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Using a discontinuous grant rule to identify the effect of grants on local taxes and spending

Abstract

When investigating the effects of federal grants on the behavior of lower-level governments, it is hard to defend the handling of grants as an exogenous factor affecting local governments; federal governments often set grants based on characteristics and performance of local governments. In this paper we make use of a discontinuity in the Swedish grant system in order to estimate the causal effects of general intergovernmental grants on local spending and local tax rates. The formula for the distribution of funds is used as an exclusion restriction in an IV-estimation. We find evidence of crowding-in, where federal grants are shifted to more local spending, but not to reduced local tax rates. Our results thus confirm a flypaper effect for Sweden.

Key words: Fiscal federalism, grants, flypaper effect, local taxation, local government expenditure, causal effects

JEL Classifications: H21, H71, H77, R51

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1. Introduction

A key issue in the design of fiscal federalism is the financing of lower-level governments. Because of the advantages of taxation at the federal level and spending at the decentralized level most countries end up with vertical fiscal imbalance. The decentralization of expenditure is typically not accompanied by equivalent revenue-raising responsibilities. Hence, there is an imbalance between where the money is spent and where revenues are collected. Intergovernmental grants consequently are an important part of the financing of decentralized government.

Although grants from the federal government primarily substitute for decentralized taxes, they are also motivated by equalization and earmarking. Understanding how these grants affect decentralized governmental behavior is of interest not only for the federal decision maker. In the basic median voter model of local public finance, grant revenue is treated as any other income in the community. Grants are expected to be allocated between local public and private goods in accordance with the income elasticities of the median voter. This insight was originally offered by Scott (1952) and Bradford and Oates (1971a,b). Federal government grants to decentralized governments will then to a large extent be handed out to the local population as reduced taxes and fees, since the decentralized governments already have arranged an optimal mix of local public goods and private consumption. However, the empirical literature has never given much support to this theory prediction. Already Gramlich (1977) summarized the empirical finding that grants tend to expand spending with the same amounts.[1] Arthur Okun is credited with the term 'flypaper effect', since 'money stick where they hit' i.e. that grants never leave the state budget and enter into the personal wallets through lower local taxes. In a modern restatement, Knight (2002) defines the two approaches as crowding-out versus crowding-in; the flypaper effect implies that federal grants crowd in local spending whereas, in the median voter framework, federal grants crowd out local spending dollar for dollar after accounting for income effects.

One explanation for the somewhat puzzling lack of evidence for the median voter model might be a methodological flaw of earlier empirical studies. When the federal decision making of grants is addressed, it is hard to defend the handling of grants as an exogenous

[1] See Hines and Thaler (1995) for a more recent overview.

factor affecting local governments.[2] However, only few studies make attempts at handling the endogeneity of federal grants. Two recent studies, that have been given some attention, are Knight (2002) and Gordon (2004).[3] Knight (2002) presents a theoretical model where legislative bargaining over grants predicts a positive correlation between grant receipts and preferences for public goods. In order to solve this endogeneity the empirical part applies instruments based on the political power of congressional delegations to account for the exogenous part of grants variation related to highway spending in the US. Knight concludes that the endogeneity can explain the flypaper effect. His estimates indicate that grants crowd out highway spending when the policy endogeneity is accounted for. Gordon (2004) applies the underlying change in data of the criteria of block grants to identify the causal effect. She takes advantage of the updating of data for key criteria in the grants allocation to school districts in the US. The updating of data given in the 1990 census leads to a discontinuous change in the grants distributed. The census-determined change in grants is calculated and used as an instrument for the actual change of grants. The spending demand effect of the demographic 'shift' is taken into account by assuming constant per-pupil spending. The estimated results show strong crowding-in (flypaper effect) during the first year of the new grants distribution.

The studies by Knight and Gordon are welcomed contributions to the literature. However, their studies concern very specific grant programs (highway spending and Title 1) under the US setting. It is likely that different grant programs may have different effects and that the results may be sensitive to the economic setting. Hence, we still know very little about the causal effects of grants on lower level governmental behavior. Furthermore, Knight´s choice of instrumental variables based on politics has some weaknesses. The proportion of state delegation serving on the transportation authorization committee is used as an instrument. If delegates typically serve in committees according to their preferences, this may not be exogenous to highway spending. The second instrument is the proportion of a state's representatives in the majority party. Given that the majority party is the same during long time periods, this is clearly a variable that captures preferences of the voters in the state.[4]

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[2] The problem of estimating incidence of endogenous policies was pointed out by Besley and Case (2000).
[3] Instrumentation of federal grants has been applied in US-studies by Becker (1996), Gamkhar and Oates (1996), and Turnbull (1998), while Berg and Rattsø (2006) take advantage of a tax reform changing the distribution of grants in Norway.
[4] Also, looking at Knight's first step estimates (see columns 3 in Table 3 in Knight) there are two reasons to worry. First, the F-test for the instrumental variables is very low (around 2.6 with 5 degrees of freedom). Second, the instruments have different signs depending on if they are measured at the House or at the Senate level.

The aim of this paper is to add to the existing literature studying the causal effects of unconditional block (lump sum) grants on local spending and taxes in Sweden. In order to identify the causal effect of grants we follow Guryan (2003) and make use of a discontinuity in the Swedish grant formula where municipalities with a net out-migration above two percent receive grants whereas municipalities with a net out-migration below two percent do not. This formula for the distribution of funds is used as an exclusion restriction in an IV-estimation where the identifying assumption is that the functional form of the direct relationship between the dependent variable and the treatment-determining covariates is not the same as the functional form of the relationship between treatment-determining covariates and grants.[5] Our approach is in some ways similar to the one adopted by Gordon in that that she uses a discontinuous change in the grant formula. However, whereas she uses a discontinuity that exists every tenth year, our discontinuity is observed each year between 1996 and 2004. We thus have a panel of 284 municipalities observed over nine years. We find evidence of crowding-in, where federal grants are shifted to more local spending, but not reduced local taxes. Our results thus confirm a flypaper effect for Sweden.

The outline of the paper is the following: the sources of grant endogeneity is laid out in section 2. Section 3 presents the grant formula that is used to identify the effects of intergovernmental grants on local spending and tax rate. Section 4 discusses our identification strategy building on the use of the grant formula in instrumentation. Data, as well as some descriptive analysis of the Swedish system, are presented in section 5. Estimated causal effects on local spending and taxation are shown in section 6. Finally, section 7 offers concluding remarks.

