Dynamic Commitment and the Soft Budget Constraint: An Empirical Test*

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Abstract

This paper develops an empirical framework for the problem of soft budgets which is explicitly based on a dynamic commitment problem, i.e., the inability of a supporting organization to commit itself not to extend more resources *ex post* to a budget-constrained organization than it was prepared to provide *ex ante*. Swedish local governments are used as a testing ground since the central government provided 1,408 *discretionary* fiscal transfers to local governments during the period 1979 to 1992. The estimated soft-budget effect is economically significant: on average, a local government increases its debt by more than 20 percent, or about 3.5 percent of total personal income, by going from a hard to a soft budget constraint.

Key words: Dynamic commitment, dynamic policy problems, credibility, rational expectations, intergovernmental fiscal transfers, instrumental variable strategies

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1. Introduction

When there is a vertical hierarchy of decisions makers, as with central and local governments, the principal's lack of commitment may impose a "soft-budget constraint" on the agents.[1] Soft budgets occur when the agents have the expectations that the principal will come to their rescue when they are in financial trouble. When such aid is expected, the agents' incentives to behave fiscally responsible are muted. Although the soft budget constraint is considered to be an important incentive problem,[2] the empirical evidence supporting it is, however, inconclusive.[3] This is partly because the soft budget issue is a challenging empirical identification problem but also partly because of the lack of relevant data to make a convincing test of the theory.

The contribution of this paper is therefore to provide a compelling empirical test of soft budgets. To this end, the empirical approach is based on theories that argue that the soft budget constraint is caused by a dynamic commitment problem (e.g., Kornai, Maskin and Roland 2003). Moreover, local governments in Sweden are used as a testing ground since the central government provided a total of 1,408 *discretionary* fiscal transfers to local governments during the period 1979 to 1992. That these fiscal transfers are discretionary rather than formula based is crucial for the identification of soft budgets, as further discussed below.

Kornai *et al.* (2003) model the soft budget constraint as a credibility problem, i.e., the inability of a supporting organization (e.g., a central government) to commit itself not to extend more resources *ex post* to a budget-constrained organization (e.g., a local government) than it was prepared to provide *ex ante*. This means that the *current* economic behavior of the budget-constrained organization will depend on its *expectations* of being supported in the *future*. Consequently, to estimate the effect of soft budget constraints (henceforth the soft-budget effect) on some economic outcome of interest, one has to measure the expectations of being helped in the future and link those expectations to the current behavior of the budget-constrained organization.

[1] The term "soft budget constraint" was first introduced by Kornai (1979).

[2] The soft budget constraint currently constitutes a central policy issue in discussions related to the loan programs of the International Monetary Fund (Rogoff 2002 and Lane and Phillips 2001), in transition economies (Maskin and Xu 2001 and Djankov and Murrell 2002), and in the debate about fiscal decentralization versus centralization in the fiscal federalism literature (Rodden and Eskeland 2003 and Oates 2006). The problem of soft budgets has also generated interests among formal theorists (Kornai, Maskin and Roland 2003).

[3] For discussions on empirical evidence of a soft budget, see Rogoff (2002), Kornai, Maskin and Roland (2003) and Djankov and Murell (2002).

If expectations are formed rationally, the soft-budget effect can be estimated using an instrumental variable or a two-stage least square approach. In the instrumental variable approach, the unobserved expectations are replaced by an indicator variable for whether a local government actually received a *discretionary* fiscal transfer in the future period. It is then argued in the paper that the share of neighboring jurisdictions (i.e. jurisdictions sharing the same borders) receiving discretionary fiscal transfers in the current period will be a valid instrument for the likelihood of a local government receiving a future fiscal transfer in the Swedish context since (i) the central government has a strong incentive to equalize the capacity of local governments to provide a certain level of public services and, for historical reasons, the fiscal equalization scheme has to a large extent been determined by geographical or regional considerations; (ii) the information about fiscal transfers to geographical neighboring jurisdictions will most likely be part of a local government's information set and (iii) the instrument is plausibly unrelated to any other type of factor that may affect the outcome of a local government.[4]

I argue that it is necessary to use an instrumental variable approach to empirically identify a dynamic commitment problem since the credibility problem is based on *rational, forward-looking* behavior and *discretionary* policy (Kydland and Prescott 1977). Thus, an estimating framework that relies on information about discretionary policy and that is consistent with both forward-looking behavior and rational expectations must be used. The instrumental variable approach takes into account forward-looking behavior since the *ex-post* policy intervention is used as a proxy for the *ex-ante* expectations of being helped in the future. The instrumental variable approach is also consistent with the weak form of rational expectations, i.e., agents make optimal use of whatever information they have to form their expectations.[5]

However, it is not enough to use an instrumental variable approach for a credible identification of soft budgets. The financial rescues made by the supporting organization must also be discretionary since the essence of the dynamic commitment problem or the credibility problem is that the supporting organization cannot precommit to a policy rule. As a result, a

[4] A rationale for this type of instrument is provided by Kornai *et al.* (2003) who note that "The more frequently financial problems elicit support in some part of the economy, the more organizations in that part of the economy will count on getting support themselves."

[5] There are other methods for estimating rational expectations models, such as a two-step estimation or a full system estimation. However, the IV approach is clearly the most robust method since it only requires an exclusion restriction while the other approaches also need the other equation in the system − the behavioral equation of the supporting organization − to be correctly specified.

formula based intergovernmental grants scheme cannot be used for obvious reasons.[6] Moreover, for a similar reason, it also seems questionable that some sort of "natural experiment" (e.g., a non-linearity in the intergovernmental grant system) could be used as an instrumental variable, since a natural experiment typically relies on some exogenous event (e.g., a non-discretionary policy change) to provide the source of variation in the variable of interest. Thus, there will necessarily be more uncertainty about the validity of the exclusion restriction in this type of instrumental variable analysis since discretionary policy choices will provide the sources of variations in the expectation of receiving help in the future.

The assumptions of the instrumental variable approach will therefore be extensively discussed in the paper and a number of tests will be performed in order to assess the plausibility of instrument validity. To begin with, the instrument – the fraction of contiguous jurisdictions receiving discretionary grants today – is highly correlated with an *ex-post* future fiscal transfer, which shows that the instrument is relevant for explaining central government behavior. Moreover, the estimated soft-budget effect is little affected when a very rich set of control variables, intended to capture economic shocks to neighbors and other confounders, is added to the instrumental variable specification, which lends credibility to the exogeneity of the instrument as discussed by Altonji, Elder, and Taber (2005a, b). Furthermore, a test of overidentifying restrictions where the lagged instrument provides the additional instrument also supports the exogeneity of the instrument. Finally, a refutability test might be the most convincing test.[7] The idea behind this test is that causal mechanisms – i.e. expectations of receiving financial support in the future – should be absent for a particular sample of local governments, namely those jurisdictions which have received no (or perhaps a few) discretionary intergovernmental transfers during the period of study. The expectations of a future fiscal transfer should *a priori* be close to zero in these sets of jurisdictions. In other words, if the instrument is valid, the reduced form effect of the instrument on the outcome of local governments in this sample should be zero unless there are some other factors (e.g. spatially correlated shocks) rather than expectations that are responsible for the correlation between the instrument and the outcome. Fortunately, the estimated reduced form effect is close to zero which once more lends credibility to the validity of the instrument.

Other explanations for the estimated soft budget effect have been proposed. For example, rational local governments anticipating future grants revenues should borrow more so as to hold local tax rates constant as they spend more in anticipation of future wealth.

[6] There could, of course, be changes in a policy rule which would then make it discretionary.
[7] See Angrist and Kreuger (1999) for discussions about refutability tests.

However, given the large number of control variables and results from the refutability test, it seems unlikely that some unmeasured *temporary* revenue or spending shock is responsible for the soft budget effect. Moreover, a "tax smoothing" hypothesis cannot easily explain the fact that those who received the highest numbers of discretionary transfers during the period 1979 to 1992 were later bailed out by the central government. Namely, in 1999 the Swedish central government established a temporary committee that was to provide additional financial resources (bailouts) to local governments that claimed that they had severe financial problems (e.g., a high level of debt) after the end of the discretionary grant program in 1992. Those local governments which received the highest numbers of discretionary transfers also had the largest accumulation of debt during the period 1979 to 1998. On the other hand, these two facts are consistent with a dynamic commitment problem since the central government's "no bailout" policy after 1992 was not credible and that some local governments still expected to receive additional grants in case they were to run into financial problems. In other words, local governments that receive bailout outs repeatedly also have the highest expectations of getting additional grants in the future and, as a result, worse fiscal policy outcomes**.**

The estimated soft-budget effect is economically very significant: a local government increases its debt by more than 20 percent, or about 3-4 percent of the average total personal income, if it is certain of receiving support in the future as compared to when it is certain of not receiving support in the future. In other words, debt increases by 20 percent in going from a hard to a soft budget constraint.

