

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

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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. Federal governments often set grants based on characteristics and performance of decentralized 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.

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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 expenditures is typically not accompanied by equivalent revenue-raising responsibilities, and there is an imbalance between where money is spent and where revenues are collected. Intergovernmental grants consequently are an important part of the financing of decentralized governments.

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What effects do grants from the federal government have? In a 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. This means that grants crowd out local spending, i.e. that some of the grants are instead distributed as lower taxes. The empirical literature has never given much support to the predictions of the theoretical model. Instead, most empirical studies have found that grants have crowding-in effects, where spending is increased with higher grants to a larger degree than expected from theory (see Gramlich, 1977, for a summation of the early literature and Hines and Thaler, 1995, for a more recent overview).

When investigating the effect of grants, it is hard to defend the handling of grants as an exogenous factor affecting local governments.¹ Only few studies make attempts at handling the endogeneity of federal grants. Two recent studies have addressed the problem.² 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 empirical finding of crowding-in. His estimates indicate that grants crowd out highway spending when the policy endogeneity is accounted for. Gordon (2004) applies the underlying 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 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 (grants for highway spending and Title 1 grants) under the US setting. It is likely that different grant programs may have different effects and that the results may be sensitive to the institutional design. 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.³

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 tax rates. Compared to the papers by Knight and Gordon, we will use an alternative and improved identification strategy. More specifically, we make use of a discontinuity in the Swedish grant formula where municipalities with a net out-migration above 2% receive extra grants whereas municipalities with a net out-migration below 2% 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.⁴ The same identification strategy has earlier been used by Guryan (2003) in a paper examining the effect of school resources on student achievement. Our approach is in some ways similar to the one adopted by Gordon in 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

¹ The problem of estimating incidence of endogenous policies was discussed by Besley and Case (2000). In addition, there are other econometric problems that make OLS inappropriate; see the discussion in Section 2.

² Instrumentation of federal grants has been applied in US-studies by Becker (1996), Gamkhar and Oates (1996), and Turnbull (1998), while Berg and Rattso (2006) take advantage of a tax reform changing the distribution of grants in Norway.

³ Also, looking at Knight’s first step estimates (see column 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.

⁴ That is, the variable that determines grants may also affect spending, as long as the way it affects spending differs from the way it determines the allocation of grants.

a panel of 279 Swedish municipalities observed over nine years. As opposed to earlier studies, we examine the effect of grants on both local spending and local tax rates. This allows us to determine whether federal resources are redistributed to private income through reduced tax rates. We find evidence of crowding-in, where federal grants are shifted to more local spending, but not to reduced local taxes.

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

These four possibilities of endogeneity indicate that estimates from an OLS specification would very likely be biased. If endogeneity of grants was the only source of OLS-bias, a straightforward comparison of IV and OLS estimates could inform us about the size and sign of this bias. However, in our context there are also other sources of bias in the OLS-estimate of a grant effect, notably that grants depend on characteristics of the local government that influence spending and taxation (like age composition of the population). It is hard to control for all these effects, and some of them are endogenous, implying that OLS also would suffer from other sources of bias. Local characteristics

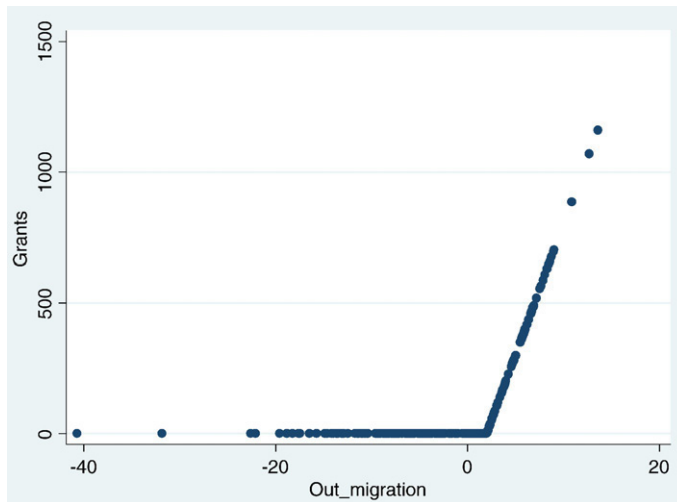


Fig. 1. Out-migration grants (SEK/capita) against net out-migration, 1999.

that are compensated in the grant system are likely to have a positive independent effect on local spending and will give upward bias in the grant estimate when they are not controlled for. Political factors may give downward bias, i.e. if local governments with non-socialist majorities are favored in political grant distribution and they are associated with lower spending. Johansson (2003) shows that political factors are of relevance for the grant distribution in Sweden. A comparison between IV and OLS estimates only reveals the net bias, and it is hard to disentangle the different sorts of bias. These additional problems with OLS strengthen the case for our approach to identification of a causal effect of grants, since our method corrects also for these other weaknesses of OLS estimation.