2. Sources of grant endogeneity

The background understanding of grant endogeneity can be described in a simple model of political decision making suggested by Besley and Case (2000) in an article discussing endogenous policies as right hand side variables. In econometric terms, the endogeneity is an omitted variables story. Assume that we want to estimate the effect of block grants on local government spending and taxation, where Y measures local public spending and taxation, **X** is

[5] That is, the variable that determines grants may also affect spending, as long as the way it affects spending differs.

a vector of local socio-economic variables that might explain local spending and taxation, and P (the policy) is central government block grants. Assume that P is a function of local socio-economic variables other than **X,** say **Q,** and political variables, **W**. **Q** and **W** are typically not controlled for in the estimation of local spending and taxation equations. Given this general set-up, Besley and Case show that the probability limit of the OLS estimate of the coefficient for the grant variable has two sources of bias (their equation 10):

1. Omitted variable bias caused by observable variables that determine policy and that have independent influence on the outcome of interest (i.e., **Q** and **W**)
2. Bias due to the presence of unobservable variables that may determine both the policy and the outcome of interest.

There are at least four cases where we can suspect a bias when investigating the effects of central government block grants on local governments spending and taxes:

(i) Political variables, **W**, might matter because the grant system is designed in negotiations between central politicians representing different local regions, or between central and local politicians, which implies that preferences for local spending and grants distribution will be correlated (this is along the lines of the Knight, 2002, story).

(ii) Even in the absence of negotiations, political variables, **W**, might matter because central politicians designing the grant system have preferences for specific economic and/or political characteristics of the municipalities associated with their spending priorities. In this case the grant variable is endogenous in a spending equation (the argument that the central government designs the grant system with an eye on the characteristics of the municipalities is developed by Johansson, 2003).

(iii) Independent of the role of political variables, local socio-economic characteristics of the municipality, **Q**, might matter. It might be the case that not all variation in the grant variable is exogenous to local spending because some local socio-economic variable(s) influence both spending and taxation and the way grants are allocated. It is hard to control for all variables that might be correlated both with local spending and taxation and with grant allocation (for a related discussion, see Gordon, 2004).

(iv) Unobserved characteristics that can be correlated with both local spending and with grant allocation might be important.

3. The grant formula used for identification

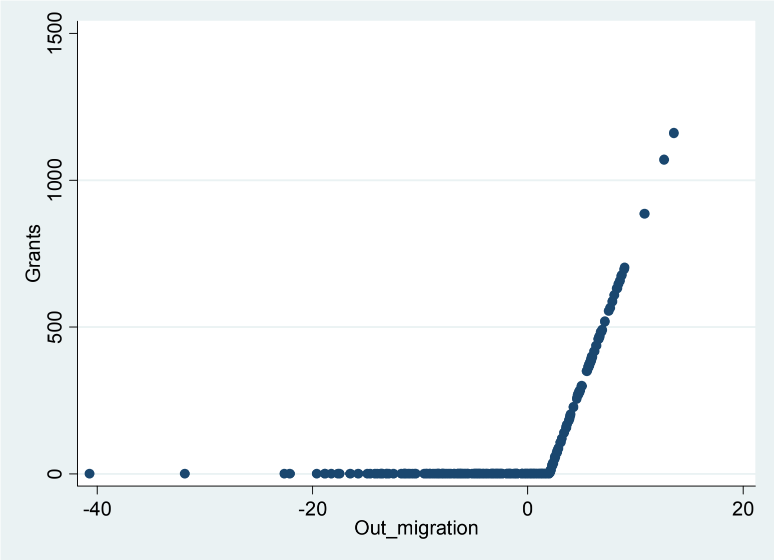
In this paper we will use an element in the cost equalizing grant formula[6] that can be applied as a source of exogenous variation in the grants, namely a component that is intended to support municipalities with a negative population growth. The grants come with no strings attached, that is, municipalities can use the money in the way they prefer. Grants are distributed to local governments with a net out-migration larger than two percent the last ten years (with a two-year lag). More formally, municipality *i* receives out-migration grants in year *t* (g_{it}^{m}) according to the following rule:

(1) $$g_{it}^{m}\begin{cases} >0 \text{ if } m_{(it-2)-(it-12)} > 2 \\ =0 \text{ if } m_{(it-2)-(it-12)} \leq 2 \end{cases},$$

where $m_{(it)-(it-j)}$ is the net out-migration rate in municipality *i* between year *t* and year *j*. The amount of out-migration grants received is proportional to the out-migration rate. Figure 1 plots grants received by the municipalities against the rule for a typical year (1999). As can be seen from the figure, there is a well-defined cut at two percent where municipalities with lower net out-migration than two percent do not receive any grants whereas municipalities above two percent receive grants.[7]

[6] The cost equalizing grant is a block grant to support municipalities that are characterized by demographic and other structural conditions associated with higher costs.
[7] The block grant is self-financed and the total cost for this grant component is divided equally (per capita) between all municipalities.

Figure 1 Out-migration grants (SEK/capita) against net out-migration, 1999



In the year 2000, an additional compensation in the form of grants was introduced to take into account the change in the number of school-age children. This compensation was conditioned on the net out-migration rate during the past three years (with a two year lag, $m_{(it-2)-(it-5)}$), which had to be larger than two percent. More formally, municipality *i* receives *additional* out-migration grants in year *t* ($g_{it}^{m,s}$) according to the following rule:

$$(2) \qquad g_{it}^{m,s} \begin{cases} > 0 \text{ if } s_{(it-2)-(it-5)} < 0 \text{ and } m_{(it-2)-(it-5)} > 2 \\ = 0 \text{ otherwise} \end{cases},$$

where $s_{(it)-(it-j)}$ is the net change in the number of school-age children in municipality *i* between year *t* and year *j*.[8] Both these elements will be used in our identification strategy discussed below.

