelago got a supplement equal to 0.75 (1.5) times the base grant for each person living in the archipelago before (after) 2002. In the sample used in the paper, 41 municipalities received the archipelago supplement, all of which are in the control group. Neither excluding these 41 municipalities from the estimations nor controlling for the archipelago supplement affects the presented results.

- In the revenue-sharing system, municipalities with potential per capita tax revenues (revenues when applying a weighted average of the tax rates) above average pay a fee equal to 40% of the difference. Before 2002, this fee could be at most 15% of the municipality's total per capita potential tax revenues, but in 2002 this cap was removed. This affected 4 municipalities, all in the control group. Excluding them from the estimations does not affect the results presented in the paper.

- Municipalities that were highly affected by the introduction of the new grant system in 1997 got transitory grants that were gradually decreased between 1997 and 2001 and were entirely removed in 2002. This removal considerably affected the group of the 13 most remotely populated municipalities, which is why they are removed from the empirical analysis. Note also that the results presented in the paper when controlling for transitory grants to the remaining municipalities are similar to the baseline results.

- Some of the activities in the local government sector are directly financed by the state to an extent that may vary over time, in which case there is an adjustment through the sector grants (grants to social services and health care and grants to education and culture). An adjustment due to increased relative financing responsibility on behalf of the municipalities in 2000 was originally to be implemented with 50% in 2001 and with 25% each in 2002 and 2003. However, it was decided that the full remaining 50% were to be implemented in 2002, implying that the increase in the sector grants was brought forward to 2002 from 2003. There were also some additional changes to the sector grants; see below.

One of the more significant reforms in 2002 aiming at stabilizing local government finances was a change in the administration of value added taxes (VAT), described in Government Bill 130/2001 and legislated in Laws 1456–1457/2001. When the municipalities' activities involve goods with VAT, they (like firms) are entitled to deductions. Prior to 2002, the municipalities had to repay these deductions to the state with an equal per capita amount. Since the amount of deductions varied considerably across regions but the repayments were the same, this made it difficult to keep stable finances and thus the repayments were abolished. Consequently, this shifted the fiscal balance in favor of the municipalities at the expense of the state.

The main reform to re-balance the fiscal relation was a decrease in the municipalities' share of revenue—and thereby an increase in the state's share—from corporate income taxation (also proposed in 130/2001 and legislated in Laws 1458-1459/2001). Part of the motivation was that this type of revenue was highly sensitive to economic fluctuations and was very unevenly distributed across municipalities depending on business locations. The municipalities' share was therefore decreased from 37.25 to 24.09%. Note that the results presented in the paper when controlling for corporate tax revenues are similar to the baseline results.

Finally, partly as a consequence of some of the previously described reforms, there were some changes to the sector grants (proposed in Government Bill 132/2001 and legislated in Law 1389/2001 for education and culture, and proposed in Government Bill 152/2001 and legislated in Law 1409/2001 for social services and health care). As previously mentioned, these grants were increased in order to adjust for the altered fiscal responsibilities between the state and the municipalities. It was additionally decided that the increase in the state's revenue due to the removal of the 15% cap in the revenue sharing system was to be transferred to the municipalities as increased grants to social services and health care. On the other hand, the reform in the VAT system implied decreased sector grants. All in all, the majority of municipalities received more sector grants in 2002 than in 2001. Note that the results presented in the paper when controlling for total grants received are similar to the baseline results.

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ESSAY 4: STIMULATING LOCAL PUBLIC EMPLOYMENT: DO GENERAL GRANTS WORK?

1 Introduction

Understanding the determinants of public employment is important for at least two reasons. First of all, the public sector in most European countries supplies important welfare services, such as child care, education and health care. The well-being of the citizens is closely linked to the quality of these services, and given that they are typically very labor intensive, a good way of influencing the quality is to decide how many and who to employ. Second, the public sector commonly accounts for large parts of aggregate labor demand; in many countries, as much as 15–20% of the labor force are publicly employed. Hence, stimulating public employment may be a promising way of reducing the overall unemployment level.

Both these issues are of vital importance for central politicians. However, in many countries, the responsibility for supplying welfare services is decentralized to the local level. Central politicians wanting to affect the quality of welfare services or stimulate public employment therefore have to influence local politicians to implement desired policies. This influence can take many forms, but given that one wants to maintain local autonomy, intergovernmental grants are the main financial means through which the central government can have an impact on lower-level governments. Thus, the effect of intergovernmental grants on local public employment is a question of great policy relevance. The purpose of this paper is to estimate causal effects of general intergovernmental grants on local public employment—both in total and disaggregated by sector—using a panel of Swedish municipalities covering the period 1996–2004.

The current economic crisis has, if anything, strengthened the focus on the effect of grants on public employment, since many federalist countries have initiated this type of policies to try to limit the negative effects of the recession on public welfare.[1] A recent example is the American Recovery and Reinvestment Act (ARRA) of 2009, where federal grants are being used to stimulate the US economy and employment. Another country where the central government has turned its hope to grants—and to their potential of stimulating public employment in particular—is Sweden; in the fall of 2009, the Swedish government decided to give a significant amount of extra general grants to local governments. The purpose of these additional grants, it was argued, was to avoid layoffs in order to guarantee a sustained welfare level and limit overall unemployment effects.

Since most decentralized public services are labor intensive, personnel costs typically account for large parts of the budget of lower-level governments. It might, therefore, be expected that more revenues in the form of increased general grants can indeed stimulate public employment. But there are theoretical arguments for why this may fail to happen. For example, local governments can substitute grants for own-source revenues by cutting local taxes. Empirically, there is, however, little support for this argument, as previous research has shown that increased grants to Swedish municipalities stimulate spending one-for-one but leave taxes unchanged (Dahlberg et al., 2008). Still, even when aggregate spending is stimulated by increased grants, it is a priori uncertain whether the additional

[1] This idea is not new. For example, in the 1970's the US introduced the Public Employment Program (PEP) and the Comprehensive Employment and Training Act (CETA) with this purpose in mind.

expenditures are used for personnel and, if so, in which sectors.

Despite the high policy relevance, the existing literature is very limited.[2] The earliest paper of which we are aware is Johnson and Tomola (1977) which evaluates US public employment programs in the 1970's. They find that the effects on employment are substantial in the short run, but that they vanish after one year due to fiscal substitution by state and local governments. More recently, an emerging body of literature tries to estimate to what extent ARRA has affected both public and total employment. The results from these studies are partly contradictory: Wilson (2011) finds that ARRA spending created or saved about 2 million jobs in its first year. Also the results in Feyrer and Sacerdote (2011) support the effectiveness of ARRA, but suggest that the effects differ for different types of spending within ARRA. Chodorow-Reich et al. (2011) focus on the Medicaid part of ARRA and find that an additional $100,000 in Medicaid spending resulted in an additional 3.1 job-years, of which the bulk consisted of jobs outside the public sector. Conley and Dupor (2011), on the other hand, find that ARRA spending saved 450,000 jobs in the government sector but destroyed 1 million private sector jobs. Common to all these papers is that they investigate the effects of grants on total employment, public as well as private. Focusing on the specific role of grants in explaining local public employment, Bergström et al. (2004) investigate how grants (and wages) affect the demand for labor by Swedish municipalities. They find that intergovernmental grants had a negligible effect on total local public employment in Sweden over the period 1988–95.