3. The grant formula used for identification

In this paper we will use an element in the cost equalizing grant formula⁵ 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 2% 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:

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

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. Fig. 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 2% where municipalities with lower net out-migration than 2% do not receive any grants whereas municipalities above 2% receive grants.⁶

In the year 2000, an additional component was introduced where municipalities were compensated also for 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 2%. More formally, municipality i receives additional out-migration grants in year t ($g_{it}^{m,s}$) according to the following rule:

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

⁵ The cost equalizing grant is a block grant to support municipalities that are characterized by demographic and other structural conditions associated with higher costs.

⁶ The block grant is self-financed and the total cost for this grant component is divided equally (per capita) between all municipalities.

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 .⁷ Our identification strategy, discussed more below, will use the discontinuities in both Eqs. (1) and (2), as well as the introduction of the rule in Eq. (2).

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

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

where y is spending or tax rate and g is intergovernmental block grants. However, if we estimate the equation directly our estimated parameters will be biased. We will focus on the endogeneity problem, and our estimation method also corrects for other sources of bias.⁸

To solve the endogeneity problem, we propose using an instrumental variables (IV) estimator, where the formula for the out-migration grant ($\Omega(m_{(it-2)-(it-12)}, s_{(it-2)-(it-5)})$) is used as an excluded instrument.⁹ 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 (2SLS) procedure is given by

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

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

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

where \hat{g} is the predicted grants obtained from the estimation of Eq. (4).¹⁰ The exclusion restriction that is required for the instrument to be valid is that the functional form of the direct relationship between local spending or local tax rates and the 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).

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 developed by Altonji et al. (2002, 2005). They suggest that the plausibility of exogeneity can be evaluated by testing whether the point estimates of 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 insensitive to unobservable determinants of the outcome variable, that is, the omitted variable bias is likely to be quite small. In Section 6.2.2 we will employ this method in order to investigate the validity of the

⁷ 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.

⁸ 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.

⁹ 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; Lee, in press). The grant formula used in this paper differs from a classical regression discontinuity design in two respects. First, all treated do not receive the same amount and, second, there is no "jump" in treatment at the cutpoint.

¹⁰ In the estimations, we use up to a fourth-order polynomial in the smooth functions $f(\cdot)$ and $h(\cdot)$.

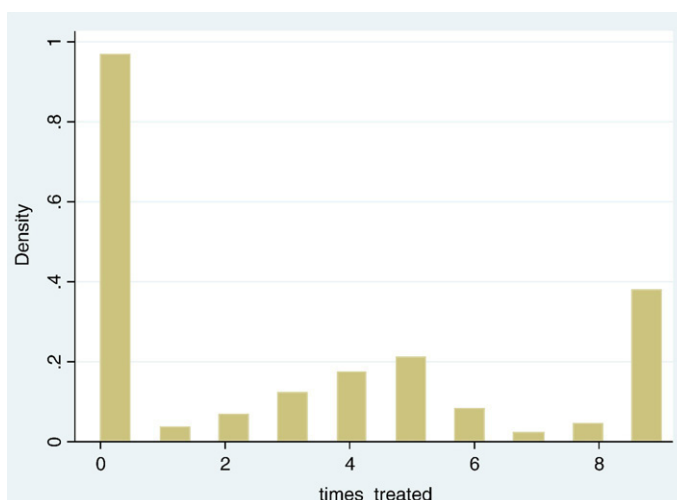


Fig. 2. Distribution of the number of times the municipalities have been treated with out-migration grants over the time period 1996–2004.

instruments. In addition, we will in Section 6.1 conduct some graphical analysis that will guide us in judging the relevance and validity of the instruments.

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 for the local government, a proportional income tax, is to the full discretion of the local government decision-makers and generates the main source of local government revenues. The local income tax gives local governments considerable discretion in their financing decisions. It follows that local governments are 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. Local tax revenues amount to about 60–70% of total current revenue. The rest is made up of fees and central government grants, where 15–20% of total revenues consist of grants, but this figure varies a lot between different municipalities. After a major grant reform in 1993, block grants dominate. Block grants consist of income equalization grants, cost equalization grants and a general per capita grant. 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.¹¹

In this paper we use a panel of 279 municipalities observed over the time period 1996–2004.¹² 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 19%, whereas the cost equalizing grants for eligible municipalities amount to around 14% of total grants. As described in Section 2, the municipalities with lower net out-migration than 2% do not receive any extra grants whereas municipalities above 2% receive these grants. From year 2000 and onwards there is also an extra compensation for those municipalities with a diminishing share of school-aged children. Over the period studied 128 municipalities were never treated (i.e. never received any out-migration grants), 50 municipalities were treated all nine years and the remaining 101 municipalities received grants some, but not all years. Fig. 2 shows the distribution of 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

¹¹ Both the equalizing grants are self-financed by equal per capita contributions from all municipalities.

