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Abstract

The effectiveness of public funds in increasing public employment has long been a question on public and labor economists' minds. In most federal countries local governments employ large fractions of the working population, meaning that a tool for stimulating local public employment can substantially affect the overall unemployment level. This paper asks whether general grants to lower-level governments have the potential of doing so. Applying the regression kink design to the Swedish grant system, we are able to estimate causal effects of intergovernmental grants on personnel in different local government sectors. Our robust conclusion is that personnel in the central administration increased substantially after a marginal increase in grants, but that such an effect was lacking both for total personnel and personnel in child care, schools, elderly care, social welfare and in technical services. We suggest several potential reasons for these results, such as heterogeneous treatment effects and bureaucratic influence in the local decision-making process.

Keywords: Fiscal federalism, intergovernmental grants, public employment, regression kink design, instrumental variables
JEL-codes: C33, H11, H70, J45

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1 Introduction

The potential for public funds to stimulate public employment in times of worldwide economic recession is now a most salient policy issue. The public sector constitutes an important source of employment in many countries; it often employs around 15–20 percent of a country's labor force, and stimulating employment in this sector may be a promising way to reduce the overall unemployment level. From a federalist point of view, intergovernmental grants are the most important financial means through which the central government can impact lower-level governments. We can expect, therefore, that many federalist countries will apply this type of tool to try to limit the negative consequences on public welfare that today's recession is likely to have.[1] One country where the central government has turned its hope to public employment is Sweden; in the fall of 2009 the Swedish government decided to give a significant amount of extra general grants to local governments. The purpose of these additional grants, they argued, is to avoid layoffs in order to guarantee a sustained welfare level and limit overall unemployment effects. But will these grants work? Are general grants effective in stimulating local public employment?[2]

In this paper, we estimate causal effects of general intergovernmental grants on municipal personnel in Sweden for the period 1996–2004, both in total and disaggregated by sector. In doing this, we add to the literature examining the effects of public funds on public employment – a literature that is surprisingly sparse, given its policy relevance.[3] For the specific role of grants in explaining local public employment, we know of only one study; Bergström et al. (2004). Using the Arellano and Bond GMM estimator, they find that intergovernmental grants have a negligible effect on total local public employment in Sweden.[4] This paper differs from theirs in three important ways. First, we have more detailed panel data on the number of employees in different local government sectors, data which enables us to study not only local public employment in total, but also disaggregated by sector. Second, we give more consideration to likely endogeneity problems and have a more convincing identification strategy. Third, we use data from a period (1996-2004) in which the local governments did not face any constraints on their self-governance. One problem with the period under study (1988-1995) in Bergström et al. (2004) was that it covered several

[1] This idea is, of course, not new. For example, in the 1970's the U.S. introduced the Public Employment Program (PEP) and the Comprehensive Employment and Training Act (CETA) with this purpose in mind.

[2] We know from previous research on Swedish data (see Dahlberg et al., 2008) that increased grants stimulate spending one-for-one, but we don't know for what the grants are used.

[3] The literature on private employment is considerably larger. However, drawing inference from this literature to the public sector is very likely misleading, since public employment differs from private employment in many important ways. For example, governments do not, in contrast to private firms, operate under the objective of profit maximization, but rather, their goal is (ideally) to maximize the welfare of their citizens.

[4] Dahlberg and Mörk (2006) study the determinants of Swedish municipal labor separately for bureaucrats, child care, elderly care and schools. Like Bergström et al. (2004) they, too, do not find that grants have any sizeable effects on the level of employment. However, because their main focus is on estimating wage elasticities in the different sectors, they simply treat grants as a control variable. It is therefore likely that their estimate of the grants effect is biased.

years (1991-1995) in which the central government effectively froze the local income tax at the 1990 level.

Estimating causal effects of grants on personnel employed by the municipality is not straightforward since it is likely that grants are endogenous. The root of the endogeneity problem is that grants are not randomly distributed to municipalities, but, rather, that municipalities receive grants motivated by some underlying need. Such needs are likely to be directly related to labor demand, implying that perceived correlations between grants and employment partly stem from the *determinants* of the grant distribution rather than the causal effect of grants in itself.[5] In this paper, in order to solve this endogeneity problem we will adopt a version of the identification strategy used by Dahlberg et al. (2008). The idea is to make use of a kinked assignment rule in the Swedish grant system whereby municipalities with a net out-migration above two percent receive grants, whereas those below two percent do not. Because any direct effect of out-migration on personnel can be assumed to be smooth, a kinked relationship between out-migration and personnel can be attributed to differences in the amount of grants received.

Our method is similar in spirit to the regression discontinuity design (RDD) (see, e.g., Angrist and Lavy, 1999; Hahn et al., 2001; Lee, 2008), and is labeled Regression Kink Design (RKD) by Nielsen et al. (2010). Card et al. (2009) derive formal identifying assumptions and resulting testable predictions for this method.[6] In this paper we adopt a fuzzy version of the RKD, where the identifying assumption of no kink in the direct effect of out-migration on the outcome (i.e., of smooth direct effects) is used as an exclusion restriction in an IV-estimation.

Earlier studies on the demand for public sector labor can, along the lines of Ehrenberg and Schwarz (1986), be divided into two strands – one that focuses on estimating wage elasticities and one that focuses on the effectiveness of public funds in increasing public employment and thereby decreasing overall unemployment. See Ehrenberg (1973) and Dahlberg and Mörk (2006) for contributions on estimating wage elasticities, Johnson and Tomola (1977) for an evaluation of early public employment programs, and the above-mentioned study by Bergström et al. (2004) for an investigation of effects of intergovernmental grants on public employment. The present paper investigates whether grants can stimulate public employment and thereby fits into the second strand.

Sweden has one of the largest public sectors in the world and local governments play a significant role in sustaining the welfare state through the delivery of child care, education

[5] See also Besley and Case (2000) for a discussion of endogenous policies in general, and Knight (2002), Gordon (2004) and Dahlberg et al. (2008) for further discussions of potential endogeneity of grants.
[6] Both Nielsen et al. (2010) and Card et al. (2010) have empirical applications on the RKD, as do Guryan (2003), and Simonsen et al. (2010).

and care for the elderly. Of the approximately 25 percent of the labor force working in the public sector, the vast majority are employed by a municipality, and wages and payroll taxes make up close to 50 percent of municipal expenditures. Thus, Sweden is well suited for studying issues concerning public employment. At the same time, since the public sector is important in many other countries as well, these issues are of general concern.

Our results show that an increase in intergovernmental grants has no effect on the total number employed by the municipality. This result is in line with the findings in Bergström et al. (2004). When looking at employment disaggregated by sector we only find a positive, statistically significant effect on administrative personnel, whereas the personnel in other sectors (child care, schools, elderly care, social welfare and technical services) are unaffected (in a statistical as well as economical sense). Furthermore, the estimated impact on administrative personnel is rather large, with a one standard deviation increase in grants causing administration to increase by 0.85 standard deviations – an economically significant effect. Focusing on wages on the other hand, we find that these are increased in most sectors, as is total spending.

The outline of the rest of the paper is as follows: Section 2 presents the strategy used to identify causal effects of intergovernmental grants on different types of local government personnel. Section 3 provides a description of the role of local governments and intergovernmental grants in Sweden, along with a description of the data. Section 4 gives the baseline results, as well as a detailed examination of the validity of the identifying assumptions. Section 5 presents further findings on effects of grants on other outcome variables, and section 6 concludes by discussing possible interpretations of the results.

2 Identification strategy

We are interested in the causal effect of intergovernmental grants on different types of municipal personnel, i.e., the relationship we want to identify is given by

(1) $$y_{i,t} = \alpha_0 + \alpha_1 g_{i,t} + \varepsilon_{i,t},$$

where $y_{i,t}$ is the number of personnel employed by municipality i in year t (total and disaggregated by sector), and $g_{i,t}$ are grants received by the municipality. A (naïve) OLS-estimate of α_1 will most likely be biased. The bias can be due to, e.g., simultaneity of grants and personnel – i.e., that there is some mechanism through which the level of public employment affects the grant distribution. As explained in the introduction, a second possible source of the bias is omission of key variables that determine both the grant distribution and local labor demand. Note, however, that even in cases where the grant formula is completely known, it may be impossible to identify the causal effect of grants simply by including all the determinants in the regression, because by doing so there is no remaining identifying variation in the grants variable of interest. Irrespectively of the source of the bias, to eliminate it, ideally we would like to conduct an experiment where municipalities are randomly given different amounts of grants and then study their behavior. Because such an experiment will most likely never be conducted (it seems quite politically infeasible), we turn to institutional details that allow us to come as close as possible to randomization of grants. Following Dahlberg et al. (2008), we will use a kinked assignment rule in the Swedish cost-equalizing grants as a source of exogenous variation. The cost-equalizing grants come with no strings attached and are intended to support municipalities that are characterized by demographic and other structural conditions associated with higher costs. We will return to the role played by these grants in section 3.

