Granting Public or Private Consumption? Effects of Grants on Local Public Spending and Income Taxes*

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Most of the early empirical estimates on effects of intergovernmental grants contradict theoretical predictions. In the more recent literature that emphasizes the importance of convincing empirical strategies, the results are more mixed. This paper contributes to this literature by estimating causal effects on local expenditures and income taxes of general, unconditional grants. This is done in a difference-in-difference model utilizing policy-induced increases in grants to a group of remotely populated municipalities in Finland. The finding is that increased grants have a statistically and economically significant positive immediate effect on local expenditures. The effect on local income taxes, while statistically significant, is considerably smaller in magnitude. Furthermore, there is no evidence of dynamic crowding-out—i.e., that the immediate response in expenditures is reversed in later years.

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

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1 Introduction

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

Although they are widely used, interestingly, theory speaks against grants having the intended effect. Central policy makers who increase grants to local governments usually do so with the more or less explicit aim to stimulate the local public sector rather than to finance tax cuts. But theoretically, equivalent to a tax base increase, intergovernmental grants are predicted to induce a pure income effect and should therefore affect expenditures according to the overall marginal propensity to spend on public goods and services, i.e. with around 15–20% for most countries.[1] Since this implies that the majority of a grant increase is either spent in other than the intended area or substituted for other sources of revenue, grants according to these theoretical models are said to have a *crowding-out effect* on spending. On the contrary, early empirical estimates were in fact in line with most policy-makers' intentions, suggesting a larger stimulatory effect on expenditures than what would be predicted by theory. It seemed that the money stuck where it first hit, which is why this apparent crowding-in effect was dubbed the "flypaper effect".[2]

So although policies aiming to finance local public goods and services cannot easily be backed up by theoretical arguments, their prevalence is perhaps not surprising, as the empirical literature at least partly still supports its usage. The aim of this paper is to contribute to this literature with convincing estimates of effects of grants on local expenditures and income taxes. The estimates presented are the result of a thorough empirical identification strategy, and are therefore likely to be causal. By utilizing policy-induced increases in intergovernmental grants to a group of municipalities in Finland, in the baseline model I identify and estimate causal effects of grants in a difference-in-difference (DID) approach. Additionally, by exploiting a discontinuous structure of the grant that was increased, in a robustness check I am able to estimate the effects in a combined DID and

[1] These predictions vary depending on the type of grant (e.g., conditional or unconditional). And while the predictions also depend on the model use, in general they seem quite robust to various assumptions. For example, 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.

[2] Surveys of the literature on effects of intergovernmental grants include, e.g., Gramlich (1977); Bailey and Connolly (1998); Hines Jr and Thaler (1995); and Inman (2008).

regression discontinuity (RD) approach. And because the grant increase lasted for several years, I can also analyze the dynamics in the response to investigate whether it occurs with a lag or, alternatively, if immediate responses are reversed in later years.

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

One of the explanations that has been suggested for the apparent flypaper effect is simply that it is not real but a mere statistical artifact (Becker, 1996).[3] It is therefore imperative that empirical studies solve the identification problem properly. This means that researchers are required to isolate variation in grants that is *exogenous* to the outcome. 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 (for example, a large share of elderly) typically receive more grants exactly because they need to be spending more. Therefore, it is highly likely that perceived relations between grants and expenditures simply reflect such needs. A tempting remedy for this inherent endogeneity problem is to control for all characteristics that determine expenditures in a regression analysis. However, depending on the design of the grant system, such an approach would typically kill all variation in grants. A more promising remedy is therefore to closely study how grants are determined and search for experimental-type features where the amount of grants varies but the underlying needs do not—that is, the strategy aimed for here.

The study complements a rather recent empirical literature that has investigated the flypaper effect with this same type of experimental-type approach. The specific context in which previous studies have identified causal variation in grants varies. A few have in common with this study that they looked at broad economic outcomes such as total spending and taxes (Dahlberg et al., 2008; Lutz, 2010; Litschig, 2012). Others have focused on specific types of expenditures and whether or not targeted grants

[3]Other explanations highlight the political aspect of grants (Filimon et al., 1982), or higher marginal costs of public funds with decentralized taxation leading to efficiency gains when grants, rather than local taxes, are used finance local expenditures (Hamilton, 1986).

stick to the specific sector as intended (Knight, 2002; Gordon, 2004; Evans and Owens, 2007; Brooks, 2008; Singhal, 2008). All these studies provide mixed evidence; some find support for the flypaper effect, while others do not. It thus seems as whether increased grants are used as intended and do indeed stimulate the public sector, or whether they finance increased private consumption (via decreased taxes), is quite context-dependent.

The robust finding of this paper is that following a grant increase, there is a positive immediate response in local expenditures. This effect is both statistically and economically significant. While there is also a statistically significant negative effect on local income taxes, the size of this response is of an order of magnitude smaller. Specifically, for one of the groups of treated municipalities, expenditures increased by around 65 euro per capita as a result of the reform, whereas own-source revenues were only cut with 8 euro per capita. Furthermore, there is no evidence that the immediate response in expenditures was reversed in later years.

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

The remainder of the paper goes as follows: The next section describes the data and its variables. Section 3 describes the particular grant supplement subject to the policy reform in 2002 and how the reform makes it possible to circumvent the grant endogeneity problem. Section 4 presents the baseline results accompanied by a thorough robustness check to investigate (i) the validity of the identifying assumption of parallel trends by controlling for, among other things, other simultaneous policy implementations; and (ii) the robustness to an alternative identification strategy that combines the DID approach with an RD design. While it is outside the scope of the paper to provide a complete and fully convincing answer not only *how* grants affect local governments but also *why*, Section 5 briefly looks in the data for evidence for two different potential mechanisms. Finally, Section 6 briefly concludes the paper.

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

2 Descriptive data

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

The original data gathered for the paper consists of a seven-year panel between 1998 and 2004 of all Finnish municipalities. From this data, the main sample restrictions are that 52 municipalities that were consolidated with another around this period are dropped,[5] as are 16 municipalities belonging to the autonomous island of Åland, and 11 municipalities with discrepancies concerning entitlement to the supplemental grant. For reasons to be discussed below, the 13 most remotely populated municipalities 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 1 for different subsamples—for the treatment and control group, separately pre and post treatment. The table also provides the t-statistics from tests for pre-treatment differences between the treatment and control group. The details of the difference-in-difference strategy are explained in the next section, but for now, it may be noted that treatment is defined based on an index measuring how remotely populated the municipality is (the higher the remote index, the more remote is the population). In particular, in 2002, only municipalities with a remote index of at least 0.50 were treated with increased grants. As seen from the table, with 330 municipalities in the pre-treatment period, the control group constitutes the majority of observations. Most of the treated municipalities are located in the mid and especially mid-eastern parts of the country. The table also shows that three of the municipalities in the pre-treatment control group belong to the treatment group after treatment took place. Thus, with only 3 out of 367 municipalities changing groups, selection into treatment is hardly a severe problem.

The expenditure variable in the top of Table 1 is one of the two outcomes of interest. It is defined in per capita terms net of investments, and the largest shares go to social services and health care (on average around 50%) and education and culture (around 25%). The largest single item of expenditure is wages to municipal employees (around 30%).[6]

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

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

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 amounted to around 45% of total revenue, while the corresponding percentage for property and corporate income taxation was merely around 3 and 6, respectively. Tax *rates* on private income and properties are set locally whereas the rate of taxation of corporate income is centralized. All tax *bases* are also set centrally. The tax rate on private income is thus the most important tax instrument under local discretion, and as such, is the other outcome of interest. To facilitate the interpretation of the results, the two outcomes will be expressed in comparable measurement units. Therefore, the effects on tax rates will be evaluated in terms of tax revenues, and in particular in terms of tax revenues keeping the tax base fixed at the pre-treatment (2001) level. Table 1 presents both these variables—i.e., the tax rate on private income and the constructed tax revenues.[7]

Not too surprisingly, Table 1 reveals differences between treated and controls in many of the variables. Of the outcome variables, especially expenditures are higher in the treatment group, whereas tax rates do not seem to vary to any great extent. By the nature of the remote index that defines the treatment groups (see the formula in (1) below), it is not surprising that municipality area is considerably larger in the treatment group, since larger municipalities naturally have more people living far from the city center. Population size is also notably smaller. Despite these cross-sectional differences, from an identification point of view it is promising that—aside from the outcome and grants variables—there are no large differential changes over time. For example, the table shows similar decreases in corporate tax revenues in the post-treatment period among treated and control municipalities (explained by the decrease in the share accruing to the municipalities from 37.25 to 24.09%; see Section 3 and the Appendix). Still, to ascertain that the effects of increased supplemental grants are not confounded by other factors, the empirical analysis presents regressions that control for relevant variables from Table 1.