¹² The dataset covers all municipalities except for eight that were affected by consolidations (Bollebygd, Borås, Lekeberg, Örebro, Nykvarn, Södertälje, Knivsta and Uppsala) and three that have responsibilities that the other municipalities do not have (Gotland, Malmö and Göteborg). We have 9 missing observations for local spending.

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

Variable	Mean	St dev	Min	Max
Spending	38,050	7,320.9	20,606	68,380
Local tax rate	21.08	1.22	14.86	23.79
Cost equalizing grants	523.1	2,452.2	−3,471.0	13,196
Migration grants	121.4	316.5	−135.8	1,577.2
Net out-migration	−0.789	7.92	−42.95	16.64
Change in population 7–18	2.65	4.08	−10.25	23.59
Share of population 0–6	0.079	0.013	0.047	0.128
Share of population 7–15	0.122	0.012	0.068	0.164
Share of population 80+	0.054	0.014	0.012	0.091
Population	27,229	48,884	2,575	761,721
Tax base	105,957	19,219	69,399	256,754
Share foreign born	0.040	0.027	0.006	0.291

Notes: Spending, grants and tax base are defined in SEK per capita. Net out-migration and change in the population 7–18 are given in percent. Data is obtained from Statistics Sweden.

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 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).

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

6.1. A graphical analysis

As a starting point, we 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 2% in the net out-migration rate.¹³ The graphical analysis gives a first indication of whether we will be able to identify any effects via the grant formula. In addition, the graphical analysis tells us something about the validity of the instruments as well as of their relevance.

We start by plotting the two outcome variables against net out-migration (i.e. only using raw correlations). In the left part of Fig. 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.¹⁴ The left part of Fig. 3 shows an increasing 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 2%.¹⁵ This pattern is not observed for the local tax rates (c.f. the right part of Fig. 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.

¹³ For illustrative reasons, we will only use the years 1996–1999 in the graphical analysis, since it was only one variable (the net out-migration rate) that determined whether a municipality received grants or not for those years. From 2000 the change in number of school aged children was added, making it hard to illustrate it graphically for the latter part of the period.

¹⁴ Remember that we would not 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.

¹⁵ The vertical line in the figure is at a net out-migration rate of 2%. 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.

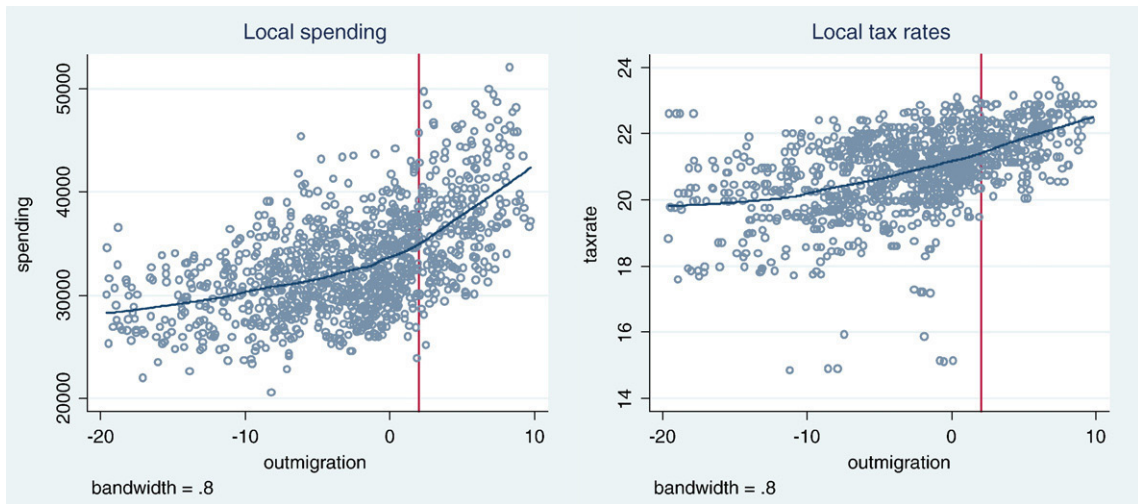


Fig. 3. Outcome variables against net out-migration rate, raw correlations, 1996–1999.

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 change in the slope at an out-migration rate of 2%. To examine this, we estimate the second step equation (i.e., Eq. (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 2%). We run the regression

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

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.¹⁶ The relationship between the estimated residuals from Eq. (6) (with a fourth-order polynomial in the treatment determining covariate) and the net out-migration rate are displayed in Fig. 4.¹⁷ This can be seen as the graphical analog to the second stage estimates in the 2SLS estimations.

