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*Heléne Lundqvist*

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## Abstract

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 to specific type of recipients. The results from these studies are mixed and make clear that knowledge about grants effects is to this date still insufficient. This paper contributes by estimating causal effects on local expenditures and income tax rates of general, non-targeted grants to Finnish municipalities. This is done in a difference-in-difference model utilizing policy-induced increases in grants to a group of remotely populated municipalities. The robust finding is that increased grants have a negligible effect on local income tax rates, but that there is an immediate one-to-one correspondence between grants and local expenditures. Furthermore, expenditures continue to increase also some time after the grant increase, although this response is estimated less precisely. The flypaper behavior displayed by the treatment group can potentially be explained by "sepa-rate mental accounting'" – i.e., voters treating the government budget constraint separately from their own.

Key words: Intergovernmental grants, difference-in-difference model,
fly-paper effect

JEL classification numbers: C23, H71, H72, H77, R51

# Summary

Most fiscally decentralized economies rely heavily on transfers from upper- to lower-level governments as well as equalizing transfers between lower-level governments. Knowledge about how and to what extent these intergovernmental grants are spent is therefore crucial for designing public policy that relates 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.

When conducting empirical research on effects of intergovernmental grants it is important to give proper attention to the likely endogeneity problems in grants. In other words, to provide credible results one needs 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 the outcome of interest. The problem is particularly evident for the case of expenditures: jurisdictions with characteristics associated with high expenditures such as, e.g., a large share of elderly, typically receive more grants exactly because they need to be spending more. It is therefore 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, typically such an approach would kill all of the variation in grants. A more promising remedy is therefore to closely study how grants are determined in search for experimental-type features where the amount of grants vary but the underlying needs do not.

And although the literature on how intergovernmental grants affect the budget of receiving jurisdictions is numerous, very few studies that explicitly deal with these endogeneity problems focus on grants targeted towards specific sectors or to specific type of recipients. The results from these studies are mixed and make clear that knowledge about grants effects is to this date still insufficient. This paper contributes by estimating causal effects on local expenditures and income tax rates of general, non-targeted grants to Finnish municipalities. This is done in a difference-in-difference model utilizing policy-induced increases in a supplemental grant distributed to a group of remotely populated municipalities. In other words, I compare the change in expenditures and tax rates in a treatment group of municipalities that received increased supplemental grants to the change in expenditures and tax rates in a control group of municipalities that never received the supplemental grant. Assuming that the treatment group and the control group had otherwise evolved identically, this comparison yields a correct estimate of the effects of a grant increase.

The policy-induced variation that enables circumventing the endogeneity problem increased a particular grant supplementary to remotely populated municipalities, so the claim that effects of general grants are evaluated demands its justification. The Finnish grant system is made up of several types of grants of which the particular supplement in

question is a rather small part. But during the period relevant to here, 1997–2005, the grant system was structured so that all grants were distributed to the municipalities as a general sum with no strings attached. This means that as far as the receiving municipalities are concerned, increases in the particular supplement is exactly equivalent to increases in any of the other broader grant categories that together comprise the general sum.

The robust finding of the paper – in line with the results of a related study on Swedish municipalities by Dahlberg et al. (2008) – is that increased grants have a negligible effect on local income tax rates, but that there is an immediate one-to-one correspondence between grants and local expenditures. Furthermore, expenditures continue to increase also some time after the grant increase, although this response is estimated less precisely. The common effects of general grants to Finnish municipalities as found here and to Swedish municipalities as found in Dahlberg et al. suggest that these results may be externally valid at least to other federations characterized by comprehensive local independence.

The focus in this paper is to give a convincing answer to *how* local governments respond to increases in grants. In concluding I do, however, also discuss my ideas of *why* these municipalities exclusively use grant increases to increase spending and not to cut taxes. My explanation is related to a mechanism labeled "separate mental accounting" – i.e. that voters treat the government budget constraint separately from their own – which is something I believe to be at work both on behalf of the local governments and of the state. The apparent reluctance to use grants to finance local tax cuts as well as the intention of the state to distribute grants in order to first and foremost finance expenditures is indicative of such behavior. Although the latter point is not explicitly stated, it is implicit from the labeling of grants as "grants to social services and health care" and "grants to education and culture" despite that all grants are in fact non-targeted. These labels are also likely to further encourage increased local spending if the municipalities fear that by instead responding with tax cuts they may disqualify for 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 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. Yet, the policy recommendation that emerges from this study is that federal governments who wish to increase disposable income should do so directly by lowering federal tax rates rather than rely on local governments to use increased grants to finance tax cuts, and federal governments that wish to induce increased local spending by distributing general grants can indeed succeed in doing so.

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# 1 Introduction

Most fiscally decentralized economies rely heavily on transfers from upper- to lower-level governments as well as equalizing transfers between lower-level governments. Knowledge about how and to what extent these intergovernmental grants are spent is therefore crucial for designing public policy that relates 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 can not hope 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 often instead goes through careful evaluation of many different policies that are more narrowly targeted. However, while the literature on effects of intergovernmental grants has a long history,[1] so far the studies that thanks to such an approach are truly convincing are too few for the puzzle on grants effects to be complete. By adding a piece to this puzzle, the contribution of this paper should therefore be most welcome. Utilizing policy-induced increases in intergovernmental grants to a group of Finnish municipalities I identify and estimate causal effects of grants on local expenditures and income tax rates in a difference-in-difference (DID) model. 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 this particular supplement.