This paper is related to a number of literatures apart from those mentioned in the first paragraph of the introduction. It is related to the literature analyzing dynamic policy problems in macroeconomics,[8] the literature dealing with the estimation of rational expectations models,[9] and the literature on intergovernmental grants.[10]

The rest of the paper is structured as follows. Section 2 develops the empirical framework and section 3 discusses the data. Section 4 presents the results, while section 5 concludes the paper.

2. Empirical framework

In this section, I discuss the assumptions required to empirically estimate the effect of soft budgets on the behavior of a budget-constrained organization in a way that is consistent with

[8] For an overview of the literature on dynamic policy problems see, e.g., Persson and Tabellini (2000).
[9] For discussions related to the estimation of rational expectations models see, e.g., Hansen and Singleton (1982), McCallum (1976a, 1976b) and Pagan (1984, 1986).
[10] For an overview of the intergovernmental grants literature see, e.g., Smart (2007).

the inability of the supporting organization to make dynamic commitments. These issues will be discussed in the specific context of the relationship between central and sub-national governments but the framework should also apply more generally.

Treating the soft budget constraint as a dynamic commitment problem implies that local government *i*'s behavior in the current period *t*, Y_{it}, depends on whether it will receive financial support from the central government in the future. Since the local government does not know whether it will receive a fiscal transfer, it must form expectations about the probability of receiving a financial transfer in the future. Let P_{it+1} denote the *ex ante* expectations (i.e., these expectations are formed in period *t*) of getting a fiscal transfer *ex-post* (i.e., in period *t*+1). We can now express the problem of the soft budget constraint as the following structural relationship

$$Y_{it} = \alpha + \beta P_{it+1} + v_{it}, \tag{1}$$

where v_{it} denotes *all* other determinants (observed or unobserved) of local government *i*'s outcome in period *t*. The parameter of interest is β – the soft-budget effect – which measures the effect on the outcome of going from a zero probability to a probability of one of getting a future transfer, that is, equation (1) should be interpreted in counterfactual terms, namely for a given v_{it}, it gives the optimal local government response for *any* possible degree of expectations. In other words, β measures the impact of going from a hard to a soft budget constraint.

The key issue is now how equation (1) should be estimated since P_{it+1} is unobserved (at least to the econometrician). It seems obvious to require that expectations are rationally formed, otherwise the empirical approach would not be consistent with a dynamic commitment problem as discussed in the introduction. Thus, we assume that the local government forms expectation according to

$$P_{it+1} = E[T_{it+1} \mid \Omega_{it}] = Prob\{T_{it+1}=1|\Omega_{it}\},$$

where T_{it+1} is an indicator variable taking the value of 1 if the local government receives a discretionary fiscal transfer in period *t*+1, and zero otherwise, and Ω_{it} is the local government's information set at time *t*.

There are a number of different estimation methods for rational expectations models but the most robust approach is to use an instrumental variable method as previously noted.[11] In the instrumental variable approach, the unobserved expectations P_{it+1} in equation (1) are

[11] There are also full system methods but they require at least as many assumption as the two-step method. See Pagan (1984, 1986) and Murphy and Topel (1986) for a discussion of the two-step method. See McCallum (1976a, b) or Cumby, Huizinga and Obstfeld (1983) for discussions about the instrumental variable approach.

replaced by T_{it+1}. Parameter β can now be consistently estimated with an IV approach, where the instrumental variable must be relevant and exogenous, i.e., unrelated both to the prediction error, $\varepsilon_{it+1}=T_{it+1}-P_{it+1}$, and the structural error v_{it}.

I argue that that the share of neighboring jurisdictions (i.e., jurisdictions sharing the same borders) receiving discretionary fiscal transfers in the current period, $\overline{T}_{-it}$,[12] fulfills the requirements for instrument validity for the following three reasons.

First, the Swedish central government has a strong incentive to equalize the capacity of local governments to provide a certain level of public services and, for historical reasons, the fiscal equalization scheme has to a large extent been determined by geographical considerations, as will be further discussed below. Thus, the share of neighboring jurisdictions receiving discretionary fiscal transfers in the current period will therefore be correlated with T_{it+1}. Second, the information about fiscal transfers to geographical neighbors is most likely to be in a local government's information set, since they share the same local media market. As a result, the prediction error ε_{it+1} is likely to be uncorrelated with $\overline{T}_{-it}$. Third, the instrument is also plausibly uncorrelated with the structural error term, v_{it}, since the instrument is neither a characteristic of the local government nor a characteristic of the neighboring jurisdictions.[13]

The last two assumptions regarding exogeneity of the instrumental variable may need some more clarifications. The requirement that the prediction error must be orthogonal to the elements of the information set implies that the instrumental variable must be part of the local government information set. However, if a local government does use more information to predict its future likelihood of a fiscal transfer, the IV estimate will still be consistent. In other words, to avoid an inconsistent estimate, the instrumental variable should preferably be based on information that is most likely to be in a local government's information set, as discussed by McCallum (1976a).[14] Nonetheless, geographical neighbors may face common shocks which could make the instrumental variable correlated with the structural error term v_{it}. Thus, when considering instrument validity in the context of the dynamic commitment problem,

[12] A Swedish local government on average has about six neighboring jurisdictions. Thus, if three neighbors receive fiscal transfers in period *t* then $\overline{T}_{-it}$=3/6=0.5, if none receives transfers then $\overline{T}_{-it}$=0, and if all receive transfers then $\overline{T}_{-it}$=1.

[13] The literature on strategic interactions among governments typically uses characteristics of neighbors as instrumental variables (e.g., Besley and Case 1995 and Brueckner 2003). These types of instruments have been criticized for not being valid, however.

[14] McCallum (1976a) discusses the instrumental variables assumptions in the rational expectations context. He argues that one should "limit the *Z* [the instrumental variables] to those variables that are 'most likely' to be considered by market participants [the budget-constrained organization]" in order to avoid inconsistent estimates.

there is a trade-off between these two assumptions for instrument exogeneity. To take a striking example, suppose that one were to construct the instrumental variable based on whether the local government received a transfer in the last period, T_{it-1}. This instrument will certainly be part of the information set but is also likely to be correlated with municipality-specific economic shocks. Put differently, the problem with this instrument is that the central government acts like a doctor – it goes in where there are current problems, and these problems may persist for more than one period.

To address whether the proposed instrument is likely to be exogenous, a rich set of controls that are intended to capture both municipality-specific economic shocks and spatially correlated shocks or other confounders will be included in the specification. The idea is that if the estimate of a soft budget effect is largely unaffected by the inclusion of the control variables, then it is more likely that the instrument fulfills the exogeneity assumption as discussed by Altonji, Elder and Taber (2005a, b).[15] Specifically, the following instrumental variable specification will be the most general:

$$Y_{it} = \alpha + \beta T_{it+1} + \pi_1 Y_{it-1} + \bar{Y}_{-it} + \pi_3 T_{it} + \pi_4 X_{it} + \pi_5 \bar{X}_{-it} + \mu_i + \lambda_t + r_{it}, \quad (2)$$

where T_{it+1} will be instrumented by $\bar{T}_{-it}$, Y_{it-1} is the lagged outcome for government i, $\bar{Y}_{-it}$ is the mean outcome for geographical neighbors in the current period t, T_{it} is an indicator of whether the locality received a fiscal transfer in the current period t, X_{it} is a vector of local government characteristics in period t, and $\bar{X}_{-it}$ is a vector of characteristics of neighboring jurisdictions in period t. The two vectors of covariates, X_{it} and $\bar{X}_{-it}$, will include the most common/important control variables used in the local public finance literature (e.g., population size, age structure, income, unemployment, political factors, and rule-based intergovernmental grants). The specification will also include fixed local government μ_i and time fixed effects λ_t. Arguably, including these rich set of controls thus makes specification (2) a powerful test of the soft budget constraint hypothesis since these controls should be able to capture the most important confounders. For example, the outcome of geographical neighbors $\bar{Y}_{-it}$ and their characteristics $\bar{X}_{-it}$ should be related to shocks to contiguous jurisdictions, while lagged outcome Y_{it-1}, discretionary fiscal transfers in the current period t, T_{it}, and own characteristics X_{it} should be related to municipality-specific economic shocks but

[15] Even if the point estimate is sensitive, the instrument might still be unrelated to the structural error term, that is, instruments may require conditioning on a set of covariates to be valid, as discussed by Angrist and Imbens (1995) and Abadie (2003).

possibly also to common economic shocks. The estimated soft-budget effect would be sensitive if the instrument were correlated with any of these factors.

