An Empirical Approach for Evaluating Soft Budget Constraints[⊥]

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Abstract

In this paper, we develop an empirical framework for detecting the existence and estimating the magnitude of the softness of a budget constraint. The defining feature of a soft budget constraint is a subordinate organization's expectations of being bailed out by a superior organization in case of financial trouble. This implies that one has to link the organization's expectations for being bailed out to its fiscal behavior in order to quantify the extent of the soft budget constraint. We postulate that expectations for bailouts are formed rationally and make use of an instrumental variable method to get consistent estimates of the parameters of interest. We argue that past own experience of being bailed out and bailouts of other subordinate organizations can be used to construct credible instruments for the formation of bailout expectations. We apply our empirical approach to a unique panel data set of 286 Swedish local governments where the central government provided a total of 1,697 bailouts between 1974 and 1992. Our results strongly suggest the existence of a soft budget constraint; a local government increases its level of debt by 6-10 percent if it expects to be bailed out with probability one as compared to when the likelihood is zero due to previous experience of being bailed out, while the effect on debt from bailouts of its geographical neighbors is roughly four times as large.

Keywords: Soft budget constraint, Bailout, Fiscal distress, Intergovernmental relations

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1. Introduction

The concept "Soft Budget Constraint" (henceforth SBC) was first introduced by Kornai (1979, 1980) were it referred to a situation in which socialist firms were recurrently bailed out by state-agencies when revenues did not cover costs, but the concept is now being used in other branches of economics as well.[1] According to Kornai, Maskin, and Roland (2003), the SBC phenomena is characterized by a vertical relationship of superiority and subordination between two organizations where the subordinate organization faces a budget constraint, i.e., it must cover its expenditures out of its initial endowments and own revenues. The problem of the SBC is said to occur when the superior organization is ready to provide financial support to its subordinate in case of financial trouble. In other words, if a subordinate organization can expect to be rescued from trouble and those expectations in turn affect its behavior, then the budget constraint is soft.

In this paper, we develop an empirical framework for detecting the existence and estimating the magnitude of the SBC. The approach is then applied to the relationship between the central and local governments in Sweden. The relationship between central and local governments is an attractive testing ground of the SBC since it has often been argued that if the central government is expected to provide bailouts to fiscally distressed sub-national governments, the incentives of sub-national governments to respect the fiscal discipline imposed by their budget constraints may be weakened. Swedish local governments were bailed out recurrently over the period 1974-1992, making it possible that they expected to be rescued from financial trouble, which, in turn, might have had deteriorating effects on their budget discipline.

The SBC can be modeled formally as a simultaneous equation model, where one equation describes the behavior of the subordinate organization (e.g., a local government) and another equation describes the behavior of the superior organization (e.g., the central government). As is well-known, one needs exclusion restrictions i.e., certain variables need to be excluded from some equations (i.e., instrumental variables), to be able to

[1] For example, SBCs have been used to analyze financial systems structure (Dewatripont and Maskin, 1995) and federalism (Qian and Roland, 1998).

identify the parameters of such a model. Ideally, one would like to be able to identify all the parameters in both equations. However, this would require, for example, that we could specify all the relevant motives for a superior organization to provide bailouts to its subordinate organizations; a formidable challenge considering the plethora of possible motives identified in the SBC literature.[2] Our approach is instead to focus on the equation describing the subordinate organization fiscal behavior since it is the key in quantifying the degree of softness of the budget constraint, namely linking the subordinate's fiscal behavior to its expectation of being rescued from financial straits. Nevertheless, the equation describing the bailout behavior of the superior organization will provide us with the necessary instruments for the formation of bailout expectations. In other words, we will use an instrumental variable (IV) approach where the fiscal behavior of a subordinate organization is the structural equation of interest, while the reduced form equation is a linear projection of realized bailouts on instruments. This estimating procedure also implies that expectations for bailouts are formed rationally.

The key of the IV method is to find valid instruments. We argue that both past own experience of being bailed out and bailouts of other organizations in the same part of the economy can provide attractive instruments. The use of these instruments can also be motivated from theoretical models stressing the link between SBCs and dynamic commitment,[3] i.e., the superior organization would not wish to commit itself contractually to provide support; its incentive to bail the subordinate organization out arises only ex post. Interpreting the SBC problem as arising from the inability of the superior organization to make dynamic commitments suggests that one can use information about previous bailouts in constructing instruments since they should contain information about the superior organization's capability of making such commitments. Put differently, previous bailouts provide the source of variation used to identify the effect of expectation of bailouts on the subordinate organization's fiscal behavior.

The main finding is that the SBC problem exists and that it is sizeable; the estimates of past own experience indicate that a local government facing a probability of

[2] See Kornai et al. (2003) for a discussion of motives.
[3] See, for example, Dewatripont and Maskin (1995).

one of being bailed out increases its debt with approximately SEK 700-1,000 per capita as compared to if the probability is zero, which corresponds to 6-10 percent of average debt. However, the estimate of the effect on debt from bailouts of geographical neighbors is at least four times as large as the effect from past own experience.

The outline of the paper is as follow. In the next section, we present the empirical approach for evaluating soft budget constraints, while section 3 discusses the details of the empirical application. Section 4 presents the empirical results and section 5 summarizes and gives some concluding remarks.

2. Empirical framework

In this section, we develop an empirical approach for evaluating soft budget constraints. In doing so, we have to consider the following issues: (i) the link between a subordinate organization's expectations for bailouts and its fiscal behavior, (ii) the formation of a subordinate organization's bailout expectations, (iii) the subordinate organization's information set, and (iv) estimation. These issues will, in turn, be discussed in the next four sub-sections.

2.1 Linking bailout expectations to fiscal behavior

The major challenge of an empirical investigation of the SBC is to link the fiscal behavior of a subordinate organization to its expectation of being rescued from financial straits, since "[i]f a bailout is entirely unanticipated there is little point in ascribing the event to a SBC. We normally say that the syndrome is truly at work if organizations can *expect* to be rescued from trouble, and those expectations in turn affect their behavior" (Kornai et al., 2003). We therefore assume that a subordinate organization's fiscal behavior can be expressed as

$$S_{it} = \beta_0 + \alpha B_{it}^e + X_{it}\beta + u_{it}, \quad (1)$$

where S_{it} is an observed measure of fiscal behavior, such as the level of debt, in subordinate organization i in time period t, B_{it}^e is expectations for bailouts as perceived by the subordinate , and X_{it} is a vector of observable variables that might be related to both S_{it} and B_{it}^e.

