BAILOUTS AND AUSTERITY

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Abstract

This paper studies with disaggregated budget data how expenditures, revenues, and borrowing evolve in municipalities that receive bailouts. It asks whether higher-level governments enforce austerity measures after bailing out indebted municipalities. The sample consists of 421 municipalities in the German federal state of Hesse over the 1997-2010 period. The results indicate that municipalities cut personnel, construction, and social expenditures, increase tax revenues and property tax rates, and reduce deficits after they receive a bailout from the state government. The state government appears to be both able and willing to enforce austerity after granting a bailout.

Keywords: Subnational bailouts, soft budget constraints, local fiscal policy

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1 Introduction

Bailouts of subnational jurisdictions by higher-level governments are a frequent phenomenon in many fiscally decentralized countries. They have been observed, inter alia, in Swedish municipalities (Pettersson-Lidbom, 2010), Argentinian provinces (Nicolini et al., 2002), German Länder (Baskaran, 2012a), and American cities (Wildasin, 1997)[1]. Consequently, a large theoretical literature on the consequences of subnational bailouts has recently emerged (Wildasin, 1997; Inman, 2001; Goodspeed, 2002; Doi and Ihori, 2006; Akai and Sato, 2008; Crivelli and Staal, 2013; Breuillé and Vigneault, 2010; Goodspeed and Haughwout, 2012). One of the main results in this literature is that on top of the direct costs[2], bailouts have large indirect costs. The argument is that when bailouts are possible, subnational jurisdictions' perceived individual costs of borrowing decline below the social costs. This variant of the common pool problem, called soft budget constraints[3], creates incentives to over-borrow. Pettersson-Lidbom (2010), for example, estimates for Sweden that soft budget constraints are responsible for an increase in municipal debt by more than 20%.

Existing theoretical contributions suggest that to avoid subnational soft budget constraints, higher-level governments should commit to a no-bailout policy. If there is no possibility of a bailout, then the fiscal incentives of subnational governments will remain undistorted and they will have no incentive to over-borrow. However, it is not clear that a strict no-bailout policy is necessarily socially optimal. Subnational governments might encounter fiscal difficulties for exogenous reasons, such as natural catastrophes or unfortu-

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[1]See also `http://money.cnn.com/2013/09/27/news/economy/detroit-federal-help/` for the recent bailout of Detroit.

[2]For example, bailouts provided to two German states by the federal government in 1992 cost about 15 billion Euros.

[3]The soft budget constraint concept was originally developed by Kornai (1986) to explain the behavior of firms in socialist countries.

nate economic developments. In such circumstances, it seems unfair and, from a welfare perspective, even inefficient to refuse bailouts.[4]

An alternative to a strict no-bailout policy is to provide bailouts selectively and only in conjunction with austerity conditions. This seems to be how most countries handle the question of subnational bailouts. Central governments, while rarely implementing a strict hard budget constraint, do not simply bail out every municipality that claims to be indebted. Instead, they require that municipalities that receive a bailout implement austerity measures and consolidate their budgets.

This paper studies the ex-post fiscal consequences of subnational bailouts for the recipient municipalities in a setting where bailouts are granted only in conjunction with austerity conditions. More specifically, I ask whether local governments reduce expenditures and increase taxes after they receive a bailout and thus comply with austerity conditions set by the higher-level government. Furthermore, I explore what type of expenditures are cut and what tax rates are increased. Based on this analysis, I attempt to infer who bears the costs of austerity.

To answer these questions, I make use of a unique dataset that covers 421 municipalities in the German federal state of Hesse. Over the period of 1997 to 2010, the Hessian state government granted 126 bailouts to 40 municipalities. I study how these bailouts subsequently affected fiscal policy in the bailout municipalities relative to comparable municipalities that did not receive a bailout.

This paper is primarily related to the empirical literature on the causes and consequences of subnational bailouts.[5] Most of this literature studies how the possibility of bailouts affects

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[4] In addition, some theoretical contributions derive that hard budget constraints might be inefficient even if subnational over-borrowing is not due to adverse economic shocks (Besfamille and Lockwood, 2008).

[5] This paper also contributes to the general empirical literature on the causes and consequences of discretionary intergovernmental grants. The causes of intergovernmental grants are typically analyzed from a political economy perspective. Recent contributions are Dahlberg and Johansson (2002), Johansson (2003), Arulampalam et al. (2009), Sorribas-Navarro (2011), and Brollo and Nannicini (2012). The consequences of grants are studied both by the political economy and the public finance literature. Relevant papers are

fiscal policy before the bailouts are actually granted, i. e. how expected future bailouts affect ex-ante fiscal decisions, and in particular municipalities' borrowing policies. A seminal contribution to this literature is Pettersson-Lidbom (2010), who establishes that bailout expectations lead to higher borrowing in Swedish municipalities. Further related studies are Bordignon and Turati (2009) for Italy and Rodden (2005) and Baskaran (2012a) for Germany.

Few studies look at the ex-post fiscal consequences of bailouts, i. e. after they have been granted. The closest related contribution in terms of setting is Baskaran (2013), who studies the political consequences of bailouts with a sample of Hessian municipalities and a selection on observables approach. The results suggest that bailouts curry no electoral benefits for the state government. The most closely related paper in terms of research question is Dietrichson and Ellegard (2013). These authors study for Sweden how bailouts that are provided with austerity conditions affect municipal fiscal policy, relying on the synthetic control methodology for identification (Abadie and Gardeazabal, 2003).[6] Apart from the different methodology, my paper differs from Dietrichson and Ellegard (2013) in the nature of the bailout program. The bailouts studied by them are provided once and at the same time to all eligible municipalities, while the bailouts that I study are provided recurringly and at different points in time. The recurring nature of bailouts implies that in my setting, the soft budget constraints problem is potentially more severe. Second, Swedish municipalities have to comply with the austerity conditions only before they receive the bailouts. Even though the authors explore whether budget consolidations persist after the bailout, there are no formal requirements for municipalities to reduce deficits ex-post. In contrast, in the Hessian

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Levitt and Snyder (1997), Solé-Ollé and Sorribas-Navarro (2008), Litschig and Morrision (2009), Knight (2002), Gordon (2004), Dahlberg et al. (2008), Singhal (2008), Baskaran (2012b), Lundqvist et al. (2013), and Litschig and Morrision (2013).

[6]Another related paper is Hopland (2013), who studies how increased central government scrutiny of municipal budgets affects municipalities' subsequent fiscal policy. He does not study austerity conditions in conjunction with bailouts, however.

setting, municipalities are explicitly expected to consolidate their budgets after they receive a bailout. It is, however, unclear whether they conform to these expectations.

The key methodological challenge in my setting is that bailouts are not granted randomly. Consequently, unobserved or omitted variables might result in different fiscal trends in bailout and no-bailout municipalities. That is, bailout municipalities might need to spend more and be less able to tax because of inherent economic difficulties. One way to identify the effect of bailouts on fiscal policy would be to rely on a natural experiment. Unfortunately, in Hesse, as presumably in most other settings where subnational bailouts take place, there is no institutional mechanism that would induce quasi-random variation in bailout allocations. A further contribution of this paper is therefore to apply a new approach to establish the ex-post fiscal consequences of bailouts. The main idea is to identify for each bailout municipality a suitable control – a no bailout municipality – that was likely subject to the same fiscal trends as the respective bailout municipality – and then compare with difference-in-difference regressions fiscal developments in these two sets of municipalities in pre- and post-bailout periods. If bailouts entail austerity, municipalities should cut expenditures, increase taxes, and reduce deficits relative to municipalities that were subject to similar trends but did not receive a bailout.

The results show that bailouts are indeed associated with fiscal adjustments in the receiving municipalities. Most notably, municipalities that receive a bailout increase tax revenues and raise property tax rates. I also find that bailouts have a negative effect on many expenditure items, even if the estimated coefficients are only robustly significant for personnel, construction, and social expenditures. Finally, bailouts tend to have a negative effect on debt and deficits, and a positive effect on debt redemptions. Hence, municipalities appear to consolidate in the aftermath of a bailout. The state government seems to be both able and willing to enforce austerity. Property owners, municipal employees, and beneficiaries of social and construction spending bear the lion's share of the austerity costs.

2 Institutional details

Hesse is a federal state in Germany. It is inhabited by about six million inhabitants who live in 426 general-purpose municipalities. Municipalities fulfill many tasks. These tasks can be classified according to whether they are constitutionally designated as voluntary or compulsory. For voluntary tasks (*freiwillige Selbstverwaltungsaufgaben*), municipalities can decide whether and how to provide them. For example, the provision of cultural venues is a voluntary task. Municipalities can decide not to maintain e. g. a theater. If a municipality chooses to maintain a theater, it can freely decide on its size and budget. Other important voluntary tasks are of a social nature, such as care for the poor, old age care, and drug addiction counseling; or they relate to local living conditions, e. g. traffic infrastructure and water and electricity supply.

Compulsory tasks can be subdivided into "own compulsory tasks" (*pflichtige Selbstverwaltungsaufgaben*) and "transferred compulsory tasks" (*übertragene Aufgaben*). Municipalities are constitutionally required to fulfill own compulsory tasks such as fire protection, child care, or the provision of school buildings. They have, however, discretion about how to fulfill them.

Transferred compulsory tasks belong constitutionally to the domain of the state or the federal tier, but they have been transferred by the higher tiers of government to the municipalities for implementation. Municipalities have no room to decide whether nor how to fulfill these tasks. Examples for the transferred compulsory tasks are construction supervision (*Bauaufsicht*) and the guaranteeing of public order (*Ordnungsverwaltung*).

The voluntary and compulsory tasks result in expenditures. These expenditures are financed from several sources. First, municipalities can raise revenues through a number of own-source local taxes known as *Realsteuern* ("real" taxes). The characteristic feature of the *Realsteuern* is that the base is the same throughout the federation. Municipalities may,

however, set different "multipliers", which determine the tax rates. I therefore use in the following the terms tax rate and tax multiplier interchangeably.

The most important *Realsteuern* are the business tax and the property tax B. The business tax is levied on the profit of local businesses and represents the largest source of municipal own source revenues. Total revenues in Hesse from this tax amounted to about 3bn Euros in 2012. The property tax B is levied on residential property and represents the second largest source of tax revenues. The third prominent, albeit in terms of revenues fairly unimportant, *Realsteuer* is the property tax A. This tax is levied on agricultural property. Total revenues from the two property taxes amounted to about 770 million Euros in 2012.

