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Heterogeneous Effects of
Intergovernmental Grants

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HOW LOCAL ARE LOCAL GOVERNMENTS?
HETEROGENEOUS EFFECTS OF INTERGOVERNMENTAL GRANTS

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How Local are Local Governments? Heterogeneous Effects of Intergovernmental Grants*

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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 three groups of remotely populated municipalities. The results show no statistically significant response in expenditures to the policy overall. However, when investigating the extent of heterogeneity I find a large, significant effect for two out of the three groups of treated municipalities but a likely null or even negative effect for the third group. The tax rate response is small and seems to be more homogeneous.

Keywords: Intergovernmental grants, difference-in-difference, heterogeneous treatment effects, flypaper effect

JEL codes: C23, H71, H72, H77, R51

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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 thereto 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 three pre-defined groups of remotely populated municipalities in 2002 whereas a fourth group 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

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

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 flypaper 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

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

²Revenue 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).

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 grants³ 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. 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 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. Given 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, they apply a regression kink design where it is somewhat unclear how different municipalities are weighed into treatment, whereas I apply a DID model with distinct treatment and control groups. Although not new to the public finance literature, identification

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

through DID strategies has a clear advantage in its transparency and analogy to experimental designs.

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, implying that if effects differ for different types of grants it is because a particular grant is typically received by municipalities of certain characteristics. That is, unless there are heterogeneous treatment effects of grants the results from studying a particular grant which is part of a larger, general sum can be extended to other, broader grant categories.

In light of this, a contribution of this paper is that the extent of heterogeneity in responses to increases in grants is assessed by estimating both an overall average effect of the policy as well as separate effects for each of three groups of receiving, or treated, municipalities. The three groups consists of municipalities that differ in how remote their population is and that were granted different amounts of the supplement both before and after the policy reform in 2002. This pre-defined separation into three groups of treated municipalities thus seem particularly suitable for studying whether different sets of characteristics are likely to matter for how local expenditures and tax rates respond to increased non-targeted general grants, something that to my knowledge has never been done.

And indeed, I estimate quite heterogeneous effects on expenditures. When pooling all treated municipalities the resulting effect on expenditures of an additional euro in grants is around 0.50 cents, but due to poor precision this estimate is not statistically different from zero. When I instead separate the groups I find a statistically significant one-to-one correspondence between increased grants and spending for two of three groups, while there is a likely null or even negative effect for the third group. Further results suggest that those groups who immediately respond positively continue to do so to the ex-

tent that the expenditure increase is actually larger than the grant increase. As for the effect on the tax rate, although some estimates are statistically significant they are too modest in size to have any major economic relevance, and there is no evidence of heterogeneity in the tax response.

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

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 as well as an alternative identification and estimation strategy to the standard DID. The section ends with an analysis of dynamic treatment effects. Section 5 concludes the paper with a general discussion of the results.

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

⁴Since 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 certain amount of expenditures above which receiving jurisdictions are often spending. This implies that also matching grants effectively induce a pure income effect.

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

$$remote\ index_i = \frac{15,000 - pop_i^{25km}}{15,000} + \frac{60,000 - pop_i^{50km}}{60,000}, \quad (1)$$

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 1997–2005⁶ the supplemental grant was distributed based on this index as described in table 1 and illustrated in figure 1⁷. 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, 1–1.50, or 1.50–2 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 this amount varied around 30 euro.⁸

The sharp increase in the supplemental grant in 2002 seen in figure 1 is

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

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

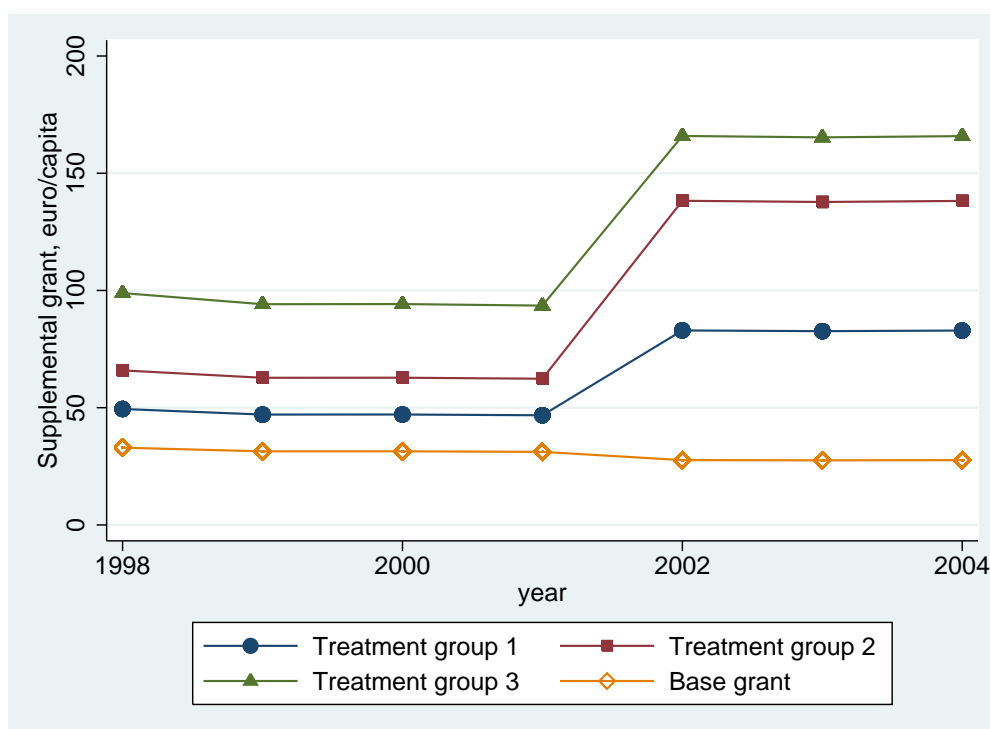
⁷Due to lack of data the figure only illustrates how the supplemental grant was distributed during 1998–2004.