Fig. 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 2%). For the treated municipalities, however, there is a positive relationship between local spending and the net out-migration rate, from the cut-off point of 2%.¹⁸ We interpret this as evidence that grants have a positive effect on spending and that our control function is flexible enough to control for any direct effects of net out-migration on local spending (i.e., a validation of our instrument). Turning to the tax rate residuals, there is no sign of change at an out-migration of 2%, suggesting a zero relationship for all out-migration rates.

To get an indication of the relevance of the instrument, we can also conduct a graphical analysis that corresponds to the first-stage estimates in the 2SLS procedure. Relevance requires that the out-migration grants have an effect on the cost-equalizing grants. To investigate this, we run the following regression

$$g_{it} = \gamma + \phi' f(m_{(it-2)-(it-12)}) + \eta_{it}, \tag{7}$$

¹⁶ Eq. (6) is similar to Eq. (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).

¹⁷ In the figures, we show the relationship between the two variables for those municipalities with a net out-migration rate larger than -20% and lower than 10%. 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 Figs. A1–A3 in the Appendix.

¹⁸ That the slope starts to increase somewhat just before the threshold is quite likely due to the fact that lowest smoothing uses data in a moving window, implying that just before the cut-off point, it uses data from both treated and non-treated municipalities.

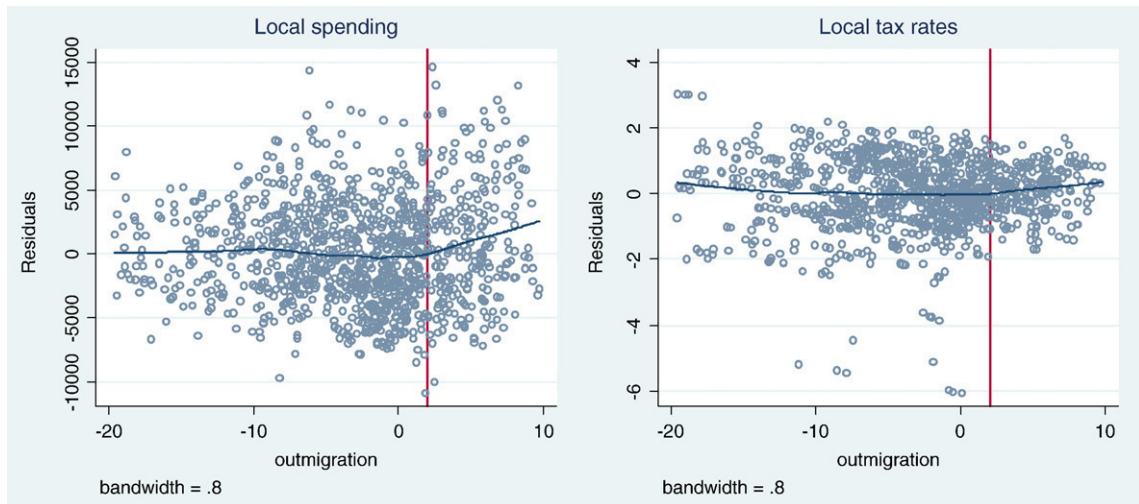


Fig. 4. Estimated residuals from Eq. (6), with a fourth-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

where g is cost-equalizing grants and $f(\cdot)$ is up to a fourth-order polynomial in the historic out-migration rate and plot the estimated residuals against the net out-migration rate.¹⁹ For relevance we would like to see a positive relationship between these two variables for the treated municipalities. The relationship between Eq. (7) (with a fourth-order polynomial in the treatment determining covariate) and the net out-migration rate is displayed in Fig. 5.²⁰ From the figure it seems like the instrument is clearly relevant.

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 and relevant 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. An econometric analysis

6.2.1. Baseline estimates

In this section we present the first stage estimates (to examine the relevance of the excluded instrument) and the two stage least squares (2SLS) estimates (to investigate the causal effect of grants on local spending and local tax rates). Given that the polynomials manage to control for all direct effects of the treatment-determining covariates, there should be no omitted variable problem, and controlling for additional covariates should not affect the point-estimates. In the baseline specification we have therefore chosen to estimate a parsimonious model where we, except for polynomials in the treatment-determining covariates, only include yearly time dummies. In the subsequent section, we then test the validity of the instrument by adding more explanatory variables to the model.

When specifying the control functions, we will use three different approaches in the baseline analysis. In the first column in Tables 2–4 we only control for the net out-migration, in order to keep the number of estimated parameters at a minimum (and not lose degrees of freedom). The argument for using the net out-migration rate on its own is that it is the most important treatment-determining covariate in the sense that it is in effect for the whole time period. In the second column in Tables 2–4 we follow the specifications given in Eqs. (4) and (5) and include the net out-migration rate as well as the net change in the number of school-age children in the control functions. An advantage with this approach is that the change in the allocation formula in the year 2000 (to include the migration of school-age children) can potentially help us in identifying the effect we are looking for.²¹ Finally, in the last column in Tables 2–4 we also

¹⁹ Eq. (7) is similar to Eq. (4), 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).