The table provides statistics for two categories of grants; generic grants and total grants. These are the broader grant categories to which the grant given to remotely populated municipalities belong.[8] During the period studied, the grant system was structured so that total grants consisted of three

health sector.

[7] Unless indicated otherwise, from hereon *tax revenues* refer to this constructed variable. The reason for not studying actual tax revenues is that only the tax rate is under local discretion. The tax base, on the other hand, reflects individual labor supply decisions and is therefore quite variable. This makes actual tax revenues a much noisier variable.

[8] As shown in Section 4.2, the policy-induced increase in the grant to remotely populated municipalities indeed induced corresponding increases in the broader grant categories. Whether or not the increase was sufficiently large to yield any behavioral response is then, of course, an empirical question.

Table 1: Summary statistics

	Treatment group		Control group		Difference
	1998–2001 mean/sd	2002–04 mean/sd	1998–2001 mean/sd	2002–04 mean/sd	1998–2001 t-stat
Expenditures	4094.3	4728.0	3461.6	3921.7	11.20
	(346.5)	(534.6)	(491.0)	(550.9)	
Tax rate	18.59	18.83	18.06	18.40	7.43
	(0.428)	(0.327)	(0.689)	(0.626)	
Tax revenues	1465.6	1482.2	1753.5	1790.9	-9.69
	(142.9)	(143.7)	(335.2)	(342.1)	
Generic grants	83.60	125.1	29.15	25.24	33.44
	(9.623)	(27.84)	(14.36)	(23.01)	
Total grants	1575.9	1956.8	947.1	1197.9	14.41
	(246.7)	(278.6)	(399.7)	(492.4)	
Population	5288.9	4839.7	13010.0	13269.2	-3.50
	(4140.3)	(3832.1)	(38034.3)	(39024.6)	
Area	1864.3	1799.5	422.3	417.0	3.92
	(2240.4)	(2172.8)	(316.3)	(309.5)	
Remote index	0.901	0.948	-7.007	-7.231	11.81
	(0.267)	(0.267)	(12.12)	(12.57)	
School-aged children	0.129	0.120	0.115	0.113	3.84
	(0.0204)	(0.0199)	(0.0259)	(0.0256)	
Elderly	0.192	0.215	0.180	0.188	2.11
	(0.0300)	(0.0325)	(0.0447)	(0.0461)	
Welfare recipients	0.0994	0.0818	0.0753	0.0649	5.59
	(0.0263)	(0.0251)	(0.0270)	(0.0232)	
Income tax base	7670.9	8219.1	9454.1	10075.0	-10.14
	(840.5)	(741.9)	(2089.3)	(2004.5)	
Corp. tax revenues	347.3	230.3	332.5	250.1	0.67
	(121.7)	(149.7)	(175.6)	(213.9)	
Observations	148	120	1320	981	1468
Municipalities	37	40	330	327	367

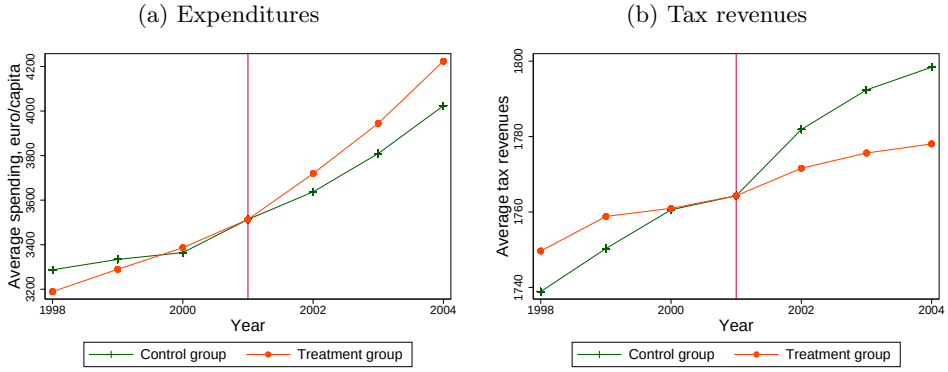
Note: Expenditures, grants, and tax base are in euro per capita deflated to 2001 year values, tax revenues are defined as the tax rate times the 2001 tax base, school-aged children, elderly and welfare recipients are in shares of overall population and area is in square kilometers. Elderly and welfare recipients contain 105 and 14 missing values, respectively. Corporate tax revenues are not available for the years 1998–2000. For the years 1998–2001, the exchange rate 1 euro = 5.94573 Finnish marks is used.

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

main components; (i) generic grants (around 5%); (ii) so-called sector grants to social services and health care (68%); and (iii) sector grants to education and culture (27%). Further, the first component—generic grants—was comprised of the subcomponents grants to remotely populated municipalities (i.e., the grant supplement studied here), to archipelago municipalities, to urban municipalities and to bilingual municipalities as well as a general per capita grant given to all municipalities (below referred to as the base grant). For the average municipality in the country, all these grants amount to around 15–20% of total revenue. Note, though, that the distribution of grants to municipalities is quite unequal, implying that the importance of the different grants varies considerably.

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

Figure 1: Average per capita expenditures and tax revenues



Note: For the treatment group, the variables are scaled to equal the control group in 2001.
Source: Government Institute for Economic Research.

Table 1 shows slight increases in both outcome variables between the pre- and post-treatment period, both for treated and control municipalities.[9] In

[9]The increase in per capita expenditures is around 630 euro in the treatment group and 460 euro in the control group. The corresponding increases in per capita tax revenues are 17 and 37 euro. In a formal means test, all these four increases are statistically significant at the 1% level.

order to get a more complete view of the evolution over time, Figure 1 plots yearly averages of expenditures and tax revenues in the treatment and control group. To improve the visual comparison between the two groups, the variables are scaled for the treatment group so that the difference from the control group in 2001 (the year before the treatment) is zero. The overall picture is a positive trend in both variables. Immediately before the year of the treatment of increased grants in 2002, the trends run parallel, but less so further back in time. After the treatment, expenditures increase more for the treatment group while taxes increase less, thus suggesting a positive effect of grants on expenditures and a negative effect on tax revenues. The section that follows now explains how these suggested effects can be estimated econometrically and the conditions under which they can be interpreted causally.

3 Identifying causal effects of grants: A difference-in-difference approach

Let us begin the section with a description of the supplemental grant given to remotely populated municipalities and the policy in 2002 that enables identification of causal effects of intergovernmental grants in a DID approach. The supplemental grant is given to municipalities where few inhabitants live close to the city center but rather have their population remotely located. In order to decide which municipalities that qualify for the grant supplement, every fifth year starting in 1997, Statistics Finland has assigned a remote index to each municipality according to the formula:[10]

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

where pop^{25km} and pop^{50km} are the population within a 25 and 50 kilometer radius from the municipal center, respectively. As is apparent from (1), the remote index can range from negative values to +2, where +2 corresponds to a situation where the entire population lives outside the 50 kilometer radius. In 1997–2005, the supplemental grant was distributed based on this index as described in Table 2 and illustrated in Figure 2.[11] 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

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

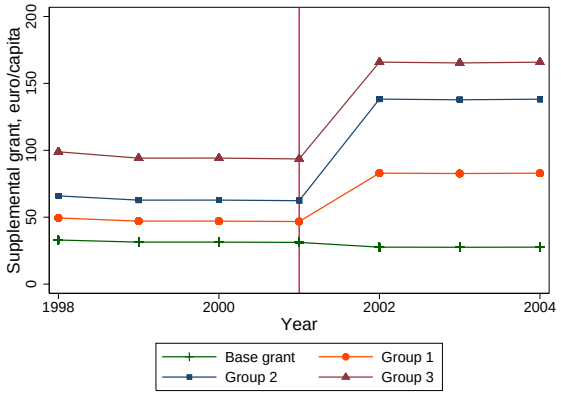
[11] In 2006, a new grant system came into place where this as well as many other grant types were changed considerably, but due to lack of data, the figure only illustrates how the supplemental grant was distributed during 1998–2004.