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

Table 1: Distribution of the supplemental grant

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

Figure 1: The supplemental grant



due to a policy reform.⁹ Relative to the base grant, the reform doubled the supplemental grant for the first and third group of receiving municipalities and more than doubled the grant for the middle group. To finance this 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 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.

The particular policy-induced increases displayed in figure 1 will be used in a DID model to identify causal effects of grants on municipal expenditures and on local proportionate income tax rates.¹⁰ The treatment is defined as increased supplemental grants, and the control group accordingly consists of municipalities with a remote index smaller than 0.50 that never received

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

¹⁰The 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 cutoffs at which the effects of grants could potentially be identified. However, careful analyses have shown that 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 is through annual variation in grants (such as the policy-induced increase in 2002) rather than through discontinuous variation at the cutoffs.

this particular grant. As explained in the introduction, the setup suits two alternative DID models, of which one is to estimate the following:

$$Y_{it} = \bar{\tau} \text{supplement}_{it} + \mu_i + T_t + \varepsilon_{it}, \quad (2)$$

where Y_{it} is either per capita expenditures or tax rate in municipality i at time t , μ_i and T_t are municipality and time fixed effects, and ε_{it} is the error term. This specification identifies the parameter $\bar{\tau}$, which is the effect of a one euro per capita increase in supplemental grants on average across all treated municipalities. As an alternative I will also separate the three groups of treated municipalities as defined in table 1 and estimate separate, potentially heterogeneous, treatment parameters:

$$Y_{it} = \tau_1 D_1 \text{supplement}_{it} + \tau_2 D_2 \text{supplement}_{it} + \tau_3 D_3 \text{supplement}_{it} + \mu_i + T_t + \varepsilon_{it}, \quad (3)$$

where D_k is an indicator variable for treatment group k whose treatment effect is τ_k .

The estimator in (2) will always identify the overall average effect no matter the extent of heterogeneity, and will be efficient if effects are homogeneous. But the second specification can provide important additional insights. In particular, policy makers considering future reforms that similarly to the one studied here target these groups differently would need to have knowledge about the potential heterogeneity in order to do a proper cost-benefit analysis. Perhaps more importantly, an additional insight that can be gained from estimation of equation (3) concerns to what extent effects of a type of grant like this to jurisdictions with certain characteristics are externally valid. If effects are homogeneous the result from studying the particular grant supplement can be generalized to other, broader grant categories. But if there

is considerable heterogeneity additional studies of grants to other types of municipalities are needed.

Why, then, would one suspect that the policy-induced grant increases affected the three groups of municipalities differently? One reason is that the treatment intensity varied across the three treatment groups. As table 1 illustrates, if we think of treatment intensity in absolute terms group 2 and 3 got the same treatment (in both these groups the increase was three times the amount of the base grant), but in relative terms the treatment intensity was the same for group 1 and 3 (for both of which the grant doubled). If the effect per euro increase differs when the treatment intensity (defined in either of these two ways) differs, then the preferred specification is (3), wherein treatment effectively is binary since treatment intensity is constant across municipalities within each treatment group. Thus, the group indicator variables, D_k , could have been left as dummies. By interacting the group indicators with the size of the supplemental grant increase, however, the treatment is scaled so that the interpretation of τ_k still is the effect on Y of an additional euro grants per capita (to a municipality in the k_{th} treatment group).

A second reason as to why one would suspect heterogeneous behavior across the three groups is that these municipalities differ from each other along certain dimensions that one could argue matter for how they respond to increased grants. These differences can be readily seen from the data in the next section, but by construction we know that the share of population living far from the municipal center varies across the groups. If the argument by Filimon et al. (1982) holds, i.e. that the level of expenditures is set by budget maximizers whose scope is larger the less informed voters are, then we would expect larger effects on expenditures in the more remotely populated municipalities—at least if a long distance to the city center makes it more difficult to stay informed.¹¹

For the treatment effects in either (2) or (3) to be identified we require that, conditioning on the differences prior to the grant increase in 2002, the

¹¹Related to this are results from Romer et al. (1992) that suggest that the scope for budget maximization is larger the larger the population.

outcome of the control group represents the potential outcome of the treatment groups had there been no treatment.¹² In other words, there can be no other factor except for the supplemental grant increase that causes the pre-treatment difference between the control group and treatment groups to change at the time of treatment. 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

According to Bertrand et al. (2004), inference from DID models is more problematic the more time periods are used for estimation. One of the suggested solutions that performs fairly well in their particular application is therefore to ignore time series information and collapse data into two averaged observations—one pre and one post the intervention. For the main part this paper also uses one pre and one post intervention observation, but without collapsing the data. The reason is that by averaging over several years we would be unable to detect interesting dynamics in the treatment response. Such dynamics—investigated in section 4.3—would be present if, for example, it takes time for municipalities to fully adjust their expenditures or to gain a majority in favor of lowering tax rates. The alternative to estimate the model using more years and include municipality-specific trends is then also inappropriate since, as shown by Wolfers (2006), the inclusion of such panel-specific trends can bias the estimate when there are dynamic effects.¹⁴ Therefore, I will estimate equations (2) and (3) using data only on

¹²It may be worth noting that, given heterogeneous response to treatment, the specifications in (2) and (3) identify the average treatment effects (ATE) *on the treated*. 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.

