The political economy of the German Länder deficits: weak governments meet strong finance ministers

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The political economy of the German Länder deficits

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Abstract

We analyze the deficits of the German Länder (regional states) for the period from 1960 to 2005 and test a number of hypotheses derived from the literature on the political economy of public deficits. Estimating a dynamic panel data model, we find evidence for an opportunistic political business cycle: German voters seem to favor fiscal discipline as debt issue is significantly lower in pre-election years. Coalition governments with a weak finance minister issue significantly more debt than single-party governments while there is no difference in borrowing between single-party governments and coalitions with a strong finance minister. There is no evidence for partisan behavior; so, party ideology seems to play a negligible role.

Keywords: Public Deficit, German Länder, Political Economy, Dynamic Panel Data Model.

JEL classification numbers: D72, E60, H62.

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1. Introduction

Public deficits vary widely between jurisdictions. It is broadly accepted that economic variables such as economic growth or the interest rate alone cannot explain these differences. In fact, political variables and political institutions play an important role in the development of public debt (Persson and Tabellini, 1997). When comparing different countries, however, one can hardly disentangle the effect of political variables and the impact of political institutions. This identification problem disappears when the influence of political variables on public debt in the German states (Länder) is analyzed as the jurisdictions have almost identical political institutions and electoral rules. However, they differ quite substantially in other dimensions such as fiscal policy outcomes and per capita income. Thus, our data set offers a promising opportunity to solely test for the influence of political variables on public deficits.

We test a number of hypotheses taken from the theoretical literature on the political economy of debt issue. The empirical literature on opportunistic behavior, where political behavior is solely designed to win the next elections, gives no clear picture. Nordhaus (1975) finds evidence of this for two out of four elections in the United States. Galli and Rossi (2002) find only weak support for the opportunistic school. Evidence for partisan politics, where policy is primarily driven by party ideology, is also mixed. Alesina (1989), Boix (2000), Cusack (1997), Hibbs (1977), Tavares (2004) and Reed (2006), for example, find support for the partisan theory, whereas Heckelman (2002), Seitz (2000), and Galli and Rossi (2002), for instance, find no evidence for it.[3]

Coalition governments are expected to issue more debt than single-party governments. Again, empirical evidence is not clear cut. Roubini and Sachs (1989) find support. Re-estimating the

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[3]Table 1 in the Appendix offers a more detailed (but still partial) review of the empirical literature related to our

Roubini and Sachs model, Edin and Ohlsson (1991) challenge their view and argue that the coalition effect identified in Roubini and Sachs (1989) is a result of minority governments rather than political fragmentation. Using a different data set, de Haan and Sturm (1997) find no coalition effects – neither with the dispersion index used by Roubini and Sachs nor with the one used by Edin and Ohlsson. In a recent study on debt of the Flemish municipalities, Ashworth, Geys and Heyndels (2005) find that political fragmentation affects local indebtedness only in the short but not in the long run. Within a coalition government, a strong finance minister can make the rest of the cabinet consider the full costs of increased borrowing. This, in turn, may result in lower deficits (see von Hagen, 1992; Hallerberg and von Hagen, 1999).

We estimate a dynamic panel data model and find evidence for opportunistic behavior: debt issue is significantly lower in pre-election years. Thus, German voters seem to favor fiscal discipline or, at least, the incumbent may believe they do. There is no evidence for partisan behavior. We find evidence for coalitions issuing more debt than single party governments. This effect, however, vanishes if the finance minister and the prime minister have the same party affiliation - a situation where the finance minister is likely to be in a strong position.

The first econometric study with German data that considers political variables as covariates is Frey and Schneider (1979). The current paper, however, is most related to Seitz (2000) and Galli and Rossi (2002) who also analyze the political economy of German Länder fiscal policy. Seitz considers the time period from 1976 to 1996 whereas Galli and Rossi analyze the period from 1974 to 1994. While Seitz concentrates on partisan politics and deficit data, Galli and Rossi are more ambitious and additionally test for political business cycles using deficits, expenditures and expenditure categories. Although we focus on public deficits, we extend these two studies along three lines. First, we explicitly address the role of coalition governments and the position of the finance minister within a coalition government and

study.

thereby gain a number of new insights. Second, due to the availability of better estimation methods, we also econometrically go beyond Seitz and Galli and Rossi. Seitz only applies the least squares dummy variable estimator. As this estimator may be severely biased in short panels we use the bias corrected least squares dummy variable estimator that clearly outperforms the uncorrected version (see, e.g., Bruno, 2005). Galli and Rossi deal with heteroscedasticity but also ignore the bias. Finally, we use data from 1960 to 2005 and thereby extend the analysis from 21 to 46 years. Note that this extension is crucial since all three studies rely on within state variation to tease out statistically significant covariates.[4]

The paper is organized as follows. In Section 2 the institutional background for Germany is provided. The hypotheses to be tested are derived in Section 3. We thereby review the theoretical literature on the political economy of debt issue. The empirical model and the different estimators applied are introduced in Section 4. The data set and the results are presented in Section 5. Finally, Section 6 offers some concluding remarks.

2. Institutional background

1. Germany's federal political design

The name "Federal Republic of Germany" (FRG) already highlights the country's federal structure that is reflected by the levels of government: federal (Bund), state (Land) and local (Gemeinde). Since German unification in 1990 Germany consists of sixteen Länder, the ten Länder of former West Germany, the five new Länder of former East Germany (German Democratic Republic, GDR), and Berlin. From World War II to unification, Berlin was divided into West Berlin and East Berlin, where the latter was the capital of the GDR. Additionally, there are about 14,000 cities and communities, which form the local level (Seitz,

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[4]We ran our empirical models on shorter time periods, including those used in Seitz (2000) and Galli and Rossi (2002) and largely lose significance. In other words, the differences in results between our paper and the other two are mainly due to the much longer time period that we consider.

2000, p. 188).

The Länder are not mere provinces, they are states endowed with their own powers. These powers and responsibilities are specified in the Basic Law (Grundgesetz), Germany's constitution. The Basic Law also guarantees the local authorities the right to independently administer their own affairs. As the local authorities rely heavily on grants from the states, their independence is rather limited. Three large German cities, namely, Berlin, Bremen and Hamburg, form their own states (Länder). These are the so-called "city-states" (Stadtstaaten) that do not have local administrative bodies. In contrast, the other German states are called "non-city-states" (Flächenländer). This distinction is important since the budgets of the city-states include expenditures and revenues that are part of the local budgets in non-city-states. Moreover, the expenditures of the non-city-states include grants to the local authorities whereas there are no such grants to local authorities in the city-states. Consequently, public expenditures or public debt of the two types of states are not directly comparable.[5]

Our study examines the budget deficits of the Länder without taking the local authorities into account. As mentioned above, local authorities have their own budgets and their own parliaments. Election dates typically differ between local and state jurisdictions. As a consequence, the aggregated local political structure will hardly ever match the political structure of the state. Since the state government cannot be held responsible for deficits at the local level (net of state grants) it is logically consistent to concentrate on state level debt and neglect deficits arising at the local level.[6]

2. Fiscal federalism in Germany

Although the Länder are endowed with their own powers, an almost total lack of tax setting autonomy exists. Additionally, a large fiscal equalization system harmonizes revenues across

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[5]In our empirical model, the state fixed effects account for that fundamental difference as well as for other time invariant state characteristics.
[6]For the same reason, the other two papers analyzing fiscal variables of the German states, namely, Seitz (2000)

states, calculated on the basis of several fiscal and economic indicators, and this strongly distorts incentives to increase the tax base. The situation in Germany, therefore, differs in one major aspect from the theoretical literature on the political economy of public expenditures: typically the government has two options for financing expenditures – taxes and debt. But, due to the lack of tax setting autonomy and the equalization scheme, total revenue of every Land is more or less fixed (for a more detailed overview see Seitz, 2000, pp. 188-190). To finance public expenditures, Länder governments only have one discretionary source of financing at their disposal, namely debt. We therefore concentrate on public deficits and their political determinants.

There are two more important aspects: First, in 1990, the five new Länder of former East Germany and East Berlin joined the FRG, enlarging the population from around 64 million to roughly 80 million, while the GDP only increased by less than 10 per cent. The integration of East Germany into the West German social security system, the huge investments in infrastructure and various other costs of transformation created a substantial fiscal shock. Although during the first five years after unification most of the direct financial burden was borne by the federal government via a so-called unification fund (Fonds Deutsche Einheit), we control for unification in our empirical analysis. Secondly, from 1995 onwards, the new German Länder, i.e., former East Germany, and Berlin were included in the fiscal equalization system. A large part of this equalization is amongst the Länder (horizontal equalization). As the new participants were net recipients, this introduced a fiscal burden on the Western Länder, an effect that we account for in our analysis.

Finally, two German states, namely Bremen and Saarland, were bailed out by the federal government. From 1994 onwards they received transfers over and above those of the fiscal equalization scheme. This bailout is likely to reduce debt issue in both states. Moreover, one could imagine that the occurrence of a federal bailout alters the incentives of the states to

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and Galli and Rossi (2002), also concentrate on data at the state level.

issue debt in general. Our empirical model considers all these aspects.

3. Political parties

In Germany, there are four major parties. The Christian Democratic Union (CDU), the Social Democratic Party (SPD), the Free Democratic Party (FDP) and the Green party (GREEN). While CDU, SPD and FDP ran for elections in the entire period under study here, the Greens did not. The Green party was founded in 1980 and first won parliamentary seats at the state level in Hamburg and Hesse in 1982 and at the federal level in 1983. Due to historical developments after World War II, the CDU has never run for elections in Bavaria. Instead their so-called sister party, the Christian Social Union (CSU), participates. The programs of CDU and CSU, however, are very similar and they always form one parliamentary group in the federal parliament (Bundestag). Therefore, we do not distinguish between them and label both CDU.

After unification, the Party of Democratic Socialism (PDS) was founded, a successor to the United Socialist Party (SED), the party that ruled East Germany for more than 40 years. Although the PDS has significant influence in the new Länder, it has not succeeded in gaining any influence in the Western Länder.[7] [8]As the democratic history of the East German states is rather short, we abstain from including them in our analysis. Due to its special status, Berlin is also eliminated from the data set (see below for more details).

Since 1960 the West German Länder have either been governed by majority governments of the CDU or SPD or by a coalition that mostly consisted of two parties. The SPD has formed coalitions with all three other parties, whereas the CDU has only formed coalitions with the SPD (a so-called 'grand coalition') or the FDP. Minority governments as well as other government constellations have played a negligible role. Table 3 in the Appendix provides,

[7]In 2007 the PDS merged with the WASG. The latter largely consisted of disappointed former social democrats and union members. The so-formed new party, DIE LINKE, is about to gain influence in West-German states.
[8]To some extent, Berlin is an exception as the SPD currently forms a coalition with the DIE LINKE. Note,

among other things, an overview of government formations in the West German states.[9]

3. The political economy of public deficits

There is a large number of conflicting theories explaining the formation and the evolution of public deficits. In this section we review (part of) the theoretical literature and derive the respective hypotheses to be tested in Section 5. Our focus is on three theories, namely, political opportunism, partisan theory, and fragmented governments.

1. Political opportunism

Opportunistic governments are assumed to be primarily interested in being reelected. There are no ideological motives. Although originally introduced in the context of the 'Phillips curve' (see Nordhaus, 1975; MacRae, 1977), Alesina and Perotti (1994) and Persson and Tabellini (1997) demonstrated that the theory of political opportunism can also be applied to public deficits: to appear competent to voters, the government has an incentive to boost the economy thereby improving the chances of being reelected. Such policies mostly require raising transfers or increasing public investments (e.g., infrastructure, housing, and hospitals). As German states are hardly able to influence their returns, the augmented public expenditure will result in (further) debt, especially in election years.[10]

This strategy only works if voters do not (fully) anticipate that the debt burden must be borne after the election. Rogoff and Sibert (1988) demonstrated, however, that opportunistic cycles may also occur under rational expectations. They developed a theory where opportunistic cycles originate in temporary information asymmetries between government and voters. The government tries to exploit its information advantage by running low deficits; this signals that the government can provide a given level of public goods reasonably efficiently. Since

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however, that today's Berlin is not a former Western Land.
[9]For an explanation of the variables see Table 2 in the Appendix.
[10]Of course, one may argue that if the election is early in the year expenditures should raise in the pre-election

deficits are visible to voters with a time lag, low deficits are expected to occur in pre-election years.

The main idea behind rational opportunistic cycles (Rogoff and Sibert, 1988) is the same as behind opportunistic cycles of the Nordhaus type: in order to win the upcoming election, the government is prepared to introduce distorted policies. In the former case this leads to lower deficits in pre-election years and, in the latter case, to higher deficits in election and possibly pre-election years. Without any time pattern in deficits around elections there is no evidence for opportunistic cycles.

Hypothesis 1: Public deficits in pre-election years and in election years are no different than in all other years.

Consider that a government can be sure of being reelected. In the German case, Bavaria serves as an example, where the CSU has been in office since the 1950s. Without any risk of being thrown out of office, there is no incentive to introduce distorted policies. Thus, finding no evidence for political opportunism may simply reflect political stability.

2. Partisan theory

Partisan theory suggests that government politics are primarily driven by ideological motives and, accordingly, predicts a more expansionary policy for left governments than for right governments. Left governments, for instance, are typically more inclined to favor redistributive policies. Public spending may therefore be directed towards mitigating income inequality by increasing transfers. With fixed returns, as in our case, such programs may require debt issue. In short, partisan theory suggests that if left governments are in office then debt issue will be higher than otherwise. To actually identify partisan effects, ideology of competing parties must be sufficiently different and ideally time invariant - and this is exactly

year. We discuss this in some detail in Section 5.1.

what was traditionally assumed (see, e.g., Hibbs, 1977).