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 2, during 1998–2004 the size of the base grant varied around 30 euro per capita.

Table 2: Distribution of the supplemental grant

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

Figure 2: The supplemental grant



Source: Government Institute for Economic Research.

The sharp increase in the supplemental grant in 2002 seen in Figure 2 is due to a policy reform.[12] Relative to the base grant, the reform doubled the supplemental grant for groups 1 and 3, and more than doubled the grant for group 2. To finance these supplemental increases, the base grant—which *all* municipalities received—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. As to the general importance of the supplemental grant, among the receiving municipalities it made up 70–80% of the broader grant category generic grants, which, in turn, was around 10% of total grants.

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

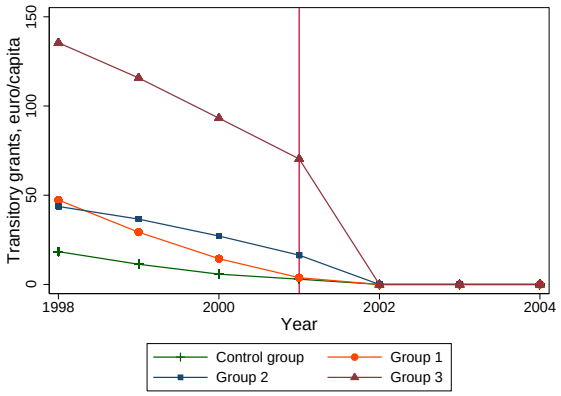
The supplemental grant increase was part of a group of policy reforms implemented in 2002 motivated by the fact that economic conditions varied across municipalities despite rather stable finances for the country in general. The overall aim of these policy reforms was to stabilize the local government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

As part of an overall reform of the grant system, the launch of the supplemental grant in 1997 coincided with other changes in the grant distribution. Municipalities that were highly affected by this grant reform were compensated with transitory grants that were gradually phased out through 2001 and were entirely removed in 2002. Among other things, the previous grant system had put more weight on large areas than did the 1997 system and thus, large municipalities received larger amounts of transitory grants. Because having a remotely located population is correlated with a large area, the coinciding removal of the transitory grants reduces any potential effects of the supplemental grant increase in 2002 for the most remotely populated municipalities. As can be seen in Figure 3, plotting transitory grants separately for the same three groups as in Figure 2 along with a control group consisting of municipalities with remote indices below 0.50, the problem is especially apparent for group 3. In fact, for the 13 municipalities in this group, the average decrease in transitory grants just about equals their supplemental grant increase. For groups 1 and 2, however, the size of the transitory grant decrease is more modest. Motivated by this, the empirical analysis will focus on municipalities in these two groups (which in 2001 consisted of 24 and 13 municipalities, respectively).[13]

The particular policy-induced increases displayed for groups 1 and 2 in Figure 2 will be used in a DID strategy to identify causal effects of grants on municipal expenditures and on local income tax rates evaluated in terms

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

Figure 3: Transitory grants



Source: The Association of Finnish Local and Regional Authorities.

of tax revenues.[14] Because all grants are added up and paid to the municipalities with no strings attached, increased supplemental grants also mean increased generic grants (i.e., the type of grant that the supplement is part of) as well as increased total grants (of which generic grants subsequently are part). In addition to estimating the reduced form effect of the policy-induced increase, this grant structure makes it possible to estimate the effect of increases in these broader grant categories in a two-stage procedure. Although the exogenous variation used for identification is the same, the two different specifications—described in detail below—partly yield different insights.

3.1 Reduced form specification

In the reduced form framework, treatment is defined as the changes (increases) in supplemental grants to municipality i, ΔSG_i, that occurred in 2002. The treatment group is thus comprised of municipalities with remote indices in the range 0.5–1.5, and the control group accordingly consists of municipalities with remote indices smaller than 0.50 that never received this particular grant. A two-period DID model that identifies the effect of ΔSG_i on the total changes between the pre- and post-treatment period in either of the outcome variables per capita expenditures or tax revenues is then

$$\Delta Y_i = \tau \Delta SG_i + T_{post} + \varepsilon_i, \tag{2}$$

$$\text{with } \Delta Y_i = \overline{Y}_{i,post} - \overline{Y}_{i,pre} = \sum_{t=2002}^{2004} Y_{i,t}/3 - \sum_{t=1998}^{2001} Y_{i,t}/4.$$

[14] Recall from the previous section that only the tax *rate* is under local discretion and that unless indicated otherwise, *tax revenues* refer to the constructed variable measuring tax revenues keeping the tax base fixed at the pre-treatment level.

T_{post} controls for changes in outcomes common to all municipalities, and ε_i is the error term. Since $SG_i = 0$ for all control municipalities and $SG_i > 0$ for all treated municipalities, the parameter τ captures how much a euro per capita increase in SG caused Y to change between the pre- and post-treatment period (in total).

It is also of interest to see in which year(s) the effect took place.[15] The supplemental grant increase in 2002 was not a temporary increase. That means that municipalities that, say, used the extra grants to increase spending did not have to cut back in the following years. On the contrary, one possibility is that adjustment to a larger budget is not immediate but that it takes time to decide where to spend, suggesting positive effects should also be expected in subsequent years. Or, alternatively, jurisdictions may over time substitute increased grants with own-source revenues, which would imply negative effects in later years.[16] In order to investigate these dynamics, the following model breaks down the total effect of grants, τ, into separate effects in different years, τ_t:

$$\Delta Y_{i,t} = \tau_{2001}\Delta SG_i + \tau_{2002}\Delta SG_i + \tau_{2003}\Delta SG_i + \tau_{2004}\Delta SG_i + T_t + \varepsilon_{i,t} \quad (3)$$

For $t \in [2001, 2004]$, each of the parameters τ_t represents the incremental effect of ΔSG_i between year t and year $t-1$.[17] Because the supplemental grant increase ΔSG_i took place in year 2002, τ_{2002} represents the immediate effect on $\Delta Y_{i,t}$, whereas τ_{2003} and τ_{2004} represent the *additional* effects one and two years later. Finally, τ_{2001} captures the "effect" of ΔSG_i one year before the treatment actually took place, whose estimate is a test of the identifying assumption (its expectation is zero if the assumption holds).[18]

For the treatment effects in equations (2) and (3) to be identified, it is required that, conditioning on the differences prior to the grant increase in 2002, the outcome of the control group represents the potential outcome of the treatment group had there been no treatment.[19] In other words, there can be no other factor except the supplemental grant increase that causes

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

[16]See Gordon (2004).

[17]Note that, just as τ in equation (2) is a parameter for the *total* change between the pre- and post-treatment period, τ_{2001}–τ_{2004} are parameters for the specific *annual* changes. Technically, the year-specific estimates are obtained by interacting SG_i with year dummies.

[18]In the estimation of equations (2) and (3), the standard errors are robust to heteroskedasticity and, in equation (3) which has more than one time period, to within-municipality serial correlation (i.e., the standard errors are clustered at the municipality level).

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

the pre-treatment difference between the control group and the treatment group to change at the time of treatment (or within two years after treatment for the dynamic effects τ_{2003} and τ_{2004}). This is the maintained identifying assumption of parallel trends.

Since the treatment was targeted towards remotely populated municipalities and hence was not random, it is a priori not obvious that the assumption of parallel trends should hold. As mentioned above, an insignificant estimate of τ_{2001} capturing differences in pre-treatment trends strengthens the assumption that also the counterfactual post-treatment trends would be the same.[20] Still, if other policies implemented in 2002 (like those mentioned above and described in more detail in the Appendix) affected the treated and control municipalities differently, the parallel trends assumption would be violated. In particular, the other reforms of the grant system may cause concerns that municipalities with supplemental grant increases in 2002 received proportionately larger increases also in other types of grants (in 2002 or later). The two-stage least squares (2SLS) model specified below will be used to investigate whether this is a problem.