The cost-equalizing component we focus on supports jurisdictions with a diminishing population by distributing out-migration grants, $g^m_{i,t}$, to municipality i in year t according to the following kinked assignment rule:

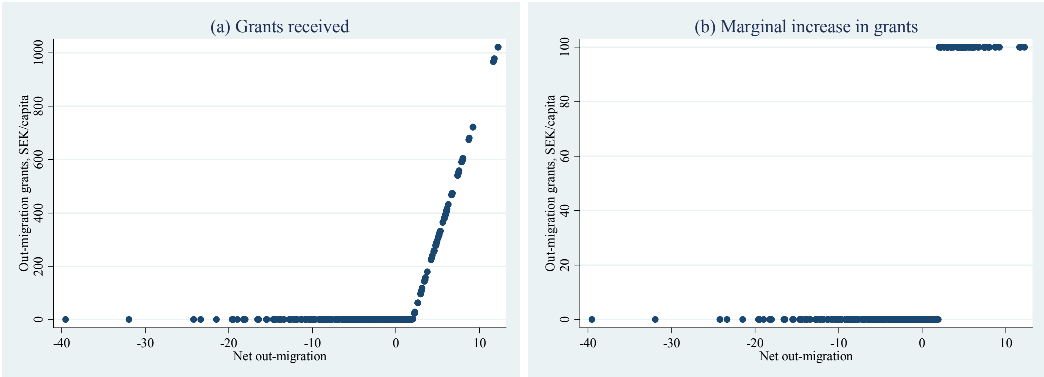
$$(2) \qquad g^m_{i,t} \begin{cases} = b(m_{i,t} - 2) & \text{if } m_{i,t} > 2 \\ = 0 & \text{if } m_{i,t} \leq 2 \end{cases}$$

where the assignment variable $m_{i,t}$ is the percentage decrease in the size of the population $n_{i,t}$ during a ten-year period with a two-year lag, i.e., $m_{i,t} = 100\left(1 - n_{i,t-2} / n_{i,t-12}\right)$. Although the assignment variable may partly reflect changes in mortality rate and birthrate, we will refer to it as (net) out-migration rate.

The assignment mechanism in (2) prescribes that municipalities with out-migration rates below two percent do not receive any out-migration grants, whereas those with out-migration rates above two percent do, implying a kink-point at two percent. For those above the kink-point, the increase in per capita grants due to a percentage point increase in out-migration is represented by the parameter b, which was constant and equal to 100 SEK during our period under study.

Figure 1 illustrates the assignment mechanism in (2) by plotting out-migration grants received by the municipalities, both the total amount and the marginal increase, against the assignment variable. As can be seen from panel (a), there is a well-defined kink-point where municipalities with net out-migration rates lower than two percent do not receive any grants, whereas municipalities with net out-migration rates above two percent do receive grants.[7] Panel (b) of the figure also shows that the marginal increase in grants for each percentage increase in out-migration is 100 SEK per capita. Thus, as these graphs clearly show, there is a non-linear relationship between grants and out-migration, and a discontinuous relationship between grant *increases* and out-migration.

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



Card et al. (2009) derive necessary and sufficient conditions for a kinked assignment rule, such as that in (2), to identify a local average treatment effect (LATE) on some outcome. For our application, it is required that, first, the derivative of the density of the net out-migration rate is smooth – i.e., that the distribution is twice continuously differentiable – at two percent and, second, that the marginal effect of out-migration on personnel is smooth.[8] The first assumption rules out extreme sorting, or precise manipulation of the assignment variable, which for our application seems innocuous. The second assumption says that, although we allow for the assignment variable to have a direct effect on the outcome, there can be no kink in this relationship. Or in other words, there can be no jump in the *marginal* effect of the assignment variable on the outcome (like the one in panel (b) of Figure 1). The implication is that there should not be any kinks in pre-determined covariates, an implication that can be used to test the assumption.

In this paper we adopt a fuzzy version of the RKD. Analogously to in the RDD, the fuzzy version of RKD is appropriate when treatment is not entirely determined by distance to the kink-point. For our case, although the assignment rule in (2) is entirely deterministic regarding treatment of out-migration grants, treatment of cost-equalizing grants is not deterministic due to kink-points in other components of the cost equalization that – by

[7] The total cost for this grant component is divided equally (per capita) between all municipalities, implying that it is neutral in terms of the federal budget.
[8] Note that these assumptions are somewhat stronger than in the regression discontinuity framework, where one only needs to assume smoothness in the level (and not in the marginal effect).

coincidence – are close to the kink-point we have in mind.[9] This setting implies that one could potentially formulate a model that would estimate a separate treatment effect for each kink. Such an approach is however not viable for two reasons: First, the structure of the other components containing kinks are typically very complicated and do therefore not allow for summarizing an RKD treatment effect in a single parameter. Second, such a model would require the inclusion of flexible functions of all the different assignment variables, and on some of these we lack data.

Our focus will instead solely be on the component for out-migration grants and the assignment rule in (2), which we will use in an IV-estimation of the effect of an increase in cost-equalizing grants, $g_{i,t}$. As in the sharp RKD, the identifying assumption (i.e., the exclusion restriction) is still that of no kink in the direct effect of out-migration on the outcome. We additionally need to assume that any direct effects of other variables subject to kinked assignment rules are also captured by the inclusion of the control function in out-migration.

The first and the second stage in the two-stage least squares (2SLS) are given by the following two equations:

(3) $$g_{i,t} = \gamma_0 + \gamma_1 \left(m_{i,t} - k\right) D + \sum_{p=1}^{\bar{p}} \phi_p \left(m_{i,t} - k\right)^p + T_t + \eta_{i,t} \text{ for } \left|m_{i,t} - k\right| \leq h$$

(4) $$y_{i,t} = \alpha_0 + \alpha_1 \hat{g}_{i,t} + \sum_{p=1}^{\bar{p}} \delta_p \left(m_{i,t} - k\right)^p + T_t + \varepsilon_{i,t} \text{ for } \left|m_{i,t} - k\right| \leq h,$$

where $\hat{g}_{i,t}$ are predicted cost-equalizing grants obtained from estimation of the first stage, $k = 2$ is the kink-point, the term interacted with $D = 1\left[m_{i,t} > k\right]$, which is an indicator for out-migration rates above the kink-point, is the excluded instrument and T_t are time fixed effects. Thus, the excluded instrument captures the kinked relationship between out-migration and personnel stemming from increased out-migration grants.[10] The direct effect of out-migration is represented by the term summing over order of polynomial *p*, with $\bar{p}$ being the highest order of polynomial included in the regression, and *h* is the bandwidth that determines which observations are included (i.e., $[k-h, k+h]$). We will present results with different combinations of $\bar{p} = \{1,2,3\}$ and $h = \{5,10,15,\infty\}$.

[9] For a description of the cost equalization during the period under study, see Svenska Kommunförbundet (2003).

[10] Note that if the assignment rule in (2) were entirely deterministic regarding treatment of cost-equalizing grants, the parameter γ_1 in equation (3) would be identical to the parameter *b* in (2), and there would be no need for a two-stage procedure.

The resulting estimate of α_1 is a weighted LATE-estimate, with weights proportional to the ex-ante probability of being close to the kink-point. Due to the non-linearity in the grant formula we are thus able to mimic an experimental setting quite closely. In terms of Figure 1, after controlling for the smooth direct effects of out-migration on personnel, municipalities on opposite sides of the kink are similar in all respects except that some are eligible for the grant and others are not. A kink in the relation between out-migration and personnel can therefore be attributed to differences in the amount of grants received.

It is worth noting that as our suggested procedure estimates a direct causal effect, omitted variable bias is no longer a concern, and for identification purposes there is no need to control for additional covariates or municipality fixed effects.[11] As seen from equations (3) and (4), we will, however, include year fixed effects to control for aggregate shocks that affect all jurisdictions in the same way (e.g., inflation).

In order for the estimated coefficient to represent a causal effect, the instrument needs to be relevant, the exclusion restriction of no kink in the direct effect needs to hold and the control function of out-migration needs to capture the direct effects of any other variables subject to kinked assignment rules. The relevance of the excluded instrument can simply be examined by looking at the *t*-statistic and its associated p-value for the estimated parameter of this term in the first-stage estimation. As noted above, an implication of smooth direct effects is that there should not be any kinks in pre-determined covariates, and consequently, this assumption can be tested by estimating the model on such covariates and see whether there is a “treatment effect”. This, as well as other tests of the different identifying assumptions, will be further discussed in section 4.4.

3 Institutional background

This section provides a description of local governments in Sweden and the role played by intergovernmental grants, along with a description of the data used in the paper.

3.1 Fiscal federalism in Sweden

Decentralized governments in Sweden are among the largest in the world, with a comprehensive range of responsibilities, including primary and secondary education, child care and care for the elderly. The production of these services is very labor-intensive and roughly 20 percent of the entire work force is employed by the municipalities, making them the largest employer in the country. As a consequence, costs for personnel constitute around

[11] We will return to the role of additional covariates when testing the validity of the identifying assumption. See section 4.4 for more details.

half of all municipal expenditures. The most important source of municipal revenue is a proportional local income tax, which makes up 60–70 percent of total revenues. The rest is made up of user fees and grants. Because equalization is a major feature of the grant system, the importance of grants as a source of revenue varies substantially across jurisdictions. The average share is just above 15 percent, but there are also three municipalities in the Stockholm region that actually were net contributors to the grant system during the entire 1996–2004 period.

A major grant reform in 1993 replaced a system of targeted central government grants to all municipal services (education, child and elderly care, social services and infrastructure) with general grants, i.e., the majority of grants were no longer earmarked. After some early changes, the new system that came into place in 1996 consisted of a per capita grant, income-equalizing grants and cost-equalizing grants, all distributed as general grants with no strings attached. The cost equalization aimed at reducing differences in effective costs due to variable structural conditions across municipalities, whereas the purpose of the income equalization was to bring per capita tax revenues close to the national average.[12] It is worth noting that the same grant system prevailed during the entire period studied here.

