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Searching for Political Business Cycles in Germany

Helge Berger*
Ulrich Woitek

University of Munich
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Abstract
One result of the revitalised literature on political business cycles is that there are signs in German data that elections and ideology have an impact. However, using monthly data for the period 1950-89, and standard methods of time series analysis, we reject both the Nordhaus hypothesis of opportunistic cycles and the partisan approaches by Hibbs and Alesina. We show that some results are sensitive to the assumption of stationarity. There are signs of policy cycles in M1. It is hard, though, to bring this in line with the Rogoff hypothesis of rational opportunistic cycles because of the German institutional setting.
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* Correspondence: Center for Economic Studies, Universität München, Schackstr. 4, 80539 München. Tel: +49(89)2180-6273, Fax: +49(89)897303, E-Mail: h.berger@ces.vwl.uni-muenchen.de.
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1. Introduction

There are two strands of thought which formalize the common notion that politics matter for the conduct of economic policy, i.e. the *Opportunistic* and the *Partisan Schools*.[1] The opportunistic school argues that incumbent governments have an incentive to use expansionary policy to improve their economic performance before the election (Nordhaus (1975), McRae (1977)). In the absence of rational expectations this would help to increase real economic activity and re-election chances if voters judged incumbents by economic performance. After re-election governments would contract the economy to prepare it for a new upswing. Even if voters were rational, there might be reasons for expansionary policy measures before an election: In the models by Rogoff and Sibert (1988) and Rogoff (1990) governments exploit temporary information asymmetries to signal competence before election day by increasing the fiscal deficit, creating something like a *Rational Opportunistic Cycle*.

The other line of thought in the field, the partisan school, was initiated by Hibbs (1977). He argued that politicians are not opportunistic but decide the course of economic policy according to their ideological preferences. "Right" governments will, in general, pursue a less expansionary policy than "left" governments. If the economy was the non-rational, Phillips-curve type, this difference in policy would influence the real economy, probably generating something like a *Partisan Cycle* or partisan trends. Even if expectations were rational, one could expect to find real effects, since election results can (by assumption) never be fully anticipated and thus their revelation will generate at least a short "blip" in the macroeconomic data when agents re-contract the allocation of resources - a *Rational Partisan Cycle* occurs (Alesina (1987), Alesina (1988) and Alesina and Sachs (1988)).

Both the opportunistic and the partisan school have found some support in empirical studies (Alesina and Roubini (1990, 1992), Paldam (1991a, 1991b), Alesina, Cohen and Roubini (1992), Lang and Welzel (1992)) that have revitalised a debate that began in the late 1970s (see e.g. McCallum (1978), Frey (1978), Paldam (1979),

Kirchgässner (1983, 1984)) taking advantage of the meanwhile increased data base. Much to the surprise of the authors some support for both types of hypotheses came from German data. Since German economic policy is rather well known for its sound fiscal manners and its independent *Bundesbank* and not so much for manifest partisan or opportunistic influences a closer look on the matter is called for. In order to do so, we will extend the database in terms of frequency (monthly instead of annual or quarterly data), range (including the 1950s - already a period of intense stabilisation policy debates in Germany), and time series (including the *Bundesbank* discount rate as a policy instrument) compared to the studies mentioned above. In addition we control for the problems of seasonality and stationarity in the time series and correct for mistakes in the political data used so far.

The paper continues as follows. In Section 2 we briefly discuss our data. Then, in Section 3, we summarize our results on the predictions of the partisan school and review the empirical literature so far in more detail. In Section 4 we focus on the opportunistic school in the same manner. Finally, Section 5 concludes.