4. Identification strategy using the discontinuous grant rule

We are interested in the causal effect of block grants on local government spending and local tax rates, i.e., the relationship we want to identify is given by

$$(3) \qquad y_{it} = \alpha_0 + \alpha_1 g_{it} + \varepsilon_{it},$$

[8] During 2000–2001, this compensation was based on changes in the age group 7–15 and in 2002–2004 on changes in the age group 7–18.

where y is spending or taxes and g is intergovernmental block grants. However, if we estimate the equation directly our estimated parameters will be biased for at least two reasons. First, there is the obvious problem of omitted variables. Second, as argued earlier, grants are likely to be endogenous. Below we will focus on how we solve the endogeneity problem.[9]

To solve the endogeneity problem, we propose using an instrumental variables (IV) estimator, where the formula for the out-migration grant ($\Omega\left(m_{(it-2)-(it-12)}, s_{(it-2)-(it-5)}\right)$) is used as an excluded instrument.[10] Since $m_{(it-2)-(it-12)}$ and $s_{(it-2)-(it-5)}$ might have a direct effect on the outcome of main interest (i.e., on local spending and local taxes) we need to control for these variables directly in the estimations; otherwise the instrument will not be valid. Since we do not know the exact form of this direct effect we allow for as flexible functional form as possible. This implies that the first stage in the two-stage least squares procedure is given by

$$(4) \qquad g_{it} = \gamma_0 + \gamma_1 \Omega\left(m_{(it-2)-(it-12)}, s_{(it-2)-(it-5}\right) + \phi' f\left(m_{(it-2)-(it-12)}\right) + \theta' h(s_{(it-2)-(it-5}) + \eta_{it},$$

where $f(\cdot)$ and $h(\cdot)$ are smooth functions of the treatment-determining covariates, and that the second step (where the relationship of main interest is estimated) is given by

$$(5) \qquad y_{it} = \alpha_0 + \alpha_1 \hat{g}_{it} + \delta' f\left(m_{(it-2)-(it-12)}\right) + \lambda' h\left(s_{(it-2)-(it-5}\right) + \varepsilon_{it},$$

where $\hat{g}$ is the predicted grants obtained from the estimation of equation (4).[11]

The exclusion restriction that is required for the suggested instrument to be valid is that the functional form of the direct relationship between local spending or local taxes and the

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[9] Since our suggested procedure estimates direct causal effect, omitted variable bias is not a concern. This assumption will be examined when testing for instrument validity. See below for more details.

[10] A similar approach is taken by Guryan (2003) when estimating the effect of school spending on students' test scores. The grant nonlinearity can possibly be analyzed by the regression-discontinuity method (see, e.g., Angrist and Lavy, 1999; Hahn et al., 2001; and Lee, 2005). However, the grant formula used in this paper differs from a classical regression discontinuity design in two aspects. First, there is no "jump" in treatment at the cutpoint. Second, all treated do not receive the same amount, but the grants are proportional to treatment-determining variables (net out-migration and net change in school-aged children). Future research may allow us to study this using a regression-discontinuity approach with continuous treatment depending on covariates.

[11] In the estimations, we use up to a fourth-order polynomial in the smooth functions $f(\cdot)$ and $h(\cdot)$.

treatment-determining covariates (as given by the smooth functions in net out-migration and in net change in the number of school-age children) is not the same as the functional form of the relationship between treatment-determining covariates and grants (described by the discontinuous out-migration grant formula, i.e. the selection mechanism).

When specifying the control functions, we will use two different approaches. In the first, and perhaps most direct approach, we will follow the specifications given in equations (4) and (5) and include the net out-migration rate and the net change in the number of school-age children as separate variables in the control functions. An advantage with this approach is that the fact that the allocation formula changed in the year 2000 (to include the migration of school-age children) can potentially help us in identifying the effect we are looking for.[12] However, when using high-order polynomials, we end up with a large number of covariates, potentially leading to too limited variation for identification of the parameter of main interest. In the second approach, we therefore construct a single variable based on the two rules determining grant assignment (given by equations (1) and (2)). The variable, which will be used in a single control function, is constructed as follows; when $g_{it}^{m,s} = 0$ (c.f. equation (2) above) the argument within parentheses in the control function is the net out-migration rate ($m_{(it-2)-(it-12)}$), just as in $f(\cdot)$ above. When $g_{it}^{m,s} > 0$ the argument will instead be the sum of the net out-migration rate ($m_{(it-2)-(it-12)}$) and the change in the number of school-age children ($s_{(it-2)-(it-5)}$). This rule will be referred to as the composite rule.

We need to test the validity and the relevance of our suggested instrumental variable. The relevance of the excluded instrument can be examined by looking at the *t*-value for the coefficient on the excluded variable in the first-stage estimates. How to test for validity of the instrument is not obvious in our context. The model is not overidentified, which rules out tests for overidentifying restrictions (like the Sargan test). Another more indirect way of testing the validity has been suggested by Altonji *et al.* (2002, 2005). They suggest that the plausibility of exogeneity can be evaluated by testing whether the point estimates from the instrumental variable regression are sensitive to the inclusion of additional control variables. The idea is that if the estimates are insensitive to controlling for observables then they should also be

[12] The control functions are meant to capture the direct effects of the treatment-determining covariates on local spending and local tax rates. We argue that this direct effect is likely be the same before the year 2000, when the grant formula was changed, and after. Therefore, the change in school-aged children will be included in the control function both before and after 2000.

insensitive to unobservable determinants of the outcome variable, that is, the omitted variable bias is likely to be quite small. For this method to work in practice, the set of control variables must be powerful in the sense that they should pick up the most important confounding variables. In the context of the flypaper effect, the demographic structure, the share of foreign citizens and the tax base are perhaps the most important variables to control for given the responsibilities of the local governments in Sweden.

5. Institutional background and data

Decentralized government in Sweden is among the largest in the world, with a comprehensive range of responsibilities, notably for primary education, child care and care for the elderly. The tax available, a local proportional income tax, is to the full discretion of the local government decision-makers and generates the main source of local government revenue; about 60–70 percent of total current revenue. The rest is made up of fees and central government grants, where 15–20 percent of total revenues consist of grants. After the grant reform in 1993, block grants dominate. The grant system was slightly changed in 1996 and the block grant was separated into income equalization and cost equalization in addition to a general per capita grant, and some transitional regulations. The general objective criteria of the grant system primarily relate to the private income level and demographics (age structure of the population). More specifically, the cost equalization aims at reducing the differences in structural cost conditions across municipalities, whereas the purpose of income equalization is to bring per capita tax revenues close to the national average. The local income tax gives local governments considerable discretion in the financing. Local governments are for example able to distribute federal grants to the local population by way of reducing the income tax. Hence, both crowding-in and crowding-out are possible outcomes in the Swedish setting.

In this paper we use an unbalanced panel of 283 municipalities observed over the time period 1996–2004.[13] The grant-formula that is used for identification is an element of the cost equalization grants specified to compensate for out-migration of the local governments. During the time period 1996–2004, the average out-migration grant as a fraction of total cost equalizing grants for eligible municipalities' amounts to around 14 percent.