The reason why the effects of grants are somewhat puzzling has to do with 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 percent for most countries (grants targeted to specific sectors or projects on which 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 on 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 theory would predict. 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 to 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 fly-

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

paper effect with poorly informed voters that enable budget-maximizing policy makers to pursue their own objective. Hamilton (1986) offers a different explanation that instead is 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]

Because one possible explanation to the apparent flypaper effect simply is 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 the outcome of interest. The problem is particularly evident for the case of expenditures: jurisdictions with characteristics associated with high expenditures such as, e.g., a large share of elderly, typically receive more grants exactly because they need to be spending more. It is therefore 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, typically such an approach would kill all of the variation in grants. A more promising remedy is therefore to closely study how grants are determined in search for experimental-type features where the amount of grants vary but the underlying needs do not.

As argued, there is a lack of studies that convincingly deal with the likely endogeneity problem in grants. To be fair, however, there is not a total absence. But the ones that do provide mixed evidence. 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, 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 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 structures 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. A third innovative example is Dahlberg et al. (2008), who utilize a non-linearity in the distribution of grants to Swedish municipalities with a diminishing population to identify causal effects, and show that there is a one-to-one correspondence between grants and local expenditures but no effect of grants on local income taxes.

This paper is similar to that of Dahlberg et al. in that these are the only two studies that focus on the effects of *general* grants on overall expenditures and tax rates, which in

[2]Revenue raising on 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 indeed is the rationale behind federal systems with intergovernmental grants).

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

turn are two highly general (and relevant) economic outcomes. One could argue that such a general setting is better suited for the flypaper literature since it is closer linked to theory than what grants targeted towards specific sectors or projects are. Because, so far, only a single study has adopted this wider focus in terms of variables, it is interesting to see whether municipalities in a different country behave similarly, or if the results in Dahlberg et al. are likely to be valid only for Sweden. Furthermore, a problem with their study is that they apply a "fuzzy" version of the regression kink design (Nielsen et al., 2008; Card et al., 2009) with a strangely large first stage-estimate that is left unexplained. I apply a DID model, which—although not new to the public finance literature—has a clear advantage in its transparency and analogy to experimental designs. And in lieu of the findings in Gordon (2004) that large stimulatory effects of grants vanish over time, a particularly important benefit of the DID approach is that treatment takes place in a distinct point in time. This enables investigating the dynamics in the responses to grant increases, which is impossible in the regression kink framework where a treated municipality is likely to receive similar grant increases at several consecutive points in time.

The policy-induced variation that enables identification increased a particular grant supplementary to remotely populated municipalities, so the claim that effects of general grants are evaluated demands its justification. The Finnish grant system is made up of several types of grants of which the particular supplement in question is a rather small part. But during the period relevant to here, 1997–2005, the grant system was structured so that all grants were distributed to the municipalities as a general sum with no strings attached. This means that as far as the receiving municipalities are concerned, increases in the particular supplement is exactly equivalent to increases in any of the other broader grant categories that together comprise the general sum.

The robust finding of the 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 an immediate one-to-one correspondence between grants and local expenditures. Furthermore, expenditures continue to increase also some time after the grant increase, although this response is estimated less precisely. 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 valid at least to other federations characterized by comprehensive local independence. Indeed, the scope for targeted grants to crowd out spending on specific projects seem much larger than for general grants to crowd out total expenditures.

As far as I am aware this is the first paper that estimates effects of intergovernmental grants on Finnish data taking explicit account to potential endogeneity problems, but there are a few other studies on the matter. 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. 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]

[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

The remainder of this paper goes as follows. The next section describes the particular grant supplement subject to the policy reform in 2002 and how that enables circumventing the endogeneity problem in grants. Section 3 describes the data and its variables. Section 4 presents the baseline results accompanied by a robustness check, and suggests an alternative identification and estimation strategy to the standard DID. The section ends by looking at the evolution of debt in the municipalities. Section 5 concludes the paper with a general discussion of the results.

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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.

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

This section describes the structure of the grant supplement 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 $pop^{50km}$ is 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[6] the supplemental grant was distributed based on this index as described in table 1 and illustrated in figure 1[7]. 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.[8]

Table 1: Distribution of the supplemental grant

| Remote index | Supplemental grant 1997–2001 | 2002–05 |
|---|---|---|
| <0.50 | 0 | 0 |
| 0.50 to 0.99 | 1.5*base grant | 3*base grant |
| 1.00 to 1.49 | 2*base grant | 5*base grant |
| 1.50 to 2 | 3*base grant | 6*base grant |

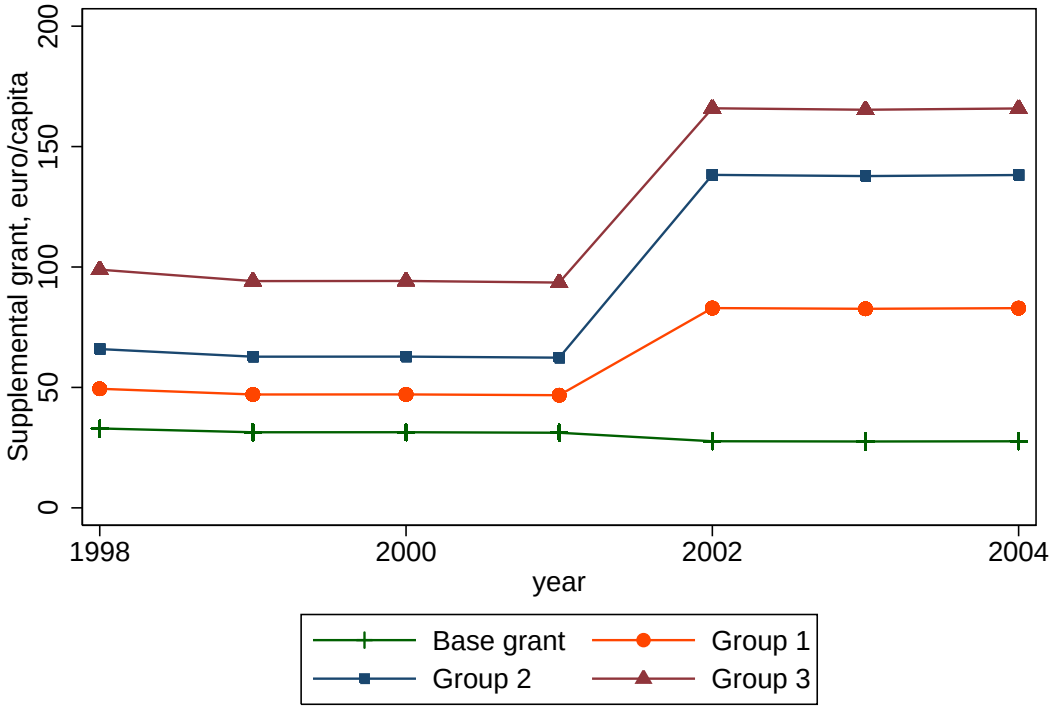
[5]The remote index assignment relevant for our purpose took place in 2002.