Apart from the *Realsteuern*, municipalities levy a number of additional taxes, such as a tax on pet dogs or amusement taxes. But revenues from these taxes are small compared to the revenues from the *Realsteuern*. Similarly, municipalities raise revenues from user fees, but these too are relatively unimportant.

Another important source of municipal revenues is the income tax. The income tax differs from the taxes and user fees mentioned above in that municipalities are not allowed to adjust rates or bases. Since the income tax is a shared tax that jointly accrues to the federal and state governments and the municipalities, the latter simply receive a fraction of the income tax revenues collected within their administrative boundaries.

Besides taxes, another important source of municipal revenues is debt. Technically, municipalities are only allowed to borrow in order to finance investments. Municipal budgets, and in particular their borrowing, is also subject to approval from the local regulatory body (*Kommunalaufsicht*). Even though municipalities may only borrow for investment purposes, there is considerable variation between municipalities' stock of debt, suggesting that these particular borrowing restrictions (tying debt to investments and the need to get borrowing approved) have not been particularly effective in the past.

Apart from taxes and debt, the third important source of revenues for municipalities are intergovernmental transfers. While both the federal and state tiers provide a multitude of transfers to the municipalities, the state tier's transfer programs are much larger. State transfers can be classified according to whether they are rule-based or discretionary. The most important rule-based transfers are unconditional grants paid according to municipalities' fiscal need. A municipality's fiscal need is essentially the difference between the aggregate value of its various tax bases and a hypothetical expenditure need calculated by a formula specified in the local fiscal equalization code.[7] The purpose of this transfer program is to ensure that all municipalities have sufficient revenues to cover their expenditure needs, i. e. to enable them to provide several essential public goods and services. Other rule-based transfer programs are of conditional nature. They are paid, for example, to finance schools, and municipalities are expected to use these revenues for their intended purpose.

The second block of transfers is paid discretionarily. Bailout transfers belong to this category. The bailout transfer program in Hesse is labeled *Zahlungen aus dem Landesausgleichsstock*. The professed goal of this transfer program is to provide additional resources to municipalities that face "extraordinary" fiscal difficulties. These transfers are paid from a fund set up by the state government (*Landesausgleichsstock*) and do not have to be repaid. The decision about whether a municipality receives these transfers is made by the state government and an annexed governmental body, the so called *Regierungspräsidium* (regional council).

The procedure for the approval of bailout transfers works as follows. First, a municipality has to apply for bailout transfers for a particular year. This application is forwarded to the relevant *Regierungspräsidium*. There are three *Regierungspräsidien* in Hesse, each responsible for all lower level governments within a certain geographical area. The *Regierungspräsidien* are an attempt by the state government to coordinate policies within

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[7]See Baskaran (2012b) for details on Hesse's rule-based transfer program.

a geographical region. That is, state ministries are split according to functional categories but are responsible for the state as a whole; the *Regierungspräsidien*, on the other hand, are split according to geographical areas and are responsible for implementing all state policies within their area. As the ministries, the *Regierungspräsidium* is a political body that is headed by a politician rather than by a bureaucrat. Since the president of this body is a politician, he has the status of a *politischer Beamter* (political official) and can therefore be dismissed by the state government without reason (which typically happens if the incumbent state government loses the next election).

Once a municipality applies for a bailout, its *Regierungspräsidium* assesses whether the application has some merit. If the *Regierungspräsidium* deems the application to be unfounded, it can reject it already at this stage. If it assesses the application to be reasonable, it passes it on to the state interior ministry. The final decision on whether to approve the bailout is made by the state interior ministry in consultation with the state finance ministry, and ultimately by the interior minister.

There are official guidelines (*Richtlinien über die Gewährung von Zuweisungen aus dem Landesausgleichsstock* – guidelines for the provision of transfers from the state equalization fund) published by the state government detailing the general criteria according to which a municipality may be granted a bailout.[8] The guidelines are, however, relatively vague. They state that a bailout can only be granted if (i) a municipality cannot overcome its fiscal difficulties on its own and (ii) if the difficulties are not self-inflicted. But there are no clear thresholds for e. g. debt or any other fiscal variables that would automatically induce a bailout. Nor are there clear statements about when the fiscal problems should be deemed self-inflicted. Consequently, the *Regierungspräsidium*, and later the state interior ministry, have a significant degree of discretion in whether to grant a bailout.

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[8]Three versions of the *Richtlinien* were relevant during the sample period, but they have essentially the same stipulations. The current guidelines are available at `http://verwaltung.hessen.de/irj/HMdI_Internet?cid=d88699aa5c108709a72a01c89823be73`.

When the state government approves a bailout, it always stipulates austerity conditions. These conditions typically encompass the obligation for municipalities to cut expenditures and raise revenues, and more generally to consolidate the budget. This austerity plan can be fairly detailed, e. g. sometimes explicitly stating which revenue items should be increased and what expenditures should be cut. It is, however, unclear whether and for how long the state government enforces these stipulations. First, it is a different (albeit technically subordinate) administrative body, the *Kommunalaufsicht*, that is responsible for keeping track of consolidation efforts and approving the budgets of municipalities that received a bailout. Second, political considerations may preclude the strict enforcement of the austerity measures not only by the state government, but even more so by the *Kommunalaufsicht*. The head of the *Kommunalaufsicht* for regular municipalities is the county governor (*Landrat*). Since she is elected by the entire county population, including the inhabitants of the bailout municipalities, she might be wary to impose too much austerity (Scharting, 2013).

Second, the specific measures specified in the state government's austerity plan are typically too few to fully consolidate the budgets of bailout municipalities. If a municipalities wants to reduce deficits seriously, it needs to take further steps. In doing so, the principle of local fiscal autonomy implies that it is up to the municipality to decide what expenditures to cut and which taxes to raise. This principle of local fiscal autonomy is also reflected in the language that the state government uses when detailing the austerity conditions in the official correspondence with the bailout municipalities. The conditions are typically phrased in a wording that suggests recommendations (albeit very strong ones) instead of direct orders. Even though the state government has in principle the authority to dissolve the local council and to appoint a special commissioner tasked with balancing the budget (§141 and §141a of the Hessian local government code), such extreme measures are rarely taken (in fact I am not aware of a single case in Hesse).

In summary, Hessian municipalities offer an excellent opportunity to study various questions surrounding the fiscal consequences of subnational bailouts. First, do municipalities consolidate their budgets in the aftermath of bailouts? Second, assuming they do attempt to consolidate, how do they consolidate their budgets? Do they raise taxes or cut expenditures? What taxes do they raise and what expenditures do they cut? Third, do consolidation efforts persist for a long time or do they fizzle out after a few years?

3   Data

The dataset encompasses 421 Hessian municipalities. I exclude five so called county-free cities (essentially very large cities), since they are subject to different fiscal arrangements and hence not comparable to regular municipalities. The sample period is 1997-2010. The sample starts in 1997 because no data on bailouts is available before this year; even though there were bailouts before 1997, the state government did not keep track of them.

The map in Figure 1 indicates those municipalities that received a bailout and those that unsuccessfully applied for one during the sample period. Figure 2 provides some descriptive statistics on the bailouts paid by the Hessian state governments between 1997 to 2010. The total number of bailouts per year varied over time. While the total number was around six or seven in the beginning of the sample period, it peaked in 2003 with 15 bailouts, but declined again to around six at the end of the sample period. Similarly, average real[9] bailout payments per capita were around 10 Euros in 1997, they had increased to 63 Euros in 2008, in 2010 they stood at around 31 Euro. Overall, 40 municipalities received a bailout during the sample period, i. e. around 10% of all Hessian municipalities.

The dataset has furthermore information on various municipal expenditures. I consider the following expenditure categories: personnel, construction, social, culture and education, and

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[9]I normalize nominal values with the federal CPI.

health and sports. Personnel expenditures encompass spending across a range of functional categories, whereas all other expenditures are classified according to individual areas. I omit highly aggregated expenditure categories such as total expenditures because they are uninformative for the purposes of this paper: for example debt redemptions are defined as expenditures in the municipal fiscal accounting systems in Germany, and it is therefore unclear how to interpret e. g. increasing total expenditures in the aftermath of bailouts.

On the revenue side of the budget, I consider total tax revenues and the multipliers for the three important municipal taxes: the business tax and the property taxes A and B. As stated above, the multipliers are a factor that the municipalities choose and which deterministically results in the effective tax rate on the business and property tax bases. As for expenditures, I omit highly aggregated revenue categories, such as total revenues, since some debt operations and also bailout transfers are treated as revenues in the accounting system.

Finally, I look at the effect of bailouts on debt, deficits, and debt redemptions. The debt variable is the stock of debt for investments in the core budget (*Investitionskredite der Kernhaushalte*) and does not include short-term debt for financing temporary budget gaps and borrowing by municipality-owned firms. This debt definition specifically refers to investments because municipalities are only allowed to assume long-term debt for investment purposes. Deficits are defined as the change in the stock of debt in a given year.

Figure 3 compares the level of the fiscal variables in bailout and no-bailout municipalities in the pre-sample period (1990-1996). Bailout municipalities had on average slightly higher personnel and noticeably higher construction expenditures and considerably lower social expenditures. There were no big differences in other expenditures categories, however. Tax revenues were slightly lower in bailout than in no-bailout municipalities. Notable differences exist for the borrowing variables. Bailout municipalities had considerably higher stocks of debt, higher deficits, and higher debt redemptions. Note that higher debt redemptions are to some extent a mechanical consequence of higher stocks of debt because of refinancing

operations. Finally, there were also some differences for the property tax multipliers: those in bailout municipalities were higher, in particular for property tax B. No big differences were present for the business tax multipliers, however.