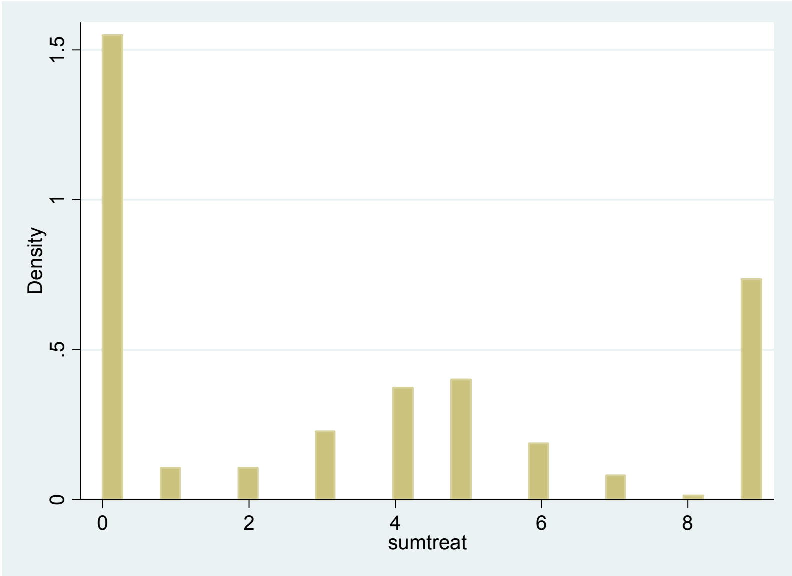
[13] The dataset covers all municipalities except for four that were affected by consolidations (Nykvarn, Södertälje, Knivsta and Uppsala) and three that have responsibilities that the other municipalities do not have (Gotland, Malmö and Göteborg).

As described in section 2, the municipalities with lower net out-migration than 2 percent do not receive any grants whereas municipalities above 2 percent receive grants. From year 2000 and forward there is also an extra compensation for those municipalities with a diminishing share of school-aged children. Over the studied period, 116 municipalities were never treated, 55 municipalities were treated all nine years and the remaining 112 municipalities received grants some, but not all years. Figure 2 shows the number of times the municipalities have been treated over the years. Table 1 gives summary statistics for the two grants variables (cost equalizing grants and migration grants, both measured per capita), the two dependent variables (local current spending, measured per capita, and the local tax rate) and the variables used in the control functions (net out-migration and change in population 7–18). As is clear from the table, the two outcome variables exhibits quite a large variation, reflecting the autonomy that the Swedish municipalities have in making their spending and taxing decisions. The negative minimum values of cost equalizing and migration grants reflect the fact that the grant system is self-financed. The table also presents the socio-economic variables used when testing the validity of the instruments (share of population in the age interval 0–6, share of population in the age interval 7–15, share of population aged 80 years or older, population size, the per capita tax base, and the share of foreign-born citizens).

Table 1 Summary statistics for the variables used in the empirical analysis.

Variable	Mean	St dev	Min	Max
Spending	38,027	7,303.8	20,606	68,380
Local tax rate	21.08	1.21	14.86	23.79
Cost equalizing grants	523.1	2,437.8	-3,471.0	13,196
Migration grants	118.7	315.1	-135.8	1577.2
Net out-migration	0.847	7.88	-42.95	16.64
Change in population 7–18	2.66	4.10	-10.25	23.59
Pop share 0–6	0.079	0.013	0.047	0.128
Pop share 7–15	0.122	0.012	0.068	0.164
Pop share 80+	0.054	0.014	0.012	0.091
Population	27,675	49,073	2,575	761,721
Tax base	105,963	19,163	69,399	256,754
Share foreign born	0.040	0.027	0.006	0.291

Figure 2: Distribution of the number of times the municipalities have been treated with out-migration grants over the time period 1996–2004.



6. The causal effect of grants on local spending and taxation

To examine the causal effect of grants on local spending and local tax rates, we conduct a graphical analysis before turning to the econometric evidence.

6.1 A graphical analysis

As a starting point, we take a descriptive look at the data to examine if there is a change in the relationship between the outcome variables (i.e., local spending or local tax rates) and the net out-migration rate at the cut-off point of two percent in the net out-migration rate.[14] This can give a first indication of whether we will be able to identify any effects via the grant formula.

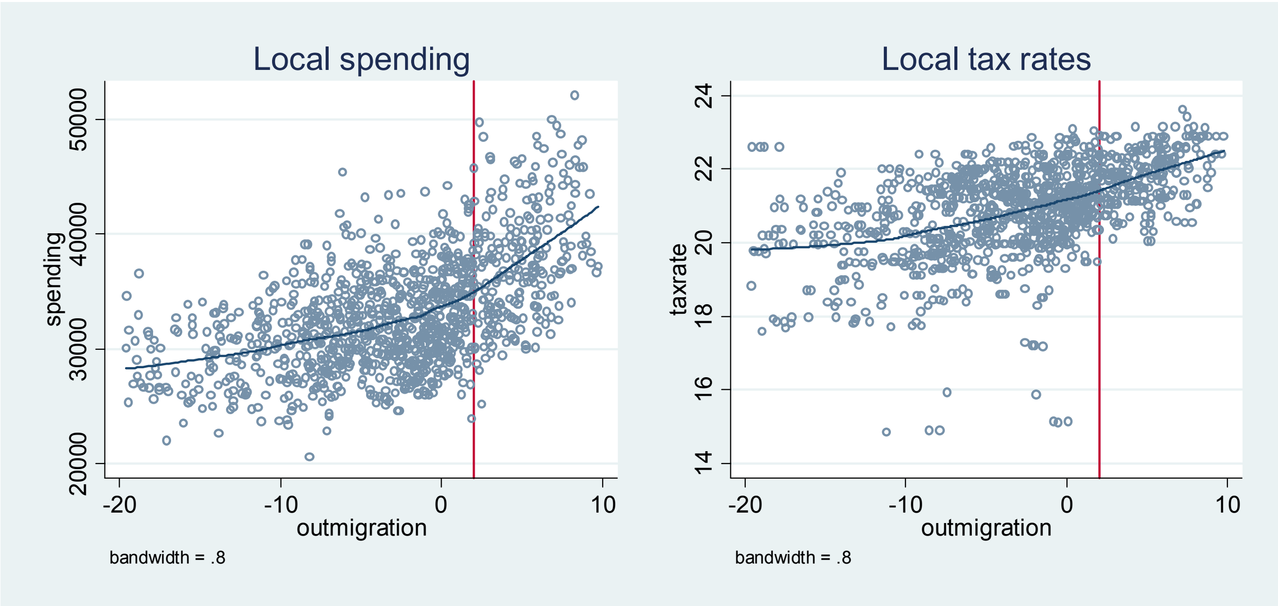
We start by plotting the two outcome variables against net out-migration (i.e. only using raw correlations). In the left part of Figure 3 we present the correlation between local spending and the net out-migration rate and in the right part we present the correlation between local tax rates and the net out-migration rate.[15] The left part of Figure 3 shows an increasing

[14] Since the cut-off point of two percent is only strict for the years 1996–1999, we will, for illustrative reasons, only use those years in the graphical analysis.