3.2 Data

This paper uses a panel of 279 Swedish municipalities observed over the time period 1996–2004.[13] As mentioned in section 2, the grant formula that is used for identification is an element of the cost-equalizing grants designed to compensate local governments for additional costs due to sizeable out-migration. During 1996–2004, the average out-migration grant as a fraction of total cost-equalizing grants for eligible municipalities amounted to around 18 percent, whereas the cost-equalizing grants for eligible municipalities amounted to around 20 percent of total grants. As described previously, jurisdictions with net out-migration below two percent were not eligible for out-migration grants, whereas those above two percent were. Over the study period, 112 municipalities never received any out-migration grants, 55 municipalities received grants all nine years and the remaining 112 municipalities received grants some, but not all, years. As can be seen from the map in Figure 2, receiving municipalities can be found all over the country, even though they are concentrated in the north of Sweden.

[12] Both of the equalizing grants were self-financed by equal per capita contributions from all municipalities.

[13] 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).

Figure 2 Distribution of out-migration grants over the period 1996–2004.

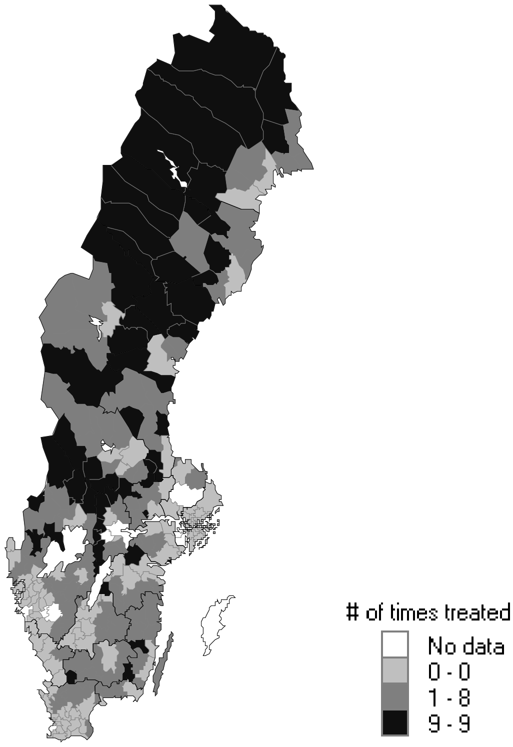


Table 1 displays summary statistics for the two grant variables (cost-equalizing grants and migration grants, both measured in SEK per capita), the dependent variables (total personnel and personnel in central administration, child care, schools, elderly care, social welfare and technical services,[14] all measured in full-time equivalents per 1,000 capita) and the assignment variable (net out-migration rate).[15] The large standard deviations relative to the means and the negative minimum values of cost-equalizing and out-migration grants reflect the fact that the grant system is self-financed. As for personnel, most people are employed in elderly care and schools – 21 and 17 full-time equivalents per 1,000 inhabitants, respectively – followed by personnel in child care and in technical services, with around 10 full-time equivalents per 1,000 inhabitants. An aggregated version of these numbers is illustrated in Figure 3, which shows the sector-wise evolution of the total number of people, in full-time equivalents, employed by the municipalities in the country. We see that employment in elderly care has increased quite substantially (due to the aging population), as has employment in schools, whereas fewer are employed in child care.[16] Employment in the remaining sectors has been fairly stable, and, all in all, the number of full-time equivalents

[14] The way in which total personnel is disaggregated into the various sectors is in accordance with the Swedish Association of Local Authorities and Regions.
[15] It is worth noting that the net out-migration rate that determines whether or not a municipality is eligible is taken from register data. Hence, there is no room for the municipality to manipulate the figures in order to become eligible for the grants.
[16] Part of the employment decrease in child care and increase in schools is explained by a transfer of pre-schools for 6-year olds from the child care sector to the school sector.

increased from around 460,000 in 1996 to 475,000 in 2004. This slight increase runs parallel with a positive privatization and outsourcing trend taking shape during the 1990s. For example, of everyone working in the child care sector, the share employed by a municipality was 94 percent in 1995 and 90 percent in 1999 (Statistics Sweden, 2001). Although the public share of employment has experienced similar decreases in other sectors as well, as of 2004 the vast majority working in areas traditionally dominated by public providers were still employed by a municipality.

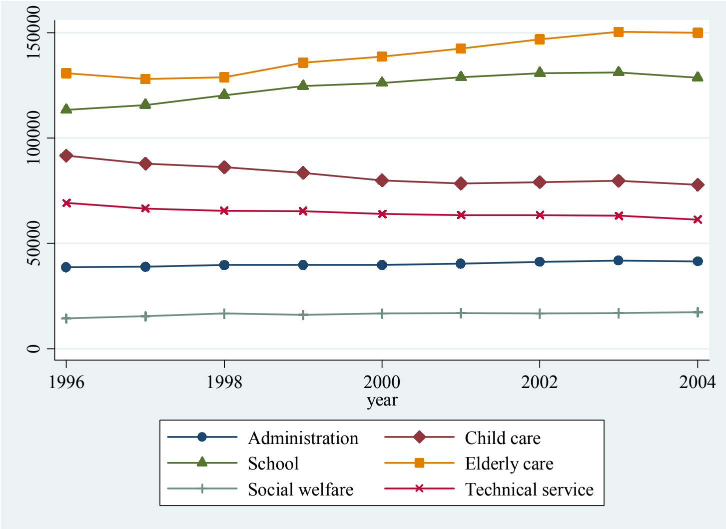
Table 1 also presents the socio-economic variables that will be used to test the identifying assumption of smooth direct effects of out-migration (see section 4.4): population size, share of population aged 0–6, share of population aged 7–15, share of population aged 80 years or older and share of foreign-born citizens. These variables show large variations across municipalities, as does the amount of migration (which of course is the underlying reason why there is a need for equalization).

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

Variable	**Mean**	**Std dev**	**Min**	**Max**
Personnel, total	65.03	9.88	30.89	101.8
Personnel, central administration	5.48	1.13	2.19	12.86
Personnel, child care	10.55	1.72	3.33	17.90
Personnel, schools	16.71	2.71	8.45	32.09
Personnel, elderly care	21.20	6.66	1.48	41.11
Personnel, social welfare	1.78	0.706	0.084	6.82
Personnel, technical	9.31	2.42	1.63	17.79
Cost-equalizing grants	532.1	2,452	-3,471	13,196
Migration grants	111.7	286.8	-125.5	1,384
Net out-migration	-0.789	7.92	-42.95	16.64
Population	27,229	48,884	2,575	761,721
Share of population 0–6	7.92	1.31	4.71	12.80
Share of population 7–15	12.23	1.19	6.78	16.43
Share of population 80+	5.41	1.38	1.25	9.14
Share foreign born	4.01	2.73	0.56	29.06
# of observations	2,511			

Notes: Grants are defined in SEK per capita, personnel are defined as number of full-time equivalents per 1,000 inhabitants, and net out-migration and population shares are given in percent. Data is obtained from Statistics Sweden and from The Swedish Association of Local Authorities and Regions.

Figure 3 Number of full-time equivalents in different municipal sectors, 1996-2004.



4 Effects of grants on local government personnel

In this section we present two-stage least squares (2SLS) estimates of the model given in equations (3) and (4) to examine what effects increased grants have on different types of personnel. In order to test whether the instrument is relevant (i.e., whether the kinked rule assigning out-migration grants explains variation in cost-equalizing grants), we present the results from the first stage. Thereafter, we turn to the estimates of the causal effects from the second stage. We will also conduct a thorough analysis of the identifying assumptions. But before turning to the econometrics we begin with a graphical analysis.