It is important to stress that the control variables in specification (2) will typically not have a causal interpretation. Indeed, the inclusion of the fixed effects μ_i and the lagged dependent variable Y_{it-1} may lead to a biased estimate of π_1, unless the number of time periods is sufficiently large. However, this does not affect the consistency of the soft budget effect since the error term is allowed be correlated with the control variables under the assumption of conditional exogeneity of the instrument (e.g., Angrist Imbens 1995 and Abadie 2003).

It is also noteworthy that including the lagged outcome means that the dependent variable could equally be expressed as the change in the dependent variable, $\Delta Y_{it} = Y_{it} - Y_{it-1}$, conditional on including Y_{it-1} as a control variable. This means, for example, that if the level of debt is used as the outcome of interest and lagged debt is included among the covariates, this regression is equivalent to a specification that controls for past level of debt but instead uses the change in debt as the dependent variable. Thus the dependent variable is now a measure of the budget deficit.

Finally, it is also important to note that the instrument – the share of neighboring jurisdictions receiving discretionary fiscal transfers – is quite distinct from the instruments used in the literature on strategic interactions among subnational governments. This literature often uses neighbor characteristics $\overline{X}_{-it}$ as instruments.[16] Here, these variables are instead used as controls for confounding factors shared by contiguous jurisdictions.

3. Swedish local governments

In this section, I describe the intergovernmental fiscal transfers system that was in place in Sweden until 1992 and how it can be used, together with the empirical framework developed in the previous section, to estimate the effect of the soft budgets. I will also describe the data in detail but before turning to the description, it might be helpful to digress briefly on the workings of Swedish local governments.

As of 2009, there are 290 local governments in Sweden which cover the entire country. Local governments play a very significant role in the Swedish economy. They are, for example, responsible for the provision of day care, education, care of the elderly, and social welfare services, and their share of spending out of GDP is about 20 percent and they employ roughly 20 percent of the total Swedish workforce. Swedish local governments also

[16] See, e.g., Besley and Case (1995) and Brueckner (2003) for discussions of the use of the characteristics of neighbors as instrumental variables.

have the constitutional right of self-government, no restrictions on borrowing, and no balanced budget rules.[17] Moreover, on average, 20 percent of their income are from intergovernmental grants, whereas the rest mostly comes from a proportional income tax, which can be set freely by each municipality. In other words, they have a relatively large degree of fiscal freedom.

3.1 Intergovernmental fiscal transfers

Turning to a discussion of the Swedish intergovernmental fiscal transfers system,[18] a very significant goal of the Swedish central government is that individuals should have similar levels of public services, independent of where in the country they live. Since most of the public services such as education, child care, and care of the elderly, are provided by the local governments, the central government has a strong incentive to equalize the capacity of local governments to provide a certain level of public services.

To this end, a general intergovernmental equalization system was established in 1966 but various types of intergovernmental grants have existed since 1917. The equalization scheme during the period of study consisted of two parts: one based on formula and one based on discretion, i.e., yearly decisions made by the central government

As previously discussed, the rule-based intergovernmental grants cannot be used to estimate the effect of the soft budgets as caused by a dynamic commitment problem since they are not determined by discretionary choices of the central government. Nonetheless, they still provide an illustration of the Swedish central government being strongly motivated by geographical or regional concerns in the distribution of grants. The rule-based income equalization grant was distributed according to following formula

$$\text{Per capita grants} = t(\theta\bar{y} - y),$$

where t is the local income tax rate, θ is a parameter set by the central government, y is the per capita income and $\bar{y}$ is the national average of the per capita income. In 1980, for example, Sweden was divided into 12 regions where each region had a specific value of parameter θ. The lowest value was 103 percent and the highest 136 percent. The local governments in the very northern part of Sweden had the highest value of θ, since they were considered to be the local governments with most need for additional resources, which was partly due to the harsh climate and the sparsely populated areas. In other words, parameter θ in the intergovernmental

[17] As from the year 2000, there exists a weak form of a balanced budget rule.
[18] See, e.g., Smart (2007) for a discussion of intergovernmental grants.

grants formula reflects the very strong regional policy concerns of the Swedish central government.

To be able to test that a soft budget constraint is caused by a dynamic commitment problem, the fact that the central government was empowered by law to distribute *discretionary* fiscal transfers to local governments will be exploited.[19] One of the motivations for introducing the discretionary grants was that formula based equalization systems did not compensate enough for economic disparities between local governments. However, as will become clear below, these discretionary transfers were causing rather than solving economic problems.

Local governments could apply for the discretionary grants before the end of March each year.[20] The central government then made its decisions during the fall in the same year and the grants were paid out during the subsequent year.[21] Roughly 25 to 60 percent of the applicants received grants in each year.[22] Local governments typically claimed that they had severe financial problems and that they would therefore be unable to fulfill their responsibilities without additional financial resources. They also claimed that the economic problems were due to factors they could not affect, such as high unemployment, an unbalanced age structure, and deteriorating income tax bases. The local governments rarely applied for a specific size of the grant but instead asked for the highest possible amount of grants.

There is little or no information on the specific criteria used by the central government in its decisions. Indeed, there was a great deal of criticism that the decision process was not at all transparent. After reading the Finance Committee Reports, it seems that the main criterion for approving temporary grants application was the general economic status of the local government but the meaning of economic condition is not explained.

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[19] The following laws gave the central government the right to distribute discretionary fiscal transfers: SFS 1973:433, SFS 1979:362 and SFS 1988:491.

[20] This date applies to the period 1980-1992. For the year 1974, municipalities had to apply before June 30th, and for the period 1975-1979 they had to apply before January 31st.

[21] In some cases, the local governments could also get discretionary grants without directly having to apply for them. In the data, however, I am unable to identify whether the financial relief grants were initiated by the central government or the local governments. Therefore, I am forced to treat the whole financial relief program as being informative about the dynamic commitment problem. However, this seems to be the correct procedure since it is the expectations of local governments to be rescued in case they get into trouble that constitute the core of the credibility problem and therefore, all fiscal transfers from this program should contain valuable information about such expectations.

[22] We have information on the numbers of applicants for the financial relief grants for three years: In 1982, 125 municipalities applied for, but only 51 received grants, in 1985, 123 municipalities applied for, but only 51 received grants, and in 1988, 119 municipalities applied for, but only 41 received grants.

Figure 1 shows the total number of local governments receiving transfers each year during the period 1979 to 1992.[23] The total number of transfers was 1,408. There is quite a large variation in the number of recipients across years, the average number of local governments receiving transfers in a particular year is 90, but the minimum is 28 and the maximum is 173, which underscores the discretionary feature of the program.

Figure 2 shows the distribution of fiscal transfers across local governments. This figure shows that slightly more than 30 jurisdictions received no transfers during the period 1979 to 1992 while 10 received 14 transfers, i.e., the maximum possible number of grants. The average jurisdiction received 5 discretionary grants. Thus, there is a very large variation in the number of discretionary transfers across local governments.

Figure 3 shows a map of Swedish local governments and how the total number of discretionary grants received is geographically distributed during the years 1979 to 1992. This map clearly shows that these grants are concentrated to specific regions. Specifically, local governments in the northern part of Sweden receive more discretionary transfers than those in the southern part.

Figure 4 shows the distribution of the instrument – the share of neighboring jurisdictions receiving discretionary fiscal transfers. It shows that in 30 percent of the total number of observations (3657), none of the neighbors received any transfers while in more than 10 percent, all neighbors received grants.

Turning to a discussion on the instrumental variable – there is a number of reasons for this particular choice. First, it seems that the central government distributes the discretionary grants based on regional policy concerns, as illustrated in Figure 3. Second, fiscal transfers of the very closest geographical neighbors are likely to be part of the information set of a Swedish local government, since geographical neighbors typically share the same local media market. Most of the daily newspapers in Sweden are highly local in nature (e.g., they typically focus on local politics) while a few papers (e.g., *Dagens Nyheter*) have a broader scope but very limited coverage outside Stockholm (the capital). This suggests that a local government's information set is to a large extent geographically based. For example, in 1986, there existed 161 newspapers (Sweden had 284 local governments at that time) with a total circulation of 3.2 million. A typical Swedish newspaper in 1986 covered about 5.8 jurisdictions and its

[23] The reason for choosing the period 1979 to 1992 is that local governments do not face any borrowing restrictions after 1977 and that the discretionary fiscal transfer program ended in 1992.

coverage was 30.3 percent within a jurisdiction.[24] These numbers clearly illustrate that the Swedish media market was very localized. Thus, it seems quite reasonable to assume that the instrument – the share of neighboring jurisdictions receiving discretionary fiscal transfers – is part of the information of a local government. Once again, it is important to note that the estimated soft budget effect is consistent even though a local government may base its expectations about receiving future fiscal transfers on more information than from adjacent jurisdictions, as previously discussed. On the other hand, if they do not have any information about fiscal transfers to neighboring jurisdictions other than those sharing the same borders, the estimated soft budget effect would be biased if the instrumental variable were to be constructed based on a more generous definition of neighbors.