The parameter of interest is α, which measures the effect of expectations for bailouts on fiscal behavior, or the degree of softness of the budget constraint. Equation (1) should be interpreted in counterfactual terms, that is, for given X_{it} and u_{it}, it gives the optimal fiscal response for *any* possible degree of expectations for bailouts faced by a subordinate organization. Thus, the size of α will be the causal effect on S_{it} of going from a zero probability to a probability of one of being bailed out (i.e., $\alpha = \mathrm{E}(S \mid B^e = 1, \mathrm{X}) - \mathrm{E}(S \mid B^e = 0, \mathrm{X})$. If we cannot reject the null hypothesis of no effect, the conclusion is that there does not seem to be a problem of SBC.

2.2 The formation of bailout expectations

A problem with equation (1) is that we cannot directly estimate it since expectations are not directly observed. Thus, we need to model the formation of bailout expectations.[4] Postulating rational expectations is the most common assumption in the literature estimating econometric models that includes an unobservable expectations variable. Thus, by assuming that expectations are formed rationally, i.e.,

$$B_{it}^{e} = E\left[B_{it} \mid I_{it-1}\right], \tag{2}$$

where I_{it-1} is the information available to organization i at time $t-1$, equation (1) becomes estimable.

There are, basically, two methods of estimation for rational expectations models as described by equations (1) and (2): The "substitution method" (SM) or the "errors-in-variables method" (EVM).[5] In the SM, the rationally expected variable, i.e., $E\left[B_{it} \mid I_{it-1}\right]$, is replaced by a forecast $\hat{B}_{it}$, whereas the EVM is an Instrumental Variable (IV) method, which replaces the expectational variable by its observed realization, thereby creating a classical measurement error, i.e., $B_{it} = B_{it}^{e} + v_{it}$, and where the factors included in the formation set I_{it-1} provide the instrumental variables since they by assumption will be uncorrelated with the forecast error v_{it}.

In this paper, we will use the EVM to estimate the subordinate organization's bailout expectations. The reason for this is that the EVM offers several attractive features compared to the SM in the present context of making causal inference of the relation between the expectation for bailouts and fiscal behavior, i.e. to consistently estimate the parameter α.

First, by using EVM instead of SM, we do not have to model the motives for the superior organization to provide bailouts since EVM only requires a valid instrumental variable, whereas SM hinges on that we can *correctly* specify the conditional

[4] An alternative to modeling the formation of bailout expectation is to elicit expectations through survey data. This is the method used by Anderson, Korsun, and Murell (2000). The potential problem with this method is whether subjective data reliably reflect respondents' thinking and how to get comparable measures of expectations across respondents. See Dominitz and Manski (1997) for a discussion of the problem of using survey data to measure expectations.

[5] The literature on the estimation of rational expectations models is large. For example, see Lahiri (1993), McCallum (1976), Nelson (1975), Pagan (1984, 1986), Wallis (1980), and Wickens (1982).

expectation $E[B_{it} \mid I_{it-1}]$. Thus, the use of EVM greatly simplifies the investigation of the SBC problem considering the plethora of possible motives for the superior organization to provide support. Nevertheless, the robustness of the EVM to misspecifications comes at a cost. In the case we are interested in the reasons for the superior organization to provide bailouts we would have to model $E[B_{it} \mid I_{it-1}]$ explicitly in order to make causal statements.

A second attractive feature with EVM compared to SM is that we do not need to get the functional form of the first stage right, in the IV approach, to obtain consistent estimates of α in equation (1). Kelejian (1971) shows that it is unnecessary to obtain consistent estimates of the parameters of the reduced form equation in order to consistently estimate a structural equation. Thus, we can estimate a simple linear probability model in the first stage of the IV method. As will become clear below, the EVM approach allows us to circumvent the notorious difficult problem of estimating dynamic nonlinear panel data models with unobserved heterogeneity as discussed by Heckman (1981) and Honoré and Kyriazidou (2000) among others.

A third attractive feature of EVM is that the standard errors will automatically be valid since the IV procedure uses realized bailouts, not the predicted ones, when computing the standard errors. If the SM is used instead, the standard errors typically need to be corrected, as discussed by Pagan (1984) and Murphy and Topel (1985).

2.3 The subordinate organization's information set

The key question now is to determine what factors are relevant for the formation of bailout expectations (i.e., I_{it-1}) since those will function as instruments in the EVM method.[6] As was noted in the introduction, interpreting the SBC problem as a dynamic commitment problem suggests that we could construct instruments using information about previous bailouts since they should contain information about the superior organization's ability to dynamically commit.

[6] We can use any variable dated at *t-1*, provided that it is actually used by the subordinate organization when forming bailout expectations, as instruments since the rational expectations hypothesis rules out the possibility of any relationship between the prediction error v_{it} and the organization's information set I_{it-1}

Logically, there are two ways that previous bailouts could affect the expectations for bailouts, namely either by previous own experience of receiving bailouts or by observing that other subordinate organizations receive bailouts.[7] To take both channels into account in the estimation of the effect of bailout expectations on fiscal behavior, we re-specify the bailout expectations by replacing the term αB_{it}^{e} in equation (1) with the following expression:

$$\alpha^{p} B_{it}^{e(p)} + \alpha^{c} B_{it}^{e(c)} \quad , \tag{3}$$

where $B_{it}^{e(p)}$ is the expectations for bailouts based only on past own experience, and $B_{it}^{e(c)}$ is the expectation for bailouts based on observing bailouts of other organizations. In other words, the parameter α^{p} should be interpreted as the effect of expectations on fiscal behavior as originating from private incentives, that is, from a purely private calculus of decision making ignoring the effects from other organizations, whereas the parameter α^{c} would be interpreted as the effect of expectations on fiscal behavior due to bailouts of other organizations while holding private incentives fixed.