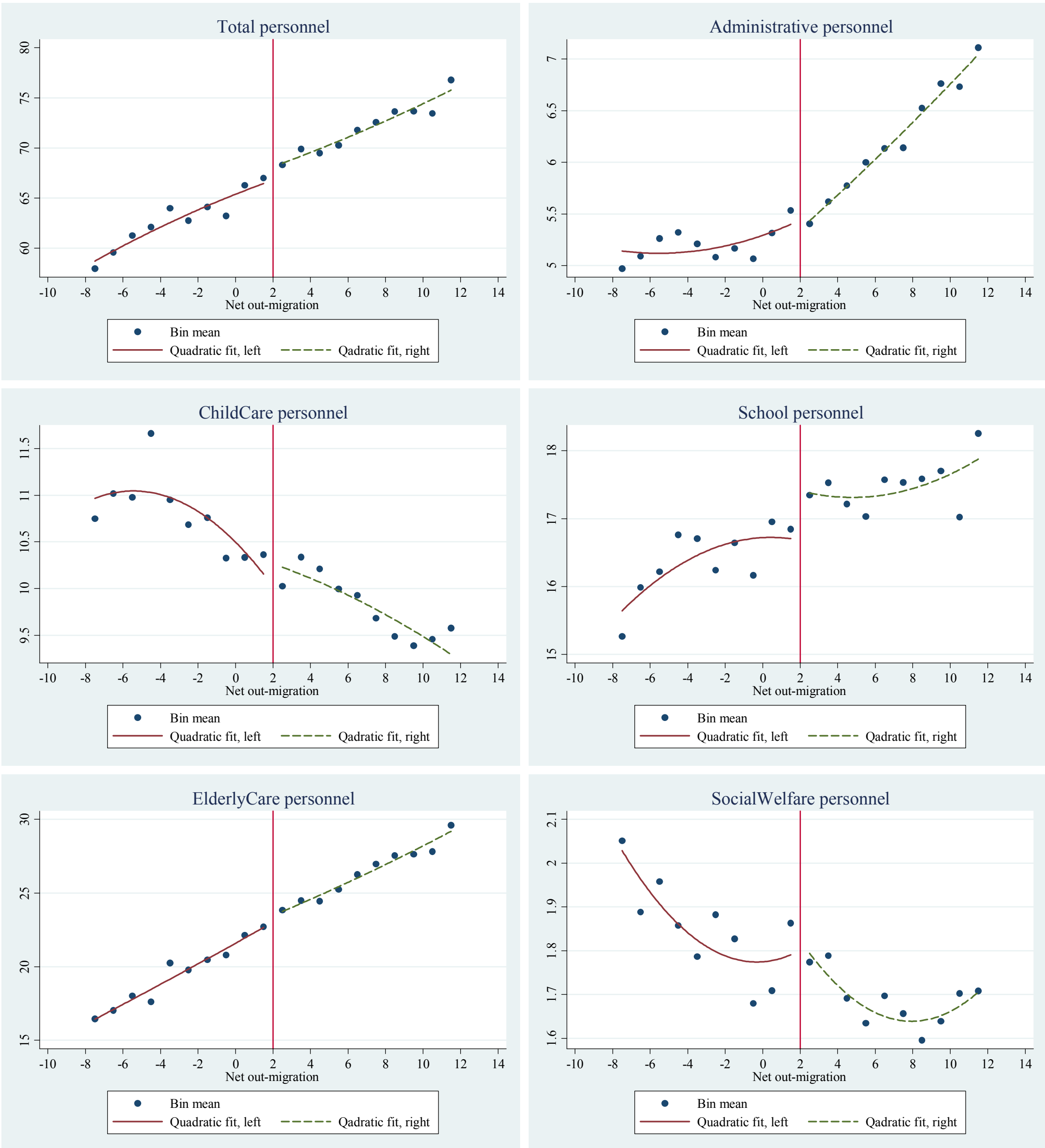
4.1 Graphical analysis

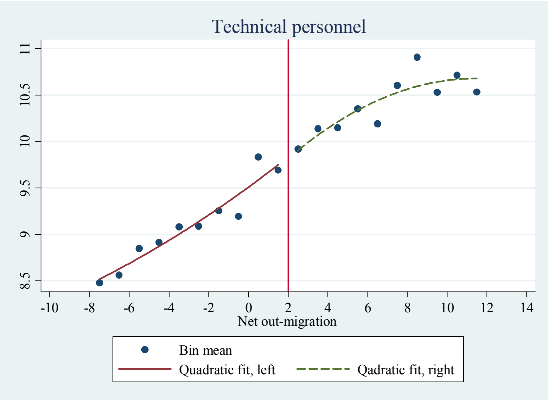
As in the RDD, visual inspections are important parts of RKD applications because of the nature of the estimator – if there is an effect, we expect to see a kink in the outcome corresponding to the kink in the assignment rule. For an as fair picture of this relation as possible, it is customary to plot means of the outcome within a specified bin width of the assignment variable, along with fitted polynomials on each side of the kink-point. Focusing on out-migration rates +/-10 from the kink-point, Figure 4 does this for total personnel and for personnel within each of the different categories, where we have chosen a quadratic fit and a bin size of one percentage point.[17] For administrative personnel there is a distinct kink around

[17] Lee and Lemiuex (forthcoming) suggest two formal tests for choosing bin size in graphical regression discontinuity analyses, and with a slight modification these tests can also be applied to the RKD. The first test is an F-test of a model with C separate slope coefficients (intercepts for RDD) against a model with 2C separate slope coefficients. If the test is not rejected, C bins are enough. The second test is an F-test of a model with C separate slope coefficients (intercepts for RDD) against a model with C separate slopes *and C separate coefficients on quadratic terms* (linear for RDD). Again, if the test is not rejected, C bins are enough. Performing these tests for all of our outcome variables, 20 bins (implying a bin size of one) are never rejected at the 10 percent level.

two percent out-migration, suggesting a positive treatment effect on this personnel category. However, neither for total personnel nor for any of the other personnel categories are any such kinks present (although out-migration appears to have a quite strong direct effect, albeit with a large variance for school and social welfare personnel). The econometric analysis to follow will show how these graphical results correspond to statistical estimates.

Figure 4 Net out-migration against municipal personnel





4.2 First-stage estimates

As the graphical analysis in the previous section suggest, it is important to allow the assignment variable to have a direct effect on personnel. We do this by including a flexible function of this variable in the regression. One might worry that when doing this, there will be no explanatory power left in the instrument, which would thereby fail to be relevant. Whether or not this is the case will be clear from the first-stage estimates − more precisely, from the p-values of the estimate of the *incremental* effect of out-migration on total cost-equalizing grant, $g_{i,t}$, at the kink-point. This estimate corresponds to γ_1 in the first-stage regression given in equation (3). If γ_1 is statistically significant, we know that after controlling for the direct effects of the smooth function of net out-migration, the out-migration grant still has an impact on total cost-equalizing grant, implying that our instrument is relevant.

Because we do not know the form of the direct effects of the assignment variable, we wish to be as flexible as possible. Therefore, we use up to a 3rd order polynomial in net out-migration. Table 2 shows the first-stage estimates and associated standard errors of γ_1 in equation (3). The standard errors are robust to arbitrary residual heteroskedasticity and serial correlation within municipality.[18] Each row corresponds to an estimation of equation (3) with $\overline{p} = \{1,2,3\}$, while each column corresponds to an estimation where either all observations are included, or where the estimation sample is restricted to observations +/- 15, 10 and 5 percentage points from the kink-point. Note that with the two narrowest bandwidths we only estimate with $\overline{p} = 1$.

[18] Rigidities in hiring and firing suggest that there is a time-lag in employment, which is also shown to be the case in Bergström et al. (2004). This time-lag emphasizes the importance of adjusting the standard errors accordingly – something we do by using the “cluster” command in STATA.

Table 2 First-stage estimates (c.f. eq. (3); only estimates for γ_1 reported).

	Full sample	*h=15*	*h=10*	*h=5*
Cost-equalizing grants				
$\overline{p}=1$	417.351***	363.635***	398.802***	197.997**
	(68.420)	(73.943)	(74.439)	(96.125)
$\overline{p}=2$	317.629***	311.837***		
	(76.091)	(105.477)		
$\overline{p}=3$	334.972***	407.556***		
	(103.580)	(135.075)		
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and order of polynomials, $\overline{p}$, the table reports estimates of γ_1 in the first-stage equation (3) on cost-equalizing grants. Standard errors clustered on municipality are in parentheses, and *** and ** denote significance at the 1 and 5 percent level, respectively.

From the table it is clear that all estimates are highly statistically significant, irrespectively of order of polynomial and bandwidth. The magnitude of the estimates is around 300, although that differs somewhat across the different specifications. Note that owing to other kinks in the cost equalization close to the kink in the out-migration component, the parameter is estimated to differ from 100, which would be the case if treatment of cost-equalizing grants were a fully deterministic function of the assignment rule in (2). As explained above, this is the rationale for adopting a fuzzy version of the RKD, and poses no identification problem as long as any direct effects of assignment variables subject to other kinks are captured by the included control function. The validity of this claim will be investigated in section 4.4, but for now we conclude that the instrument works well in the sense that it is relevant, and turn to the two-stage least square estimates of the causal effect of grants on municipal personnel.

4.3 Two-stage least squares estimates

Table 3 presents the 2SLS estimates of α_1 from equation (4) for total personnel, as well as for personnel disaggregated by the six sectors (administration, child care, schools, elderly care, social welfare and technical services). Again, for each outcome the three rows correspond to estimation with $\overline{p}=\{1,2,3\}$, while each column corresponds to estimation with different bandwidths. For bandwidths where we vary the order of polynomial, the order preferred according to the Akaike information criterion (AIC) is in bold. All results are evaluated at personnel per 1,000 capita for the dependent variable and 100 SEK per capita for the grants variable (i.e., the increase in grants associated with a one percentage point increase in net out-migration from two percent).

To start with the results for total personnel, it is clear from the first three rows of Table 3 that there is, in fact, no overall effect of grants.[19] Not only are the estimated coefficients statistically insignificant, but in some specifications the sign is even negative. In fact, the

[19] This result is well in line with the results in Bergström et al. (2004).

disaggregated effects on the various sectors in subsequent rows show that insignificant, negative estimates seem to be a rather consistent pattern. The only positive, statistically significant effect is that on administrative personnel, for which the estimates are around 0.03-0.04. This point estimate is fairly robust to different bandwidths and order of polynomials, although for the smallest bandwidth the standard error increases to the extent that the estimate is no longer statistically significant. For the other personnel categories on which we find no statistically significant effects, the estimates are somewhat more sensitive to the different specifications, although much less so if one only focuses on the specifications preferred according to the AIC.

Concerning the size of the effect on the administration, we consider it to be of economic significance as well – a 100 SEK increase in per capita grants leads to an increase of 0.03-0.04 full-time equivalents per 1,000 capita, which is around 0.5 percent of the mean for this personnel category. Given that the mean of the absolute value of the cost-equalizing grant is around 1,500, implying that 100 SEK constitute a 6-7 percent increase, this is a substantial effect.[20]

4.4 Inspection of the identifying assumptions

Our baseline result is thus that grant increases cause the administrative personnel to increase substantially, but that such an effect is lacking both for total personnel and personnel in child care, schools, elderly care, social welfare and technical services. The validity of these results rests on the identifying assumptions, and in this section we test each of them starting with the assumption of smooth density of out-migration. We then investigate the assumption that the control function captures any direct effects of other variables subject to kinked assignment rules, followed by a test of the assumption of smooth direct effects of out-migration by studying pre-determined covariates. The section ends by utilizing an additional non-linear grant component, which yields two instruments and thus a more efficient estimator (and the possibility to test the model specification by means of the Hansen *J* statistic).