¹³Also 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.

¹⁴The solution in Wolfers (2006) to take dynamic effects into account and control for

years 2001 and 2002.

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,¹⁵ as are 16 municipalities belonging to the autonomous island Åland. 11 municipalities with discrepancies concerning entitlement to the supplemental grant are also dropped. This leaves a balanced panel of 380 municipalities amounting to 2,660 observations for the full sample period 1998–2004 and 760 observations for our main sample period 2001–02.

Summary statistics of the variables used in the empirical analysis are presented for different subsamples in tables 2 and 3—table 2 separates year 2001 from 2002 but pools the three treatment groups, while table 3 separates each treatment group as well as the control group but pools both years.¹⁶ The control group constitutes the majority of observations (657) followed by treatment group 1 (49). Most of the treated municipalities are located in the so-called Suomenselä area and in the northern and eastern parts of the country. As seen in table 2, three of the municipalities in the pre treatment control group we find in one of the treatment groups (group 1) after treatment took place. In addition, two municipalities in treatment group 1 pre

panel-specific trends requires quite long time series. He extends his panel to cover more than 30 years, whereas the full data set available here only covers seven years, which most likely is far too short.

¹⁵Statistics 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.

¹⁶The equivalent of table 3 for the full sample period is found in the Appendix.

treatment switched into treatment group 2 post treatment (not seen in the tables). With only five out of 380 municipalities changing groups we can thus dismiss selection into treatment groups as a severe problem.

The expenditure variable is defined 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).¹⁷ 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 type of 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 expenditures per capita increases monotonically from the control group to the third 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 (see (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 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 changes over time.

As seen from table 3, there are some differences also between the three treatment groups. In particular, group 3 has both higher tax rate and tax base compared to that of group 1 and 2. To the extent that this is evidence of a less elastic tax base among municipalities in the third group, in line with Hamilton (1986) the tax rate response to increased grants optimally ought to be smaller in these municipalities, all else equal. At the same time the level of

¹⁷Most 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.

expenditures as well as total grants are higher in group 3, which should yield a smaller response also in expenditures if there is diminishing marginal utility of public goods and/or decreasing income effect. Hence, the data displays some interesting differences among the treatment groups—differences that do not lead to a clear a priori prediction about the relative size of the effects of grants on the two outcome variables across the three treatment groups, but that rather leave the empirical question open.

The two descriptive tables include two grants variables, namely general grants and total grants. Total grants consist of three main components, and general grants is the component that includes the grant supplement to remotely populated municipalities. In addition to this supplement, general 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 general grants, which in turn was around 10 percent of total grants. However, due to a rather uneven distribution of grants across municipalities this figure is closer to 5 percent overall. Aside from general 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.

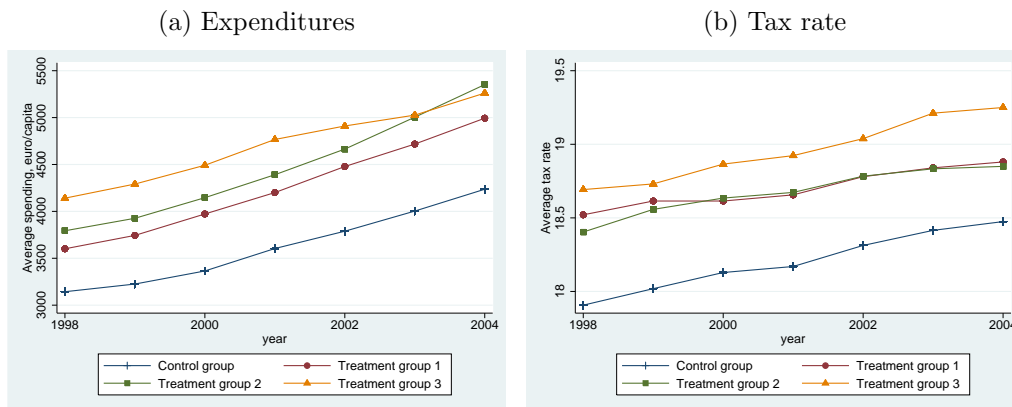
In addition to the three grant components there is a revenue sharing system where tax revenues are (partly) equalized between municipalities. A fixed percentage of the revenue sharing grant or fee is added to or subtracted from each of the three grant components before the final grant is paid to the municipality as a general sum.¹⁸ Whenever there are major regime shifts in the grant system municipalities who are largely affected also get a grant (or pay a fee) that is gradually decreased in order to ease the transition. Such

¹⁸The terminology may be somewhat confusing. Although only one of the grant components are called “general grants”, all of the components are general grants in the sense that they are non-matching and non-targeted.

transitory grants were used between 1997 and 2001 after the implementation of a new grant system in 1997. Finally, as mentioned above, 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 broader view of the evolution over time figure 2 plots expenditures and tax rate for the full sample period. 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 we are unable to visually detect any aggregate effects of increased grants in 2002 for treatment groups 1 and 2. For group 3, however, the growth rate in expenditures seems to decrease around that time. We will get back to this below, but first let us turn to the estimation of the treatment effects.

