# Stimulating Local Public Employment: Do General Grants Work?*

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**Abstract**

We apply the regression kink design to the Swedish grant system and estimate causal effects of intergovernmental grants on local public employment. Our robust conclusion is that grants do *not* stimulate local public employment. We find no statistically significant effects on total local public employment, and we can exclude even moderate effects. When disaggregating the total effect by sector, we find that personnel in the traditional welfare sectors are unaffected, a conclusion which applies to both publicly and privately employed in these sectors. The only positive and statistically significant effect of grants is that on administrative personnel.

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

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# 1 Introduction

Understanding the determinants of public employment is important for at least two reasons. First of all, the public sector in most European countries supplies important welfare services, such as child care, education and health care. The well-being of the citizens is closely linked to the quality of these services, and given that they are typically very labor intensive, a good way of influencing the quality is to decide how many and who to employ. Second, the public sector commonly accounts for large parts of aggregate labor demand; in many countries, as much as 15–20 percent of the labor force are publicly employed. Hence, stimulating public employment may be a promising way of reducing the overall unemployment level.

Both these issues are of vital importance for central politicians. However, in many countries, the responsibility for supplying welfare services is decentralized to the local level. Central politicians wanting to affect the quality of welfare services or stimulate public employment therefore have to influence local politicians to implement desired policies. This influence can take many forms, but given that one wants to maintain local autonomy, intergovernmental grants are the main financial means through which the central government can have an impact on lower-level governments. Thus, the effect of intergovernmental grants on local public employment is a question of great policy relevance. The main purpose of this paper is to estimate causal effects of general intergovernmental grants on local public employment—both in total and disaggregated by sector—using a panel of Swedish municipalities covering the period 1996–2004.

Starting from negligible shares, in the 1990s many local government services became subject to outsourcing to private contractors. For a given quality and labor composition of the service produced, it is irrelevant for the well-being of the citizens and for the total unemployment level whether the provider is public or private. Although the privatized shares are on average still very small, to complete the analysis, we also study how intergovernmental grants affect private employment in these sectors.

The current economic crisis has, if anything, strengthened the focus on the effect of grants on public employment, since many federalist countries have initiated this type of policies to try to limit the negative effects of the recession on public welfare.[1] A recent example is the American Recovery and Reinvestment Act (ARRA) of 2009, where federal grants are being used to stimulate the US economy and employment. Another country where the central government has turned its hope to grants—and to their potential of stimulating public employment in particular—is Sweden; in the

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

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, it was argued, was to avoid layoffs in order to guarantee a sustained welfare level and limit overall unemployment effects.

Since most decentralized public services are labor intensive, personnel costs typically account for large parts of the budget of lower-level governments. It might, therefore, be expected that more revenues in the form of increased general grants can indeed stimulate public employment. But there are theoretical arguments for why this may fail to happen. For example, local governments can substitute grants for own-source revenues by cutting local taxes. Empirically, there is, however, little support for this argument, as previous research has shown that increased grants to Swedish municipalities stimulate spending one-for-one but leave taxes unchanged (Dahlberg et al., 2008). Still, even when aggregate spending is stimulated by increased grants, it is a priori uncertain whether the additional expenditures are used for personnel and, if so, in which sectors.

Despite the high policy relevance, the existing literature is very limited.[2] The earliest paper of which we are aware is Johnson and Tomola (1977) which evaluates US public employment programs in the 1970's. They find that the effects on employment are substantial in the short run, but that they vanish after one year due to fiscal substitution by state and local governments. More recently, an emerging body of literature tries to estimate to what extent ARRA has affected public as well as total employment. The results from these studies are partly contradictory: Wilson (2012) finds that ARRA spending created or saved about 2 million jobs in its first year. Also the results in Feyrer and Sacerdote (2011) support the effectiveness of ARRA, but suggest that the effects differ for different types of spending within ARRA. Chodorow-Reich et al. (2012) focus on the Medicaid part of ARRA and find that an additional \$100,000 in Medicaid spending resulted in an additional 3.1 job-years, of which the bulk consisted of jobs outside the public sector. Conley and Dupor (2011), on the other hand, find that ARRA spending saved 450,000 jobs in the government sector but destroyed 1 million private sector jobs. Common to all these papers is that they investigate the effects of grants on total employment, public as well as private. Focusing on the specific role of grants in explaining local public employment, Bergström et al. (2004) investigate how grants (and wages) affect the demand for labor by Swedish municipalities. They find that intergovernmental grants had a negligible effect on total local public employment in Sweden over the period 1988–95.[3]

[2]See Ehrenberg and Schwarz (1986) for a survey of the early literature.

[3]Evans and Owens (2007) also investigate how grants affect local public employment, but with a more narrow focus in the sense that they study the effectiveness of grants targeted to new police hires in US states and localities.

All the above mentioned studies face the methodological challenge that grants are likely to be endogenous, meaning that OLS estimates are likely to be biased. The root of the endogeneity problem is that grants are not randomly distributed to lower-level governments but, rather, that these 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.[4] Bergström et al. (2004) use the Arellano-Bond GMM estimator where lagged values of grants (and wages) are used as instruments, and rely on the Sargan test for overidentifying restrictions in order to decide whether the instruments are valid. But because the Sargan test is known to have low power, it is quite possible that the grant endogeneity problem remains unsolved. Wilson (2012) and Conley and Dupor (2011) both use the formula-based parts of the ARRA as instruments for grants. However, it is questionable whether the formulas are set exogenously, implying that they might be correlated with local public employment, which would bias the estimated grant effects. Inspired by Knight (2002), Feyrer and Sacerdote (2011) instead use the mean seniority of the House delegation as instrument for ARRA spending received by the state. The validity of this instrument rests on the assumption that similar political economy factors do not affect the employment level in the state, which is a strong assumption. The most convincing identification strategy in the above papers is the one in Chodorow-Reich et al. (2012) which uses past Medicaid reimbursements as an instrument for ARRA. Conditional on a number of control variables, they argue that this instrument is exogenous—a claim that is supported by a set of placebo investigations.

In this paper, we adopt a version of the identification strategy used by Dahlberg et al. (2008) to solve the grant endogeneity problem. 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)[5] and is labeled regression kink design (RKD) by Nielsen et al. (2010). Card et al. (2009) derive formal identifying assumptions and re-

[4]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.

[5]See Angrist and Lavy (1999), Hahn et al. (2001), Lee (2008) and Lee and Lemieux (2010) for important contributions.

sulting 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 the assignment variable on the outcome is used as an exclusion restriction in an IV estimation. For our application, this approach identifies the causal effect of grants as long as the direct effect of out-migration on personnel is smooth.

Our empirical analysis shows that grants do *not* stimulate local public employment.[7] Not only are the effects on total personnel statistically insignificant, the small coefficients are estimated with relatively good precision such that even moderate elasticities can be rejected. When disaggregating the total effect by sector, we find that personnel in the traditional welfare sectors—e.g., child care, schools, elderly care and social welfare—are unaffected. This zero result is found both in the main analysis focusing on employment by the municipalities as well as in the subanalysis focusing on the smaller share employed by a private contractor. The only positive and statistically significant effect is that on administrative personnel, where the size of the effect is moderate to large.[8] However, since this is a small group relative to total local public employment, this hardly matters for the effectiveness of grant to improve the overall employment level.[9]

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 analyzes how private employment in sectors that have been subject to outsourcing are affected by grant increases. Section 6 looks in more detail at the positive grants effects obtained on administrative personnel, and Section 7 concludes the paper by discussing possible interpretations of the results.