[20] Because of the equalizing feature of the grants, more than half of the observations on cost-equalizing grants are negative. Such negative values imply that evaluating the size of a grant increase in the context of the mean value from Table 1 would be misleading.

Table 3 Effects of grants on municipal personnel (2SLS estimates).

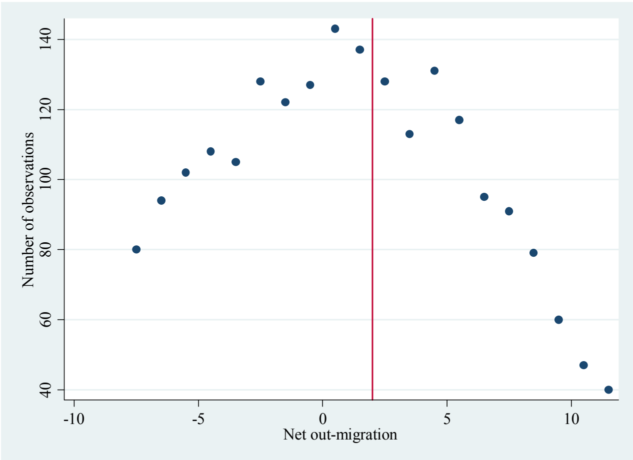
	Full sample	*h=15*	*h=10*	*h=5*
Total personnel				
$\overline{p}=1$	**0.052**	-0.059	-0.015	-0.218
	(0.048)	(0.070)	(0.057)	(0.212)
$\overline{p}=2$	-0.164	0.001		
	(0.107)	(0.134)		
$\overline{p}=3$	-0.108	**0.039**		
	(0.138)	**(0.107)**		
Administrative personnel				
$\overline{p}=1$	0.030***	**0.030*** **	0.038***	0.036
	(0.006)	**(0.008)**	(0.007)	(0.022)
$\overline{p}=2$	**0.029*** **	0.050***		
	(0.009)	(0.018)		
$\overline{p}=3$	0.041***	0.045***		
	(0.014)	(0.016)		
Child care personnel				
$\overline{p}=1$	0.004	**-0.004**	-0.001	0.013
	(0.007)	**(0.009)**	(0.011)	(0.036)
$\overline{p}=2$	**-0.007**	0.034		
	(0.013)	(0.036)		
$\overline{p}=3$	-0.006	0.018		
	(0.025)	(0.023)		
School personnel				
$\overline{p}=1$	**-0.013**	**-0.033**	-0.031	-0.096
	(0.014)	**(0.021)**	(0.020)	(0.081)
$\overline{p}=2$	-0.040	-0.054		
	(0.029)	(0.048)		
$\overline{p}=3$	-0.060	-0.034		
	(0.045)	(0.036)		
Elderly care personnel				
$\overline{p}=1$	**0.043**	**-0.026**	-0.022	-0.119
	(0.029)	**(0.041)**	(0.036)	(0.129)
$\overline{p}=2$	-0.105	-0.063		
	(0.064)	(0.091)		
$\overline{p}=3$	-0.110	-0.032		
	(0.089)	(0.077)		
Social welfare personnel				
$\overline{p}=1$	**-0.007** **	-0.001	0.000	-0.007
	(0.003)	(0.005)	(0.005)	(0.016)
$\overline{p}=2$	0.005	0.004		
	(0.007)	(0.013)		
$\overline{p}=3$	0.007	**-0.003**		
	(0.011)	**(0.010)**		
Technical personnel				
$\overline{p}=1$	-0.003	-0.025	0.000	-0.049
	(0.016)	(0.022)	(0.017)	(0.055)
$\overline{p}=2$	-0.046	**0.029**		
	(0.031)	**(0.039)**		
$\overline{p}=3$	**0.019**	0.044		
	(0.036)	(0.030)		
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and order of polynomials, $\overline{p}$, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables total personnel as well as personnel disaggregated by the different sectors. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively. The preferred polynomial according to the AIC is in bold.

4.4.1 Smooth density of the assignment variable

As shown by Card et al. (2009), one of the identifying assumptions in the RKD is that the derivative of the density of the assignment variable is smooth around the kink-point. This implies that extreme sorting needs to be ruled out, something which is best done graphically as in Figure 5. This figure plots the number of observations within each 1-percentage point bin of net-outmigration in the range (-8, 12). Although the density of observations does not evolve smoothly within the entire range, it is comforting to see that nothing dramatically happens around the kink-point at two percent (marked by the vertical line).[21] Given the type of assignment variable in our application this comes perhaps has no surprise: it seems quite difficult for municipalities to perfectly manipulate their rate of out-migration during a ten-year period. Also, Figure 5 shows that there are a large number of municipalities around the kink-point, indicating that the weighted LATE-estimate that we estimate in fact applies to quite many municipalities.

Figure 5 Density of net out-migration.



4.4.2 Omitted kinked assignment variables

As explained in section 2, due to kinked relations in other components of the cost equalization, treatment of cost-equalizing grants is not an entirely deterministic function of the kinked rule assigning out-migration grants. This is the reason why we adopt the fuzzy version of the RKD. As a consequence, we need the additional assumption that any direct effects of other variables subject to kinked assignment rules are captured by the control function in net out-migration. This section provides evidence that this assumption is likely to hold.

[21] The econometric complement to the graphical validation of this identifying assumption is to run the regression in equation (3), with the number of observations within each bin of a specified size as the dependent variable, and test whether γ_1 differs from zero. Doing this we do not reject the null for any $\overline{p} \geq 2$.

Our approach to investigating the validity of this assumption is to control for those additional components that appear particularly problematic, and see both how the first and the second stage results are affected. After a detailed study of all the 15 remaining components in the cost equalization (not counting the out-migration component), we concluded that the components for child care, social welfare and settlement structure are those that – for what appears to be a coincidence – contain non-linearities close to the kink-point at two percent out-migration.[22] If our assumption that the effect of the variables underlying these components are well captured by the control function in out-migration holds, then controlling for grants received from these components should not affect the second stage-estimates much, although it may – and in fact should – affect the first-stage results.

Looking first at Table 4, which presents the estimate of γ_1 from estimating equation (3) while controlling for grants received from the child care, social welfare and settlement structure components, we see results in line with our predictions. Compared to the baseline first-stage estimates in Table 2, these are much closer to 100.[23] The corresponding second-stage estimates are found in Table 5. To economize space we only present the estimate with the order of polynomial preferred by the AIC.[24] Interestingly given the change in first-stage estimates from the previous table, these results are quite similar to those from the baseline specification in Table 3 – the point estimates for administration, in particular, are extremely robust, and those for total personnel and for the remaining personnel categories are, overall, again close to zero. An exception to this stability is however displayed by school personnel, for which the negative effect increases quite substantially and turns statistically significant. While this seems odd and indeed quite worrisome, later results will show that there is a potential explanation for this finding. Thus, the combination of such a stark reduction in the first-stage estimate and the fact that the vast majority of the second-stage estimates remain mainly unchanged is evidence that the assumption concerning direct effects of other assignment variables holds.

[22] In a fairly complicated manner, these components assign grants to municipalities based on municipal characteristics such as share of children, labor market situation, share of foreign citizens, number of single mothers, population density and remotely located population.

[23] Recall that γ_1 would be 100 if the kinked assignment rule in (2) fully captured the variation in cost-equalizing grants.

[24] All remaining tables presenting second-stage estimates will, to save space, from now on contain only the estimate preferred by the AIC (among those using the full sample or h=15). Among the 14 such regressions in Table 5, $\overline{p} = 1$ is preferred in nine, $\overline{p} = 2$ in two and $\overline{p} = 3$ in three. Full sets of results are available upon request.

Table 4 First-stage estimates when controlling for other types of grants.

	Full sample	*h=15*	*h=10*	*h=5*
Cost-equalizing grants				
$\overline{p} = 1$	218.760***	192.132***	214.564***	151.835***
	(33.387)	(35.780)	(38.958)	(54.852)
$\overline{p} = 2$	164.993***	157.257**		
	(38.895)	(66.908)		
$\overline{p} = 3$	130.823**	177.552**		
	(64.185)	(82.381)		
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and order of polynomials, $\overline{p}$, the table reports estimates of γ_1 in the first-stage equation (3) on cost-equalizing grants when controlling for grants received from the following parts of the cost equalization: child care, individual and family care and settlement structure. Standard errors clustered on municipality are in parentheses, and *** and ** denote significance at the 1 and 5 percent level, respectively.

Table 5 Effects of grants on municipal personnel when controlling for other types of grants (2SLS estimates).

