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An Empirical Approach for Estimating the Causal Effect of Soft Budget Constraints on Economic Outcomes*

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Abstract

In this paper, we develop an empirical framework for estimating the causal effect of soft budget constraints on economic outcomes. The point of departure is that the problem of the soft budget constraint is a problem of credibility, that is, inability of a supporting organization to commit itself not to extend more resources (i.e., bailouts) *ex post* to a budget-constrained organization than it was prepared to provide *ex ante*. This means that current economic behavior of a budget-constrained organization will depend upon its *expectations* of being bailed out in the future. Thus, to estimate the causal effect of soft budget constraints (i.e., bailout expectations) on economic outcomes one has to measure these expectations and link those to the current behavior of the budget-constrained organization. We argue that one can use information about realized bailouts to construct credible measures of bailout expectations and use an instrumental variable strategy to solve problems of measurement error and endogeneity associated with the proxy variable for bailout expectations. The empirical framework is applied to Swedish local governments, who provide an attractive testing ground of the soft budget constraint since the central government has extended a total of 1,697 bailouts over the period 1974 to 1992. We find strong evidence that bailout expectations have a causal effect on economic behavior. The estimated effect is also quite sizeable: on average, a local government increases its debt with 30 percent if it is certain of being bailed out as compared to when it is certain of not being bailed out.

Key words: Credibility, dynamic commitment, time inconsistency, expectations, bailouts, instrumental variable strategies, and neighborhood effects

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1. Introduction

The problem of soft budget constraint (henceforth SBC) is a problem of dynamic commitment, that is, inability of a supporting organization (henceforth S-organization) to prevent an *ex ante* financial budget from being renegotiated *ex post* as discussed by Kornai, Maskin, and Roland (2003).[1] Treating the SBC problem as a time consistency problem implies that expectations will play a key role since current behavior of economic agents will depend on expected future policies (see, e.g., Kydland and Prescott, 1977). Thus, to empirically investigate whether soft budget constraints has a causal impact on economic outcomes one has to measure the *expectations* held by a budget constrained organization (henceforth BC-organization) about receiving bailouts in the future, and link those expectations to its current behavior.

The challenge of isolating causal effects is particularly difficult in the context of SBC due to the strategic interaction between players and the unobservability of expectations. For example, measuring expectations through a survey (preferably along the lines suggested by Manski, 2004) is probably not going to be a useful approach since a BC-organization has strong incentives of not revealing its true bailout expectations since it can exploit the S-organization *ex-post* (e.g., SBC is a dynamic commitment problem where the agent is predatory and the principal is weak).

Our approach is to use realized bailouts in period *t* as a proxy for unobserved bailout expectations in the same period and estimate the causal effect of bailout expectations on economic outcomes via an instrumental variable (IV) strategy to address problems of measurement error and endogeneity associated with the expectational proxy, where the instrument will be based on information about realized bailouts in period *t*-1. Our IV-strategy thus captures the essence of the dynamic commitment problem, namely BC-organizations' *rational* forward-looking behavior (i.e., what agents do depend on what they expect the policymaker to do) and the inability of the S-organization to commit to a no-bailout policy. In other words, we capture the dynamic aspect of the time consistency problem by constructing the instruments from information about bailouts in period *t*-1 and assuming that BC-organizations uses this information to make the best possible forecast of the likelihood of receiving bailouts in the future. The idea that agents use past policy to predict future policy can be motivated from theoretical models of the time consistency problem. There is, for

[1] The concept of SBC was first introduced in the seminal work of Kornai (1979, 1980).

example, a literature that argues that reputational forces can substitute for the lack of commitment technology suggesting that there may be a link between current policy and expected future policy.[2]

The key to the instrumental variable approach is, of course, to find a valid instrument. Apart from the usual assumptions that the instruments should be relevant and validly excluded from the outcome equation, there are two additional requirements of the instrument in the context of SBC. The first requirement is that the instrument should reflect discretionary decisions of the S-organization since the problem of time consistency emanates from the sequential nature of policymaking. This requirement makes it even more difficult than usual to find a valid instrument since it precludes the use of *rule-based* government policies (i.e., regulated transfer schemes between the S-organization and the BC-organization) as exogenous sources of variation.[3] The second requirement is that the instrument should be based on information used by a BC-organization to form bailout expectations; otherwise the empirical results will not be credible.[4] This suggests, it is desirable to construct the instruments from those variables that are "most likely" to be considered by a BC-organization. However, the attractiveness of our IV approach is that we *do not* have to correctly specify a BC-organization's entire information set to get a consistent estimate of the causal effect; the only requirement is that the postulated information is *actually* used by the BC-organization.[5]

An obvious source of information that a BC-organization would use to forecast the likelihood of being bailed out is the own history of previous bailouts. However, using past own bailout as an instrumental variable is quite questionable since it is likely to violate the exclusion restriction. Put differently, there are likely to be omitted variables (e.g., economic shocks) that are plausibly correlated with both past bailouts and the outcome of interest which causes past bailouts to have a direct effect on the outcome. Therefore, we argue that past own bailouts should be used as a control variable instead since it will capture *all* factors, including economic shocks, that may otherwise be difficult to control for that causes the S-organization to extend bailouts to a particular BC-organization. This specific feature makes past own bailout a powerful control variable.

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[2] See Persson and Tabellini (1994) for a collection of articles on time consistency in macroeconomic policy.
[3] See Meyer (1995) for a discussion of the attractiveness of using government policy as an exogenous source of variation. See Besley and Case (2000), however, for a more critical view.
[4] Dominitz and Manski (1997) and Friedman (1979) criticize the empirical work that estimates rational expectation models for making noncredible assumptions about the information set economic agents are using to form expectations.
[5] See McCallum (1976) for a discussion of this point.

Instead, we suggest that one could construct a plausible instrument from information about bailouts in a BC-organization's "neighborhood", where neighborhood can be defined according to geographic proximity or some other metric of closeness such as being in the same part of the economy.[6] In fact, Kornai et al. (2003) argue that bailout "expectations have much to do with collective experience. 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." Thus, it seems reasonable to assume that a BC-organization uses information about bailouts in the neighborhood to form bailout expectations. This line of reasoning is also related to the neighborhood effects or social interactions literature discussed by Manski (2000) and Moffitt (2001), among others. For example, Manski (2000) argues that an economic "agent forming expectations may seek to draw lessons from observation of the actions chosen and outcomes experienced by others. Such *observational learning* generates expectations interactions." Ehud and Lehrer (1993), in turn, show that observational learning leads to rational expectations in repeated interactions. Hence, the instrumental variable approach we suggest is consistent with rational expectations since it is based the idea of expectations interactions.

In order for bailouts of neighbors to serve as a valid instrument to identify the effect of bailout expectations on economic outcomes, it must be the case that the instrument only affects the outcome through bailout expectations. The exclusion restriction is invalid if there are other variables (e.g., common shocks) that are both correlated with the economic outcome and bailouts of neighbors. However, we argue that if one control for past own bailouts and the lag of the economic outcome of interest, then bailouts of neighbors in t-1 should be a valid instrument.

To evaluate the plausibility of the exclusion restriction, we conduct a number of specification tests.[7] First, we examine whether the point estimate from the IV-regression is sensitive to the inclusion of additional control variables (e.g., variables capturing economic shocks in the SBC case). The basic idea is that if the point estimate does not change as additional covariates are included in the regression it is less likely to change if we were able to add some of the potentially missing omitted variables. Second, we test whether there is a direct effect between the instruments and the economic outcome in a sample where one would

[6] Akerlof (1997) provides a formal framework to think about the appropriate definition of neighborhood according to the notion of proximity versus distance in "social space".
[7] See Altonji, Elder and Taber (2002, 2005) and Angrist and Krueger (1999) for discussions about specification tests that can be used to evaluate instrumental variables.

expect the causal effect to be absent. In the case of SBC, this sample would be made up by those BC-organizations' that has no expectations of being bailed out. If there were no association between the instruments and the economic outcome in this sample, this would provide strong support for a causal effect of bailout expectations on economic outcomes. Third, we use different lags of the instrument. One would expect that the point estimate from the instrumental variable regressions to be affected differently for different lags if there are omitted variables. Put differently, if the point estimates are similar across the different lags it will give support for a causal interpretation. Finally, we conduct overidentifying restrictions tests such as the Sargan test but also the recently proposed test by Hahn and Hausman (2002).