All the above mentioned studies face the methodological challenge that grants are likely to be endogenous, meaning that OLS estimates are likely to be biased. The root of the endogeneity problem is that grants are not randomly distributed to lower-level governments but, rather, that these receive grants motivated by some underlying need. Such needs are likely to be directly related to labor demand, implying that perceived correlations between grants and employment partly stem from the *determinants* of the grant distribution rather than the causal effect of grants in itself.[3] Bergström et al. (2004) use the Arellano-Bond GMM estimator where lagged values of grants (and wages) are used as instruments, and rely on the Sargan test for overidentifying restrictions in order to decide whether the instruments are valid. But because the Sargan test is known to have low power, it is quite possible that the grant endogeneity problem remains unsolved. Wilson (2011) and Conley and Dupor (2011) both use the formula-based parts of the ARRA as instruments for grants. However, it is questionable whether the formulas are set exogenously, implying that they might be correlated with local public employment, which would bias the estimated grant effects. Inspired by Knight (2002), Feyrer and Sacerdote (2011) instead use the mean seniority of the House delegation as instrument for ARRA spending received by the state. The validity of this instrument rests on the assumption that similar political economy factors do not affect the employment level in the state, which is a strong assumption. The most convincing identification

[2] See Ehrenberg and Schwarz (1986) for a survey of the early literature.

[3] See also Besley and Case (2000) for a discussion of endogenous policies in general, and Knight (2002), Gordon (2004) and Dahlberg et al. (2008) for further discussions of potential endogeneity of grants.

strategy in the above papers is the one in Chodorow-Reich et al. (2011) which uses past Medicaid reimbursements as an instrument for ARRA. Conditional on a number of control variables, they argue that this instrument is exogenous—a claim that is supported by a set of placebo investigations.

In this paper, we adopt a version of the identification strategy used by Dahlberg et al. (2008) to solve the grant endogeneity problem. The idea is to make use of a kinked assignment rule in the Swedish grant system whereby municipalities with a net out-migration above 2% receive grants, whereas those below 2% do not. Because any direct effect of out-migration on personnel can be assumed to be smooth, a kinked relationship between out-migration and personnel can be attributed to differences in the amount of grants received.

Our method is similar in spirit to the regression discontinuity design (RDD)[4] and is labeled regression kink design (RKD) by Nielsen et al. (2010). Card et al. (2009) derive formal identifying assumptions and resulting testable predictions for this method.[5] In this paper, we adopt a fuzzy version of the RKD where the identifying assumption of no kink in the direct effect of the assignment variable on the outcome is used as an exclusion restriction in an IV estimation. For our application, this approach identifies the causal effect of grants as long as the direct effect of out-migration on personnel is smooth.

Our empirical analysis shows that an increase in intergovernmental grants has no effect on the total number employed by the municipality. This result is in line with the findings in Bergström et al. (2004). When looking at employment disaggregated by sector, we only find a positive, statistically significant effect on administrative personnel. Personnel in the other sectors—child care, schools, elderly care, social welfare and technical services—are however unaffected (in a statistical as well as economical sense). Furthermore, the estimated impact on administrative personnel is a rather large, economically significant effect; a 100 SEK increase in per capita grants, which is an increase of around 6-7% from the mean, leads to an increase of around 0.5% of the mean for this personnel category.[6]

The outline of the rest of the paper is as follows: Section 2 presents the strategy used to identify causal effects of intergovernmental grants on different types of local government personnel. Section 3 provides a description of the role of local governments and intergovernmental grants in Sweden, along with a description of the data. Section 4 gives the baseline results, as well as a detailed examination of the validity of the identifying assumptions. Section 5 looks in more detail at the positive grants effects obtained on administrative personnel, and Section 6 concludes the paper by discussing possible interpretations of the results.

[4] See Angrist and Lavy (1999), Hahn et al. (2001), Lee (2008) and Lee and Lemieux (2010) for important contributions.

[5] As the RKD is still rather new, it has so far not been extensively applied. However, both Nielsen et al. (2010) and Card et al. (2009) have empirical applications, as do Guryan (2003), Simonsen et al. (2010) as well as the study by Dahlberg et al. (2008) referred to above.

[6] When contrasting these results to the earlier literature reviewed above, it should be stressed that the period we study differs from that of the ARRA, in the sense that the Swedish economy was in a relatively good state during the period studied.

2 *Identification strategy*

We are interested in the causal effect of intergovernmental grants on different types of municipal personnel, i.e., the relationship we want to identify is given by

$$y_{i,t} = \beta_0 + \beta_1 g_{i,t} + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is the number of personnel employed by municipality i in year t (in total and disaggregated by sector), and $g_{i,t}$ are grants received by the municipality. A (naïve) OLS estimate of β_1 will most likely be biased. The source of the bias can either be simultaneity of grants and personnel—i.e., that the level of public employment in the municipality affects the amount of grants received—or omission of key variables that determine both grants and personnel. Note, however, that even when the grant formula is completely known, it is not possible to identify the causal effect of grants simply by including all grant determinants in a regression, since that would leave no remaining variation in the grants variable to identify the effect of interest.[7] To eliminate all sources of bias, an experiment where municipalities are randomly given different amounts of grants would instead be ideal. Because such an experiment will most likely never be conducted (it seems quite politically infeasible), we turn to institutional details that allow us to come as close as possible to randomization of grants. Following Dahlberg et al. (2008), we use a kinked assignment rule in the Swedish cost-equalizing grants as a source of exogenous variation. The cost-equalizing grants come with no strings attached and are intended to support municipalities that are characterized by demographic and other structural conditions associated with higher costs. We return to the role played by these grants in Section 3.

The component on which we focus supports jurisdictions with a diminishing population by distributing out-migration grants, $g^m_{i,t}$, to municipality i in year t according to the kinked assignment rule

$$g^m_{i,t} = \begin{cases} a(m_{i,t} - 2) & \text{if } m_{i,t} > 2 \\ 0 & \text{if } m_{i,t} \leq 2. \end{cases} \tag{2}$$

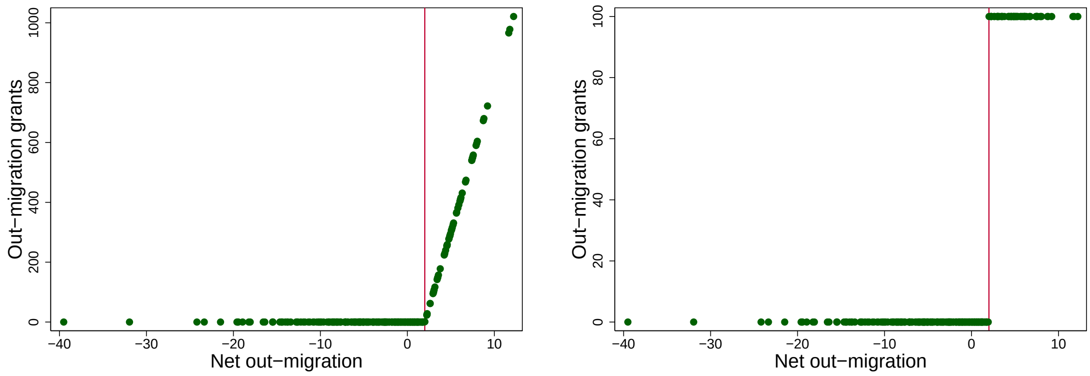
The assignment variable $m_{i,t}$ is the percentage decrease in the size of the population $n_{i,t}$ during a ten-year period with a two-year lag, i.e., $m_{i,t} = 100(1 - n_{i,t-2}/n_{i,t-12})$. Although the assignment variable partly reflects changes in mortality rate and birthrate, we will refer to it as (net) out-migration rate.