[15] Remember that we wouldn't expect a zero correlation between any of the two outcome variables and the net out-migration rate when looking at raw correlations - the IV method we use allows the historical net out-migration rate to have a direct impact on local spending and local tax rates. What we expect, is that the correlation is *different* before and after the cut-off point.

relationship between local spending and historical net out-migration and the relationship seems to be like a spline-function, where the slope is steeper after a net out-migration rate of two percent.[16] This pattern is not observed for the local tax rates (c.f. the right part of Figure 3). Even though there is an increasing relationship between the local tax rate and the net out-migration rate, there is no indication of a change in the steepness of the relationship beyond the cut-off line.

Figure 3. Outcome variables against net out-migration rate, raw correlations, 1996–1999



Our identifying assumption is that, once we have controlled for smooth functions of the outcome determining covariates, we have controlled for all direct effect of these covariates on the outcome. Any remaining relationship between the outcome variable and the net out-migration variable will then come from the grant formula, implying that there should be some action at an out-migration rate of two percent. To examine this, we estimate the second step equation (i.e., equation (5)), but leave out the predicted grants variable, and plot the residuals from that equation against net out-migration. What we would like to see is a zero relationship between the two variables for those municipalities that have not been treated (i.e., those municipalities with a net out-migration rate below two percent). We run the regression

[16] The vertical line in the figure is at a net out-migration rate of two percent. The estimated relationship is obtained through lowess smoothing, using the lowess command in STATA. Lowess carries out a locally weighted regression of the dependent variable on the net out-migration rate. In the figures, we have used the default bandwidth of 0.8.

(6) $$y_{it} = \alpha + \beta f\left(m_{(it-2)-(it-12)}\right) + \varepsilon_{it},$$

where y is either local spending or local tax rates and $f(\cdot)$ is up to a fourth-order polynomial in the historic out-migration rate. Equation (6) is similar to equation (5), but where we have excluded the grants variable and the smooth function in the change in the number of school-age children (since we are only using the years 1996-1999). The relationship between the estimated residuals from equation (6) (with a fourth-order polynomial in the treatment determining covariate) and the net out-migration rate are displayed in Figure 4.[17]

Figure 4. Estimated residuals from equation (6), with a fourth-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

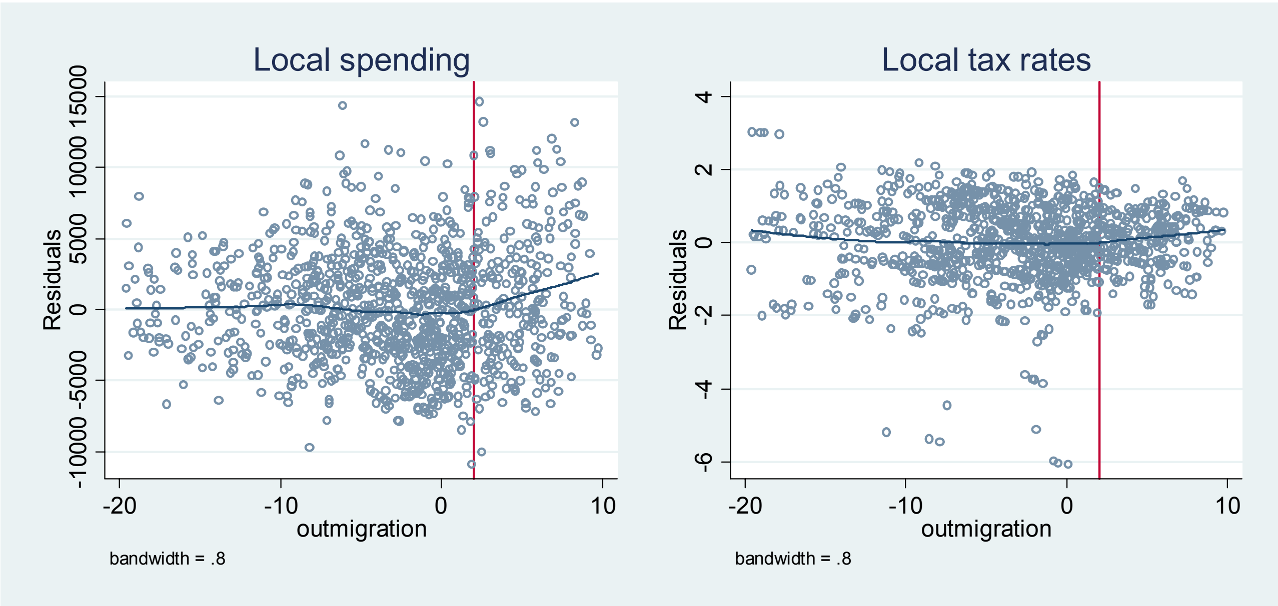


Figure 4 shows that there is more or less a zero relationship between spending and out-migration rate for those municipalities that have not been treated (i.e., those municipalities with a net out-migration rate below two percent). For the treated municipalities, however, there is a positive relationship between local spending and the net out-migration rate, from the

[17] In the figures, we show the relationship between the two variables for those municipalities with a net out-migration rate larger than -20 percent and lower than 10 percent. This constitutes the mass of the observations. The reason for excluding some municipalities with extreme out-migration rates is that they are so few and that the locally weighted regressions used in constructing the estimates in the figures are sensitive to the number of observations within the bandwidth. Graphs of residuals when we are using first-, second- and third-order polynomials in the treatment-determining covariate are presented in Figures A1–A3 in the Appendix.

cut-off point of two percent. We interpret this as evidence that our control function is flexible enough to control for any direct effects of net out-migration on local spending, which validates our instrument. Turning to the tax rate residuals, there is no sign of change at an out-migration of two percent, suggesting a zero relationship for all out-migration rates.

Overall, the graphical analysis points at two interesting facts. First, it suggests a positive effect of grants on spending and no effect on tax rates. Second, it gives an indication that the instrument we use is valid conditional on a flexible functional form in the treatment-determining variable. For a more thorough investigation, we next turn to the econometric analysis.

6.2 IV-estimates

Below, we present the results from the first stage estimations to examine the relevance of the excluded instrument and the two-stage least squares (2SLS) estimates.