Figure 2: Average per capita expenditures and tax rate



4 Results

Our first set of results is of the estimated pooled average treatment effect in equation (2), presented in table 4. It shows, in the two respective columns, the estimated effects of a grant increase of one euro per capita on total per capita expenditures and on the income tax rate, with associated standard

Table 2: Summary statistics, by year

	Control group		Treatment groups 1,2,3	
	2001 mean/sd	2002 mean/sd	2001 mean/sd	2002 mean/sd
Expenditures	3603.8 (491.1)	3788.6 (503.8)	4397.1 (493.7)	4637.0 (457.6)
Tax rate	18.17 (0.659)	18.31 (0.641)	18.73 (0.367)	18.84 (0.351)
General grants	27.47 (13.65)	25.90 (23.62)	90.45 (20.90)	144.2 (37.53)
Total grants	740.7 (226.8)	814.8 (248.2)	1173.8 (209.6)	1330.2 (232.7)
Population	13085.1 (38613.0)	13221.1 (38952.0)	4784.4 (3676.8)	4617.8 (3550.3)
Area	422.4 (316.6)	417.1 (309.6)	2680.5 (3056.7)	2585.4 (2995.3)
Remote index	-7.007 (12.14)	-7.231 (12.58)	1.098 (0.415)	1.128 (0.400)
Students	0.115 (0.0256)	0.115 (0.0256)	0.125 (0.0201)	0.123 (0.0191)
Elderly	0.182 (0.0451)	0.184 (0.0453)	0.187 (0.0307)	0.196 (0.0334)
On welfare	0.0689 (0.0245)	0.0666 (0.0233)	0.0997 (0.0266)	0.0923 (0.0270)
Tax base	9746.6 (2072.0)	10092.0 (2076.3)	8010.4 (832.9)	8308.3 (815.6)
<i>N</i>	330	327	50	53

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 (VATT) and the Association of Finnish Local and Regional Authorities

Table 3: Summary statistics, by group

	Control group mean/sd	Group 1 mean/sd	Group 2 mean/sd	Group 3 mean/sd
Expenditures	3695.8 (505.6)	4342.2 (428.4)	4537.2 (375.8)	4838.6 (547.5)
Tax rate	18.24 (0.653)	18.72 (0.384)	18.73 (0.311)	18.98 (0.308)
General grants	26.69 (19.27)	89.95 (16.90)	130.5 (38.36)	157.8 (35.42)
Total grants	777.6 (240.4)	1128.1 (205.7)	1299.1 (174.8)	1443.8 (197.1)
Population	13152.8 (38752.6)	4523.1 (4141.9)	5894.6 (3565.6)	3741.5 (1859.5)
Area	419.8 (312.9)	1068.7 (1046.2)	3164.0 (2987.9)	5003.7 (3779.4)
Remote index	-7.118 (12.35)	0.751 (0.140)	1.229 (0.135)	1.671 (0.143)
Students	0.115 (0.0256)	0.123 (0.0183)	0.126 (0.0210)	0.122 (0.0208)
Elderly	0.183 (0.0452)	0.202 (0.0306)	0.191 (0.0296)	0.171 (0.0293)
On welfare	0.0678 (0.0239)	0.0847 (0.0244)	0.0964 (0.0248)	0.117 (0.0216)
Tax base	9918.5 (2079.8)	7997.3 (711.3)	8101.4 (905.4)	8544.4 (876.8)
<i>N</i>	657	49	28	26

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 (VATT) and the Association of Finnish Local and Regional Authorities

errors that allow for clustering within municipality. From here we conclude that, overall, the effect of increased grants on expenditures is not statistically different from zero. Although the point estimate is reasonably large, the precision is too poor to be able to draw any inference. Regarding the effect on the tax rate quite the opposite is true—this coefficient is estimated with greater precision leading to statistical significance but is small in size implying limited economic relevance. The estimate of -0.0008 means that an increase in grants of 100 euro per capita leads to a decrease of the tax rate with a mere 0.08 percentage points.

Table 4: Results, pooled treatment groups

	Expenditures	Tax rate
$\bar{\tau}$	0.497 (0.366)	-0.000823* (0.000485)
Observations	760	760

Standard errors in parentheses

$\bar{\tau}$ represents the effect of one euro per capita increase in supplemental grants to municipalities in any of the three treatment groups as defined in table 1

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

For reasons discussed above it is also of interest to study the effects on the three treatment groups as defined in table 1 separately. If effects are heterogeneous the results in table 4, while true on average, hide essential parts of how municipalities respond to the grant treatment as well as why they do so. Table 5 consequently presents the estimation results of equation (3). Starting with expenditures in the first column, the estimated effect is positive and statistically significant from zero for treatment group 1 and 2 but not for group 3. The estimates for the first two groups are around 1.6 and 0.9 respectively, both of which can not be rejected to differ from 1. For these groups of municipalities the effect of increased grants on expenditures thus seem highly economically relevant. For the third group, on the other hand, the estimate is not even of expected sign. Furthermore, the p-values from t-tests of equal coefficients presented in the bottom of the table reveal

a significantly different effect for the first and second treatment group compared to that for the third group. That is, the effects of increased grants on municipal expenditures indeed appear quite heterogeneous.