We apply the above instrumental variable strategy to a data set from Swedish local governments. This data set is particularly attractive for an empirical investigation of the problem of SBC since the central government has extended a total of 1,697 bailouts over the period 1974 to 1992. Importantly, the decisions to extend bailouts were not rule-based but discretionary (i.e., one of the additional requirements for an instrument in the SBC context). We find that bailout expectations have an impact on local governments' debt behavior. When applying the specification tests discussed above, we never reject the hypothesis that neighbors' bailouts in t-1 is a valid instrument, which provides strong support for a causal interpretation of the effect of bailout expectations on debt. The estimated effect on debt from bailout expectations is also economically very large: on average, a local government increases its debt with 30 percent if it is certain of being bailed out as compared to when it is certain of not being bailed out.

The outline of the paper is as follows. In the next section, we present the empirical for estimating the causal effect from bailout expectations (or soft budget constraints) on economic behavior. Section 3 discusses the data while section 4 presents the empirical results. Section 5 summarizes and gives some concluding remarks.

2. Empirical framework

Empirical studies of the soft budget constraint typically run regressions of the form[8]

$$(1) \qquad Y_i = \psi_0 + \psi_1 S_i + X_i b + \eta_i,$$

where i denotes a BC-organization (e.g., a firm, a local government, a bank etc,), Y_i is the economic outcome of interest, S_i is a proxy variable for soft budgets, and X_i is a set of control variables. There are basically two problems with this type of regression: First, the proxy variable being used, S_i, is at best an error-ridden variable of bailout expectations,[9] which causes the estimate of ψ_1 to be biased toward zero (attenuation bias) if the measurement error is of the "classical" type.[10] Second, regression (1) does not address problems of omitted-variable bias, that is, there will likely be unmeasured economic shocks that both affect the economic outcome of the BC-organizations and the measure of soft budgets S_i.[11] These two problems will lead to biased inference since the error term will be correlated with the variable S_i, i.e., Cov(η_i, S_i) „ 0. In the following, we attempt to solve these problems using a new approach.

As was discussed in the introduction, a measure of soft budgets should capture *subjective expectations* of a BC-organization as to whether it will receive financial help, i.e., be bailed out, from an S-organization in times of trouble.[12] Let B^e_{it} denote these subjective expectations for BC-organization i in time period t. Then the structural model or population regression of interest is,

$$(2) \qquad Y_{it} = \alpha + \pi B^e_{it} + X_{it}\beta + u_{it}.$$

The parameter of interest is π, which measures the effect of expectations of bailouts on economic behavior. Equation (2) should be interpreted in counterfactual terms, that is, for given X_{it} and u_{it}, it gives the optimal economic response for *any* possible degree of expectations for bailouts faced by a BC-organization. In other words, the size of π will be the

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[8] See Djankov and Murrell (2002) and Kornai et al. (2003) for discussions of the quite limited number of previous empirical studies of the SBC problem.

[9] Kornai et al. (2003) criticize the bulk of existing studies of the SBC problem for using "loop-sided" measures of soft budget constraints.

[10] The classical measurement error assumption is that the measurement error is uncorrelated with the unobserved explanatory variable. See Bound et al. (2001) for a general treatment of measurement errors problems.

[11] Djankov and Murrell (2002) also have some discussion of the econometric problems in the SBC literature but they do not provide any solution.

[12] That soft budget constraint has to do with subjective expectations and the degree of hardness and softness ought to be measured on a continuous scale is discussed from the very beginning in Kornai's work on the SBC (Kornai, 1979).

causal effect on Y_{it} of going from a zero probability to a probability of one of being bailed out, i.e., $\pi = E(Y_{it} \mid B^e_{it} = 1, X_{it}) - E(Y_{it} \mid B^e_{it} = 0, X_{it})$. Put another way, the parameter π measures the impact of going from a soft to a hard to budget constraint since B^e is measuring the degree to which the budget constraint is soft.[13] Therefore, we would expect that $\pi > 0$ if there is a problem of SBC; otherwise the problem does not exist.

Equation (2) is not, however, directly estimable since expectations are unobserved. One way to make it estimable is to replace B^e_{it} with a survey measure of bailout expectations but, as mentioned in the introductory section there are serious problems when using survey data in the context of the SBC problem.[14] Anderson, Lee, and Murrell (2000) make an attempt to use this approach, but they fail to find any evidence that soft budgets has an impact on economic outcomes. However, their failure to find an effect could be due to the fact that their survey measure does not capture the true expectations for bailouts and that this would cause the estimate to be biased toward zero due to an error-in-variables problem.

Our approach is instead to replace the expectational variable B^e_{it} in equation (2) with its observed realization in period t, i.e., $B_{it} = 1$ if BC-organization i was bailed out in period t and zero otherwise, thereby creating a measurement error problem, $B_{it} = B^e_{it} + e_{it}$, that is,

$$Y_{it} = \alpha + \pi B_{it} + X_{it}\beta + v_{it}, \tag{3}$$

where $v_{it} = u_{it} - \pi e_{it}$. We can now consistently estimate π by using an instrumental variable strategy where the instrumental variable must fulfill the following assumptions: (i) the instrument is correlated with the expectational proxy variable B_{it}, (ii) the instrument is uncorrelated with the measurement error e_{it}, and (iii) the instrument is uncorrelated with the structural error term u_{it}. As discussed previously, an instrument in the SBC context must also fulfill two additional requirements: i.e., constructed from *discretionary* bailouts and based on information that the BC-organization is *actually* using to form bailout expectations, which makes it even more difficult than usual to find a valid instrument (due to problems of policy endogeneity as discussed by Besley and Case, 2000). Nevertheless, the strength of our IV-

[13] Although, theoretical analyzes often oppose two polar cases (hard vs. soft budget constraints), this is just a simplification since the decision to bail out or not to bailout does not have pure strategy as discussed by Kornai et al (2003), that is, the S-organization may choose to bailout with probability q and not to bailout with probability $1-q$.

[14] There are also some general problems with using survey data in order to make causal inference. For example, there is the well-known problem with measurement errors, but there may also be other problems; survey responses might for example not be independent of the economic outcome they are thought to explain. See Bound, Brown and Mathiowetz (2001) for a discussion of the problem of making causal inference using survey data.

approach is that it is consistent with the essence of the dynamic commitment problem, namely that a BC-organization is characterized by rational forward-looking behavior, i.e., B^e_{it}=E[B_{it}| I_{it-1}] where I_{it-1} is the information available to the BC-organization at the time the prediction is made in period t-1, and the inability of the S-organization to dynamically commit, i.e., B_{it}=1. Theory also suggests that there is link between past bailouts and the expectation of future bailouts, a fact that will be exploited below to construct an instrumental variable.

When thinking about how to construct an instrumental variable (that is, what information about past bailouts that a BC-organization is likely to use to form bailout expectations) we face a trade off between exogeneity and spurious effects since variables taken from a BC-organization are more likely to have direct influences on the organization's expectation for bailouts but are also more likely to be endogenous. On the other hand, variables taken from the neighborhood are less likely to be endogenous to the outcomes of a BC-organization but are also farther removed from its expectation for bailouts, resulting in a greater danger of getting a spurious correlation or an incorrectly specified mechanism. For example, one obvious source of information that a BC-organization is likely to use to form bailout expectations is whether they have been bailed out or not in the past. In this case one could use lagged own bailout (i.e., $B_{i,t-k}$=1 if BC-organization i has been bailed out in period t-k where k =1,2,…, T, and zero otherwise). However, lagged bailout is not likely to fulfill the exclusion restriction. In other words, lagged own bailout is likely to have a direct effect on the economic outcome since it will be a function of economic shocks which makes this instrument highly questionable.

An instrument that is more likely to be exogenous is to construct it from information about bailouts in a BC-organization's neighborhood since it also seems reasonable to assume that bailout expectations are partly influenced by the extent to which other BC-organizations have received bailouts as discussed previously. This requires that we are able to define the relevant neighborhood that affects the expectations for a particular BC-organization. The definition of the neighborhood will typically depend on the particular application of the SBC. Nevertheless, for some applications, the literature about neighborhood effects or social interactions effects can inform us how to define neighborhoods more generally.[15] This literature argues that in a world where human interactions and exchange of ideas are the main forces behind neighborhood effects, measures of neighborhood can be based on geographic proximity (see, e.g., Durlauf, 2004). For example, when the SBC problem is applied to central

[15] See Akerlof (1997) for a discussion of how to think about definition of neighborhoods.

and local governments it seems reasonable to assume that expectations of local governments are affected more by events that are geographically close than by events that happen at a distance. Thus, in our case, a geographical definition (e.g., sharing the same border) of neighborhood seems appropriate.