[6]In 2006 a new grant system where this as well as many other grant types were changed considerably came into place.

[7]Due to lack of data the figure only illustrates how the supplemental grant was distributed during 1998–2004.

[8]For the years prior to 2002 (in which the euro was introduced) the exchange rate 1 euro $=$ 5.94573 Finnish marks is used.

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 policy reform.[9] Relative to the base grant, the reform doubled the supplemental grant for group 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.

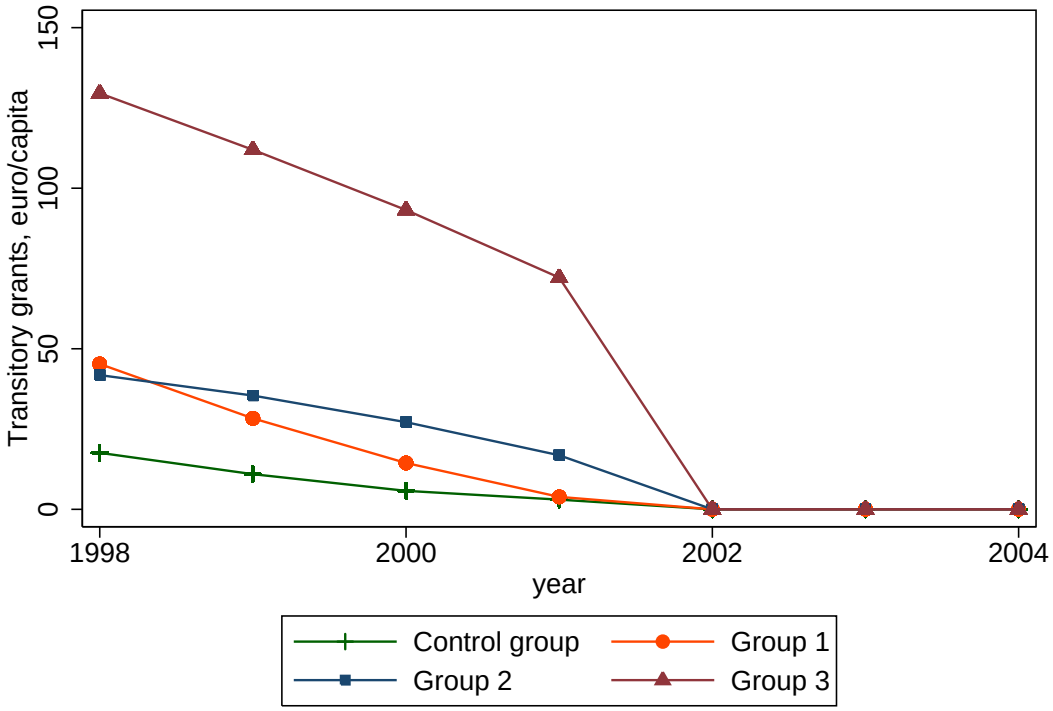
The supplemental grant increase was part of a group of policy reforms implemented in 2002 motivated by the fact that economic conditions varied across municipalities despite rather stable finances for the country in general. Of these policies the two most significant were the abolishment of a system with repayments of value added taxes from the municipalities to the state, and a decrease in the municipalities' share of revenue from corporate taxation. The details of these and related reforms are described in the Appendix, but for now we note 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 through a special application procedure could (and still can) be granted municipalities with extraordinary financial difficulties. The intention was however that the fiscal relation between the state and the municipalities were not to be altered due to these changes on the whole.

As a 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

[9]The reform is proposed by the government in bill 128/2001 and legislated in law 1360/2001.

were highly affected by this grant reform were compensated with transitory grants that were gradually decreased through 2001 and were entirely removed in 2002. Among other things, the previous grant system had put more weight to 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 is seen in figure 2 plotting transitory grants separately for the same three groups as in figure 1 along with a control group constituting 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 group 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.[10]

Figure 2: Transitory grants




Source: the Association of Finnish Local and Regional Authorities

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.[11] The treatment group is comprised of municipalities $i$ with

[10]Combining figures 1 and 2 suggests that for group 3, because of the counteracting effect from decreased transitory grants, 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.

[11]The reader may have observed that the structure of the supplemental grant also is suitable for a regression discontinuity (RD) design, in which the remote index is the forcing variable that contains three cut-offs at which the effects of grants could potentially be identified. However, careful analyses have shown that

remote indices in the range 0.5–1.5 and treatment is defined as changes (increases) in supplemental grants, $\Delta SG_i$, 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 $\Delta SG_i$ on changes between year $t$ and $t-1$ in either of the outcome variables per capita expenditures or tax rate, $\Delta Y_{it}$, is then

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

with $T_t$ denoting year fixed-effects and $\varepsilon_{it}$ 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.