It may be a bit naive to claim that a party's policy is solely driven by ideology; parties also care about winning the next election. But then the policies of two competing parties will converge unless voters are irrational or at least one party has a sufficiently low discount factor (Alesina, 1988). Since policy convergence precludes identification of partisan effects a closer look at this topic is warranted.

Consider a two-party system where both parties, right and left, are equally well informed and both care about winning elections. In electoral competition both parties will adopt the same platform – the one that maximizes the probability of being elected. If parties are not committed to their platform then, once elected, they implement their most favored policy. Irrational voters will not anticipate the parties' incentives to deviate from their platform and partisan effects may result. Alesina (1988) argued that this result also holds under rational expectations when electoral competition is considered a one-shot game. Rational voters anticipate the parties' incentive to deviate from any announced policy other than their optimal policy so that the only time-consistent equilibrium must have diverging platforms.

In an infinitely repeated game the ideological difference between parties may be blurred when a cooperative policy (that both parties agree upon prior to elections) can be supported as an equilibrium (see Alesina, 1988). If the elected party deviates and implements its most favored policy, then cooperation becomes incredible and parties end up playing their non-cooperative Nash strategies. Deviation is beneficial if the current gain of implementing the desired policy is larger than the future loss originating in the breakdown of cooperation. This is likely to be the case for low discount factors, a situation where reputation only plays a minor role. Note that sustainable cooperation precludes partisan effects even if parties' ideologies diverge. This is unproblematic for testing for partisan effects since we are not trying to identify different

ideologies but whether different ideologies find their way into fiscal policy.[11] We can, therefore, write our second hypothesis as:

Hypothesis 2: Deficits are independent of government ideology, that is, deficits of left governments are no different to deficits of right governments.

3. Fragmented governments

The theories discussed so far have modeled electoral competition between two parties that simultaneously aim at political power. With only two parties, there is no conflict once one party is elected. With more than two parties, coalition governments may arise, opening up another stage of conflict.

In a coalition government each coalition partner tries to allocate as much of the budget as possible to its constituency. Partners come up with spending proposals that are asymmetric in the sense that benefits primarily go to the respective constituency but costs are equally shared amongst coalition partners. Since costs are not fully internalized coalition governments face a common pool problem where too high spending proposals translate into higher budget deficits (Persson and Tabellini, 1997, pp. 68-71.) Since the fraction of internalized costs decreases with coalition size, borrowing is expected to increase with coalition size. Our next hypothesis, again in its null-form, can then be stated as follows:

Hypothesis 3: Borrowing is independent of how many parties form the government.

An obvious criticism of the Persson/Tabellini argument is that all partners have control over some part of the budget and none of the parties is responsible for the entire budget.

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[11]Note that we concentrate on public deficits. It may well be that there are no partisan effects in borrowing but in the structure of public spending (see, for instance, Drazen and Eslava, 2005).

Centralization of financial responsibility would yield efficiency.[12] The extent to which the common pool problem actually translates into higher debt depends on the degree of centralization of fiscal policy and thus on the position of the finance minister in the coalition government. Like most politicians, finance ministers will typically care about their prestige, which is partly determined by their ability to form a solid budget. As a result, the interests of the finance minister in terms of borrowing should be well aligned with those of the society (Hallerberg and von Hagen, 1999). A strong finance minister is therefore expected to be able to mitigate the common pool problem so that borrowing is lower as compared to coalitions with a weak finance minister.[13]

Hypothesis 4: The position of the finance minister in a coalition government has no impact on borrowing.

The actual strength of the finance minister is difficult to measure. If, however, finance and prime minister are members of the same party, then the position of the finance minister is likely to be stronger than otherwise.

4. Empirical model

In recent studies of public deficits or public expenditures the variable of interest has typically been transformed before running regressions. Cusack (1997) and Seitz (2000), for example, take its first difference as a share of the GDP. This is basically done in order to obtain stationary time series. We consider growth rates for the same purpose. The major advantage of our approach is that the GDP is not used in the construction of the dependent variable

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[12] An alternative theory that explains higher deficits for coalition governments is offered by Alesina and Drazen (1991) and Alesina and Perotti (1994, pp. 22-29): consider a permanent fiscal shock. Coalition partners will then fight about the allocation of the fiscal burden to the respective constituencies. This situation is well modeled by the 'war of attrition'. In general, delayed adjustment to the fiscal shock will obtain, allowing debt to accumulate.
[13] Von Hagen (1992) found that a strong finance minister, or a dominant prime minister, advances fiscal

which could otherwise be a source of endogeneity. We consider the following dynamic panel

data model

$$d_{it} = \gamma d_{i,t-1} + \beta_1' x_{it} + \beta_2' z_{it} + \mu_i + \varepsilon_{it}, \qquad (1)$$

where d_{it} denotes the nominal growth rate of public debt in state $i = 1,...,N$ at time $t = 1,...,T$

and $d_{i,t-1}$ its first lag, $t = 2,...,T$.[14] The political variables are summarized in the vector x_{it},

the control variables in z_{it}. We control for nominal GDP growth (GDP), the first and second

oil crisis (OIL1, OIL2) as well as for German unification (UNIFIC) and for the inclusion of

the East German states into the fiscal equalization scheme (EQUAL).[15] As an identifying

assumption we suggest that all relevant time effects are picked up by specifying a dynamic

model and by including the additional time control variables: the oil crises dummies and the

unification and fiscal equalization dummies. We refrain from adding time fixed effects to the

model. This would introduce (imperfect) multi-collinearity into the model and prevent us

from testing for political opportunism (Hypothesis 1) where variation over time is essential.[16]

Potential direct and indirect effects of the federal government bailout are picked up by the

variables BAILOUT, BAILOUTHB and BAILOUTSL, where the latter two are interactions

between the variables BAILOUT and the state fixed effects for Bremen (HB) and Saarland

(SL), respectively.[17] Finally, debt issue may respond to the financial costs of borrowing,

namely, the real interest rate (INTRATE). Note that the interest rate varies over time but not

over states. This limits the explanatory power to within state variation.

The time invariant state effect is given by μ_i. We will consider these effects as fixed rather

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discipline.

[14]Note that nominal debt growth is simply nominal deficit over nominal debt.

[15]Definition of all variables can be found in the Appendix in Table 2.

[16] Elections are not uniformly distributed over time. This is why year dummies are correlated with the variables ELECTION and PREELEC. While the ELECTION coefficient is never statistically different from zero we lose significance for PREELEC in a model with time fixed effects. All other results reported below are robust to this alternative specification.

[17]One may argue that debt issue is influenced by the possibility of a federal bailout so that the bailout variables are endogenous. Since debt started to accumulate in the 1970s in most states, about 20 years prior to the first and so far only bailout, it is hard to imagine that borrowing incentives were influenced by the possibility of a bailout.

than random. It can be argued that there is no room for random effects as the entire population, i.e., all ten West German states, are included in the study. A more substantial argument is the existence of long-lasting governments. Bavaria, for instance, was ruled by the CSU for the entire period considered here. North-Rhine Westphalia is an example of almost continuous SPD government. Obviously we will have $E(x_{it}\mu_i) \neq 0$, i.e. state fixed effects.[18] Random disturbance is $\varepsilon_{it} \sim N(0, \sigma_\tau^2)$. Let $w_{it}^{'} = (x_{it}^{'} \mid z_{it}^{'})$, then the assumptions of the model can be summarized as follows

$$E(\varepsilon_{it}\varepsilon_{js}) = 0 \text{ for } i \neq j \text{ or } t \neq s$$

$$E(\mu_i\varepsilon_{jt}) = 0 \text{ for all } i,j,t \quad (2)$$

$$E(w_{it}\varepsilon_{js}) = 0 \text{ for all } i,j,s,t$$

As is well known, the ordinary least squares (OLS) estimator is inconsistent when a dynamic panel data model, like the one in equation (1), is to be estimated. The estimates of γ will be biased upwards and the coefficients of the exogenous variables will be biased towards zero (see Hsiao, 1986, pp. 76-78). The fixed effects estimator (or Least-Squares Dummy Variable, LSDV, estimator) eliminates this source of inconsistency by taking account of the Länder fixed effects μ_i. There nevertheless remains a bias, as the lagged endogenous variable is correlated with the transformed error term. Nickell (1981) showed that the fixed effects estimator for γ may be seriously biased downwards in short panels.[19]

Several consistent instrumental variable methods have been developed that, in general, can improve on the LSDV estimates. These estimators typically consider the first difference version of the model described in equation (1),

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Moreover, there is no rule or directive specifying when the federal government has to step in. There is, thus, no reason to believe that the corresponding variables are endogenous.

[18]The Hausman test suggests that the random effects model is consistent. Note, however, that the test requires that the fixed effects estimator is consistent. As this is clearly violated in a dynamic model (see below) we follow our intuitive argument and use fixed effects.

[19]He also showed, however, that the bias approaches zero as T tends to infinity. Since T is relatively large in our study ($T = 46$), the bias is likely to be moderate. Note that although T is much smaller in Seitz (2000,

$$\Delta d_{it} = \gamma \Delta d_{i,t-1} + \beta_1' \Delta x_{it} + \beta_2' \Delta z_{it} + \Delta \varepsilon_{it}, \tag{3}$$

where Δ is the first difference operator, e.g., $\Delta d_{it} = d_{it} - d_{i,t-1}$. This transformation eliminates the (time invariant) fixed effects. The estimator developed by Anderson and Hsiao (1982, AH estimator), for example, uses $d_{i,t-2}$ as an instrument for $\Delta d_{i,t-1}$ and thereby removes the source of the bias. The generalized method of moments estimator of Arellano and Bond (1991), henceforth AB estimator, uses all valid lags of the dependent variable (in levels) as instruments for Δd_{it}. The AB estimator is consistent and asymptotically efficient (when N tends to infinity).[20] Due to the larger set of instruments, AB is more efficient than AH. There is a homoscedastic (one-step) version of the AB estimator and a two-step version, that, by allowing for heteroscedasticity, may improve efficiency. Simulation studies have shown, however, that the two-step AB is – in most cases – less efficient than the one-step AB, i.e. the two-step AB yields higher standard errors (see, e.g., Arellano and Bond, 1991; Kiviet, 1995; Judson and Owen, 1997). In principle, efficiency gains may be achievable when applying the system GMM estimator developed by Arellano and Bover (1995) and Blundell and Bond (1998), henceforth BB estimator. However, both the AB and the BB estimator are micro panel data estimators and have poor finite sample properties. As N is small in our study ($N = 10$), results of both estimators should mainly be seen as robustness checks.

A more reliable estimator is the bias corrected LSDV estimator (LSDVC). The bias may be approximated to the order of $O(T^{-1})$ when using the approximation derived in Nickell (1981), $O(T^{-1}N^{-1})$ when using Kiviet (1995), and $O(T^{-1}N^{-2})$ when using Kiviet (1999). In a simulation study, Bun and Kiviet (2003) show that the Kiviet (1999) approximation accounts for about 90 per cent of the actual bias. Several simulation studies have shown that the

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$T = 21$) and Galli and Rossi (2002, $T = 21$) both studies use the LSDV estimator.

[20]We consider the regressors summarized in w_{it} as strictly exogenous so that variables themselves and all their lags are valid instruments. Furthermore, note that the AB estimator takes first order autocorrelation of $\Delta \varepsilon$ into account. Thus, neither consistency nor efficiency is affected by first order autocorrelation. But second order

LSDVC estimator outperforms the consistent estimators described above in terms of both bias and standard errors (see, e.g., Bruno, 2005 and Judson and Owen, 1997, 1999). We therefore use the LSDVC estimator for our analysis.

To actually correct the bias one needs an initial consistent estimate of the coefficients and each of the three estimators AH, AB and BB may be used. As the AB estimator typically outperforms the AH estimator and appears more robust than the BB estimator (see Bruno, 2005), we opt for the AB estimator and use the Kiviet (1999) bias approximation. Standard errors are bootstrapped with 100 repetitions.[21]

5. Empirical analysis

The data set comprises yearly data for 10 West German states from 1960 to 2005. In the early years of the FRG, i.e. before 1960, the party structure was relatively unstable. Several small regional parties joined state governments for short periods and disappeared afterwards. Additionally, different coalitions governed within one election period. As this was clearly just a post-war phenomenon, we do not include these years into our analysis. As already mentioned, Berlin and the five new German Länder have not been included in our sample. Berlin is excluded for two reasons. First, Berlin was divided before 1990. While East Berlin was the capital of the GDR, West Berlin was part of the FRG. Second, West Berlin received generous grants from the federal government, making debt issue more or less unnecessary. Data for the East German Länder are available from 1990 onwards. We nevertheless do not include them, as the period is simply too short to obtain sufficient (political) within state variation. We arrive at a balanced panel with 460 observations. The average annual nominal GDP growth was 5.8 per cent, whereas the average annual nominal debt grew with 9.7 per cent. We capture the costs of borrowing by the interest rate. Since borrowing incentives are

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correlation implies inconsistency (Arellano and Bond, 1991, pp. 281-282).