Moving along to the second column and the results on tax rate, the picture is quite different also here. As seen from the estimates that are similar in size to the pooled estimate as well as from the p-values in the bottom of the table, the effects are rather homogeneous across the three treatment groups. Although only statistically significant from zero for treatment group 2, a grant increase of 100 euro affects the tax rate negatively with around 0.05–0.1 percentage points. Again, the size of this effect must be considered modest.

Table 5: Baseline results, separated treatment groups

	Expenditures	Tax rate
τ_1	1.584*** (0.443)	-0.000729 (0.000894)
τ_2	0.891*** (0.343)	-0.00109** (0.000508)
τ_3	-0.683 (0.817)	-0.000479 (0.000919)
$\tau_1 = \tau_2$	0.167	0.665
$\tau_1 = \tau_3$	0.0143	0.840
$\tau_2 = \tau_3$	0.0744	0.546
Observations	760	760

Standard errors in parentheses

τ_1 , τ_2 , and τ_3 represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

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

Thus, allowing for heterogeneous treatment effects we find that increased grants have a large and positive effect on expenditures for two out of three groups but a likely null effect on the third group, and a negligible effect on the tax rate for all three groups. This conclusion is rather different than

what would be drawn from only studying the overall effect of the grant increase when all treated municipalities were lumped together. Interestingly, the pattern is inconsistent with the asymmetric information story told by Filimon et al. (1982) according to which the expenditure effects would be larger, not smaller, the more remotely populated the municipality is (given that people living far from the city center are less informed about the amount of intergovernmental grants received).

4.1 Sensitivity analysis

This section presents various alternatives to the above baseline specification in order to certify that the resulting estimates are the true causal effects of grants. As a first sensitivity check municipalities with remote indices substantially smaller than in the treated groups are excluded from the control group. Recall, first, that in order to receive treatment 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 6, 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, these results indicate 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.

Table 6: Sensitivity analysis, restricting the control group

Remote index	Expenditures		Tax rate	
	> -10	> -5	> -10	> -5
τ_1	1.532*** (0.446)	1.471*** (0.454)	-0.000749 (0.000905)	-0.000514 (0.000917)
τ_2	0.857** (0.346)	0.818** (0.352)	-0.00110** (0.000518)	-0.000949* (0.000524)
τ_3	-0.717 (0.819)	-0.757 (0.822)	-0.000492 (0.000926)	-0.000337 (0.000930)
$\tau_1 = \tau_2$	0.179	0.195	0.671	0.603
$\tau_1 = \tau_3$	0.0152	0.0165	0.836	0.887
$\tau_2 = \tau_3$	0.0746	0.0751	0.546	0.545
Observations	646	514	646	514

Standard errors in parentheses

τ_1 , τ_2 , and τ_3 represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

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

The first candidate to be included in the estimation is the remote index, i.e. the variable that determines the size of the supplemental grant.¹⁹ The resulting estimated effects of grants on expenditures and taxes are presented in the first column of table 7 and 8, 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 that into account would bias the estimates of the effect of increased supplemental grant.²⁰ The third column instead adds various other variables that are likely to be key determinants of expenditures and taxes; per capita tax base, overall population, the share of school-aged children, and the share of elderly. The last column combines the three previous, i.e. adds the remote index, total grants, as well as additional outcome-determinants.

Again, looking at the results in the table gives no strong indications that the baseline estimates are biased in any direction. Adding the remote index does not affect the results at all, whereas total grants and the additional covariates only have a slight effect on the size of the estimates (but increase the standard errors to the extent that the effect on expenditures for the second treatment group is no longer statistically significant in the third and fourth columns)²¹.

4.2 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

¹⁹The remote index was updated by Statistic Finland in 2002, meaning that there still is variation in the remote index after controlling for municipal fixed effects.

²⁰A description of how other types of grants changed in 2002 is found in the Appendix.

²¹The rationale for including additional covariates in an otherwise identified model is usually increased efficiency. For the most part this does however not seem to be the case here.

Table 7: Sensitivity analysis for expenditures, adding covariates

Added variables	Remote index	Total grants	Additional X:s	Remote index, total grants, additional X:s
τ_1	1.587*** (0.446)	1.515*** (0.431)	1.304*** (0.483)	1.251*** (0.477)
τ_2	0.893*** (0.344)	0.697** (0.351)	0.674 (0.425)	0.499 (0.421)
τ_3	-0.680 (0.819)	-0.645 (0.691)	-0.774 (0.813)	-0.715 (0.697)
$\tau_1 = \tau_2$	0.168	0.104	0.253	0.172
$\tau_1 = \tau_3$	0.0145	0.00722	0.0201	0.0120
$\tau_2 = \tau_3$	0.0748	0.0760	0.0936	0.104
Observations	760	760	756	756

Standard errors in parentheses

τ_1 , τ_2 , and τ_3 represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

Other X:s are: Per capita tax base, population, share of school-aged children, and share of elderly