4   Empirical methodology

My aim is to establish the effect of the austerity conditions that accompany bailouts on individual budget items. Bailouts are provided if (i) a municipality applies for one and (ii) the state government approves the application. The link between bailouts and a fiscal variable $Y_{i,t}$ in municipality i and year t can thus be formalized by a model that consists of three stages. In the first stage, the municipality i decides whether or not to apply for a bailout in year t, a decision captured by the dummy variable $A_{i,t}$. The municipality makes this decision based on a set of covariates $W_{i,t}$. Then, the state government (or, respectively, the appropriate *Regierungspräsidum*) decides whether to grant the bailout to the municipality, a decision captured by the dummy variable $B_{i,t}$. The state government's decision is again based on a set of covariates $V_{i,t}$. The set of covariates $W_{i,t}$ can overlap with $V_{i,t}$. The full model is hence as follows:

$$Y_{i,t} = B_{i,t} + \epsilon_{i,t} \tag{1}$$

$$B_{i,t} = V_{i,t}\beta_{vs} + \nu_{i,t} \tag{2}$$

$$A_{i,t} = W_{i,t}\beta_{ws} + \mu_{i,t}. \tag{3}$$

I am interested in Equation 1 of this model. When estimating Equation 1, endogeneity is a concern. The primary source for endogeneity here are omitted variables that induce different trends in the outcome variables in bailout and no-bailout municipalities, or more formally that $E(\epsilon_{i,t}, \nu_{i,t}), E(\epsilon_{i,t}, \mu_{i,t}) \neq 0$. For example, municipalities might be forced to spend more

and raise fewer taxes because of adverse economic shocks that persist into post-bailout periods while at the same time being more likely to both apply and receive a bailout. Therefore, simply comparing fiscal developments in municipalities after they receive a bailout with developments in no-bailout municipalities will likely result in estimates that are confounded by group-specific trends.

Given that bailouts are by definition granted discretionarily in Hesse, there is no institutional mechanism that would result in quasi-random allocations of bailouts. More specifically, the discretionary nature of bailouts implies that their effect on fiscal outcomes cannot be identified by means of a natural experiment. An alternative would be to explicitly control for all determinants of bailouts, i. e. $V_{i,t}$ and $W_{i,t}$, in the outcome regression. However, it is difficult to control for all possible confounding variables. A further problem here is that many of the likely determinants of bailouts, e. g. levels of debt or tax rates, are precisely the fiscal variables I am interested in. Explicitly controlling for these variables will render an analysis of their evolution problematic due to the bad controls problem (Angrist and Pischke, 2009).

For these reasons, I employ the following methodology. The idea is to first find for each municipality that received a bailout during the sample period a control municipality that did not receive a bailout, but which was presumably subject to similar fiscal trends. Then, I estimate the effect of bailouts on fiscal outcomes by relating the differences in fiscal outcomes between each bailout municipality and its control to dummy variables indicating post-bailout periods in a difference in difference framework.

More formally, I identify for each bailout municipality i a suitable control municipality j. Thereafter, I calculate for each year the difference in the outcome variables $Y_{i,t}$ (with $Y_{i,t}$ being one of the expenditure, revenue, or borrowing variables mentioned above) between a bailout municipality and its control no-bailout municipality, i. e.

$$\Delta Y_{i,t} = Y_{i,t} - Y_{j,t} \tag{4}$$

With this dependent variable, I run difference in difference regression that relate various dummy variables which distinguish "bailout-periods" from "no-bailout-periods", to the difference in the outcome variables between a bailout municipality and its control no-bailout municipality. The econometric specification that I estimate is hence:

$$\Delta Y_{i,t} = \alpha_i + \gamma_t + \beta \mathrm{BP}_{i,t} + \eta_{i,t}, \tag{5}$$

where $\mathrm{BP}_{i,t}$ is a dummy for bailout periods rather than just the dummy $B_{i,t}$ for the year when a bailout was granted as in Equation 1. This more permissive specification for bailout periods allows me to take into account that the austerity programs that accompany bailouts have long-run consequences. That is, I consider not only fiscal effects in the year of the bailout but operate with bailout dummies that define up to ten years after a bailout as post-bailout periods.[10] Hence, $\mathrm{BP}_{i,t}$ are dummy variables that are 1 in the first one, three, five, seven, and ten years after a bailout and else 0 (the year of the bailout is always set to one). If a municipality receives another bailout within each of these periods, the counter resets. To give an example: the five-year bailout variable is 1 in the year of the bailout and continues to be 1 for five more years. If, however, the municipality receives another bailout e. g. two years after the first bailout, the five year bailout variable resets and remains 1 for altogether seven years.

If a bailout municipality is subject to the same trends as its control no-bailout municipality, this framework can provide a credible estimate for the fiscal effects of bailouts. The challenge is to find for each bailout municipality a suitable control municipality. To do so, I rely primarily on a simple matching technique based on the developments of the fiscal variables

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[10]Table A.1 in the Appendix collects all variable definitions.

in the pre-sample period. More specifically, I run a cross-sectional probit regression where I relate the propensity that a municipality receives a bailout during the 1997-2010 period on the average change in all outcome variables within a municipality during the 1990-1996 period, i. e.

$$B_{i,1997-2010} = \alpha_i + \sum_k \beta_k (Y_{i,1996;k} - Y_{i,1990;k}) + \zeta_i, \tag{6}$$

with $B_{i,1997-2010} = 1$ if a municipality has received at least one bailout during the 1997-2010 period. This regression simply relates bailout receipts in the sample period to pre-sample trends in all outcome variables k, but has no causal interpretation.

Based on this regression, I predict for each municipality the propensity for receiving a bailout during the 1997-2010 period. Thereafter, I match each municipality that actually received a bailout during the 1997-2010 period with the no-bailout municipality that has the closest propensity score to the bailout municipality in question, and then calculate for each year the difference for each of the outcome variables between the two municipalities as indicated in Equation 4.

The idea underlying this matching approach is that bailout and no-bailout municipalities with similar propensities for receiving a bailout based on their fiscal developments in the pre-sample period, should have continued to experience similar trends in their fiscal variables in the sample period. This approach relies, in particular, on the assumption that the reason why despite similar fiscal trends one municipality receives a bailout while the other does not, are differences in the levels of the fiscal variables. I will try further below to confirm this assumption for the bailout municipalities and their matches, but ultimately it is not testable. As a second strategy, therefore, I experiment in robustness tests with other approaches to identify for the bailout municipalities controls that were subject to similar trends. In particular, I restrict potential matches to municipalities that applied but did not receive a

bailout during the sample period, and to no-bailout municipalities that border a particular bailout municipality. Finally, I also estimate placebo regressions with random treatments to evaluate whether the effects found in the baseline regressions are spurious.

Despite these attempts for credible identification, it is worthwhile to contemplate the direction of any bias in case that the approaches to construct suitable controls fail. Municipalities that received a bailout during the sample period should have experienced worse fiscal developments than a random no-bailout municipality. That is, any differences in unobserved trends between bailout and no-bailout municipalities should lead to disproportionately higher expenditures and lower revenues in the former. Hence, the estimated effects of the bailout dummies for expenditures and debt will tend to be biased upward – in the direction of higher expenditures and borrowing – and for revenues downward – in the direction of lower revenues. Observing a significantly negative effect of bailouts on expenditures or borrowing or a significantly positive effect on revenues provides therefore strong evidence that a bailout and the accompanying austerity package have a causal effect on subsequent municipal fiscal policies.

A second issue that needs to be mentioned is that municipalities might start to adjust their fiscal policies even before they are granted a bailout. That is, since the state government's *Richtlinien* claim that municipalities will only be given a bailout if they cannot resolve their fiscal difficulties on their own, municipalities that want to apply for a bailout might already attempt to consolidate their budgets before they submit the application. Consequently, the effect of the bailout dummies might be underestimated.

On the other hand, the possibility of a bailout might incentivize municipalities to over-borrow, as argued by much of the theoretical literature and found in some empirical studies. If these incentives suddenly dissipate after a municipality has received a bailout because it is for the time being "saturated" by the additional resources, expenditures may decline and taxes may increase by themselves and not because of the austerity measures. However, it is

unlikely that incentives to exploit soft budget constraints suddenly diminish after a bailout. From the perspective of the bailout municipalities, it seems unreasonable to voluntarily forgo the opportunity to keep up high levels of spending and low levels of taxes.

Another problem derives form the fact that I do no have information on which municipalities received bailouts before 1997. Thus, some no-bailout municipalities might have been subject to austerity conditions in the pre-sample period, and the effect of these conditions might have persisted to the sample period. Nevertheless, given that most municipalities (around 90%) did not receive a bailout in the sample period, it is unlikely that the bailout municipalities will be systematically matched with municipalities that did not receive a bailout in the sample period but did receive a bailout in the pre-sample period. Moreover, if the matched no-bailout municipalities received a bailout in the pre-sample period and if these effects persisted to the post-sample period, the estimates for the fiscal effects of bailouts will again be biased toward underestimating the consolidation efforts. It would hence be harder to detect a significant effect of bailouts on fiscal outcomes.

A related issue is that some municipalities received several bailouts during the sample period. If a bailout municipality had received a bailout just prior to 1997, the effect of the austerity conditions might persist to the sample period. However, this issue is only relevant for a small part of the sample period. Moreover, it again implies that consolidation efforts will at worst be underestimated.

5   Baseline results

This section collects the results using the matching approach to identify suitable controls. To validate the design, I first compare the average level and the average change of the outcome variables for bailout municipalities and their matches in the pre-sample period in Table 1. Note that all fiscal variables are in logs to allow for a percentage interpretation later in the

regressions. Exceptions are the tax multipliers, which are in levels, and deficit, which is the difference in log debt.

Column (I) of Table 1 lists the mean changes in the outcome variables in bailout municipalities during the pre-sample period. Column (II) lists the mean changes in the matched no-bailout municipalities. Column (III) collects the difference in means between bailout and matched no-bailout municipalities and the t-statistics from t-tests for whether the difference is significant. Correspondingly, columns (IV)-(VI) lists the same statistics for the levels of the fiscal variables.

The first observation from Table 1 is that fiscal developments in the 1990-1996 period were similar in bailout and matched no-bailout municipalities. That is, average changes in the various expenditure categories, revenues and tax rates, and the borrowing variables are not statistically different. On the other hand, the levels of some of the variables differ. In particular, personnel and construction expenditures, the property tax rates, debt, and redemptions are noticeably higher for bailout than no-bailout municipalities in the pre-sample period. Social expenditures, in contrast, are lower. These findings corroborate that between the bailout municipalities and their matches, bailouts are granted primarily based on (observable) differences in the level of fiscal variables rather than based on changes in the fiscal variables or on unobservable trends.