[21]The estimates with BB as initial estimator have slightly higher standard errors. Apart from that results remain

primarily influenced by the real interest rate, we use – without qualitatively changing results – the real interest rate as a control variable rather than the nominal one.[22]

Before testing the political economy of debt issue, we briefly discuss the results with economic indicators and some controls only (Model 1). The regression results are shown in the first column of Table 4 (see Appendix). With a coefficient of around .36, autoregression is relatively moderate. The impact of nominal GDP growth is, as expected, significantly negative. When nominal GDP growth drops by one percentage point debt growth gears up by roughly 0.3 percentage points. This may be due to expenditure programs, reduced tax revenues, or both. Both the first and second oil crisis, OIL1 and OIL2, respectively, increased debt growth significantly. German unification had a negative impact on debt growth in West Germany. Its insignificance may be due to the fact that most of the financial burden of unification was borne by the social security systems and the German unification fund and not by the states. Moreover, the economy boomed right after unification, increasing tax revenues. This may explain why no further state debt was needed. We find no significant effect of the inclusion of the East German Länder into the fiscal equalization system (EQUAL) on public debt growth. The costs of borrowing, measured by the real interest rate (INTRATE), have the expected negative but insignificant impact on debt issue. Finally, the federal government bailout helped to consolidate the budgets of Saarland and Bremen.[23] A comparison of coefficients and standard errors of models 1 to 3 (Table 4) reveals that results are largely robust to including political variables.

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unchanged. The complete estimates for the BB and AH estimator are available upon request.

[22]Deficit data are taken from the Statistisches Bundesamt (Federal Statistical Office, 2005). Data for the gross domestic product (GDP) was provided by the Statistical Office of Baden Württemberg and the Federal Statistical Office. Interest rates were deflated by the consumer price index for all households obtained from the Statistisches Bundesamt (2006), because we are convinced that politicians take real not nominal interest rates as decision parameter for raising new/additional debts. The election dates as well as the election results in both percentage of votes and numbers of seats were taken from the Forschungsgruppe Wahlen (2007, Election Research Team).

[23]Note that the variables EQUAL and BAILOUT are highly correlated; they only differ in 1994. Even if we drop one of them we do not gain significance of the other. All results remain unchanged if we rerun regressions without BAILOUTHB and BAILOUTSL. So there seems to be no significant incentive effect on debt issue arising from the occurrence of the bailout.

1. *Political opportunism (Hypothesis 1)*

Before we test the hypotheses let us first take a brief look at the descriptive statistics shown in Table 3 (part A and B) in the Appendix. The average debt growth rate calculated over all years and all states is 9.7 per cent. For election years, we find a growth rate of as much as 10.4 per cent and for pre-election years 8.0 per cent.

These numbers suggest that there may be an opportunistic cycle. To actually test Hypothesis 1 we include two dummy variables in our regression: ELECTION and PREELEC. The first variable equals 1 in election years and zero otherwise, the second accordingly for pre-election years.

Table 4, column 2, in the Appendix reveals that debt growth in election years is not significantly different from reference years. In contrast debt growth is significantly lower in pre-election years. Debt growth in preelection years is about two percentage points smaller than in reference years. Although the effect is statistically significant the magnitude appears to be small. But when relating this number to average debt growth we arrive at a 20 per cent lower debt growth in pre-election years: the effect is substantial! We are thus able to reject Hypothesis 1: there is an opportunistic cycle that brings about significantly lower deficits in pre-election years. This result allows us to conclude that German voters seem to favor fiscal discipline.[24] Although results do not allow us to discriminate between rational and non-rational expectations, our result is in line with the theory developed by Rogoff and Sibert (1988).

Defining the variables ELECTION and PREELEC using the calendar year of the election date is arbitrary. Table 5 in the Appendix shows that there are only minor changes when alternative (and also arbitrary) cut-off dates are used. Suppose the cut-off is January 31. If an election is, for example, held in January 1982, then 1981 is considered the election year and

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[24]This differs from Galli and Rossi (2002) who found significantly positive election year effects but no pre-election year effects.

1980 the pre-election year. An election in February 1982 would then have 1982 as the election year and 1981 as the pre-election year. Estimation results with this cut-off are shown in column 2 of Table 5 (column 1 repeats the results reported in Table 4 to ease comparison). The remaining 3 columns have cut-offs February 28/29, March 31, and June 30, respectively. Results for cut-offs January, February, and March do not significantly differ from those reported in the first column (December). This robustness is very reassuring as we actually found an opportunistic cycle. This robustness, however, is not too surprising since there are very few elections early in a year (only 8 per cent of all elections were held in January or February). The results with the June cut-off are different, though. We find a negative effect in election and pre-election years and both effects are statistically significant.[25] This strengthens our assertion that German voters seem to favor fiscal discipline.

One might ask whether the strategy of lower debt issue in pre-election years is used equally across parties or whether there are some government constellations that make more use of this tool than others (see Table 4, Model 3 for results). We constructed pre-election party interaction for all parties and, controlling for party effects, find no significant difference in coefficients.

2. Partisan theory (Hypothesis 2)

To check whether fiscal policy is driven by party ideology, we have to assign every government constellation to either left or right. We categorize SPD governments, SPD/FDP coalitions and SPD/GREEN coalitions as left. CDU governments and CDU/FDP coalitions are labeled right. It is difficult to ascribe a political orientation to grand coalitions, i.e., coalitions formed by SPD and CDU. There are basically two alternatives. First, do not label such coalitions at all and use them as a reference category in the estimation. Second, use the

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[25]This result is not unexpected. As compared to the calendar coding (December cut-off), about half of the cases that were categorized as pre-election years now are election years so that, loosely speaking, the significance is partially transferred from the pre-election variable to the election variable.

party affiliation of the prime minister to assign an orientation. We opted for the second alternative as 27 observations of grand coalition governments out of 460 observations are simply too few observations for a sensible reference category.[26] A similar reasoning applies to all other government constellations summarized in ELSE (also 27 of 460 observations). These government constellations are considered left when the Social Democrats were involved and right when the Christian Democrats were. When the government turns over from left to right, or vice versa, the question of whether the government should be labeled left or right in that particular year becomes an issue. We consider the new government's ideological position if its inaugural date was prior to July 1 of the respective year.

Before we interpret estimation results let us again first take a look at the descriptive statistics. Table 3 (part C) in the Appendix identifies right governments as the ones issuing more debt. As the difference in debt growth rates between right governments (9.9 per cent) and left governments (9.4 per cent) is – as compared to the standard errors – rather low, a significant partisan effect can hardly exist. So, not surprisingly, the corresponding coefficient is not statistically different from zero (Table 4, Model 2). Note, however, that the coefficient obeys the 'correct' sign.

Although we are unable to reject Hypothesis 2 – which is well in line with Seitz (2000) and Galli and Rossi (2002) – interpretation remains difficult (see also the discussion in Subsection 2). It may well be that there are no partisan trends in German Länder fiscal policy – that ideology plays a negligible role. This is, however, not necessarily true. Once the parties care not only about ideology but also about winning the next election, platform convergence will occur. Since elections can well be considered a repeated game, parties will stick to their platforms. Otherwise they risk their reputation: identifying the opponent as a liar is a powerful weapon in electoral competition. If reputation is decisive, then platform convergence implies policy convergence and, with it, adaptation of fiscal policies. Differences can hardly be

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[26]Results are independent of the alternative adopted.

detected. And indeed, for Germany, it is usually claimed that both major parties, SPD and CDU, are close to the center.

3. Fragmented governments (Hypotheses 3 and 4)

231 observations of coalition governments yield an average debt growth of 9.4 per cent compared to 9.9 per cent for the 219 observations with single-party governments. In 172 of these 231 observations of coalitions governments – or in 74 per cent of the cases – the prime minister and the finance minister belonged to the same party. For these 172 cases debt grew by 8.7 per cent on average. These descriptive statistics (see also Table 3, part D in the Appendix) raise doubts as to whether we will be able to reject Hypotheses 3 and 4.

However, one should be cautious when interpreting cross state averages. We, therefore, define the indicator variable COAL that assumes a value of 1 whenever more than one party formed the government and zero otherwise.[27] We find a highly significant coalition effect, that is, we can reject Hypothesis 3. The positive sign is perfectly in line with the theory discussed in Section 3. Note also that if compared to the pre-election effect (in absolute terms), the coalition effect is about three times as high: debt growth in a coalition government is more than 6 percentage points higher than with single party governments.

As argued in Section 3 the more parties forming the coalition, the more severe the common pool problem. We are unable to address this issue for the German states since there are only 12 observations where more than two parties formed a coalition.

Hypothesis 4 states that the position of the finance minister is irrelevant for a government's borrowing decision. A strong finance minister, however, may be able to mitigate the common pool problem by centralizing fiscal policy at least to some degree. We define the variable SAMECOAL that assumes the value 1 whenever there is a coalition government where the prime minister and the finance minister have the same party affiliation and zero otherwise.

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[27] Again, in years of government changes, we use the inaugural date of the new government and July 1 as the cut-

This variable is then used as a proxy for the power of the finance minister. We find a statistically significant negative effect on borrowing. This effect exactly offsets the coalition effect (the absolute values of the estimated coefficients of COAL and SAMECOAL are not statistically distinguishable) so that coalition governments with a strong finance minister do not suffer from the common pool problem at all and borrow like single party governments. In other words, a strong finance minister solves the common pool problem.[28]

6. Conclusion and policy implications

We analyzed the political determinants of the West German Länder deficits from 1960 to 2005. Since political institutions and electoral rules are almost identical across German states, our study does not suffer from the fundamental problem of disentangling the effects of political variables from the impact of political institutions that typically arises when comparing jurisdictions.

Overall we addressed four hypotheses taken from the broad theoretical literature on the political economy of public expenditures and/or public debt issue. While ideological motives play no role, we found that debt growth is significantly lower in pre-election years. This is well in line with the Rogoff and Sibert (1988) argument of signaling fiscal competence via low debt. With a 20 per cent lower debt growth rate in pre-election years the effect is large. We also found a positive and significant coalition effect on debt issue. In absolute terms, the effect is about three times larger than the pre-election effect. There seems to be some kind of coordination failure within coalition governments. Interestingly, this problem vanishes if the prime minister and the finance minister belong to the same party (within a coalition government), so that the finance minister can be considered powerful. Borrowing of coalition governments then is not significantly different to the borrowing of single-party governments.

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off date to assign a value to COAL.

[28]We are indebted to an anonymous referee for motivating us to investigate the role of the finance minister.

Three policy implications can be derived from our results. First, the problem of opportunistic political business cycles results in inefficient borrowing over time, an issue that can be countered by improving transparency of the budgetary process (Alt and Lassen, 2005). First measures are about to be implemented in Germany. The largest of the 16 German Länder, North-Rhine Westphalia, is currently implementing a report system on fiscal sustainability of its budget. The aim is to increase transparency in fiscal affairs. There are a number of international examples where measures have been taken to advance transparency. The US, Australia, and the UK, for instance, have installed report mechanisms on budgetary and fiscal developments. This eases the assessment of medium and long-term fiscal development and also informs the electorate about the key fiscal indicators.

Second, Duverger's law suggests that countries with majoritarian electoral systems are more likely to have single-party governments and countries with proportional electoral systems are more likely to have coalition governments.[29] Thus, the question of whether coalition governments have different incentives to issue debt than single-party governments is of some importance for the design of the electoral system. As coalition governments (with a weak finance minister) yield significantly higher debt growth than single-party governments, a switch to a majoritarian electoral system may contribute to fiscal stabilization.

Finally, and related to the previous paragraph, the internal organization of a government is decisive for fiscal stability. So when coalition governments form, economic advisers may wish to recommend that the prime minister and the finance minister belong to the same party. As our results suggest, debt growth can then expected to be lower than otherwise.

Acknowledgements

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[29]For an excellent survey of Duverger's law see Riker (1982).

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Appendix

Table 1: Literature overview

Study	Data	Dependent variable	Political variables & results
Alesina (1989)	12 OECD countries (1966-1986)	economic growth unemployment, inflation	evidence for partisan political business cycles
Alesina, Cohen and Roubini (1993)	14 OECD countries (1960-1987)	economic growth, unemployment, public expenditures, inflation, money supply	evidence for rational opportunistic business cycles (Rogoff and Sibert) only for the last three dep. variables
Alesina and Sachs (1988)	United States (1949-1984)	economic growth money supply (M1)	evidence for partisan effects for both dependent variables
Ashworth, Geys and Heyndels (2005)	Flemish Municipalities (1977-2000)	government debt	government fragmentation plays a role in the short but not in the long run
Belke (2000)	Germany (1970-1996)	various labor market variables	evidence for short and long term partisan influences
Boix (2000)	19 OECD countries (1960-1993)	interest rate public debt	evidence for partisan trends
Bräuninger (2005)	19 OECD countries (1971-1999)	government spending (general and social security)	partisan effects in spending patterns
Cusack (1997)	16 OECD countries (1955-1989)	general government spending	evidence for partisan trends
Drazen and Eslava (2005)	Colombian municipalities (1987-2000)	government spending (expenditure categories)	evidence for partisan effects, voters penalize incumbents for deficits occurring prior to elections