	Full sample	*h=15*	*h=10*	*h=5*
Total personnel	-0.021	0.002	-0.135	-0.327
	(0.083)	(0.221)	(0.093)	(0.238)
Administrative personnel	0.030*	0.033**	0.043***	0.038
	(0.017)	(0.014)	(0.013)	(0.028)
Child care personnel	-0.002	-0.007	-0.032*	-0.063*
	(0.013)	(0.043)	(0.016)	(0.038)
School personnel	-0.069***	-0.109***	-0.117***	-0.177*
	(0.026)	(0.037)	(0.037)	(0.102)
Elderly care personnel	0.050	0.009	-0.014	-0.047
	(0.043)	(0.131)	(0.052)	(0.121)
Social welfare personnel	-0.006	-0.004	-0.006	-0.017
	(0.006)	(0.008)	(0.009)	(0.021)
Technical personnel	-0.030	0.077	-0.008	-0.064
	(0.028)	(0.078)	(0.029)	(0.065)
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and order of polynomials, $\overline{p}$, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables total personnel as well as personnel disaggregated by the different sectors, when controlling for grants received from the following parts of the cost equalization: child care, social welfare and settlement structure. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively. The preferred polynomial according to the AIC is in bold.

4.4.3 Pre-determined covariates

Aside from the assumption of smooth derivatives of the density in the assignment variable that we tested in section 4.4.1, the second main identifying assumption derived by Card et al. (2009) is that the marginal effect of the assignment variable on the outcome is smooth. In terms of our application, we thus require that there is no kink in the marginal effect of out-migration on personnel. The implication of this assumption is that there should not be any kink in pre-determined covariates: if the direct effects of out-migration on personnel are smooth, the effect on pre-determined covariates should also be smooth. This means that a straightforward test of this assumption is to run the model on such covariates and test whether there is a "treatment effect".

A prerequisite for this test to work is that the chosen variables are pre-determined – i.e., they can not be the result of the treatment. In a local government setting this may be easier said than done, since, to some extent, most things are interdependent both across time and space. Motivated by such concerns, our choice of covariates used to test the identifying assumption are the following: total population, share of population aged 0–6, share of population aged 7–15, share of population aged 80+ and share of foreign-born citizens. With these variables as outcomes, we thus re-estimate the model in (3) and (4) with different combinations of $\overline{p}$ and h, and the resulting α_1 estimates are provided in Table 6.[25]

A few more estimates are statistically significant than what we would require to be fully convinced, at least for population shares aged 7-15. This is, on the other hand, not hard evidence *against* smooth marginal effects of out-migration. And as will be clear from next section, the statistical significance of the share of school-aged children may explain the change in the estimated effect on school personnel as found in Table 5. Also, since none of the other pre-determined covariates show any consistent signs of kinks, overall these results are not enough to shed any serious doubts on the validity of the identifying assumption, and therefore not on our baseline estimates.[26]

[25] Again, Table 6 only shows the estimate from the regression with the order of polynomial preferred by the AIC. $\overline{p} = 1$ is preferred in seven of the regressions, and $\overline{p} = 3$ in three (of the ten using the full sample or h=15).

[26] An alternative way of testing whether the effect on pre-determined covariates is smooth around the kink-point is to rerun the original model on personnel while controlling for them, and see if the baseline estimates change. When doing this we found that the original results are insensitive to the inclusion of the pre-determined covariates in Table 6, strengthening the validity of the identifying assumption of smooth marginal effects of out-migration. Results are available upon request.

Table 6 Effects of grants on pre-determined covariates (2SLS estimates). 100 SEK

	Full sample	*h=15*	*h=10*	*h=5*
Total population	-147.313 (175.719)	475.852 (419.652)	663.835** (294.788)	839.953 (707.707)
Share of population 0-6	-0.002 (0.003)	-0.004 (0.004)	-0.006 (0.004)	0.007 (0.014)
Share of population 7-15	-0.011** (0.004)	-0.020** (0.009)	-0.022*** (0.008)	-0.034 (0.033)
Share of population 80+	-0.002 (0.007)	-0.003 (0.015)	-0.007 (0.008)	-0.017 (0.023)
Share of foreign born	0.009 (0.013)	0.039 (0.037)	0.045** (0.021)	-0.026 (0.051)
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and for the order of polynomial, $\overline{p}$, preferred by the AIC, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables total population and population shares aged 0-6, aged 7-15, aged 80+ and foreign born. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively. The preferred polynomial according to the AIC is in bold.

4.4.4 Using an additional policy rule as instrument

In the year 2000, a second distinct, non-linear component was added to the out-migration component of the cost equalization, whereby municipalities were also compensated for having to adjust to fewer school-aged children. This compensation was conditioned on the net out-migration rate during the past three years (with a two-year lag), which had to be larger than two percent. Municipality *i* thus received *additional* out-migration grants in year *t*, $g_{i,t}^{m,s}$ according to the following kinked assignment rule:

$$(7) \qquad g_{i,t}^{m,s} \begin{cases} = b^s s_{i,t} \text{ if } s_{i,t} > 0 \text{ and } \tilde{m}_{i,t} > 2 \\ = 0 \text{ otherwise} \end{cases}$$

where the additional assignment variable $s_{i,t}$ is the percentage decrease in the number of school-aged children $n_{i,t}^s$ during a three-year period with a two-year lag, i.e., $s_{i,t} = 100\left(1 - n_{i,t-2}^s / n_{i,t-5}^s\right)$, and the conditioning variable $\tilde{m}_{i,t}$ is the out-migration rate during the same period, i.e. $\tilde{m}_{i,t} = 100\left(1 - n_{i,t-2} / n_{i,t-5}\right)$. The parameter b^s represents the size of the per capita grant increase due to a percentage point increase in $s_{i,t}$ (given $\tilde{m}_{i,t} > 2$), which was

100 SEK during 2000–01 and 55 SEK during 2002–04.[27] That is, for the shorter time period 2000–04, we have two non-linear grant rules;[28] the rule in equation (2) and the rule in equation (7).

Assuming that the direct effect on personnel of diminishing numbers of school-aged children is smooth, we thus have a second exclusion restriction for this latter period. As a third approach to investigate the exogeneity of the instrument, we make use of both the kinked assignment rules and re-estimate our model with two instruments − a more efficient estimator, but for a shorter time period.[29] Thereafter, we can also conduct the Hansen *J* test of instrument validity/correct model specification.[30]

For the period 2000–04, the first stage is now given by

$$(8)\qquad \begin{aligned} g_{i,t} &= \gamma_0 + \gamma_1 \left(m_{i,t} - k\right) D + \gamma_2 \left(s_{i,t} - k^s\right) D^s + \sum\nolimits_{p=1}^{\bar{p}} \left(\phi_p \left(m_{i,t} - k\right)^p + \theta_p \left(s_{i,t} - k^s\right)^p\right) + \\ & T_t + \eta_{i,t} \text{ for } \left|m_{i,t} - k\right| \le h \text{ and } \left|s_{i,t} - k^s\right| \le h, \end{aligned}$$

where $k^s = 0$ is the second kink-point and $D^s = 1\left[s_{i,t} > k^s\right] \times 1\left[\tilde{m}_{i,t} > 2\right]$ is an indicator for decreases in the number of school-aged children (above the kink),[31] given a three-year out-migration rate above two percent.

The second-stage counterpart to equation (8) is then given by

$$(9)\qquad \begin{aligned} y_{i,t} &= \alpha_0 + \alpha_1 \hat{g}_{i,t} + \sum\nolimits_{p=1}^{\bar{p}} \left(\delta_p \left(m_{i,t} - k\right)^p + \lambda_p \left(s_{i,t} - k^s\right)^p\right) + \\ & T_t + \varepsilon_{i,t} \text{ for } \left|m_{i,t} - k\right| \le h \text{ and } \left|s_{i,t} - k^s\right| \le h. \end{aligned}$$

[27] During 2000–01, this compensation was based on changes in the age group 7–15, and in 2002–04 on changes in the age group 7–18. The mean of the school grants variable is 9.7 SEK per capita, and its standard deviation is 60.3 (the minimum value is -10.2 and the maximum value is 752.1). The mean of the net change in the number of school-aged children is 2.65 percent, and the standard deviation is 4.08 (the minimum value is -10.25 and the maximum value is 23.59).

[28] As previous sections have made clear, the cost equalization in fact contains several non-linear rules. But importantly, for the shorter time period 2000-04 we have two kinked grant rules that are not too complicated for being summarized in a single RKD treatment parameter.

[29] As noted in the introduction, the identification strategy in this paper is a version of the one in Dahlberg et al. (2008). Besides the more thorough specification checks performed throughout this paper, a difference is that they did not separate the two non-linearities into two instruments and were thus not able to test any overidentifying restrictions.

[30] It is worth noting that the Hansen *J* statistic really tests whether several distinct instruments result in the same estimate. To rely on the Hansen *J* as a test for instrument exogeneity, one therefore has to assume i) that one of the instruments is exogenous and ii) homogenous treatment effects. Hence, to fully rely on the Hansen test when evaluating instrument-validity is not recommended.

[31] The indicator D^s is adjusted according to the change in the parameter b^s from 100 to 55 in year 2002 so as to give a proper interpretation of the γ_2 parameter.

As previously, we will estimate equations (8) and (9) with different combinations of $\overline{p} = \{1,2,3\}$ and $h = \{5,10,15,\infty\}$. Note, though, that we choose the same bandwidth and maximum polynomial for the two respective assignment variables.

Before turning to the results, we want to know whether the same municipalities that already received out-migration grants also received this extra grant. Looking at this in our data, we find that in the year 2000, i.e., the first year that this additional grant existed, there were 167 municipalities that did not receive any of these two grants, 83 municipalities that received the out-migration grant but not the school grant, 6 municipalities that received the school grant but not the out-migration grant, and 23 municipalities that received both these grants. There is hence some overlapping, but far from all municipalities that were granted according to the rule in equation (2) were also granted according to the rule in equation (7).