We need to explicitly measure how previous bailouts affect bailout expectations. The simplest approach is to assume that expectations are formed based only on information about bailouts in period *t-1*. Thus, the instrument for own previous bailouts would then be defined as an indicator variable taking the value 1 if subordinate organization *i* was bailed out in period *t-1*, and zero otherwise ($B_{i,t-1}$).

To construct an instrument for the expectation for bailouts as caused by bailouts of other subordinate organizations, we also need to define how subordinates form reference groups, i.e., to define the particular group that has an impact on the formation of bailout expectations for a specific subordinate organization. In the SBC, it seems

[7] Kornai et al. (2003) argue that expectations of bailouts have much to do with "collective experience" since "[t]he 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." This type of explanation is related to the literature about externalities, group effects, neighboring effects, also known as social interactions effects (see, e.g., Brock and Durlauf, 2001, and the references cited therein). The goal of this literature is to provide explanations of group behavior, which emerges from interdependencies across agents, where the notion of agents embraces persons, firms and other entities such as nonprofit organizations and governments. These interdependencies or social interactions across agents can be modeled in a number of different ways. In the most general set-up, the actions, preferences, expectations, and constraints of one agent are permitted to affect the actions, preferences, expectations, and constraints of another agent.

reasonable to use a geographical definition of reference group since this capture the idea that geographical neighbors belong to the same media market and that they therefore have good information about whether neighbors have received a bailout or not.[8] Thus, we will assume that expectations of bailouts in period t depend on the average number of bailouts received by its geographical neighbors in period $t\text{-}1$,[9] i.e., $\overline{B}_{J,t-1}=\frac{1}{J}\sum_{j} B_{jt-1}$, where J is the number of geographical neighbors and $B_{jt\text{-}1}$ is an indicator variable with value 1 if neighbor j received a bailout in period $t\text{-}1$, zero otherwise.

2.4 Estimation

To identify the effects on fiscal behavior from the two channels of bailout expectations separately, we could use the method of indirect least squares.[10] We would then estimate two reduced forms: one OLS regression of debt on the two instruments and other exogenous covariates, and another OLS regression of realized bailouts on the same set of instruments and exogenous covariates. More formally, the two estimating equations in the indirect least squares method are

$$B_{it} = \pi_0 + \pi_1 B_{i,t\text{-}1} + \pi_2 \overline{B}_{J,t-1} + X_{it}\beta + X_{Jt}\delta + \varepsilon_{it} \tag{4}$$

$$S_{it} = \theta_0 + \theta_1 B_{i,t\text{-}1} + \theta_2 \overline{B}_{J,t-1} + X_{it}\theta + X_{Jt}\varphi + \eta_{it}. \tag{5}$$

[8] Besley and Case (1995), for example, use a geographical definition of reference group in their empirical analysis of social interaction effects in the form of tax competition between neighboring U.S. States.

[9] To understand why this particular source of variation is attractive for identifying group or social interaction effects in general, it is useful to approach the question of identification by asking whether there is any randomized trials of policy interventions that could in principle identify social interaction effects. This approach is discussed by Moffitt (2001), which suggests that one could identify social interaction effects by conducting partial-population experiments, whereby only a portion of the agents within each group is given treatment. In the present context of the SBC, the superior organization makes policy interventions by providing financial help to subordinate organizations. In other words, the source of variation used here, bailouts of geographical neighbors, is a non-experimental counterpart to the partial-population experiment suggested by Mofitt. The explicit connection to randomized trials also helps in assessing whether a particular source of variation is likely to be exogenous. The identifying restriction implied by the partial-population experiment is that the fiscal behavior of a subordinate organization is not *directly* influenced by the bailouts of its neighbors. For example, this identifying restriction would be violated if geographical neighbors face similar shocks and we do not control for such effects in the empirical analysis since otherwise we might wrongly attribute behavioral significance to any correlation between a organization's fiscal behavior and its expectations of being bailed out that is really due to correlated shocks across organizations.

[10] If we think that $\alpha^p = \alpha^c$, we could estimate the model via two-stage least squares to get more efficient estimates.

where X_{it} is organization-specific covariates, and X_{Jt} is group-specific covariates.

Using the logic of indirect least squares, the parameters of equation (3) would be identified since $\hat{\theta}_1 / \hat{\pi}_1$ is an estimate of α^p and $\hat{\theta}_2 / \hat{\pi}_2$ is an estimate of α^c. Here it is interesting to note that equation (5) may have a causal interpretation on its own if one is willing to assume that the subordinate organization formed its expectations through naïve instead of rational expectations. Thus, $\hat{\theta}_1$ and $\hat{\theta}_2$ could directly be interpreted as estimates of α^p and α^c respectively in the case organizations have naïve expectations.

The method of indirect least squares has however a drawback since it does not automatically produce standard errors. But there exist an alternative estimating procedure that will both give the estimates of α^p and α^c associated with the rational expectations hypothesis, as well as their standard errors. The procedure is basically an IV-method, where the first-stage would be the OLS regression on equation (4), but the second-stage regressions for the IV procedure will be different depending on the parameter of interest Here it is important to note that equation (4) does not necessarily have a causal interpretation since it is only a linear projection of an endogenous variable on instruments and other exogenous covariates. As discussed previously, one of the attractive features of using an IV method is that we do not need to consistently estimate the parameters of the first stage, which turns out to be quite useful here since this equation constitutes a dynamic nonlinear panel data model with unobserved heterogeneity.[11]

To isolate the independent effect from each instrument on debt we will include one of them as an additional regressor in the second stage of the two-stage least squares procedure. For example, in the case of isolating the independent effect of past experience on fiscal behavior (i.e., the parameter α^p), the second stage regression is given by

$$S_{it} = \delta \hat{B}_{it} + \omega \overline{B}_{Jt-1} + X_{it}\beta + X_{Jt}\delta + \varepsilon_{it}, \tag{6}$$

Now $\hat{\delta}$ would be an estimate of α^p. The reason for including the other instrument, $\overline{B}_{Jt-1}$, as a regressor in the second stage is that we need to isolate the effect on fiscal behavior from expectations of bailouts that is only due to past own experience. The logic of this

[11] As noted earlier, to construct consistent estimates of equation (4) is known to be notoriously difficult.

procedure can perhaps most easily be explained by referring back to equation (5), the reduced form regression of debt on the instruments. There we discussed that the direct effect of the instruments on debt may have a causal interpretation in the case that subordinate organizations have naïve expectations. Here, we are interested in estimating the parameters α^p and α^c when the rational expectations hypothesis is postulated instead. We must therefore hold the influence from the other channel of bailout expectations fixed. In order to do that we exclude the instrument associated with the effect of interest in the second stage regression, while allowing for the included instrument to have a direct effect on debt. This repeated IV approach is an alternative to the indirect least square method.