It is however also of interest to see in which year(s) the effect took place. The supplemental grant increase in 2002 was not a temporary thing. That means that municipalities that, say, used the extra grants to increase spending did not have to cut back down 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 that we should expect positive effects also 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:

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

For $t \in [2001, 2004]$, each of the parameters $\tau_t$ represents the effect of $\Delta SG_i$ between year $t$ and year $t-1$. Because the supplemental grant increase took place in year 2002, $\tau_{2002}$ thus represents the immediate effect on $\Delta Y_{it}$, whereas $\tau_{2003}$ and $\tau_{2004}$ represent the additional effects one and two years later. Finally, $\tau_{2001}$ captures the "effect" of $\Delta SG_i$ 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 we require 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.[12] In other words, there can be no other factor except for the supplemental grant increase

the discontinuous variation that remains after controlling for any reasonably smooth function of the remote index is not enough. Although the RD estimator yields robust results, it indicates that identification stems from annual variation in grants (such as the policy-induced increase in 2002) rather than from discontinuous variation at the cut-offs.

[12] 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 can not be assumed to hold unless treatment effects are constant. This is always the case in standard DID models. On the contrary, 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.

that causes the pre-treatment difference between the control group and treatment group to change at the time of treatment (or within two years after treatment for the dynamic effects $\tau_{2003}$ and $\tau_{2004}$). This is our maintained identifying assumption about common trends. Importantly, included in this assumption is that all other policy reforms implemented in 2002 (like those mentioned above and described in the Appendix) on average affected the treated and control municipalities equally.[13]

[13] Also included in the assumption about common trends is that there is no systematic difference in how the different groups of municipalities were affected by the introduction of the euro.

# 3 Descriptive data

In order to familiarize the reader 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 the main sample restrictions are that 52 municipalities that were consolidated with another around this period are dropped,[14] as are 16 municipalities belonging to the autonomous island Åland, and 11 municipalities with discrepancies concerning entitlement to the supplemental grant. For reasons discussed above, the 13 municipalities belonging to group 3 (cf. 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—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 remainder 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 middle 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 in 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 percent) and education and culture (around 25 percent). The largest single item of expenditure is wages to municipal employees (around 30 percent).[15] 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 percent of total revenue, while the corresponding percentage for property and corporate income taxation was merely around 3 and 6, respectively. The tax rates on private income and properties are decided locally whereas the level of taxation of corporate income is centralized.

Not too surprisingly, table 2 reveals differences between treateds and controls in many of the variables. Of the outcome variables, especially expenditures are higher in the treatment group, whereas the tax rate does not seem to vary much. Given how the groups are defined and how the remote index is constructed (cf. the formula in (1)), the fact that 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

[14] 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 B's even for years prior to 2002. For some variables this procedure makes more or less sense, while for others (e.g., tax rate or political majority) it makes no sense at all. Consequently, there is no good option but to drop all consolidated municipalities from the data.

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

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.

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 percent of the generic grants, which in turn was around 10 percent of total grants. Due to a rather uneven distribution of grants across municipalities this figure is, however, closer to 5 percent 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 percent) and to education and culture (around 27 percent). For the average municipality all these grants amount to around 15–20 percent of total revenue.

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 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 extra financial aid due to extraordinary circumstances.

Table 2 shows a slight increase in both of the outcome variables between the pre- and post-treatment period. In order to get a more detailed view of the evolution over time, figure 3 plots the yearly averages of expenditures and tax rates in the treatment and control group. The overall picture is a positive but rather stable and parallel trend in both variables prior to the reform, suggesting that the identifying assumption about common trends holds. We also note that it is difficult to visually detect any aggregate effects of increased grants to the treatment group in year 2002, so let us now instead turn to the parametric estimation of the treatment effects.

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 | 3944.4 | 4833.7 | 3334.3 | 4009.4 |
| | (388.4) | (555.5) | (497.3) | (568.6) |
| Tax rate | 18.59 | 18.83 | 18.06 | 18.40 |
| | (0.428) | (0.327) | (0.689) | (0.626) |
| Generic grants | 80.37 | 127.8 | 28.02 | 25.80 |
| | (8.623) | (28.44) | (13.74) | (23.52) |
| Total grants | 1044.6 | 1363.1 | 683.2 | 907.3 |
| | (187.5) | (220.8) | (212.8) | (273.0) |
| 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) |
| Students | 0.129 | 0.120 | 0.116 | 0.113 |
| | (0.0207) | (0.0192) | (0.0257) | (0.0255) |
| Elderly | 0.192 | 0.215 | 0.180 | 0.188 |
| | (0.0297) | (0.0320) | (0.0448) | (0.0459) |
| On welfare | 0.0994 | 0.0818 | 0.0753 | 0.0649 |
| | (0.0263) | (0.0251) | (0.0270) | (0.0232) |
| Tax base | 7388.0 | 8402.0 | 9106.0 | 10298.9 |
| | (868.9) | (766.2) | (2054.3) | (2051.8) |
| $N$ | 148 | 120 | 1320 | 981 |
| n | 37 | 40 | 330 | 327 |

Expenditures, tax base and grants are in euro per capita

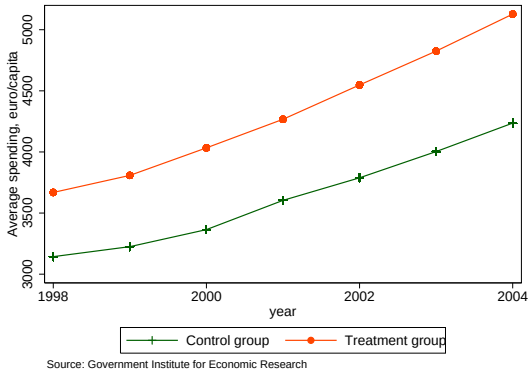
Students, elderly and on welfare are in shares of overall population