Figure 1 illustrates the assignment mechanism in (2) by plotting out-migration grants received by the municipalities, both the total amount and the marginal increase, against

[7] Note that the identification in both Wilson (2011) and Conley and Dupor (2011) partly builds on the fact that they know the formulas that are used when grants are assigned, but that they do not allow for these formulas to have any direct effect on employment.

the assignment variable. As seen from panel (a), there is a well-defined kink-point such that municipalities with net out-migration rates lower than 2% do not receive any out-migration grants, whereas municipalities with net out-migration rates above 2% do.[8] For municipalities above the kink-point, the marginal increase in per capita grants for each percentage point increase in out-migration is represented in equation (2) by the parameter a, which was constant and equal to 100 SEK (6.50 SEK≈1 USD) during the period studied. This is seen in panel (b) of the figure. Thus, as clearly illustrated by these graphs, there is a non-linear relationship between out-migration and grants, and a discontinuous relationship between out-migration *increases* and grants.

Figure 1: Out-migration grants against net out-migration



(a) Grants received (b) Marginal increase in grants

Note: Grants are measured in SEK per capita (6.50 SEK≈1 USD).
Source: Statistics Sweden & The Swedish Association of Local Authorities and Regions.

Card et al. (2009) derive necessary and sufficient conditions for a kinked assignment rule like the one in (2) to identify a local average treatment effect (LATE). For our application, these assumptions are (i) that the derivative of the density of the net out-migration rate is smooth—i.e., that the distribution is twice continuously differentiable—at 2%; and (ii) that the marginal effect of out-migration on personnel is smooth.[9] The first assumption rules out extreme sorting, or precise manipulation of the out-migration rate. This seems like an innocuous assumption considering that the out-migration rate is measured over a ten-year period, and that it is taken from official registers. The second assumption says that, although out-migration can have a direct effect on personnel, there can be no kink in this relationship. In other words, there can be no jump in the *marginal* effect of out-migration on personnel (like the one in panel (b) of Figure 1). The implication of this assumption is that there should not be any kinks in pre-determined covariates, which can be tested by checking if the baseline estimates are sensitive to the

[8] The total cost for this grant component is divided equally (per capita) between all municipalities, implying that it is neutral in terms of the federal budget.

[9] Note that these assumptions are somewhat stronger than in the regression discontinuity framework, where one only needs to assume smoothness in the level (and not in the marginal effect).

inclusion of such covariates.

In this paper, we adopt a fuzzy version of the RKD. Analogously to the RDD, the fuzzy version of RKD is appropriate when treatment is not entirely determined by distance to the kink-point. And although the assignment rule in (2) is entirely deterministic regarding the treatment of out-migration grants, the treatment of cost-equalizing grants might not be deterministic due to kink-points in other components of the cost equalization that—by coincidence—could be close to the kink-point at 2% out-migration.[10] Under this setting, it would in principle be possible to estimate a separate treatment effect at each kink. In practice, however, this is not viable for two reasons: First, the structure of the cost equalization and most of its components is very complex, so that an RKD treatment effect cannot be captured by a single parameter. Second, it would require the inclusion of flexible functions of all of the different assignment variables, but we lack data on some of these.

Our sole focus is instead on the component for out-migration grants and the assignment rule in (2), which we use in an IV estimation of the effect of an increase in cost-equalizing grants, $g_{i,t}$. As in the sharp RKD, the identifying assumption (i.e., the exclusion restriction) is still that of no kink in the direct effect of out-migration on personnel. We additionally need to assume that any direct effects of other variables subject to kinked assignment rules are captured by the direct effects of out-migration.

The first and the second stage in the two-stage least squares (2SLS) are given by the following two equations:[11]

$$g_{i,t} = \alpha_0 + \alpha_1(m_{i,t} - k)D + \sum_{p=1}^{\bar{p}} \phi_p (m_{i,t} - k)^p + T_t + \epsilon_{i,t} \tag{3}$$

$$y_{i,t} = \beta_0 + \beta_1 \hat{g}_{i,t} + \sum_{p=1}^{\bar{p}} \delta_p (m_{i,t} - k)^p + T_t + \varepsilon_{i,t}, \tag{4}$$

where $\hat{g}_{i,t}$ are predicted cost-equalizing grants obtained from estimating the first stage in (3), $k = 2$ is the kink-point, the interaction term D is an indicator for out-migration rates above the kink-point (i.e., $D = 1(m_{i,t} > k)$) and is the excluded instrument, and T_t are year fixed-effects. Thus, the excluded instrument captures the kinked relationship between out-migration and personnel stemming from increased out-migration grants. The direct effect of out-migration is represented by the term summing over order of polynomial p, with $\bar{p}$ being the highest order of polynomial included in the regression. Equations (3) and (4) can be altered by varying $\bar{p}$ as well as the bandwidth, h, that determines which observations are included (i.e., $[k - h, k + h]$). We will present results with different combinations of $\bar{p} = \{1, 2, 3\}$ and $h = \{5, 10, 15, \infty\}$.

[10] For a description of the cost equalization during the period under study, see Svenska Kommunförbundet (2003).

[11] Note that if the assignment rule in (2) were entirely deterministic regarding the treatment of cost-equalizing grants, the parameter α_1 in equation (3) would be identical to the parameter a in the assignment rule (2), and there would be no need for a two-stage procedure.

The β_1 parameter that can be identified in this setup is a weighted LATE, with weights proportional to the ex-ante probability of being close to the kink-point. Thanks to the non-linearity in the grant formula, we are thus able to mimic an experimental setting quite closely. In terms of Figure 1, after controlling for the smooth direct effects of out-migration on personnel, municipalities on opposite sides of the kink are similar in all respects except that some are eligible for the grant and others are not. A kink in the relation between out-migration and personnel can therefore be attributed to differences in the amount of grants received.

To end the section on identification, let us state the conditions under which we can recover a causal parameter with this approach, and how these conditions can be checked: (i) The instrument needs to be relevant. This can simply be checked with the statistical significance of the estimate of α_1 in the first-stage regression. (ii) The exclusion restriction of no kink in the direct effect needs to hold. An implication of this is that there should not be any kinks in pre-determined covariates. (iii) The control function of out-migration needs to capture the direct effects of any other variables subject to kinked assignment rules. The validity of condition (ii) can be examined by checking the sensitivity of the estimated treatment effect to the inclusion of pre-determined covariates. To the extent that these covariates are correlated with other variables subject to a kinked assignment rule, this approach also examines the validity of condition (iii). We return to this in Section 4.4.

3 *Institutional background*

This section first describes the role of Swedish local governments in general and the role of intergovernmental grants in particular. The section ends with a description of the data used in the paper.

3.1 *Fiscal federalism in Sweden*

Decentralized governments in Sweden are among the largest in the world, with a comprehensive range of responsibilities, including primary and secondary education, child care and care for the elderly. The production of these services is very labor-intensive and roughly 20% of the entire work force is employed by the municipalities, making them the largest employer in the country. As a consequence, costs for personnel constitute around half of all municipal expenditures. The most important source of municipal revenue is a proportional local income tax, which constitutes 60-70% of total revenues. The rest consists of user fees and grants. Because equalization is a major feature of the grant system, the importance of grants as a source of revenue varies substantially across jurisdictions. The average share is just above 15%, but there are also three municipalities in the Stockholm region that were actually net contributors to the grant system during the entire 1996–2004 period.