[6] As the RKD is still rather new, it has so far not been extensively applied. However, both Nielsen et al. (2010) and Card et al. (2009) have empirical applications, as do Guryan (2003), Simonsen et al. (2010) as well as the study by Dahlberg et al. (2008) referred to above.

[7] This result is in line with the findings in Bergström et al. (2004).

[8] This result is consistent with Strumpf (1998) who finds grants to boost public spending more the larger the public administration.

[9] When contrasting these results to the earlier literature reviewed above, it should be stressed that the period we study differs from that of the ARRA, in the sense that the Swedish economy was in a relatively good state during the period studied.

## 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

$$y_{i,t} = \beta_0 + \beta_1 g_{i,t} + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is the number of personnel employed by municipality $i$ in year $t$ (in total and disaggregated by sector),[10] and $g_{i,t}$ are grants received by the municipality. A (naïve) OLS estimate of $\beta_1$ will most likely be biased. The source of the bias can either be simultaneity of grants and personnel—i.e., that the level of public employment in the municipality affects the amount of grants received—or omission of key variables that determine both grants and personnel. Note, however, that even when the grant formula is completely known, it is not possible to identify the causal effect of grants simply by including all grant determinants in a regression, since that would leave no remaining variation in the grants variable to identify the effect of interest.[11] To eliminate all sources of bias, an experiment where municipalities are randomly given different amounts of grants would instead be ideal. 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 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 return to the role played by these grants in Section 3.

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

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

[10]In Section 5 when we analyze outsourced personnel, $y_{i,t}$ will instead be the number of people employed by a private firm operating in the various outsourced sectors in municipality $i$ in year $t$.

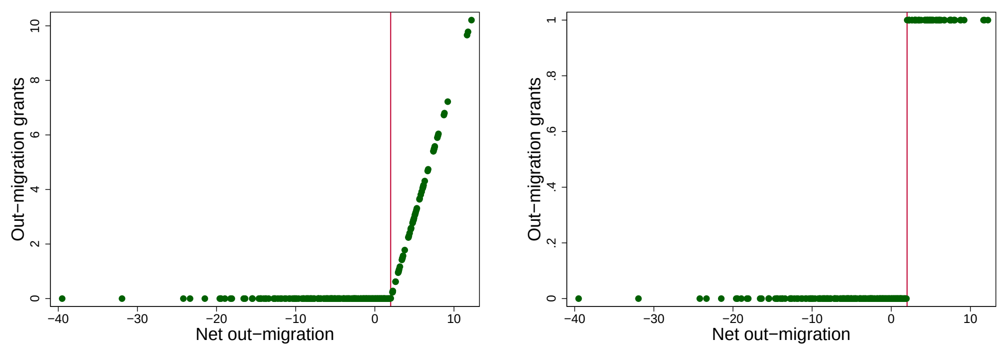
[11]Note that the identification in both Wilson (2012) and Conley and Dupor (2011) partly builds on the fact that they know the formulas that are used when grants are assigned, but that they do not allow for these formulas to have any direct effect on employment.

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., $m_{i,t} = 100(1 - n_{i,t-2}/n_{i,t-12})$. Although the assignment variable partly reflects changes in mortality rate and birthrate, we will refer to it as (net) out-migration rate.

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 seen from panel (a), there is a well-defined kink-point such that municipalities with net out-migration rates lower than two percent do not receive any out-migration grants, whereas municipalities with net out-migration rates above two percent do.[12] For municipalities above the kink-point, the marginal increase in per capita grants for each percentage point increase in out-migration is represented in equation (2) by the parameter $a$, which was constant and equal to 100 SEK (6.50 SEK≈1 USD) during the period studied. This is seen in panel (b) of the figure. Thus, as clearly illustrated by these graphs, there is a non-linear relationship between out-migration and grants, and a discontinuous relationship between out-migration *increases* and grants.

Figure 1: Out-migration grants against net out-migration

(a) Grants received (b) Marginal increase in grants




*Note:* Grants are measured in 100 SEK per capita (6.50 SEK≈1 USD).
*Data source:* Statistics Sweden & The Swedish Association of Local Authorities and Regions.

Card et al. (2009) derive necessary and sufficient conditions for a kinked assignment rule like the one in (2) to identify a local average treatment effect (LATE). For our application, these assumptions are (i) that 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 (ii)

[12]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.

that the marginal effect of out-migration on personnel is smooth.[13] The first assumption rules out extreme sorting, or precise manipulation of the out-migration rate. This seems like an innocuous assumption considering that the out-migration rate is measured over a ten-year period, and that it is taken from official registers. The second assumption says that, although out-migration can have a direct effect on personnel, there can be no kink in this relationship. In other words, there can be no jump in the *marginal* effect of out-migration on personnel (like the one in panel (b) of Figure 1). The implication of this assumption is that there should not be any kinks in pre-determined covariates, which can be tested by checking if the baseline estimates are sensitive to the inclusion of such covariates.

In this paper, we adopt a fuzzy version of the RKD. Analogously to the RDD, the fuzzy version of RKD is appropriate when treatment is not entirely determined by distance to the kink-point. And although the assignment rule in (2) is entirely deterministic regarding the treatment of out-migration grants, the treatment of cost-equalizing grants might not be deterministic due to kink-points in other components of the cost equalization that—by coincidence—could be close to the kink-point at two percent out-migration.[14] Under this setting, it would in principle be possible to estimate a separate treatment effect at each kink. In practice, however, this is not viable for two reasons: First, the structure of the cost equalization and most of its components is very complex, so that an RKD treatment effect cannot be captured by a single parameter. Second, it would require the inclusion of flexible functions of all of the different assignment variables, but we lack data on some of these.

Our sole focus is instead on the component for out-migration grants and the assignment rule in (2), which we 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 personnel. We additionally need to assume that any direct effects of other variables subject to kinked assignment rules are captured by the direct effects of out-migration.

The first and the second stage in the two-stage least squares (2SLS) are given by the following two equations:[15]

[13]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).

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

[15]Note that if the assignment rule in (2) were entirely deterministic regarding the treatment of cost-equalizing grants, the parameter $\alpha_1$ in equation (3) would be identical to the parameter $a$ in the assignment rule (2), and there would be no need for a two-stage procedure.

$$g_{i,t} = \alpha_0 + \alpha_1(m_{i,t} - k)D + \sum_{p=1}^{\bar{p}} \phi_p(m_{i,t} - k)^p + T_t + \epsilon_{i,t} \tag{3}$$

$$y_{i,t} = \beta_0 + \beta_1\hat{g}_{i,t} + \sum_{p=1}^{\bar{p}} \delta_p(m_{i,t} - k)^p + T_t + \varepsilon_{i,t}, \tag{4}$$

where $\hat{g}_{i,t}$ are predicted cost-equalizing grants obtained from estimating the first stage in (3), $k = 2$ is the kink-point, the interaction term $D$ is an indicator for out-migration rates above the kink-point (i.e., $D = 1(m_{i,t} > k)$) and is the excluded instrument, and $T_t$ are year fixed-effects. Thus, the excluded instrument captures the kinked relationship between out-migration and personnel stemming from increased out-migration grants. 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. Equations (3) and (4) can be altered by varying $\bar{p}$ as well as the bandwidth, $h$, 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 $h = \{5, 10, 15, \infty\}$.The $\beta_1$ parameter that can be identified in this setup is a weighted LATE, with weights proportional to the ex-ante probability of being close to the kink-point.

Thanks 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.