Table 7 gives the first-stage estimates. It is clear from the table that both instruments are relevant – for $\overline{p} \leq 2$, both γ_1 and γ_2 are significantly different from zero at the five percent level. For $\overline{p} = 3$, however, only the original kink-point is significant and the F-statistic for the joint test of the excluded instruments is consequently weaker. Keeping this in mind when turning to the second-stage estimates in Table 8,[32] we start by noting that according to the p-value of the Hansen *J*-statistic, we do not reject the null hypothesis of valid instruments (a correct model specification) for the vast majority of specifications. Hence, this third test also indicates that our approach is valid.

An interesting and comforting finding is that for this shorter time period and with two non-linear grant rules as excluded instruments, we get results very similar to those in the original specification (both with respect to the size of the estimates and significance levels). More importantly, the odd significantly negative effect on school personnel as found in Table 5 is now completely absent. Because of the obvious close relation between school personnel and the assignment variable in the additional kinked rule (changes in the number of school-aged children), this absence is particularly comforting. These findings thus strengthen our conclusion that marginal increases in grants increase the number of administrative personnel, but not the number of child care, school, elderly care, social welfare, or technical personnel.

[32] Among the 14 regressions in this table using the full sample or h=15, $\overline{p}$ = 1 is preferred in nine, $\overline{p}$ = 2 in four and $\overline{p}$ = 3 in one.

Table 7 First-stage estimates with two kinks.

	Full sample	*h=15*	*h=10*	*h=5*
Cost-equalizing grants				
$\overline{p}=1$, kink at m=2	341.594***	292.294***	359.149***	283.199***
	(65.858)	(75.147)	(83.004)	(99.413)
$\overline{p}=1$, kink at s=0	739.898***	736.192***	656.108**	326.711
	(263.299)	(266.384)	(265.166)	(305.103)
$\overline{p}=1$, F-statistic	[23.78]***	[15.47]***	[17.25]***	[5.71]***
$\overline{p}=2$, kink at m=2	271.335***	353.436**		
	(76.435)	(144.943)		
$\overline{p}=2$, kink at s=0	650.749**	540.995**		
	(270.208)	(268.220)		
$\overline{p}=2$, F-statistic	[11.25]***	[6.28]***		
$\overline{p}=3$, kink at m=2	375.441**	417.398**		
	(147.510)	(163.522)		
$\overline{p}=3$, kink at s=0	375.133	257.075		
	(263.536)	(256.279)		
$\overline{p}=3$, F-statistic	[5.86]***	[4.57]***		
# of observations	*1,395*	*1,319*	*1,100*	*456*

Notes: For different bandwidths, *h*, and order of polynomials, $\overline{p}$, the table reports estimates of γ_1 and γ_2 in the first-stage equation (8) on cost-equalizing grants. Standard errors clustered on municipality are in parentheses, F-statistics of the excluded instruments are in brackets, and *** and ** denote significance at the 1 and 5 percent level, respectively.

Table 8 Effects of grants with two kinks (2SLS estimates).

	Full sample	*h=15*	*h=10*	*h=5*
Total personnel	0.039	0.068	0.009	0.002
	(0.048) [0.487]	(0.100) [0.638]	(0.055) [0.200]	(0.141) [0.804]
Administrative personnel	0.028***	0.031***	0.036***	0.023
	(0.005) [0.343]	(0.006) [0.485]	(0.007) [0.882]	(0.019) [0.437]
Child care personnel	-0.017	-0.006	0.000	0.014
	(0.012) [0.244]	(0.008) [0.079]	(0.010) [0.301]	(0.033) [0.810]
School personnel	-0.009	-0.023	-0.020	-0.046
	(0.014) [0.148]	(0.019) [0.038]	(0.019) [0.100]	(0.056) [0.563]
Elderly care personnel	0.030	0.011	-0.014	0.013
	(0.027) [0.470]	(0.060) [0.656]	(0.034) [0.488]	(0.076) [0.693]
Social welfare personnel	-0.006*	0.001	0.004	0.003
	(0.003) [0.037]	(0.004) [0.194]	(0.006) [0.283]	(0.013) [0.028]
Technical personnel	0.019	0.024	0.003	-0.004
	(0.033) [0.630]	(0.030) [0.459]	(0.016) [0.777]	(0.046) [0.554]
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and for the order of polynomial, $\overline{p}$, preferred by the AIC, the table reports estimates of α_1 in the second-stage equation (9), with dependent variables total personnel as well as personnel disaggregated by the different sectors. Standard errors clustered on municipality are in parentheses, p-values of the Hansen *J* statistic are in bracket, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively. The preferred polynomial according to the AIC is in bold.

To sum up the results from the fairly thorough check of instrument validity provided in this section, we are quite confident that our identifying assumptions hold and, hence, in the baseline results presented in section 4.3.

5 Further analysis

Thus, our robust conclusion is that administrative personnel are increased after a marginal increase in grants, but that such an effect is lacking both for total personnel and personnel in any of the other five sectors. How should we understand this result? Does it mean that all money from an increase in grants goes to the administrative sector? And if so, what type of administrative personnel do municipalities employ?

In this section we will first examine whether municipalities employ administrative assistants or higher officials. Second, we will investigate possible other uses of money from the central government. There are at least two other possible ways of spending that will not show up in the estimates above: i) increase wages for those already employed, and ii) use the money for non-personnel related expenditures. In order to better understand how to interpret the asymmetric results on the administration versus other personnel categories, this section we will therefore investigate the effects of grants on wages for locally employed and total expenditures of different categories.[33]

5.1 Do municipalities employ administrative assistants or higher officials?

The pool of bureaucratic personnel in the central administration, as we have defined it so far, is fairly broad – it includes everyone from frontline employees performing basic administrative assistant services to high officials and heads of local public authorities. Using access to detailed register data on all individuals employed by the municipalities, we are able to refine our analysis by studying different types of bureaucrats separately, which may help us understand the mechanism behind our results.

Once again equation (4) is estimated with different bandwidths, with dependent variables now being administrative assistant personnel and high administrative officials, respectively. The resulting α_1 estimates are shown in Table 9 (again, only for the order of polynomial preferred by the AIC).[34] These results clearly show that increases in employment take place among the administrative assistants, for which the estimated effect is around 0.015, meaning that a per

[33] To provide a sense of the wage level in the different sectors and of the relative importance of the different expenditure categories, descriptive data is found in the Appendix.

[34] Of the four regressions in Table 9 using the full sample or h=15, $\overline{p} = 1$ is preferred in three, and $\overline{p} = 3$ in one.

capita grant increase of 100 SEK leads to an employment increase of around 0.015 full-time equivalents per 1,000 capita. This increase corresponds to an addition of 1.5 percent to this personnel category. The effect on high administrative officials, however, appears to be very similar to the other sectors analyzed above, i.e., not significantly different from zero.

Table 9 Effects of grants on different types of bureaucrats (2SLS estimates).

	Full sample	*h=15*	*h=10*	*h=5*
Administrative assistants	0.013***	0.014***	0.016***	0.021
	(0.003)	(0.004)	(0.004)	(0.015)
High administrative officials	0.002	0.008	-0.000	0.002
	(0.003)	(0.009)	(0.004)	(0.014)
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and for the order of polynomial, $\overline{p}$, preferred by the AIC, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables administrative assistant personnel (containing 35 missing values) and high administrative officials. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

5.2 Effects on wages

Costs for personnel, which make up around half of municipal expenditures, are given by two components: the number of people employed and the wages paid to these employees. We know the effect of grants on one of these from section 4 above. What about the effect on wages?

In our data we observe the total number of full-time equivalent employees as well as the total sum paid out as wages. Using these two variables we have calculated average monthly wages for employees in the local public sector, both in total and disaggregated by sector. Table 10 shows the 2SLS estimates of the effects of grants on this measure of monthly wages (for different bandwidths and the order of polynomial preferred according to the AIC)[35]. As above, the results are evaluated at a grant increase of 100 SEK per capita. From the table it is clear that increased grants are also used to raise wages in the public sector; only for school personnel is the point estimate statistically insignificant across all specifications. Because the economic significance is difficult to assess from these figures (due to the very different unit of measurement), we evaluate these effects in terms of standard deviations and then find that a one standard deviation increase in per capita grants leads to a 0.2–0.3 standard deviation increase in monthly average wages in the majority of sectors – i.e., a quite substantial wage effect.

[35] Of all the regressions estimating the wage effect either on the full sample or with h=15, $\overline{p} = 3$ is preferred in two and $\overline{p} = 1$ in the rest.

Table 10 Effects of grants on wages for municipal employees (2SLS estimates).

	Full sample	*h=15*	*h=10*	*h=5*
Overall wages	2.061 (2.679)	22.316** (10.034)	10.080*** (3.821)	12.974 (11.412)
Administrative wages	7.413 (5.860)	18.229** (7.974)	15.728** (7.543)	19.796 (18.963)
Child care wages	4.301* (2.276)	9.143*** (3.440)	10.623*** (3.938)	11.375 (11.129)
School wages	-4.368 (3.076)	0.923 (3.655)	3.095 (4.193)	8.915 (14.609)
Elderly care wages	3.591* (2.094)	8.827 (6.040)	8.147** (3.217)	7.866 (10.032)
Social welfare wages	6.579 (4.887)	10.443 (6.850)	13.181* (6.925)	27.382 (25.257)
Technical wages	3.398 (2.521)	10.469*** (3.891)	11.289*** (4.168)	18.816 (15.156)
# of observations	*2,511*	*2,346*	*2,047*	*1,241*

Notes: For different bandwidths, *h*, and for the order of polynomial, $\overline{p}$, preferred by the AIC, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables wages in the local governmental sector overall as well as disaggregated by the different sectors. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

5.3 Effects on expenditures

Labor is not the only input in the production of local government services. Even though costs for labor make up around half of municipal expenditures, the other half may be as important. This half consists of costs for, e.g., premises and material such as books, computers, maintenance of infrastructure, etc. One reason why we do not find any employment effects in any other sector besides administration may be that those sectors are in greater need of other things than more personnel. Also, some municipalities have partly outsourced services such as education and elderly care, in which case the personnel is not publicly employed.