Area is in square kilometers

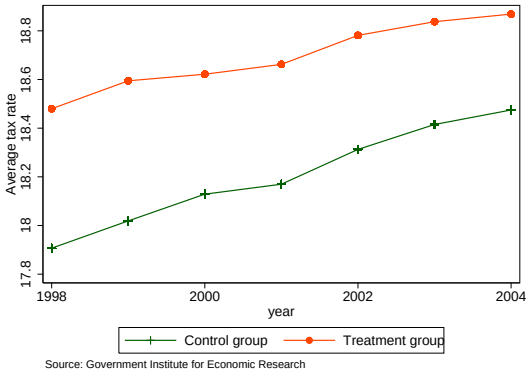
Source: Government Institute for Economic Research and the Association of Finnish Local and Regional Authorities

Figure 3: Average per capita expenditures and tax rate

(a) Expenditures




(b) Tax rate



# 4 Results

## 4.1 Baseline results

The baseline results are from the estimation of the treatment effects in equation (3), presented in table 3. The four respective rows of the table show the estimated effects of a one euro per capita increase in supplemental grants on changes in the outcome between two consecutive years for the period 2001–2004, with associated standard errors that allow for clustering within municipality. The first column presents effects on total per capita expenditures, and the second column on the income tax rate. Because the grant increase occurred in 2002, $\tau_{2002}$ represents the immediate treatment effect, whereas $\tau_{2003}$ and $\tau_{2004}$ represent the dynamic incremental effects one and two years afterwards. Finally, $\tau_{2001}$ 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: $\tau_{2002}$ is estimated essentially to 1, with the interpretation that as grants increase with one euro per capita, so do total expenditures (in the same year). Furthermore, one and perhaps even two years after the grant increase, expenditures continue to increase with an additional euro. Although the dynamic estimates for the two later years are obtained with much less precision (especially for year 2004), the pattern indicates that the expenditure response in fact exceeds the grant increase. This indication is strengthened by the fact the these large estimates are in stark contrast to the "effect" in year 2001, which is much smaller and not statistically indistinguishable from zero—thus suggesting the identifying assumption of common trends to hold.

The large treatment effects on expenditures most likely leave little room for grant increases to be used for tax cuts, a notion that is confirmed correct in the right column of table 3. Although the treatment effect in all years are negative, the statistical significant is, at best, weak. More importantly, the size of the point estimates imply 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 with a mere 0.10 percentage points.

It is comforting that also the estimate of differences in pre-treatment trends in tax rate supports the identifying assumption, as seen by the insignificant estimate of $\tau_{2001}$ on the tax rate. Hence, from what we have seen so far the conclusion is that increases in grants leave tax rates unchanged but causes expenditures to increase perhaps even more than the grant increase itself. This large expenditure increase may seem irrational, but such a response is in principle possible since Finnish municipalities do not have a balanced budget requirement and are allowed to take up loans. We return to this matter in section 4.4.

## 4.2 Sensitivity analysis

Before studying the debt response of grants, this and the next section present various alternatives to the above baseline specification in order to certify that the resulting estimates are the true causal effects of grants on expenditures and tax rates. As a first sensitivity check, municipalities with remote indices substantially smaller than in the treated groups

Table 3: Baseline results

| | Expenditures | Tax rate |
|---|---|---|
| $\tau_{2001}$ | 0.124 | -0.000145 |
| | (0.332) | (0.000347) |
| $\tau_{2002}$ | 1.044*** | -0.000997* |
| | (0.310) | (0.000531) |
| $\tau_{2003}$ | 1.122* | -0.000774 |
| | (0.578) | (0.000489) |
| $\tau_{2004}$ | 1.447 | -0.000495 |
| | (1.112) | (0.000311) |
| Observations | 2202 | 2202 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita expenditures and tax rates over the years 2001–2004

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

are excluded from the control group. Recall, first, that in order to be treated with supplemental grants the remote index had to be larger than +0.50 and, second, that characteristics such as size of population and area varied quite substantially with the remote index. Thus, we may worry that the original control group makes for a poor counterfactual. The results presented in table 4, where the estimations on expenditures and tax rate in the first and third column, respectively, are restricted to only include municipalities with remote index larger than -10 and in the second and fourth column to only include those with remote index larger than -5, do however not suggest that. On the contrary, the similarity of these results with those in table 3 indicates that the composition of the control group in this particular dimension does not matter. This is true even when more than 30 percent of the observations are lost, as seen in the second and fourth columns.

As a second alteration, a number of municipal characteristics that are likely to appear in the outcome equation are added. If the source of variation in grants is exogenous there should be no correlation with any other determinants of the outcome, and thus excluding them should not cause omitted variable bias in the estimated grants effects. In other words, the estimates should be the same irrespectively of what additional variables are included in the regression.

The first candidate to be included is the remote index, i.e. the variable that determines the size of the supplemental grant. The resulting estimated effects of grants on expenditures and taxes are presented in the first column of table 5 and 6, respectively. The second candidate is total per capita grants (net of the supplemental grant), added in the second column of the same tables. This would be an important inclusion to the model if it were the case that the treated municipalities to a larger extent than the control municipalities benefited from increases—or suffered from decreases—in other types of grants as well around this period. In such case, failing to take other grant receipts into account would

