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.

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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 indebtedness 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. 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

\footnote{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

\(^4\) 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

\(^5\)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 their 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. 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,

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8In 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.
9To 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.
among other things, an overview of government formations in the West German states.\footnote{10}

\section{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.

\subsection{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.\footnote{11}

\footnote{10}{For an explanation of the variables see Table 2 in the Appendix.}
\footnote{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.}
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.

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
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

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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).
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. 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 weak finance minister.

That the power of the finance minister in the budget process is decisive for fiscal performance

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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.
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

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

where \(d_{it}\) denotes the real growth rate of public debt in state \(i = 1, \ldots, N\) at time \(t = 1, \ldots, T\) and
$d_{i,t-1}$ its first lag, $t = 2, \ldots, T$.\textsuperscript{14} The political variables are summarized in the vector $x_n$, the control variables in $z_n$. 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).\textsuperscript{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.\textsuperscript{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.\textsuperscript{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 $\mu_i$. We will consider these effects as fixed rather 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

\textsuperscript{14}Note that real debt growth is simply real deficit over real debt.

\textsuperscript{15}Definition of all variables can be found in the Appendix in Table 2.

\textsuperscript{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.

\textsuperscript{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.
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. Random disturbance is \( \varepsilon_{it} \sim N(0, \sigma_r^2) \). Let \( w'_{it} = (x'_{it}, z'_{it}) \), then the assumptions of the model can be summarized as follows

\[
E(\varepsilon_{it} \varepsilon_{js}) = 0 \quad \text{for} \quad i \neq j \quad \text{or} \quad t \neq s \\
E(\mu_i \varepsilon_{it}) = 0 \quad \text{for all} \quad i, j, t \\
E(w_{it} \varepsilon_{it}) = 0 \quad \text{for all} \quad i, j, s, t.
\]

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 \( \gamma \) 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 \( \mu_i \). 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 \( \gamma \) may be seriously biased downwards in short panels. 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),

\[
\Delta d_{it} = \gamma \Delta d_{it-1} + \beta_1' \Delta x_{it} + \beta_2' \Delta z_{it} + \Delta \varepsilon_{it},
\]

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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.
where $\Delta$ 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 $\Delta d_{it}$. 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 LSDVC estimator outperforms the consistent estimators described above in terms of both bias

\(^{20}\)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 $\Delta \epsilon$ 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).
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 growth was 2.7 per cent, whereas the average annual real debt grew with 6.5 per cent. We

²¹The 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.
capture the costs of borrowing by the real interest rate.\textsuperscript{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.\textsuperscript{23} Before we turn to the discussion of the hypotheses formulated in Section 3, note that the federal government bailout

\textsuperscript{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).

\textsuperscript{23}Some coefficients change substantially which suggests that a regression without political variables suffers from omitted variable bias.
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.\textsuperscript{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.\textsuperscript{25} Although results do not allow us to discriminate between rational and non-rational expectations, our result is in line with the

\textsuperscript{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.

\textsuperscript{25}This differs from Galli and Rossi (2002) who found significantly positive election year effects but no pre-election year effects.
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 are labeled right. It is difficult to ascribe a political orientation to grand coalitions, i.e.,

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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).
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.\textsuperscript{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, SPD and CDU, are close to the center.

\textsuperscript{29}Results are independent of the alternative adopted.
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 coalition variable and, as Model 6 (Table 4) shows, there is no systematic difference between the two.

\(^{30}\) 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.
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 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

\(^{32}\) 74 per cent of coalition governments have a prime minister and a finance minister belonging to the same party.
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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References