3.2 2SLS specification

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

$$G_{i,t} - G_{i,2001} = \gamma_t(SG_{2002} - SG_{2001}) + T_{2001} + (e_{i,t} - e_{i,2001}), \qquad (4)$$

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

In the reduced form framework above, treatment was defined as increased supplemental grants. In the 2SLS framework, treatment is instead defined as increased generic grants or increased total grants, and equation (4) is then the first stage in the two-stage procedure. Using the predicted values from (4), $\widehat{G_{i,t} - G_{i,2001}}$, estimates of the effect of increased grants over a one-, two- and three-year period due to the policy-induced increase in supplemental

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.

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

grants between 2001 and 2002, τ_t^{IV}, are then recovered from the second stage:

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

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

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

4 Results

The result section first presents estimates both from the reduced form DID models in equations (2) and (3), as well as from the 2SLS model in equations (4) and (5). To test whether the estimates are confounded by other factors, such as other reforms also implemented in 2002, in a robustness check, a set of control variables are then added as controls. Finally, as seen in Table 2, the pre- and post-treatment level as well as the policy increase in the supplemental grant are discontinuous functions of the remote index with discrete jumps at 0.50 and 1. These discontinuities are exploited in a second robust check that estimates the effects in a combined DID and RD approach.

4.1 Reduced form estimates

Baseline reduced form results are presented in Table 3. For expenditures and tax revenues, respectively, columns 1 and 3 show the estimate of τ in equation (2), with associated standard errors that allow for clustering within municipality. As previously explained, this parameter represents the effect of a 1 euro per capita increase in supplemental grants on *total* changes in the outcome between the pre- and post-treatment period. The table reveals statistically significant effects of increased grants on both outcome variables; a 1 euro increase in grants is estimated to increase expenditures with around 2.5 euro per capita and to decrease tax revenues with around 0.25 euro per capita (through increased tax rates, since the tax base is held fixed at

the pre-treatment level). Especially for expenditures, the magnitude of the estimated response is large.

Table 3: Baseline reduced form results

	Expenditures		Tax revenues	
	(1)	(2)	(3)	(4)
Total effect; τ	2.653***		-0.268***	
	(0.727)		(0.0574)	
Effect in 2000–01; τ_{2001}		-0.227		-0.0169
		(0.325)		(0.0297)
Effect in 2001–02; τ_{2002}		0.888***		-0.114**
		(0.306)		(0.0475)
Effect in 2002–03; τ_{2003}		1.079*		-0.108***
		(0.562)		(0.0376)
Effect in 2003–04; τ_{2004}		1.445		-0.0640**
		(1.090)		(0.0259)
Observations	367	2202	367	2202

Note: The table reports estimated effects of a 1 euro per capita increase in SG on per capita expenditures and tax revenues in total (τ) and broken down over the years 2001–04 (τ_{2001}–τ_{2004}). Standard errors clustered by municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Columns 2 and 4 of Table 3 explore the timing of the responses by instead estimating equation (3). Here, the total effect τ is broken down over the years 2001–04, so that each of the estimates τ_{2001}–τ_{2004} represent the estimated effects of a 1 euro per capita increase in supplemental grants on changes in the outcome between two *consecutive* years for the period 2001–04. Again, because the grant increase occurred in 2002, τ_{2002} represents the immediate treatment effect, whereas τ_{2003} and τ_{2004} represent the dynamic incremental effects one and two years later. Finally, τ_{2001} is an estimate of the difference in pre-treatment trends displayed by the control group and the treatment group, and is thus as such a test of the identifying assumption.

Looking at column 2, the results show an immediate effect on expenditures which is both economically and statistically significant: τ_{2002} is estimated to 0.89, meaning that as grants increase by 1 euro, total expenditures increase by as much as 89 cents (in the same year). Furthermore, one and perhaps even two years after the grant increase, expenditures continue to increase by an additional euro, although the dynamic estimates for the two later years are obtained with much less precision (especially for the year 2004). There are at least two possible interpretations of this pattern: Taken at face value, these estimates together with the estimated total effect in column 1 reveal a total cumulative response that in fact exceeds the grant

increase in 2002, suggesting a path-dependence in the sense that expanding municipalities do not only get accustomed to a larger size of the budget but also to a faster growth rate. Alternatively, it is possible that the assumption of parallel trends is too strong as more and more years pass after the supplemental grant policy-reform, in which case the estimates of τ_{2003} and τ_{2004} cannot be causally interpreted. Although this would, in principle, shed some doubt also on the interpretation of τ_{2002}, the causality of this immediate response is strengthened by the fact that the "effect" in year 2001 is much smaller and not statistically indistinguishable from zero—thus suggesting that there is no difference in trends one year before the policy reform.

A comparison between columns 1 and 3 shows that the total effect on tax revenues is of an order of magnitude smaller than that on expenditures. Not surprisingly, the same is true in column 4 where the tax effect is broken down over the respective years. Although the treatment effect in all years is negative and statistically significant, the economic impact is considerably smaller. For example, evaluated at the supplemental grant increase for the group of municipalities with a remote index of 1–1.50 (a 75 euro increase), the immediate tax response implies that own-source revenues were cut by around $0.11 \times 75 = 8$ euro. This is in sharp contrast to the positive implied immediate expenditure response of around $0.89 \times 75 = 67$ euro for this group of municipalities.[21]

Also the estimate of differences in pre-treatment trends in tax revenues supports the identifying assumption, as seen by the insignificant estimate of τ_{2001} in the rightmost column. Hence, the conclusion so far is that increases in grants decrease taxes slightly but cause expenditures in the same year to increase substantially. Moreover, so far, there is no evidence of dynamic crowding-out—i.e., that the immediate response in expenditures is reversed in later years. The interpretation of the dynamic estimates is not yet clear, however, and therefore, the following section explores this dimension further.

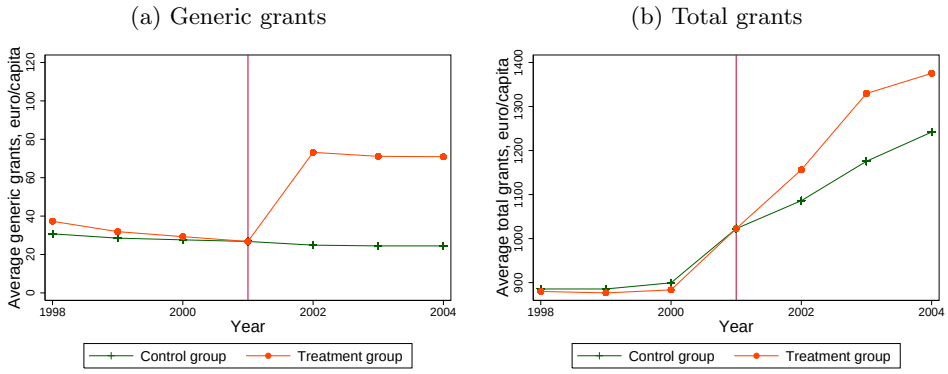
4.2 2SLS estimates

If causal, the estimated responses in expenditures one and perhaps even two years later imply very large cumulative effects. However, the large positive estimates of τ_{2003} and τ_{2004} for expenditures would be biased if municipalities treated with supplemental grant increases in 2002 received proportionately

[21] Using, instead of constructed tax revenues as in Table 3, actual tax revenues (i.e., tax rates times tax base) yields a statistically significant estimate of the total effect τ of -0.38 (to be compared to -0.27). When broken down over the separate years, however, the pattern of tax rate changes implied by Table 3 can not be reproduced. As noted above, a likely reason for this is that the tax base is quite variable, making it harder to detect whether or not the municipality has changed its tax rate (which is the only tax instrument under local discretion). With this in mind, it is noteworthy that the baseline results are robust to controlling for the per capita tax base; see Section 4.3 below.

larger increases also in other types of grants. To this aim, Figure 4 illustrates how generic grants and total grants have evolved over the sample period. As for the outcome variables in Figure 1, the grants variables in Figure 4 are scaled for the treatment group so that the difference from the control group in 2001 is zero.