To be continued next page

Study	Data	Dependent variable	Political variables & results
Edin and Ohlsson (1991)	13 OECD countries (1960-1985)	public deficit	Roubini and Sachs (1989) results are driven by minority governments
Galli and Rossi (2002)	11 West German states (1974-1994)	government expenditures deficits/surplus expenditure categories	no partisan trends evidence for political business cycle (election years)
De Haan and Sturm (1997)	21 OECD countries (1982-1992)	public deficit	no evidence for an effect of government fragmentation
Heckelman (2002)	Canada (1965-1994) Germany (1977-1994) UK (1960-1993)	economic growth unemployment	evidence for persistent partisan effects, party popularity affects business cycle
Heckelman (2006)	7 OECD countries (1960-1993)	unemployment	almost no evidence for partisan effects (except US for democrat victories)
Hibbs (1977)	12 OECD countries (1945-1969)	unemployment inflation	evidence for partisan effects
Nordhaus (1975)	9 OECD countries (1947-1972)	unemployment	evidence for opportunistic political business cycles
Reed (2006)	United States, 45 states (1960-2000)	tax burden	evidence for partisan effects (higher tax burden with democrat governments)
Roubini and Sachs (1989)	13 OECD countries (1960-1985)	public deficit	coalition governments run higher deficits than single party governments
Seitz (2000)	10 West German states (1976-1996)	expenditures, deficits and alternative economic variables	no evidence for partisan effects
Tavares (2004)	19 OECD countries (1960-1995)	tax revenue public expenditures	evidence for partisan effects (left: higher tax revenue, right: lower spending)
Veiga and Veiga (2007)	278 Portuguese municipalities (1979-2001)	debt, tax revenue, public expenditures and others	evidence for opportunistic political business cycles (pre-election effects)

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Table 2: Explanation of variables

Variable	Explanation
DEFICIT	nominal debt growth rate
DEFICIT(-1)	lagged nominal debt growth rate
GDP	nominal growth rate of gross domestic product
INTRATE	real interest rate
OIL1	= 1 from 1974 to 1975 (first oil crisis)
OIL2	= 1 from 1978 to 1981 (second oil crisis)
UNIFIC	= 1 from 1991 to 2005 (unification)
EQUAL	= 1 from 1995 to 2005 (equalization scheme)
BAILOUT	= 1 from 1994 to 2005 (federal government bailout)
BAILOUTHB	= 1 if BAILOUT = 1 and Bremen (Bailout-Bremen interaction)
BAILOUTSL	= 1 if BAILOUT = 1 and Saarland (Bailout-Saarland interaction)
ELECTION	= 1 in election years
PREELEC	= 1 in pre-election years
LEFT	= 1 for SPD dominated governments
RIGHT	= 1 for CDU dominated governments
SPD	= 1 for single-party Social Democratic governments
CDU	= 1 for single-party Christian Democratic governments
SPDFDP	= 1 for SPD coalitions with Liberals
SPDGREEN	= 1 for SPD coalitions with Greens
GRANDC	= 1 for SPD coalitions with the CDU or vice versa
SPDCDU	= 1 for GRANDC = 1 and SPD prime minister
CDUSPD	= 1 for GRANDC = 1 and CDU prime minister
CDUFDP	= 1 for CDU coalitions with Liberals
ELSE	= 1 for remaining government constellations
COAL	= 1 for coalition governments
COALSIZE	number of parties in a coalition
SAMECOAL	= 1 if prime and finance minister belong to the same party in a coalition

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Table 3: Descriptive statistics

	Variable	N	mean	s.d.	min	max
A	DEFICIT	450	.0967	.1174	-.1500	1.2632
	GDP	450	.0577	.0378	-.0242	.1906
	INTRATE	460	.0407	.0162	.0112	.0790
	SPD	460	.2043	.4037	0	1
	SPDFDP	460	.1565	.3637	0	1
	SPDGREEN	460	.0783	.2689	0	1
	SPDCDU	460	.0348	.1834	0	1
	CDU	460	.2739	.4465	0	1
	CDUSPD	460	.0239	.1529	0	1
	CDUFDP	460	.1696	.3757	0	1
	ELSE	460	.0587	.2353	0	1
	GRANDC	460	.0587	.2353	0	1
B	DEFICIT*ELECTION	110	.1037	.1232	-.0700	.9814
	DEFICIT*PREELEC	111	.0799	.0988	-.1500	.4189
C	LEFT	460	.5130	.5004	0	1
	RIGHT	460	.4870	.5004	0	1
	DEFICIT*LEFT	232	.0942	.1077	-.0886	.9814
	DEFICIT*RIGHT	218	.0992	.1272	-.1500	1.2632
D	COAL	460	.5217	.5001	0	1
	COALSIZE	460	1.5565	.5631	1	3
	DEFICIT*COAL	231	.0943	.1408	-.1500	1.2632
	DEFICIT*(1−COAL)	219	.0991	.0864	-.0621	.4767
	DEFICIT*SAMECOAL	172	.0874	.1148	-.1500	.9814

Table 4: Regression results

Variable	LSDVC (model 1)		LSDVC (model 2)		LSDVC (model 3)	
DEFICIT(-1)	.3616***	(.0470)	.3581***	(.0463)	.3631***	(.0461)
GDP	$-.2864$**	(.1504)	$-.3659$**	(.1485)	$-.3137$**	(.1517)
INTRATE	$-.3949$	(.3859)	$-.0924$	(.3985)	$-.1760$	(.4109)
OIL1	.1899***	(.0266)	.1991***	(.0265)	.1905***	(.0272)
OIL2	.0352**	(.0169)	.0438***	(.0168)	.0369**	(.0178)
UNIFIC	$-.0182$	(.0209)	$-.0177$	(.0213)	$-.0189$	(.0215)
EQUAL	.0103	(.0323)	.0077	(.0317)	.0098	(.0337)
BAILOUT	$-.0384$	(.0346)	$-.0268$	(.0343)	$-.0361$	(.0363)
BAILOUTHB	$-.0205$	(.0355)	$-.0673$*	(.0376)	$-.0399$	(.0477)
BAILOUTSL	$-.0703$**	(.0357)	$-.0706$*	(.0364)	$-.0589$	(.0387)
ELECTION			$-.0009$	(.0112)	.0004	(.0115)
PREELEC			$-.0205$*	(.0111)		
LEFT			.0044	(.0106)		
COAL			.0649***	(.0174)		
SAMECOAL			$-.0656$***	(.0182)		
PREELEC*SPD					$-.0198$	(.0229)
PREELEC*SPDFDP					$-.0325$	(.0259)
PREELEC*SPDGR					$-.0074$	(.0407)
PREELEC*GRANDC					.0230	(.0476)
PREELEC*CDU					$-.0312$	(.0217)
PREELEC*CDUFDP					$-.0226$	(.0251)
PREELEC*ELSE					.0188	(.0443)
SPD					$-.0065$	(.0174)
SPDFDP					.0139	(.0185)
SPDGR					.0013	(.0241)
GRANDC					.0112	(.0319)
CDUFDP					.0171	(.0189)
ELSE					$-.0117$	(.0314)

Dependent variable DEFICIT, $N = 430$, standard errors in brackets.

Significance levels: *** = 0.01, ** = 0.05, * = 0.10,

Table 5: Regression results for alternate election year cut-offs

Variable	Dec 31		Jan 31		Feb 28/29		Mar 31		Jun 30	
DEFICIT(-1)	.3581***	(.0463)	.3574***	(.0469)	.3572***	(.0470)	.3578***	(.0470)	.3631***	(.0465)
GDP	−.3659**	(.1485)	−.3698**	(.1588)	−.3714**	(.1586)	−.3699**	(.1587)	−.3608**	(.1590)
INTRATE	−.0924	(.3985)	−.0670	(.3770)	−.0541	(.3781)	−.0969	(.3702)	−.0606	(.3714)
OIL1	.1991***	(.0265)	.1993***	(.0240)	.1990***	(.0239)	.1991***	(.0237)	.1968***	(.0234)
OIL2	.0438***	(.0168)	.0437***	(.0168)	.0435**	(.0168)	.0448***	(.0168)	.0436***	(.0167)
UNIFIC	−.0177	(.0213)	−.0183	(.0209)	−.0180	(.0209)	−.0166	(.0208)	−.0193	(.0209)
EQUAL	.0077	(.0317)	.0065	(.0279)	.0088	(.0281)	.0019	(.0281)	.0001	(.0277)
BAILOUT	−.0268	(.0343)	−.0249	(.0316)	−.0275	(.0317)	−.0226	(.0317)	−.0179	(.0317)
BAILOUTHB	−.0673*	(.0376)	−.0707*	(.0417)	−.0705*	(.0417)	−.0707*	(.0417)	−.0725*	(.0415)
BAILOUTSL	−.0706*	(.0364)	−.0770**	(.0364)	−.0767**	(.0364)	−.0765**	(.0364)	−.0738**	(.0362)
ELECTION	−.0009	(.0112)	−.0023	(.0121)	−.0017	(.0118)	−.0068	(.0101)	−.0215**	(.0100)
PREELEC	−.0205	(.0111)	−.0231**	(.0108)	−.0221**	(.0107)	−.0194**	(.0098)	−.0273***	(.0104)
LEFT	.0044	(.0106)	.0037	(.0135)	.0038	(.0134)	.0040	(.0135)	.0034	(.0133)
COAL	.0649***	(.0174)	.0652***	(.0167)	.0654***	(.0168)	.0653***	(.0168)	.0652***	(.0167)
SAMECOAL	−.0656***	(.0182)	−.0659***	(.0203)	−.0658***	(.0203)	−.0657***	(.0203)	−.0648***	(.0201)
Dependent variable DEFICIT, $N = 430$, standard errors in brackets.										
Significance levels: *** = 0.01, ** = 0.05, * = 0.10.										

The Political Economy of the German Länder Deficits: APE-08-0831

Response to the referee

We thank you very much for your excellent report that helped us to improve the paper considerably. We completely agree that our main result is on coalition governments and the role (strength) of the finance minister therein. In the revised version we highlight these aspects – including a corresponding addition to the title of the paper.

Answers to your comments (page numbers refer to revised version)

1. Strength of the finance minister is central result of the paper.

As already mentioned above we fully agree that the role of the finance minister within coalition governments is the most novel result of the paper. In response to your comment we changed the title of the paper by adding the subtitle "Weak governments meet strong finance ministers". We thereby highlight, that one has to consider the strength of the finance minister when testing the weak government hypothesis – one cannot analyse these two aspects separately.

To some extend the new title also previews our main result: strong finance ministers solve the common pool problem of coalition governments, that is, coalition governments with a strong finance minister borrow like single party governments. To emphasize this we have reorganized abstract and introduction and now discuss these issues first. The 'centralization hypothesis' going back to von Hagen (1992), von Hagen and Harden (1995), and Hallerberg and von Hagen (1999) is made more explicit (p. 3).

The presentation of the four hypotheses in Section 3 and the presentation of results in Section 5 are still in the same order as in the original manuscript. We decided not to present the coalition and finance minister arguments first since the opportunistic and partisan incentives are equally relevant for coalition governments and single party governments. The ordering is, thus, logically consistent (p. 9). In the new Subsection 3.4 we now discuss the importance of the finance minister in a coalition cabinet in more detail. This includes a brief discussion on the difficulty to measure the strength of a finance minister (thanks for bringing the recent paper by Wehner (2009) to our attention) and an argument for why we believe we came with a promising proxy (pp. 13-14).

In the results section (Section 5) the position of the finance minister is now discussed in a separate subsection (Subsection 5.4). This is done to increase visibility of our central result. The presentation is clearer and a bit more detailed than before (p. 24). The conclusion is rewritten for the same purpose (pp. 24-25).

2. Use real values instead of nominal ones.

Again, we agree. We replaced nominal debt growth and nominal GDP growth with the respective real growth rates. This leads to some minor (and negligible) changes of results. Some coefficients of the control variables show changes (interest rate and GDP) but none of those effects is of prime interest to us. The coalition effect and the effect of a strong finance

minister within coalition governments are unchanged – debt growth is about 6 percentage points higher when coalitions have a weak finance minister and there is no coalition effect when the finance minister is strong. This result is thus independent of how debt and GDP growth are measured. The pre-election effect got somewhat weaker and smaller. While we had significance at the 5 per cent level with nominal values we now have a 10 per cent significance level (Tables 4 and 5). We take account of that (minor) change by referring to the pre-election effect now as weak evidence for opportunistic behaviour (abstract, p. 3, p. 25, footnote 26).

3. Anticipated bailout and borrowing incentives

Many thanks for suggesting alternative codes of the bailout variable. Saarland and Bremen were bailed out by the federal government in 1994 but it may be that – in anticipation of the bailout – borrowing incentives changed prior to 1994. Given that the constitutional court ruled in favour of this bailout already in 1992 it makes perfect sense to also consider the lags of the variable as you suggested. We constructed two dummy variables: a 1993 bailout variable and a 1992 bailout variable. A comparison of estimation result reveals (Models 2 to 4 in Table 4) that our results are robust to changes in bailout codes. Moreover, the bailout effect is never distinguishable from zero so that we conclude that borrowing incentives were not altered by the possibility of a federal bailout. We briefly discuss this on p. 20.

Note that, in contrast to the previous version, we now discuss the bailout effects in the context of Model 2, that is, in the context of a regression with political variables. To do that within a model with control variables only appears to be inappropriate since the bailout effect in Bremen is substantial in Model 2 but not existent in Model 1. This suggests that BAILHB coefficient is biased in Model 1.

4. Shorten the paper

We followed your suggestions and deleted the descriptive parts from the results section (pp. 20-24) and reduced policy implications to the strong finance minister effect in coalition governments (p. 25).

Minor points:

Thanks for sharing the Geys (2007) reference with us. We refer to it at several places: government fragmentation matters (pp. 3, 4), debt growth rates as dependent variable (p. 14). We also added the paper to Table 1 where we review the empirical literature.

As suggested in Geys (2007) the number of parties within a coalition government may be important for results. In order to test whether or not coalition size is important we distinguish between two-party coalitions and three-party coalitions (there never was a coalition with more than three parties). Estimation results are reported in Table 4, Model 5. The results suggest that there is no effect of coalition size (the COAL3 coefficient is not significantly different from zero and the COAL2 effect is unchanged as compared to the coalition effect reported in Model 2). One should note, however, that we effectively only have 9 observations with three-party coalitions. We briefly discuss this on p. 23.