Another issue when using the empirical framework developed in the previous section is whether the information about sizes of grants also can be used. Figure 5 shows the per capita distribution of grants. The mean is 176 (median 104) with a standard deviation of 230. Thus the per capita amount of the grant is rather dispersed. However, as currently stated, equation 2 does not take into account that the soft-budget effect may vary depending on the expected size of future grants. One way of *non-parametrically* testing whether the soft-budget effect depends on the expected size of future grants is to divide the size of future grants, $Grants_{it+1}$, into different groups using a set of dummy or indicator variables. For example, we can define three different "treatment" groups: one group which has received a "small" future grant, $1[0<Grants_{it+1}<56]$, one group which has revived a "medium" sized future grant, $1[56<Grants_{it+1}<171]$, and a group that has received a "large" future grant, $1[Grants_{it+1}>171]$. [25] The next step is then to replace βT_{it+1} in equation (2) with

(3) $\beta_{small}1[0<Grants_{it+1}<56] + \beta_{medium}1[56<Grants_{it+1}<171] + \beta_{large}1[Grants_{it+1}>171]$.

It is now possible to estimate this modified regression using an instrumental variables approach where the set of instruments is now interactions between the indicator variables and the instrument $\overline{T}_{-it}$. As a result, this regression provides three different estimates of the soft-budget effect without imposing any functional form restrictions: one estimate for a small future grant, one for a medium sized future grant, and one for a large future grant. Thus, with this equation one can test for whether the soft-budget effect differs depending on the expected future size of grants, i.e., $\beta_{small} = \beta_{medium} = \beta_{large}$. As will be shown below, I cannot reject that

[24] I thank Helena Svaleryd for providing the data on daily newspapers used to calculate these numbers. For information about the Swedish media market today, see http://www.dagspress.se/.
[25] To construct the treatment groups, I split the data into three groups of equal sizes.

these three estimates are the same. This finding is perhaps not surprising given the fact that most local governments did not apply for a specific size of the grant as noted above. Since the soft-budget effect does not depend on the expected value of the future grant, the approach in equation (2) of only using one indicator variable $T_{it+1}=1[Grants_{it+1}>0]$ is therefore the correct and the most efficient approach.[26]

3.2 Data

As the local government outcome of interest, debt measured in per capita terms and at constant prices will be used as the baseline. However, other measures of the fiscal stance will also be used.[27] Debt seems to be a suitable measure of local government fiscal (mis)behavior in Sweden since the local governments have no restrictions on borrowing and did not meet any balanced budget rules under the period of study. This measure includes both short- and long-term debt.[28]

As previously noted, when lagged debt is included among the control variables, the dependent variable is effectively the change in real debt, which is one measure of the budget deficit. Another measure of the budget deficit is the cash or primary deficit; total spending minus total revenues. Below, I will show that the results regarding the estimate of the soft budget effect are very similar notwithstanding if the fiscal stance of the local government is measured as level of debt, change in debt, cash deficit or total spending.

A large number of control variables will be included in the specification (2) as previously noted. First, there will be a number of local government characteristics that are typically used as control variables in the local public finance literature (e.g., Besley and Case 2003). These are population size, per capita income, unemployment rate, proportion of the population below 15 and proportion of the population above 64. The same set of characteristics of the neighboring jurisdictions will also be included as controls. Moreover, a number of political factors will also be controlled for, i.e., an indicator variable for left-wing party control and left-wing vote shares, since these political factors may be related to local government outcomes (e.g., Pettersson-Lidbom 2001, 2008). There is also a literature that argues that intergovernmental grants can be used for political reasons. For example, the

[26] In sharp contrast, if one should impose linearity and replace the indicator variable T_{it+1} with $Grants_{it+1}$, then the result from the IV approach would be completely different. In fact, the estimate of the soft budget effect would even be negative: -0.001125 (s.e. =0.000647). Thus by wrongly imposing linearity, the result would suggest a negative soft-budget effect.

[27] I have used the implicit GDP deflator, expressed in 1991 values. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices.

[28] Long-term debts are defined as debts with a maturity of one year or longer, while short-term debts have a maturity of up to one year.

models developed by Lindbeck and Weibull (1987, 1993) and Dixit and Londregan (1996) show that those jurisdictions with many "swing" voters will get more grants. On the other hand, Cox and McCubbins (1986) and Dixit and Londregan (1998) argue that grants will also be distributed to regions where the central government already has high support.[29] However, there is no *a priori* reason why the instrument – the share of neighboring jurisdictions receiving discretionary fiscal transfers – should be related to either swing voters or core supporters in a specific local government.[30] Nevertheless, the inclusion of party control and vote share should effectively deal with these possibilities, since they will effectively control for the partisanship of government and for elections with a narrow vote margin (a large number of swing voters). The rule-based fiscal equalizations grants will also be included as a control variable. Finally, and perhaps most importantly, a number of controls for economic shocks will be included. These are lagged outcome, geographical neighbor's outcomes, and an indicator for whether the locality has received a discretionary fiscal transfer in the current period. Arguably, these three variables should be highly associated with economic shocks since lagged outcome and the indicator for a fiscal transfer today should capture municipality-specific shocks, while the outcome for neighbors should capture shocks common to the geographical neighboring jurisdictions.

Traditional summary statistics (mean, standard deviation, minimum and maximum values) for the dependent variable, the explanatory variable, the instrumental variable and the control variables are presented in Table 1.

4. Results

In this section, I present the results from estimating the soft-budget effect using the instrumental variable approach discussed in Section 2, i.e., equation 2. The instrumental variable is the fraction of contiguous jurisdictions receiving discretionary grants today and the dependent variable is debt per capita expressed in logarithmic form, which means that the soft-budget effect will have a percentage interpretation. The standard errors are clustered at the local government level and, therefore, robust to any type of serial correlation within clusters and heteroscedasticity of unknown type (e.g., Bertrand et al. 2004).

[29] For Swedish studies, see Dahlberg and Johansson (2002) and Johansson (2003).

[30] I have also explicitly tested for whether the discretionary fiscal transfers are distributed according to political motives. For example, I have estimated the following specification when there has been a left-wing central government: *fiscal transfers*= β_1 + β_2*left-wing local government* + $\beta_3 f$(*vote share for left-wing local government*) + v, and a similar specification when there has been a right-wing central government. If the central government distributes these grants for political reasons, β_2 should differ from zero. However, I find no evidence of political effects when I use this type of regression-discontinuity specifications. These results are available upon request.

Column 1 in Table 2 shows the IV results for a specification without any control variables. The estimated soft-budget effect is 16.3 percent, i.e., on average a local government increases its debt by 16 percent when going from a zero probability to a probability one of receiving a future fiscal transfer. The estimated effect is statistically significantly different from zero at the 1 percent level. Further down the column, I report the *F*-statistics for the power of the instrument in the first-stage equation. The *F*-tests (i.e., 376) indicate that the instrument is a very strong predictor of whether the local government will receive a fiscal transfer in the future.

To investigate whether the instrument is likely to be exogenous, I test whether the point estimate is sensitive to the inclusion of additional control variables (i.e., equation 3). For this test to be useful in practice, the number of controls must be large enough, they must have significant explanatory power, and they must be representative of the full range of factors that determine the outcome as discussed by Altonji *et al.* (2005b).

In Columns 2-7 in Table 2, I present the results from using a large number of control variables that are likely to fulfill these requirements for a convincing test of instrument validity. A number of standard controls for local government characteristics, i.e., population size, proportion of young, proportion of elderly, income, and unemployment is included in Column 2. The estimated soft-budget effect is 22.9 percent which is only somewhat larger than in column 1. Tested jointly, the *F*-test on the set of government characteristics is 63 which suggests that they have significant explanatory power. The instrument is once more highly relevant; the *F*-test (258) once more indicates that the instrument is a very strong predictor of whether the local government will receive a fiscal transfer in the future.