According to the evidence in Dahlberg et al. (2008), increased grants stimulate total expenditures krona for krona. In order to understand whether non-personnel expenditures are affected by grants, we estimate our original model in equations (3) and (4), but on various expenditure categories rather than personnel categories. Results from these estimations are given in Table 11.[36]

[36] As in the previous tables, Table 11 only reports the estimate preferred according the AIC. Of all the expenditure regressions on the full sample or with h=15, $\overline{p} = 2$ is preferred by one, $\overline{p} = 3$ is preferred by one and $\overline{p} = 1$ by the rest.

Although the categorization into different types of personnel is not exactly analogous to the expenditure categories, it is clear from the estimation results that expenditures actually increase in most of the sectors when general grants increase, at least to some degree.[37] Noting that some of the estimates are sensitive to the choice of bandwidth but focusing on the statistically significant point estimates, we see that out of 100 additional krona in grants around 5 percent is spent on cultural and political activities, 10 is spent on child care, social welfare and recreational projects, 20 on schools and as much as 30 on elderly care and infrastructure.[38]

Table 11 Effects of grants on different categories of expenditure (2SLS estimates).

	Full sample	*h=15*	*h=10*	*h=5*
Child care expenditures	5.703* (3.178)	8.047* (4.399)	10.702** (5.177)	27.900 (19.182)
School expenditures	34.568*** (5.309)	18.304** (7.833)	17.702** (7.383)	-6.344 (33.044)
Elderly care expenditures	44.041*** (11.595)	25.140 (15.492)	27.100** (13.422)	-16.038 (43.000)
Social welfare expenditures	-2.587 (3.795)	9.303* (5.617)	10.374* (5.954)	21.812 (20.623)
Recreational expenditures	1.792 (1.924)	2.336 (2.474)	5.254* (2.700)	18.407* (10.884)
Cultural expenditures	1.133 (1.128)	1.116 (1.485)	3.700** (1.786)	1.618 (5.513)
Political expenditures	5.843** (2.799)	5.040*** (1.144)	4.892*** (1.301)	6.451 (4.721)
Infrastructural expenditures	25.087** (10.584)	29.050*** (8.737)	29.943*** (8.053)	27.871 (18.483)
# of observations	*2,502*	*2,337*	*2,039*	*1,234*

Notes: For different bandwidths, *h*, and for the order of polynomial, $\overline{p}$, preferred by the AIC, the table reports estimates of α_1 in the second-stage equation (4), with dependent variables per capita expenditures in the respective sectors. Standard errors clustered on municipality are in parentheses, and ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

Unfortunately, there is no separate category for administrative expenditures, but instead these costs are part of the different categories in Table 11. Along the same lines, an increase in monthly average wages may indicate two things; first, wages for those already employed may have been increased, and second, the composition of the pool of employees may have

[37] Recalling the negative and significant effect on school personnel as found in Table 5, it is worth stressing that school expenditures are in no way exceptional to the pattern of expenditure effects displayed in Table 11.
[38] Due to rounding errors and quite large standard errors these effects do not exactly sum to 100.

changed as a result of the municipality laying off low-wage personnel and employing high-wage personnel. With the data available to us, we cannot distinguish between these two possibilities.

6 Concluding remarks

The effectiveness of public funds in stimulating public employment has long been a question on public and labor economists' minds and remains, in these times of worldwide economic recession, as interesting as ever. In many federalist countries where local governments are major employers, increased intergovernmental grants have the potential of limiting overall unemployment. In this paper, by applying the regression kink design to the Swedish grant system, we are able to estimate causal effects of intergovernmental grants on personnel in different local government sectors. We examine the validity of the identifying assumptions in a variety of ways and verify that the exclusion restriction of there being no kink in the direct relation between out-migration and grants indeed seems to hold. Therefore, we can be quite confident that the estimated results represent causal effects and not only correlations. Our robust conclusion is that employment of bureaucratic personnel in the central administration is increased after a marginal increase in grants but that such an effect is lacking both for total personnel and personnel in any of the other five sectors (child care, schools, elderly care, social welfare and technical services). Furthermore, when we estimate the effects on administrative personnel separately for those with basic administrative assistance duties and high officials, we find that the effect comes from the former group.

Our results raise interesting questions regarding the particular characteristics of central administration. In line with the hypothesis and results in Dahlberg and Mörk (2006), one possible explanation could be that bureaucrats are able to influence the local decision-making process in ways that other types of personnel cannot. Such bureaucratic power has long been recognized by economists.[39] But why would bureaucrats wish to employ more fellow bureaucrats, and specifically, more administrative assistant personnel? We can think of a couple of reasons for this. First, a large staff of assistants will be able to cover a variety of tasks that would otherwise be assigned to someone further up the ladder. Hence, by employing more assistants, the high officials can reduce their own work load. Second, having a large staff of assisting personnel increases the number of subordinates, which could give the higher officials a sense of increased power.

[39] Early contributions discussing the role of bureaucrats are Tullock (1965), Downs (1957), Niskanen (1971) and Romer and Rosenthal (1979). Later contributions include Moene (1986), who shows that deviations from the socially optimally bureau are likely. More recent authors argue that bureaucrats are driven by career concerns; see, e.g., Dewatripont et al. (1999) and Alesina and Tabellini (2007). That bureaucrats matter for the political decision-making process as well as in the implementation phase has also been long recognized in the political science literature; see, e.g., Peters (1995), Wilson (1989) and Lipsky (1980).

On the other hand, we also saw that other sectors benefit from a grant increase through larger budgets and higher wages. It is therefore quite possible that the explanation behind the asymmetric effects on employment is less “cynical”. Even though we control for the direct effects of out-migration on personnel, the fact remains that our estimated effects represent local average treatment effects for those municipalities with diminishing population. Perhaps it is too risky for such municipalities to use increased grants to employ more personnel in for example child care and schools. Labor demand in these sectors is likely to be more sensitive to demographic changes than in the administrative sector, with the implication that risk-averse decision-makers (be they politicians, bureaucrats, or both) are reluctant to hire any personnel at all in the sectors where the demand is more volatile and uncertain. Given the widespread union power in Swedish municipalities and difficulties in firing personnel, this explanation may well be likely. Such heterogeneous treatment effects would mean that the overall average effect of grant increases on personnel could potentially be bigger than the local average effect identified here, which is good news for future policies aiming at increasing local public employment by means of grant increases.

A third, and more optimistic, possible explanation for the results is that employing more administrative personnel could actually improve efficiency. Such improvement would be possible if other personnel, in the absence of a grant increase, are preoccupied with administrative duties for which they are overqualified due to lack of enough resources to hire administrative assistant personnel. Finally, it could be that increased grants are systematically associated with the need for an enlarged administration to handle the distribution of these grants. However, we believe this idea to be quite unlikely, given that the grants arrive with no strings attached and that there is no application procedure in place.

The answer to the question posed in the title of this paper is hence negative: giving general grants to lower-level governments does not seem to be an effective way of stimulating local public employment. It is, of course, important to consider how much these results can be generalized. Specifically, there are two things that we would like to stress about the setting in this paper. First, the years studied here constitute a quite prosperous time period, and it is possible that the effects would be different in times of economic recessions, such as the current one. Second, in Sweden intergovernmental grants are distributed to municipalities with no strings attached, meaning that they can spend the money on whatever they wish. Thus, public funds *targeted* at stimulating employment might have different effects.

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Appendix – Additional Summary Statistics

Table A1 contains a data description of wages to municipal employees, defined as the total monthly sum paid out per full-time equivalent, and of municipal expenditures, measured in SEK per capita.

Table A1 Summary statistics.

Variable	Mean	Std dev	Min	Max
Wages, total personnel	17,352	1,770.9	14,027	22,785
Wages, administrative personnel	20,039	2,353	15,094	28,657
Wages, child care personnel	15,995	1,708	12,458	20,514
Wages, school personnel	20,055	1,937	16,075	25,092
Wages, elderly care personnel	15,608	1,545	12,621	20,965
Wages, social welfare personnel	19,186	2,268.4	14,081	26,452
Wages, technical personnel	15,982	1,557.8	12,945	21,778
Expenditures, total	38,049	7,321	20,606	68,380
Expenditures, child care	4,462	871.8	2,277	8,474
Expenditures, schools	11,958	2,165	5,292	21,070
Expenditures, elderly care	12,056	3,466	2,926	24,831
Expenditures, social welfare	1,941	699.8	128	4,973
Expenditures, recreational	1,021	336.0	84	3,145
Expenditures, culture	814.9	243.6	326	2,721
Expenditures, political	621.9	269.6	85	2,641
Expenditures, infrastructure	2,947	1,179	732	18,897

Notes: The number of observations is 2,511 on wages and 2,502 on expenditures.

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