6.2.1 First stage estimates

The first stage estimates from the 2SLS procedure (i.e. estimation of equation (4)) are presented in Table 2, where total cost equalizing aid is regressed on the net migration aid and on a polynomial of the treatment-determining covariates.[18] Each estimate/t-ratio-pair represents one regression. The estimates allow us to test whether the excluded instrument is relevant, by looking at the t-values for the grant formula (i.e. the exclusion restriction). In column i), we do not control for municipality-specific fixed effects, whereas these are included in column ii).[19] In the top panel we control for a polynomial (2^{nd}, 3^{rd} and 4^{th} order) of both net out-migration and change in the number of school-age children (measured as change in population aged 7–18), and in the bottom panel we specify the control function according to the composite rule (out-migration in years 1996–1999 and out-migration plus change in school-aged children for 2000–2004). The results using different specifications show that out-migration aid has a positive and statistically significant effect on total cost equalizing aid.

[18] We only report the first stage estimates for the baseline models. However, the results for the models estimated in the sensitivity analyses are very similar, with t-ratios clearly above 1.96 for the instrument.

[19] It is not clear from the literature whether on should control for municipality specific fixed effects or not. On the one side one could argue that given that we manage to capture any direct effects from the treatment-determining covariates in the control functions, fixed effects would not matter. On the other hand, the identifying variation will differ depending on whether fixed effects are controlled for or not; if fixed effects are excluded, both between and within variation are used, whereas only within variation is used when fixed effects are included.

Thus, the requirement that the formula must affect grants received by the municipality is fulfilled and our instrument is relevant.

Table 2 First stage estimates (out-migration grant on total cost-equalizing grant).

Control functions	Net out-migration rate and change in population aged 7–18	
2nd order polynomial	3.719 ***	1.476 ***
	(0.251)	(0.106)
3rd order polynomial	5.373 ***	1.248 ***
	(0.472)	(0.135)
4th order polynomial	6.507 ***	1.262 ***
	(0.505)	(0.139)
Control functions	Composite rule	
2nd order polynomial	3.150***	1.932 ***
	0.257	(0.095)
3rd order polynomial	3.317***	1.952 ***
	0.303	(0.107)
4th order polynomial	3.16***	1.944 ***
	0.314	(0.107)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2,538	2,538

Notes: Robust standard errors within parentheses. *** denotes significance at the 1 % level.

6.2.2 2SLS estimates

The second step estimates (estimations of equation (5)) are presented in Table 3 (spending) and Table 4 (taxes). As previously, we estimate the model both without (column i)) and with (column ii)) municipality-specific fixed effects. We control for a 2nd, 3rd, or 4th order polynomial in the treatment-determining covariates (i.e. in the top panel, out-migration, out-migration squared, change in students, change in students squared etc). Again, each estimate/t-ratio-pair corresponds to one regression.

All estimates in Table 3 are positive and significantly different from zero (in most cases they are significant at the one percent significance level). However, the estimated effects of grants on spending are considerably larger when we do not control for fixed effects; when using a 4th order polynomial of the control function the effect is 1.3 when fixed effects are excluded and 0.76 when fixed effects are included. Also the effect diminishes when controlling for higher order polynomials. Finally, when the control function is set according to the composite rule, we find somewhat larger estimates. It can be noted that none of the estimates are significantly different from 1; that is, we cannot reject the null hypothesis that an increase in general grants with 1 SEK/capita increases local spending with 1 SEK/capita. In other words, we cannot reject the null hypothesis of full crowding-in of block grants.

Table 3 Effects of grants on local spending. 2SLS estimates.

Control function	Net out-migration and change in population aged 7–18	
2nd order polynomial	1.104 ***	1.488 ***
	(0.112)	(0.274)
3rd order polynomial	1.416 ***	0.837 **
	(0.142)	(0.392)
4th order polynomial	1.315 ***	0.760 *
	(0.139)	(0.409)
Control function	Composite rule	
2nd order polynomial	1.554***	1.52***
	(0.148)	(0.183)
3rd order polynomial	2.117***	1.437***
	(0.201)	(0.193)
4th order polynomial	2.223***	1.436***
	(0.213)	(0.193)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2538	2538

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level.

Turning to the effects of grants on local tax rates, we see from *Table 4* that the results are somewhat sensitive to whether we control for fixed effects or not; when not controlling for fixed effects we sometimes get significant effects (although very tiny) whereas the estimated effects are insignificant when controlling for fixed effects. However, since the significant effects are quantitatively close to zero we conclude that the results are consistent with the flypaper hypothesis that grants are not transmitted to lower taxes.

Table 4 Effects of grants on local tax rate. 2SLS estimates.

Control function	Net out-migration and change in population aged 7–18	
2^{nd} order polynomial	-0.0001 ***	-0.00007
	(0.00004)	(0.00005)
3^{rd} order polynomial	0.00008 **	-0.00009
	(0.00004)	(0.00007)
4^{th} order polynomial	0.0001 ***	-0.0001
	(0.00003)	(0.00007)
Control function	Composite rule	
2^{nd} order polynomial	-8.31e-07	4.25e-06
	(0.00004)	(0.00003)
3^{rd} order polynomial	0.0002***	-3.01e-06
	(0.00004)	(0 .00003)
4^{th} order polynomial	0.0002***	0.00001
	(0.00004)	(0.00003)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2547	2547

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level.

6.2.3 Are the instruments valid?

One underlying assumption for the above results to be valid is that the instruments are truly exogenous. The graphical analysis in Figure 4 supports this exogeneity assumption. Altonji *et al.* (2002, 2005) suggest an alternative way of testing the assumption. The idea is that if the results are insensitive to the inclusion of additional observed covariates, they are probably also insensitive to the inclusion of other (potentially unobserved) variables. In Table 5 (spending) and Table 6 (taxes) we present the 2SLS estimates when controlling for the municipal tax base, share of population of age 0–6, share of population of age 7–15, share of population 80 years and older, total population, and the share of population born abroad. The estimated effect of grants on spending is marginally lower, and somewhat less precisely estimated. The estimated effect of grants on local taxes is quite similar when we include the socio-economic control variables. We conclude that in all, the result supports the claim that the instrument is exogenous.

Table 5 Effects of grants on local spending. 2SLS estimates, controlling for other covariates.