We are happy that you appreciate our efforts to make the empirical methodology and model selection transparent. Accordingly, Section 4 is unchanged.

Other changes:

In the previous version of the paper we used the calendar year to determine election and pre-election years. We argued that the choice of the cut-off date (December 31) is largely arbitrary and demonstrated that our results were robust to changes in the cut-off (Table 5). In the current version of the paper Table 5 shows that this robustness result still holds with real growth rates. So, in principle, it would have been possible to stick with the December cut-off. But we decided not to for reasons discussed in the paper: "So far we have been silent about how the variables ELECTION and PREELEC are defined. Using the calendar year (cut-off is December 31) seems to be natural but only at a first sight. We want the pre-election variable to pick-up the incentives described in Rogoff and Sibert (1988). But then information on debt in the pre-election year must be readily available when elections actually take place. Since this is highly unlikely for elections held in January, the cut-off January 31 appears more plausible than December 31. A similar argument can be made about elections held in February and March so that we opted for March 31 as cut-off for Models 2 to 6 in Table 4. Although it seems plausible to deviate from the calendar year, the actual choice of a cut-off date is largely arbitrary. As Table 5 shows our results are robust to changes in cut-offs" (p. 21).

The political economy of the German Länder deficits: Weak governments meet strong finance ministers

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Abstract

We analyze the deficits of the German Länder (regional states) for the period from 1960 to 2005 and test a number of hypotheses derived from the literature on the political economy of public deficits. We find evidence for the weak government hypothesis, that is, coalition governments issue significantly more debt than single party governments – a result that is typically explained by the common pool problem. As our data suggest, this result crucially hinges on the position or strength of the finance minister within coalition governments. We find that coalition governments with a strong finance minister are – in terms of borrowing – not significantly different from single party governments.. In addition we find (weak) evidence for an opportunistic political business cycle. As borrowing is significantly lower in pre-election years it appears that German voters favor fiscal discipline. There is no evidence for partisan behavior; so, party ideology seems to play a negligible role.

Keywords: Public Debt, German Länder, Political Economy, Weak Governments, Strong Finance Ministers.

JEL classification numbers: D72, E60, H62.

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1. Introduction

Public deficits vary widely between jurisdictions. It is broadly accepted that economic variables such as economic growth or the interest rate alone cannot explain these differences. In fact, political variables and political institutions play an important role in the development of public debt (Persson and Tabellini, 1997). When comparing different countries, however, one can hardly disentangle the effect of political variables and the impact of political institutions. This identification problem disappears when the influence of political variables on public debt in the German states (Länder) is analyzed as the jurisdictions have almost identical political institutions and electoral rules. However, they differ quite substantially in other dimensions such as fiscal policy outcomes and per capita income. Thus, our data set offers a promising opportunity to solely test for the influence of political variables on public deficits.

We test a number of hypotheses taken from the theoretical literature on the political economy of debt issue. Most importantly, we address the weak government hypothesis: the common pool problem suggests that coalition governments can be expected to issue significantly more debt than single-party governments. Political fragmentation received considerable attention in the literature but results give no clear picture. Roubini and Sachs (1989) find support for the weak government hypothesis. Re-estimating the Roubini and Sachs model, Edin and Ohlsson (1991) challenge their view and argue that the coalition effect identified in Roubini and Sachs (1989) is a result of minority governments rather than political fragmentation. Using a different data set, de Haan and Sturm (1997) find no coalition effects – neither with the dispersion index used by Roubini and Sachs nor with the one used by Edin and Ohlsson. In a recent study on debt of the Flemish municipalities, Ashworth, Geys and Heyndels (2005) find that political fragmentation affects local indebtness in the short but not in the long run.

Using the same data set Geys (2007) demonstrated that the level of fragmentation (number of parties forming the coalition government) may contribute to explaining political outcomes.

To what extent the common pool problem actually translates into higher debt (if at all) also depends on the degree of centralization of fiscal policy. A strong position of the finance minister within the cabinet may result in lower deficits (see von Hagen, 1992; Hagen and Harden (1995), Hallerberg and von Hagen, 1999). To the best of our knowledge there is no study that explicitly addressed this issue. We fill this gap by distinguishing coalition governments with a strong finance minister form those with weak a finance minister.

For completeness we also test for opportunistic behavior and partisan politics. The opportunistic school suggests that political behavior is solely designed to win the next election. Empirical evidence is mixed. Nordhaus (1975) finds evidence for opportunistic cycles for two out of four elections in the United States. Easaw and Garratt (2000) report that expenditures of conservative UK governments are more responsive to national income in pre-election periods. Galli and Rossi (2002) find only weak support for the opportunistic school. Evidence for partisan politics, where policy is primarily driven by party ideology, is also mixed. Alesina (1989), Boix (2000), Cusack (1997), Hibbs (1977), Reed (2006) and, Tavares (2004), for example, find support for the partisan theory. Carlsen (1997) only observes evidence for partisan politics when unemployment is high or rising, whereas Heckelman (2002), Seitz (2000), and Galli and Rossi (2002), for instance, find no evidence for it.[3] In a recent study by Andrikopoulos et al. (2006) on European Union data evidence for both, opportunistic cycles and partisan behavior is lacking.

We estimate a dynamic panel data model and find evidence for the weak government

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[3]Table 1 in the Appendix offers a more detailed (but still partial) review of the empirical literature related to our study.

hypothesis, that is, coalition governments issue significantly more debt than single-party governments. This result, however, only applies to coalition governments with a weak finance minister. The coalition effect vanishes when the finance minister is strong[4]: borrowing of single party governments is not statistically different from borrowing of coalition governments with a strong finance minister. In contrast to Geys (2007) the number of parties in a coalition government (coalition size) is irrelevant – a result that is likely to be due to insufficient variation in coalition size in our sample. There is some evidence for opportunistic behavior: debt issue is significantly lower in pre-election years. Thus, German voters seem to favor fiscal discipline or, at least, the incumbent may believe they do. There is no evidence for partisan behavior.

The first econometric study with German data that considers political variables as covariates is Frey and Schneider (1979). The current paper, however, is most related to Seitz (2000) and Galli and Rossi (2002) who also analyze the political economy of German Länder fiscal policy. Seitz considers the time period from 1976 to 1996 whereas Galli and Rossi analyze the period from 1974 to 1994. While Seitz concentrates on partisan politics and deficit data, Galli and Rossi are more ambitious and additionally test for political business cycles using deficits, expenditures and expenditure categories. We extend these two studies along three lines. First, we explicitly address the role of coalition governments and the position of the finance minister therein and thereby gain a number of new insights (finance minister weakness is a prerequisite for the weak government hypothesis to hold.) Second, due to the availability of better estimation methods, we also econometrically go beyond Seitz and Galli and Rossi. Seitz only applies the least squares dummy variable estimator. As this estimator may be severely biased in short panels we use the bias corrected least squares dummy variable estimator that clearly outperforms the uncorrected version (see, e.g., Bruno, 2005). Galli and

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[4] We consider a finance minister to be strong when he or she has the same party affiliation as the prime minister.

Rossi deal with heteroscedasticity but also ignore the bias. Finally, we use data from 1960 to 2005 and thereby extend the analysis from 21 to 46 years. Note that this extension is crucial since all three studies rely on within state variation to tease out statistically significant covariates.[5]

The paper is organized as follows. In Section 2 the institutional background for Germany is provided. The hypotheses to be tested are derived in Section 3. We thereby review the theoretical literature on the political economy of debt issue. The empirical model and the different estimators applied are introduced in Section 4. The data set and the results are presented in Section 5. Finally, Section 6 offers some concluding remarks.

2. Institutional background

1. Germany's federal political design

The name "Federal Republic of Germany" (FRG) already highlights the country's federal structure that is reflected by the levels of government: federal (Bund), state (Land) and local (Gemeinde). Since German unification in 1990 Germany consists of sixteen Länder, the ten Länder of former West Germany, the five new Länder of former East Germany (German Democratic Republic, GDR), and Berlin. From World War II to unification, Berlin was divided into West Berlin and East Berlin, where the latter was the capital of the GDR. Additionally, there are about 14,000 cities and communities, which form the local level (Seitz, 2000, p. 188).

The Länder are not mere provinces, they are states endowed with their own powers. These powers and responsibilities are specified in the Basic Law (Grundgesetz), Germany's

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[5]We ran our empirical models on shorter time periods, including those used in Seitz (2000) and Galli and Rossi (2002) and largely lose significance. In other words, the differences in results between our paper and the other two are mainly due to the much longer time period that we consider.

constitution. The Basic Law also guarantees the local authorities the right to independently administer their own affairs. As the local authorities rely heavily on grants from the states, their independence is rather limited. Three large German cities, namely, Berlin, Bremen and Hamburg, form their own states (Länder). These are the so-called "city-states" (Stadtstaaten) that do not have local administrative bodies. In contrast, the other German states are called "non-city-states" (Flächenländer). This distinction is important since the budgets of the city-states include expenditures and revenues that are part of the local budgets in non-city-states. Moreover, the expenditures of the non-city-states include grants to the local authorities whereas there are no such grants to local authorities in the city-states. Consequently, public expenditures or public debt of the two types of states are not directly comparable.[6]

Our study examines the budget deficits of the Länder without taking the local authorities into account. As mentioned above, local authorities have their own budgets and their own parliaments. Election dates typically differ between local and state jurisdictions. As a consequence, the aggregated local political structure will hardly ever match the political structure of the state. Since the state government cannot be held responsible for deficits at the local level (net of state grants) it is logically consistent to concentrate on state level debt and neglect deficits arising at the local level.[7]

2. Fiscal federalism in Germany

Although the Länder are endowed with theirs own powers, an almost total lack of tax setting autonomy exists. Additionally, a large fiscal equalization system harmonizes revenues across states, calculated on the basis of several fiscal and economic indicators, and this strongly distorts incentives to increase the tax base. The situation in Germany, therefore, differs in one

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[6]In our empirical model, state fixed effects account for that fundamental difference as well as for other time invariant state characteristics.

[7]For the same reason, the other two papers analyzing fiscal variables of the German states, namely, Seitz (2000) and Galli and Rossi (2002), also concentrate on data at the state level.

major aspect from the theoretical literature on the political economy of public expenditures: typically the government has two options for financing expenditures – taxes and debt. But, due to the lack of tax setting autonomy and the equalization scheme, total revenue of every Land is more or less fixed (for a more detailed overview see Seitz, 2000, pp. 188-190). To finance public expenditures, Länder governments only have one discretionary source of financing at their disposal, namely debt. We therefore concentrate on public debt and their political determinants.

There are two more important aspects: First, in 1990, the five new Länder of former East Germany and East Berlin joined the FRG, enlarging the population from around 64 million to roughly 80 million, while the GDP only increased by less than 10 per cent. The integration of East Germany into the West German social security system, the huge investments in infrastructure and various other costs of transformation created a substantial fiscal shock. Although during the first five years after unification most of the direct financial burden was borne by the federal government (via the so-called unification fund, Fonds Deutsche Einheit), we control for unification in our empirical analysis. Secondly, from 1995 onwards, the new German Länder, i.e., former East Germany, and Berlin were included in the fiscal equalization system. A large part of this equalization is amongst the Länder (horizontal equalization). As the new participants were net recipients, this introduced a fiscal burden on the Western Länder, an effect that we account for in our analysis.

Finally, two German states, namely Bremen and Saarland, were bailed out by the federal government. From 1994 onwards they received transfers over and above those of the fiscal equalization scheme. This bailout is likely to reduce debt issue in both states. Moreover, one could imagine that the occurrence of a federal bailout alters the incentives of the states to issue debt in general. Our empirical model considers all these aspects.

3. Political parties

In Germany, there are four major parties. The Christian Democratic Union (CDU), the Social Democratic Party (SPD), the Free Democratic Party (FDP) and the Green party (GREEN). While CDU, SPD and FDP ran for elections in the entire period under study here, the Greens did not. The Green party was founded in 1980 and first won parliamentary seats at the state level in Hamburg and Hesse in 1982 and at the federal level in 1983. Due to historical developments after World War II, the CDU has never run for elections in Bavaria. Instead their so-called sister party, the Christian Social Union (CSU), participates. The programs of CDU and CSU, however, are very similar and they always form one parliamentary group in the federal parliament (Bundestag). Therefore, we do not distinguish between them and label both CDU.

After unification, the Party of Democratic Socialism (PDS) was founded, a successor to the United Socialist Party (SED), the party that ruled East Germany for more than 40 years. Although the PDS has significant influence in the new Länder, it has not succeeded in gaining any influence in the Western Länder.[8,9] As the democratic history of the East German states is rather short, we abstain from including them in our analysis. Due to its special status, Berlin is also eliminated from the data set (see below for more details).

Since 1960 the West German Länder have either been governed by majority governments of the CDU or SPD or by a coalition that mostly consisted of two parties. The SPD has formed coalitions with all three other parties, whereas the CDU has only formed coalitions with the SPD (a so-called 'grand coalition') or the FDP. Minority governments as well as other

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[8]In 2007 the PDS merged with the WASG. The latter largely consisted of disappointed former social democrats and union members. The so-formed new party, DIE LINKE, is about to gain influence in West-German states.
[9]To some extent, Berlin is an exception as the SPD currently forms a coalition with the DIE LINKE. Note, however, that today's Berlin is not a former Western Land.

government constellations have played a negligible role. Table 3 in the Appendix provides, among other things, an overview of government formations in the West German states.[10]

3. The political economy of public deficits

There is a large number of conflicting theories explaining the formation and the evolution of public deficits. In this section we review (part of) the theoretical literature and derive the hypotheses to be tested in Section 5. Our focus is on four theories, namely, government fragmentation (weak governments), 'centralization' of fiscal policy (strength of the finance minister), political opportunism, and partisan theory. Since the motives formulated in the latter two theories are equally relevant for single-party governments and coalition governments it is logically consistent to start this section with political opportunism and partisan theory followed by the peculiarities of coalition governments and their finance ministers.