Table 4: Sensitivity analysis, restricting the control group

| | Expenditures | | Tax rate | |
|---|---|---|---|---|
| Remote index | $> -10$ | $> -5$ | $> -10$ | $> -5$ |
| $\tau_{2001}$ | 0.0357 | -0.0590 | -0.0000857 | -0.0000940 |
| | (0.336) | (0.339) | (0.000350) | (0.000365) |
| $\tau_{2002}$ | 1.001*** | 0.967*** | -0.000978* | -0.000909* |
| | (0.314) | (0.321) | (0.000544) | (0.000550) |
| $\tau_{2003}$ | 1.055* | 0.988* | -0.000732 | -0.000711 |
| | (0.582) | (0.590) | (0.000497) | (0.000511) |
| $\tau_{2004}$ | 1.416 | 1.375 | -0.000391 | -0.000490 |
| | (1.122) | (1.134) | (0.000311) | (0.000323) |
| Observations | 1860 | 1464 | 1860 | 1464 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita expenditures and tax rates over the years 2001–2004

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

bias the estimates of the effect of increased supplemental grant.[16] The third column adds various other variables that are likely to be key determinants of expenditures and taxes; per capita tax base, size of population, and the population shares of school-aged children and of elderly. The last column combines the three previous; in this column the remote index, total grants, as well as additional outcome-determinants are added.

Looking at the results in tables 5 and 6 gives, again, no strong indications that the baseline estimates are biased in any direction. Adding the remote index does not affect the results at all,[17] whereas total grants and the additional covariates only have a slight effect on the size of the estimates. Thus, despite that some of these variables are likely to be key determinants of the outcome variables, omitting them does not induce any bias. This validates the claim that the policy-induced increase in the supplemental grant is exogenous, and hence that our results can be given a causal interpretation.

## 4.3 Alternative identification: 2SLS

It is not too often that researchers come across a convincing identification strategy. And, safe to say, it is rather rare with more than one seemingly equivalent strategies to identify the same parameter. The current setting, however, allows us to do just that. Above we have defined treatment to be increased supplemental grants. If we instead define treatment to be increased generic grants (i.e. the type of grant that the supplement is part of) or even increased total grants (of which generic grants subsequently are part), the policy-induced variation may be seen as allocating treatment in an imprecise way, making it suitable for

[16] A description of how other types of grants changed in 2002 is found in the Appendix.

[17] Recall that the remote index is updated every fifth year by Statistic Finland, and was done so in 2002 and is therefore constant between the other years.

Table 5: Sensitivity analysis for expenditures, adding covariates

| Added variables | Remote index | Total grants | Additional X:s | Remote index, total grants, additional X:s |
|---|---|---|---|---|
| $\tau_{2001}$ | 0.124 | 0.104 | -0.0586 | -0.0666 |
| | (0.332) | (0.329) | (0.363) | (0.360) |
| $\tau_{2002}$ | 1.051*** | 0.999*** | 0.956*** | 0.932*** |
| | (0.311) | (0.308) | (0.328) | (0.328) |
| $\tau_{2003}$ | 1.122* | 1.017* | 1.116* | 1.030* |
| | (0.578) | (0.577) | (0.586) | (0.587) |
| $\tau_{2004}$ | 1.447 | 1.467 | 1.326 | 1.346 |
| | (1.113) | (1.115) | (1.155) | (1.157) |
| Observations | 2202 | 2202 | 2100 | 2100 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita expenditures and tax rates over the years 2001–2004

Additional X:s are: Per capita tax base, population size, and population shares of students and elderly

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

Table 6: Sensitivity analysis for tax rate, adding covariates

| Added variables | Remote index | Total grants | Additional X:s | Remote index, total grants, additional X:s |
|---|---|---|---|---|
| $\tau_{2001}$ | -0.000145 | -0.000173 | -0.0000837 | -0.0000967 |
| | (0.000347) | (0.000347) | (0.000357) | (0.000356) |
| $\tau_{2002}$ | -0.000989* | -0.00106** | -0.00109** | -0.00113** |
| | (0.000532) | (0.000533) | (0.000525) | (0.000528) |
| $\tau_{2003}$ | -0.000774 | -0.000924* | -0.000818 | -0.000963* |
| | (0.000489) | (0.000503) | (0.000509) | (0.000520) |
| $\tau_{2004}$ | -0.000496 | -0.000467 | -0.000485 | -0.000450 |
| | (0.000311) | (0.000311) | (0.000326) | (0.000326) |
| Observations | 2202 | 2202 | 2100 | 2100 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita expenditures and tax rates over the years 2001–2004

Additional X:s are: Per capita tax base, population size, and population shares of students and elderly

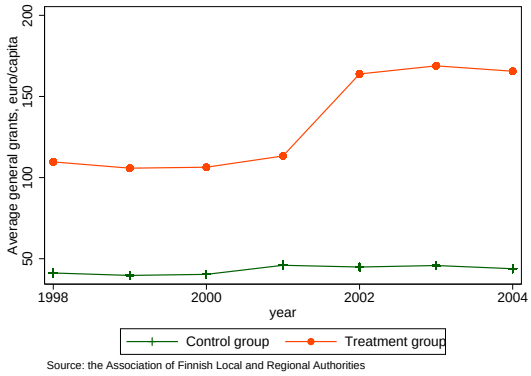
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a two-stage least square (2SLS) estimation.