To end the section on identification, let us state the conditions under which we can recover a causal parameter with this approach, and how these conditions can be checked: (i) The instrument needs to be relevant. This can simply be checked with the statistical significance of the estimate of $\alpha_1$ in the first-stage regression. (ii) The exclusion restriction of no kink in the direct effect needs to hold. An implication of this is that there should not be any kinks in pre-determined covariates. (iii) The control function of out-migration needs to capture the direct effects of any other variables subject to kinked assignment rules. The validity of condition (ii) can be examined by checking the sensitivity of the estimated treatment effect to the inclusion of pre-determined covariates. To the extent that these covariates are correlated with other variables subject to a kinked assignment rule, this approach also examines the validity of condition (iii). We return to this in Section 4.4.

# 3 Institutional background

This section first describes the role of Swedish local governments in general and the role of intergovernmental grants in particular. The section ends 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 half of all municipal expenditures. The most important source of municipal revenue is a proportional local income tax, which constitutes 60-70 percent of total revenues. The rest consists 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 were actually net contributors to the grant system during the entire 1996–2004 period.

Swedish municipalities have a long standing tradition of high autonomy both with respect to the size and the composition of their spending. For example, they are free to set their own tax rate and are able to borrow funds. The local autonomy of municipalities was further strengthened in 1993 when a major grant reform replaced a system of targeted central government grants to all municipal services (education, child and elderly care, social services and infrastructure) with general grants—that is, the majority of grants were no longer earmarked. After some early changes, the new system officially came into place in 1996 and consisted of a per capita grant, income-equalizing grants and cost-equalizing grants, all distributed as general grants with no strings attached. The purpose of the cost equalization was (and still is) to reduce differences in effective costs due to unequal structural conditions across municipalities, whereas the income equalization guarantees per capita tax revenues of a fixed percentage of the national average.[16] The system for cost-equalizing grants is constantly undergoing changes. However, there were no such changes during the period studied in our paper, but the same grant system prevailed during the entire investigated period.

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

## 3.2 Data

The data in the paper consists of a panel of 279 Swedish municipalities observed over the time period 1996–2004.[17] As described 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%, whereas the cost-equalizing grants for eligible municipalities amounted to around 20% of total grants. Thus, for eligible municipalities, the out-migration grant is a non-negligible source of revenue.[18] Over the period studied, 112 municipalities never received any out-migration grants (as they never had an out-migration rate below 2%), 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 but are concentrated in the north.

Table 1 displays summary statistics for the two grant variables (cost-equalizing grants and out-migration grants, both measured in 100 SEK per capita), the main outcome variables (total municipal personnel and municipal personnel in administration, child care, schools, elderly care, social welfare and technical services,[19] all measured in full-time equivalents per 1,000 capita) and the assignment variable (net out-migration rate). The large standard deviations relative to the means and the negative minimum values of cost-equalizing and out-migration grants seen in Table 1 reflect the fact that these parts of the grant system are self-financed.

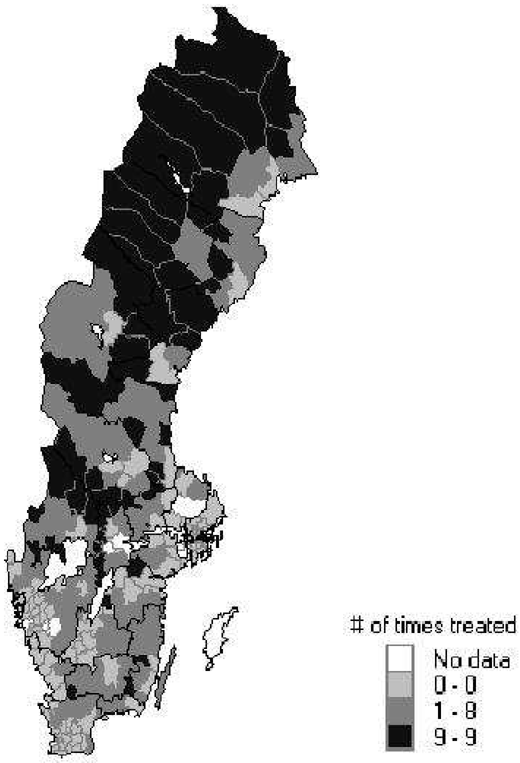
As for personnel, most people are employed in elderly care and schools—21 and 17 full-time equivalents per 1,000 inhabitants, respectively—followed by personnel in child care and technical services, with around 10 full-time equivalents per 1,000 inhabitants. The national aggregates of these variables are illustrated in panel (a) of Figure 3, showing the sector-wise evolution of the total number of full-time equivalents employed by a municipality. As seen from the figure, employment in elderly care has increased

[17]Data 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).

[18]Also suggesting that the out-migration grants indeed matter is the paper by Dahlberg et al. (2008), who study the same period with the same—but at the time somewhat less developed—research design as here, where increased grants are found to stimulate public expenditures considerably.

[19]The way in which total personnel is disaggregated into the various sectors is in accordance with The Swedish Association of Local Authorities and Regions. According to this categorization, administrative personnel include high officials, heads of local public authorities as well as administrative assistants not working in a particular sector.

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




*Data source:* Statistics Sweden.

Table 1: Summary statistics for main variables

| | mean | std.dev | min | max | obs |
|---|---|---|---|---|---|
| Personnel, total | 65.0 | 9.88 | 30.9 | 101.8 | 2511 |
| Personnel, administration | 5.48 | 1.13 | 2.19 | 12.9 | 2511 |
| Personnel, child care | 10.5 | 1.72 | 3.33 | 17.9 | 2511 |
| Personnel, schools | 16.7 | 2.71 | 8.45 | 32.1 | 2511 |
| Personnel, elderly care | 21.2 | 6.66 | 1.48 | 41.1 | 2511 |
| Personnel, social welfare | 1.78 | 0.71 | 0.084 | 6.82 | 2511 |
| Personnel, technical services | 9.31 | 2.42 | 1.63 | 17.8 | 2511 |
| Cost-equalizing grants | 5.32 | 24.5 | -34.7 | 132.0 | 2511 |
| Out-migration grants | 1.12 | 2.87 | -1.26 | 13.8 | 2511 |
| Net out-migration | -0.79 | 7.92 | -43.0 | 16.6 | 2511 |
| Population | 27229.4 | 48883.9 | 2575 | 761721 | 2511 |
| Population aged 0–6 | 7.92 | 1.31 | 4.71 | 12.8 | 2511 |
| Population aged 7–15 | 12.2 | 1.19 | 6.78 | 16.4 | 2511 |
| Population aged 80+ | 5.41 | 1.38 | 1.25 | 9.14 | 2511 |
| Foreign born | 4.01 | 2.73 | 0.56 | 29.1 | 2511 |

*Note:* Grants are measured in 100 SEK per capita (6.50 SEK≈1 USD) and personnel are measured in full-time equivalents per 1,000 capita. The variables net out-migration, population aged 0–6, population aged 7–15, population aged 80+ and foreign born are given in percent.