²⁰ Figures when Eq. (7) is estimated with lower-order polynomials in the net out-migration rate are presented in the Appendix.

²¹ 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.

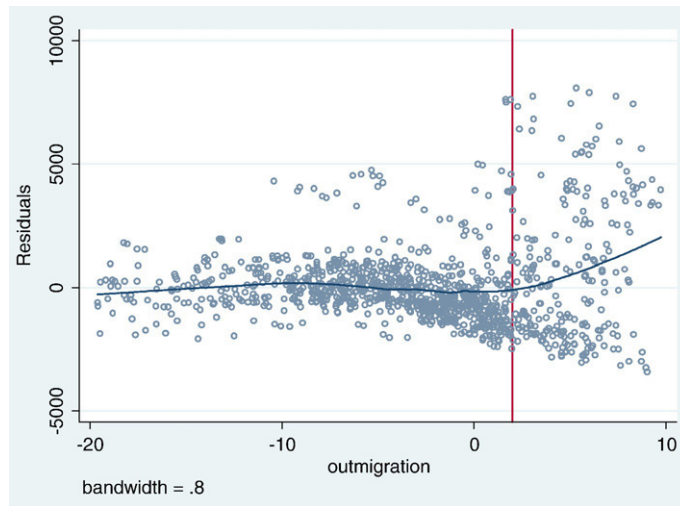


Fig. 5. Estimated residuals from Eq. (7), with a fourth-order polynomial in the net out-migration rate, against the net out-migration rate.

control for polynomials in the interaction between the two treatment-determining covariates. The last specification thus provides us with a very flexible functional form in the treatment-determining covariates. In all specifications, we control for a 2nd, 3rd, or 4th order polynomial in the treatment-determining covariates.

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

	Control functions		
	Net out-migration rate (OM)	OM+change in school age children (SAC)	OM+SAC+OM*SAC
2nd order polynomial	3.431*** (0.252)	3.714*** (0.253)	3.504*** (0.312)
3rd order polynomial	4.858*** (0.457)	5.355*** (0.475)	5.474*** (0.505)
4th order polynomial	5.981*** (0.492)	6.485*** (0.509)	6.737*** (0.524)
Time dummies	Yes	Yes	Yes
Fixed effects			
Clustered errors			
Other covariates			
No of observations	2502	2502	2502

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

Table 3
Effects of grants on local spending

	Control functions		
	Net out-migration rate (OM)	OM+change in school age children (SAC)	OM+SAC+OM*SAC
2nd order polynomial	1.000*** (0.123)	1.075*** (0.113)	1.252*** (0.128)
3rd order polynomial	1.217*** (0.161)	1.396*** (0.142)	1.332*** (0.142)
4th order polynomial	1.172*** (0.160)	1.305*** (0.139)	1.303*** (0.141)
Time dummies	Yes	Yes	Yes
Fixed effects			
Clustered errors			
Other covariates			
No of observations	2502	2502	2502

2SLS estimates.

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

Table 4
Effects of grants on local tax rate

	Control functions		
	Net out-migration rate (OM)	OM+change in school age children(SAC)	OM+SAC+OM*SAC
2nd order polynomial	−0.0002*** (0.00004)	−0.0001*** (0.00004)	−0.00008* (0.00004)
3rd order polynomial	2.42e−06 (0.00004)	0.00008** (0.00004)	0.00004 (0.00004)
4th order polynomial	0.00005* (0.00003)	0.0001*** (0.00003)	0.00008** (0.00003)
Time dummies	Yes	Yes	Yes
Fixed effects			
Clustered errors			
Other covariates			
No of observations	2,511	2,511	2,511

2SLS estimates.

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

The first stage estimates from the 2SLS procedure (i.e. estimation of Eq. (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.²² Each estimate/standard errors-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). The results show that out-migration aid has a positive and statistically significant effect (at the one-percent level) on total cost equalizing aid, regardless of how we specify the control functions. Increasing the number of polynomials increases the point-estimate somewhat. The overall conclusion from the first stage estimates is that the requirement that the formula must affect grants received by the municipality is fulfilled and, hence, that our instrument is relevant.

The 2SLS estimates (estimations of Eq. (5)) are presented in Table 3 (spending) and Table 4 (tax rates).²³ All estimates in Table 3 are positive and significantly different from zero at the 1% significance level. 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. The results are not much affected by the specification of the control functions (compare the results in the first column with those in the second and last columns). This is in particular true for the inclusion of the interaction between the two treatment-determining covariates.²⁴

Turning to the effects of grants on local tax rates, we see from Table 4 that there seems to be no effect. In some cases, we get no (or very low) statistical significance, and when the point estimates are statistically significant, the estimated effects are quantitatively very close to zero. We conclude that there seems to be no effect of grants on the local tax rate.²⁵ As for the spending equation, it is clear that the results are not much affected by the specification of the control functions. An implication of this is that we will only conduct further analysis on the specifications in the

²² These are the first stage estimates for the spending-equation. For the tax rate-equation, there are nine more observations, but the estimates are almost identical.