Control function	Net out-migration and change in population aged 7–18	
2nd order polynomial	1.060 ***	1.308 ***
	(0.099)	(0.300)
3rd order polynomial	1.291 ***	0.663 *
	(0.149)	(0.377)
4th order polynomial	1.252 ***	0.615
	(0.149)	(0.394)
Control function	Composite rule	
2nd order polynomial	1.372***	1.29***
	(0.127)	(0.241)
3rd order polynomial	1.850***	1.17***
	(0.188)	(0.253)
4th order polynomial	1.925***	1.162***
	(0.199)	(0.247)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2,538	2,538

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level. The estimations control for the following covariates: tax base, share of population of age 0–6, share of population of age 7–15, share of population 80 years and older, total population, and share of population born abroad.

Table 6 Effects of grants on local tax rate. 2SLS estimates controlling for other covariates.

Control function	Net out-migration and change in population aged 7–18	
2nd order polynomial	-0.00002	-0.00009 *
	(0.00003)	(0.00005)
3rd order polynomial	0.0001 ***	-0.0002 **
	(0.00004)	(0.00007)
4th order polynomial	0.0001 ***	-0.00025 **
	(0.00003)	(0.00007)
Control function	Composite rule	
2nd order polynomial	0.00006*	0.00005
	(0.00003)	(0.00004)
3rd order polynomial	0.0002***	0.00005
	(0.00004)	(0.00004)
4th order polynomial	0.0002***	0.00005
	(0.00005)	(0.00004)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2,547	2,547

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level. The estimations control for the following covariates: tax base, share of population of age 0–6, share of population of age 7–15, share of population 80 years and older, total population, and share of population born abroad.

6.2.4 Controlling for general autocorrelation in the error terms

So far, we have assumed that the error terms are independently distributed. Earlier studies focusing on Swedish local governments have, however, shown that dynamics matter in local decision-making (see, e.g., Dahlberg & Lindström, 1998; Dahlberg & Johansson, 1998, 2000; Bergström *et al*, 2004). Hence, we re-estimate the models presented in Table 3 and Table 4, allowing for arbitrary serial correlation within municipality (see, e.g., Kézdi, 2002).[20] The estimations are given in *Table 7* (spending) and *Table 8* (taxes). Allowing for potential autocorrelation in the errors yields, as expected, larger standard errors and, hence, lower significance. However, the effects of grants on spending are still statistically significant when not controlling for fixed effects and significant for the estimates controlling for fixed effects using a 2nd or 4th order polynomial in the top panel and for all polynomials in the bottom panel. Looking at taxes, the results are weaker when accounting for autocorrelation in the error terms, as compared to the baseline estimates. Since some of the estimates that earlier were statistically significant now have turned insignificant, this strengthens the conclusion that grants are not shifted into lower taxes,.

Table 7 Effects of grants on local spending. 2SLS estimates allowing for autocorrelation in the errors.

Control function	Net out-migration and change in population aged 7–18	
2nd order polynomial	1.104 ***	1.488 ***
	(0.209)	(0.412)
3rd order polynomial	1.416 ***	0.837
	(0.235)	(0.624)
4th order polynomial	1.315 ***	0.760 *
	(0.223)	(0.639)
Control function	Composite rule	
2nd order polynomial	1.554 ***	1.52***
	(0.282)	(0.285)
3rd order polynomial	1.372 ***	1.437***
	(0.227)	(0.318)
4th order polynomial	2.117 ***	1.436***
	(0.343)	(0.315)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2538	2538

Notes: Robust standard errors clustered on cross-sectional units within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level.

[20] This is handled by clustering the standard errors at the municipality-level, using the clustering routine in STATA.

Table 8 Effects of grants on local tax rate. 2SLS estimates allowing for autocorrelation in the errors.

Control function	Net out-migration and change in population aged 7–18	
2nd order polynomial	-0.0001	-0.00007
	(0.00008)	(0.00005)
3rd order polynomial	0.00008	-0.00009
	(0.00008)	(0.00007)
4th order polynomial	0.0001 *	-0.0001
	(0.00005)	(0.00007)
Control function	Composite rule	
2nd order polynomial	-8.31e-07	4.25e-06
	(0.00009)	(0.00005)
3rd order polynomial	0.0002 ***	-3.01e-06
	(0.00007)	(0.00005)
4th order polynomial	0.0002 ***	0.00001
	(0.00007)	(0.00005)
Time dummies	Yes	Yes
Fixed effects	No	Yes
No of observations	2547	2547

Notes: Robust standard errors clustered on cross-sectional units within parentheses. ***, ** and * denotes significance at the 1, 5 and 10 % level.

7. Concluding remarks

The paper investigates the causal effect of intergovernmental block grants on local public spending and taxes. Very few studies exist making a serious attempt at handling the possible endogeneity of grants. In this study we solve the endogeneity problem by using a discontinuity in the Swedish grant system. More precisely, we use an element of the grant system where only municipalities above a specific out-migration rate receive extra grants as an exclusion restriction in an IV-estimation. The analyses indicate that the instrument is both relevant and valid. The main conclusion is that we find evidence of crowding-in, where federal grants are shifted to more local spending, but not to reduced local taxes. Our results thus are consistent with a flypaper effect for Sweden.

The finding of a flypaper effect does not necessarily indicate irrational behavior of lower level governments. Rather, it seems realistic to assume that the existence of a flypaper effect depends on the fiscal institution studied. We study the integrated public sector in Sweden and argue that there may be a rational flypaper effect where federal government has better tax instruments than local governments while local governments has an advantage in service delivery. Modeling the flypaper effect under such a setting is a task for future research. Also, there is a need for more empirical work taking the likely endogeneity of federal grants seriously.

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Appendix

Figure A1. Estimated residuals from equation (6), with a first-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