To summarize, the empirical approach is to link a subordinate organization's expectations for being bailed out to its fiscal behavior in order to quantify the extent of the bailout problem. We postulate that expectations for bailouts are formed rationally and make use of an instrumental variable method to get consistent estimates of the parameters of interest. We argue that past own experience of being bailed out and bailouts of other subordinate organizations can be used to construct credible instruments for the formation of bailout expectations. Next we turn to an empirical application of the approach.

3. Empirical application

To be able to make use of our empirical approach of linking subordinate organizations' expectations of bailouts to their fiscal behavior, it is necessary to have data on realized bailouts of these organizations, across organizations as well as over time. For this reason, we will apply our empirical approach of the SBC problem to the relationship between central and local governments in Sweden, where local governments (municipalities) recurrently received financial support from the central government over the period 1974-1992.

Before turning to a description of the bailout program, it might be worth stressing that bailout of local governments is 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, i.e., bailouts, from the central government.[12] 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.[13]

3.1 The bailout program

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, these municipalities received SEK 166 per capita (with a standard deviation of 224).[14]

There are two features of the Swedish financial relief program that makes it attractive for studying the SBC problem. First, 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

[12] The perhaps best-known example is the bailout of the city of Sao Paolo in Brazil in the 1990s.
[13] See, e.g., the forthcoming books from the Inter-American Development Bank (edited by Fernandez-Arias, von Hagen, and Stein, 2002) and from the World Bank (edited by Rodden and Eskeland, 2003).
[14] $1 dollar is roughly equal to SEK 6 (in 1991year prices).

transfer schemes are to a large extent heavily regulated or rule based. In contrast, this financial relief program was at the central governments discretion, and the central government had to make a new decision of the distribution of the relief grants each year. Second, the financial relief program was targeted explicitly to financially distressed municipalities. These particular attributes of the financial relief program facilitates an empirical investigation of the bailout problem since the program provided similar incentives as in the bailout problem: Municipalities can expect to be rescued from trouble and this can have a negative incentive effect on their budget discipline.

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 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.[15] 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.[16] Typically, these municipalities claimed that 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 out-migration from the municipality, high unemployment rates, and, mainly as a consequence of the first two, a deteriorated tax base. 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 effects due to a change in the tax system and for reducing high income tax rates in certain municipalities.

[15] 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.
[16] 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.

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 bailout 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 bailout problem and therefore all the fiscal transfers from this program should contain valuable information about such expectations.

Figures 1-3 describe our bailout data in greater detail. 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 (the standard deviation is 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 a quite a large variation in the number of recipients, the average number being 90, with a minimum of 28 and a maximum of 173.

As discussed in section 2, we are interested in the effect of both past own experience and bailouts of other local governments on fiscal behavior. One way of describing the variation that will be used to identify past experience is to create a frequency distribution, i.e., histogram, of bailouts across municipalities. Figure 3 shows the frequency distribution of bailouts based on information from the total number of bailouts each municipality has received over the period 1974-1992. This figure reveals a large variation in the number of bailouts across municipalities. For example, 3 municipalities (approximately 1 percent of the whole sample) received the maximum number of bailouts (19) whereas 23 municipalities (approximately 8 percent of the total sample) did not receive any bailouts at all. The average number of bailouts during the period is 6, which also roughly corresponds to the median number of bailouts.

In the case of measuring the effect of bailouts of other local governments on fiscal behavior, we assume that a municipality form use geographical neighbors as the reference group when forming its bailout expectations. The Swedish municipalities have,

on average, 4.95 neighbors; the minimum is zero (1 municipality, the island of Gotland, has no neighbors; 7 municipalities have only one neighbor), and the maximum is 10 (1 municipality has 10 neighbors). The reference group received on average 1.6 bailouts, but the variation is large (the standard deviation is 1.8).

3.2 Measure of fiscal distress

As measure of fiscal distress, we will use the level of local government debt, measured in per capita terms and at constant prices.[17] Since Swedish local governments have no restrictions on borrowing and do not meet any balanced budget rules, debt seems to be a suitable measure of fiscal discipline. 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.[18] 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 4 provides information on how the average level of debt per capita has evolved during the period 1974 to 1992. Figure 4 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.

3.3 Econometric considerations

When taking the model defined in equations (4) and (5) to the data, it is important to deal with confounding effects such as heterogeneity and correlated shocks in the econometric specification in order for the expectations for bailouts to be causally related to local government behavior.

This is not the least important when it comes to own past experience of bailouts, since any differences in observed fiscal behavior could also simply reflect some unobserved heterogeneity across local governments, and that this heterogeneity also is

[17] 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.
[18] Long-term debts are defined as debts with a maturity of 1 year or longer, while short-term debts have a maturity of up to 1 year. Data on social security liabilities are only available from 1988.

responsible for the variation in observed bailouts. For example, there may be correlated shocks across time that could induce a positive correlation between measures of fiscal distress and previous bailouts. There could also be local government specific characteristics that could produce such a correlation. In other words, we need to control for observable shocks and unobserved heterogeneity in order to identify whether past experience of bailouts have a causal effect on local government fiscal behavior.