To measure the degree of influence a neighborhood is having on a BC-organization's bailout expectation, one can construct a weighted average of bailouts in the neighborhood, i.e., $neighbors_{(-i)t-k}=1/J(\Sigma I_{j,t-k})$ where J is the number of other BC-organizations in i's neighborhood and $I_{j,t-k}$ is an indicator variable taking the value 1 if BC-organization j in the neighborhood received a bailout in period $t-k$, zero otherwise. This measure says that the larger the number of other BC-organization in the neighborhood receiving bailouts in period $t-1$ or earlier, the higher is the likelihood that BC-organization i will be bailed out in period t.

There may be a problem with using *neighbors* as an instrument if there are common economic shocks to the neighborhood which cannot be properly accounted for in (3). However, we argue that if we use lagged own bailouts B_{it-k} as a control variable together with the lagged economic outcome, $Y_{i,t-1}$, then *neighbors* should be a valid instrument. In this case, B_{it-k} effectively controls for *all* factors (including economic shocks) that causes the S-organization to extend bailouts to a BC-organization in periods $t-1$, $t-2$,.., $t-k$,, which makes it a very powerful control variable. The lagged dependent variable is also capturing the impact of the economic environment on the performance of a BC-organization since bailouts in period t might be functions of the economic outcome in period $t-1$, i.e., $B_{it} = f(Y_{i,t-1})$. In other words, the crucial assumption justifying the use of bailouts of geographical neighbors as an instrument is that, *conditional* on B_{it-k} and $Y_{i,t-1}$, *neighbors* is unrelated to the error term v_{it} in equation (3).

A last comment on the instrumental variable strategy concerns assumption (ii). This assumption is valid if a BC-organization makes efficient use of whatever information is available. In other words, if a BC-organization forms expectations rationally this implies that the measurement error (prediction error) e_{it} would be uncorrelated with the entire information set that is available to BC-organization i at time $t-1$.[16] The assumption about rational expectations seems to be a reasonable benchmark in the context of the SBC since a BC-organization has strong incentives to make the best possible forecast about the future likelihood of being bailed out in order to exploit the S-organization *ex-post*. Moreover, the

[16] There are other ways one can estimate rational expectations models than by an instrumental variable method. However, these other methods require much more restrictive assumption to yield consistent estimates (see, e.g., Wickens, 1982).

time inconsistency problem is built on the very notion that economic agents are *rationally* forward-looking. The attractiveness of using an instrumental variable strategy when estimating a rational expectations model is that we do not have to specify the whole information set available to a BC-organization to get a consistent estimate of π; the only requirement is that the BC-organization is actually using the postulated information, otherwise the measurement error e_{it} will *not* be uncorrelated with the instrument.[17] Thus, this is another reason than the one discussed above (to avoid spurious effects) for using variables from the local neighborhood when constructing instruments for bailout expectations, namely to avoid that assumption (ii) does not hold.

To sum up, we will estimate regressions of the form

$$Y_{it} = \alpha + \pi B_{it} + \theta B_{i,t-1} + \lambda Y_{i,t-1} + X_{it}\beta + X_{(-i)t}\delta + v_{it} \quad (4)$$

where we argue that $neighbors_{(-i),t-1}$ is a valid instrument for B_{it}, i.e., that it fulfill requirements (i)-(iii), conditional on $B_{i,t-1}$ and $Y_{i,t-1}$. It is important to note that we will not only include municipality specific covariates, X_{it}, but also neighborhood specific covariates, $X_{(-i)t}$, into (4) to control for correlated shocks across neighborhoods.

2.1 Specification tests

To be certain that we are estimating a causal link between bailout expectations and the outcome variable, we need to establish that the instrument is relevant and valid (i.e., that it fulfill requirements (i)-(iii)). In this section, we will discuss different specification tests that can be used for assessing whether these three assumptions are likely to be valid.

Assumption (i) is that the instrument is relevant. Relevance of an instrument can and should be tested to detect problems of weak instruments. One such test is the F-statistic for the joint significance of the instruments in the first stage equation (as a diagnostic of the power of the instruments). If the F-statistic is larger than 10, there should be no problem associated with weak instruments (see, e.g., Staiger and Stock, 1997, and Stock, Wright and Yogo, 2002).[18]

Assumptions (ii) and (iii) about the instruments is not directly testable. Nevertheless, there exist various indirect methods that can be used to evaluate the plausibility that the

[17] See the discussion in McCallum (1976).
[18] See also Hansen, Hausman and Newey (2004).

suggested instruments is uncorrelated with v_{it} (i.e., uncorrelated with e_{it} and u_{it}) and thus validly excluded from the outcome equation.[19] In the following, we discuss four different approaches that can be used to assess whether the instruments are likely to fulfill the exclusion restriction or not.

The first approach that one can use to evaluate the plausibility of exogeneity is to test whether the point estimates from the instrumental variable regression is sensitive to the inclusion of additional control variables. Here the idea is that if the estimates are insensitive to controlling for observables then they should also be insensitive to unobservables, that is, the omitted variable bias is likely to be quite small. For this method to work in practice, the set of control variables must be powerful in the sense that they should pick up the most important confounding variables. In the context of SBC, economic shocks are perhaps the most important variables to control for since economic problems may trigger bailouts independent of soft budgets. One could also include other BC-organization specific characteristics and neighborhood specific factors that vary across time that are likely to reflect the economic environment. There might also be other confounding factors such as common shocks or BC-organization specific factors that are time invariant. These factors can be controlled for by including time and BC-organization specific fixed effects. These fixed effects will not only control for any unobserved BC-organization characteristics that are constant across time, but they also control for any unobserved neighborhood characteristics which may be important for the identification of neighborhood effects (see, e.g., Brock and Durlauf 2001).

The second approach that can be used to evaluate the plausibility of exogeneity is to use further lags of the instruments. This has to do with the hypothesis that economic shocks may be correlated across time which may cause the instruments in period *t-1* to be invalid, but instruments in period *t-2* or in some earlier time period might then be valid. For example, if a BC-organization experience correlated economic shocks across time then one could run the following regression

$$(5) \qquad Y_{it}=\alpha + \rho B_{it} + \theta_1 B_{i,t-1} + \theta_2 B_{i,t-2} + \theta_3 B_{i,t-3} + \theta_4 B_{i,t-4} + \lambda Y_{i,t-1} + X_{it}\beta + X_{(-i)t}\delta + v_{it}$$

and use $neighbors_{(-i),t-4}$ as an instrument for B_{it}. The idea to use *neighbors* in period *t-4* is that it is less likely that any economic shock (or any other confounding variables that is a function

[19] See Altonji et al. (2002) and Krueger and Angrist (1999) for discussions and examples of such tests.

of past own bailouts) that is not captured by the BC-organization's individual bailout history, i.e., $B_{i,t-1}$, $B_{i,t-2}$, $B_{i,t-3}$ and $B_{i,t-4}$, is persistent as long as for four periods. If the estimated effect of bailout expectations on the economic outcome is sensitive to the lag structure of the instrument, then there are reasons to suspect that there are unmeasured shocks. On the other hand, if the estimates are not sensitive to the lag structure of the instrument, this suggests that the instrument is valid.

A third indirect way of testing for exogeneity of the instrument is to find a sample where the causal effect would be absent; if we cannot reject that instrument does not have a direct effect on the outcome of interest, this would provide strong support for the case that the instrument is likely to be exogenous. In the context of SBC, we should find a sample of BC-organizations where the expectation of being bailed out is zero. One such sample is where the BC-organizations have been bailed out at most once. This is also consistent with Kornai (1998) where he argues that a single instance of occasional assistance to an enterprise will not produce the SBC phenomenon. To perform this test we run the following regression

$$(6) \qquad Y_{it} = \alpha + \omega neighbors_{(-i),t-1} + \theta B_{i,t-1} + \lambda Y_{i,t-1} + X_{it}\beta + X_{(-i)t}\delta + v_{it}$$

on this sample and test whether the estimated coefficient for the instrumental variable, $\hat{w}$, is significantly different from zero. In other words, if the instrument only affects the outcome variable Y_{it} through bailout expectations, we would not expect any direct relationship in the sample of BC-organizations that have received at most one bailout. On the other hand, if the instrument affects the economic outcome for a reason other than bailout expectations (e,g., unobserved economic shocks), we would expect the instrumental variable to be directly related to the outcome variable in this sample.