In Column 3, I also include a number of other control variables, in addition to the controls for local government characteristics, which should be related to both municipality-specific shocks and shocks common to geographical neighboring jurisdictions as previously discussed. There are three such control variables: lagged own debt, average debt of neighboring jurisdictions, and a dummy variable for whether a local government has received a discretionary fiscal transfer in the current period. The estimated soft-budget effect is 19.3 percent which is not very different from the previous ones. Jointly tested, the *F*-test on these controls is 6,586, suggesting that they have an enormous explanatory power, i.e., one of the key requirements of Altonji *et al.* (2005b).

In Column 4, local government fixed effects and time fixed effects are added to the specification in Column 3. Thus, the identification is only based on the within local government variation. The estimated soft-budget effect is 23.6 percent which once more

differs little from the previous ones. After "partialling out" the fixed local government effects, the *F*-test on the remaining controls is 3,586, suggesting that they once more have enormous explanatory power.

In Column 5, the average characteristics of neighboring jurisdictions are added to the previous specification in Column 4, i.e., the average population size of neighbors, the average proportion of young of neighbors, the average proportion of elderly of neighbors, the average per capita income of neighbors, and the average unemployment rate of neighbors. The estimated effect is 22.0 percent. Thus, the inclusion of these characteristics of neighboring jurisdictions does not affect the estimated soft-budget effect.

In Column 6, the political characteristics of local governments and the neighboring jurisdictions are added to the specification in Column 5. The political variables include an indicator variable for left-wing party control and left-wing vote share. Thus, a total of four new control variables is added. The estimated soft-budget effect is once more not affected to any large extent since the estimated effect is 23.7 percent.

Finally, the size of the rule-based equalization grants distributed to local governments and the average grants distributed to neighboring jurisdictions are added to the specification in column 7. The estimate is 23.5 percent which is almost identical to the estimate in column 6.[31]

Regarding the issue about weak instruments, the *F*-statistics for the excluded instrument as reported in columns 3 to 7 are between 25.8 and 37.8, which is considerably larger than the rule-of thumb of 10 suggesting that the instrument is not weak.

Another way of testing whether the instrument is likely to be exogenous is to make an overidentfying restriction test, i.e., a *J*-test. To perform such a test, an additional instrument is needed. I will use the lagged instrument as an additional instrument. In Column 8, I present 2TSL results from the same specification as in Column 7 but where two instruments are being used. The estimated effect is 24.8 percent. The *J*-test indicates that the additional instruments can be excluded from the debt equation since it has a *p*-value as low as 0.74. The first stage *F*-statistics for this specification is 15.2, which once more suggests that the instruments are not weak.[32]

[31] Following a suggestion from an anonymous referee, I have also controlled for lagged revenues and lagged spending. The estimated soft-budget effect is hardly affected since the estimated effect is .224 with a standard error of .109.

[32] I have also calculated coverage-corrected confidence intervals following the suggestion of Moreira (2003) and these are not different from those presented in Table 2. Thus, there is no indication that the instrument is weak. Furthermore, I have also estimated specification 8 with the LIML estimator and Fuller's (1977) modified LIML estimator since they might have better properties than the 2SLS estimator if the instruments are weak, as discussed by Andrews and Stock (2005). The point estimate from these alternative estimators is almost identical

Another issue that has not yet been taken into considerations is that the soft-budget effect may depend on the expected size of future grants. Using the extended regression specification discussed in Section 3.1 (i.e., equation 3) makes it possible to investigate this issue. This specification, with the full set of control variables, yields the following results: the estimate for β_{small} is 0.2079 (s.e = 0.1214), the estimate for β_{medium} is 0.2987 (0.2684), and the estimate for β_{large} is 0.2992 (0.1667). Thus, the result shows that these three estimates are similar. Indeed, an F-test of equality of these estimates cannot be rejected (F(2, 282) = 0.64, Prob > F =0.5272). Moreover, these estimates should be compared with the baseline estimate of 0.248 (0.109) in column 8 in Table 2. All three estimates are similar to this estimate but the standard errors are also significantly larger than 0.109. Thus, the soft-budget effect does not depend on the expected value of the future grant. As a result, the approach of only using one indicator variable T_{it+1}=1[$Grants_{it+1}$>0] is therefore the correct and the most efficient approach.

To further probe whether the expected size has an effect on the outcome, I have also added an additional control – the average size of grants received by neighboring jurisdictions in the current period – to the specification in column 8 in Table 2. The idea is that this variable should be a good predictor for the expected future size of grants. The estimate of β is hardly affected by this (0.2195, s.e.=0.0908) and the estimate on the average grant size is not significantly different from zero (0.0000499, s.e.=0.000076). Once more, this finding suggests that local governments do not respond to the size of future grants

As an additional test of the validity of the instrument, I have constructed a refutability test as previously discussed. In this test, the local governments are split into two groups of roughly equal size (i.e., the statistical power of the test should be roughly equal in the two groups) depending on the total number of fiscal transfers they have received during the sample period 1979-1992. One group consists of those local governments that have received at most four fiscal transfers, while the other group consists of those that have received more than four fiscal transfers. The average number of fiscal transfers is 1.4 in the latter group while it is 8.5 in the former group. The idea is that the expectations of future fiscal transfers should be very different in these two groups and that one can therefore construct a refutability test by looking at the reduced form between outcome and instrument. In other words, if the instrument − the share of neighboring jurisdictions receiving transfers today − is valid, one

to that in Column 8. Thus, there is once more no indication that the 2SLS estimator suffers from a weak instrument problem. These results are available upon request.

should find a much larger effect on the outcome in the sample where the probability of receiving a future fiscal transfer is large as compared to the sample with a low probability.

Table 3 shows the results from the refutability test. Column 1 indicates that the instrument has *no* effect (estimate -0.019, t-value -0.38) on the probability of receiving a fiscal transfer in the sample with few fiscal transfers, which thus confirms the a priori reasoning that the expectations of getting a future fiscal transfer should be close to zero in this group of jurisdictions. In sharp contrast, Column 3 shows that the instrument has a large and significant impact (estimate 0.21, *t*-value 4.46) in the sample with many fiscal transfers. Turning the relationship between the instrument and the outcome, Column 2 shows that the reduced form effect is close to zero and not statistically significant in the sample with few fiscal transfers (estimate 0.012, t-value=0.42) while the reduced form effect is large and statistically significant at the 1% level (estimate 0.064, t-value 2.75) in the sample with many fiscal transfers in Column 4.[33] Thus, the fact that the instrument has no effect on the outcome in the sample where the likelihood of a future fiscal transfer cannot be forecasted (i.e., where the expectations of a future fiscal transfer are close to zero) while the instrument has a large and significant impact on the outcome where the expectations of a future fiscal transfer are quite high lends further credibility to the validity of the instrument.

In Table 4, I also check the robustness of the estimated soft-budget effect to other measures of the government's fiscal stance. The following five measures of fiscal stance are being used: (i) the level of debt, (ii) the change in debt, (iii) the cash budget deficit (primary deficit), (iv) total spending and (v) total revenues. For the estimated soft budget effect to be comparable across the different measures of the fiscal stance, they will all be expressed in real per capita terms without taking the logarithm. Nonetheless, the specifications are otherwise identical to the specification in Column 8, which is the most elaborate of all specifications in Table 2.

The result for real debt per capita is displayed in Column 1 in Table 4. The estimated soft-budget effect is 2,305. This effect is about 23 percent of average debt since the average per capita debt is 9,958 for the period 1979 to 1992. Thus, it is noteworthy that the estimated soft budget effect remains basically unaltered if the outcome is expressed in logarithmic form (24.7 % Column 8 in Table 2) or in levels. The estimated soft-budget effects could also be

[33] Alternatively, it is also possible to estimate the reduced form relationship between the instrument and the outcome in the sample of local governments which have received no transfers at all (of course, for this sample the first-stage relationship – the expectations of receiving a future grants – cannot be estimated since no local government has received any grants). For this sample of 31 local governments, the estimated effect is -.035 with a standard error of .071,

expressed in terms of total personal income. Since the average per capita personal income is 72,263, the estimated soft budget effect is 3.2 percent of total personal income.

Turning to the change in debt as a measure of the fiscal stance, the outcome variable could equally have been expressed as the change in debt when the lagged dependent variable is included among the regressors as was previously discussed. Consequently, the estimated soft budget effect in Column 1 in Table 4 is exactly the same as if one were instead to have used the change as the measure of the fiscal stance. For this reason, it could also be interesting to express the dependent variable as the change in debt without controlling for lagged debt. Column 2 in Table 4 shows that the estimated budget effect is 2,666, which is only slightly larger than the estimate of 2,305 in Column 1. Thus, it is noteworthy that the estimated soft-budget in a fixed effect specification is unaffected whether a lagged dependent variable is included or not since it is typically claimed that this leads to biased estimates (e.g., Nickell 1981). While this statement is correct for a pure fixed effect analysis where the number of time periods is small, this claim does not apply to the 2SLS approach since the control variables are allowed to be correlated with the error term under the assumption of conditional exogeneity of the instrument, as was previously discussed.