*Data source:* Statistics Sweden & The Swedish Association of Local Authorities and Regions (SALAR).

quite substantially (due to the aging population), as has employment in schools, whereas fewer are employed in child care.[20] Employment in the remaining sectors has been fairly stable and, all in all, the number of full-time equivalents 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. Panel (b) of Figure 3 partly illustrates this trend by plotting the aggregate number of people (here in head counts rather than full-time equivalents) employed by a non-profit or for-profit private firm operating in typical local public sectors.[21] Except for schools, our data here is unfortunately limited to the period 2002–04, but one may still deduce from the graphs that, although the number of privately employed has increased, the vast majority working in areas traditionally dominated by public providers were still employed by a municipality by the end of the studied period. Therefore—and partly also because of the data limitations—our main analysis in Section 4 will focus on public employment, but will be supplemented by the subanalysis in Section 5 on private

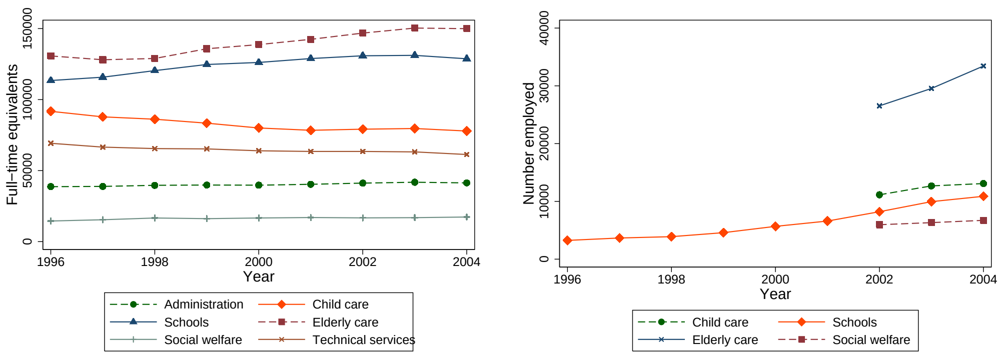
[20]Part of the employment decrease in child care and the increase in schools is explained by a transfer of pre-schools for 6-year olds from the child care sector to the school sector.

[21]The data on public/private personnel in the various sectors do not perfectly match each other. This is partly because there is no private personnel in some sectors, and partly because the data comes from two different sources where the categorization into the various sectors is done somewhat differently.

employment in outsourced sectors.

Figure 3: Aggregate employment in different local government sectors; 1996–2004

(a) Publicly employed (b) Privately employed




*Data source:* Statistics Sweden & The SALAR.

Table 1 also presents the socio-economic variables that will be used to examine the sensitivity of the baseline estimates to the inclusion of time-varying covariates (see Section 4.4): population size, share of the population aged 0–6, share of the population aged 7–15, share of the 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 is, of course, the underlying reason why there is a need for equalization).

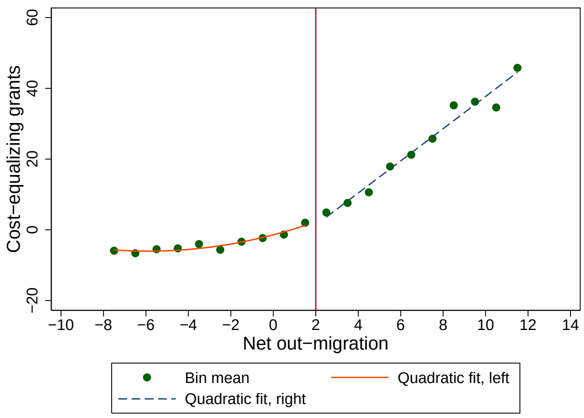
# 4 Effects of grants on local government personnel

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

## 4.1 Graphical analysis

The nature of the RKD makes visual inspections particularly attractive—if there is an effect, this is seen as a kink in the outcome corresponding to the kink in the assignment rule. To this aim, 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 first does this for cost-equalizing grants (the treatment variable of interest), where we have chosen a quadratic fit and a bin size of 1 percentage point.[22] The figure clearly reveals the same kink in the relationship between out-migration and cost-equalizing grants as that between out-migration and out-migration grants (cf. Figure 1). This is indicative of a strong first stage in our two-stage procedure.

Figure 4: Cost-equalizing grants against net out-migration




*Note:* Grants are measured in 100 SEK per capita (6.50 SEK≈1 USD).

*Data source:* The SALAR.

Figure 5 displays equivalent graphs, but for total personnel and for personnel within each of the different sectors (the outcome variables of interest). For administrative personnel, there is a distinct kink around two percent out-migration, whereas there are no such kinks visible neither for total personnel nor for any of the other personnel categories. Thus,

[22]Lee and Lemieux (2010) 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 our outcome variables, 20 bins (implying a bin size of 1) are never rejected at the 10 percent level.

the graphics are highly suggestive of a positive effect of grants only on administrative personnel. The econometric analysis to follow will show how these graphical results correspond to statistical estimates.

## 4.2 First-stage estimates

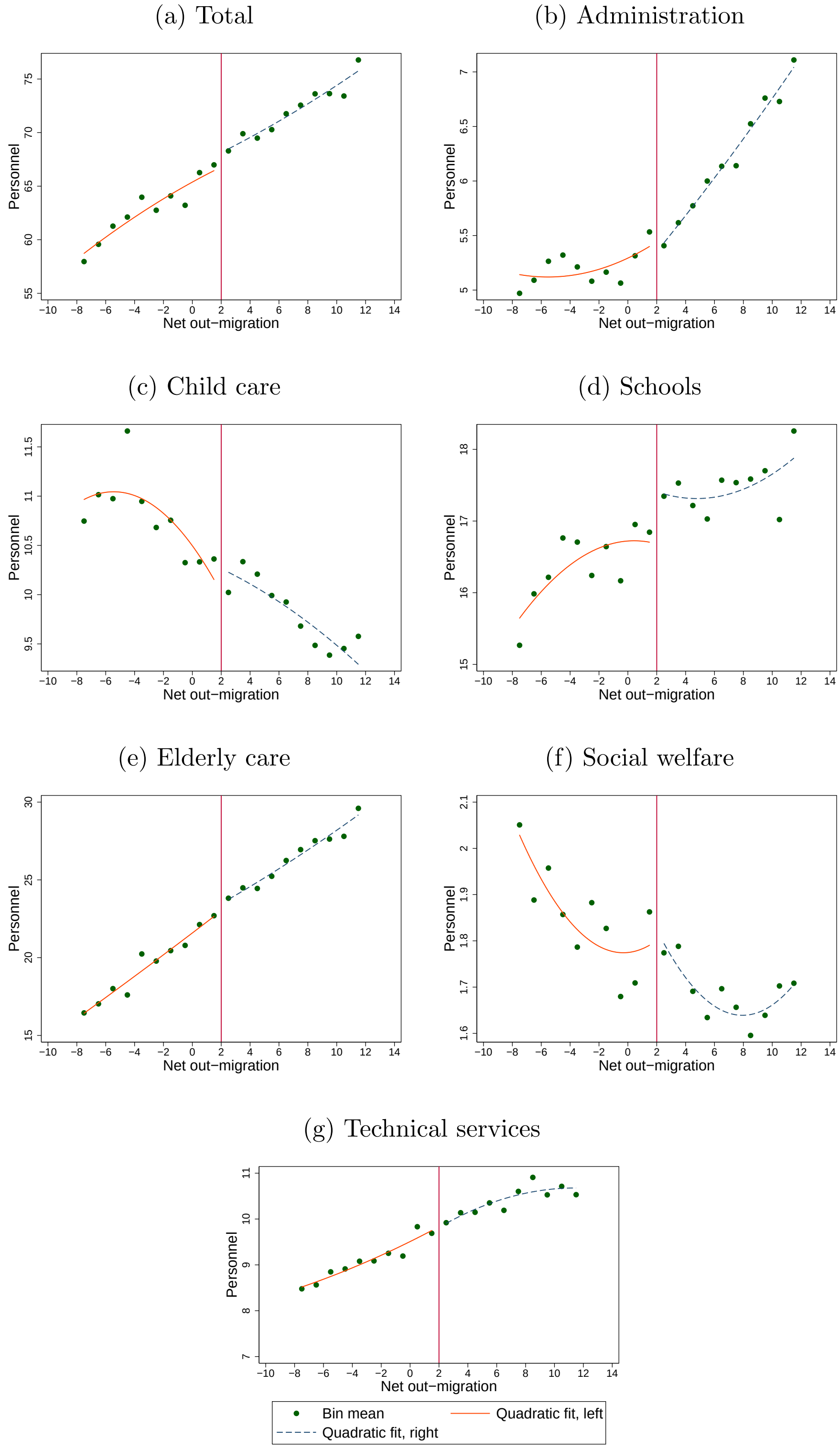
Although it is hard to detect any kinked relations between out-migration and personnel in most of the sectors in Figure 5, out-migration appears to have quite strong direct effects in some of the sectors. In the econometrics, we control for these direct effects by including a flexible function of out-migration 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 regressions—or, specifically, from the statistical significance of the estimate of the *incremental* effect of out-migration on total cost-equalizing grants, $g_{i,t}$, at the kink-point. This estimate corresponds to $\alpha_1$ in the first-stage equation (3). If $\hat{\alpha}_1$ 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 grants, implying that the instrument is relevant.