A fourth test of exogeneity is to conduct overidentifying restriction tests. One such test is the Sargan test, but it has been shown that this test might have low power (see, e.g., Newey, 1985). However, Hahn and Hausman (2002) have developed a new overidentifying restriction's test, which also is a test for weak instruments. They noted that when the instruments are valid, normalization of the regression (the choice of the dependent variable) should not matter. Thus the "forward" (conventional) 2SLS estimate of the coefficient of the right-hand side endogenous variable should be very similar to the inverse estimate from the "reverse" (normalization is changed) 2SLS regression using the same set of instruments.

3. Data

We will use data from Swedish local governments as a testing ground for the problem of SBC. The relationship between central and local governments in Sweden is an attractive testing ground for the SBC problem since local governments (municipalities) recurrently received financial support (i.e., bailouts) from the central government over the period 1974-1992.[20] Bailouts of local governments are also an interesting issue in its own right. During the last 10-15 years there has been an increase in the number of sub-national governments with financial problems, both in the developed and in the developing world. In several of these instances, local governments received financial help from the central government.[21] Despite the fact that the violation of fiscal discipline at the local level is considered to be a serious economic problem, there have been few attempts to systematically evaluate the reasons for why sub-national governments end up in financial problems. Typically, the empirical work is based on case studies.[22]

Before turning to a description of the data, we digress briefly on the workings of Swedish local governments. Sweden is currently divided into 290 local governments (or municipalities), which cover the entire country. Local governments play an important role in the Swedish economy, both in terms of the allocation of functions among different levels of government and economic significance. They are, for example, responsible for the provision of day care, education, care of the elderly, and social welfare services. To quantify their economic importance, note that in the 1980s and 1990s their share of spending out of GDP was in the range of 20 to 25 percent and they employed roughly 20 percent of the total Swedish workforce. Swedish local governments also have a large degree of autonomy. They have the constitutional right of self-government, they have no restrictions on borrowing, the state plays no part in either monitoring or approving local government accounts, and they have no balanced budget rules.[23] Moreover, during the period of investigation (1974-1992), the bulk of revenues were raised trough a proportional income tax, which each municipality

[20] There existed financial relief programs also after 1992. As a matter of fact, several of the municipalities ran into severe financial problems during the 1990s (some of them where very close to bankruptcy) and many of these municipalities where bailed out by the central government in the second half of the 1990s.
[21] The perhaps best-known example is the bailout of the city of Sao Paolo in Brazil in the 1990s.
[22] See Rodden and Eskeland (2003) for a collection of case studies.
[23] As from year 2000 there is a balanced budget rule.

was allowed to set freely,[24] and only 20 percent of the total revenues came from intergovernmental grants.

3.1 Bailouts of Swedish local governments

During the period 1974-1992, the central government was empowered by law (e.g. SFS 1973:433, SFS 1979:362, and SFS 1988:491) to provide financial relief grants to local governments. In 1,697 cases the central provided financial support or bailouts to local governments. On average, bailed out municipalities received a transfer of SEK 166 per capita (St. Dev. 224) which constitute 1.6 percent of average total debt (10,216).[25]

There are two features of the Swedish financial relief program that makes it quite attractive for studying the SBC problem. First, and most importantly, the relief program was not part of a regular intergovernmental transfer scheme that typically characterizes the fiscal arrangement between central/federal and sub-national units in most countries. Such transfer schemes are to a large extent heavily regulated or rule based. In contrast, the financial relief program was at the central governments discretion, and the central government had to make a new decision of the distribution of the fiscal transfers each year. Thus, these bailouts can be used for the identification of the SBC problem since they fulfill the requirement of being discretionary as discussed previously.

The second feature of the financial relief program was that is was explicitly targeted to financially distressed municipalities. The program was set up so that local governments could receive financial support in two different ways. The central government could distribute relief grants at its own initiative or the municipalities could apply directly to the central government. In either of these cases, the financial support from the central government was explicitly intended to be distributed to financially distressed municipalities. For those local governments that choose to apply for help, the application process was the following: The municipalities had to hand in their application before the end of March.[26] The central government then made its decisions during the fall the same year and the financial relief grants were finally paid out during the subsequent year. During each year, there were roughly 25 to 60 of the applicants that received grants.[27] Typically, these municipalities claimed that

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[24] From 1991 to 1993, however, the central government imposed a temporary tax cap.
[25] $1 dollar is roughly equal to SEK 6 (in 1991year prices).
[26] 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.
[27] 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.

they had severe financial problems and that they would be unable to fulfill their responsibilities without additional resources. Moreover, they also argued that their financial problems were due to external factors such as high unemployment rates and deteriorating income tax bases. In the case that the financial relief grants were distributed at the central governments initiative, the reasons for providing these additional grants were mainly based on compensating for adverse economic outcomes.[28]

Figure 1 shows the amount of money (in MSEK at fixed 1991 prices) that was distributed annually during the period 1974 to 1992. On average, the central government distributed MSEK 282 each year. Figure 1 also reveals quite large fluctuations in the annual sum (St. Dev. 127), which reinforces the discretionary feature of the program. Figure 2 shows the number of local governments receiving bailouts on an annual basis. There is quite a large variation in the number of recipients, the average number being 90, with a minimum of 28 and a maximum of 173, which again underscores the discretionary feature of the program.

Table 1 describes the bailout data in greater detail, revealing a large variation in the number of bailouts across municipalities (c.f. the first two columns). For example, 3 municipalities (approximately 1 percent of the whole sample) received 19 bailouts over the period 1974-92, the highest possible number, whereas 23 municipalities (approximately 8 percent of the total sample) did not receive any bailouts at all over the same time period. The average number of bailouts received by a municipality during the period was 6, which also roughly corresponds to the median number of bailouts.

An important consideration when using instrumental variables is how much variation the instruments induce in the expectations for bailouts. This has to do with the problem of extrapolation, that is, with the applicability of the results to data points outside the sample actually used in the analysis. Table 2, presenting the range of variation for the instrumental variable, *neighbors*, reveals that there seems to be sufficient variation in *neighbors* across the entire 0-1 range.

[28] In our data, however, we are unable to identify whether the financial relief grants were distributed at central governments initiative or via the local governments' application process. Therefore, we are forced to treat the whole financial relief program as being informative about the SBC problem. However, we do think that this is the correct procedure in any case since it is the expectations of local governments of being rescued in case they should go into trouble that constitute the core of the SBC problem and therefore all the fiscal transfers from this program should contain valuable information about such expectations.

3.2 Economic outcomes and control variables

We will use the level of local government debt, measured in per capita terms and at constant prices, as our measure of economic outcome.[29] Debt seems to be a suitable measure of the extent of fiscal discipline since Swedish local governments have no restrictions on borrowing and did not meet any balanced budget rules under the studied period. There are several measures of debt in the official financial position of municipalities but we have chosen to work with short- and long-term debt, not including social security liabilities.[30] We made this choice so as to have a comparable measure of debt in the sample period, but also because the social security liabilities probably are not a good measure of fiscal misbehavior. Figure 3 provides information on how the average level of debt per capita has evolved during the period 1974 to 1992. Figure 3 also provides information about the variation, i.e., a one standard deviation bound, and the minimum and the maximum values. The figure shows that the average debt decreased slightly until 1987, but slowly increased thereafter. However, the basic message is that the average level of debt has more or less been constant, but there is large variation across municipalities.

As discussed previously, economic shocks affecting the municipalities' economic situation are the most important factors to control for. Two important controls for economic shocks are average income and unemployment rates in the municipalities. Since municipalities raise the bulk of revenues trough a proportional income tax, the income variable will capture any economic shocks that affects the income tax base. As controls for demographic shocks we use population size, population density, proportion of the population above 64, and proportion of the population below 16.

Traditional summary statistics (mean, standard deviation, minimum and maximum values) for the outcome and control variables are presented in Table 3. In Table 1, where the municipalities are classified into groups based on the number of bailouts received over the period 1974-92, average values of the outcome and control variables are presented by group. From Table 1, it seems like a municipality that have received many bailouts typically has a higher unemployment rate than a municipality that has received few bailouts. It also seems to be smaller, more sparsely populated and to have a smaller share of young persons and a

[29] We 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.

[30] 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. Data on social security liabilities are only available from 1988.

higher share of elderly than a municipality that has received few bailouts. There does however not seem be a strong relationship between the number of bailouts and average income.

4. Results

In this section, we present the results of the effect bailout expectations on debt. In the first subsection, we present the baseline results, i.e., the results when we use B_{it} as a proxy for bailout expectations B^e_{it} and use the weighted average of bailouts of geographical neighbors *neighbors* as an instrument for B_{it} (c.f. equations (3) and (4)). To evaluate whether *neighbors* is a valid instrument we apply the four different specification tests discussed in section 2.1. In the second subsection, we present two extensions.