Another measure of the government's fiscal stance is the cash budget deficit (primary deficit), i.e., the difference between total spending and total revenues. Column 3 in Table 4 shows that the estimated soft budget effect of using the measure is 2,135. This is once more very similar to the baseline estimate in Column 1.

Since the cash deficit can be affected both by spending and revenues, Column 4 in Table 4 shows the estimated soft budget effect for total spending while Column 5 shows it for total revenues. The estimated effect is 2,748 for spending and 613 for revenues, which means that the soft-budget effect is only due to an increase in government spending rather than a reduction in revenues.

Other explanations for the estimated soft-budget effect than a dynamic commitment problem have been proposed, as discussed in the introduction. For example, rational local governments anticipating future grants revenues should borrow more so as to hold local tax rates constant as they spend more in anticipation of future wealth. However, given the large number of control variables and results from the refutability test, it seems unlikely that some unmeasured *temporary* revenue or spending shock is responsible for the soft budget effect.

Nonetheless, one way of ruling out a "tax-smoothing" explanation based on temporary revenue or spending shocks is to analyze the relationship between central and local governments during a long period of time since then local governments should have no

incentive to use deficits as a buffer, i.e., to smooth taxes over time.[34] On the other hand, from a long-term perspective, running deficits on a regular basis and thus accumulating debt may be optimal if a local government expects to be bailed out in the future. Consequently, the starting point of the long-run analysis is to analyze whether some local governments still expect to be bailed out by the central government even after the end of the discretionary transfer program in 1992.

Soon after 1992, a number of local governments claimed that they had severe financial problems and that they would therefore be unable to fulfill their responsibilities without additional financial resources. This suggests that the central government's "no bailout" policy after 1992 was not credible and that some local governments still expected to receive additional resources in case they were to run into financial problems. Indeed, the Swedish central government established a temporary finance committee in 1999 that was to provide additional financial resources or "bailouts" to local governments that had severe financial problems (e.g. a high level of debt).[35]

One way of indirectly testing for a dynamic commitment problem is to relate the decision to apply for additional financial resources in 1999 and the number of grants from the earlier discretionary fiscal transfer program. The rationale for this test is that the expectations of receiving future grants should be highest for those with the largest number of previous transfers.[36] The following regression will be used for this test:

$$D = a + b\,\text{Number of grants} + \text{controls} + v, \tag{3}$$

where D is an indicator variable taking the value of 1 if the local government applied for financial rescue in 1999. As can be seen from estimates in columns 1 in Table 5, there is a strong positive correlation between the total number of discretionary fiscal transfers received in the previous program and the likelihood of applying for additional financial help from the central government in 1999.[37] This correlation therefore suggests that those with the highest expectations of future transfers still expect to be bailed out by the central government even after 1992.

[34] In some sense, any fiscal policy can be rationalized from a tax smoothing perspective, if expectations are a "free" variable as discussed by Alesina and Perotti (1995). Thus, if shocks to revenues or spending are highly persistent, but not permanent, tax smoothing could still explain debt accumulation.
[35] 54 local governments applied to a temporary finance committee "kommundelegationen" and 36 of those received additional grants.
[36] For example, there are 48 out of 276 local governments that have received more than 9 bailouts during the 14-year period 1978 to 1992 (see Figure 2).
[37] This correlation also holds for those 36 that actually received bailouts.

The next step is to analyze to what extent these expectations affected the accumulation of debt. Therefore, I test whether the total number of discretionary government transfers is related to the accumulation of local government debt from 1978 to 1998 using the following specification:

(4) $Log(debt\ per\ capita\ in\ 1998) = c + dNumber\ of\ grants + log(debt\ per\ capita\ in\ 1978) + v.$

Column 2 in Table 5 displays the results from this regression. The estimated effect is 0.033 which means that debt increases by 3.3 percent for each additional discretionary grant received. In other words, a local government that has received the maximum amount of discretionary transfers during the period 1979 to 1992 (14 times) has increased its debt by 46 percent as compared to a local government that has received zero grants.[38]

Finally, as an additional piece of evidence of the dynamic commitment problem, is to analyze whether the number of previous grants – the proxy variable for bail out expectations – is related to persistent economic shocks. The rationale behind this test is to rule out that the number of previous grants is correlated with persistent economic shocks which otherwise could explain the accumulation of debt as documented in Column 2 in Table 5.[39] To perform this test, a real shock measure instead of debt is used as a dependent variable in equation (4).[40] Column 3 in Table 5 shows the estimate for one measure of real shocks – the average unemployment rate during the period 1979-1992. The estimate is not significantly different from zero which suggests that there is no relationship between persistent shocks and the distribution of grants across time.[41] Put differently, the proxy variable for bail out expectations is not confounded by persistent shocks.

5. Conclusions

In this paper, I estimate the effect of soft budget constraints on the fiscal behavior of Swedish local governments using 1,408 discretionary intergovernmental fiscal transfers over the period 1979 to 1992. The estimation approach, an instrumental variable method, is consistent with theories that argue that the soft budget constraint is caused by a dynamic commitment

[38] I have estimated regression (4) using the debt in 1992, which is the last year of the discretionary transfer program, and the estimate is .020 with a standard error of .008. Interestingly, the estimated soft budget effect of about 20-24 percent in Table 2 is of roughly the same size as the implied effect of having the maximum number of bailouts on the level of debt at the end of the sample period, namely 28 percent (.02*14). Thus, the finding that these two different ways of estimating the size of the soft-budget effect yields similar results lends further credibility to a causal interpretation of the soft-budget effect.

[39] On this point, see footnote 34.

[40] I thank an anonymous referee for the suggestion of this test.

[41] Regressing the total number of grants on the average unemployment rate (the reverse regression) also yield the same conclusion

problem. The instrumental variable is the share of neighboring jurisdictions receiving discretionary fiscal transfers today. I argue that in the Swedish context, this is a valid instrument since the central government's distribution of fiscal transfers is affected by regional considerations, transfers to geographical neighbors are part of a local government's information set, and there is no compelling reason why this instrument should not be excludable from a local government's outcome equation. A large number of specification checks also suggest that the instrument is likely to be valid. The estimated soft budget effect is economically large; on average, a local government increases its debt by more than 20 percent by going from a hard to a soft budget constraint. Thus, Swedish local governments seem to face soft budgets to a significant degree.

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Table 1. Summary statistics

	Mean	St. Dev	Minimum	Maximum
Dependent variable: Debt per capita	9,958	4,720	797	37,699
Explanatory variable: Discretionary fiscal transfers in period t+1, T_{it+1}	.37	.48	0	1
Instrumental variable: the share of neighboring jurisdictions receiving discretionary fiscal transfers in the current period, $\overline{T}_{-it}$	.35	.34	0	1
Control variables		*Local governments' characteristics*		
Income per capita	72,263	11,476	15,945	162,962
Unemployment rate	2.48	1.53	0.19	12.24
Population size	29,689	52,062	2,950	674,452
Proportion of people aged 0 to15	20.71	2.64	12.65	36.3
Proportion of people aged 65 or above	17.98	4.26	4.32	27.73
Left-wing party control	.46	.50	0	1
Left-wing vote share	48.35	11.46	17.56	76.69
Rule-based fiscal equalization transfers per capita	1,910	1,665	-3,110	9,324
		Neighbors' characteristics		
Debt per capita	10,082	2,846	3,748	25,200
Income per capita	72,484	9,560	50,852	123,193
Unemployment rate	2.50	1.30	.40	9.94
Population size	36,426	35,506	5,370	259,223
Proportion of people aged 0 to 15	20.53	1.71	15.84	39.35
Proportion of people aged 65 or above	18.01	3.11	7.00	25.16
Left-wing party control	.45	.33	0	1
Left-wing vote share	48.19	8.14	25.38	69.45
Rule-based fiscal equalization transfers per capita	1,835	1,278	-1,153	7,617

Debt per capita, income per capita and rule-based fiscal transfers per capita are expressed in 1991 fixed prices.