1. Political opportunism

Opportunistic governments are assumed to be primarily interested in being reelected. There are no ideological motives. Although originally introduced in the context of the 'Phillips curve' (see Nordhaus, 1975; MacRae, 1977), Alesina and Perotti (1994) and Persson and Tabellini (1997) demonstrated that the theory of political opportunism can also be applied to public deficits: to appear competent to voters, the government has an incentive to boost the economy thereby improving the chances of being reelected. Such policies mostly require raising transfers or increasing public investments (e.g., infrastructure, housing, and hospitals). As German states are hardly able to influence their returns, the augmented public expenditure

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[10]For an explanation of the variables see Table 2 in the Appendix.

will result in (further) debt, especially in election years.[11]

This strategy only works if voters do not (fully) anticipate that the debt burden must be borne after the election. Rogoff and Sibert (1988) demonstrated, however, that opportunistic cycles may also occur under rational expectations. They developed a theory where opportunistic cycles originate in temporary information asymmetries between government and voters. The government tries to exploit its information advantage by running low deficits; this signals that the government can provide a given level of public goods reasonably efficiently. Since deficits are visible to voters with a time lag, low deficits are expected to occur in pre-election years.

The main idea behind rational opportunistic cycles (Rogoff and Sibert, 1988) is the same as behind opportunistic cycles of the Nordhaus type: in order to win the upcoming election, the government is prepared to introduce distorted policies. In the former case this leads to lower deficits in pre-election years and, in the latter case, to higher deficits in election and possibly pre-election years. Without any time pattern in deficits around elections there is no evidence for opportunistic cycles.

Hypothesis 1: Public deficits in pre-election years and in election years are no different than in all other years.

Consider that a government can be sure of being reelected. In the German case, Bavaria serves as an example, where the CSU has been in office since the 1950s. Without any risk of being thrown out of office, there is no incentive to introduce distorted policies. Thus, finding no evidence for political opportunism may simply reflect political stability.

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[11]Of course, one may argue that if the election is early in the year expenditures should raise in the pre-election year. We discuss this in some detail in Section 5.1.

2. Partisan theory

Partisan theory suggests that government politics are primarily driven by ideological motives and, accordingly, predicts a more expansionary policy for left governments than for right governments. Left governments, for instance, are typically more inclined to favor redistributive policies. Public spending may therefore be directed towards mitigating income inequality by increasing transfers. With fixed returns, as in our case, such programs may require debt issue. In short, partisan theory suggests that if left governments are in office then debt issue will be higher than otherwise. To actually identify partisan effects, ideology of competing parties must be sufficiently different and ideally time invariant – and this is exactly what was traditionally assumed (see, e.g., Hibbs, 1977).

It may be a bit naive to claim that a party's policy is solely driven by ideology; parties also care about winning the next election. But then the policies of two competing parties will converge unless voters are irrational or at least one party has a sufficiently low discount factor (Alesina, 1988). Since policy convergence precludes identification of partisan effects a closer look at this topic is warranted.

Consider a two-party system where both parties, right and left, are equally well informed and both care about winning elections. In electoral competition both parties will adopt the same platform – the one that maximizes the probability of being elected. If parties are not committed to their platform then, once elected, they implement their most favored policy. Irrational voters will not anticipate the parties' incentives to deviate from their platform and partisan effects may result. Alesina (1988) argued that this result also holds under rational expectations when electoral competition is considered a one-shot game. Rational voters anticipate the parties' incentive to deviate from any announced policy other than their optimal

policy so that the only time-consistent equilibrium must have diverging platforms.

In an infinitely repeated game the ideological difference between parties may be blurred when a cooperative policy (that both parties agree upon prior to elections) can be supported as an equilibrium (see Alesina, 1988). If the elected party deviates and implements its most favored policy, then cooperation becomes incredible and parties end up playing their non-cooperative Nash strategies. Deviation is beneficial if the current gain of implementing the desired policy is larger than the future loss originating in the breakdown of cooperation. This is likely to be the case for low discount factors, a situation where reputation only plays a minor role. Note that sustainable cooperation precludes partisan effects even if parties' ideologies diverge. This is unproblematic for testing for partisan effects since we are not trying to identify different ideologies but whether different ideologies find their way into fiscal policy.[12] We can, therefore, write our second hypothesis as:

Hypothesis 2: Deficits are independent of government ideology, that is, deficits of left governments are no different to deficits of right governments.

3. Fragmented governments

The theories discussed so far have modeled electoral competition between two parties that simultaneously aim at political power. With only two parties, there is no conflict once one party is elected. With more than two parties, coalition governments may arise, opening up another stage of conflict.

In a coalition government each coalition partner tries to allocate as much of the budget as

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[12]Note that we concentrate on public deficits. It may well be that there are no partisan effects in borrowing but in the structure of public spending (see, for instance, Drazen and Eslava, 2005).

possible to its constituency. Partners come up with spending proposals that are asymmetric in the sense that benefits primarily go to the respective constituency but costs are equally shared amongst coalition partners. Since costs are not fully internalized coalition governments face a common pool problem where too high spending proposals translate into higher budget deficits (Persson and Tabellini, 1997, pp. 68-71.) Since the fraction of internalized costs decreases with coalition size, borrowing is expected to increase with coalition size. Our next hypothesis, again in its null-form, can then be stated as follows:

Hypothesis 3: *Borrowing is independent of how many parties form the government.*

4. Position of the finance minister

An obvious criticism of the Persson/Tabellini argument from above is that all partners have control over some part of the budget and none of the parties is responsible for the entire budget. Centralization of financial responsibility would yield efficiency.[13] The extent to which the common pool problem actually translates into higher debt, thus, depends on the degree of centralization of fiscal policy. Von Hagen and Harden (1995) argue that in situations where spending ministers independently develop their spending plans a suboptimally large budget is adopted. Only ministers without a sectoral budget, like the prime minister or the finance minister, have greater incentives to consider the overall impact of higher taxation. Like most politicians, finance ministers will typically care about their prestige, which is largely determined by their ability to form a solid budget. As a result, the interests of the finance minister in terms of borrowing should be well aligned with those of the 'average' taxpayer (Alesina and Perotti, 1996, pp. 20-21). Hence, a strong finance minister is expected to mitigate the common pool problem so that borrowing is lower as compared to a cabinet with a

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[13]An alternative theory that explains higher deficits for coalition governments is offered by Alesina and Drazen (1991) and Alesina and Perotti (1994, pp. 22-29): consider a permanent fiscal shock. Coalition partners will then fight about the allocation of the fiscal burden to the respective constituencies. This situation is well modeled by the 'war of attrition'. In general, delayed adjustment to the fiscal shock will obtain, allowing debt to accumulate.

weak finance minister.

That the power of the finance minister in the budget process is decisive for fiscal performance was analytically demonstrated by von Hagen and Harden (1995). The actual strength of the finance minister, however, is difficult to measure (for an excellent recent discussion see Wehner, 2009). We consider the following proxy: when the finance minister has the same party affiliation as the prime minister, the finance minister is likely to be in a strong position. This proxy is well in line with the argument put forward in Hallerberg and von Hagen (1999), who suggest that the finance minister must be backed up by the prime minister in order to be able to shape fiscal policy according to his/her (and the tax payers) preferences. If prime and finance minister belong to the same party then it is more likely that they share political views and, in turn, that the prime minister provides the necessary support to strengthen his/her finance minister.

Hypothesis 4: The position of the finance minister in a coalition government has no impact on borrowing.

4. Empirical model

In recent studies of public deficits or public expenditures the variable of interest has typically been transformed before running regressions. Cusack (1997) and Seitz (2000), for example, take its first difference as a share of the GDP. This is basically done in order to obtain stationary time series. Like Geys (2007) we consider growth rates for the same purpose. The major advantage of our approach is that the GDP is not used in the construction of the dependent variable which could otherwise be a source of endogeneity. We consider the following dynamic panel data model

$$d_{it} = \gamma d_{i,t-1} + \beta_1' x_{it} + \beta_2' z_{it} + \mu_i + \varepsilon_{it}, \tag{1}$$

where d_{it} denotes the real growth rate of public debt in state $i = 1, ..., N$ at time $t = 1, ..., T$ and

$d_{i,t-1}$ its first lag, $t = 2, ..., T$.[14] The political variables are summarized in the vector x_{it}, the

control variables in z_{it}. We control for real GDP growth (GDP), the first and second oil crisis

(OIL1, OIL2) as well as for German unification (UNIFIC) and for the inclusion of the East

German states into the fiscal equalization scheme (EQUAL).[15] As an identifying assumption

we suggest that all relevant time effects are picked up by specifying a dynamic model and by

including the additional time control variables: the oil crises dummies and the unification and

fiscal equalization dummies. We refrain from adding time fixed effects to the model. This

would introduce (imperfect) multi-collinearity and prevent us from testing for political

opportunism (Hypothesis 1) where variation over time is essential.[16]

Potential direct and indirect effects of the federal government bailout are picked up by the

variables BAILOUT, BAILHB and BAILSL, where the latter two are interactions between

the variables BAILOUT and the state fixed effects for Bremen (HB) and Saarland (SL),

respectively.[17] Finally, debt issue may respond to the financial costs of borrowing, namely,

the real interest rate (INTRATE). Note that the interest rate varies over time but not over

states. This limits the explanatory power to within state variation.

The time invariant state effect is given by μ_i. We will consider these effects as fixed rather

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[14]Note that real debt growth is simply real deficit over real debt.

[15]Definition of all variables can be found in the Appendix in Table 2.

[16] Elections are not uniformly distributed over time. This is why year dummies are correlated with the variables ELECTION and PREELEC. While the ELECTION coefficient is never statistically different from zero we lose significance for PREELEC in a model with time fixed effects. All other results reported below are robust to this alternative specification.

[17]One may argue that debt issue is influenced by the possibility of a federal bailout so that the bailout variables are endogenous. Since debt started to accumulate in the 1970s in most states, about 20 years prior to the first and so far only bailout, it is hard to imagine that borrowing incentives were influenced by the possibility of a bailout. Moreover, there is no rule or directive specifying when the federal government has to step in. There is, thus, no reason to believe that the corresponding variables are endogenous.

than random. It can be argued that there is no room for random effects as the entire population, i.e., all ten West German states, are included in the study. A more substantial argument is the existence of long-lasting governments. Bavaria, for instance, was ruled by the CSU for the entire period considered here. North-Rhine Westphalia is an example of almost continuous SPD government. Obviously we will have $E(x_{it}\mu_i) \neq 0$, i.e. state fixed effects.[18] Random disturbance is $\varepsilon_{it} \sim N(0, \sigma_\tau^2)$. Let $w'_{it} = (x'_{it} \mid z'_{it})$, then the assumptions of the model can be summarized as follows

$$
\begin{aligned}
E(\varepsilon_{it}\varepsilon_{js}) &= 0 \ \ \text{for} \ \ i \neq j \ \ \text{or} \ \ t \neq s \\
E(\mu_i\varepsilon_{jt}) &= 0 \ \ \text{for all} \ \ i, j, t \\
E(w_{it}\varepsilon_{js}) &= 0 \ \ \text{for all} \ \ i, j, s, t.
\end{aligned}
\tag{2}
$$

As is well known, the ordinary least squares (OLS) estimator is inconsistent when a dynamic panel data model, like the one in equation (1), is to be estimated. The estimates of γ will be biased upwards and the coefficients of the exogenous variables will be biased towards zero (see Hsiao, 1986, pp. 76-78). The fixed effects estimator (or Least-Squares Dummy Variable, LSDV, estimator) eliminates this source of inconsistency by taking account of the Länder fixed effects μ_i. There nevertheless remains a bias, as the lagged endogenous variable is correlated with the transformed error term. Nickell (1981) showed that the fixed effects estimator for γ may be seriously biased downwards in short panels.[19]

Several consistent instrumental variable methods have been developed that, in general, can improve on the LSDV estimates. These estimators typically consider the first differenced version of the model described in equation (1),

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[18]The Hausman test suggests that the random effects model is consistent. Note, however, that the test requires that the fixed effects estimator is consistent. As this is clearly violated in a dynamic model (see below) we follow our intuitive argument and use fixed effects.