After establishing that the bailout municipalities and their matches were subject to similar pre-sample trends, I report the regression results for the effects of the austerity programs. As stated, I estimate the effect of bailouts on fiscal outcomes for up to a relatively long horizon of ten years. The results are collected in Table 2. The estimated coefficients on the bailout dummies are negative for almost all expenditure items. The estimates are significant in at least some models for personnel, construction, and social expenditures. For these expenditure items, bailouts have a lasting negative effect. For example, in the first five years after a bailout personnel expenditures are on average about 6 percent lower, construction

expenditures are about 10 percent lower, and social expenditures are about 12 percent lower. I also observe a strong negative effect of bailouts on expenditures for culture and education in the first year after the bailout, but the size of the coefficient varies somewhat for longer definitions of bailout variables and is once even positive.

The results are equally consistent but even more significant for the tax variables. The coefficient estimate for tax revenues and the tax multipliers is significantly positive even if the post-bailout period is defined to last up to ten years. In the first five years after a bailout, municipalities appear to raise on average about 6 percent more taxes, and to increase the business tax multiplier by 5 points, the property tax A multiplier by 13 points, and the property tax B multiplier by 17 points.

The consolidation efforts that follow from the declining personnel, construction, and social expenditures and the increasing levels of taxation are also reflected in the borrowing variables. The stock of debt begins to decline as evidenced by the negative coefficient estimate for all but the first bailout dummy. The coefficient is never significant, but this is presumably because the decline is too small relative to the existing stock of debt to result in statistically detectable effects. The deficit variable is a better indicator for consolidation efforts because short-term adjustment can be more easily detected. The sign of the bailout dummies on the deficit is always significantly negative, thereby confirming that municipalities consolidate after they receive a bailout. Finally, the effect of bailouts on debt redemptions is also consistently positive (albeit only once significant), thus again reflecting the consolidation efforts.

Overall, the results indicate that the austerity programs which accompany bailouts have significant fiscal consequences. They tend to either force or at least provide sufficient incentives to municipalities to cut expenditures, increase taxes, and reduce levels of debt.

6 Robustness

6.1 Matching with municipalities that were refused bailouts

It can be questioned whether the above matching approach fully accounts for different trends in bailout and no-bailout municipalities. Bailout municipalities and the matched no-bailout municipalities may have experienced similar trends in the pre-sample period, but these trends might have diverged already before the relevant bailouts took place. More specifically, a matched no-bailout municipality might not provide a good counterfactual if the respective bailout municipality had encountered economic difficulties that led to diverging trends at some point during the sample period.

To account for this issue, I make use of the fact that I have information on which municipalities were refused a bailout during the sample period. As mentioned above, the prerequisites for a municipality to be granted a bailout are relatively vague, allowing for some degree of discretion on the side of the *Regierungspräsidium* and the state government, especially in assessing whether the stock of debt is sufficiently high or whether the fiscal problems are self-inflicted. Consequently, among the subgroup of municipalities that apply for a bailout, there is likely some randomness in whether they will be granted one, even if they have similar levels of debt and were subject to similar trends.

Thus, I replicate the matching procedure from above with the innovation that a suitable match for a bailout municipality must belong to the group of municipalities that were refused a bailout. This approach has the disadvantage that the pool of potential matches is smaller, but the advantage is that the matches are possibly more likely to have experienced similar trends during the sample period. In particular, any intrinsic fiscal difficulties in bailout municipalities should be similar to the "refused-bailout" municipalities, so if fiscal developments diverge after a bailout, this can be considered as reasonably strong evidence

that they do because of the bailout, or more specifically because of the austerity conditions that accompany bailouts.

The results are collected in Table 3. They largely confirm the findings for the unrestricted matching approach. In the first five years after a bailout, personnel expenditures decline on average by about 5 percent. Social expenditures are about 9 percent and construction expenditures are about 15 percent lower after a bailout.

Tax revenues, too, are found once more to be consistently larger after a municipality receives a bailout. They increase in bailout municipalities on average by about five percent in the first five post-bailout years. For the tax multipliers, the results are also largely in line with the baseline results, even though I observe some differences for the size of the estimates. While the estimated coefficients are consistently positive, they are smaller than in the baseline models. The business tax multiplier is on average only about 4 points higher in the first five years after a bailout while the property tax A and B are about 6 and 7 points higher, respectively.

Finally, the results for the borrowing variables are also similar to the baseline estimates. The decline in debt after a bailout seems to begin slightly later in these regressions, however. The one year bailout dummy is even significantly positive, suggesting that bailout municipalities had considerably higher stocks of debt than the municipalities that were refused a bailout (which might be the reason why the latter were refused a bailout in the first place). Still, the size of the coefficient declines the more the definition for a post-bailout period is extended, turning negative for the seven year bailout dummy. Correspondingly, the effect of the bailout dummies on the deficit variable is consistently negative and significant from the third year dummy onwards. Redemptions are as in the baseline regressions positive and significant.

6.2 Contiguous municipalities

It is possible that the matching based on propensity scores does not ensure similar trends, even if only municipalities that were refused a bailout are used as matches. To validate the robustness of the baseline results further, I take another route to construct a plausible counterfactual for the bailout municipalities. The idea is to use for each bailout municipality the neighboring municipalities as comparisons. More specifically, I construct for each bailout municipality the average of the relevant fiscal variable in all contiguous no-bailout municipalities and then calculate for each year the difference in fiscal outcomes between municipality i and the average of the contiguous municipalities j that did not receive a bailout during the sample period. The dependent variable for this robustness test is hence:

$$\Delta Y_{i,t} = Y_{i,t} - \frac{1}{N_j} \sum_j Y_{i,j,t}, \tag{7}$$

where $Y_{i,j,t}$ is the value of the relevant fiscal variable in the no-bailout municipality j that is contiguous to the bailout municipality i. The idea underlying this comparison is that trends in a bailout municipality and its neighboring municipalities should be relatively similar. Hence, if the bailout dummies suggest a significant effect, we can reasonably ascribe this effect to the austerity package that accompanies a bailout.

One concern with this approach is the validity of the stable unit treatment value assumption (SUTVA). More specifically, the effect of providing a bailout to some municipalities should not spill over to municipalities that did not receive a bailout to ensure unbiased estimates. This may be a questionable assumption here in view of Petterson-Lidbom's (2010) result for Sweden that soft budget constraints increase in municipalities that neighbor a municipality which receives a bailout. In Hesse, however, such spillovers are unlikely to diminish the validity of SUTVA. The reason why Pettersson-Lidbom (2010) can rely on neighborhood effects to identify soft budget constraints are information asymmetries in Sweden. That is,

the assumption underlying his identification strategy is that a Swedish municipality updates its belief about the likelihood of receiving a bailout when a neighboring municipality receives one. This is a plausible argument for such a geographically large country as Sweden, where information on the fiscal stance of other municipalities and their likelihood for receiving bailouts might be hard to obtain. Hesse is geographically much smaller and no meaningful informational asymmetries exist given the unified media market, regular state-wide meetings of municipal officials, and the long history of the Hessian bailout program. All municipalities should hence be equally aware of the state's bailout program and be able to asses their approximate likelihood for receiving additional transfers. Bailouts received by neighboring municipalities should therefore not provide any meaningful information that would result in a change in fiscal behavior.

The results from using neighboring municipalities as matches are collected in Table 4. Overall, they point in the same direction as the baseline results. The main difference is that they are less significant for the expenditure items. Social expenditures is the only expenditure item that is significant, and only so for the narrowest of the post-bailout definitions. The results are statistically more significant for tax revenues and the property taxes. Tax revenues and property tax multipliers increase more in bailout municipalities after a bailout than in contiguous no-bailout municipalities. In the first five years, tax revenues increase on average by about six percent and the property tax multipliers increase on average by about about six points. While the effect on the business tax multiplier is positive, the coefficient is small and never significant.

Results for debt and deficits are also in line with the baseline estimates. Debt begins to decline in the post-bailout period. The treatment effect turns negative for the five-year bailout dummy. Deficits in bailout municipalities are consistently lower than in neighboring municipalities after a bailout, and remain so for up to ten years. These estimates are also

consistently significant. Similarly, redemptions are consistently larger after a municipality receives a bailout.

While the insignificance of the bailout dummies in the expenditure regressions are a source for concern, the reason is presumably that taking the average for all neighboring no bailout municipalities does not result in a good counter-factual. Neighboring municipalities sometimes have vastly different characteristics (for example, many smaller municipalities surround urban centers), and therefore developments in a bailout municipality and average developments in its neighboring municipalities might not always be sufficiently comparable. As an alternative, therefore, I have also run regressions where I match each bailout municipality with the contiguous no-bailout municipality that has the most similar development in the outcome variables in the pre-sample period (as above according to propensity scores). The results are more in line with the baseline estimates for the expenditure items (unreported but available upon request).

6.3   Placebo regressions

As a further robustness test, I run a set of placebo regressions for each of the outcome variable. The placebo regressions are based on the baseline regressions insofar as bailout municipalities are matched to no-bailout municipalities according to propensity scores derived from pre-sample developments in fiscal variables. The difference here is that I redefine the bailout dummies such that treatments systematically sets in at dates that do not correspond to the correct bailout dates. More specifically, I run simulations after shifting for each bailout municipality the bailout dummies by some random number of years. I run these simulation twenty times for each each of the five bailout dummies. These simulations result in 100 placebo coefficient estimates for each outcome variable.

In the spirit of DellaVigna and La Ferrara (2010), Figure 4 collects for each outcome variable the cumulative distribution of the z-statistics for the bailout dummy estimates from

the placebo regressions. The majority of the z-statistics should be insignificant since the true bailout periods do in general not overlap with the placebo periods. To asses the significance of the placebo coefficients, I indicate the 10% significance levels (z=1.65) with vertical lines in each plot.

The placebo bailout coefficients for the expenditure categories are rarely significant. In particular, only about 10 of the 100 coefficient estimates are significant for personnel expenditures, and most of these significant estimates have a positive sign, while the baseline estimates are consistently negative. Similarly, for most other expenditure items, there are only a few significant coefficient estimate, and a large fraction of these are positive.