Figure 4: Average generic grants and total grants



Note: For the treatment group, the variables are scaled to equal the control group in 2001.
Source: The Association of Finnish Local and Regional Authorities.

Since the supplemental grant constitutes as much as 80% of generic grants to municipalities in the treatment group, it is not surprising that Figure 4 displays an increase in generic grants in 2002 of a similar magnitude as the increase in the particular supplement. Also total grants increase relatively more for the treated municipalities, but do so both in 2002 and in 2003.

The econometric counterpart to the graphics in Figure 4 is the first stage of the 2SLS model; see equation (4). The results from running this equation along with the second-stage equation (5) are provided in Table 4. In columns 1–3, $G_{i,t}$ is defined as generic grants and, in columns 4–6, as total grant. In the upper panel, $t = 2002$ and the differences are thus over one year; in the mid panel, $t = 2003$ with differences over two years; and in the bottom panel, $t = 2004$ with differences over three years. For each t, the table includes the first-stage estimates of γ_t and the second-stage estimates of τ_t^{IV} for both expenditures and tax revenues.

Column 1 of Table 4 shows rather precise first-stage estimates of γ_t that are essentially 1 for the one-year time span with $t = 2002$ when the supplemental grant increased occurred, as well as for the longer time spans with $t = \{2003, 2004\}$. This says that, aside from the supplemental grant increase, there were no systematic differences in the changes of generic grants to municipalities in the treatment group as compared to those in the control group over the period 2001–04.

Moving along to columns 2 and 3, as expected from the size of the

Table 4: Baseline 2SLS results

	Instrument: Generic grants			Instrument: Total grants		
	1^{st} stage	Expenditures	Tax revenues	1^{st} stage	Expenditures	Tax revenues
Years 2001–02; γ_{2002}, τ^{IV}_{2002}	0.977***	0.908***	-0.116**	1.473***	0.602***	-0.0772**
	(0.00996)	(0.313)	(0.0484)	(0.174)	(0.211)	(0.0323)
Years 2001–03; γ_{2003}, τ^{IV}_{2003}	0.946***	2.079***	-0.235***	2.883***	0.682***	-0.0770***
	(0.00983)	(0.706)	(0.0593)	(0.328)	(0.226)	(0.0212)
Years 2001–04; γ_{2004}, τ^{IV}_{2004}	0.942***	3.620***	-0.303***	2.596***	1.314**	-0.110***
	(0.00959)	(1.350)	(0.0630)	(0.340)	(0.542)	(0.0259)
Observations	734	734	734	734	734	734

Note: Columns 1 and 4 report first-stage estimates of a 1 euro per capita increase in SG on per capita generic grants and per capita total grants, respectively. Columns 2–3 and 5–6 report second-stage estimates of a 1 euro per capita increase in generic grants and total grants, respectively, on per capita expenditures (columns 2 and 5) and tax revenues (columns 3 and 6). Standard errors clustered by municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

first-stage estimate, the second-stage estimates for the one-year time span with $t = 2002$ are very similar to the corresponding reduced form estimates above (τ_{2002} in Table 3). The interpretation of these coefficients is that a 1 euro increase in generic grants stemming from the policy-induced increase in supplemental grants causes expenditures to increase by around 90 cents and tax revenues to decrease by around 12 cents (in the same year)—again, effects very similar to the reduced form counterparts. Also as expected given the first stage, the second-stage estimates for longer periods are similar in magnitude to the sum of the incremental effects τ_t estimated above (or equivalently, to the total effect τ).

Thus, on the one hand, it is reassuring that there are no systematic differences between the treatment and the control group in the amount of generic grants received even over the longer period. On the other hand, the very large cumulative effects remain somewhat puzzling. We gain additional insight into this by, as in the three rightmost columns of Table 4, instead defining $G_{i,t}$ as total grants.

Looking first at the results for the one-year period in the upper panel, the first-stage estimate exceeds 1 but not by much, and is estimated with less precision (the standard error is more than 15 times that for generic grants). Consequently, the one-year period second-stage estimates are slightly compressed—that is, they are slightly smaller in absolute terms, both for expenditures and tax rates. For longer time-periods, however, the first-stage estimates tend to exceed 1 considerably, and the second-stage estimates are more unstable across time and alternative specifications.

All in all, because there are no large systematic differences in the changes in neither generic nor total grants between the treatment and control group over the 2001–02 period (aside from the supplemental grant increase), the analysis in this section supports a causal interpretation of the immediate effect of increases in grants; be they supplemental, generic or total grants, a 1 euro increase causes expenditures to increase approximately by as much as 60–90 cents but causes tax revenues to decrease by a mere 8–12 cents. On the contrary, since there appear to be systematic differences in the amount of total grants received over longer periods, a causal interpretation of the dynamic effects of grant increases is more problematic. But if anything, the results seem to suggest that the stimulatory effects on expenditures remain 2–3 years after the grant increase rather than that the grant increase is crowded out by decreases in own-sources revenues.

4.3 Robustness to control variables

While the previous section gained insight into the trends in different types of grants received, this and the next section perform a set of robustness checks to investigate the validity of the identifying assumption of parallel trends further. In the first set, presented in the different columns of Tables 5 and 6

for expenditures and tax revenues, respectively, various control variables are added to the baseline specification—now only the reduced form specification in (3) that breaks down the total effect by year.

In the reduced form framework, an alternative way of investigating trends in other types of grants is to include total grants as a control variable. Column 1 of Tables 5 and 6 therefore adds first-differenced per capita grants to the baseline specification. Motivated by the removal of the transitory grants that also occurred in 2002, first-differenced per capita transitory grants are also added.[22] Further, column 2 additionally includes the 2001 level of total and transitory grants to allow for trends in expenditures and tax revenues that differ depending on pre-treatment amounts of grants received.

Motivated by the reform also implemented in 2002 that decreased the share of corporate tax revenues accruing to the municipalities, columns 3–4 instead add the first-difference and the 2001 level of per capita corporate tax revenues.[23] To investigate whether the estimated effects are confounded by differences in trends in other variables that are key determinants of expenditures and taxes, columns 5–6 instead add the first-difference and the 2001 level of per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Finally, column 7 combines all of the above.

The overall conclusion across the columns of Tables 5 and 6 is that the baseline results are quite robust to the inclusion of these controls.[24] For expenditures as well as for tax revenues, the only absolute reduction in the point estimates seems to be induced by the inclusion of the pre-treatment level of total and transitory grants (columns 2 and 7), but the immediate effect on expenditures is still (weakly) significant. Note that as the absolute size of the estimates decreases both for expenditures and tax revenues when these variables are included, the relative response between spending increases and tax cuts is similar to the baseline estimates obtained without further controls. Note, also, that the estimate of τ_{2001} capturing differences in pre-treatment trends is statistically insignificant across all seven columns for both outcome variables.

4.4 Robustness to alternative identification

Moving along to a second, different type of robustness check of the baseline results, the structure of the supplemental grant is exploited. Recall from Section 3 that the pre- and post-treatment *level* as well as the policy-induced

[22] Note that transitory grants are not included in total grants, $G_{i,t}$, as defined and used in the 2SLS analysis above.

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

[24] Equivalent robustness checks of the baseline 2SLS specification show that also these results are robust to the inclusion of controls. The results are available upon request.