Table 2. Soft budget effects: Instrumental variable estimates

Dependent variable log (debt per capita):	1	2	3	4	5	6	7	8
Soft budget effect	.163**	.229***	.193**	.236**	.220**	.237**	.235**	.248**
	(.079)	(.076)	(.091)	(.096)	(.110)	(.114)	(.115)	(.109)
Control variables								
Local government characteristics		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic shocks			Yes	Yes	Yes	Yes	Yes	Yes
Fixed local government effects				Yes	Yes	Yes	Yes	Yes
Year fixed effects				Yes	Yes	Yes	Yes	Yes
Neighbor characteristics					Yes	Yes	Yes	Yes
Political variables						Yes	Yes	Yes
Rule based intergovernmental grants							Yes	Yes
First-stage *F*-statistic	376	258	29	38	26	26	26	15
Hansen *J* test (*p*-value within parenthesis)								0.11
								(0.74)
Test of joint significance of the control variables χ^2-test (*p*-value within parenthesis)		63	6,586	3,586	3,711	3,732	3,784	3,708
		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.000)	(0.0000)	(0.0000)
Number of observations	3,657	3,649	3,647	3,647	3,647	3,647	3,647	3,647

Note- Huber-White robust standard errors clustered at the municipality level are in parentheses. The explanatory variable is an indicator of whether the local government has receives a fiscal transfer in the future, i.e., period t+1.The instrument is the share of neighboring jurisdictions (i.e. jurisdictions sharing the same borders) receiving discretionary fiscal transfers in the current period. The controls for government and neighbor characteristics are income per capita, population size, unemployment rate, proportion of people aged 0 to 15, and proportion of people aged 65 or above. The controls for economic shocks are lagged debt, neighbor debt and an indicator variable for discretionary fiscal transfers in the current period. Political variables include an indicator for left-wing party control and left-wing vote share. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.

Table 3. Refutability test

	Jurisdictions with at most 4 fiscal transfers (average number of transfers =1.4)		Jurisdictions with more than 4 fiscal transfers (average number of transfers =8.5)	
Dependent variables	Fiscal transfers in period t+1	Log(debt per capita)	Fiscal transfers in period t+1	Log(debt per capita)
	(1)	(2)	(3)	(4)
The share of neighboring jurisdictions receiving discretionary fiscal transfers in the current period *t*	-.019	.012	.210***	.064***
	(.048)	(.028)	(.045)	(.022)
Local government characteristics	Yes	Yes	Yes	Yes
Economic shocks	Yes	Yes	Yes	Yes
Fixed local government effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Characteristics of neighbors	Yes	Yes	Yes	Yes
Political variables	Yes	Yes	Yes	Yes
Rule based intergovernmental grants	Yes	Yes	Yes	Yes
Number of observations	1,814	1,814	1,833	1,833

Note- Huber-White robust standard errors clustered at the municipality level are in parentheses. The controls for government and neighbor characteristics are income per capita, population size, unemployment rate, proportion of people aged 0 to 15, and proportion of people aged 65 or above. The controls for economic shocks are lagged debt, neighbor debt and an indicator variable for discretionary fiscal transfers in the current period. Political variables include an indicator for left-wing party control and left-wing vote share. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.

Table 4. The soft budget effect: Instrumental variable estimates using other measures of the dependent variable.

Dependent variable:	Level of debt	Change in debt	Cash budget deficit	Total spending	Total revenues
	1	2	3	4	5
Soft budget effect	2,305**	2,666**	2,135**	2,748***	613
	(1,074)	(1,093)	(863)	(1,226)	(1,146)
Controls	Yes	Yes	Yes	Yes	Yes
First-stage *F*-statistic	15	15	15	15	15
Hansen *J* test (*p*-value	0.001	0.16	0.92	0.12	1.27
within parenthesis)	(0.97)	(0.69)	(0.34)	(0.73)	(0.26)
Number of observations	3,647	3,647	3,647	3,647	3,647

Note. Huber-White robust standard errors clustered at the municipality level are in parentheses. The included control variables in columns 1, 3 and 4 are the same as in Table 2, column 8. Since the lagged level of debt is included in column 1, the dependent variable could be equivalently expressed as the change in debt. In column 2, lagged debt has been excluded from the specification. The cash budget deficit is defined as current total spending minus current total revenues. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.

Table 5. Additional tests

	Dependent variable: Requesting financial help in year 1999=1	Dependent variable: Log (debt per capita in 1998)	Dependent variable: Average unemployment rate
	(1)	(2)	(3)
Number of discretionary grants	.025***	.033***	-6.39e-06
	(.008)	(.011)	(.00019)
Log (debt per capita in 1978)	-.16***	.14*	-.0009
	(.06)	(.08)	(.0010)
Average population size	-3.54e-07	3.00e-06***	-4.69e-10
	(3.74e-07)	(4.84e-07)	(1.04e-08)
Average income per capita	7.28e-06*	-2.17e-06	-3.28e-07***
	(3.85e-06)	(5.38e-06)	(1.04e-07)
Average proportion of people aged 0 to 15	.044	.049	-.0037***
	(.027)	(.041)	(.0007)
Average proportion of people aged 65 or above	.033*	.024	-.0019***
	(.018)	(.026)	(.0005)
Average unemployment rate	3.70	1.04	-
	(3.76)	(4.18)	
Average left majority	-.0025	.028	-.0007
	(.0883)	(.147)	(.0020)
Average left vote share	.0063	.0059	.00031***
	(.0045)	(.0065)	(.00007)
Average non-discretionary grants	-2.61e-06	.000017	4.38e-06***
	(.00003)	(.000038)	(8.74e-07)
R2	0.2128	0.1685	0.6544
Observations	276	276	276

Note. Huber-White robust standard errors in parentheses. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.

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Figure 1. Number of discretionary grants by year