For the revenue variables, the conclusions are similar. For tax revenues, there are only a few significant coefficient estimates, and about half of these are negative, unlike in the baseline regressions where the coefficients were consistently positive. For the tax multipliers, there are a few significant estimates, but the majority of them is again negative.

For debt and deficits, I do not find strong evidence for the significance of the placebo treatments either. For debt, there are some significantly negative coefficients, but an equal number of positive coefficients as well; and most estimates are insignificant. For deficits, the conclusion is very similar, there are barely any significant placebo estimates. For redemptions, there are a number of significantly positive estimates, but the majority of the estimates are again insignificant.

Overall, the placebo regressions cannot replicate the baseline results, especially not their consistency across different models. Hence, they confirm that the significant effects found for the bailout dummies in the baseline models were not merely a statistical artifact.

7 Conclusion

This paper studies the fiscal consequences of municipal bailouts in the German State of Hesse. It asks, in particular, whether the austerity conditions that accompany bailouts incentivize municipalities to consolidate their budgets. To separate the effects of bailouts from group specific trends, I compare fiscal developments in bailout municipalities with developments in no-bailout municipalities that were presumably subject to similar trends. I find that municipalities that receive a bailout and are consequently subject to austerity conditions reduce various expenditures, in particular personnel, social, and construction expenditures, raise local tax rates, in particular property taxes, and reduce debt and deficits. These results indicate that bailouts entail fiscal costs for the recipients, because the state government is both able and willing to impose austerity. Municipal employees, beneficiaries of social and construction expenditures, and property tax payers bear most of the austerity costs.

One reason why personnel expenditures decline is presumably that this is one of the largest items in municipal budgets and thus offers significant potential for savings. Construction is also a fairly large budget item, and is often financed by debt as buildings, streets, or bridges are typically considered as investments. Social expenditures, too, are a large item. Moreover, the beneficiaries of social expenditures have presumably little political clout. That property owners bear a larger share of the austerity costs than businesses can also be explained by different degrees of political influence. In most municipalities, a small number of firms pay most of the business tax revenues. These firms can credibly threaten to move to low tax municipalities if business tax rates are raised too much.

That bailouts entail austerity indicates that it is, in principle, possible to maintain both the flexibility to grant bailouts while avoiding deliberate subnational over-borrowing. This possibility might be the reason why many countries do not impose a strict hard budget constraint. However, despite the prevalence and apparent effectiveness of austerity conditions,

little is known about how they are decided upon and how they should be designed to ensure allocative efficiency and redistributive equity. An interesting avenue for future theoretical work on subnational bailouts would be to analyze the structure and consequences of austerity conditions. This analysis could proceed either from a political economy perspective, deriving e. g. how ideological alignment between higher-level governments and the bailout recipients influence the stringency of austerity programs, or from a welfare economics perspective, analyzing e. g. how to structure austerity programs in order to maximize social welfare.

Another avenue for future research relates to the external validity of the findings in this paper. Despite their conclusiveness, it is possible that austerity conditions are ineffective in other settings, in particular if politically powerful actors – state governments in federations or nation states in political unions such as the EU – are the recipients of bailouts. The advantage that the Hessian state government has in its relations with the municipalities is that the latter are clearly subordinate. The state government can not only refuse to pay additional bailouts to an obstinate bailout recipient in the future, it can in principle dismiss the local administration and appoint a special commissioner tasked with balancing the budget. Hence, there are strong sanctioning mechanism in the background, even if they are rarely used in practice. In other settings where the recipients of bailouts are politically more independent, they might simply refuse to comply with the austerity conditions once they have received a bailout. In order to gain a comprehensive understanding about when austerity conditions are effective, future empirical work should focus on the fiscal consequences of bailouts in settings where subnational governments have significant legislative independence and political power.

Acknowledgments

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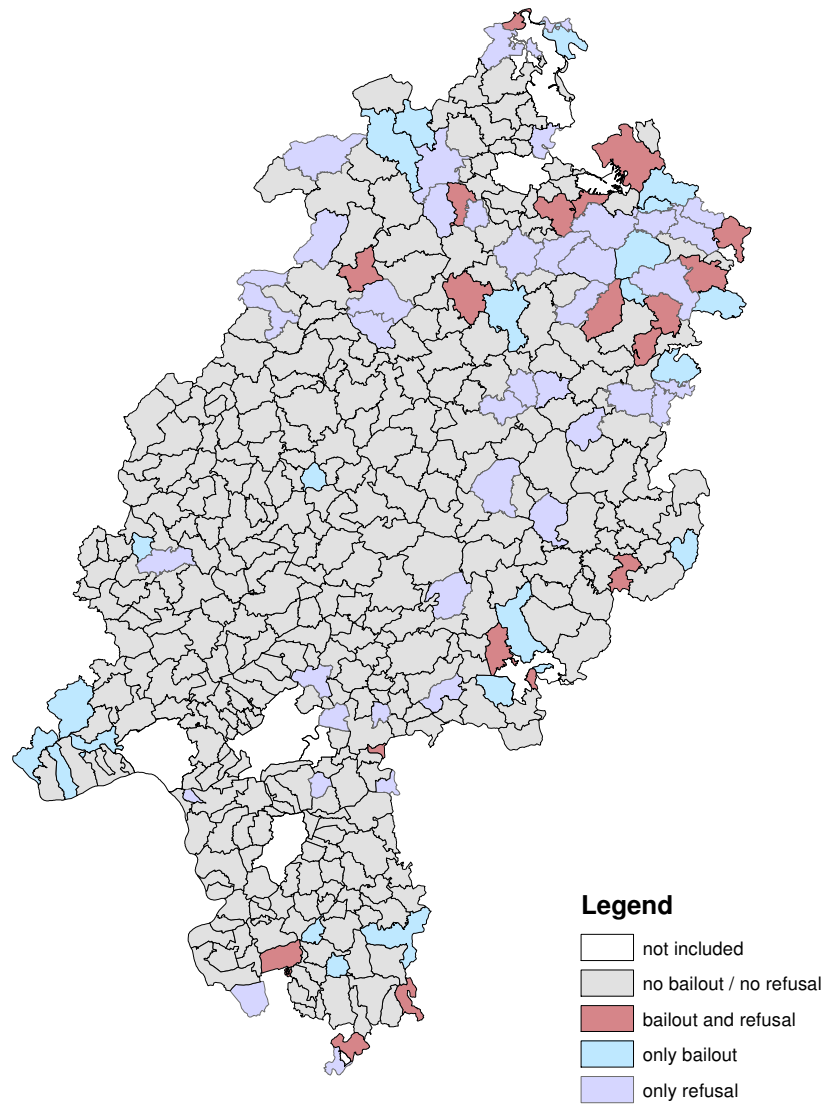
Figure 1: Bailouts and refused bailouts in Hesse. This figure indicates the municipalities that during the sample period (a) received at least one bailout and were never refused one, (b) were at least refused a bailout once and never received a bailout, and (c) both received and were refused a bailout at least once. County-free cities and forest areas are dropped.
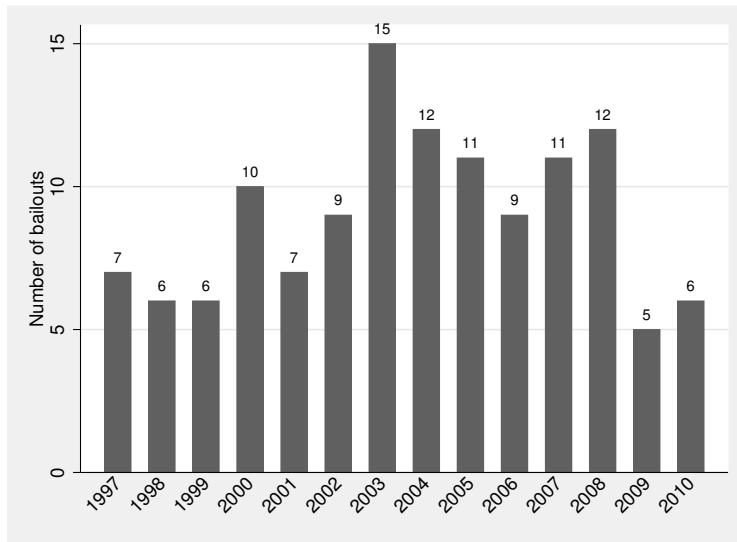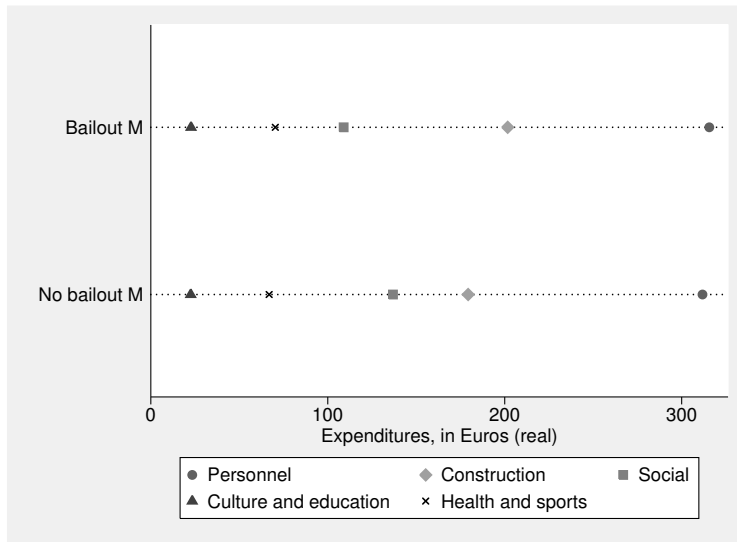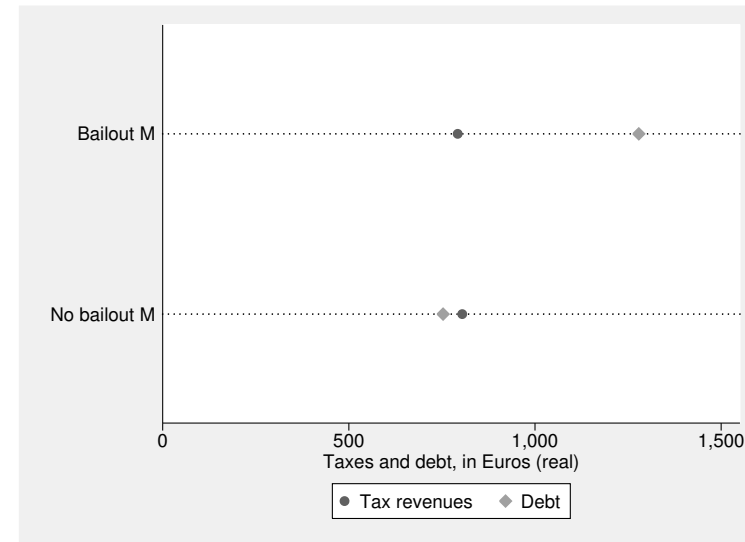
(a) Number of bailouts per year
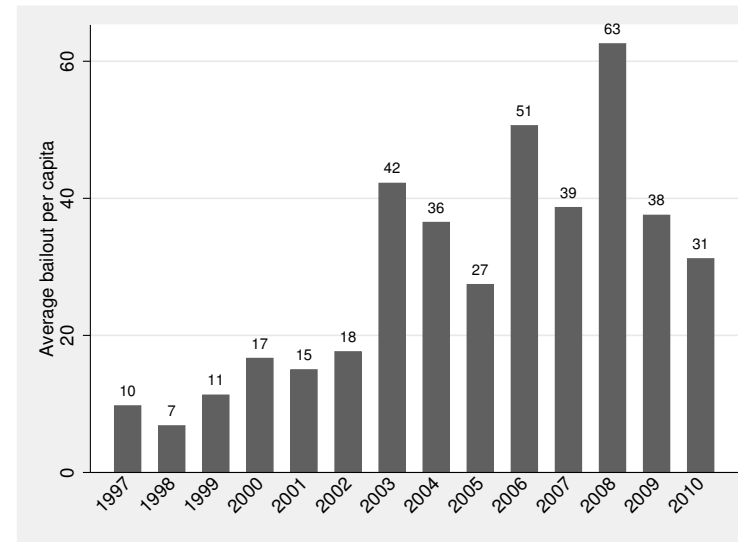
(b) Average (real) bailout payments per capita per year