Table 5: Sensitivity analysis for expenditures; adding controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect in 2000–01; τ_{2001}	-0.205	-0.496	-0.227	-0.229	-0.230	-0.398	-0.526
	(0.323)	(0.322)	(0.325)	(0.326)	(0.339)	(0.338)	(0.335)
Effect in 2001–02; τ_{2002}	0.892***	0.620**	0.889***	0.887***	0.915***	0.729**	0.602*
	(0.304)	(0.306)	(0.305)	(0.306)	(0.321)	(0.321)	(0.325)
Effect in 2002–03; τ_{2003}	1.010*	0.733	1.096*	1.096*	1.191**	1.005*	0.813
	(0.562)	(0.587)	(0.564)	(0.564)	(0.577)	(0.589)	(0.617)
Effect in 2003–04; τ_{2004}	1.459	1.123	1.438	1.435	1.411	1.226	1.004
	(1.092)	(1.058)	(1.091)	(1.092)	(1.116)	(1.107)	(1.066)
Observations	2202	2202	2202	2202	2100	2100	2100
ΔGrants	yes	yes	no	no	no	no	yes
Grants_{2001}	no	yes	no	no	no	no	yes
ΔCorp. tax rev.	no	no	yes	yes	no	no	yes
$\text{Corp. tax rev.}_{2001}$	no	no	no	yes	no	no	yes
$\Delta\mathbf{X}$	no	no	no	no	yes	yes	yes
$\mathbf{X}_{2001}$	no	no	no	no	no	yes	yes

Note: The table reports estimated effects of a 1 euro per capita increase in *SG* on per capita expenditures, broken down over the years 2001–04. For $t \in [2001, 2004]$, τ_t represents the incremental effect between years t and $t-1$. $\mathbf{X}$ is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 6: Sensitivity analysis for tax revenues; adding controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect in 2000–01; τ_{2001}	-0.0192	0.0374	-0.0170	-0.0182	0.0000359	0.0276	0.0502
	(0.0303)	(0.0334)	(0.0297)	(0.0298)	(0.0308)	(0.0335)	(0.0354)
Effect in 2001–02; τ_{2002}	-0.121**	-0.0672	-0.115**	-0.116**	-0.115**	-0.0833*	-0.0672
	(0.0476)	(0.0469)	(0.0469)	(0.0468)	(0.0470)	(0.0456)	(0.0467)
Effect in 2002–03; τ_{2003}	-0.124***	-0.0737*	-0.120***	-0.120***	-0.108***	-0.0758*	-0.0763*
	(0.0399)	(0.0402)	(0.0391)	(0.0391)	(0.0412)	(0.0417)	(0.0451)
Effect in 2003–04; τ_{2004}	-0.0608**	0.000481	-0.0593**	-0.0608**	-0.0584**	-0.0272	0.00260
	(0.0256)	(0.0274)	(0.0261)	(0.0263)	(0.0273)	(0.0291)	(0.0305)
Observations	2202	2202	2202	2202	2100	2100	2100
ΔGrants	yes	yes	no	no	no	no	yes
Grants_{2001}	no	yes	no	no	no	no	yes
ΔCorp. tax rev.	no	no	yes	yes	no	no	yes
$\text{Corp. tax rev.}_{2001}$	no	no	no	yes	no	no	yes
$\mathbf{\Delta X}$	no	no	no	no	yes	yes	yes
$\mathbf{X}_{2001}$	no	no	no	no	no	yes	yes

Note: The table reports estimated effects of a 1 euro per capita increase in SG on per capita tax revenues, broken down over the years 2001–04. For $t \in [2001, 2004]$, τ_t represents the incremental effect between years t and $t-1$. $\mathbf{X}$ is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

change in the supplemental grant are discontinuous functions of the remote index with discrete jumps at 0.50 and 1 (cf. Table 2). In this section, this discontinuous structure is exploited to identify the effect of increased grants in a combined DID and RD framework.[25] By combining the DID specification with the discrete jumps in the remote index, the discontinuities in the change in the supplemental grant help to identify the effect (i.e., not the discontinuities in the level).

Identification within the RD framework is typically quite data demanding, and by RD standards the sample available here is small. It is, for example, not possible to perform a full non-parametric analysis. Further, in order to save power, the RD analysis focuses on the total effect of grants as measured by the total change in the outcome between the pre- and post-treatment period, and are not broken down by the separate years. In other words, the robustness of the parameter τ in equation (2) is analyzed, but not of the parameters τ_{2001}–τ_{2004} in equation (3).

The RD analysis starts with a graphical representation of the results, seen in Figure 5. Along the x-axis is the remote index, and along the y-axis is the total change in the outcome between the pre- and post-treatment period, ΔY_i;[26] the outcome being per capita expenditures in panel (a) and per capita tax revenues in panel (b). The figures plot binned averages of these ΔY_i variables, with a bin size of 0.025, along with second-order polynomials in the remote index that are separately fitted in each of the intervals 0–0.50, 0.50–1 and 1–1.50.

From visual inspection, the positive effect on expenditures estimated above is confirmed by the jump in the fitted polynomial at the first cut-off at 0.50, while the negative effect on tax revenues is suggested by the jump at the second cut-off at 1, although the latter is plagued by more noise.

To not only get a graphical confirmation but to also to investigate whether the actual estimates from above are robust to this alternative identification strategy, the RD analysis continues by estimating the following equation:

$$\Delta Y_i = \tau \Delta SG_i + f(\textit{Remote index}, \Gamma) + T_{post} + \varepsilon_i, \tag{6}$$

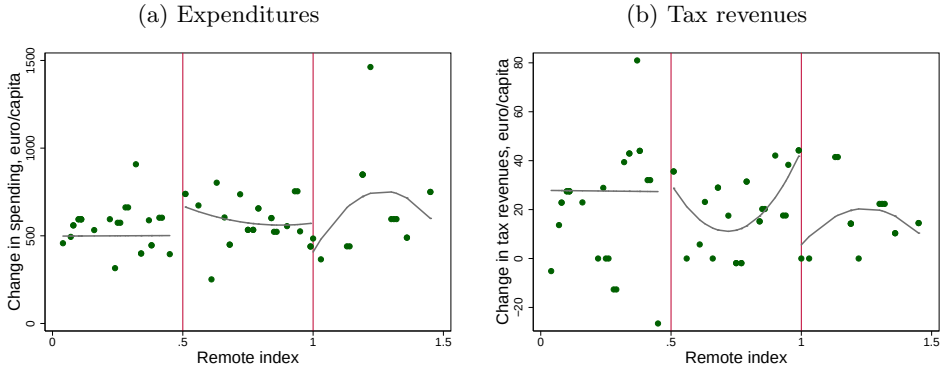
where $f(\cdot)$ is a function of the remote index. Acknowledging that it might be at the expense of some bias, note that for efficiency reasons, equation (6) restricts the treatment effect to be the same at both cut-offs.[27] The robustness of the results will be examined by estimating versions of equation (6). In particular; (i) it will be estimated on the full sample as well as when

[25] See, e.g., Hahn et al. (2001) and Van der Klaauw (2002) for important methodological contributions on the RD design.

[26] Recall from equation (2) in Section 3 that this variable is defined as $\Delta Y_i = \overline{Y}_{i,post} - \overline{Y}_{i,pre} = \sum_{t=2002}^{2004} Y_{i,t}/3 - \sum_{t=1998}^{2001} Y_{i,t}/4$.

[27] To see how equation (6) identifies the treatment effect τ in an RD design, see the derivation in the Appendix.

Figure 5: Total change in per capita expenditures and tax revenues between the pre- and post-treatment period



Note: The figures plot averages within 0.025-sized bins of the remote index along with fitted second order polynomials.
Source: Government Institute for Economic Research.

restricting the control group to only include municipalities with a remote index larger than -10 and -5, respectively; (ii) the function $f(\cdot)$ will either be linear or quadratic in the remote index; and (iii) the function $f(\cdot)$ will either be restricted to be the same for the control group as for treatment groups 1 and 2 (i.e., in the three different intervals 0–0.50, 0.50–1 and 1–1.50), or it will be allowed to vary between the three groups.

The resulting estimates are given in Tables 7 for expenditures and Table 8 for tax revenues. Again, as these are the total effects, they should be compared to the estimates of τ from the baseline results in Table 3 (which are incidentally reproduced in the top left of Tables 7 and 8). The precision of the estimates is reduced somewhat when the control group is restricted to those with remote indices larger than -10 and -5 (as one moves down the different panels of the tables), and quite a lot when the $f(\cdot)$ function is allowed to vary between the three segments (columns 4–5). But overall, the tables provide a very stable picture, with most of the point estimates varying only a little. In other words, that grant increases caused expenditures to increase considerably, and caused taxes to decrease but with an order of magnitude smaller than the expenditure effect, is shown to be a robust result.