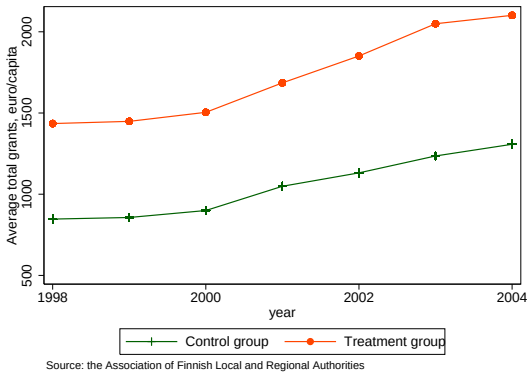
Figure 4 illustrates how generic and total grants have evolved over the sample period. Since the supplemental grant constitute around 80 percent of generic grants to municipalities in the treatment group, it is not surprising that the policy in 2002 yielded an increase of a similar magnitude in generic grants as 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




In the alternative identification and estimation strategy where we define treatment to be increased generic grants or total grants, figure 4 is the graphical equivalent of the following first stage equation:

$$\Delta G_i = \gamma_{2002} \Delta SG_i + T_{2002} + \Delta e_i \tag{4}$$

That is, the change in generic or total grants, $\Delta G_i$, is instrumented with the supplemental grant increase. As hinted by the labeling of the first-stage estimate, note that the policy only induces variation in $\Delta G_i$ between year 2001 and 2002, and hence the estimation sample is now restricted to these two years. Using the predicted values, $\widehat{\Delta G_i}$, from (4), we then recover the estimate of the immediate effect of increased grants, $\tau^{IV}_{2002}$, in the second stage:

$$\Delta Y_i = \tau^{IV}_{2002} \widehat{\Delta G_i} + T_{2002} + \Delta \varepsilon_i \tag{5}$$

In the context of 2SLS, the earlier baseline estimates of $\tau_{2002}$ are thus the reduced form results. What is special about the current setting is that the reduced form and 2SLS are expected to yield the same estimate. 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.[18] Hence, with 2SLS as an alternative

[18]The insight from Imbens and Angrist (1994) is that IV estimators identify a weighted local ATE, with

estimation strategy we in some sense have an additional robustness check—ableit not of the full dynamics, but at least of the immediate treatment effect.

First-stage estimates of $\gamma_{2002}$ from equation (4) are presented in table 7, with $G_i$ defined as generic grants and total grants in the two respective columns. Looking at the left column, the prior that the supplemental grant ought to be highly correlated with generic grants is verified. The point estimate is highly statistically different from zero, but is not distinguishable from one. From the right column we note that the standard errors in the regression of total grants are more than ten times the size of those for generic grants (as suggested by the graphical representation), and that the point estimate is not as close to one. But also in total grants does the policy increase in supplemental grant induce statistically significant variation, implying that the two-stage procedure works fine also here.

Table 7: 2SLS, first stage estimates

| | Generic grants | Total grants |
|---|---|---|
| $\gamma_{2002}$ | $1.018^{***}$ | $1.648^{***}$ |
| | (0.0132) | (0.179) |
| Observations | 734 | 734 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita general grants and total grants in year 2002

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

The subsequent second-stage estimates are given in table 8. Comparing with the estimated immediate treatment effects from above (cf. the estimate of $\tau_{2002}$ in table 3), these results provide essentially the same picture. When instrumenting generic grants, the point estimates of the immediate effect of a euro per capita increase are in all regards identical to their correspondence in table 3 for both of the outcome variables. When instead instrumenting total grants, the point estimates are reduced somewhat to around 0.65 and -0.0006 for expenditures and tax rate, respectively, but is still positive and statistically significant for the former. Although the results would have been more comforting had the point estimates not been reduced, it is as expected given the larger first-stage estimate for this grant category. In connection to this it is worth highlighting that adding total grants received (net of the supplemental grant) to the original specification did not affect the results, as was shown in tables 5 and 6 in the previous section.

With the caveat that the first-stage estimate was larger than one and that there consequently was a reduction in the second-stage estimates when instrumenting total grants, the primary conclusion from this exercise is still that the alternative 2SLS estimation supports the original orignial DID. The baseline results that increased grants stimulate expenditures considerably—both immediately and some time afterwards—but that the effect of

positive weights for so called compliers. The current setting where the grant distribution is formula-based and hence not under the influence of the municipalities implies full compliance to the treatment. And because all types of grants are lumped together, complying to increased supplemental grants is equivalent to complying to increased total grants. Thus, the implicit IV weights will not differ from in the reduced form estimations above.

increased grants on the tax rate is negligible, seem convincing.

Table 8: 2SLS, second stage estimates

| | Expenditures | | Tax rate | |
|---|---|---|---|---|
| Grant treatment | Generic | Total | Generic | Total |
| $\tau^{IV}_{2002}$ | 1.063*** | 0.657*** | -0.000971* | -0.000606 |
| | (0.305) | (0.194) | (0.000507) | (0.000629) |
| Observations | 734 | 734 | 734 | 734 |

Standard errors in parentheses

The table reports estimated effects of one euro per capita increase in *SG* on per capita expenditures and tax rates in year 2002