Figure 2: Descriptive statistics on bailouts: This figure presents the number and volume of bailouts across municipalities in Hesse during the 1997-2010 period.

(a) Expenditures p.c.

(b) Tax revenues and debt p.c.
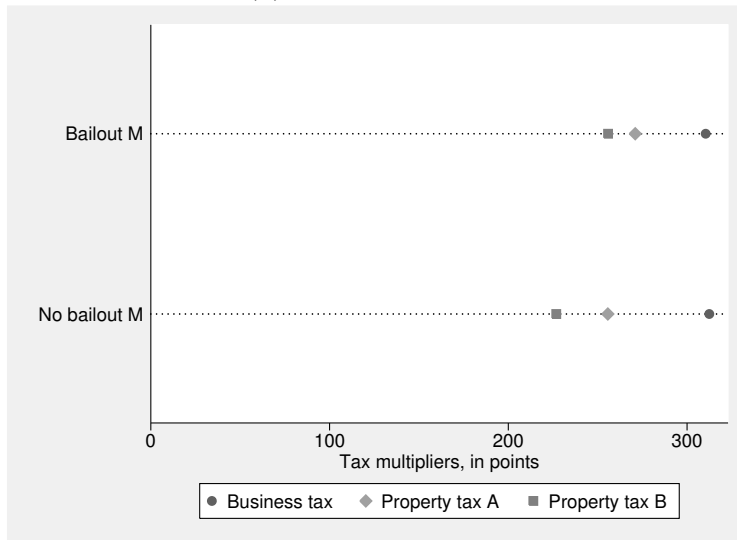
(c) Tax multipliers
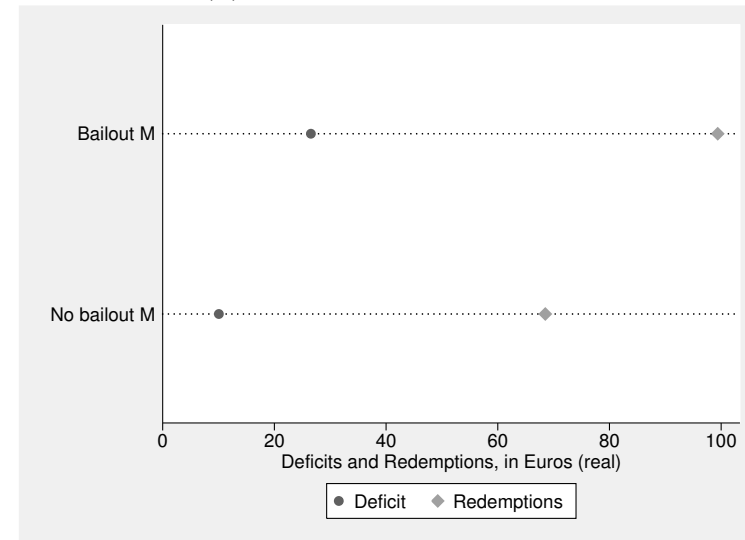
(d) Borrowing

Figure 3: Fiscal variables in bailout and no-bailout municipalities: This figure shows the pre-sample (1990-1996) average value for various fiscal variables in bailout and no-bailout municipalities during the 1997-2010 period. All variables except the tax multipliers are in real per capita values.
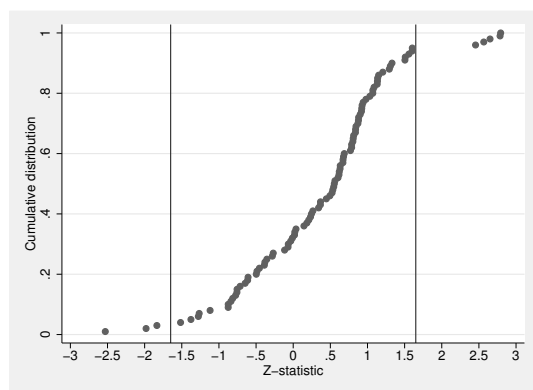
(a) Personnel
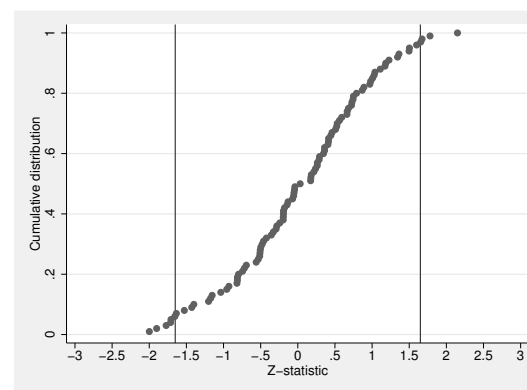
(b) Construction
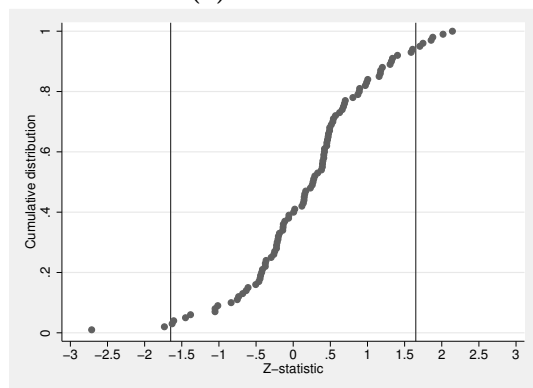
(c) Social
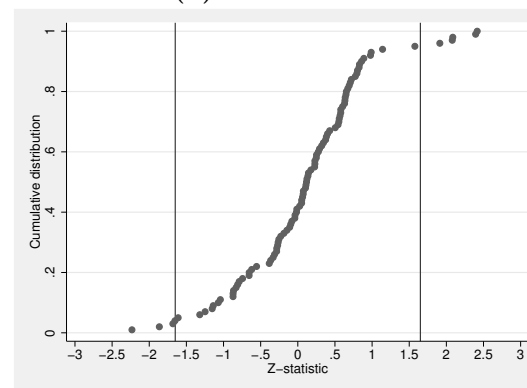
(d) Culture and eduction
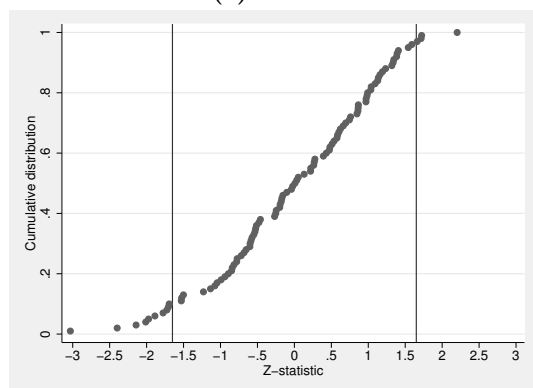
(e) Health and sports
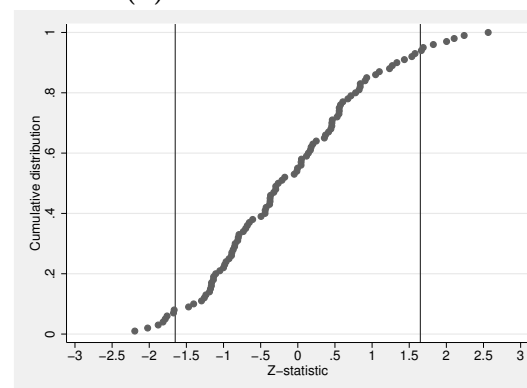
(f) Total tax revenues