We follow the usual approach of reporting Huber-White robust standard errors. However, since there can be serial dependence in the errors within municipalities, we also report (within brackets) the more conservative Huber-White standard errors clustered at the municipality level (following the suggestions of Bertrand, Duflo and Mullainathan, 2004, and Kézdi, 2002).

4.1 Baseline results

Table 4 shows the results when using bailouts of neighbors, *neighbors*, as an instrument. The first column displays the results from a specification without any control variables. However, as argued in section 2, *neighbors* should be a valid instrument once we condition on past own bailouts and the lagged dependent variable. Therefore we always include at least B_{it-1} and lagged debt as control variables in all the other specifications in Table 4.

We start by using *neighbors* in period t-1 as an instrument in the specifications shown in columns 1 to 4. These specifications differ according to the included control variables; in column 2, we only control for past own bailouts and lagged debt, in column 3 we add income, population size, population density, the proportion of population above 65, proportion of population below 15 as additional covariates both at the local government level and the neighborhood level, while column 4 also adds the unemployment rate both at the local government level and the neighborhood level as additional controls.[31] In addition, municipality-specific fixed effects and year-specific fixed effects are also included in columns 3 and 4. The estimates are remarkably similar in columns 1 to 4 except for the specification without any controls (column 1) which is somewhat smaller (1894 SEK per capita). The other estimates are in the range 2358 to 2608. As discussed in section 2, the insensitivity of the estimates to the inclusion of additional control variables suggests that instrument is likely to

[31] The reason for excluding the unemployment variable in column 3 is that it is only available for the shorter period 1979 to 1992.

be exogenous. In other words, if the estimates change very little when we include (important) observable variables, then the estimates should also change very little if we were to include any unobserved factor (implying that the omitted variable bias is likely to be small). All the estimates in columns 1 to 4 are also statistically significant at the 5 % level or better. The size of the effects is economically large since it corresponds to a nearly 30 percent increase in the average level of debt (10,218) of going from a probability of zero to a probability of one of being bailed out.

The second approach for evaluating the exogeneity of the instrument is to use lags of the instrument and to see whether the estimates are sensitive to this alteration. Here the idea is that instruments in period *t-2* or in some earlier time period may be less affected by unobserved economic shocks that are correlated across time than the instrument in period *t-1*, and therefore one would expect that the estimates would change a lot if there are such unobserved shocks. In columns 5 to 7 in Table 4, we use *neighbors* in period *t-2* as an instrument. We apply the same set of specifications as we did in columns 2 to 4 with the important exception that we also control for B_{it-2}. The estimates are in the range 2615 to 2839. Thus, the estimates change very little when we add the same set of controls as we did in columns 3 and 4. More importantly, however, the estimates in column 5 to 7 are very similar to the estimates in column 2 to 4 when we used *neighbors* in period *t-1* as instrument. In column 8 and 9, we lag *neighbors* even further; in column 8, we use *neighbors* lagged three periods, and in column 9, we use *neighbors* in period *t-4*. Again, we also control for further lags of past own bailouts; in column 8 we add B_{it-3} while in column 9 we add both $\mathrm{B}_{\mathrm{it-3}}$ and $\mathrm{B}_{\mathrm{it-4}}$. As can be noted, the results are very similar to the previous ones. Thus, this further supports the notion that the instrument is likely to be exogenous (i.e., fulfill the assumptions (ii) and (iii)).

Another approach for evaluating the plausibility of the exclusion restriction is to estimate if there is a direct link between *neighbors* and debt for a sample of municipalities with zero bailout expectations. For such a sample, the causal effect should be absent; if we cannot reject that *neighbors* do not have a direct effect on the municipalities' debt, this provides strong support for the case that the instrument is likely to be exogenous. In other words, if *neighbors* affects the debt for the municipalities for this subsample, it is likely that the instrument picks up variation from unobserved variables (e.g., economic or demographic shocks). In the sample of local governments that should have close to zero bailout expectations, we include municipalities that have been bailed out at most once.[32] The results for this subsample are presented in the first column in Table 5.[33] The point estimate is close to zero (18) but it is not very precisely measured. As a comparison, we show the reduced form estimate for the full sample in the second column; it is statistically significant and considerably larger in magnitude (334).

In the last approach for evaluating the exogeneity of the instrument, we conduct overidentifying restrictions tests. We conduct both the conventional Sargan tests and the tests suggested by Hahn and Hausman (2002). These tests are presented in Table 6. Starting with the conventional Sargan tests, we cannot reject the joint hypothesis that the instruments are valid/the model specification is correct. Turning to the overidentifying restriction's test developed by Hahn and Hausman (2002)[34], it is clear that the forward and the reverse estimates are very similar to each other, which strengthen our case.[35] Table 7 reports an easy-to-interpret version of the overidentifying test. It adds *neighbors* in period *t*-1 as an exogenous regressor while using *neighbors* in period *t*-3 as an instrument in column 1 and *neighbors* in period t-*4* in column 2. If *neighbors* in period *t*-1 had a direct effect on debt, we would expect it to come in significant. In both cases, the estimate is close to zero (-49 and -19) and statistically insignificant. The estimates of the effect of bailout expectations on debt are again very similar to the previous ones in Tables 4 and 6.

Finally, *neighbors* is not a weak instrument as can bee seen from the first-stage *F*-statistic, presented in the second to last row in Tables 4 and 6. The *F*-statistic is the statistic obtained when testing whether the excluded instrument is zero in the first stage regression (i.e., from the reduced form estimates of the instruments on realized bailouts B_{it}). From the statistic it is clear that we can strongly reject the null hypotheses that the instrument is weak since it is usually much larger than 10 (c.f. the discussion in section 2). The test developed by Hahn and Hausman (2003) is also a test of weak instrument and as can be seen from Table 6 the forward and reversed estimates are very close to each other suggesting that the instruments are strong.

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[32] As noted earlier, this is consistent with Kornai (1998), who argues that a single instance of occasional assistance to an enterprise will not produce the SBC phenomenon.

[33] Provided that the municipalities that have received zero or one bailouts have, on average, several neighbors that have received bailouts (c.f. the first two rows in Table 1), there is enough variation to examine this question.

[34] Hahn and Hausman (2002) noted that when the instruments are valid, normalization of the regression (the choice of the dependent variable) should not matter. Thus the "forward" (conventional) 2SLS estimate of the coefficient of the right-hand side endogenous variable should be very similar to the inverse estimate from the "reverse" (normalization is changed) 2SLS regression using the same set of instruments.

[35] The reverse estimate in column 1 is 0.000321, implying that the inverse of the reverse estimate is 3114.

4.2. Extensions

In this subsection, we report results from two extensions. The first extension is to add more control variables to the specifications. Compared to the specifications in Tables 4 and 6, we have added the average neighborhood debt in period *t*, three intergovernmental grant variables (a taxbase equalization grant, an investment grant and a grant for running expenses), and three political variables; two indicators for party control (defining whether the local council is run by a left-wing majority or an undefined majority), and the left-wing vote share. The average debt is additional control for common shocks to the neighborhood, while the three intergovernmental grants variables controls for the impact that intergovernmental policy rules may have on the economic outcome. The reason for not controlling for these factors in the baseline specification is that some of these factors are likely to be endogenous and can therefore potentially bias the results. As can be seen from Table 8, the results with the extended set of controls are almost identical to the baseline results (for ease of comparison, the first row in Table 8 restates the results from the specifications in Table 4 and Table 6 with control variables), which give additional support for a causal interpretation of our results.

The second extension is to treat own lagged bailouts (B_{it-k}) as an instrumental variable instead of a control variable and to perform the four tests for instrument validity as we did for *neighbors* in the baseline case. The reason for conducting this exercise is the following. As argued earlier in the paper, it is doubtful whether one can use own past bailouts as an instrumental variable since there are likely to be omitted variables (e.g., persistent economic shocks) that might be correlated with both past bailouts and the outcome of interest, causing past bailouts to have a direct effect on the outcome variable. By applying the four different approaches for examining instrument exogeneity on B_{it-k}, the tests should indicate that it is not a valid instrument. If the tests are capable of detecting the likely problems with own past bailouts as an instrument, this lends further support to the baseline findings that *neighbors* are likely to be a valid instrument since it passes all four approaches for examining instrument validity. In other words, this can be interpreted as a test of power of the specification tests.

From the results in Tables 9-12, it is clear that the specifications tests are capable of detecting a problematic instrument. Starting with the results in Table 9, the first four columns show that the point estimates of the coefficient for bailout expectations varies quite dramatically (from -152 to 2685) when additional controls are added to the specification. From the last five columns of Table 9, it is clear that the instability of the point estimate for bailout expectations remains when longer lags are used as instruments. These results indicate that the instrument is not valid.