To control for unobserved heterogeneity we include fixed locality and time specific effects. The fixed locality effects will pick up any *unobserved* and unchanging characteristics of a local government that are both related to its fiscal behavior and the variables describing the formation of expectations. The time specific effects will pick up any unobserved macro effects that affect all local governments in the same way. There may also be idiosyncratic shocks that could induce spurious correlation between the instruments and S_{it} in equation (1). To control for such shocks, we include time varying local specific measures of the tax base, the unemployment rate, the age structure, the population size, and the population density. Some of these variables, such as population density and age structure, are also meant to capture the cost of providing public services.[19]

In the case of isolating the effect on fiscal behavior from bailouts of other local governments, we must also control for unobserved heterogeneity.[20] Since the reference group is fixed, and since we control for fixed municipality effects, any unobserved time invariant characteristics of the municipality's reference group will not be part of the identification of the neighboring effect. We will also include reference group (geographical neighbors) characteristics, i.e., group means of the set of covariates described above. These reference group specific characteristics thus controls for observerable correlated shocks within reference groups.

A final issue in the context of a panel data model with rational expectations is whether the instruments can be considered as strictly exogenous or if they are only

[19] Table 1 provides summary statistics of the variables used in the data analysis. All the data was obtained from Statistics Sweden or its publications.
[20] See Manski (1993) for a discussion of identification of social interaction effects.

sequentially exogenous.[21] This concern has to do with whether one should use a fixed-effect (FE) transformation or a first-difference (FD) transformation to remove the fixed municipality effect μ_i. This issue would be of no importance if the number of time periods T is very large since then either of the transformations would give consistent estimates. If T is small, however, and the instruments are only sequentially exogenous, the FE estimator would produce biased estimates whereas the FD estimator would still yield consistent estimates. Therefore, we will use both the FE estimator and the FD estimator.

3.4 Institutional background

Before we present the results, 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.[22] Moreover, during the period of investigation (1974-1992), the bulk of revenues were raised trough a proportional income tax, which each municipality was allowed to set freely,[23] and only 20 percent of the total revenues came from intergovernmental grants.

[21] See Lahiri (1993) or Wooldridge (2002, chapter 11) for a discussion of the assumptions for the IV approach to be valid in the context of panel data.
[22] As from year 2000 there is a balanced budget rule.
[23] From 1991 to 1993, however, the central government imposed a temporary tax cap.

4. Results

In this section we present the results for the SBC problem, that is, to what extent local governments' expectations for bailouts affect their debt behavior. As discussed in section 2, our empirical approach is to assume that expectations for bailouts are formed rationally and to use ex-post realized bailouts as a proxy of the unobserved expectation for bailouts. This approach implies that we have an errors-in-variable problem, which necessitates the use of an instrumental variable approach in order to give consistent estimates of the effect of the unobserved expectation variable. The two instruments that we argued could have an affect on the formation of bailout expectations is past experience, measured as an indicator variable defining whether a municipality was bailed out or not in the previous period, and bailouts of other municipalities, measured as the average number of bailouts received by a municipality's geographical neighbors in the previous period. We start by presenting the baseline results, which is then followed by a sensitivity analysis.

4.1 Baseline results

Before turning to the estimates of the causal parameter of interest, i.e., the effect of local governments' expectations for bailouts on debt behavior, we begin by presenting the reduced form estimates, i.e., estimates obtained from the estimation of equations (4) and (5).

Reduced form estimates

The results from the reduced form estimations are presented in Table 2. The first three columns show the reduced form estimates of the instruments on realized bailouts, while the last three columns show the reduced form estimates of the instruments on debt. We will also control for other variables that could induce a spurious relation between debt and the expectations for bailouts. In the most parsimonious specification, model I, we only include time- and municipality specific fixed effects. Model II includes all the other municipality specific covariates except municipal unemployment together with the means of the geographical neighbors' covariates. Model III also adds the unemployment rate to the set of controls.[24] The first two rows show that both instruments have a positive and

[24] The reason for excluding the unemployment variable in model II is that it is only available for the shorter period 1979 to 1992.

significant association with realized bailouts in all three specifications. From both the *t*-statistics and the *F*-statistics of the partial *F*-tests presented in Table 2, it is clear that we can strongly reject the null hypotheses that the two instruments are either individually or jointly zero. Thus, we do not seem to suffer from the problem of weak instruments.[25]

The specifications presented in the last three columns of Table 2 are the reduced form OLS regressions of debt on the instruments. Both past own experience and bailouts of neighbors are significantly associated with an increase in debt. As noted above, this reduced form can have a causal interpretation if local governments use naïve expectations when forming their expectation of bailouts. Therefore, we can interpret the point estimates as the effect on debt of going from a zero probability to the probability of one of being bailed out. Thus, the effect from past own experience of bailouts is in the range of SEK 240-330 per capita, which is roughly 2-3 percent of the average debt (the average debt is SEK 10,218 per capita). The effect from bailouts of geographical neighbors is in the order of SEK 500-1,250 per capita, which constitute approximately 5-13 percent of average debt. However, if local governments have rational expectations, these estimates will be biased toward zero since these variables will effectively be proxies of the true bailout expectation. Here we can use the logic of indirect least square to get the consistent estimates. For example, taking the estimate of past experience of bailouts from Model 1 in column 4 (i.e., 329) and dividing it with the estimate from column 1 (i.e., 0.356), we will get the rational expectation estimate from past experience, which is SEK 924 per capita. A similar calculation for the effect from bailouts of neighbors on debt gives SEK 6,890 per capita. The estimates from Models II and III are 695 and 1,009 for past own experience, and 4,123 and 3,603 for bailouts of neighbors. The problem with the indirect least square method is that it does not automatically give standard errors. Therefore we now turn to the repeated instrumental variable method discussed in section 2.

[25] The reduced form estimates of the instruments on the endogenous variable, i.e., the first stage regression in the IV approach, provide information about the relevance of the instruments. Staiger and Stock (1997) suggest using the *F*-statistic for the joint significance of the instruments in the first stage equation as a diagnostic of the power of the instruments. They argue that if the *F*-statistic is larger than 10, there should be no problem associated with weak instruments.