Swedish municipalities have a long standing tradition of high autonomy both with respect to the size and the composition of their spending. For example, they are free to set their own tax rate and are able to borrow funds. The local autonomy of municipalities was further strengthened in 1993 when a major grant reform replaced a system of targeted central government grants to all municipal services (education, child and elderly care, social services and infrastructure) with general grants—that is, the majority of grants were no longer earmarked. After some early changes, the new system officially came into place in 1996 and consisted of a per capita grant, income-equalizing grants and cost-equalizing grants, all distributed as general grants with no strings attached. The purpose of the cost equalization was (and still is) to reduce differences in effective costs due to unequal structural conditions across municipalities, whereas the income equalization guarantees per capita tax revenues of a fixed percentage of the national average.[12] Note that the same grant system prevailed during the entire period studied here.

3.2 Data

The data in the paper consists of a panel of 279 Swedish municipalities observed over the time period 1996–2004.[13] As described in Section 2, the grant formula that is used for identification is an element of the cost-equalizing grants designed to compensate local governments for additional costs due to sizeable out-migration. During 1996–2004, the average out-migration grant as a fraction of total cost-equalizing grants for eligible municipalities amounted to around 18%, whereas the cost-equalizing grants for eligible municipalities amounted to around 20% of total grants. Over the period studied, 112 municipalities never received any out-migration grants (as they never had an out-migration rate below 2%), 55 municipalities received grants all nine years and the remaining 112 municipalities received grants some, but not all, years. As can be seen from the map in Figure 2, receiving municipalities can be found all over the country but are concentrated in the north.

Table 1 displays summary statistics for the two grant variables (cost-equalizing grants and out-migration grants, both measured in SEK per capita), the outcome variables (total personnel and personnel in administration, child care, schools, elderly care, social welfare and technical services,[14] all measured in full-time equivalents per 1,000 capita) and the assignment variable (net out-migration rate). The large standard deviations relative to the means and the negative minimum values of cost-equalizing and out-migration grants seen in Table 1 reflect the fact that these parts of the grant system are self-financed.

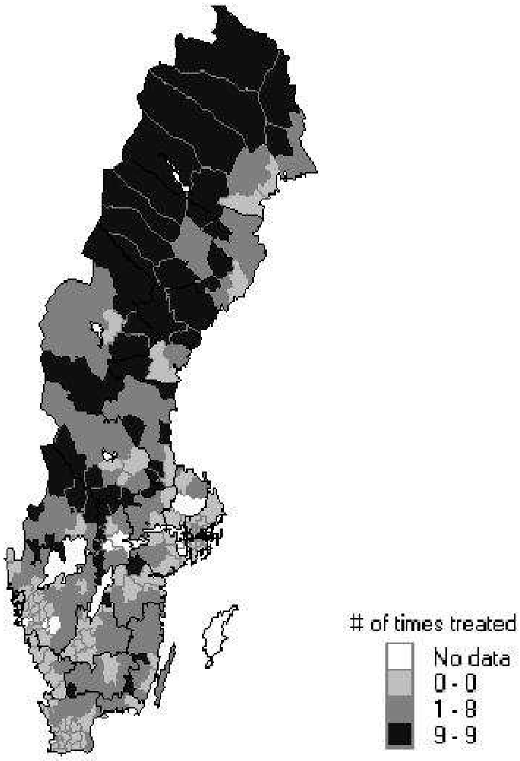
As for personnel, most people are employed in elderly care and schools—21 and 17 full-time equivalents per 1,000 inhabitants, respectively—followed by personnel in child

[12] Both of the equalizing grants were self-financed by equal per capita contributions from all municipalities.

[13] Data covers all municipalities except for eight that were affected by consolidations (Bollebygd, Borås, Lekeberg, Örebro, Nykvarn, Södertälje, Knivsta and Uppsala) and three that have responsibilities that the other municipalities do not have (Gotland, Malmö and Göteborg).

[14] The way in which total personnel is disaggregated into the various sectors is in accordance with The Swedish Association of Local Authorities and Regions. According to this categorization, administrative personnel include high officials, heads of local public authorities as well as administrative assistants not working in a particular sector.

Figure 2: Distribution of out-migration grants over the period 1996–2004



Source: Statistics Sweden.

Table 1: Summary statistics for main variables

	mean	std.dev	min	max	obs
Personnel, total	65.0	9.88	30.9	101.8	2511
Personnel, administration	5.48	1.13	2.19	12.9	2511
Personnel, child care	10.5	1.72	3.33	17.9	2511
Personnel, schools	16.7	2.71	8.45	32.1	2511
Personnel, elderly care	21.2	6.66	1.48	41.1	2511
Personnel, social welfare	1.78	0.71	0.084	6.82	2511
Personnel, technical services	9.31	2.42	1.63	17.8	2511
Cost-equalizing grants	532.1	2452.2	-3471	13196	2511
Out-migration grants	111.7	286.8	-125.5	1383.6	2511
Net out-migration	-0.79	7.92	-43.0	16.6	2511
Population	27229.4	48883.9	2575	761721	2511
Population aged 0–6	7.92	1.31	4.71	12.8	2511
Population aged 7–15	12.2	1.19	6.78	16.4	2511
Population aged 80+	5.41	1.38	1.25	9.14	2511
Foreign born	4.01	2.73	0.56	29.1	2511

Note: Grants are measured in SEK per capita (6.50 SEK≈1 USD) and personnel are measured in full-time equivalents per 1,000 capita. The variables net out-migration, population aged 0–6, population aged 7–15, population aged 80+ and foreign born are given in percent.

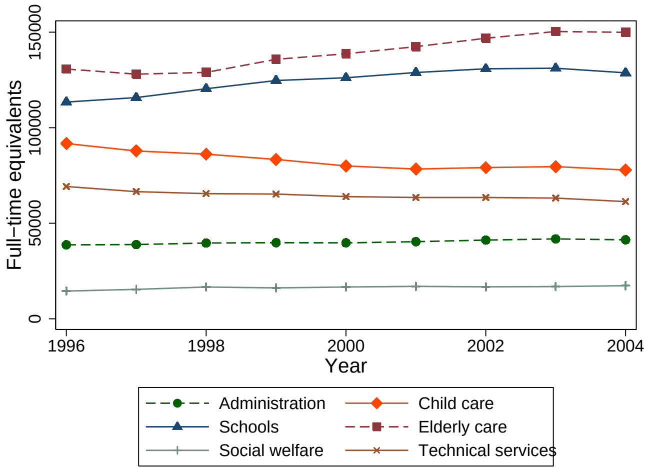
Source: Statistics Sweden & The Swedish Association of Local Authorities and Regions.

care and technical services, with around 10 full-time equivalents per 1,000 inhabitants. The national aggregates of these variables are illustrated in Figure 3, showing the sector-wise evolution of the total number of full-time equivalents employed by a municipality. As seen from the figure, employment in elderly care has increased quite substantially (due to the aging population), as has employment in schools, whereas fewer are employed in child care.[15] Employment in the remaining sectors has been fairly stable and, all in all, the number of full-time equivalents increased from around 460,000 in 1996 to 475,000 in 2004. This slight increase runs parallel with a positive privatization and outsourcing trend taking shape during the 1990s. For example, of everyone working in the child care sector, the share employed by a municipality was 94% in 1995 and 90% in 1999 (Statistics Sweden, 2001). Although the public share of employment has experienced similar decreases also in other sectors, as of 2004 the vast majority working in areas traditionally dominated by public providers were still employed by a municipality.