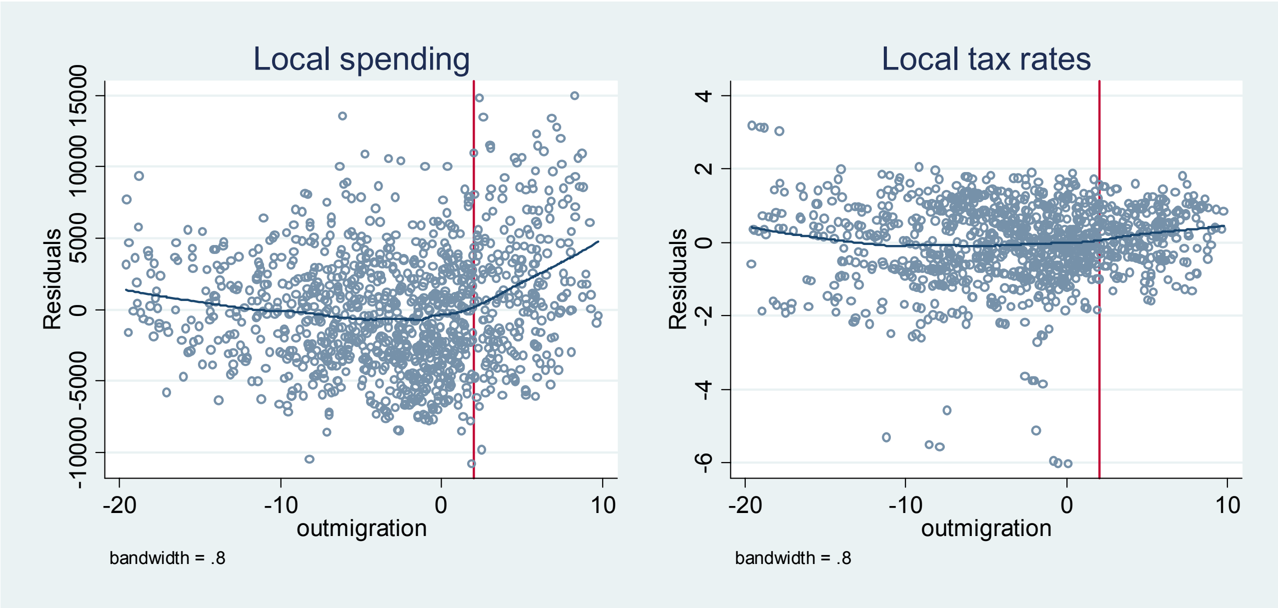


Figure A2. Estimated residuals from equation (6), with a second-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

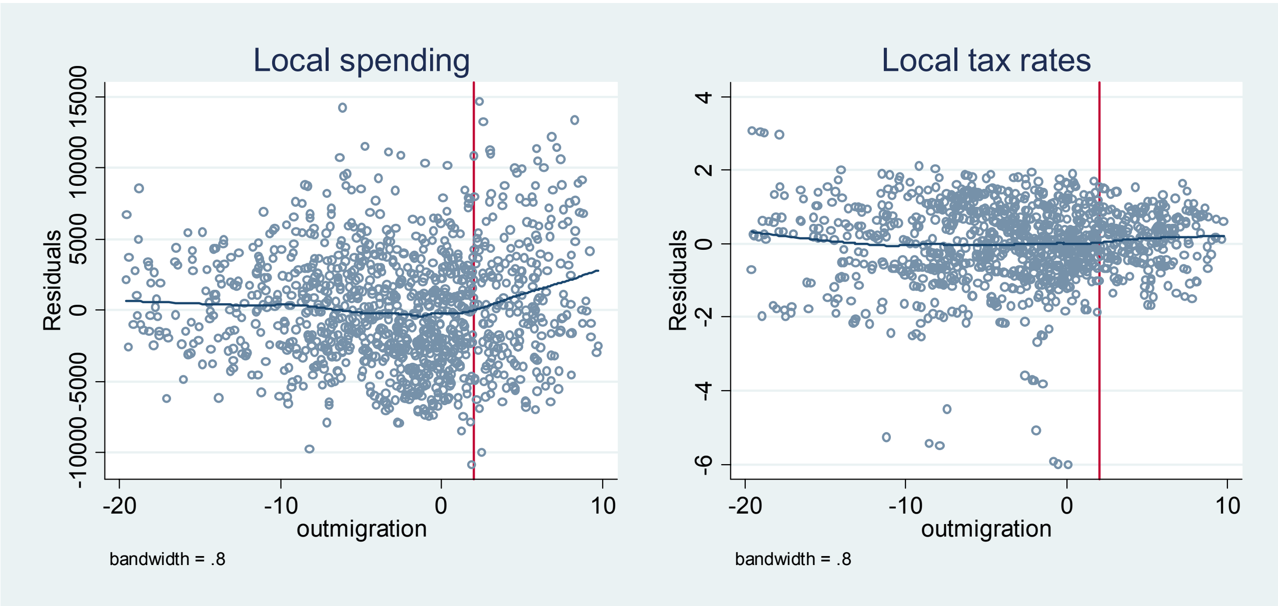
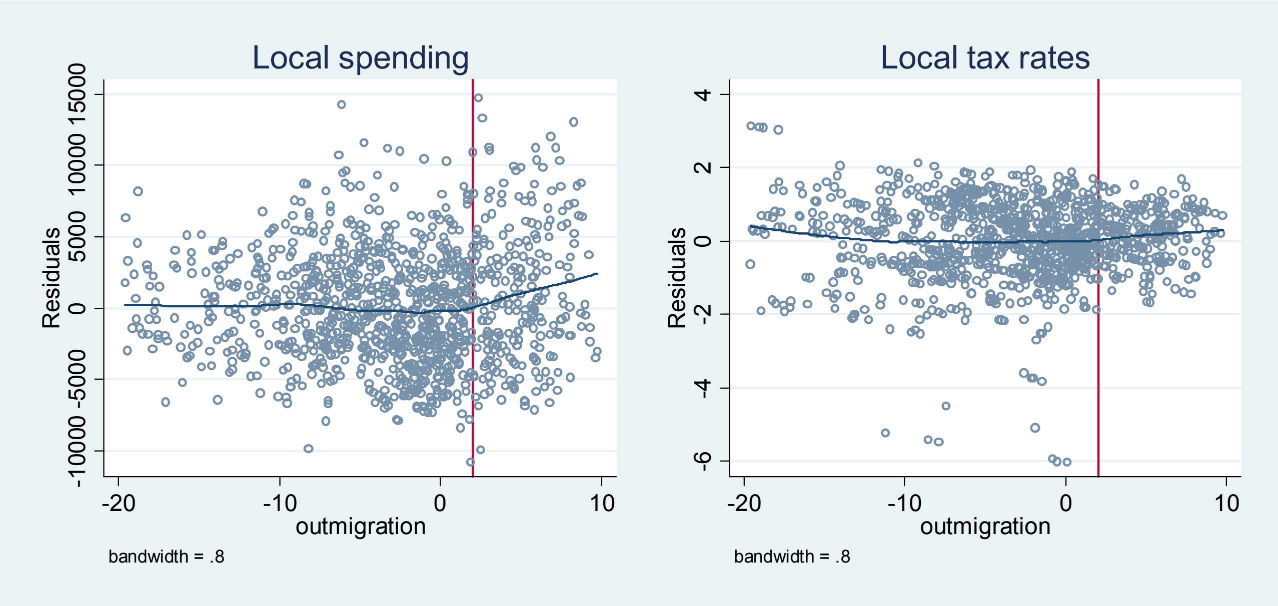


Figure A3. Estimated residuals from equation (6), with a third-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.



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