Figure 4: Placebo regressions This figure shows z-statistics from placebo regressions where each of the bailout dummies is perturbed randomly for each municipality.
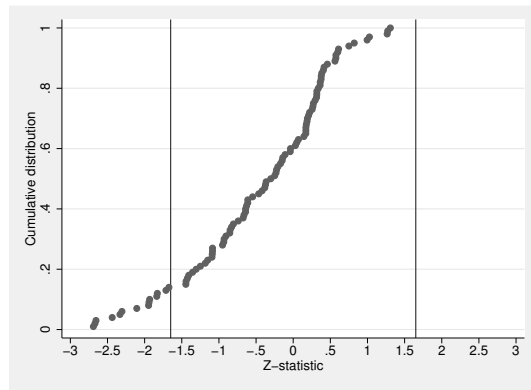
(g) Business tax rate
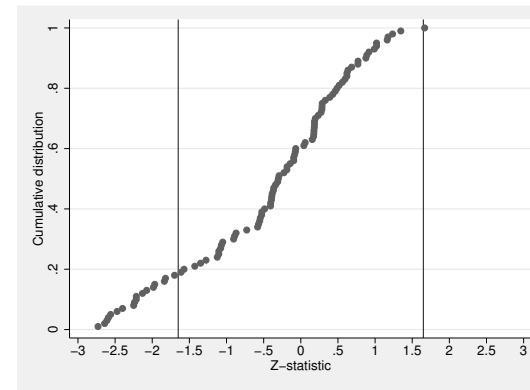
(h) Property tax A rate

(i) Property tax B rate
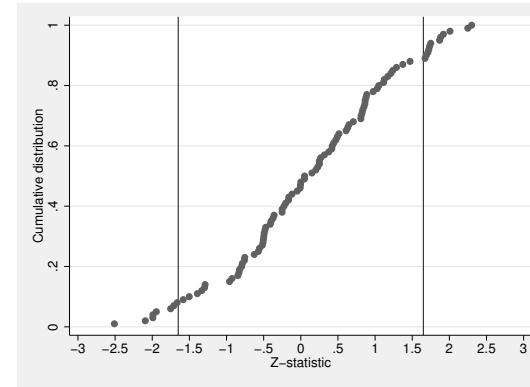
(j) Debt
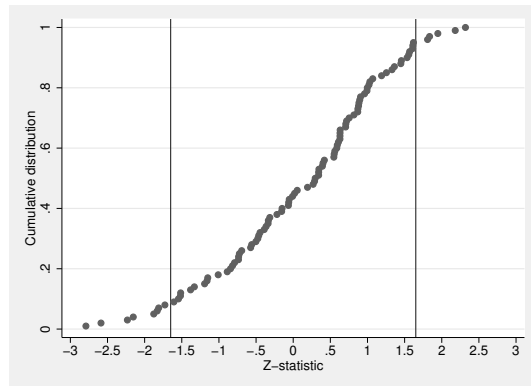
(k) Deficit
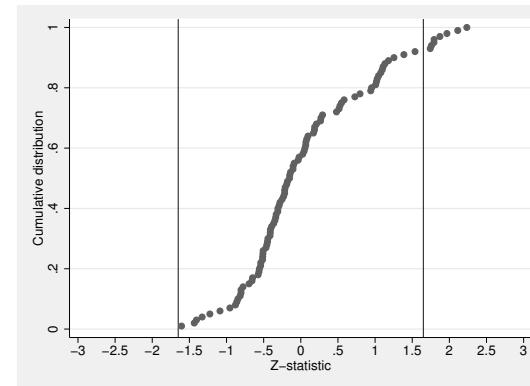
(l) Redemption

Figure 4: Placebo regressions This figure shows z-statistics from placebo regressions where each of the bailout dummies is perturbed randomly for each municipality.

Table 1: Average change and level of fiscal variables in bailout and matched no-bailout municipalities in the pre-sample period (1990-1996)

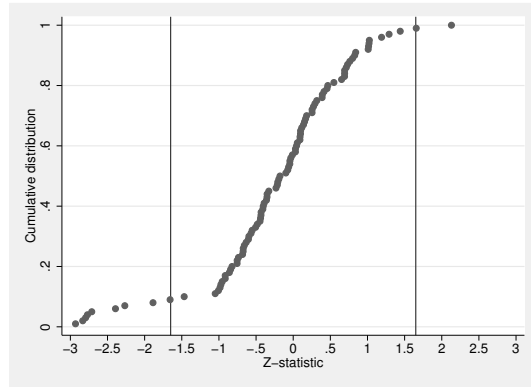|	Change			Level		
	Mean B (SD)	Mean NB (SD)	Difference t-stat (p-value)	Mean B (SD)	Mean NB (SD)	Difference t-stat (p-value)
Expenditures						
Personnel	0.009	0.009	0.101	-1.175	-1.212	1.689*
	(0.045)	(0.049)	(0.920)	(0.216)	(0.271)	(0.092)
Construction	-0.029	-0.006	-0.550	-1.737	-2.006	5.564***
	(0.396)	(0.441)	(0.583)	(0.546)	(0.530)	(0.000)
Social	0.041	0.061	-0.604	-2.402	-2.087	-6.217***
	(0.382)	(0.290)	(0.547)	(0.593)	(0.544)	(0.000)
Culture and eduction	-0.049	-0.023	-0.503	-4.214	-4.358	1.427
	(0.497)	(0.504)	(0.616)	(1.003)	(1.030)	(0.155)
Health and sports	-0.032	-0.045	0.295	-3.013	-3.130	1.332
	(0.486)	(0.397)	(0.768)	(0.951)	(0.980)	(0.184)
Revenues						
Total tax revenues	-0.019	-0.009	-0.755	-0.258	-0.257	-0.047
	(0.158)	(0.115)	(0.451)	(0.215)	(0.255)	(0.963)
Business tax rate	2.250	1.625	0.884	310.417	311.813	-0.564
	(8.110)	(6.999)	(0.378)	(24.440)	(34.184)	(0.573)
Property tax A rate	2.100	2.100	0.000	271.000	244.950	6.911***
	(9.234)	(9.581)	(1.000)	(43.681)	(35.254)	(0.000)
Property tax B rate	2.125	1.100	1.570	255.813	223.542	9.126***
	(7.805)	(6.018)	(0.118)	(37.629)	(39.335)	(0.000)
Borrowing						
Debt	-0.022	-0.035	0.712	7.030	6.363	9.609***
	(0.145)	(0.214)	(0.477)	(0.570)	(0.827)	(0.000)
Deficit	0.010	0.014	-0.136	-0.022	-0.035	0.712
	(0.206)	(0.263)	(0.892)	(0.145)	(0.214)	(0.477)
Redemption	-0.028	-0.026	-0.026	4.306	3.968	4.322***
	(0.744)	(0.823)	(0.979)	(0.693)	(0.982)	(0.000)

This table compares differences in the change and the level (both in logs) of the outcome variables between bailout municipalities and their respective matched municipalities in the matching regressions during the pre-sample period (1990-1996).

Table 2: THE EFFECT OF BAILOUTS ON FISCAL AND ECONOMIC OUTCOMES, HESSIAN MUNICIPALITIES, 1997-2010, BAILOUT MUNICIPALITIES AND MATCHES

	t=1	t=3	t=5	t=7	t=10
Expenditures					
Personnel	-0.029	-0.049**	-0.055**	-0.044	-0.018
	(0.020)	(0.025)	(0.027)	(0.030)	(0.032)
Construction	-0.078	-0.144*	-0.105	-0.066	-0.072
	(0.062)	(0.086)	(0.108)	(0.100)	(0.110)
Social	-0.097**	-0.094*	-0.115**	-0.134**	-0.107
	(0.044)	(0.051)	(0.058)	(0.068)	(0.067)
Culture and education	-0.206*	-0.058	0.041	-0.024	-0.063
	(0.124)	(0.141)	(0.159)	(0.172)	(0.201)
Health and sports	0.059	0.157	0.128	0.153	0.195
	(0.109)	(0.161)	(0.153)	(0.147)	(0.154)
Revenues					
Total tax revenues	0.067***	0.044**	0.061***	0.073***	0.064**
	(0.018)	(0.019)	(0.023)	(0.025)	(0.027)
Business tax rate	2.388*	4.400**	5.391*	5.267*	3.575
	(1.428)	(1.946)	(2.807)	(2.899)	(2.210)
Property tax A rate	7.418***	9.781***	12.690***	12.750***	13.212**
	(2.298)	(2.823)	(4.071)	(4.854)	(5.167)
Property tax B rate	12.213***	14.588***	17.277***	18.321***	15.198***
	(2.866)	(3.571)	(5.382)	(6.178)	(5.480)
Borrowing					
Debt	0.033	-0.062	-0.129	-0.132	-0.086
	(0.064)	(0.092)	(0.102)	(0.109)	(0.119)
Deficit	-0.075***	-0.076***	-0.075***	-0.077***	-0.082***
	(0.017)	(0.020)	(0.027)	(0.021)	(0.023)
Redemption	0.130*	0.065	0.063	0.098	0.154
	(0.076)	(0.113)	(0.133)	(0.138)	(0.119)

This table presents difference in difference regression results relating bailouts to fiscal outcomes. The sample consists of all Hessian municipalities that received at least one bailout during the sample period (N=40). The dependent variable is the difference of each outcome variable, as indicated in the relevant row, between each bailout municipality and its match based on pre-sample developments in fiscal variables. The sample period is 1997-2010. Estimates for the effect of bailouts after (i) 1, (ii) 3, (iii) 5, (iv) 7, and (v) 10 years are collected. Hypothesis tests are conducted with heteroscedasticity robust standard errors. Standard errors are also clustered at the level of the municipality. Stars indicate significance levels at 10% (*), 5% (**) and 1%(***).