Table 1 also presents the socio-economic variables that will be used to examine the sensitivity of the baseline estimates to the inclusion of time-varying covariates (see Section 4.4): population size, share of the population aged 0–6, share of the population aged 7–15, share of the population aged 80 years or older and share of foreign-born citizens. These variables show large variations across municipalities, as does the amount of migration (which is, of course, the underlying reason why there is a need for equalization).

[15] Part of the employment decrease in child care and the increase in schools is explained by a transfer of pre-schools for 6-year olds from the child care sector to the school sector.

Figure 3: Aggregate public employment in different municipal sectors; 1996–2004



Source: The Swedish Association of Local Authorities and Regions.

4 *Effects of grants on local government personnel*

In this section, we present two-stage least squares (2SLS) estimates of the model given in equations (3) and (4) to examine what are the effects of increased grants on different types of local government personnel. In order to test whether the instrument is relevant (i.e., whether the kinked rule assigning out-migration grants explains the variation in cost-equalizing grants), we first present the results from the first stage. Thereafter, we turn to the estimates of the causal effects from the second stage. We will also conduct an analysis of the identifying assumptions. But before turning to the econometrics, we begin with a graphical analysis.

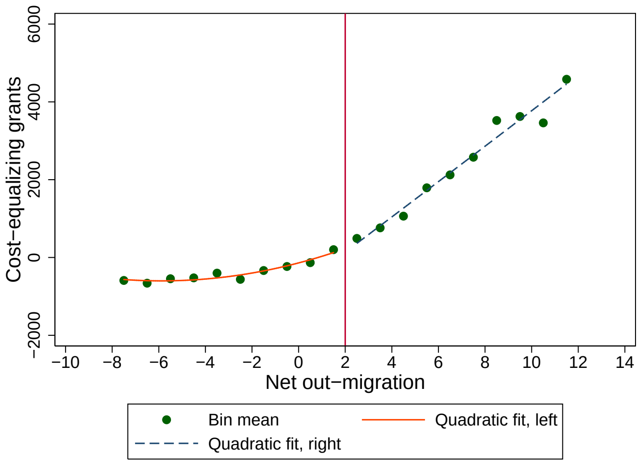
4.1 *Graphical analysis*

The nature of the RKD makes visual inspections particularly attractive—if there is an effect, this is seen as a kink in the outcome corresponding to the kink in the assignment rule. To this aim, it is customary to plot means of the outcome within a specified bin width of the assignment variable, along with fitted polynomials on each side of the kink-point. Focusing on out-migration rates ±10 from the kink-point, Figure 4 first does this for cost-equalizing grants (the treatment variable of interest), where we have chosen a quadratic fit and a bin size of 1 percentage point.[16] The figure clearly reveals the

[16] Lee and Lemieux (2010) suggest two formal tests for choosing bin size in graphical regression discontinuity analyses and, with a slight modification, these tests can also be applied to the RKD. The first test is an F-test of a model with C separate slope coefficients (intercepts for RDD) against a model with $2C$ separate slope coefficients. If the test is not rejected, C bins are enough. The second test is an F-test of a model with C separate slope coefficients (intercepts for RDD) against a model with C separate slopes *and C separate coefficients on quadratic terms* (linear for RDD). Again, if the test is not

same kink in the relationship between out-migration and cost-equalizing grants as that between out-migration and out-migration grants (cf. Figure 1). This is indicative of a strong first stage in our two-stage procedure.

Figure 4: Cost-equalizing grants against net out-migration



Note: Grants are measured in SEK per capita (6.50 SEK≈1 USD).

Source: The Swedish Association of Local Authorities and Regions.

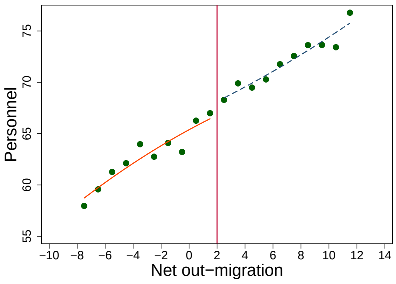
Figure 5 displays equivalent graphs, but for total personnel and for personnel within each of the different sectors (the outcome variables of interest). For administrative personnel, there is a distinct kink around 2% out-migration, whereas there are no such kinks visible neither for total personnel nor for any of the other personnel categories. Thus, the graphics are highly suggestive of a positive effect of grants only on administrative personnel. The econometric analysis to follow will show how these graphical results correspond to statistical estimates.

4.2 First-stage estimates

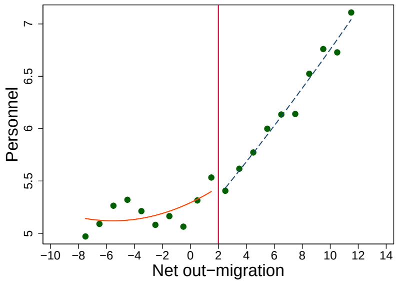
Although it is hard to detect any kinked relations between out-migration and personnel in most of the sectors in Figure 5, out-migration appears to have quite strong direct effects in some of the sectors. In the econometrics, we control for these direct effects by including a flexible function of out-migration in the regression. One might worry that when doing this, there will be no explanatory power left in the instrument, which would thereby fail to be relevant. Whether or not this is the case will be clear from the first-stage regressions—or, specifically, from the statistical significance of the estimate of the *incremental* effect of out-migration on total cost-equalizing grants, $g_{i,t}$, at the kink-point. This estimate corresponds to α_1 in the first-stage equation (3). If $\hat{\alpha}_1$ is statistically

rejected, C bins are enough. Performing these tests for all our outcome variables, 20 bins (implying a bin size of 1) are never rejected at the 10% level.

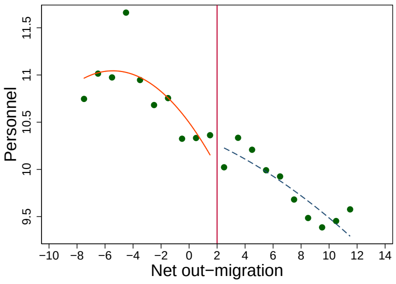
Figure 5: Municipal personnel against net out-migration



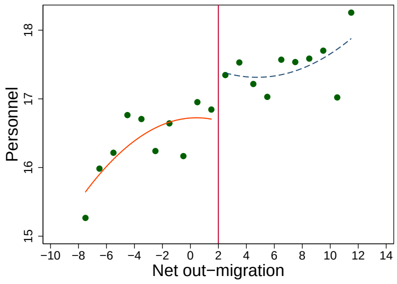
(a) Total



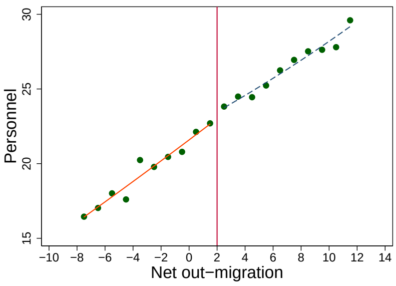
(b) Administration



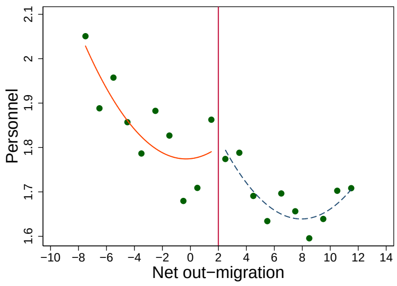
(c) Child care



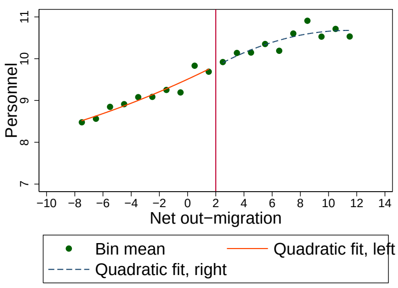
(d) Schools



(e) Elderly care



(f) Social welfare



(g) Technical services

Note: Personnel is measured in full-time equivalents per 1,000 capita.