Because we do not know the form of the direct effects, we run regressions controlling for out-migration linearly as well as with a $2^{nd}$ and $3^{rd}$ order polynomial. Table 2 shows first-stage estimates and associated standard errors of $\alpha_1$ in equation (3). The standard errors are robust to arbitrary residual heteroskedasticity and serial correlation within municipality.[23] Each row corresponds to an estimation of equation (3) with different order of polynomial $\bar{p} = \{1, 2, 3\}$, while each column corresponds to an estimation where either all observations are included, or the estimation sample is restricted to observations $\pm 15, 10$ and 5 percentage points from the kink-point. Note that with the two narrowest bandwidths, due to the sample reduction, we only estimate with $\bar{p} = 1$.

It is clear from the table that all estimates are highly statistically significant, irrespective of order of polynomial and bandwidth. The magnitude of the estimates is around 3, 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 1, which would be the case if treatment of cost-equalizing grants were a fully deterministic function of the assignment rule

[23]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.

Figure 5: Municipal personnel against net out-migration




(a) Total (b) Administration (c) Child care (d) Schools (e) Elderly care (f) Social welfare (g) Technical services

*Note:* Personnel is measured in full-time equivalents per 1,000 capita.

*Data source:* The SALAR.

Table 2: First-stage estimates

| | *Full sample* | $h = 15$ | $h = 10$ | $h = 5$ |
|---|---|---|---|---|
| $\bar{p} = 1$ | 4.174*** | 3.636*** | 3.988*** | 1.980** |
| | (0.684) | (0.739) | (0.744) | (0.961) |
| $\bar{p} = 2$ | 3.176*** | 3.118*** | | |
| | (0.761) | (1.055) | | |
| $\bar{p} = 3$ | 3.350*** | 4.076*** | | |
| | (1.036) | (1.351) | | |
| Observations | 2511 | 2346 | 2047 | 1241 |

*Note:* For different bandwidths, $h$, and order of polynomials, $\bar{p}$, the table reports estimates of $\alpha_1$ in the first-stage equation (3) on cost-equalizing grants. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.
*Data source:* The SALAR.

in (2).[24] 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 from Table 2 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 $\beta_1$ in equation (4) for total local government personnel and for personnel disaggregated by the six sectors (administration, child care, schools, elderly care, social welfare and technical services). As above, each column corresponds to an estimation with different bandwidths, while the three rows per outcome variable correspond to an estimation with different polynomials. 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 full-time equivalents per 1,000 capita for the dependent variable and 100 SEK per capita for the grants variable (which is 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

[24]Note that the marginal increase in out-migration grants for each percentage point increase in out-migration above two percent is 100 SEK per capita, and that cost-equalizing grants are defined in 100 SEK per capita. In other words, the parameter $a$ in equation (2) equals 1.

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

| | | *Full sample* | $h = 15$ | $h = 10$ | $h = 5$ |
|---|---|---|---|---|---|
| Total personnel | $\bar{p} = 1$ | **0.0521** | -0.0593 | -0.0152 | -0.218 |
| | | **(0.0478)** | (0.0697) | (0.0569) | (0.211) |
| | $\bar{p} = 2$ | -0.164 | 0.000877 | | |
| | | (0.106) | (0.133) | | |
| | $\bar{p} = 3$ | -0.108 | **0.0389** | | |
| | | (0.137) | **(0.106)** | | |
| Administrative personnel | $\bar{p} = 1$ | 0.0300*** | **0.0298***** | 0.0375*** | 0.0358 |
| | | (0.00568) | **(0.00750)** | (0.00735) | (0.0223) |
| | $\bar{p} = 2$ | **0.0289***** | 0.0499*** | | |
| | | **(0.00929)** | (0.0183) | | |
| | $\bar{p} = 3$ | 0.0414*** | 0.0449*** | | |
| | | (0.0144) | (0.0163) | | |
| Child care personnel | $\bar{p} = 1$ | 0.00353 | **-0.00417** | -0.000811 | 0.0127 |
| | | (0.00669) | **(0.00881)** | (0.0107) | (0.0360) |
| | $\bar{p} = 2$ | **-0.00721** | 0.0341 | | |
| | | **(0.0129)** | (0.0354) | | |
| | $\bar{p} = 3$ | -0.00593 | 0.0177 | | |
| | | (0.0246) | (0.0228) | | |
| School personnel | $\bar{p} = 1$ | **-0.0134** | **-0.0327** | -0.0306 | -0.0965 |
| | | **(0.0140)** | **(0.0206)** | (0.0200) | (0.0806) |
| | $\bar{p} = 2$ | -0.0402 | -0.0542 | | |
| | | (0.0285) | (0.0477) | | |
| | $\bar{p} = 3$ | -0.0602 | -0.0339 | | |
| | | (0.0444) | (0.0357) | | |
| Elderly care personnel | $\bar{p} = 1$ | **0.0425** | **-0.0264** | -0.0218 | -0.119 |
| | | **(0.0287)** | **(0.0410)** | (0.0356) | (0.128) |
| | $\bar{p} = 2$ | -0.105 | -0.0628 | | |
| | | (0.0642) | (0.0905) | | |
| | $\bar{p} = 3$ | -0.110 | -0.0318 | | |
| | | (0.0886) | (0.0771) | | |
| Social welfare personnel | $\bar{p} = 1$ | **-0.00730**** | -0.00116 | 0.000415 | -0.00659 |
| | | **(0.00328)** | (0.00465) | (0.00516) | (0.0160) |
| | $\bar{p} = 2$ | 0.00518 | 0.00369 | | |
| | | (0.00700) | (0.0126) | | |
| | $\bar{p} = 3$ | 0.00720 | **-0.00259** | | |
| | | (0.0114) | **(0.00983)** | | |
| Technical personnel | $\bar{p} = 1$ | -0.00333 | -0.0247 | 0.0000700 | -0.0485 |
| | | (0.0157) | (0.0217) | (0.0170) | (0.0544) |
| | $\bar{p} = 2$ | -0.0462 | **0.0293** | | |
| | | (0.0306) | **(0.0389)** | | |
| | $\bar{p} = 3$ | **0.0188** | 0.0435 | | |
| | | **(0.0357)** | (0.0302) | | |
| Observations | | 2511 | 2346 | 2047 | 1241 |