Table 3: THE EFFECT OF BAILOUTS ON FISCAL AND ECONOMIC OUTCOMES, HESSIAN MUNICIPALITIES, 1997-2010, BAILOUT AND REFUSED-BAILOUT MUNICIPALITIES

	t=1	t=3	t=5	t=7	t=10
Expenditures					
Personnel	-0.038*	-0.051**	-0.057*	-0.044	-0.033
	(0.020)	(0.026)	(0.033)	(0.032)	(0.036)
Construction	-0.048	-0.149**	-0.146*	-0.133*	-0.140
	(0.052)	(0.073)	(0.078)	(0.078)	(0.086)
Social	-0.090**	-0.062	-0.094	-0.118*	-0.089
	(0.039)	(0.050)	(0.058)	(0.062)	(0.070)
Culture and education	-0.091	-0.100	-0.094	-0.150	-0.187
	(0.134)	(0.166)	(0.196)	(0.196)	(0.223)
Health and sports	0.087	0.043	0.037	0.059	0.047
	(0.088)	(0.132)	(0.130)	(0.110)	(0.126)
Revenues					
Total tax revenues	0.043**	0.040**	0.043*	0.050**	0.049*
	(0.019)	(0.020)	(0.024)	(0.024)	(0.026)
Business tax rate	-0.050	1.822	3.558	3.767	2.636
	(1.726)	(2.226)	(2.593)	(3.027)	(3.255)
Property tax A rate	2.940	5.468*	6.342	4.024	3.109
	(2.618)	(3.198)	(3.976)	(4.028)	(4.112)
Property tax B rate	5.688*	5.687*	6.843*	4.343	3.611
	(2.943)	(3.236)	(3.763)	(3.716)	(3.781)
Borrowing					
Debt	0.113**	0.078	0.005	-0.070	-0.065
	(0.056)	(0.072)	(0.080)	(0.097)	(0.095)
Deficit	-0.025	-0.055**	-0.088***	-0.081***	-0.078***
	(0.020)	(0.023)	(0.028)	(0.027)	(0.030)
Redemption	0.168**	0.206**	0.182*	0.131	0.172
	(0.072)	(0.093)	(0.110)	(0.112)	(0.116)

This table presents difference in difference regression results relating bailouts to fiscal outcomes. The sample consists of those Hessian municipalities that (i) received and (ii) that were refused at least one bailout (N=40). The dependent variable is the difference of each outcome variable, as indicated in the relevant row, between each bailout municipality and its match based on pre-sample developments in fiscal variables. The sample period is 1997-2010. Estimates for the effect of bailouts after (i) 1, (ii) 3, (iii) 5, (iv) 7, and (v) 10 years are provided. Hypothesis tests are conducted with heteroscedasticity robust standard errors. Standard errors are also clustered at the level of the municipality. Stars indicate significance levels at 10% (*), 5% (**) and 1%(***).

Table 4: THE EFFECT OF BAILOUTS ON FISCAL AND ECONOMIC OUTCOMES, HESSIAN MUNICIPALITIES, 1997-2010, BAILOUT AND NEIGHBORING MUNICIPALITIES

	t=1	t=3	t=5	t=7	t=10
Expenditures					
Personnel	-0.012	-0.019	-0.000	0.014	0.020
	(0.013)	(0.017)	(0.019)	(0.021)	(0.022)
Construction	-0.036	-0.102	-0.099	-0.104	-0.115
	(0.041)	(0.064)	(0.077)	(0.071)	(0.079)
Social	-0.063*	-0.022	-0.046	-0.048	-0.025
	(0.037)	(0.044)	(0.047)	(0.057)	(0.064)
Culture and education	-0.051	0.055	0.043	0.020	0.024
	(0.104)	(0.118)	(0.126)	(0.129)	(0.147)
Health and sports	0.073	0.052	0.014	0.040	0.040
	(0.096)	(0.126)	(0.132)	(0.135)	(0.156)
Revenues					
Total tax revenues	0.072***	0.047***	0.057**	0.060***	0.056**
	(0.016)	(0.017)	(0.023)	(0.022)	(0.023)
Business tax rate	0.771	1.231	2.125	1.827	1.823
	(1.390)	(1.639)	(1.923)	(2.154)	(2.363)
Property tax A rate	3.988**	6.115***	6.545**	6.712**	7.583**
	(1.830)	(2.329)	(2.677)	(3.035)	(3.524)
Property tax B rate	4.738**	4.773**	5.989**	6.452**	6.020*
	(2.263)	(2.419)	(2.638)	(3.072)	(3.249)
Borrowing					
Debt	0.002	0.010	-0.007	-0.064	-0.048
	(0.058)	(0.050)	(0.063)	(0.081)	(0.093)
Deficit	-0.063***	-0.047*	-0.074***	-0.071***	-0.058**
	(0.020)	(0.025)	(0.025)	(0.026)	(0.029)
Redemption	0.140**	0.149*	0.161**	0.135	0.158
	(0.064)	(0.076)	(0.079)	(0.090)	(0.101)

This table presents difference in difference regression results relating bailouts to fiscal outcomes. The sample consists of all Hessian municipalities that received at least one bailout during the sample period (N=40). The dependent variable is the difference of each outcome variable, as indicated in the relevant row, between each bailout municipality and the average of all contiguous no-bailout municipalities. The sample period is 1997-2010. Estimates for the effect of bailouts after (i) 1, (ii) 3, (iii) 5, (iv) 7, and (v) 10 years are provided. Hypothesis tests are conducted with heteroscedasticity robust standard errors. Standard errors are also clustered at the level of the municipality. Stars indicate significance levels at 10% (*), 5% (**) and 1%(***).

Table A.1: DEFINITION AND SOURCE OF VARIABLES

Label	Description	Source
Expenditures		
Personnel	Log of real personnel expenditures per capita.	Hessian State Statistical Office
Construction	Log of real expenditures for construction per capita.	Hessian State Statistical Office
Social	Log of real social expenditures per capita.	Hessian State Statistical Office
Culture and eduction	Log of real expenditures per capita for culture and education.	Hessian State Statistical Office
Health and sports	Log of real expenditures per capita for health and sports.	Hessian State Statistical Office
Revenues		
Total tax revenues	Log of real total total tax revenues per capita.	Hessian State Statistical Office
Business tax rate	Business tax multipler.	Hessian State Statistical Office
Property tax A rate	Property tax A multiplier.	Hessian State Statistical Office
Property tax B rate	Property tax B multiplier.	Hessian State Statistical Office
Borrowing		
Debt	Log of debt per capita.	Hessian municipal statistics yearbook (*Hessiche Gemeindestatistik*)
Deficit	First difference of log debt per capita.	Hessian municipal statistics yearbook (*Hessiche Gemeindestatistik*
Redemption	Log of real debt redemptions per capita.	Hessian municipal statistics yearbook (*Hessiche Gemeindestatistik*

Table A.2: SUMMARY STATISTICS, BAILOUT MUNICIPALITIES

Variable		Mean	Std.	Min.	Max.	Obs.
Expenditures						
Personnel	overall	-1.121	0.260	-2.066	-0.515	559
	between		0.242	-1.669	-0.677	40
	within		0.102	-1.877	-0.760	13.975
Construction	overall	-1.953	0.520	-3.752	-0.040	560
	between		0.356	-2.824	-1.347	40
	within		0.383	-3.536	0.126	14.000
Social	overall	-2.140	0.456	-4.969	-0.901	560
	between		0.340	-2.861	-1.481	40
	within		0.308	-4.819	-0.472	14.000
Culture and eduction	overall	-4.166	1.035	-7.120	-0.491	560
	between		0.777	-5.856	-2.690	40
	within		0.695	-6.281	-1.108	14.000
Health and sports	overall	-3.281	1.087	-7.054	-0.269	560
	between		0.909	-5.235	-1.718	40
	within		0.611	-5.348	-0.543	14.000
Revenues						
Total tax revenues	overall	-0.225	0.222	-1.248	0.948	560
	between		0.138	-0.383	0.319	40
	within		0.175	-1.122	0.768	14.000
Business tax rate	overall	325.911	23.229	270.000	390.000	560
	between		22.053	295.000	390.000	40
	within		8.035	296.625	357.339	14.000
Property tax A rate	overall	292.591	37.273	200.000	350.000	560
	between		34.910	215.714	350.000	40
	within		14.105	228.305	349.163	14.000
Property tax B rate	overall	280.307	34.777	200.000	350.000	560
	between		31.585	211.429	330.000	40
	within		15.331	216.021	316.021	14.000
Borrowing						
Debt	overall	7.031	0.646	1.029	8.488	558
	between		0.508	5.670	8.330	40
	within		0.408	1.695	8.607	13.950
Deficit	overall	0.002	0.195	-1.893	1.043	517
	between		0.041	-0.125	0.101	40
	within		0.191	-1.765	1.031	12.925
Redemptions	overall	4.292	0.723	1.073	6.718	556
	between		0.537	3.177	5.435	40
	within		0.493	1.841	5.994	13.900

The means for expenditures, tax revenues, debt, and redemptions are for the logged values.

Table A.3: SUMMARY STATISTICS, NO-BAILOUT MUNICIPALITIES

Variable		Mean	Std.	Min.	Max.	Obs.
Expenditures						
Personnel	overall	-1.129	0.278	-4.472	-0.133	5327
	between		0.243	-1.730	-0.285	381
	within		0.136	-4.195	-0.634	13.982
Construction	overall	-1.946	0.490	-6.123	0.772	5327
	between		0.326	-3.028	-0.884	381
	within		0.367	-5.461	0.002	13.982
Social	overall	-1.909	0.466	-7.387	-0.280	5326
	between		0.395	-3.155	-0.652	381
	within		0.248	-6.340	-0.018	13.979
Culture and eduction	overall	-4.205	1.086	-9.832	-0.283	5325
	between		0.912	-8.131	-1.556	381
	within		0.590	-8.947	-0.380	13.976
Health and sports	overall	-3.273	1.105	-9.902	-0.147	5319
	between		0.969	-7.803	-1.039	381
	within		0.552	-6.818	0.378	13.961
Revenues						
Total tax revenues	overall	-0.183	0.265	-1.720	2.418	5333
	between		0.223	-0.445	1.604	381
	within		0.144	-1.929	1.110	13.997
Business tax rate	overall	322.450	28.288	200.000	430.000	5334
	between		26.917	250.000	430.000	381
	within		8.801	231.021	387.450	14.000
Property tax A rate	overall	265.318	43.474	0.000	450.000	5298
	between		43.521	0.000	450.000	381
	within		12.799	40.318	391.746	13.906
Property tax B rate	overall	250.460	37.232	140.000	400.000	5334
	between		32.790	152.857	364.286	381
	within		17.709	170.460	391.174	14.000
Borrowing						
Debt	overall	6.311	0.886	0.031	8.622	5296
	between		0.820	2.161	7.966	381
	within		0.405	2.888	9.798	13.900
Deficit	overall	0.020	0.273	-5.096	4.173	4910
	between		0.083	-1.101	0.362	380
	within		0.264	-4.378	4.071	12.921
Redemptions	overall	3.695	0.941	-2.706	6.839	5294
	between		0.748	-0.352	5.192	381
	within		0.586	0.193	8.069	13.895

The means for expenditures, tax revenues, debt, and redemptions are for the logged values.