Source: The Swedish Association of Local Authorities and Regions.

Table 2: First-stage estimates

	Full sample	$h = 15$	$h = 10$	$h = 5$
$\bar{p} = 1$	417.4***	363.6***	398.8***	198.0**
	(68.42)	(73.94)	(74.44)	(96.13)
$\bar{p} = 2$	317.6***			
	(76.09)			
$\bar{p} = 3$	335.0***			
	(103.6)			
Observations	2511	2346	2047	1241

Note: For different bandwidths, h, and order of polynomials, $\bar{p}$, the table reports estimates of α_1 in the first-stage equation (3) on cost-equalizing grants. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

significant, we know that after controlling for the direct effects of the smooth function of net out-migration, the out-migration grant still has an impact on total cost-equalizing grants, implying that the instrument is relevant.

Because we do not know the form of the direct effects, we run regressions controlling for out-migration linearly as well as with a 2^{nd} and 3^{rd} order polynomial. Table 2 shows first-stage estimates and associated standard errors of α_1 in equation (3). The standard errors are robust to arbitrary residual heteroskedasticity and serial correlation within municipality.[17] Each row corresponds to an estimation of equation (3) with different order of polynomial $\bar{p} = \{1, 2, 3\}$, while each column corresponds to an estimation where either all observations are included, or the estimation sample is restricted to observations $\pm 15, 10$ and 5 percentage points from the kink-point. Note that with the two narrowest bandwidths, due to the sample reduction, we only estimate with $\bar{p} = 1$.

It is clear from the table that all estimates are highly statistically significant, irrespective of order of polynomial and bandwidth. The magnitude of the estimates is around 300, although that differs somewhat across the different specifications. Note that owing to other kinks in the cost equalization close to the kink in the out-migration component, the parameter is estimated to differ from 100, which would be the case if treatment of cost-equalizing grants were a fully deterministic function of the assignment rule in (2). As explained above, this is the rationale for adopting a fuzzy version of the RKD, and poses no identification problem as long as any direct effects of assignment variables subject to other kinks are captured by the included control function. The validity of this claim will be investigated in Section 4.4, but for now, we conclude from Table 2 that the instrument works well in the sense that it is relevant, and turn to the two-stage least square estimates of the causal effect of grants on municipal personnel.

[17] Rigidities in hiring and firing suggest that there is a time-lag in employment, which is also shown to be the case in Bergström et al. (2004). This time-lag emphasizes the importance of adjusting the standard errors accordingly—something we do by using the "cluster" command in STATA.

4.3 *Two-stage least squares estimates*

Table 3 presents the 2SLS estimates of β_1 in equation (4) for total local government personnel and for personnel disaggregated by the six sectors (administration, child care, schools, elderly care, social welfare and technical services). As above, each column corresponds to an estimation with different bandwidths, while the three rows per outcome variable correspond to an estimation with different polynomials. For bandwidths where we vary the order of polynomial, the order preferred according to the Akaike information criterion (AIC) is in bold. All results are evaluated at full-time equivalents per 1,000 capita for the dependent variable and 100 SEK per capita for the grants variable (which is the increase in grants associated with a 1 percentage point increase in net out-migration from 2%).

To start with the results for total personnel, it is clear from the first three rows of Table 3 that there is, in fact, no overall effect of grants.[18] Not only are the estimated coefficients statistically insignificant, but in some specifications the sign is even negative. In fact, the disaggregated effects on the various sectors in subsequent rows show that insignificant, negative estimates seem to be a rather consistent pattern. The only positive, statistically significant effect is the one on administrative personnel, for which the estimates are around 0.03–0.04. This point estimate is fairly robust to different bandwidths and order of polynomials, although for the smallest bandwidth the standard error increases to the extent that the estimate is no longer statistically significant. For the other personnel categories on which we find no statistically significant effects, the estimates are somewhat more sensitive to the different specifications, although much less so if only focusing on the specifications preferred according to the AIC.

Concerning the size of the effect on the administration, we consider it to be of economic significance as well—a 100 SEK increase in per capita grants leads to an increase of 0.03–0.04 full-time equivalents per 1,000 capita, which is around 0.5% of the mean for this personnel category. Given that the mean of the absolute value of the cost-equalizing grant is around 1,500—implying that 100 SEK constitute a 6–7% increase—this is a substantial effect.[19]

4.4 *Testing the identifying assumptions*

The validity of the above results rests on the identifying assumptions, and in this section, we first test the assumption of smooth density of out-migration. Then, we investigate the assumptions that the direct effects of out-migration are smooth and that the control function captures any direct effects of other variables subject to kinked assignment rules. This is done by investigating whether the baseline estimates are sensitive to the inclusion of a set of control variables.

[18] This result is well in line with the results in Bergström et al. (2004).

[19] Because of the equalizing feature of the grants, more than half of the observations on cost-equalizing grants are negative. Such negative values imply that evaluating the size of a grant increase in the context of the mean value from Table 1 would be misleading.

Table 3: Effects of grants on municipal personnel (2SLS estimates)