*Note:* For different bandwidths, $h$, and order of polynomials, $\bar{p}$, the table reports estimates of $\beta_1$ in the second-stage equation (4), with the dependent variables total personnel as well as personnel disaggregated by the different sectors. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

*Data source:* The SALAR.

three rows of Table 3 that there is, in fact, no overall effect of grants.[25] Not only are the estimated coefficients statistically insignificant, but in some specifications the sign is even negative. And although the point estimates are somewhat sensitive to order or polynomial and bandwidth, they are all of limited economic relevance. Consider, for example, the largest point estimate of 0.05 which is obtained with the full sample and with $\bar{p} = 1$; had this estimate been statistically significant, this would have implied an increase of 0.08 percent in total personnel. 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 implies an elasticity of total personnel with respect to grants of a mere 0.01.[26] Thus, from the point estimate we conclude that there are, if any, very small effects on total personnel. This conclusion remains also when the uncertainty in this estimate is taken into account; with a 95 percent confidence interval of [-0.04;0.15], elasticities any larger than 0.035 can be rejected at conventional significance levels. Furthermore, the largest upper bound of the elasticity of total personnel implied by *any* of the point estimates and its standard error in Table 3 is 0.06.

In fact, the 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 the one 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 only focusing on the specifications preferred according to the AIC.

Concerning the size of the statistically significant effect on the administration, its economic significance is of an order of magnitude larger than that on total personnel; the point estimate of around 0.03 is around 0.5 percent of the mean for this personnel category, which implies an elasticity (evaluated, as above for total personnel, in terms of the mean of the absolute value of cost-equalizing grants) of around 0.08. And the largest upper bound of this elasticity implied by any of the point estimates and its

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

[26]The elasticity, $e$, is defined as $e = \widehat{\beta_1} \times \overline{|g|}/\overline{Y}$, where $\overline{|g|}$ is the mean of the absolute value of cost-equalizing grants and $\overline{Y}$ is the mean of the specific personnel category (in total or in the specific sector). We base the elasticity on the mean of the *absolute* value because of the equalizing feature of the grants variable, which means that 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.

standard error is 0.23 (as compared to 0.06 for total personnel).

Elasticities are informative for evaluating the size of the estimated effects, especially since the units of measurement of the outcome variable (full-time equivalents per 1,000 capita) and the independent variable of interest (100 SEK per capita grants) are very different. As an alternative interpretation, we also calculate how much of a 100 SEK grant increase that would be accounted for by the estimated effects on full-time equivalents, while keeping the sector-specific average wage fixed. We refer to this alternative outcome variable as "hypothetical wage costs", and measure it in 100 SEK per capita.[27]

Table 4 evaluates the effects estimated in the previous table using this variable, but to economize on space, only the AIC-preferred estimates are shown (for the full sample and with $h = 10$ where we indeed estimate the model with higher order polynomials). For reference, the rightmost column reports the sample mean of the ratio of hypothetical wage costs to total municipal expenditures. Note that the difference between these estimates and those in Table 3 is simply the scaling, and that the precision and significance levels are exactly as above.[28] Thus, the result that the only statistically significant, positive and robust effect is that on the administration is reproduced. As the table shows, of the 100 SEK grant increase, around 10 SEK would be spent on wages due to the increased employment of administrators. Considering that wage costs to administrative personnel on average merely accounts for five percent of total expenditures, this is, again, a large effect on this personnel category. Further, compare this to the largest (but statistically insignificant) point estimate on total personnel, which implies that 14 out of the additional 100 SEK would be spent on wages in total—even though total wages accounts for almost half of all municipal expenditures.

Thus, the overall conclusion from Tables 3 and 4 is that grants do *not* stimulate local public employment. Not only are the effects on total personnel statistically insignificant, the small coefficients are estimated with relatively good precision such that even moderate elasticities can be rejected. When disaggregating the total effect by sector, we find that personnel in the traditional welfare sectors are unaffected. The only positive and statistically significant effect is that on administrative personnel, where the size of the effect is moderate to large. However, since this is a small

[27] The wage information available in the data is total wages paid to the various personnel categories. To better account for the full wage costs, we add payroll taxied proxied to 30 percent, which is close to the level of payroll taxes for the majority of workers during the period studied.

[28] The purpose of this analyze is solely to get an alternative interpretation of the effects on employment as presented in Table 3. In principle, one could also look at effects of grants on actual wages. However, for such an analysis, the wage measure available to us is too crude.

Table 4: Effects of grants on municipal personnel, evaluated at hypothetical wage costs (2SLS estimates)

| | *Full sample* | $h = 15$ | $h = 10$ | $h = 5$ | $\frac{\text{wages}}{\text{expenditures}}$ |
|---|---|---|---|---|---|
| Total personnel | 0.141 | 0.105 | -0.0410 | -0.591 | 0.470 |
| | (0.129) | (0.287) | (0.154) | (0.571) | |
| Administrative personnel | 0.0903*** | 0.0931*** | 0.117*** | 0.112 | 0.046 |
| | (0.0290) | (0.0235) | (0.0230) | (0.0696) | |
| Child care personnel | -0.0180 | -0.0104 | -0.00202 | 0.0318 | 0.072 |
| | (0.0322) | (0.0220) | (0.0267) | (0.0898) | |
| School personnel | -0.0420 | -0.102 | -0.0958 | -0.302 | 0.140 |
| | (0.0437) | (0.0643) | (0.0626) | (0.252) | |
| Elderly care personnel | 0.104 | -0.0643 | -0.0531 | -0.289 | 0.135 |
| | (0.0700) | (0.0998) | (0.0866) | (0.312) | |
| Social welfare personnel | -0.0218** | -0.00775 | 0.00124 | -0.0197 | 0.014 |
| | (0.00983) | (0.0294) | (0.0154) | (0.0480) | |
| Technical personnel | 0.0470 | 0.0731 | 0.000175 | -0.121 | 0.062 |
| | (0.0890) | (0.0969) | (0.0424) | (0.136) | |
| Observations | 2511 | 2346 | 2047 | 1241 | 2502 |

*Note:* For different bandwidths, $h$, columns 1–4 report estimates of $\beta_1$ in the second-stage equation (4), with the dependent variables hypothetical wage costs for total personnel as well as for personnel disaggregated by the different sectors. Column 5 reports the sample mean of the ratio of hypothetical wage costs to total municipal expenditures. Hypothetical wage costs are defined as 100 SEK per capita costs for employing the actual number of full-time equivalents while fixing the average wage at the sample mean. For all personnel categories, the regressions are estimated with the AIC-preferred polynomial (cf. Table 3). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

*Data source:* Statistics Sweden & The SALAR.

group relative to total local public employment, this hardly matters for the effectiveness of grants to improve the overall employment level.

## 4.4 Testing the identifying assumptions

The validity of the above results rests on the identifying assumptions, and in this section, we first test the assumption of smooth density of out-migration. Then, we investigate the assumptions that the direct effects of out-migration are smooth and that the control function captures any direct effects of other variables subject to kinked assignment rules. This is done by investigating whether the baseline estimates are sensitive to the inclusion of a set of control variables.