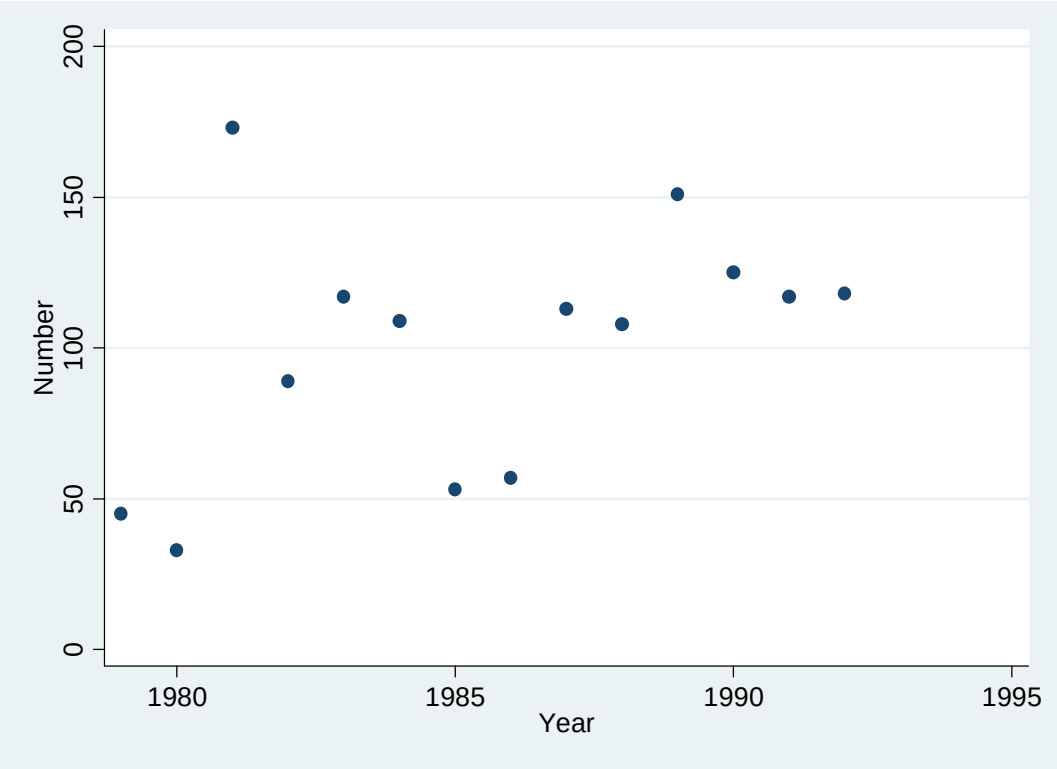

Figure 2. Distribution of discretionary grants across local governments

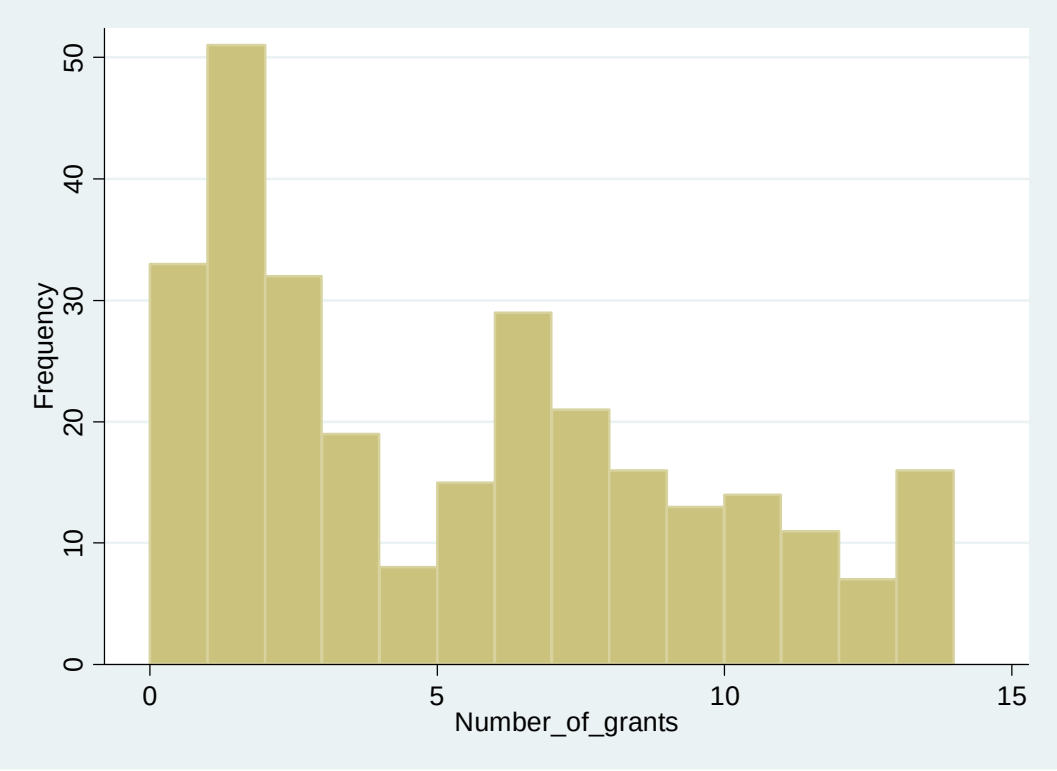

Figure 3. Map showing the distribution of total number of discretionary grants across Swedish local governments

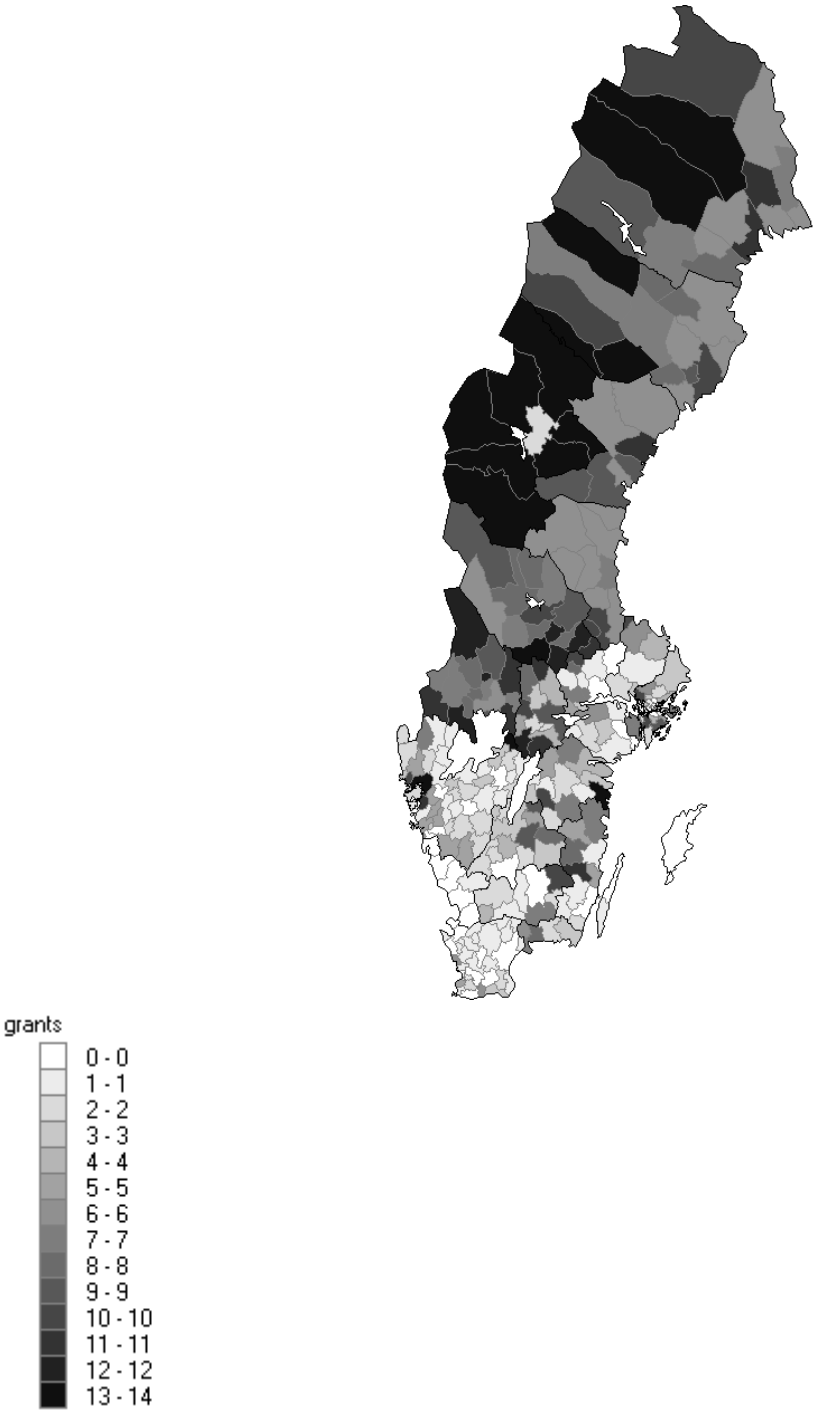

Figure 4. Distribution of the number of observations for the instrument – share of neighboring jurisdictions receiving discretionary fiscal transfers.

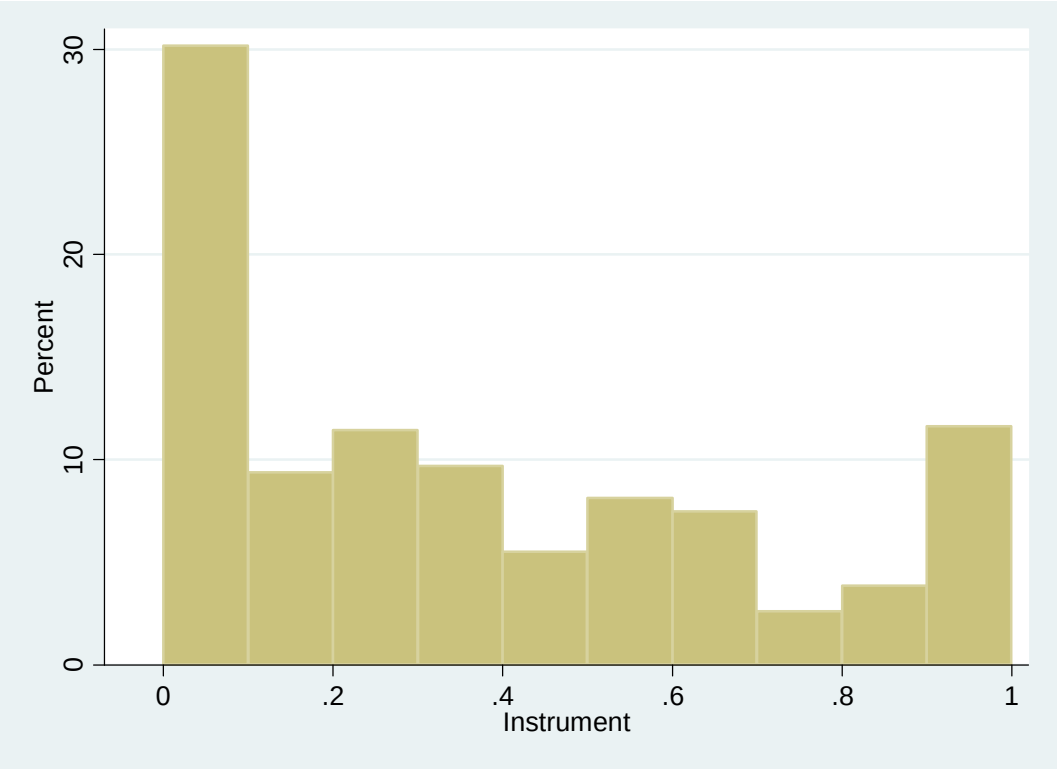

Figure 5. The size distribution of the per capita grant