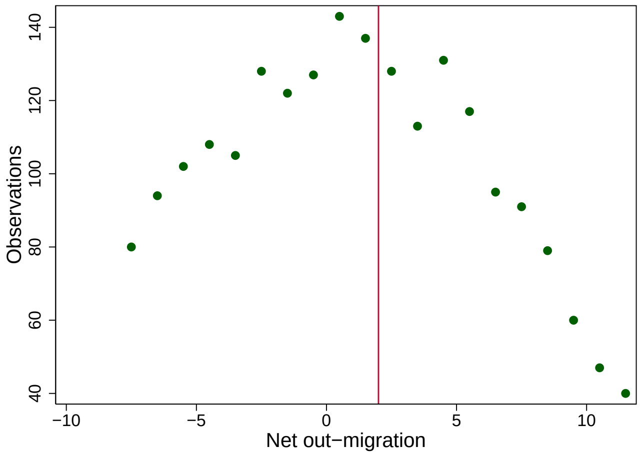
		Full sample	$h = 15$	$h = 10$	$h = 5$
Total personnel	$\bar{p} = 1$	**0.0521**	-0.0593	-0.0152	-0.218
		(0.0478)	(0.0697)	(0.0569)	(0.211)
	$\bar{p} = 2$	-0.164	0.000877		
		(0.106)	(0.133)		
	$\bar{p} = 3$	-0.108	**0.0389**		
		(0.137)	**(0.106)**		
Administrative personnel	$\bar{p} = 1$	0.0300***	**0.0298*****	0.0375***	0.0358
		(0.00568)	**(0.00750)**	(0.00735)	(0.0223)
	$\bar{p} = 2$	**0.0289*****	0.0499***		
		(0.00929)	(0.0183)		
	$\bar{p} = 3$	0.0414***	0.0449***		
		(0.0144)	(0.0163)		
Child care personnel	$\bar{p} = 1$	0.00353	**-0.00417**	-0.000811	0.0127
		(0.00669)	**(0.00881)**	(0.0107)	(0.0360)
	$\bar{p} = 2$	**-0.00721**	0.0341		
		(0.0129)	(0.0354)		
	$\bar{p} = 3$	-0.00593	0.0177		
		(0.0246)	(0.0228)		
School personnel	$\bar{p} = 1$	**-0.0134**	**-0.0327**	-0.0306	-0.0965
		(0.0140)	**(0.0206)**	(0.0200)	(0.0806)
	$\bar{p} = 2$	-0.0402	-0.0542		
		(0.0285)	(0.0477)		
	$\bar{p} = 3$	-0.0602	-0.0339		
		(0.0444)	(0.0357)		
Elderly care personnel	$\bar{p} = 1$	**0.0425**	**-0.0264**	-0.0218	-0.119
		(0.0287)	**(0.0410)**	(0.0356)	(0.128)
	$\bar{p} = 2$	-0.105	-0.0628		
		(0.0642)	(0.0905)		
	$\bar{p} = 3$	-0.110	-0.0318		
		(0.0886)	(0.0771)		
Social welfare personnel	$\bar{p} = 1$	**-0.00730****	-0.00116	0.000415	-0.00659
		(0.00328)	(0.00465)	(0.00516)	(0.0160)
	$\bar{p} = 2$	0.00518	0.00369		
		(0.00700)	(0.0126)		
	$\bar{p} = 3$	0.00720	**-0.00259**		
		(0.0114)	**(0.00983)**		
Technical personnel	$\bar{p} = 1$	-0.00333	-0.0247	0.0000700	-0.0485
		(0.0157)	(0.0217)	(0.0170)	(0.0544)
	$\bar{p} = 2$	-0.0462	**0.0293**		
		(0.0306)	**(0.0389)**		
	$\bar{p} = 3$	**0.0188**	0.0435		
		(0.0357)	(0.0302)		
Observations		2511	2346	2047	1241

Note: For different bandwidths, h, and order of polynomials, $\bar{p}$, the table reports estimates of β_1 in the second-stage equation (4), with the dependent variables total personnel as well as personnel disaggregated by the different sectors. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Smooth density of the assignment variables

As shown by Card et al. (2009), one of the main identifying assumptions in the RKD is that the derivative of the density of the assignment variable is smooth around the kink-point. This implies that extreme sorting needs to be ruled out, something which is best done graphically as in Figure 6. This figure plots the number of observations within each 1-percentage point bin of net-outmigration in the range ±10 from the kink-point. Although the density of observations does not evolve smoothly within the entire range, it is comforting to see that there is no dramatic increase in the density at the kink-point (marked by the vertical line).[20] Given the type of assignment variable in our application, this might not come as a surprise: it seems quite difficult for municipalities to perfectly manipulate their rate of out-migration during a ten-year period. Interestingly, Figure 6 also shows that there is a large number of municipalities around the kink-point, implying that the weighted LATE estimate that we identify in fact applies to a quite large number of municipalities.

Figure 6: Density of net out-migration



Source: The Swedish Association of Local Authorities and Regions.

Controlling for pre-determined covariates

The second main identifying assumption derived by Card et al. (2009) is that the marginal effect of the assignment variable on the outcome is smooth. In terms of our application, we thus require that there is no kink in the marginal effect of out-migration on personnel. The implication of this assumption is that there should not be

[20] The econometric complement to the graphical validation of this identifying assumption is to run the regression in equation (3), with the number of observations within each bin of a specified size as the dependent variable, and test whether α_1 differs from zero. Doing this we do not find any evidence of bunching on the right-hand side of the kink-point.

any kinks in pre-determined covariates: if the direct effects of out-migration on personnel are smooth, the effect on pre-determined covariates should also be smooth. This means that this identifying assumption can be tested by adding pre-determined covariates to the baseline specification and examine how the estimates react. To the extent that these pre-determined covariates are correlated with assignment variables other than out-migration that are also subject to kinked assignment rules, this is also a test of the assumption that the direct effects of such other assignment variables are captured by the control function of out-migration.

A prerequisite for this test to work is that the chosen variables are pre-determined—i.e., they cannot be the result of the treatment. In a local government setting, this may be easier said than done since, to some extent, most things are interdependent both across time and space. Motivated by such concerns, our choice of covariates used to test the identifying assumption is the following: total population, share of the population aged 0–6, share of the population aged 7–15, share of the population aged 80+ and share of foreign-born citizens.

The estimates of β_1 in equation (4) obtained when these variables are included are provided in Table 4.[21] To economize on space, the table only includes the estimate with the order of polynomial preferred by the AIC in the baseline specification (cf. Table 3). If the identifying assumptions are valid, the only thing that should change from the baseline results is—if anything—that the precision of the estimates is improved. Comparing Table 4 to the estimates in Table 3 (and particularly those in bold), it is clear that the baseline results are not affected to any considerable extent by the inclusion of the additional covariates, neither in terms of point estimates nor significance levels (although the standard errors do indeed decrease somewhat). We take this as evidence that the assumption of smooth marginal effects of out-migration on personnel is valid, and that the control function of out-migration captures all other relevant direct effects. This, in turn, means that our econometric specification is likely to estimate a causal effect of grants. Thus, the main finding from Table 3 that grants only have an effect on personnel in the administration remains.[22]

5 *Do municipalities employ administrative assistants or higher officials?*

A robust result found in the former section is hence that, following a grant increase, more administrative personnel are employed. In this section we will examine whether municipalities employ administrative assistants or higher officials.

The pool of bureaucratic personnel in the administration, as we have defined it so far, is fairly broad—it includes everyone from frontline employees performing basic

[21] Because the first-stage estimates obtained when including these variables are very similar to those in the baseline specification, we do not report them. The results are, of course, available upon request.

[22] A potential alternative robustness check is to not only include pre-determined time-varying covariates, but also municipality fixed-effects. Doing this, the main results still hold but are less stable across the different bandwidths and order of polynomial (the results are available upon request). This is most likely due to lack of sufficient within-municipality variation in out-migration rates for stable estimates across specifications.