### 4.4.1 Smooth density of the assignment variables

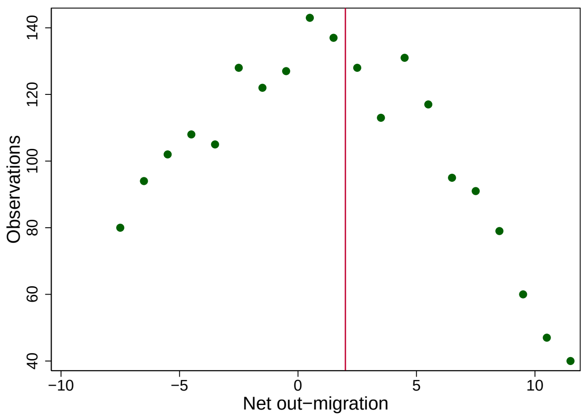
As shown by Card et al. (2009), one of the main identifying assumptions in the RKD is that the derivative of the density of the assignment variable is smooth around the kink-point. This implies that extreme sorting needs to be ruled out, something which is best done graphically as in Figure 6. This figure plots the number of observations within each 1-percentage point bin of net-outmigration in the range $\pm 10$ from the kink-point. Although the density of observations does not evolve smoothly within the entire range, it is comforting to see that there is no dramatic increase in the density at the kink-point (marked by the vertical line).[29] Given the type of assignment variable in our application, this might not come as a surprise: it seems quite difficult for municipalities to perfectly manipulate their rate of out-migration during a ten-year period. Interestingly, Figure 6 also shows that there is a large number of municipalities around the kink-point, implying that the weighted LATE estimate that we identify in fact applies to a quite large number of municipalities.

### 4.4.2 Controlling for pre-determined covariates

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 kinks in pre-determined covariates: if the direct effects of out-migration on personnel are smooth,

[29]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 $\alpha_1$ differs from zero. Doing this we do not find any evidence of bunching on the right-hand side of the kink-point.

Figure 6: Density of net out-migration




*Data source:* The SALAR.

the effect on pre-determined covariates should also be smooth. This means that this identifying assumption can be tested by adding pre-determined covariates to the baseline specification and examine how the estimates react. To the extent that these pre-determined covariates are correlated with assignment variables other than out-migration that are also subject to kinked assignment rules, this is also a test of the assumption that the direct effects of such other assignment variables are captured by the control function of out-migration.

A prerequisite for this test to work is that the chosen variables are pre-determined—i.e., they cannot 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 is the following: total population, share of the population aged 0–6, share of the population aged 7–15, share of the population aged 80+ and share of foreign-born citizens.

The estimates of $\beta_1$ in equation (4) obtained when these variables are included are provided in Table 5.[30] Again, to economize on space, the table only includes the estimate with the order of polynomial preferred by the AIC in the baseline specification (cf. Table 3). If the identifying assumptions are valid, the only thing that should change from the baseline results is—if anything—that the precision of the estimates is improved. Comparing Table 5 to the estimates in Table 3 (and particularly those in bold), it is clear that the baseline results are not affected to any consider-

[30]Because the first-stage estimates obtained when including these variables are very similar to those in the baseline specification, we do not report them. The results are, of course, available upon request.

Table 5: Effects of grants on municipal personnel when controlling for pre-determined covariates (2SLS estimates)

| | *Full sample* | $h = 15$ | $h = 10$ | $h = 5$ |
|---|---|---|---|---|
| Total personnel | 0.0509 | 0.0523 | -0.0233 | -0.187 |
| | (0.0395) | (0.0876) | (0.0463) | (0.134) |
| Administrative personnel | 0.0239*** | 0.0251*** | 0.0310*** | 0.0283** |
| | (0.00725) | (0.00620) | (0.00619) | (0.0139) |
| Child care personnel | -0.0212** | -0.00847 | -0.00453 | -0.0102 |
| | (0.0104) | (0.00770) | (0.00914) | (0.0221) |
| School personnel | -0.0107 | -0.0270 | -0.0327* | -0.0829 |
| | (0.0130) | (0.0169) | (0.0177) | (0.0523) |
| Elderly care personnel | 0.0425** | 0.000852 | -0.0112 | -0.0654 |
| | (0.0198) | (0.0253) | (0.0251) | (0.0670) |
| Social welfare personnel | -0.00762*** | -0.00621 | -0.00535 | -0.0146 |
| | (0.00295) | (0.00872) | (0.00443) | (0.0112) |
| Technical personnel | 0.0274 | 0.0328 | -0.000535 | -0.0439 |
| | (0.0294) | (0.0311) | (0.0145) | (0.0374) |
| Observations | 2511 | 2346 | 2047 | 1241 |

*Note:* For different bandwidths, $h$, the table reports estimates of $\beta_1$ in the second-stage equation (4), with the dependent variables total personnel as well as personnel disaggregated by the different sectors, when controlling for the following pre-determined covariates: total population, share of the population aged 0–6, share of the population aged 7–15, share of the population aged 80+ and share of foreign-born citizens. For all personnel categories, the regressions are estimated with the AIC-preferred polynomial in the respective baseline specification (cf. Table 3). Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

*Data source:* Statistics Sweden & The SALAR.

able extent by the inclusion of the additional covariates, neither in terms of point estimates nor significance levels (although the standard errors do indeed decrease somewhat). We take this as evidence that the assumption of smooth marginal effects of out-migration on personnel is valid, and that the control function of out-migration captures all other relevant direct effects. This, in turn, means that our econometric specification is likely to estimate a causal effect of grants. Thus, the main conclusion from Tables 3 and 4 remains; grants do not stimulate local public employment, since the only significant and positive effect is that on the relatively small personnel category of administrators.[31]

# 5 Effects of grants on outsourced personnel

As described above, the vast majority of employees in areas traditionally dominated by public providers were still employed by a municipality by the end of our sample period and, for our purposes, the personnel categories above are therefore the most important to study. However, the increasing trend of privatization and outsourcing that took off in the 1990s must also be addressed.

In Figure 7 we do this by plotting analogous graphs to those in Figure 5, but with private personnel instead of public/municipal personnel on the y-axis (for the sectors on which we have data). Recall from the previous figure that it complemented the econometric results quite nicely, in the sense that only the graph for administrative personnel displayed a kink corresponding to the kink in the out-migration grant. By the same reasoning, the conclusion from these graphs is that there are no effects of grants on private employment neither in child care, schools, elderly care nor in social welfare—i.e., the same zero result as that found for *public* employment in these sectors.

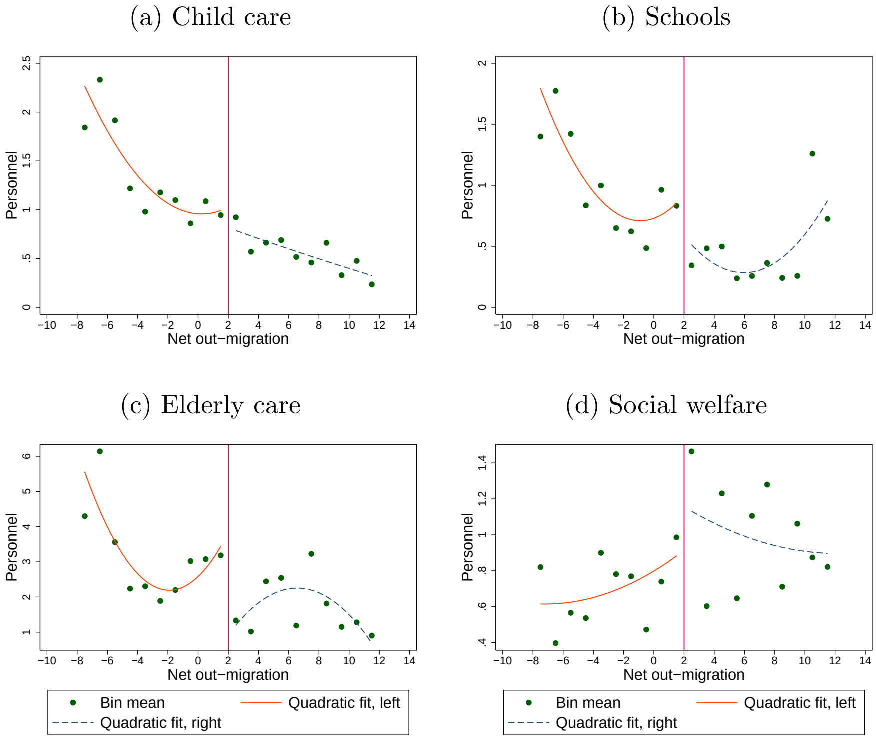
Except for schools, the data underlying Figure 7 only covers the last three years of our study, leaving us with a small sample. The econometric estimates of these effects are therefore somewhat noisy, but by and large confirm the graphical picture:[32] A few of the estimated effects for child care and schools are positive and significant, but are highly sensitive to the choice of bandwidth. And for schools, we find no effects when estimating