Instrumental variable estimates

Table 3 shows the structural estimates of the effect of the municipalities' expectations for bailouts on the municipalities' debt behavior. The result in each cell in the table is the outcome of a separate instrumental variable regression. The covariates used, but not reported, in each regression corresponds to those for models I-III in Table 2. We present the separate effects of past own experience of bailouts and bailouts of geographical neighbors on debt. This is technically done by excluding one instrument at the time while holding the other instrument fixed, as discussed in section 2.4 (c.f. equations (4) and (6) for the case of isolating the independent effect of past experience on debt).

The first row in Table 3 shows the independent effect of past experience of bailouts, whereas the second row shows the independent effect of bailouts of geographical neighbors. The estimates of past experience are statistically significant in all three specifications. The point estimates indicate that a municipality that has a probability of one of being bailed out increases its debt with approximately SEK 700-1,000 per capita compared to if the probability is zero, which corresponds to 6-10 percent of average debt. Turning to geographical neighbors, the estimated effect is also significant in all specifications and the corresponding effect on the debt varies between SEK 3,600-6,900 per capita, i.e., 10-20 percent of the average debt. Table 3 also reveals that the size of the effect from neighbors is consistently larger, roughly four times larger or more, than the effect from past experience.

So far we have used a fixed effect transformation of the data, but if the strict exogeneity assumption fails, then the within, or fixed effect (FE), transformation to remove the fixed effect is problematic unless the number of time periods (T) is large, as discussed in section 3.3. Under a FD transformation, the instruments must be lagged at least twice to be valid.[26] Thus, the cost of the FD instead of the FE transformation is that we loose more information in the data, implying that we will end up with less precise point estimates. The FD estimates are presented in Table 4, which should be compared to the corresponding estimates in Table 3. Those two tables reveal quite similar estimates across the two IV estimators, but, as expected, the FD estimates are less precisely

[26] This IV-estimator is similar to the one proposed by Anderson and Hsiao (1982) in a dynamic panel data context.

measured. The large degree of similarity of the estimates across the two IV methods is also an indication of the instruments being strictly exogenous (Hausman, 1978).

4.2 Sensitivity analysis

In order to investigate how sensitive the results obtained in the last section are to changes in the model specifications, we will in this section examine some alternative specifications of the baseline model.

In the baseline specification, we assumed that the expectations were formed based only on information in *t-1*. However, it is likely that information further back in time, such as period *t-2* or *t-3*, also counts in the formation of expectations. To test the sensitivity to this, we re-estimated the baseline model with other lag structures on the instruments. These results, presented in Table 5, should be compared to the baseline estimates in Tables 3 and 4 (to facilitate comparisons, the first row in each panel simply restate the results obtained in Tables 3 and 4).

The first panel in Table 5 shows the effect of past experiences of bailouts on debt. It turns out that the estimates from the specifications of all three models are very stable when adding more lags of the instruments, which is true for both the fixed effects and the first differenced estimations. Furthermore, the estimates obtained when the identifying variation comes from past bailouts of neighboring municipalities, presented in the second panel, are also very similar to the baseline specification. Thus, it seems like our results are robust to changes in the lag structure of the instruments.

So far we have assumed that the effects on debt from the two channels of bailout expectations are different, and we used an iterated instrumental variable method. However, if these effects are similar (i.e., $\alpha^p = \alpha^c$ in equation (3)) we could instead use a traditional two-stage least squares (2SLS) method to estimate a single parameter, and thus getting a more efficient estimate. Table 6 shows the 2SLS estimates for all three specifications and for both the FE and FD transformations of the data. As is clear from the table, the estimated effect of a municipality's expectations of being bailed out on its debt is both statistically significant and economically sizeable.

5. Summary and concluding remarks

In this paper, we develop an empirical approach for investigating soft budget constraints. The approach is to link a subordinate organization's expectations for being bailed out to its fiscal behavior in order to quantify the extent of the bailout problem. We postulate that expectations for bailouts are formed rationally and make use of an instrumental variable method to get consistent estimates of the parameters of interest. We argue that past own experience of being bailed out and bailouts of other subordinate organizations can be used to construct credible instruments for the formation of bailout expectations. The use of these instruments can be motivated from theoretical models stressing the link between soft budget constraints and dynamic commitment.

We apply the empirical approach of the soft budget constraint problem to the relationship between central and local governments in Sweden, where local governments (municipalities) recurrently received financial support from the central government over the period 1974-1992. We assume that a municipality bases its expectation on own past experience of bailouts and bailouts of its geographical neighbors, and use this information to construct instrumental variables. The use of an instrumental variable approach implies that we do not have to correctly specify all the information used by municipalities when forming expectations. The only requirement is that they actually do use past own experience and bailouts of neighbors to form bailout expectations.

The main finding is that the SBC problem exists and is sizeable; the estimates of past own experience indicate that a local government that has a probability of one of being bailed out increases its debt with approximately SEK 700-1,000 per capita compared to if the a probability is zero, which corresponds to 6-10 percent of average debt. However, the estimate of the effect on debt from bailouts of its geographical neighbors is at least four times as large as the effect from past own experience.

We also made a number of robustness checks on the sensitivity of our results to changes in the information set, i.e., to different lag structures of the instruments, and to changes in estimation method. The baseline results were very robust to these alterations. In the estimations, we have also tried to control for unobserved heterogeneity, observable municipality specific shocks, and observable shocks within reference groups, that could

induce a spurious correlation between the fiscal behavior and the expectations for bailouts. The results are robust to these considerations.

In the empirical approach, we also assumed that municipalities possess rational expectations. However, as a byproduct of our empirical approach, the reduced form estimates of the two instruments on debt imply a naïve model of expectations formation. These results are qualitatively similar to the ones from the rational expectations specifications. Despite this fact, we do think that rational expectations are a more reasonable approximation of how municipalities actually form expectations, not the least since they are economically significant organizations run by professional policymakers with the help of a sizeable bureaucracy. Nevertheless, it is still reassuring that our findings are robust to the assumptions of how municipalities process their information.

Finally, it can be noted that there are certain aspects of the bailout problem that we have not touched upon in this paper. One such thing is the motive for the central government in providing bailouts. As mentioned earlier, the econometric strategy that we have used do not allow for a causal interpretation of the first-stage estimations, i.e., the estimations where we have the bailout indicator as the dependent variable. This implies that we cannot infer anything about the reasons for the central governments to provide bailouts. It could for example be interesting to examine the role of politics in a central government's decision to provide bailouts.[27] To that end, we must however make use of a different empirical strategy.