Further, and perhaps even stronger, support for the claim that the instrument is not valid is provided in Table 10. The point estimate obtained when conducting the refutability test is significantly different from zero (and negative!). Hence, there seems to be a direct link between the instrument and the outcome variable (debt) for a sample of municipalities with zero bailout expectations. Since the causal effect should be zero for this sample to be valid, we have a further indication that past own bailouts is probably not a valid instrument but is quite likely to pick up variation from unobserved economic shocks. The overidentifying restrictions tests in Table 11 and Table 12 reinforce the picture that own earlier bailouts are not valid instruments. However, the standard Sargan test for overidentifying restriction is not able to detect that instrument is not valid which illustrates the poor power properties of the Sargan test as discussed by Newey (1985).

Given that the four approaches in unison rejects own bailouts as an instrument, we feel even more comfortable with the earlier unison approval of neighbors' bailouts as a valid instrument.

5. Summary and concluding remarks

In this paper, we develop an empirical framework for estimating the causal effect of soft budget constraints on economic outcomes. The starting point is that the problem of the soft budget constraint is a problem of credibility, that is, inability of a supporting organization to commit itself not to extend more money (i.e., bailouts) *ex post* to a budget-constrained organization than it was prepared to provide *ex ante*. This means that current economic behavior of a budget-constrained organization will depend upon its *expectations* of being bailed out in the future. Thus, to estimate the causal effect of soft budget constraints (i.e., bailout expectations) on economic outcomes one has to measure these expectations and link those to the current behavior of the budget-constrained organization.

We argue that one can use information about realized bailouts to construct credible measures of bailout expectations and use an instrumental variable strategy to solve problems of measurement error and endogeneity associated with the proxy variable for bailout expectations. We suggest that one could construct a plausible instrument from information about bailouts among a budget-constrained organization's neighbors.

The empirical framework is applied to Swedish local governments, who provide an attractive testing ground of the soft budget constraint since the central government has extended a total of 1,697 bailouts over the period 1974 to 1992.

We find that bailout expectations have a causal effect on economic behavior. Specifically, the estimated effect, when the average number of bailouts of geographical neighbors is used as an instrument (conditional on past own bailouts and lagged debt) is insensitive to the inclusion of a powerful set of controls, namely municipality-specific covariates (unemployment, income, population, population density, age structure, rule-based fiscal transfers, and party controls), neighborhood-specific covariates (debt, unemployment, income, population, population density, and age structure), and time and municipality-specific fixed effects. We also tested if there is a direct effect between our instrument and the outcome for a subsample of municipalities where bailout expectations should be zero, namely those municipalities that have been bailed out no more than once during the sample period. It turns out that there is no relationship between the average number of bailouts of neighbors and debt in this sample. We also checked whether the point estimate was sensitive to different lags of the instrument, but it turned out not to be. Finally, different tests of overidentifying restrictions also supported the exclusion restriction.

As a robustness check, we also performed all the above specification tests when we used past own bailouts as an instrument. Since past own bailouts is likely not to be a valid instrument on *a priori* grounds (due to unobserved shocks), all the specification tests should be able to detect that this is the case for the baseline analysis to be trustworthy. It turns out that all specification tests reject that past own bailouts is a valid instrument.

Taken together, the results obtained in this paper constitute strong evidence that bailout of neighbors is likely to be a valid instrument. The estimated effect on debt from bailout expectations is also economically very large: on average, a local government increases its debt with 30 percent if it is certain of being bailed out as compared to when it is certain of not being bailed out.

While this work was motivated by and applied to the problem of SBC, the empirical approach here is quite general and could be applied to any situation where future expected policy affects current behavior, such as in other models of time inconsistency.

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Table 1. Classification of municipalities based on number of bailouts received over the period 1974-92.

Number of bailouts	Number of municipalities	Bailouts of neighbors (%)	Debt	Unemployment (%)	Income	Population	Population density	Age0-15 (%)	Age65+ (%)
0	23	12	9648	2.8	86444	64827	527	21.4	16.0
1	42	15	9165	3.1	75238	26896	103	21.6	17.1
2	33	25	9811	2.9	74249	28524	142	21.6	17.2
3	24	23	10644	2.9	75207	22504	132	22.7	16.0
4	12	26	11229	3.1	74916	29817	82	21.6	17.4
5	12	27	11785	3.6	75027	80869	245	21.1	17.3
6	21	33	10120	4.0	72213	18976	73	21.2	17.8
7	20	44	11312	4.2	74675	29915	31	20.5	17.9
8	16	41	10204	4.3	72940	20391	33	20.4	18.7
9	14	42	11488	3.5	73662	25054	37	20.9	18.3
10	13	50	12309	4.8	74181	27137	30	19.7	19.1
11	17	44	11605	4.3	72716	18279	20	19.9	20.0
12	8	42	13294	4.2	78583	29946	40	22.1	14.5
13	8	56	13549	4.7	72073	12156	20	19.1	20.8
14	6	46	12284	4.2	74418	42561	74	20.9	17.9
15	5	42	9386	4.6	71501	13633	9	19.7	20.1
16	2	37	16127	4.7	73771	31164	38	18.7	20.6
17	2	39	19565	5.4	74187	5349	1	19.2	20.6
18	5	63	11872	4.7	66712	11175	2	18.7	23.2
19	3	70	13589	3.9	63308	9089	2	19.3	22.7

Note- Average figures (within groups) are presented in the last seven columns. Debt and income are expressed in per capita terms and in 1991 prices.

Table 2 Information about the instrumental variable (*neighbors*)

	Number of observations	Percent of total observations
0.0< *neighbors* £0.1	1969	36.9
0.1< *neighbors* £0.2	644	12.1
0.2< *neighbors* £0.3	414	7.8
0.3< *neighbors* £0.4	626	11.7
0.4< *neighbors* £0.5	435	8.1
0.5< *neighbors* £0.6	146	2.7
0.6< *neighbors* £0.7	242	4.5
0.7< *neighbors* £0.8	227	4.2
0.8< *neighbors* £0.9	60	1.1
0.9< *neighbors* £1.0	569	10.6

Table 3. Summary statistics

	Mean	St. Dev	Min[a]	Max
Debt	10,218	4,808	797	38,024
Own bailouts, B_{it}	.318	.466	0	1
Neighbors, time t	.316	.335	0	1
		Municipality's characteristics		
Income	71,413	11,852	15,944	162,962
Proportion of young, 0-15	21.2	2.9	12.6	36.7
Proportion of old, 65+	17.5	4.3	3.3	27.7
Population size	29,699	52,403	2,924	681,318
Population density	113	367	0.3	3638
Unemployment	2.64	1.62	.19	12.23
		Neighborhood characteristics		
Income	71,527	10,783	0	123,192
Proportion of young, 0-15	21.0	2.4	0	32.0
Proportion of old, 65+	17.5	3.5	0	25.2
Population size	36,670	35,864	0	261,185
Population density	116	290	0	2432
Unemployment	2.64	1.41	0	9.94

[a] One municipality, the island of Gotland, has no neighbors.

Table 4. The effect of bailout expectations on debt using *neighbors* as instrument

	1	2	3	4	5	6	7	8	9
	IV: *t*-1	IV: t-1	IV: *t*-1	IV: *t*-1	IV: *t*-2	IV: *t*-2	IV: *t*-2	IV: *t*-3	IV: *t*-4
Bailout expectations	1894	2608	2358	2450	2825	2615	2839	2968	2833
	(365)	(906)	(1056)	(1366)	(1238)	(1881)	(2445)	(1598)	(1573)
	[736]	[687]	[946]	[1317]	[1028]	(1814)	[2442]	[1533]	[1564]
Lagged debt		.84	.74	.66	.84	.72	65	.65	.65
		(.01)	(.02)	(.03)	(.01)	(.03)	(.04)	(.3)	(.03)
B_{it-1}		-1249	-935	-822	-1231	-987	-880	-928	-875
		(508)	(400)	(488)	(631)	(716)	(806)	(538)	(534)
B_{it-2}					-341	-222	-220	-281	-281
					(218)	(132)	(179)	(143)	(142)
B_{it-3}								159	150
								(117)	(122)
B_{it-4}									34
									(123)
Municipality controls	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Neighborhood controls	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Unemployment (municipal and neighborhood levels)	No	No	No	Yes	No	No	Yes	Yes	Yes
Municipality effects	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year effects	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
First-stage *F*-statistic	24	60	33	20	54	14	9	20	22
	[6]	[50]	[31]	[21]	[53]	[18]	[11]	[20]	[23]
Number of observations	5063	5047	5047	3944	4763	4763	3937	3932	3925

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets. In the appendix, we present the first-stage and reduced-form estimates from regressions 2, 3 and 4.