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

Table 8: Sensitivity analysis for tax rate, adding covariates

Added variables	Remote index	Total grants	Additional X:s	Remote index, total grants, additional X:s
τ_1	-0.000650 (0.000905)	-0.000689 (0.000900)	-0.00112 (0.000968)	-0.000981 (0.000983)
τ_2	-0.00105** (0.000513)	-0.000976* (0.000513)	-0.00138** (0.000569)	-0.00118** (0.000573)
τ_3	-0.000456 (0.000922)	-0.000501 (0.000923)	-0.000856 (0.000947)	-0.000891 (0.000965)
$\tau_1 = \tau_2$	0.629	0.734	0.751	0.821
$\tau_1 = \tau_3$	0.876	0.881	0.836	0.944
$\tau_2 = \tau_3$	0.556	0.648	0.604	0.788
Observations	760	760	756	756

Standard errors in parentheses

τ_1 , τ_2 , and τ_3 represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

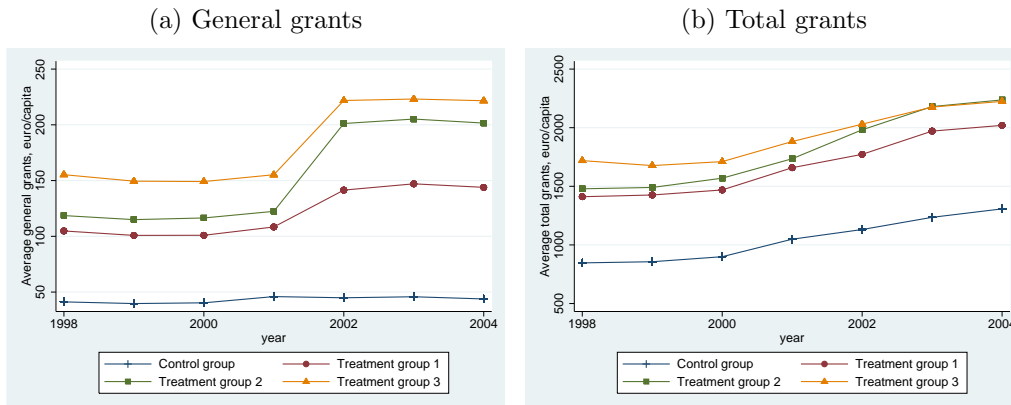
Other X:s are: Per capita tax base, population, share of school-aged children, and share of elderly

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

increased supplemental grants. If we instead define treatment to be increased general grants (i.e. the type of grant that the supplement is part of) or even increased total grants (of which general grants subsequently is part), the policy-induced variation may be seen as allocating treatment in an imprecise way, making it suitable for a two stage least square (2SLS) estimation.

Figure 3 illustrates how general and total grants have evolved over the sample period. Since the supplemental grant constitute around 80 percent of general grants to municipalities in the treatment groups it is not surprising that the policy in 2002 yielded an increase of a similar magnitude in general 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 changes except for group 2, whose grants increase to the level of group 3 in 2002.²²

Figure 3: Average general grants and total grants



In the alternative identification and estimation strategy, figure 3 is the graphical equivalent of the following first stage equation, in which general grants or total grants, $grants_{it}$, are instrumented with the supplemental grant:

²²In connection to this it is worth recalling from previous section that controlling for total grants did not affect the estimates.

$$\begin{aligned}
grants_{it} &= \gamma_1 D_1 supplement_{it} + \gamma_2 D_2 supplement_{it} + \gamma_3 D_3 supplement_{it} \\
&+ \mu_i + T_t + e_{it}
\end{aligned} \tag{4}$$

Using the predicted values, \widehat{grants}_{it} , from (4), we then recover the pooled estimate of the effect of increased grants in the second stage:

$$y_{it} = \bar{\tau}^{2SLS} \widehat{grants}_{it} + \mu_i + T_t + \varepsilon_{it} \tag{5}$$

In the context of 2SLS, the results from above is thus the reduced form estimates. 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.²³ Hence, with 2SLS as an alternative estimation strategy we in some sense have an additional robustness check. Note, however, that this strategy only identifies the overall average effect of the policy reform, meaning that it will not be possible to make any strong statements about the extent of heterogeneity. As seen in (4), the three treatment groups are instead separated into three distinct excluded instruments. This allows for testing the model with the Hansen J overidentification test, where the null hypothesis is that several instruments yield the same second stage estimate. Given the assumption that the source of variation generated by the policy is exogenous, a rejection of the null is therefore indicative of heterogeneous treatment effects.

First stage estimates of the γ_k :s from equation (4) are presented in table 9,

²³The insight from Imbens and Angrist (1994) is that IV estimators identify a weighted local ATE, with 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.

with $grants_{it}$ defined as general grants and total grants in the two respective columns. Looking at the first column the priors that the supplemental grant ought to be highly correlated with general grants is verified. The point estimates for all three treatment groups are essentially one, with a partial F statistic for the excluded instruments as high as 2376. From the second column we note that the standard errors in the regression of total grants are more than ten times the size of those for general grants, and that the estimate of the supplemental grant increase for the third treatment group is insignificant (as suggested by the graphical representation). But also for total grants is the F statistic still well above conventional significance levels.