²³ For the sake of comparison, we have estimated an OLS-model where we investigate how total grants affect local spending and tax rates. The results from the OLS-estimations are given in the Appendix. As argued in Section 2 above, the OLS estimates are expected to be biased both because of the endogeneity of grants and because grants depend on demographic and other characteristics of the local governments. It follows that we cannot conclude about the grant endogeneity bias separately by comparing OLS and IV estimates. A comparison will show only the net of several sources of bias.

²⁴ Looking at the coefficients for the interaction variables (not reported), it also turns out that they are almost never significant, only in one or two cases out of all specifications, and they are never significant in specifications with polynomials of higher order than two.

²⁵ We have also analyzed the effect of grants on local tax revenues instead of the tax rate. The lack of a tax rate effect may result from an out-migration that reduces the tax base. It turns out that grants have no significant effect on local tax revenues once we apply a 3rd order polynomial or higher in the control function (results not reported).

Table 5
Sensitivity analysis: the effects of grants on local spending

	i)	ii)	iii)	iv)	v)	vi)	vii)
2nd order	1.461***	1.038***	1.075***	1.266***	1.461***	1.038***	1.266***
polynomial	(0.279)	(0.100)	(0.209)	(0.305)	(0.419)	(0.177)	(0.434)
3rd order	0.780**	1.282***	1.396***	0.596	0.780	1.282***	0.596
polynomial	(0.404)	(0.150)	(0.234)	(0.386)	(0.640)	(0.247)	(0.603)
4th order	0.693*	1.247***	1.305***	0.541	0.693	1.247***	0.541
polynomial	(0.421)	(0.149)	(0.223)	(0.404)	(0.652)	(0.241)	(0.620)
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes			Yes	Yes		Yes
Observable covariates		Yes		Yes		Yes	Yes
Cluster on municipality			Yes		Yes	Yes	Yes
No of obs	2502	2502	2502	2502	2502	2502	2502

2SLS estimates.

Notes: Robust standard errors within parentheses. **, ** and * denotes significance at the 1, 5 and 10% level. The set of observable covariates include municipal tax base, share of population aged 0–6, share of population aged 7–15, share of population 80 years and older, total population, and the share of population born abroad.

second column (in order not to lose too many degrees of freedom). Next we turn to an examination of the validity of the instrument.²⁶

6.2.2. 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 Fig. 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. We can think of three different ways of examining this. First, we control for municipality-specific fixed effects. When controlling for municipality-specific fixed effects we control for factors (potentially unobserved) that differ between municipalities, but are constant over time within each municipality. Second, we control for time-varying covariates, since spending and tax rates might also depend on factors that vary over time. By including a number of covariates that typically are used when analyzing local governmental behavior (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) we can control for these factors. Third, we cluster the standard errors on cross-sectional units. Earlier studies focusing on Swedish local governments have shown that dynamics matter in local decision-making (see, e.g., Dahlberg and Lindström, 1998; Dahlberg and Johansson, 1998, 2000; Bergström et al., 2004). Hence, we re-estimate the models presented in Tables 3 and 4, allowing for arbitrary serial correlation within municipality (see, e.g., Kézdi, 2002 for a discussion about this).²⁷

The results from these sensitivity analyses are given in Table 5 (spending) and Table 6 (tax rates).²⁸ In column i) we add fixed effects to the baseline specification, in column ii) we add time-varying covariates to the baseline specification, and in column iii) we allow the standard errors to be serially correlated within municipality. In columns iv)–vii) we thereafter add different combinations of these three aspects to the baseline specification.

Starting with the effects of grants on local spending in Table 5, we see that including fixed effects (column i) makes the estimates somewhat lower; for the 3rd order polynomial we find an effect of 0.78 compared with the estimate of 1.40 in the baseline specification. The effect is still not statistically different from 1. If we instead control for a number of time-varying observed covariates, not much happens (see column ii). When allowing the

²⁶ It is of interest to investigate the possible effect of heterogeneity across municipalities with respect to demographics. We have re-estimated the baseline specification allowing for different effects of grants for municipalities with many elderly and many school-aged children respectively. Interaction effects are estimated using a dummy taking the value one for the municipalities with the 25th percentile largest share of population aged 7–16 and, in another regression, using a dummy variable for the municipalities with the 25th percentile largest share of population aged 80 or older. Heterogeneous effects based on age of the population seem to be unimportant (results not reported).

²⁷ This is handled by clustering the standard errors at the municipality-level, using the clustering routine in STATA.

²⁸ We only report the second-stage estimates for these specifications. However, the first-stage results are very similar to the ones in the baseline model, with *t*-ratios clearly above 1.96 for the instrument.