[19]He also showed, however, that the bias approaches zero as T tends to infinity. Since T is relatively large in our study ($T = 46$), the bias is likely to be moderate. Note that although T is much smaller in Seitz (2000, $T = 21$) and Galli and Rossi (2002, $T = 21$) both studies use the LSDV estimator.

$$\Delta d_{it} = \gamma \Delta d_{i,t-1} + \beta_1' \Delta x_{it} + \beta_2' \Delta z_{it} + \Delta \varepsilon_{it}, \tag{3}$$

where Δ is the first difference operator, e.g., $\Delta d_{it} = d_{it} - d_{i,t-1}$. This transformation eliminates the (time invariant) fixed effects. The estimator developed by Anderson and Hsiao (1982, AH estimator), for example, uses $d_{i,t-2}$ as an instrument for $\Delta d_{i,t-1}$ and thereby removes the source of the bias. The generalized method of moments estimator of Arellano and Bond (1991), henceforth AB estimator, uses all valid lags of the dependent variable (in levels) as instruments for Δd_{it}. The AB estimator is consistent and asymptotically efficient (when N tends to infinity).[20] Due to the larger set of instruments, AB is more efficient than AH. There is a homoscedastic (one-step) version of the AB estimator and a two-step version, that, by allowing for heteroscedasticity, may improve efficiency. Simulation studies have shown, however, that the two-step AB is – in most cases – less efficient than the one-step AB, i.e. the two-step AB yields higher standard errors (see, e.g., Arellano and Bond, 1991; Kiviet, 1995; Judson and Owen, 1997). In principle, efficiency gains may be achievable when applying the system GMM estimator developed by Arellano and Bover (1995) and Blundell and Bond (1998), henceforth BB estimator. However, both the AB and the BB estimator are micro panel data estimators and have poor finite sample properties. As N is small in our study ($N = 10$), results of both estimators should mainly be seen as robustness checks.

A more reliable estimator is the bias corrected LSDV estimator (LSDVC). The bias may be approximated to the order of $O(T^{-1})$ when using the approximation derived in Nickell (1981), $O(T^{-1}N^{-1})$ when using Kiviet (1995), and $O(T^{-1}N^{-2})$ when using Kiviet (1999). In a simulation study, Bun and Kiviet (2003) show that the Kiviet (1999) approximation accounts for about 90 per cent of the actual bias. Several simulation studies have shown that the

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[20]We consider the regressors summarized in w_{it} as strictly exogenous so that variables themselves and all their lags are valid instruments. Furthermore, note that the AB estimator takes first order autocorrelation of $\Delta \varepsilon$ into account. Thus, neither consistency nor efficiency is affected by first order autocorrelation. But second order correlation implies inconsistency (Arellano and Bond, 1991, pp. 281-282).

LSDVC estimator outperforms the consistent estimators described above in terms of both bias and standard errors (see, e.g., Bruno, 2005 and Judson and Owen, 1997, 1999). We therefore use the LSDVC estimator for our analysis.

To actually correct the bias one needs an initial consistent estimate of the coefficients and each of the three estimators AH, AB and BB may be used. As the AB estimator typically outperforms the AH estimator and appears more robust than the BB estimator (see Bruno, 2005), we opt for the AB estimator and use the Kiviet (1999) bias approximation. Standard errors are bootstrapped with 100 repetitions.[21]

5. Empirical analysis

The data set comprises yearly data for 10 West German states from 1960 to 2005. In the early years of the FRG, i.e. before 1960, the party structure was relatively unstable. Several small regional parties joined state governments for short periods and disappeared afterwards. Additionally, different coalitions governed within one election period. As this was clearly just a post-war phenomenon, we do not include these years into our analysis. As already mentioned, Berlin and the five new German Länder have not been included in our sample. Berlin is excluded for two reasons. First, Berlin was divided before 1990. While East Berlin was the capital of the GDR, West Berlin was part of the FRG. Second, West Berlin received generous grants from the federal government, making debt issue more or less unnecessary. Data for the East German Länder are available from 1990 onwards. We nevertheless do not include them, as the period is simply too short to obtain sufficient (political) within state variation. We arrive at a balanced panel with 460 observations. The average annual real GDP

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[21]The estimates with BB as initial estimator have slightly higher standard errors. Apart from that results remain unchanged. The complete estimates for the BB and AH estimator are available upon request.

growth was 2.7 per cent, whereas the average annual real debt grew with 6.5 per cent. We capture the costs of borrowing by the real interest rate.[22]

Before testing the political economy of debt issue, we briefly discuss the results with economic indicators and some controls only (Model 1). The regression results are shown in the first column of Table 4 (see Appendix). With a coefficient of around .32, autoregression is relatively moderate. The impact of real GDP growth is, as expected, significantly negative. When real GDP growth drops by one percentage point debt growth gears up by roughly 0.5 percentage points. This may be due to expenditure programs, reduced tax revenues, or both. While the first oil crisis, OIL1, increased debt growth significantly, the second oil crisis, OIL2, had no effect. German unification had a negative impact on debt growth in West Germany. Its insignificance may be due to the fact that most of the financial burden of unification was borne by the social security systems and the German unification fund and not by the states. Moreover, the economy boomed right after unification, increasing tax revenues. This may explain why no further state debt was needed. We find no significant effect of the inclusion of the East German Länder into the fiscal equalization system (EQUAL) on public debt growth. The costs of borrowing, measured by the real interest rate (INTRATE), have the expected negative and significant impact on debt issue.

When adding political variables (Model 2) the first thing to note is that the results mentioned above remain qualitatively the same. The only exception is the effect of the second oil crisis which is now statistically significant at the 10 per cent level.[23] Before we turn to the

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[22]Deficit data are taken from the Statistisches Bundesamt (Federal Statistical Office, 2005). Data for the gross domestic product (GDP) was provided by the Statistical Office of Baden Württemberg and the Federal Statistical Office. All nominal numbers were deflated by the consumer price index for all households obtained from the Statistisches Bundesamt (2006). Our main results do not change qualitatively when using nominal values instead. Election dates were taken from Forschungsgruppe Wahlen (2007, Election Research Team).

[23] Some coefficients change substantially which suggests that a regression without political variables suffers from omitted variable bias.

discussion of the hypotheses formulated in Section 3, note that the federal government bailout helped consolidate the budgets of Saarland (BAILSL) and Bremen (BAILHB). The variable BAILOUT assumes the value 1 starting in 1994 and 0 otherwise. It should pick up any bailout related changes in states' borrowing incentives. As the constitutional court ruled in favor of a federal bailout already in 1992 one may well argue that borrowing incentives may have changed prior to 1994. We address this issue in Models 3 and 4 where the bailout variable assumes the value 1 starting in 1993 and 1992, respectively. A comparison of Models 2 to 4 shows that all results discussed in turn are robust to these alternative codes of the bailout variable.[24] We stick to the 1994 version of the bailout variable and consider Model 2 our baseline specification.

1. Political opportunism (Hypothesis 1)

To test Hypothesis 1 we include two dummy variables in our regression: ELECTION and PREELEC. The first variable equals 1 in election years and zero otherwise, the second accordingly for pre-election years. Model 2 reveals that debt growth in election years is not significantly different from reference years. In contrast, debt growth in pre-election years is about 1.6 percentage points smaller than in reference years. Although the effect is statistically significant the magnitude appears to be small. But when relating this number to average debt growth, we arrive at a 25 per cent lower debt growth in pre-election years: the effect is substantial! We are thus able to reject Hypothesis 1: there is an opportunistic cycle that brings about significantly lower deficits in pre-election years. This result allows us to conclude that German voters seem to favor fiscal discipline.[25] Although results do not allow us to

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[24]Note that the 1994 bailout variable and EQUAL are highly correlated; they only differ in 1994. Even if we drop one of them we do not gain significance of the other. Although correlation is dampened under the alternative codes of the bailout variable it remains insignificant.

[25]This differs from Galli and Rossi (2002) who found significantly positive election year effects but no pre-election year effects.

discriminate between rational and non-rational expectations, our result is in line with the

theory developed by Rogoff and Sibert (1988).[26]

So far we have been silent about how the variables ELECTION and PREELEC are defined.

Using the calendar year (cut-off is December 31) seems to be natural but only at a first sight.

We want the pre-election variable to pick-up the incentives described in Rogoff and Sibert

(1988). But then information on debt in the pre-election year must be readily available when

elections actually take place. Since this is highly unlikely for elections held in January, the

cut-off January 31 appears more plausible than December 31.[27] A similar argument can be

made about elections held in February and March so that we opted for March 31 as cut-off for

Models 2 to 6 in Table 4. Although it seems plausible to deviate from the calendar year, the

actual choice of a cut-off date is largely arbitrary. As Table 5 shows our results are robust to

changes in cut-offs. This robustness is very reassuring that we actually found an opportunistic

cycle.[28] The results with the June cut-off are slightly different, though. We find a significant

negative effect for both, election and pre-election years. But this only strengthens our

assertion that German voters seem to favor fiscal discipline.

2. Partisan theory (Hypothesis 2)

To check whether fiscal policy is driven by party ideology, we have to assign every

government constellation to either left or right. We categorize SPD governments, SPD/FDP

coalitions and SPD/GREEN coalitions as left. CDU governments and CDU/FDP coalitions

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[26] Although we will eventually adopt Model 2 one should note that evidence for political opportunism is not overwhelming. The p-value is 9.4 per cent and only 2 of 5 specifications (Models 2 to 6) show significance at the 10 per cent level.

[27] So suppose the cut-off is January 31. Then an election in, say, January 1982, would have 1981 as election year and 1980 as pre-election year. An election in February 1982 would have 1982 as election year and 1981 as pre-election year.

[28] This robustness is not too surprising since there are only very few elections early in a year (8 per cent of all elections were held in January or February).

are labeled right. It is difficult to ascribe a political orientation to grand coalitions, i.e., coalitions formed by SPD and CDU. There are basically two alternatives. First, do not label such coalitions at all and use them as a reference category in the estimation. Second, use the party affiliation of the prime minister to assign an orientation. We opted for the second alternative as 27 observations of grand coalition governments out of 460 observations are simply too few observations for a sensible reference category.[29] A similar reasoning applies to all other government constellations summarized in ELSE (also 27 of 460 observations). These government constellations are considered left when the Social Democrats were involved and right when the Christian Democrats were. When the government turns over from left to right, or vice versa, the question of whether the government should be labeled left or right in that particular year becomes an issue. We consider the new government's ideological position if its inaugural date was prior to July 1 of the respective year.

The coefficient of LEFT is not statistically different from zero (Table 4, Model 2). So a significant partisan effect cannot be found. Note, however, that the coefficient obeys the 'correct' sign. Although we are unable to reject Hypothesis 2 – which is well in line with Seitz (2000) and Galli and Rossi (2002) – interpretation remains difficult (see also the discussion in Subsection 2). It may well be that there are no partisan trends in German Länder fiscal policy – that ideology plays a negligible role. This is, however, not necessarily true. Once parties care not only about ideology but also about winning the next election, platform convergence will occur. Since elections can well be considered a repeated game, parties will stick to their platforms. Otherwise they risk their reputation: identifying the opponent as a liar is a powerful weapon in electoral competition. If reputation is decisive, then platform convergence implies policy convergence and, with it, adaptation of fiscal policies. Differences can hardly be detected. And indeed, for Germany, it is usually claimed that both major parties,

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[29]Results are independent of the alternative adopted.

SPD and CDU, are close to the center.

3. Fragmented governments (Hypothesis 3)

We define the indicator variable COAL that assumes a value of 1 whenever more than one party formed the government and zero otherwise.[30] We find a highly significant coalition effect, that is, we can reject Hypothesis 3 (Table 4, Model 2). The positive sign is perfectly in line with the theory discussed in Section 3. Note also that if compared to the pre-election effect (in absolute terms), the coalition effect is about three times as high: debt growth in a coalition government is more than 6 percentage points higher than with single party governments.

As argued in Section 3 the more parties forming the coalition, the more severe the common pool problem. To test this assertion we construct two new dummy variables, one for coalition governments with two parties (COAL2) and one for those with three parties (COAL3).[31] The resulting Model 5 (Table 4 in the Appendix) reveals that coalition size appears to be irrelevant. This should not be too surprising, since only 15 observations have governments with three party coalitions. Moreover, due to using lagged growth rates we loose six of these observations (three in each 1960 and 1961).

Finally, one might ask whether the strategy of lower debt issue in pre-election years as discussed in the opportunism section is used equally across government constellations or whether single party governments make more use of this strategic tool than coalition governments. Accordingly, we construction election and pre-election interactions with the

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[30]Again, in years of government changes, we use the inaugural date of the new government and July 1 as the cut-off date to assign a value to COAL.

[31] There never was a coalition government with more than three parties.

coalition variable and, as Model 6 (Table 4) shows, there is no systematic difference between the two.

4. Position of the finance minister (Hypothesis 4)

Hypothesis 4 states that the position of the finance minister within a coalition is irrelevant for a government's borrowing decision. A strong finance minister, however, may be able to mitigate the common pool problem by centralizing fiscal policy (at least to some degree). As already argued above, a finance minister is likely to be in a strong position if he or she has the support of the prime minister in budget negotiations. This tends to be the case if both ministers have the same party affiliation and may be less so otherwise. Consequently we define the variable STRONG such that it assumes the value 1 whenever there is a coalition government where the prime minister and the finance minister belong to the same party and zero otherwise and consider it a proxy for the power of the finance minister.[32] We find a statistically significant negative effect on borrowing (Table 4, Model 2), so that we conclude that the strength of the finance minister matters. Interestingly, this effect exactly offsets the coalition effect (the absolute values of the estimated coefficients of COAL and STRONG are not statistically distinguishable) so that coalition governments with a strong finance minister do not suffer from the common pool problem at all and borrow like single-party governments. In other words, the weak government hypothesis only holds with weak finance ministers but not with strong ones.

6. Conclusion

We analyzed the political determinants of the West German Länder deficits from 1960 to

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[32] 74 per cent of coalition governments have a prime minister and a finance minister belonging to the same party.