Table 9: 2SLS, first stage estimates

	General grants	Total grants
γ_1	0.981*** (0.0164)	1.282*** (0.260)
γ_2	1.033*** (0.0130)	1.791*** (0.201)
γ_3	0.934*** (0.0451)	0.844 (0.682)
1 st stage F-stat.	2376.4	31.13
Observations	760	760

Standard errors in parentheses

γ_1 , γ_2 , and γ_3 represent the partial correlation of supplemental grants with general and total grants for municipalities in the three respective treatment groups as defined in table 1

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

The subsequent second stage estimates are given in table 10. Comparing with the estimated pooled treatment effects and with what we know about the heterogeneity from the previous analysis, these results provide essentially the same picture. The point estimates are similar to those in table 4, and the Hansen J test suggests that there are heterogeneous treatment effects on expenditures but not on taxes. This is true both when instrumenting for general grants and total grants.

We conclude from this that the alternative 2SLS estimation supports the original standard DID. The baseline results that increased grants may stimulate expenditures considerably but that this effect is likely to be heterogeneous across different types of municipalities, and that the effect of increased grants on the tax rate is homogeneous and negligible, seem convincing.

Table 10: 2SLS, second stage estimates

Grant treatment	Expenditures		Tax rate	
	General	Total	General	Total
$\bar{\tau}^{2SLS}$	0.531 (0.352)	0.450** (0.189)	-0.000843* (0.000480)	-0.000599** (0.000296)
Hansen J p-value	0.0731	0.0442	0.835	0.998
Observations	760	760	760	760

Standard errors in parentheses

$\bar{\tau}^{2SLS}$ represents the effect of one euro per capita increase in general or in total grants to municipalities in any of the three treatment groups as defined in table 1

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

4.3 Dynamic responses

The policy-induced increase in the supplemental grant in 2002 was not just temporary. That means that the municipalities that increased their spending due to the increased grants did not have to cut back down the following years. It is on the contrary likely that the adjustment to a larger budget is not immediate, but that it takes time to decided where to spend or what the new preferred level of taxation is. In order to investigate this the following dynamic response equation is estimated:

$$Y_{it+j} = \tilde{\tau}_1 D_1 supplement_{it} + \tilde{\tau}_2 D_2 supplement_{it} + \tilde{\tau}_3 D_3 supplement_{it} + \mu_i + T_{t+j} + \epsilon_{it+j}, \quad (6)$$

where $\tilde{\tau}_k$ is the effect for a municipality in treatment group k of increased supplemental grants in year t on expenditures or on the tax rate j years ahead in time.

Since data only extends to 2004 equation (6) is estimated for $j = \{1, 2\}$, and the first two columns of table 11 and 12 show the results for expenditures and tax rate, respectively. Similarly to the immediate effects the dynamic response in the tax rate is modest. For the second group the negative estimate of the effect after two years is statistically significant but as small as -0.00056. Expenditures are however largely affected one year after the grant increase (but due to large standard errors we can not conclude anything about the effects on expenditures after two years). In fact, for treatment group 1 and 2 the estimated increase after one year is about as large as the immediate effect. Although not very precisely estimated, this suggest that expenditures are increased far more than the amount of the grant increase for these groups. This may seem irrational, but such a response is in principle possible since Finnish municipalities do not have a balanced budget requirement but are allowed to take up loans. Perhaps even more puzzling is the suggested behavior of the third group, for which the expenditure effect that in previous estimations consistently has been negative but not statistically insignificant is actually highly statistically significant one year after the grant increase.

Given that we believe in these results, how are we to understand them? One the one hand, municipalities whose expenditures respond positively to a grant increase do so to a greater extent than what seems rational, whereas other municipalities respond by decreasing their expenditures. A suggested common feature is that these municipalities are quite path-dependent. That is, not only do the expanding municipalities get accustomed to a larger size but also to a faster growth rate of the budget. Such behavior would explain the results for the first two groups of municipalities. But what about the third group? The graphics from section 3 illustrate that this group of municipalities spent considerably more than the others prior to the policy reform in 2002, but that by 2003 their level of expenditures was the same as that of the second group. Although it is of course possible that this relative decrease

is due to something else entirely, there is one mechanism (admittedly a bit farfetched) through which part of this decrease could be an effect of the 2002 policy reform. Namely, if municipalities belonging to the third group perceived the treatment of increased grants as a signal that they had been over-spending, this could have encouraged them to turn the long-run trend of increasing expenditures around.

Table 11: Dynamic responses in expenditures

Response year	2003	2004	2001
$\tilde{\tau}_1$	0.918* (0.482)	2.090 (2.523)	0.388 (0.491)
$\tilde{\tau}_2$	1.332* (0.736)	1.286 (0.871)	-0.0519 (0.345)
$\tilde{\tau}_3$	-1.360*** (0.504)	0.0542 (0.844)	0.509 (0.447)
$\tilde{\tau}_1 = \tilde{\tau}_2$	0.597	0.730	0.392
$\tilde{\tau}_1 = \tilde{\tau}_3$	0.000933	0.436	0.854
$\tilde{\tau}_2 = \tilde{\tau}_3$	0.00246	0.298	0.312
Observations	760	760	760

Standard errors in parentheses

$\tilde{\tau}_1$, $\tilde{\tau}_2$, and $\tilde{\tau}_3$ represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

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

One must also recognize the possibility that the identifying assumption about common trends fails to hold for the third treatment group, implying that we should not put too much weight on those estimates. In such case the causal interpretation of the other results must also be questioned. However, the rightmost column of tables 11 and 12 sheds some positive light on this matter. It shows estimates of a different type of “dynamic” effect, namely of effects on expenditures and taxes backwards in time, or in terms of equation (6), for $j = -1$. If the effects in the years of or after the policy truly represent effects of the policy and not some other systematic difference between