Table 6
Sensitivity analysis: the effects of grants on local tax rates

	i)	ii)	iii)	iv)	v)	vi)	vii)
2nd order	−0.00007	−0.00002	−0.00012	−0.00010*	−0.00007	−0.00002	−0.00010
polynomial	(0.00005)	(0.00003)	(0.00008)	(0.00005)	(0.00008)	(0.00005)	(0.00008)
3rd order	−0.00010	0.00012***	0.00008	−0.00017**	−0.00010	0.00012	−0.00017
polynomial	(0.00007)	(0.00004)	(0.00008)	(0.00007)	(0.00011)	(0.00007)	(0.00010)
4th order	−0.00012*	0.00013***	0.00010*	−0.00017**	−0.00012	0.00013**	−0.00017*
polynomial	(0.00007)	(0.00003)	(0.00005)	(0.00007)	(0.00011)	(0.00005)	(0.00010)
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes			Yes	Yes		Yes
Observable covariates		Yes		Yes		Yes	Yes
Cluster on municipality			Yes		Yes	Yes	Yes
No of obs	2511	2511	2511	2511	2511	2511	2511

2SLS estimates.

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10% level. The set of observable covariates include municipal tax base, share of population aged 0–6, share of population aged 7–15, share of population 80 years and older, total population, and the share of population born abroad.

standard errors to be serially correlated within municipality, the estimated standard errors grow somewhat, as expected, but all estimates are still statistically significant at the one-percent level. In the next four columns we combine these three extensions, and in column vii) we control for fixed effects, as well as observed covariates and serial correlation in the standard errors. In summary, we conclude that our baseline estimates are quite robust to the inclusion of other factors potentially affecting spending. We interpret this as an indication that our instrument is valid, something that was also indicated by the graphical analysis.

The results from the same analysis for the local tax rates are given in Table 6. We find that the baseline results are not much affected by the inclusion of other covariates; the point estimates are still of a negligible magnitude, and the estimates are often statistically insignificant.

To sum up, the main conclusions from our baseline estimates remain when controlling for other potential determinants of local spending and tax rates: We find evidence of crowding-in, where federal grants are shifted to more local spending, but not to reduced local taxes.

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 contradict with the results of Knight (2002), who finds that grants crowd out spending when grants are instrumented. One explanation for the differing results might be that he studies highway grants and highway spending under the US setting, whereas we study general grants and total spending under the Swedish setting. Another explanation might be that the instruments used by Knight might be problematic, as discussed earlier in this paper.

It is interesting to note that our results are consistent with a flypaper effect (i.e. that money stick where they hit) for Sweden. This result contradicts the theoretical prediction from the median voter theory, and is in line with much of the earlier empirical evidence. The somewhat puzzling lack of evidence for the median voter model is thus not necessarily due to methodological flaws of the earlier empirical literature. 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. In such a setting 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.

Appendix A

Table A.1
OLS-estimates

	Spending	Tax rate
Grants	0.316*** (0.046)	0.000003 (0.000009)
Time dummies	Yes	Yes
Fixed effects	Yes	Yes
Observable covariates	Yes	Yes
Cluster on municipality	Yes	Yes
No of obs.	2,502	2,511

Notes: Robust standard errors within parentheses. ***, ** and * denotes significance at the 1, 5 and 10% level. The set of observable covariates include municipal tax base, share of population aged 0–6, share of population aged 7–15, share of population 80 years and older, total population, and the share of population born abroad.

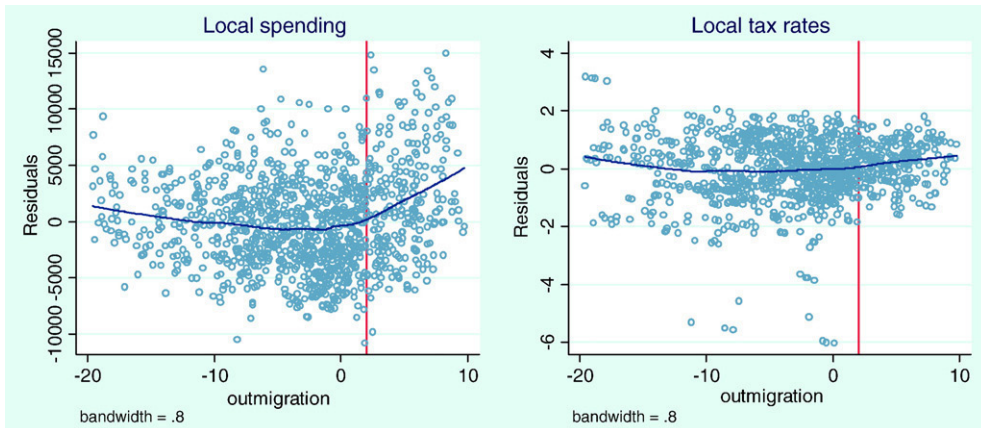


Fig. A.1. Estimated residuals from Eq. (6), with a first-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