[27] That politics might matter when a central government distributes money that it has large discretion over has been shown in Dahlberg and Johansson (2002).

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Table 1. Summary statistics

	Mean	**St. Dev**	**Min**[a]	**Max**
Debt	10,218	4,808	797	38,024
Bailout	.318	.466	0	1
Own past experience of bailouts	.318	.466	0	1
Bailouts of neighbors	.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
Neighbors' 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 2. Reduced form estimates

	Bailouts			Debt		
	Model I	**Model II**	**Model III**	**Model I**	**Model II**	**Model III**
Own experience of bailouts	.356 (19.86)	.348 (19.37)	.322 (14.92)	329 (2.63)	242 (1.99)	325 (2.38)
Bailouts of neighbors	.182 (7.56)	.146 (5.90)	.136 (4.60)	1254 (6.13)	602 (3.00)	490 (2.23)
Municipality's covariates						
Income		-2.75e-06 (1.72)	-2.35e-06 (1.02)		.012 (0.51)	-.038 (1.52)
Proportion of young, 0-15		-.006 (0.98)	.012 (1.29)		457 (6.17)	314 (3.67)
Proportion of old, 65+		.006 (0.77)	.012 (1.06)		356 (4.23)	552 (4.81)
Population size		-8.35e-07 (0.29)	1.15e-06 (0.30)		.058 (1.43)	.090 (1.75)
Population density		-.0006 (2.41)	-.0007 (1.89)		-21.65 (5.81)	-18.43 (4.92)
Unemployment (%)			1.493 (1.43)			-5174 (0.52)
Neighbors' covariates						
Income		-9.79e-07 (0.35)	1.35e-06 (0.34)		-.078 (2.40)	-.072 (1.95)
Proportion of young, 0-15		.049 (5.27)	.071 (4.72)		227 (2.12)	420 (2.78)
Proportion of old, 65+		.051 (4.02)	.071 (3.69)		346 (2.50)	663 (3.35)
Population size		-3.90e-06 (1.15)	-3.53e-06 (0.82)		-.100 (2.06)	-.017 (0.30)
Population density		-.0001 (0.22)	-.0002 (0.38)		-18.54 (2.47)	-29.75 (3.97)
Unemployment (%)			-5.184 (3.19)			-623 (0.04)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.54	0.54	0.56	0.65	0.67	0.71
Relevance of the instruments	F = 385	F = 317	F = 198			
Number of obs.	5048	5048	3944	5048	5048	3944

Note: The dependent variables are the indicator for receiving a bailout or not in period *t* (the first three columns) and the level of municipality debt (the last three columns). Figures within parentheses are t-statistics (absolute values). Robust standard errors were used in calculating t-statistics.

Table 3.The SBC problem: IV estimates using a fixed effect transformation

	Model I	**Model II**	**Model III**
Own experience of bailouts	924	697	1,007
	(2.64)	(2.00)	(2.38)
Bailouts of neighbors	6,878	4,135	3,594
	(4.88)	(2.71)	(2.04)
Number of observations	5,048	5,048	3,944

Note: The dependent variable is the municipalities' debt. Figures within parentheses are t-statistics (absolute values). Robust standard errors were used in calculating t-statistics. The results in each cell are the outcome of separate regressions. The covariates used, but not reported, in each regression corresponds to those for columns I-III in Table 1. The instruments are own experience of bailouts and bailouts of neighbours, i.e., an indicator variable of whether a municipality was bailed out or not in period *t-1*, and the average number of bailouts received by its geographical neighbours in period *t-1* respectively.

Table 4. The SBC problem: IV estimates using a first differencing transformation

	Model I	**Model II**	**Model III**
Own experience of bailouts	1,067	1,163	1,590
	(1.68)	(1.85)	(1.97)
Bailouts of neighbors	5,494	5,236	5,486
	(2.09)	(1.96)	(1.82)
Number of observations	4,763	4,763	3,657

Note: The dependent variable is the municipalities' debt. Figures within parentheses are t-statistics (absolute values). Robust standard errors were used in calculating t-statistics. The results in each cell are the outcome of separate regressions. The control variables used, but not reported, in each regression corresponds to those for columns I-III in Table 1. A first differencing transformation of the data is used to remove the fixed effect and the instruments are an indicator variable of whether a municipality was bailed out or not in the period *t-2*, and the average number of bailouts received by its geographical neighbours in period t-2.

Table 5. Sensitivity analysis: Lag structure of the instruments

Lags of instruments	**Model I**		**Model II**		**Model III**	
			Own experience of bailouts			
	FE	FD	FE	FD	FE	FD
FE: *t-1*	924	1,067	697	1,163	1,007	1,590
FD: *t-2*	(2.64)	(1.68)	(2.00)	(1.85)	(2.38)	(1.97)
FE: *t-1, t-2*	979	1,123	740	1,221	960	1,681
FD: *t-2, t-3*	(2.80)	(1.72)	(2.12)	(1.90)	(2.24)	(2.28)
FE: *t-1, t-2, t-3*	1,140	1,126	812	1,258	934	1,710
FD: *t-2, t-3, t-4*	(3.05)	(1.70)	(2.17)	(1.90)	(2.18)	(2.34)
			Bailouts of neighbors			
	FE	FD	FE	FD	FE	FD
FE: *t-1*	6,878	5,494	4,135	5,236	3,594	5,486
FD: *t-2*	(4.88)	(2.09)	(2.71)	(1.96)	(2.04)	(1.82)
FE: *t-1, t-2*	7,647	4,390	4,889	4,349	4,470	4,548
FD: *t-2, t-3*	(5.31)	(2.59)	(2.96)	(2.45)	(2.41)	(2.55)
FE: *t-1, t-2, t-3*	7,157	4,270	4,943	4,223	4,733	4,003
FD: *t-2, t-3, t-4*	(5.75)	(2.56)	(3.27)	(2.43)	(2.93)	(3.02)

Note: The dependent variable is the municipalities' debt. Figures within parentheses are t-statistics (absolute values). Robust standard errors were used in calculating t-statistics. The result in each cell is the outcome of a separate regression. The covariates included in column I-III correspond to those used in Table 2.