2005. Overall we investigated four hypotheses taken from the broad theoretical literature on the political economy of public expenditures and/or public debt issue, including the relatively new aspect of the strength of the finance minister in coalition governments. We found support for the weak government hypothesis – coalition governments borrow more than single party governments. The Hallerberg/Von Hagen thesis suggests that this kind of coordination failure within coalition governments may be mitigated when the finance minister is strong (centralization of fiscal policy). So far, the problem has been to come up with a reasonable measure for the strength of finance ministers. We offer a new approach and suggest that the finance minister can be considered strong when he or she belongs to the same party as the prime minister. It is then very likely that he or she receives the prime minister's support in budget negotiations. Interestingly, a so defined strong finance minister is able to completely offset the negative effects of coalition governments on borrowing. The weak government hypothesis, thus, only holds when the finance minister is weak. If, in contrast, a strong finance minister meets a 'weak' government, then a coalition government borrows like a single party government. So when coalition governments form, economic advisers may wish to recommend the elected prime minister to recruit the finance minister from his or her own party.

While we found no signs of partisan cycles in German fiscal policy there is some (weak) evidence for opportunistic cycles. In electoral competition, governments want to appear competent to voters in order to improve their chances of reelection. Rogoff and Sibert (1988) suggested that fiscal competence can be signaled via low deficits in pre-election years – exactly the pattern that we detected.

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Appendix

Table 1: Literature overview

Study	Data	Dependent variable	Political variables & results
Alesina (1989)	12 OECD countries (1966-1986)	economic growth unemployment, inflation	evidence for partisan political business cycles
Alesina, Cohen and Roubini (1993)	14 OECD countries (1960-1987)	economic growth, unemployment, public expenditures, inflation, money supply	evidence for rational opportunistic business cycles (Rogoff and Sibert) only for the last three dep. variables
Alesina and Sachs (1988)	United States (1949-1984)	economic growth money supply (M1)	evidence for partisan effects for both dependent variables
Andrikopoulos, Loizides and Prodromidis (2006)	European Union (1965-1997)	economic target variables and tax instruments	No evidence for political business cycles or partisan behavior
Ashworth, Geys and Heyndels (2005)	Flemish Municipalities (1977-2000)	government debt	government fragmentation plays a role in the short but not in the long run
Belke (2000)	Germany (1970-1996)	various labor market variables	evidence for short and long term partisan influences
Boix (2000)	19 OECD countries (1960-1993)	interest rate public debt	evidence for partisan trends
Bräuninger (2005)	19 OECD countries (1971-1999)	government spending (general and social security)	partisan effects in spending patterns
Carlsen (1997)	18 OECD countries (1980-1992)	structural deficit	Partisan effects when unemployment is high or rising
Cusack (1997)	16 OECD countries (1955-1989)	general government spending	evidence for partisan trends
De Haan and Sturm (1997)	21 OECD countries (1982-1992)	public deficit	no evidence for an effect of government fragmentation
Drazen and Eslava (2005)	Colombian municipalities (1987-2000)	government spending (expenditure categories)	evidence for partisan effects, voters penalize incumbents for deficits occurring prior to elections

Study	Data	Dependent variable	Political variables & results
Easaw and Garratt (2000)	UK Conservative governments (1979-1992)	government expenditures	Transfers and expenditures are responsive to national income in pre-election periods
Edin and Ohlsson (1991)	13 OECD countries (1960-1985)	public deficit	Roubini and Sachs (1989) results are driven by minority governments
Galli and Rossi (2002)	11 West German states (1974-1994)	government expenditures deficits/surplus expenditure categories	no partisan trends evidence for political business cycle (election years)
Geys (2007)	Flemish Municipalities (1977-2000)	debt growth rate	Government fragmentation (number of coalition partners) affects business cycle
Heckelman (2002)	Canada (1965-1994) Germany (1977-1994) UK (1960-1993)	economic growth unemployment	evidence for persistent partisan effects, party popularity affects business cycle
Heckelman (2006)	7 OECD countries (1960-1993)	unemployment	almost no evidence for partisan effects (except US for democrat victories)
Hibbs (1977)	12 OECD countries (1945-1969)	unemployment inflation	evidence for partisan effects
Nordhaus (1975)	9 OECD countries (1947-1972)	unemployment	evidence for opportunistic political business cycles
Reed (2006)	United States, 45 states (1960-2000)	tax burden	evidence for partisan effects (higher tax burden with democrat governments)
Roubini and Sachs (1989)	13 OECD countries (1960-1985)	public deficit	coalition governments run higher deficits than single party governments
Seitz (2000)	10 West German states (1976-1996)	expenditures, deficits and alternative economic variables	no evidence for partisan effects
Tavares (2004)	19 OECD countries (1960-1995)	tax revenue public expenditures	evidence for partisan effects (left: higher tax revenue, right: lower spending)
Veiga and Veiga (2007)	278 Portuguese municipalities (1979-2001)	debt, tax revenue, public expenditures and others	evidence for opportunistic political business cycles (pre-election effects)
Wehner (2009)	60 countries (1975-1998)	public deficits and expenditures	number of spending ministers influence budget deficits and expenditures

Table 2: Explanation of variables

Variable	Explanation
DEBT	real debt growth rate
DEBT(-1)	lagged real debt growth rate
GDP	real growth rate of gross domestic product
INTRATE	real interest rate
OIL1	= 1 from 1974 to 1975 (first oil crisis)
OIL2	= 1 from 1978 to 1981 (second oil crisis)
UNIFIC	= 1 from 1991 to 2005 (unification)
EQUAL	= 1 from 1995 to 2005 (equalization scheme)
BAILOUT	= 1 from 1994 to 2005 (federal government bailout)
BAILHB	= 1 if BAILOUT = 1 and Bremen (Bailout-Bremen interaction)
BAILSL	= 1 if BAILOUT = 1 and Saarland (Bailout-Saarland interaction)
ELECTION	= 1 in election years
PREELEC	= 1 in pre-election years
LEFT	= 1 for SPD dominated governments
RIGHT	= 1 for CDU dominated governments
SPD	= 1 for single-party Social Democratic governments
CDU	= 1 for single-party Christian Democratic governments
SPDFDP	= 1 for SPD coalitions with Liberals
SPDGREEN	= 1 for SPD coalitions with Greens
GRANDC	= 1 for SPD coalitions with the CDU or vice versa
SPDCDU	= 1 for GRANDC = 1 and SPD prime minister
CDUSPD	= 1 for GRANDC = 1 and CDU prime minister
CDUFDP	= 1 for CDU coalitions with Liberals
ELSE	= 1 for remaining government constellations
COAL	= 1 for coalition governments
COAL2	= 1 for coalition governments with two parties
COAL3	= 1 for coalition governments with three parties
ELECCOAL	= ELECTION * COAL
PRECOAL	= PREELEC * COAL
STRONG	= 1 if prime and finance minister belong to the same party in a coalition

123456789...60

Table 3: Descriptive statistics

Variable	N	mean	s.d.	min	max
DEBT	450	0.0648	0.1078	-0.1760	1.2014
GDP	450	0.0275	0.0333	-0.0476	0.1478
INTRATE	460	0.0407	0.0162	0.0112	0.0790
SPD	460	0.2043	0.4037	0	1
SPDFDP	460	0.1587	0.3658	0	1
SPDGREEN	460	0.0783	0.2689	0	1
SPDCDU	460	0.0348	0.1834	0	1
CDU	460	0.2739	0.4465	0	1
CDUSPD	460	0.0239	0.1529	0	1
CDUFDP	460	0.1696	0.3757	0	1
ELSE	460	0.0565	0.2312	0	1
GRANDC	460	0.0587	0.2353	0	1
DEBT * ELECTION	110	0.0718	0.1103	-0.0953	0.8687
DEBT * PREELEC	111	0.0480	0.0896	-0.1760	0.3536
LEFT	460	0.5130	0.5004	0	1
RIGHT	460	0.4870	0.5004	0	1
DEBT * LEFT	232	0.0633	0.0962	-0.1049	0.8687
DEBT * RIGHT	218	0.0665	0.1191	-0.1760	1.2014
COAL	460	0.5217	0.5001	0	1
COAL2	460	0.4891	0.5004	0	1
COAL3	460	0.0326	0.1778	0	1
DEBT * COAL	231	0.0657	0.1317	-0.1760	1.2014
DEBT * $(1-\text{COAL})$	219	0.0639	0.0749	-0.0869	0.3927
DEBT * STRONG	172	0.0585	0.1034	-0.1760	0.8687

Table 4: Regression results.

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
DEBT(-1)	0.3180***	0.0489	0.3134***	0.0483	0.3090***	0.0483	0.3064***	0.0480	0.3030***	0.0480	0.3130***	0.0485
GDP	-0.5133***	0.1608	-0.5842***	0.1599	-0.6507***	0.1680	-0.6946***	0.1927	-0.6071***	0.1585	-0.5815***	0.1602
INTRATE	-1.0892***	0.3568	-0.8833**	0.3570	-0.9172***	0.3498	-0.8219**	0.3499	-0.9519***	0.3544	-0.8756**	0.3562
OIL1	0.1449***	0.0225	0.1515***	0.0226	0.1490***	0.0230	0.1462***	0.0232	0.1503***	0.0225	0.1510***	0.0230
OIL2	0.0185	0.0166	0.0275*	0.0163	0.0265	0.0164	0.0247	0.0166	0.0272*	0.0162	0.0273*	0.0165
UNIFIC	-0.0101	0.0194	-0.0075	0.0197	0.0056	0.0227	0.0234	0.0341	-0.0022	0.0203	-0.0080	0.0195
EQUAL	0.0046	0.0270	-0.0017	0.0267	-0.0062	0.0237	-0.0112	0.0222	-0.0088	0.0274	-0.0019	0.0268
BAILOUT	-0.0436	0.0300	-0.0313	0.0302	-0.0429	0.0298	-0.0560	0.0414	-0.0318	0.0301	-0.0305	0.0303
BAILHB	-0.0248	0.0394	-0.0693*	0.0396	-0.0699*	0.0396	-0.0714*	0.0393	-0.0694*	0.0395	-0.0693*	0.0398
BAILSL	-0.0789**	0.0328	-0.0796**	0.0347	-0.0796**	0.0343	-0.0798**	0.0340	-0.0783**	0.0346	-0.0796**	0.0347
ELECTION			-0.0028	0.0098	-0.0029	0.0097	-0.0034	0.0097	-0.0028	0.0098	-0.0030	0.0119
PREELEC			-0.0157*	0.0094	-0.0152	0.0096	-0.0159*	0.0093	-0.0144	0.0093	-0.0142	0.0109
LEFT			0.0064	0.0129	0.0067	0.0129	0.0066	0.0129	0.0039	0.0129	0.0062	0.0129
COAL			0.0631***	0.0159	0.0640***	0.0159	0.0656***	0.0159			0.0633***	0.0158
STRONG			-0.0641***	0.0193	-0.0645***	0.0192	-0.0652***	0.0191	-0.0639***	0.0192	-0.0639***	0.0192
COAL2									0.0662***	0.0159		
COAL3									0.0030	0.0350		
ELECCOAL											0.0030	0.0155
PRECOAL											-0.0040	0.0175

Notes: Dependent variable is DEBT, N = 430. Significance levels: *** =0.01, ** = 0.05, * = 0.10.

Baseline model (Model 2) has BAILOUT = 1 for all years starting in 1994, Model 3 starting in 1993, Model 4 starting in 1992.

Table 5: Regression results for alternate cut-off dates for election and pre-election years.

	December 31		January 31		February 28/29		March 31		June 30	
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
DEBT(-1)	0.3143***	0.0484	0.3138***	0.0483	0.3135***	0.0483	0.3134***	0.0483	0.3185***	0.0479
GDP	-0.5811***	0.1582	-0.5843***	0.1591	-0.5848***	0.1588	-0.5842***	0.1599	-0.5782***	0.1590
INTRATE	-0.8350**	0.3796	-0.8580**	0.3635	-0.8468**	0.3641	-0.8833**	0.3570	-0.8366**	0.3574
OIL1	0.1507***	0.0267	0.1513***	0.0228	0.1511***	0.0228	0.1515***	0.0226	0.1497***	0.0223
OIL2	0.0264	0.0162	0.0266	0.0163	0.0264	0.0163	0.0275*	0.0163	0.0265	0.0162
UNIFIC	-0.0101	0.0201	-0.0088	0.0198	-0.0085	0.0198	-0.0075	0.0197	-0.0102	0.0198
EQUAL	0.0025	0.0301	0.0022	0.0265	0.0043	0.0267	-0.0017	0.0267	-0.0038	0.0263
BAILOUT	-0.0340	0.0327	-0.0333	0.0301	-0.0357	0.0302	-0.0313	0.0302	-0.0266	0.0301
BAILHB	-0.0658*	0.0358	-0.0694*	0.0396	-0.0692*	0.0396	-0.0693*	0.0396	-0.0709*	0.0395
BAILSL	-0.0728**	0.0346	-0.0799**	0.0346	-0.0796**	0.0347	-0.0796**	0.0347	-0.0769**	0.0345
ELECTION	0.0031	0.0106	0.0020	0.0116	0.0019	0.0113	-0.0028	0.0098	-0.0178*	0.0096
PREELEC	-0.0164	0.0105	-0.0185*	0.0104	-0.0177*	0.0102	-0.0157*	0.0094	-0.0259***	0.0099
LEFT	0.0080	0.0101	0.0062	0.0129	0.0061	0.0128	0.0064	0.0129	0.0058	0.0127
COAL	0.0633***	0.0166	0.0632***	0.0158	0.0632***	0.0159	0.0631***	0.0159	0.0632***	0.0158
STRONG	-0.0641***	0.0172	-0.0641***	0.0193	-0.0641***	0.0193	-0.0641***	0.0193	-0.0632***	0.0191

Notes: Dependent variable is DEBT, N = 430. Significance levels: *** =0.01, ** = 0.05, * = 0.10.