To sum up the result section, most of the estimated effects of a 1 euro increase in grants on the immediate response in expenditures are in the range 70–80 cents, although the response is reduced to around 60 cents in a few specifications, and all immediate expenditure effects are statistically significant at least at the 10% level but in most cases also at the 5% level. The estimated effects on the tax response from a grant increase are, however, economically much weaker. Furthermore, the overall robustness of the

Table 7: Robustness of expenditure results; exploiting discontinuities

	(1)	(2)	(3)	(4)	(5)
Full sample, n=367	2.653***	2.588***	2.088***	2.632	3.146
	(0.727)	(0.738)	(0.752)	(2.785)	(3.062)
Remote index> −10, n=308	2.545***	1.830**	0.801	2.383	3.078
	(0.732)	(0.785)	(0.901)	(2.781)	(3.056)
Remote index> −5, n=243	2.417***	1.062	2.093*	2.174	3.106
	(0.741)	(0.842)	(1.106)	(2.782)	(3.072)
$f(Remote\ index, \Gamma)$	−−	linear	quadratic	linear	quadratic
$f^1 = f^2 = f^3$	−−	yes	yes	no	no

Note: The table reports estimated effects of a 1 euro per capita increase in SG on total changes in per capita expenditures, τ. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

Table 8: Robustness of tax revenue results; exploiting discontinuities

	(1)	(2)	(3)	(4)	(5)
Full sample, n=367	-0.268***	-0.191***	-0.149**	-0.313*	-0.188
	(0.0574)	(0.0611)	(0.0640)	(0.165)	(0.180)
Remote index> −10, n=308	-0.235***	-0.141**	-0.126	-0.297*	-0.186
	(0.0586)	(0.0703)	(0.0976)	(0.164)	(0.180)
Remote index> −5, n=243	-0.195***	-0.162*	-0.158	-0.301*	-0.188
	(0.0603)	(0.0847)	(0.133)	(0.165)	(0.181)
$f(Remote\ index, \Gamma)$	−−	linear	quadratic	linear	quadratic
$f^1 = f^2 = f^3$	−−	yes	yes	no	no

Note: The table reports estimated effects of a 1 euro per capita increase in SG on total changes in per capita tax revenues, τ. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

baseline results to various alternative specifications together with the test of the identifying assumption of parallel trends validates the claim that the policy-induced increase in the supplemental grant is exogenous and hence, supports a causal interpretation of these results.

5 Potential mechanisms

The focus of the paper so far has been to give a convincing answer to *how* local governments respond to grant increases. Knowing this before thinking about possible mechanism is obviously crucial, and to provide a fully convincing answer also to *why* the municipalities respond as they do is outside the scope of this paper. Yet—if only briefly—this final section of the paper looks at whether there is evidence in the data for two particular mechanisms, namely so-called "separate mental accounting" and political alignment. Note that the evidence provided here is merely intended to be suggestive, and should be taken as a starting point for future research investigating various mechanisms more thoroughly.

Separate mental accounting, i.e. that voters and politicians treat the government budget constraint separately from the voters' own budget constraint, is an explanation that can be attributed to Tversky and Kahneman (1984) and Thaler (1985) but that is often dismissed as unlikely to (fully) explain the empirical flypaper anomaly. In contrast for Finland, it may be a relevant explanation since it is quite likely that the labeling of the so-called sector grants as "grants to social services and health care" and "grants to education and culture" (despite the fact that all grants are in fact unconditional) could trigger such mental accounting.

If this is the mechanism at work, it follows, first, that the causal effect of these sector grants should be larger on expenditures in the respective sectors than in any other sector (again, even though they are distributed as unconditional grants). Second, the causal effect of the sector grants on expenditures in the respective sectors should be larger than the causal effect of grants that are not labeled as though they were targeted, such as generic grants.

To thoroughly test for this mechanism, one would thus need to identify causal effects also of the sector grants. But as a starting point here, a tentative analysis of one of the sector grants—grants to education and culture (from hereon simply referred to as "grants to education")—and educational spending is conducted.[28] In particular, the conditional correlations between educational spending, $Y_{i,t}^{educ}$, and grants to education, on the one hand, and, on the other, generic grants are estimated by running the following regres-

[28]The main reason for analyzing this sector grant is data availability. One may also argue that it is a relevant sector for this purpose, as educational spending is a quite flexible policy variable.

sion:

$$Y_{i,t}^{educ} = \tau^{educ} G_{i,t}^{educ} + \tau^{generic} G_{i,t}^{generic} + \mu_i + T_t + \varepsilon_{i,t}, \tag{7}$$

where $G_{i,t}^{educ}$ are per capita grants to education to municipality i in year t, $G_{i,t}^{generic}$ are per capita generic grants and μ_i and T_t are municipality and year fixed effects, respectively.[29]

The parameters $\tau^{generic}$ and τ^{educ} in equation (7) thus capture the effect on educational spending of a 1 euro increase in generic and education grants, respectively, conditional on municipality and year fixed effects. These parameters are estimated in column 1 of Table 9, while column 2 also controls for a set of time-varying municipality characteristics; per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. In columns 3–4 and 5–6, equivalent regressions pairs are estimated for the two original outcome variables total expenditures and tax revenues, respectively.

Table 9 reveals several noteworthy patterns. First, column 1 clearly suggest that grants to education correlates with educational spending, and more so than what generic grants do (although the difference is not statistically significant). The point estimates are somewhat effected by the time-varying controls in column 2 (the drop is almost completely accounted for by the variable measuring the population share of school-aged children), but grants to education still have a statistically significant impact on educational spending.[30]

Second, grants to education correlates with the original outcome variables in an interesting way. For expenditures, the point estimate is much lower than that of generic grants, while it is essentially zero for tax revenues (here the differences between row 1 and 2 are statistically significant). Interestingly, the point estimates of generic grants in columns 3–6 are very similar to the total effect as estimated above using the variation in generic grants stemming from the policy-induced increase in supplemental grants.

To the extent that the results in Table 9 may represent mere correlations and not causal effects, they cannot be viewed as proof of a mechanism. But at the very least, the data patterns are consistent with a mental accounting story that may be worthwhile pursuing further in future research.

A second, different yet related explanation is that, with the labeling of the sector grants, the central government signals that its intention in distributing grants is first and foremost to finance expenditures rather than

[29]The full sample mean (standard deviation) of per capita educational spending and grants to education are 582.2 (144.8) and 174.0 (184.6), respectively.

[30]Note that rather than analyzing conditional correlations, the causal effect of generic grants on educational spending can be estimated by running the original equations (2) and (3) from above on Y^{educ}. This yields statistically insignificant and quite small estimates—i.e., similar to the second row of Table 9 (the results are available upon request).

Table 9: Effects of grants on expenditures and taxes; fixed effects regressions

	School expenditures		Total expenditures		Tax revenues	
	(1)	(2)	(3)	(4)	(5)	(6)
Grants to education	0.266***	0.118**	0.291	0.291	0.0103	0.0103
	(0.0722)	(0.0533)	(0.201)	(0.225)	(0.0251)	(0.0235)
Generic grants	0.0549	0.109	2.143***	2.045***	-0.262***	-0.299***
	(0.154)	(0.0993)	(0.636)	(0.662)	(0.0697)	(0.0684)
Observations	2569	2450	2569	2450	2569	2450
X	no	yes	no	yes	no	yes

Note: The table reports estimated effects of a 1 euro per capita increase in grants to education and generic grants on per capita school expenditures, total expenditures and tax revenues. All regressions include year and municipality fixed effects. **X** is a vector including the per capita income tax base, population size and population shares of school-aged children, elderly and welfare recipients. Standard errors clustered by municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

tax cuts. Possibly, this can further encourage increased local spending if the municipalities fear that by instead responding with tax cuts, they may be disqualified from future grants. In general, local governments should be more willing to conduct policies in line with the political agenda of the central government when they are politically aligned.[31] A heterogeneity analysis with respect to political alignment can therefore say something about the relevance of this type of mechanism.