Table 6. Effect of both own experience and bailouts of neighbors: 2SLS estimates

	Model I		**Model II**		**Model III**	
	FE	FD	FE	FD	FE	FD
Bailouts	1,907	906	1,126	1,001	1,352	1,408
	(6.50)	(1.42)	(3.73)	(1.58)	(3.81)	(1.74)
Number of observations	5,048	4,763	5,048	4,763	3,944	3,657

Note: The dependent variable is the municipalities' debt. Figures within parentheses are t-statistics (absolute values). Robust standard errors were used in calculating t-statistics. The result in each cell is the outcome of a separate regression. The covariates included in column I-III correspond to those used in Table 2.

Figure 1.
Total amount

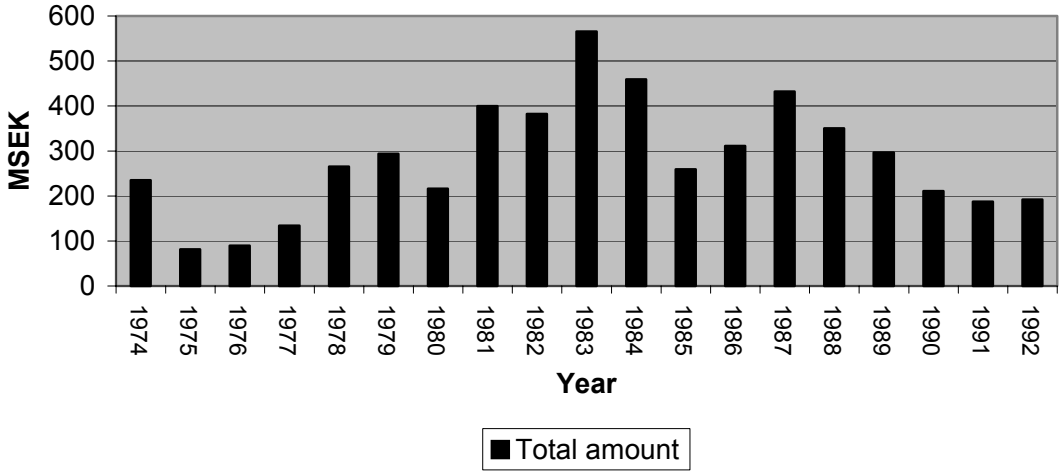

Figure 2.
Number of municipalities

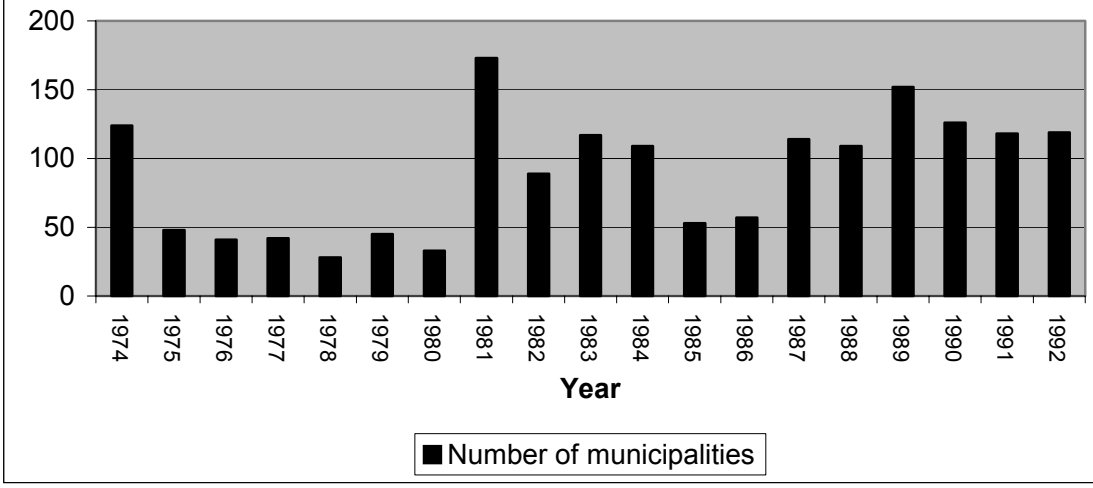

Figure 3
Histogram of bailouts

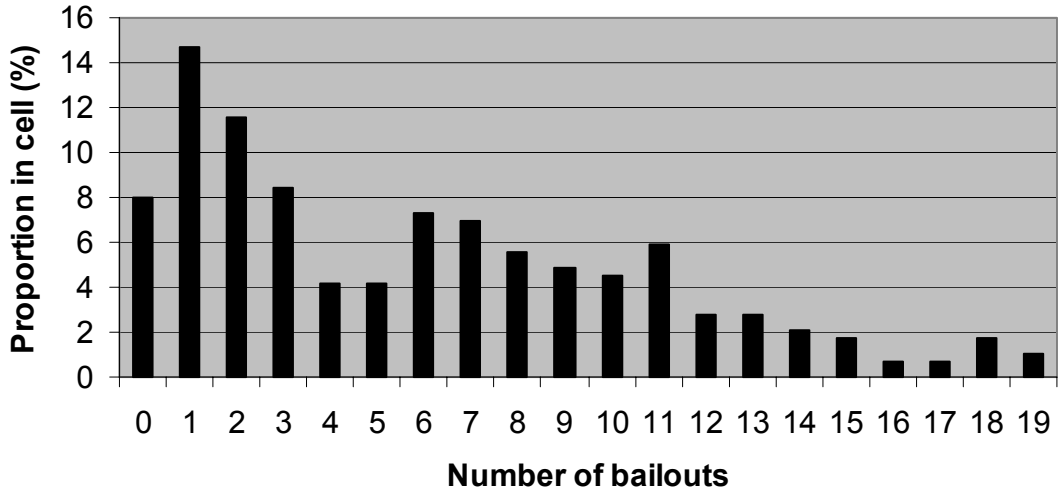

Figure 4.
Debt 1974-1992