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

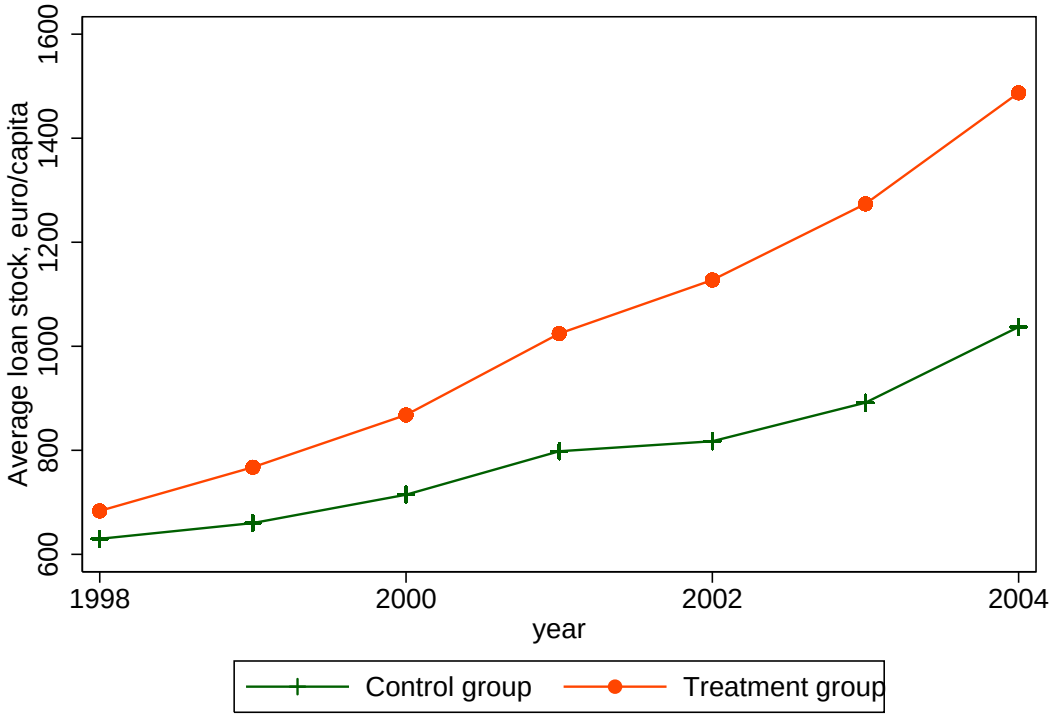
## 4.4 Evolution of debt

The very large responses in expenditures that follow an increase in grants may seem irrational and perhaps even incredible. But as mentioned above, Finnish municipalities do not have a balanced budget requirement and are allowed to take up loans. This means that if the empirical results are correct, we should see an increase in the loan stock of treated municipalities relative to control municipalities.

The pattern in figure 5, where we see how the loan stock has evolved in these two groups of municipalities over the sample period, is by and large consistent with these priors; although it appears as if a positive trend took off already in the pre-treatment period (i.e., before 2002), the treatment group certainly has increased their debt more than the control group in the post-treatment period.

It thus appears as if the results from above are credible; not only is the immediate expenditure effect of a grant increase substantial, but expenditures continue to increase also some time after the grant increase. Although not very precisely estimated, this suggests that expenditures are increased more than the amount of the grant increase. A suggested characteristic of these municipalities is therefore path-dependence. That is, not only do the expanding municipalities get accustomed to a larger size but also to a faster growth rate of the budget. A tentative answer as to why that is, and why the tax rate is so insensitive to grant increases, is provided in the next, concluding section.

Figure 5: Evolution of debt




Source: the Association of Finnish Local and Regional Authorities

# 5 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 much 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 of this paper. We have studied the effect 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 an immediate one-to-one correspondence between grants and local expenditures. A glance at a balance of payment sheet for Finnish finances shows that, on aggregate, total consumption is around 50 percent of GDP. Out of total consumption only 30 percent is public consumption and, hence, 70 percent is private consumption. The large stimulatory effects on expenditure can thus be interpreted as crowding-in effects, opposite to the crowding-out effects as found by, e.g., Knight (2002). But recall that he studied grants aimed to support state highway construction, whereas this and the Dahlberg et al. study concern general, non-earmarked grants and how they affect taxes and spending overall. It is thus 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 seem much larger than for general grants to crowd out total expenditures.

By allowing part of the response to the grant treatment to occur only after some time, we also found that expenditures not only immediately responded positively to a grant increase but continued to do so one year after the grant increase to the extent that the increase in expenditures actually exceeded the grant increase. Contrary to the results in Gordon (2004), this suggests a path-dependence in the sense that expanding municipalities not only get accustomed to a larger size of the budget but also to a faster growth rate. These results clearly highlight the importance of applying a dynamic framework when studying the behavior of local government.

The focus in this paper has been to give a convincing answer to *how* local governments respond to increases in grants. But in concluding, let me state a possible suggestion 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 often is dismissed as unlikely to fully explain the empirical flypaper anomaly. On the contrary I believe it to be quite likely both on behalf of the local governments and of the state; the apparent reluctance to use grants to finance local tax cuts as well as the intention of the state to distribute grants in order to first and foremost finance expenditures is indicative of such behavior. Although the latter point is not explicitly stated, it is implicit from the labeling of grants as "grants to social services and health care" and "grants to education and culture" despite that all grants are in fact non-targeted. These labels are also likely to further encourage increased local spending if the municipalities fear that by instead responding with tax cuts

they may disqualify for 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 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 who wish to increase disposable income should do so directly by lowering federal tax rates rather than rely 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.

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# A 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 everything that changed this year, but rather the attention is restricted to what is related to the specific policy reform studied in this paper. Specifically, the simultaneous implementation of these policies may shed doubt on the identifying assumption. Some of this we can test, but for the cases where we can not we need to rely on the assumption that those policies on average affected the treated and control municipalities equally.

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 percent 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 percent 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 results presented.

- 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 percent of the difference. Before 2002 this fee could be at most 15 percent of the municipality's total per capita potential tax revenues, but in 2002 this cap was removed. This affected four 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 affected the group of 13 most remotely populated municipalities considerably, which is why they are removed from the empirical analysis. Controlling for the amount of transitory grants received by the remaining municipalities does not affect the results presented in the paper.

- Some of the activities in the local government sector is 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 percent in 2001 and with 25 percent each in 2002 and 2003. It was however decided that the full remaining 50 percent were to be implemented in 2002, implying that the increase in the sector grants were 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. Because 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. This consequently shifted the fiscal balance in favor of the municipalities at the expense of the state.

The main reform to re-balance the 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 percent to 24.09 percent. Although implemented in 2002 by law, in practice this reform as well as the reform in the VAT system did not take effect until later years due to fiscal lags. In due time, however, these reforms affected all municipalities, meaning that there is no straightforward way of testing whether anticipatory behavior bias the results in the paper.

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 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 percent cap in the revenue sharing system was to be accrued to 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 estimation results presented in the paper when controlling for total grants received are similar to the baseline results.

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