### Table 1: Literature overview

<table>
<thead>
<tr>
<th>Study</th>
<th>Data</th>
<th>Dependent variable</th>
<th>Political variables &amp; results</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alesina, Cohen and Roubini (1993)</td>
<td>14 OECD countries (1960-1987)</td>
<td>economic growth, unemployment, public expenditures, inflation, money supply</td>
<td>evidence for rational opportunistic business cycles (Rogoff and Sibert) only for the last three dep. variables</td>
</tr>
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<td>Alesina and Sachs (1988)</td>
<td>United States (1949-1984)</td>
<td>economic growth money supply (M1)</td>
<td>evidence for partisan effects for both dependent variables</td>
</tr>
<tr>
<td>Ashworth, Geys and Heyndels (2005)</td>
<td>Flemish Municipalities (1977-2000)</td>
<td>government debt</td>
<td>government fragmentation plays a role in the short but not in the long run</td>
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<td>Carlsen (1997)</td>
<td>18 OECD countries (1980-1992)</td>
<td>structural deficit</td>
<td>Partisan effects when unemployment is high or rising</td>
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To be continued next page
<table>
<thead>
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<th>Study</th>
<th>Data</th>
<th>Dependent variable</th>
<th>Political variables &amp; results</th>
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<tbody>
<tr>
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<td>UK Conservative governments (1979-1992)</td>
<td>government expenditures</td>
<td>Transfers and expenditures are responsive to national income in pre-election periods</td>
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<td>(2000)</td>
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<td>Edin and Ohlsson</td>
<td>13 OECD countries (1960-1985)</td>
<td>public deficit</td>
<td>Roubini and Sachs (1989) results are driven by minority governments</td>
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<td>(1991)</td>
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<td></td>
<td></td>
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<tr>
<td>Galli and Rossi</td>
<td>11 West German states (1974-1994)</td>
<td>government expenditures deficits/surplus expenditure categories</td>
<td>no partisan trends evidence for political business cycle (election years)</td>
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<td>(2002)</td>
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<td></td>
</tr>
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<td></td>
<td>Germany (1977-1994)</td>
<td>unemployment</td>
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<td></td>
<td>UK (1960-1993)</td>
<td>unemployment</td>
<td>evidence for opportunistic political business cycles</td>
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<td>Hibbs (1977)</td>
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<td>unemployment</td>
<td>evidence for partisan effects</td>
</tr>
<tr>
<td>Nordhaus (1975)</td>
<td>9 OECD countries (1947-1972)</td>
<td>unemployment</td>
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</tr>
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<td>Reed (2006)</td>
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<td>tax burden</td>
<td>evidence for partisan effects (higher tax burden with democrat governments)</td>
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<td>Roubini and Sachs</td>
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<td>public deficit</td>
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<td>(1989)</td>
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<tr>
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<td>10 West German states (1976-1996)</td>
<td>expenditures, deficits and alternative economic variables</td>
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<td>Veiga and Veiga (2007)</td>
<td>278 Portuguese municipalities (1979-2001)</td>
<td>debt, tax revenue, public expenditures and others</td>
<td>evidence for opportunistic political business cycles (pre-election effects)</td>
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<td>Wehner (2009)</td>
<td>60 countries (1975-1998)</td>
<td>public deficits and expenditures</td>
<td>number of spending ministers influence budget deficits and expenditures</td>
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Table 2: Explanation of variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Explanation</th>
</tr>
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<tbody>
<tr>
<td>DEBT</td>
<td>real debt growth rate</td>
</tr>
<tr>
<td>DEBT(-1)</td>
<td>lagged real debt growth rate</td>
</tr>
<tr>
<td>GDP</td>
<td>real growth rate of gross domestic product</td>
</tr>
<tr>
<td>INTRATE</td>
<td>real interest rate</td>
</tr>
<tr>
<td>OIL1</td>
<td>= 1 from 1974 to 1975 (first oil crisis)</td>
</tr>
<tr>
<td>OIL2</td>
<td>= 1 from 1978 to 1981 (second oil crisis)</td>
</tr>
<tr>
<td>UNIFIC</td>
<td>= 1 from 1991 to 2005 (unification)</td>
</tr>
<tr>
<td>EQUAL</td>
<td>= 1 from 1995 to 2005 (equalization scheme)</td>
</tr>
<tr>
<td>BAILOUT</td>
<td>= 1 from 1994 to 2005 (federal government bailout)</td>
</tr>
<tr>
<td>BAILHUB</td>
<td>= 1 if BAILOUT = 1 and Bremen (Bailout-Bremen interaction)</td>
</tr>
<tr>
<td>BAILSL</td>
<td>= 1 if BAILOUT = 1 and Saarland (Bailout-Saarland interaction)</td>
</tr>
<tr>
<td>ELECTION</td>
<td>= 1 in election years</td>
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<tr>
<td>PREELEC</td>
<td>= 1 in pre-election years</td>
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<tr>
<td>LEFT</td>
<td>= 1 for SPD dominated governments</td>
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<td>RIGHT</td>
<td>= 1 for CDU dominated governments</td>
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<td>= 1 for single-party Christian Democratic governments</td>
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<td>= 1 for SPD coalitions with Liberals</td>
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<td>GRANDC</td>
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<td>CDUFDP</td>
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<td>COAL2</td>
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</tr>
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<td>Variable</td>
<td>N</td>
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<td>COAL3</td>
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<tr>
<td>DEBT * (1−COAL)</td>
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<td>BAILSL</td>
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<td>0.0662***</td>
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<tr>
<td>PRECOAL</td>
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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.

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<tr>
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<th>December 31</th>
<th>January 31</th>
<th>February 28/29</th>
<th>March 31</th>
<th>June 30</th>
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<tr>
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<td>s.e.</td>
<td>coeff.</td>
<td>s.e.</td>
<td>coeff.</td>
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<td>0.3138 ***</td>
<td>0.0483</td>
<td>0.3135 ***</td>
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<td>GDP</td>
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<td>-0.5843 ***</td>
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<td>INTRATE</td>
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<tr>
<td>OIL1</td>
<td>0.1507 ***</td>
<td>0.0267</td>
<td>0.1513 ***</td>
<td>0.0228</td>
<td>0.1511 ***</td>
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<td>OIL2</td>
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<td>-0.0333</td>
<td>0.0301</td>
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<td>0.0358</td>
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<td>-0.0692 *</td>
</tr>
<tr>
<td>BAILSL</td>
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<td>0.0346</td>
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<td>0.0105</td>
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<td>-0.0641 ***</td>
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<td>-0.0641 ***</td>
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</tbody>
</table>

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