Table 5. Refutability test

	Subsample: One or no bailouts	Fullsample
Neighbors, time *t*-1	18	334
	(332)	(135)
	[306]	[118]
B_{it-1}	Yes	Yes
Lagged debt	Yes	Yes
Municipality controls	Yes	Yes
Neighborhood controls	Yes	Yes
Municipality effects	Yes	Yes
Year effects	Yes	Yes
Number of observations	1126	5047

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 6. Overidentifying restrictions tests

	1	2	3
	Instrumental variables: *neighbors*, time *t*-1 and *t*-2		
Bailout expectations	3079	2841	2766
(Forward estimate)	(995)	(1118)	(1428)
	[751]	[1095]	[1434]
Bailout expectations	3114	2855	2767
(Reverse estimate)	(995)	(1190)	(1426)
	[747]	[1090]	[1441]
	.000321	.000350	.000361
	(.00010)	(.00015)	(.00019)
	[.00008]	[.00013]	[.00019]
Sargan test and	$c^2(1)=0.14$	$c^2(1)=0.03$	$c^2(1)=0.002$
p-value	(0.71)	(0.86)	(0.96)
First-stage *F*-statistic	31	15	10
Lagged debt	Yes	Yes	Yes
Municipality controls	No	Yes	Yes
Neighborhood controls	No	Yes	Yes
Unemployment (municipal and neighborhood levels)	No	No	Yes
Municipality effects	No	Yes	Yes
Year effects	No	Yes	Yes
Number of observations	4763	4763	3937

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 7. Alternative overidentifying restrictions test

	Instrumental variable: *neighbors*, time *t*-3	Instrumental variable: *neighbors*, time *t*-4
Bailout expectations	3037	2845
	(1898)	(1689)
	[1808]	[1718]
Neighbors, time *t*-1	-49	-19
	(294)	(272)
	[275]	[300]
B_{it-1}	Yes	Yes
B_{it-2}	Yes	Yes
B_{it-3}	Yes	Yes
B_{it-4}	-	Yes
Lagged debt	Yes	Yes
Municipality controls	Yes	Yes
Neighborhood controls	Yes	Yes
Unemployment (municipal and neighborhood levels)	Yes	Yes
Municipality effects	Yes	Yes
Year effects	Yes	Yes
Number of observations	3932	3925

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 8. Additional control variables

	1	2	3	4	5	6	7	8
	IV: *t*-1	IV: *t*-1	IV: *t*-2	IV: *t*-2	IV: *t*-3	IV: *t*-4	IV: *t*-1, *t*-2	IV: *t*-1, *t*-2
Bailout expectations	2358	2450	2615	2839	2968	2833	2841	2766
	[946]	[1317]	(1814)	[2442]	[1533]	[1564]	[1095]	[1434]
Bailout expectations (Additional controls)	2270	2245	2504	2545	2686	2569	2757	2540
	[977]	[1326]	[1909]	[2516]	[1527]	[1614]	[1127]	[1148]
B_{it-1}	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B_{it-2}	-	-	Yes	Yes	Yes	Yes	Yes	Yes
B_{it-3}	-	-	-	-	Yes	Yes	-	-
B_{it-4}	-	-	-	-	-	Yes	-	-
Lagged debt	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment (municipal and neighborhood levels)	No	Yes	No	Yes	Yes	Yes	No	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5047	3944	4763	3937	3932	3925	4763	3937

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 9. The effect of bailout expectations on debt using own bailouts as instruments

	1 IV: *t*-1	2 IV: t-1	3 IV: *t*-1	4 IV: *t*-1	5 IV: *t*-2	6 IV: *t*-2	7 IV: *t*-2	8 IV: *t*-3	9 IV: *t*-4
Bailout expectations	2685	427	-152	78	552	-472	-266	2126	2346
	(365)	(131)	(196)	(267)	(179)	(432)	(587)	(1264)	(5528)
	[490]	[110]	[219]	[276]	[161]	[461]	[598]	[1487]	[5918]
Lagged debt	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Neighborhood controls	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Unemployment (municipal and neighborhood levels)	No	No	No	Yes	No	No	Yes	Yes	Yes
Municipality effects	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year effects	No	No	Yes	Yes	No	Yes	Yes	Yes	Yes
First-stage *F*-statistic	2094	1947	517	319	967	128	76	20	1
	[1098]	[1053]	[430]	[231]	[528]	[106]	[59]	[17]	[1]
Number of observations	5048	5047	5047	3944	4763	4763	3937	3932	3925

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 10. Refutability test

	Subsample: One or no bailouts
B_{it-1}	-473
	(179)
	[166]
Lagged debt	Yes
Municipality controls	Yes
Neighborhood controls	Yes
Municipality effects	Yes
Year effects	Yes
Number of observations	1126

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 11. Overidentifying restrictions tests

	1	2	3
Instrumental variables: B_{it-1} and B_{it-2}			
Bailout expectations	500	-107	51
(Forward estimate)	(133)	(203)	(269)
	[112]	[204]	[281]
Bailout expectations	506	-479	624
(Reverse estimate)	(136)	(438)	(.00232)
	[125]	[467]	[.00230]
	.00198	-.00209	.00160
	(.00053)	(.00191)	(.00232)
	[.00045]	[.00204]	[.00230]
Sargan test and	$c^2(1)=0.21$	$c^2(1)=1.07$	$c^2(1)=0.45$
p-value	(0.65)	(0.30)	(0.50)
First-stage *F*-statistic	1369	259	164
	[947]	[236]	[130]
Lagged debt	Yes	Yes	Yes
Municipality controls	No	Yes	Yes
Neighborhood controls	No	Yes	Yes
Unemployment (municipal and neighborhood levels)	No	No	Yes
Municipality effects	No	Yes	Yes
Year effects	No	Yes	Yes
Number of observations	4763	4763	3937

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 12. Alternative overidentifying restrictions test

	Instrumental variable: B_{it-3}	Instrumental variable: B_{it-4}
Bailout expectations	5168	-44826
	(3858)	(766044)
	[4269]	[797104]
B_{it-1}	-1764	15565
	(1335)	(265393)
	[1467]	[275901]
Lagged debt	Yes	Yes
Municipality controls	Yes	Yes
Neighborhood controls	Yes	Yes
Unemployment (municipal and neighborhood levels)	Yes	Yes
Municipality effects	Yes	Yes
Year effects	Yes	Yes
Number of observations	3932	3925

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

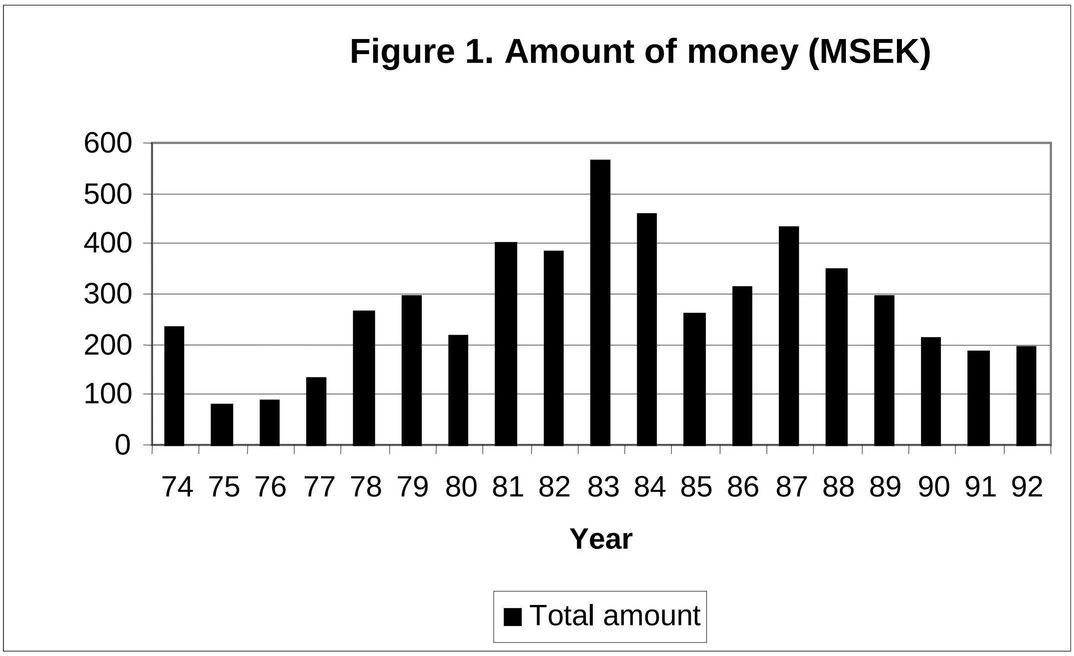

Figure 1. Amount of money (MSEK)