[31] A potential alternative robustness check is to not only include pre-determined time-varying covariates, but also municipality fixed-effects. Doing this, the main results still hold but are less stable across the different bandwidths and order of polynomial (the results are available upon request). This is most likely due to lack of sufficient within-municipality variation in out-migration rates for stable estimates across specifications.

[32] For example, the significance of the first stage is sensitive to the order of polynomial and bandwidth and, therefore, we only estimate the second stage using the three widest bandwidths while controlling for out-migration linearly.

on the full sample period 1996–2004. These results are available in a supplementary appendix, as are the main results on *public* employment using this shorter panel covering years 2002–04.[33]

Figure 7: Personnel in the local public welfare sector employed by non-profit or for-profit private firms against net out-migration




*Note:* Personnel is measured in number of employed per 1,000 capita.

*Data source:* Statistics Sweden.

# 6 Do municipalities employ administrative assistants or higher officials?

A robust result found in Section 4 is that, following a grant increase, more administrative personnel are employed. In this section we will examine whether municipalities employ administrative assistants or higher officials.

The pool of bureaucratic personnel in the 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

[33]Except for a few of the negative estimates on, e.g., child care and schools becoming statistically significant, the main results do not change with the shorter panel. In particular, the positive effects on administrative personnel is very robust.

Table 6: Effects of grants on different types of bureaucrats (2SLS estimates)

| | *Full sample* | $h = 15$ | $h = 10$ | $h = 5$ |
|---|---|---|---|---|
| Administrative assistants | 0.0189*** | 0.0184*** | 0.0225*** | 0.0313* |
| | (0.00321) | (0.00409) | (0.00492) | (0.0182) |
| High administrative officials | 0.00228 | 0.00755 | -0.000381 | 0.00196 |
| | (0.00292) | (0.00868) | (0.00419) | (0.0137) |
| Observations | 2511 | 2346 | 2047 | 1241 |

*Note:* For different bandwidths, $h$, the table reports estimates of $\beta_1$ in the second-stage equation (4), with the dependent variables administrative assistant personnel (containing 35 missing values) and high administrative officials. For both types of administrative personnel, the regressions are estimated with the respective AIC-preferred polynomial among $\bar{p} = \{1, 2, 3\}$. Standard errors clustered on municipality are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent level, respectively.

*Data source:* The SALAR & The Institute for Evaluation of Labour Market and Education Policy.

and heads of local public authorities. With 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 and polynomials, with dependent variables now being administrative assistant personnel and high administrative officials, respectively. High administrative officials are defined as top- and mid-managers of local public institutions and authorities, while administrative assistants are either secretaries and clerks, or administrators handling more qualified tasks and investigations. With these definitions, the two subcategories are roughly equal in size.[34] The resulting $\beta_1$ estimates are shown in Table 6 (for the full sample and for $h = 15$, the regressions are estimated with the AIC-preferred polynomial among $\bar{p} = \{1, 2, 3\}$, and for $h = 10$ and $h = 5$ with $\bar{p} = 1$)[35]. These results clearly show that increases in employment take place among the administrative assistants, for which the estimated effect is around 0.02, meaning that a per capita grant increase of 100 SEK leads to an employment increase of around 0.02 full-time equivalents per 1,000 capita. This increase corresponds to an elasticity with respect to grants of as high as 0.19 for 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.

[34] Specifically, the mean (standard deviation) of high administrative officials and administrative assistants is 2.21 (0.93) and 1.58 (0.81) full-time equivalents per 1,000 capita, respectively.

[35] Of the four sets of three regressions with $\bar{p} = \{1, 2, 3\}$ underlying Table 6, $\bar{p} = 1$ is preferred in three regressions and $\bar{p} = 3$ is preferred in one regression.

These asymmetric results—i.e., positive effects of grants only on administrative assistants—raise the question of what distinguishes administrative personnel from personnel in the other sectors. In line with the hypothesis and results in Dahlberg and Mörk (2006), one possibility is 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.[36] But why would bureaucrats wish to employ more fellow bureaucrats and, specifically, more administrative assistants? There is, at least, a couple of possible reasons for this. First, a large number 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, high officials can reduce their own workload. Second, having a larger number of assisting personnel increases the number of subordinates, which could give higher officials a sense of increased power.

Another, more optimistic, possible explanation for the asymmetric results is that employing more administrative personnel actually improves efficiency. Such improvement would be possible if, in the absence of a grant increase, other personnel are occupied with administrative duties for which they are overqualified due to a lack of enough resources to hire administrative assistants.

# 7 Concluding remarks

Public employment—and local public employment in particular—plays an important role in most countries. One of the main instruments with which central decision-makers can affect local public employment is grants to lower-level governments. In this paper, by applying the regression kink design to the Swedish grant system, we 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 (the assignment variable) and personnel (the outcome) indeed seems to hold. Therefore, we can be quite confident that the findings can be given a causal interpretation.

Our robust conclusion is that grants do *not* stimulate local public em-

[36]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). The fact that bureaucrats are of importance for the political decision-making process as well as in the implementation phase has also been recognized for a long time in the political science literature; see, e.g., Peters (1995), Wilson (1989) and Lipsky (1980).

ployment. We find no statistically significant effects on total local public employment, and the estimates are small and precise enough to exclude even moderate effects. When disaggregating the total effect by sector, we find that personnel in the traditional welfare sectors (e.g., child care, schools, elderly care and social welfare) are unaffected. Neither do these grants appear to be used for financing of employment in private firms operating in these sectors. The only positive and statistically significant effect of grants is that on administrative personnel, where the size of the effect is moderate to large. And when estimating 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.

The answer to the question posed in the title of the paper is hence rather negative: giving general grants to lower-level governments does not seem to be an effective way of stimulating local public employment. Since this answer may be interesting (but probably disappointing) for policy makers, it is important to consider to what extent it can be generalized. We specifically want to stress three things concerning the setting in the paper: First, 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 municipalities with diminishing population. Risk-averse decision-makers in these municipalities (be they politicians, bureaucrats, or both) might be reluctant to employ more personnel in sectors such as child care and schools, which are likely to be sensitive to demographic changes and hence where labor demand is very volatile and uncertain.

Second, the years studied constitute a quite prosperous time period, and it is possible that effects of grants are larger in times of economic recessions. And finally, the type of grants studied is distributed with no strings attached, meaning that they can be spent freely. Public funds *targeted* at stimulating employment might have larger effects.

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