Defining political alignment in Finland during the studied period is complicated by the fact the central government consisted of a coalition of several parties. The 1999–2003 government led by the social democratic prime minister Lipponen consisted of the two left-wing parties the Social Democrats and the Left Party, the two right-wing parties the National Coalition Party and the Swedish Liberal Party and the neutral Green Party. Also the 2003–07 government led by the center partist Vanhanen was a coalition government which, besides the Center Party (considered to be right-wing), consisted of the right-wing Swedish People's Party and the left-wing Social Democrats.

Among the two coalitions, the latter consisted of fewer parties and it therefore makes more sense to consider politically aligned local governments during this period. And if one is willing to define the central government according to the party of the prime minister, then the change to a Center Party prime minister in 2003 (April) introduces interesting variation in political alignment among the treated municipalities, as the Center Party tradition-

[31]This would also provide a rationale for the central government to distribute more grants to politically aligned local governments; see, e.g., Grossman (1994), Levitt and Snyder Jr (1995), Solé-Ollé and Sorribas-Navarro (2008) and Brollo and Nannicini (2012).

ally has had strong support among them.[32] Consequently, to investigate whether there are heterogeneous effects of increased supplemental grants depending on political alignment, the following regression is estimated:

$$\Delta Y_{i,t} = \sum_{t=2001}^{2004} (\tau_t \Delta SG_i + \tau_t^{Center} \Delta SG_i \times Center_i) + Center_i + T_t + \varepsilon_{i,t}, \tag{8}$$

where $Center_i$ is a dummy that equals 1 if the Center Party held at least 50% of the seats in the local council of municipality i during the 2000–04 local election period, and 0 otherwise. Thus, $Center_i$ is thought to capture political alignment with the central government that took office in 2003.[33]

Table 10: Effects of grants on expenditures and taxes; differential effects for center-governed municipalities

	Expenditures		Tax revenues	
	τ_t	τ_t^{Center}	τ_t	τ_t^{Center}
Effect in 2000–01; τ_{2001}	-0.0763	-0.303	-0.0807***	0.115***
	(0.495)	(0.621)	(0.0202)	(0.0358)
Effect in 2001–02; τ_{2002}	1.246**	-0.662	-0.181***	0.121
	(0.538)	(0.641)	(0.0571)	(0.0755)
Effect in 2002–03; τ_{2003}	0.877*	-0.0111	-0.167***	0.115**
	(0.462)	(0.811)	(0.0314)	(0.0452)
Effect in 2003–04; τ_{2004}	-1.899***	3.883***	-0.103***	0.0909***
	(0.558)	(1.322)	(0.0208)	(0.0342)
Observations	2202		2202	

Note: The table reports estimated effects of a 1 euro per capita increase in SG on per capita tax revenues, broken down over the years 2001–04. For $t \in [2001, 2004]$, τ_t represents the incremental effect between years t and $t-1$ for all municipalities and τ_t^{Center} represents the additional effect between years t and $t-1$ for center-governed municipalities. Both regressions controls for the dummy variable $Center_i$. Standard errors clustered by municipality are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level, respectively.

As above, for $t \in [2001, 2004]$, τ_t in equation (8) represents the incremental effect between years t and $t-1$ for all municipalities. The new parameter τ_t^{Center} represents the additional effect between years t and $t-1$ for center-governed municipalities. There is thus eight parameters of interest in each regression. Table 10 presents these in columns 1–2 for expenditures and

[32] The Center Party has traditionally been large in rural areas, where many of the treated municipalities are located.

[33] $Center_i = 1$ in about 80% of the treated municipalities and in about 35% of the control municipalities.

columns 3–4 for tax revenues. Two sets of results stand out; first, throughout the entire period, center-governed municipalities are less reluctant to use grants to cut taxes (as seen in column 4); second (and perhaps more interestingly), the positive effect on expenditures in 2004 is completely accounted for by the center-governed municipalities. Given that these municipalities became politically aligned with the central government in 2003 (according to the above definition), this result in combination with the insignificant estimates in the earlier years could potentially indicate a political alignment mechanism at work. Again, this result motivates continuous research on political economy explanations to the flypaper effect.

6 Conclusion

Intergovernmental grants are widely used in fiscally decentralized countries. Knowledge about the effects of these grants on the receiving jurisdiction is therefore of considerable policy relevance. In this paper, I estimate the effect on local expenditures and taxes of a policy that treated a group of remotely populated municipalities in Finland with increased grants while leaving another group serving as controls untreated.

The robust finding of the paper is that following a grant increase, there is a statistically and economically significant positive immediate response in local expenditures. While there is also a statistically significant negative effect on local income taxes, the size of this response is of an order of magnitude smaller. Specifically, for one of the groups of treated municipalities, expenditures increased by around 65 euro per capita as a result of the reform, whereas own-source revenues were only cut with 8 euro per capita. While a few specifications result in somewhat smaller effects, the absolute size of the estimates decreases both for expenditures and taxes so that the relative response between spending increases and tax cuts is robust.

A glance at a balance of payment sheet for Finnish finances shows that, on aggregate, total consumption is around 50% of GDP. Out of total consumption, only 30% are public consumption and, hence, 70% are private consumption. The large stimulatory effects on public expenditures can thus be interpreted as crowding-in effects.

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

How local governments respond to increases in grants merely tells half of the story. The next obvious step is to also figure out *why* these municipalities apparently display flypaper behavior. In light of this, the paper

discusses separate mental accounting and political alignment as two potential mechanisms. While the data exhibits patterns consistent with these ideas, these patterns are merely suggestive, and to disentangle the *causal* underlying mechanisms of grants effects is also left for future work.

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Other policies implemented in 2002

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

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

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

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

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

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

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

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

Finally, partly as a consequence of some of the previously described reforms, there were some changes to the sector grants (proposed in Government Bill 132/2001 and legislated in Law 1389/2001 for education and culture, and proposed in Government Bill 152/2001 and legislated in Law

1409/2001 for social services and health care). As previously mentioned, these grants were increased in order to adjust for the altered fiscal responsibilities between the state and the municipalities. It was additionally decided that the increase in the state's revenue due to the removal of the 15% cap in the revenue sharing system was to be transferred to the municipalities as increased grants to social services and health care. On the other hand, the reform in the VAT system implied decreased sector grants. All in all, the majority of municipalities received more sector grants in 2002 than in 2001. Note that the results presented in the paper when controlling for total grants received are similar to the baseline results.

Derivation of equation (6)

In this appendix, the RD model in equation (6) is derived.

There are two points of the remote index at which the grant increase jumps discontinuously; at 0.50 and at 1. Let D_i^1 be a dummy that equals 1 if municipality i belongs to group 1 and thus has a remote index in the interval 0.50–1, and 0 otherwise. Similarly, let D_i^2 be a dummy that equals 1 if municipality i belongs to group 2 and thus has a remote index in the interval 1–1.50, and 0 otherwise. Then, in the following model:

$$\Delta Y_i = \tilde{\tau}^1 D_i^1 + \tilde{\tau}^2 D_i^2 + f(Remote\ index, \Gamma) + T_{post} + \varepsilon_i, \tag{9}$$

the parameters $\tilde{\tau}^1$ and $\tilde{\tau}^2$ identify the effect on the outcome ΔY_i due to the discontinuous increase in the supplemental grant at remote index 0.50 and at remote index 1, respectively.

In order to evaluate the treatment effects in euro per capita grant increases, multiply the indicators D_i^1 and D_i^2 with the amount of the supplemental grant increase, ΔSG_i:

$$\Delta Y_i = \tau^1 D_i^1 \Delta SG_i + \tau^2 D_i^2 \Delta SG_i + f(Remote\ index, \Gamma) + T_{post} + \varepsilon_i. \tag{10}$$

Note that ΔSG_i is constant within the two groups, so that τ^1 and τ^2 only are rescaled versions of their $\tilde{\tau}^1$ and $\tilde{\tau}^2$ counterparts.

Finally, restrict the treatment effect to be the same at the 0.50 discontinuity as at the 1 discontinuity. That is, assume $\tau^1 = \tau^2 = \tau$, and arrive at:

$$\Delta Y_i = \tau \Delta SG_i + f(Remote\ index, \Gamma) + T_{post} + \varepsilon_i, \tag{11}$$

where the D_i:s have been removed, as they are simply indicators for $\Delta SG_i \neq 0$.