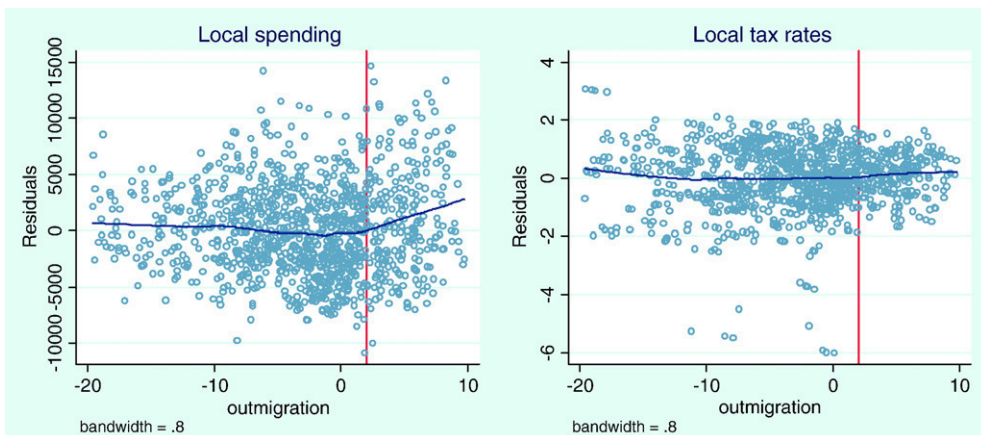


Fig. A.2. Estimated residuals from Eq. (6), with a second-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

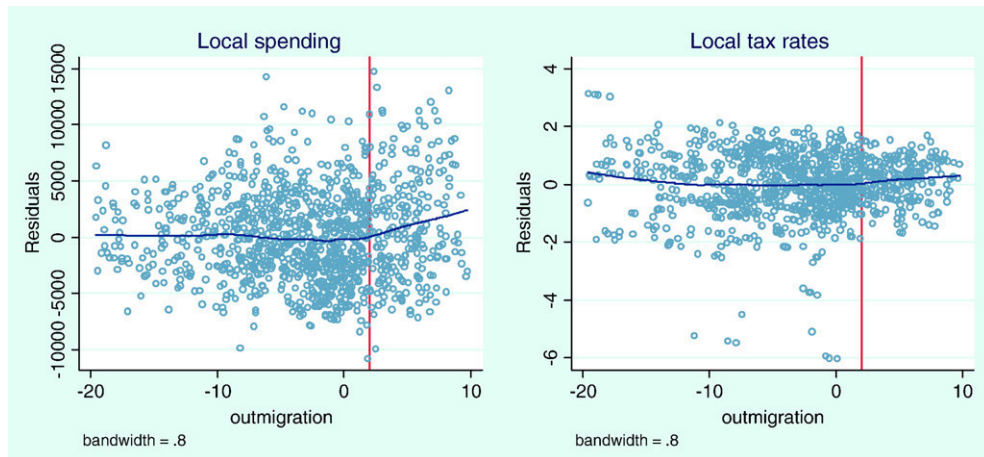


Fig. A.3. Estimated residuals from Eq. (6), with a third-order polynomial in the net out-migration rate, against net out-migration rate, 1996–1999.

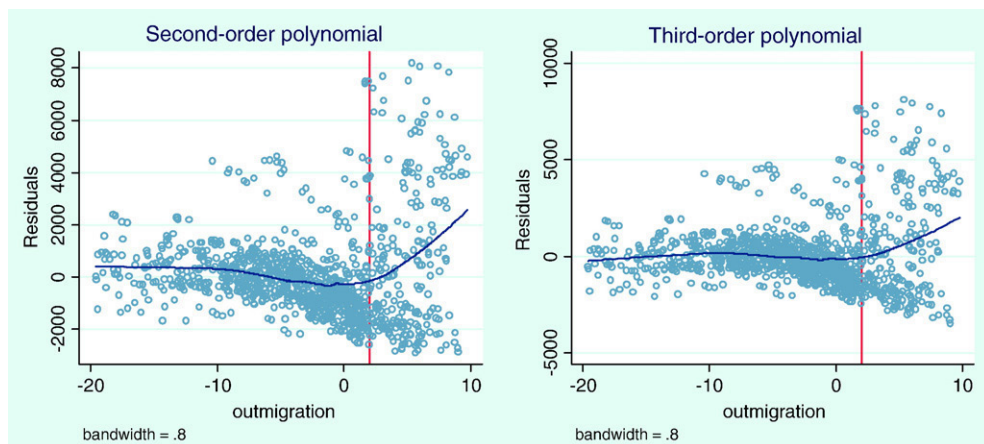


Fig. A.4. Estimated residuals from Eq. (7) against the net out-migration rate.

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