Table 4: Effects of grants on municipal personnel when controlling for pre-determined covariates (2SLS estimates)

	Full sample	$h = 15$	$h = 10$	$h = 5$
Total personnel	0.0509	0.0523	-0.0233	-0.187
	(0.0395)	(0.0876)	(0.0463)	(0.134)
Administrative personnel	0.0239***	0.0251***	0.0310***	0.0283**
	(0.00725)	(0.00620)	(0.00619)	(0.0139)
Child care personnel	-0.0212**	-0.00847	-0.00453	-0.0102
	(0.0104)	(0.00770)	(0.00914)	(0.0221)
School personnel	-0.0107	-0.0270	-0.0327*	-0.0829
	(0.0130)	(0.0169)	(0.0177)	(0.0523)
Elderly care personnel	0.0425**	0.000852	-0.0112	-0.0654
	(0.0198)	(0.0253)	(0.0251)	(0.0670)
Social welfare personnel	-0.00762***	-0.00621	-0.00535	-0.0146
	(0.00295)	(0.00872)	(0.00443)	(0.0112)
Technical personnel	0.0274	0.0328	-0.000535	-0.0439
	(0.0294)	(0.0311)	(0.0145)	(0.0374)
Observations	2511	2346	2047	1241

Note: For different bandwidths, h, the table reports estimates of β_1 in the second-stage equation (4), with the dependent variables total personnel as well as personnel disaggregated by the different sectors, when controlling for the following pre-determined covariates: total population, share of the population aged 0–6, share of the population aged 7–15, share of the population aged 80+ and share of foreign-born citizens. For all personnel categories, the regressions are estimated with the AIC-preferred polynomial in the respective baseline specification (cf. Table 3). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 5: Effects of grants on different types of bureaucrats (2SLS estimates)

	Full sample	$h = 15$	$h = 10$	$h = 5$
Administrative assistants	0.0189***	0.0184***	0.0225***	0.0313*
	(0.00321)	(0.00409)	(0.00492)	(0.0182)
High administrative officials	0.00228	0.00755	-0.000381	0.00196
	(0.00292)	(0.00868)	(0.00419)	(0.0137)
Observations	2511	2346	2047	1241

Note: For different bandwidths, h, the table reports estimates of β_1 in the second-stage equation (4), with the dependent variables administrative assistant personnel (containing 35 missing values) and high administrative officials. For both types of administrative personnel, the regressions are estimated with the respective AIC-preferred polynomial among $\bar{p} = \{1, 2, 3\}$. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

administrative assistant services to high officials and heads of local public authorities. With access to detailed register data on all individuals employed by the municipalities, we are able to refine our analysis by studying different types of bureaucrats separately, which may help us understand the mechanism behind our results.

Once again, equation (4) is estimated with different bandwidths and polynomials, with dependent variables now being administrative assistant personnel and high administrative officials, respectively.[23] The resulting β_1 estimates are shown in Table 5 (for the full sample and for $h = 15$, the regressions are estimated with the AIC-preferred polynomial among $\bar{p} = \{1, 2, 3\}$, and for $h = 10$ and $h = 5$ with $\bar{p} = 1$)[24]. These results clearly show that increases in employment take place among the administrative assistants, for which the estimated effect is around 0.02, meaning that a per capita grant increase of 100 SEK leads to an employment increase of around 0.02 full-time equivalents per 1,000 capita. This increase corresponds to an addition of 1–1.5% to this personnel category. The effect on high administrative officials, however, appears to be very similar to the other sectors analyzed above—i.e., not significantly different from zero.

6 Concluding remarks

Public employment—and local public employment in particular—plays an important role in most countries. One of the main instruments with which central decision-makers can affect local public employment is grants to lower-level governments. In this paper, by applying the regression kink design to the Swedish grant system, we estimate causal effects of intergovernmental grants on personnel in different local government sectors.

[23] High administrative officials are defined as top- and mid-managers of local public institutions and authorities. Administrative assistants are either secretaries and clerks, or administrators handling more qualified tasks and investigations.

[24] Of the four sets of three regressions with $\bar{p} = \{1, 2, 3\}$ underlying Table 5, $\bar{p} = 1$ is preferred in three regressions and $\bar{p} = 3$ is preferred in one regression.

We examine the validity of the identifying assumptions in a variety of ways and verify that the exclusion restriction of there being no kink in the direct relation between out-migration (the assignment variable) and personnel (the outcome) indeed seems to hold. Therefore, we can be quite confident that the findings can be given a causal interpretation.

Our robust conclusion is that employment of bureaucratic personnel in the administration are increased after a marginal increase in grants but that such an effect is lacking both for total personnel and personnel in any of the other five sectors (child care, schools, elderly care, social welfare and technical services). Furthermore, when we estimate the effects on administrative personnel separately for those with basic administrative assistance duties and high officials, we find that the effect comes from the former group.

These asymmetric results raise the question of what distinguishes administrative personnel from personnel in the other sectors. In line with the hypothesis and results in Dahlberg and Mörk (2006), one possibility is that bureaucrats are able to influence the local decision-making process in ways that other types of personnel cannot. Such bureaucratic power has long been recognized by economists.[25] But why would bureaucrats wish to employ more fellow bureaucrats and, specifically, more administrative assistants? We can think of a couple of reasons for this. First, a large number of assistants will be able to cover a variety of tasks that would otherwise be assigned to someone further up the ladder. Hence, by employing more assistants, high officials can reduce their own workload. Second, having a larger number of assisting personnel increases the number of subordinates, which could give higher officials a sense of increased power.

On the other hand, it is also possible that the explanation for the asymmetric effects on employment is less "cynical". Even though we control for the direct effects of out-migration on personnel, the fact remains that our estimated effects represent local average treatment effects for municipalities with diminishing population. It might be too risky for such municipalities to use increased grants to employ more personnel in child care and schools for example. Labor demand in these sectors is likely to be more sensitive to demographic changes than in the administrative sector, with the implication that risk-averse decision-makers (be they politicians, bureaucrats, or both) are reluctant to hire any personnel at all in the sectors where demand is more volatile and uncertain. Given the widespread union power in Swedish municipalities and the constraints on firing personnel, this explanation seems likely. Such heterogeneous treatment effects would mean that the overall average effect of grant increases on personnel may be larger than the local average effect identified here, which is good news for current and future policies aiming at increasing local public employment by means of grant increases.

[25] Early contributions discussing the role of bureaucrats are Tullock (1965), Downs (1957), Niskanen (1971) and Romer and Rosenthal (1979). Later contributions include Moene (1986) who shows that deviations from the socially optimally bureau are likely. More recent authors argue that bureaucrats are driven by career concerns; see, e.g., Dewatripont et al. (1999) and Alesina and Tabellini (2007). The fact that bureaucrats are of importance for the political decision-making process as well as in the implementation phase has also been recognized for a long time in the political science literature; see, e.g., Peters (1995), Wilson (1989) and Lipsky (1980).

A third, and even more optimistic, possible explanation for the results is that employing more administrative personnel actually improves efficiency. Such improvement would be possible if, in the absence of a grant increase, other personnel are occupied with administrative duties for which they are overqualified due to a lack of enough resources to hire administrative assistants.

Finally, it could be the case that increased grants are systematically associated with the need for an enlarged administration to handle the distribution of these grants. However, we believe this explanation to be quite unlikely, given that there is no application procedure and the grants are transferred with no strings attached.

The answer to the question posed in the title of the paper is hence rather negative: giving general grants to lower-level governments does not seem to be an effective way of stimulating local public employment. It is, of course, important to consider how much these results can be generalized. We specifically want to stress two things concerning the setting in the paper: First, the years studied constitute a quite prosperous time period, and it is possible that effects of grants are larger in times of economic recessions, such as the current one. Second, the type of grants studied is distributed with no strings attached, meaning that they can be spent freely. Public funds *targeted* at stimulating employment might have larger effects.

Finally, our results suggest interesting avenues for future research within the field of local labor demand. Many countries have seen a privatization trend of welfare services since the early 1990s. This trend along with our result of limited effects of grants on public employment call for future research looking into the effects of grants on employment in private firms producing publicly financed welfare services, such as charter schools and private health and child care facilities.

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