Table 12: Dynamic responses in tax rate

Response year	2003	2004	2001
$\tilde{\tau}_1$	-0.000986 (0.000628)	-0.000519 (0.000694)	-0.0000969 (0.000518)
$\tilde{\tau}_2$	-0.000755 (0.000540)	-0.000560** (0.000220)	-0.000140 (0.000365)
$\tilde{\tau}_3$	0.000975 (0.00123)	-0.000295 (0.000528)	0.000222 (0.000570)
$\tilde{\tau}_1 = \tilde{\tau}_2$	0.718	0.948	0.931
$\tilde{\tau}_1 = \tilde{\tau}_3$	0.146	0.792	0.665
$\tilde{\tau}_2 = \tilde{\tau}_3$	0.190	0.628	0.577
Observations	760	760	760

Standard errors in parentheses

$\tilde{\tau}_1$, $\tilde{\tau}_2$, and $\tilde{\tau}_3$ represent the effect of one euro per capita increase in supplemental grants to municipalities in the three respective treatment groups as defined in table 1

The bottom panel shows p-values for tests of equal coefficients

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

the treated and control municipalities one would expect a null effect on the outcome variables prior to the reform (unless there are anticipatory effects). It is therefore comforting to see that this is the case.

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 three groups of remotely populated municipalities in Finland with increased grants while leaving a fourth group serving as controls untreated.

One 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. Concerning the effects on expenditures the analysis is less conclusive. The overall effect of the policy was estimated to be reasonably large, but with too poor precision to allow for any concise inference. However, when studying heterogeneous effects across the three groups of treated municipalities we found an immediate one-to-one correspondence between grants and expenditures for two out of three groups but a likely null or even negative effect for the third group, perhaps because of diminishing marginal utility of public goods.

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 effects estimated for two of the groups can thus be interpreted as crowding-in effects, whereas grants to the third group seem to have a crowding-out effect. Of these contradictory results the former are in line with Dahlberg et al. while the latter are in line with Knight (2002). But parts of the analysis have suggested that the causal interpretation may be more valid for the first two groups than for the last. If this is the case the common effects of general grants to Finnish municipalities as found here

and to Swedish municipalities as found in Dahlberg et al. are likely to be externally valid also to other countries. 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.

If, on the other hand, the diverging expenditure effect for the third group is consistently estimated, such heterogeneity implies that we are not ready to put these questions at rest. Rather, more research that convincingly estimates causal effects of general grants to other types of municipalities is then needed.

Our look at the dynamic effects revealed the somewhat peculiar pattern that municipalities whose expenditures immediately responded positively to a grant increase 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 call for future studies looking at effects of increased grants on borrowing.

Although the main focus in this paper has been to give a convincing picture of *how* local governments respond to increases in grants, the assessment of the extent of heterogeneous effects has also provided some tentative guidelines as to *why*. In particular, the estimated pattern of expenditure effects is inconsistent with the mechanism behind the flypaper effect proposed by Filimon et al. (1982) involving poorly informed voters. In other words, to the extent that two of the treatment groups display flypaper behavior we must seek elsewhere for a reason behind that. “Separate mental accounting”—i.e. that voters treat the government budget constraint separately from their own—is an explanation that many attribute to Hines Jr and Thaler (1995) but that often is dismissed as unlikely. 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

Here 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 a 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 need to 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. It was decided that any negative consequences of this removal could entitle to the extra financial aid due to extraordinary circumstances (were the municipalities to apply for that). Controlling for the total amount of transitory grants and extra aid received 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. Since the amount of deductions varied considerably across regions but the repayments were the same, this made it more difficult to keep stable finance 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 relations was a decrease in the municipalities' share—and thereby an increase in the state's share—of revenue 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 sys-

tem 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.

B Summary statistics

Table 13: Summary statistics, full sample period, by group

	Control group mean/sd	Group 1 mean/sd	Group 2 mean/sd	Group 3 mean/sd
Expenditures	3622.1 (625.3)	4252.5 (625.1)	4501.5 (653.8)	4698.5 (630.6)
Tax rate	18.20 (0.684)	18.70 (0.432)	18.69 (0.349)	18.96 (0.419)
General grants	27.08 (18.58)	88.15 (15.87)	125.4 (36.50)	153.7 (32.90)
Total grants	778.8 (264.6)	1124.4 (246.4)	1297.9 (239.6)	1441.9 (237.1)
Population	13120.5 (38451.4)	4559.1 (4143.5)	6019.7 (3579.5)	3799.1 (1862.7)
Area	420.0 (313.3)	1067.8 (1039.5)	3188.2 (2954.5)	5004.2 (3723.4)
Remote index	-7.102 (12.31)	0.749 (0.138)	1.227 (0.133)	1.670 (0.142)
Students	0.115 (0.0256)	0.124 (0.0200)	0.127 (0.0213)	0.122 (0.0202)
Elderly	0.181 (0.0450)	0.197 (0.0302)	0.185 (0.0306)	0.164 (0.0294)
On welfare	0.0709 (0.0259)	0.0880 (0.0271)	0.0978 (0.0263)	0.122 (0.0280)
Tax base	9614.6 (2135.9)	7778.9 (936.3)	7953.3 (1010.7)	8356.5 (1006.4)
<i>N</i>	2301	171	97	91

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

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