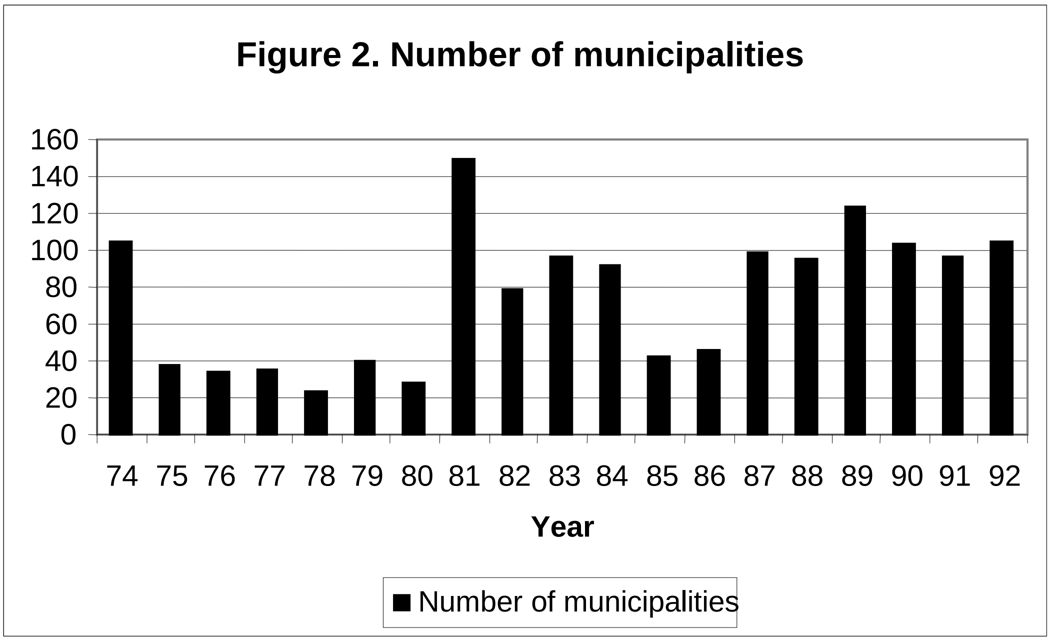
Figure 2. Number of municipalities
160
140
120
100
80
60
40
20
0
74 75 76 77 78 79 80 81 82 83 84 85 86 87 88 89 90 91 92
Year
Number of municipalities

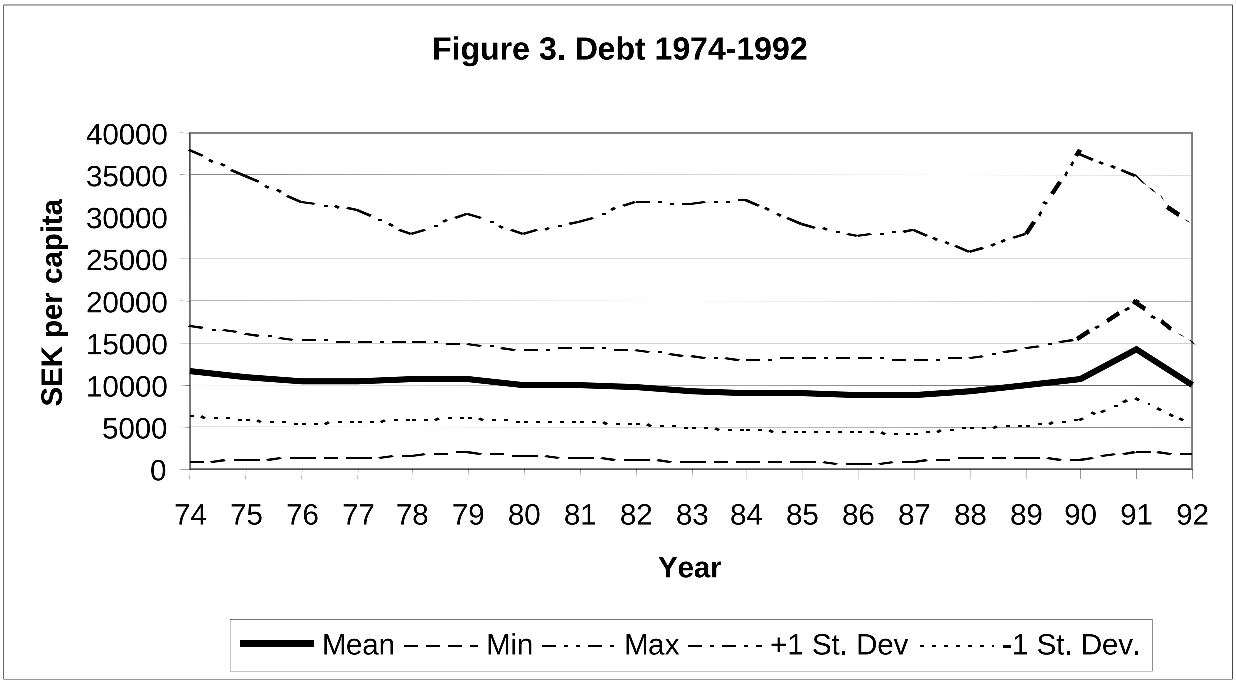
Figure 3. Debt 1974-1992
SEK per capita
40000
35000
30000
25000
20000
15000
10000
5000
0
74 75 76 77 78 79 80 81 82 83 84 85 86 87 88 89 90 91 92
Year
Mean
Min
Max
+1 St. Dev
-1 St. Dev.

Appendix

Table A1. First-stage and reduced-form estimates

	First-stage estimates			Reduced-form estimates		
	1	2	3	4	5	6
Bailneigbors, time *t*-1	0.152	0.142	0.133	396	334	327
	(0.020)	(0.025)	(0.029)	(129)	(135)	(164)
	[0.022]	[0.025]	[0.029]	[100]	[118]	[156]
Lagged debt	7.18e-06	1.18e-05	1.29e-05	0.87	0.76	0.69
	(1.06e-06)	(1.62e-06)	(1.96e-06)	(0.01)	(0.01)	(0.02)
	[1.29e-06]	[2.12e-06]	[2.56e-06]	[0.01]	[0.01]	[0.02]
Bailown, time *t*-1	0.51	0.34	0.32	90	-128	-49
	(0.02)	(0.02)	(0.02)	(77)	(76)	(98)
	[0.02]	[0.02]	[0.02]	[71]	[84]	[101]
Municipality's covariates						
Income		-2.93e-06	-2.07e-06		-0.0002	-0.02
		(1.59e-06)	(2.28e-06)		(0.01)	(0.02)
Proportion of young, 0-15		-0.012	0.006		75	-9.5
		(0.006)	(0.009)		(43)	(62)
Proportion of old, 65+		0.003	0.005		138	176
		(0.008)	(0.012)		(52)	(83)
Population size		-9.62e-07	7.00e-07		0.05	0.07
		(2.84e-06)	(3.83e-06)		(0.02)	(0.03)
Population density		-0.0004	-0.0005		-6.2	-6.4
		(0.0003)	(0.0003)		(2.4)	(2.9)
Unemployment (%)			0.014			-90
			(0.01)			(82)
Neighbors' covariates						
Income		-1.93e-07	2.16e-07		-0.03	-0.03
		(2.77e-06)	(3.91e-06)		(0.02)	(0.03)
Proportion of young, 0-15		0.05	0.07		146	274
		(0.01)	(0.01)		(68)	(107)
Proportion of old, 65+		0.05	0.06		186	318
		(0.01)	(0.02)		(91)	(145)
Population size		-2.05e-06	-2.06e-06		0.02	0.06
		(3.26e-06)	(4.09e-06)		(0.03)	(0.04)
Population density		0.00002	0.00001		-9.8	-16.3
		(0.00005)	(0.00006)		(3.7)	(4.6)
Unemployment (%)			-0.051			35
			(0.016)			(121)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Time effects	No	Yes	Yes	No	Yes	Yes
R^2	0.3546	0.5498	0.5649	0.7759	0.8599	0.8402
Number of obs.	5047	5047	3944	5047	5047	3944