2. Data and Methods

One problem, which all empirical papers on the subject of political business cycles up to now have in common, is that they restrict themselves to annual or quarterly data starting in the 1960s. Annual data and, to a lesser extent, quarterly data make it difficult to analyze political efforts aimed at a certain day of the year when the election takes place. It would be preferable to use monthly data instead. In addition, excluding the 1950s unnecessarily reduces the data base. The 1950s saw almost as much debate of anticyclical *stabilisation programs* as for instance the 1960s, and both politics and economics were much more familiar with the ideas of Keynes as it is sometimes thought (Berger (1995)).[2] Fortunately, for Germany, monthly data are available from 1950 onward for important macroeconomic variables and policy instruments, i.e. net industrial output (NPI), unemployment (UE), consumer prices (CPI), M1 and the *Bundesbank* discount rate (r). The latter is the rate the *Bundesbank* Council decides to charge for short term lending to

commercial banks. Most of the series are available through the *Bundesbank* directly or with the aid of a commercial provider.[3]

As an indicator for fiscal policy we use the net deficit generated by the federal governments (FD) as computed by the German statistical bureau in its national accounts since 1950 (Statistisches Bundesamt (1994)).[4] Unfortunately this kind of data is only available on an annual basis. Official budget figures are available for shorter intervals but are clearly inferior in terms of indicating the short term economic effects of fiscal policy (Blejer and Cheasty (1991), Arlt (1994)).[5] Using the deficit of the public sector as a whole, as Alesina, Cohen and Roubini (1992) do, would be misleading, since most of the agents who in addition to the federal government influence that variable are concerned with not federal but with local elections. Also, local elections are usually not co-ordinated.

All necessary political data on elections, government coalitions, and changes of governments are published by the Statistisches Bundesamt (Jahrbuch 1950 ff.). If not mentioned otherwise, we use the same political data as has been used in the literature up to now. For the years after 1960 see e.g. Alesina and Roubini (1992). The particular problems of this data set are discussed in Section 3.3 below.

To avoid the turmoils of German unification, our analysis starts in 1950 and ends in 1989. In general, we consider 480 observations on monthly series (or 468 observations on growth rates), 40 (39) observations on annual series, and 11 federal elections.

Our procedure differs from the literature in so far as we estimate VAR (vector autoregressive) models including all the data for the German economy mentioned above instead of only univariate AR (autoregressive) models. The VAR models are able to capture the repercussions between the time series involved.[6] We estimate:

$$\mathbf{A}_t = \mathbf{A}_1\mathbf{X}_{t-1} + \cdots + \mathbf{A}_p\mathbf{X}_{t-p} + \mathbf{g}\boldsymbol{D}_t + \mathbf{u}_t$$

where $\mathbf{X}_t$ is a (6x1) vector containing the variables NPI, M1, CPI, UE, r, and FD at time *t*. $\mathbf{A}_1,\ldots,\mathbf{A}_p$ are the parameter matrices of our VAR model, and $\mathbf{u}_t$ is a (6x1) vector of

errors following the usual assumptions.[7] D_t is the dummy for exogenous changes in government behaviour. It will take different forms according to the different theories under test. $\mathbf{g}$ is a (6x1) vector of parameters measuring the impact of the respective D_t variable on each of the elements of $\mathbf{X}_t$.

But before we can proceed, we have to make sure our series are stationary and we have to remove the seasonal component from the series. To do this without distorting the remaining structure we are mainly interested in, we have to analyze the type of non-stationarity, respectively the type of seasonality. The literature up to now mechanically transforms its (quarterly) data into annual growth rates to get rid of both problems. However, this transformation is not based on statistical tests and might or might not be correct.[8] As far as the problem of seasonality is concerned, Berger and Woitek (1995) show that the use of the annual growth rates (annual first differences of the log of the original series) of our monthly time series as our "raw" data is indeed the proper way to get rid of the seasonal component in the data. The only exeptions are the discount rate (original series in per cent) and the data on the fiscal deficit (first differences of the original series in bill. DM) which both lack a seasonal component in the first place.[9]

Then, since standard tests indicate that not all of our raw data are stationary, we compute results for a second VAR model that contains an additional polynomial trend as well.[10] As a robustness check, all our results will be presented for both VAR models with and without the additional trend component.

3. Partisan Theories

3.1. Hibbs and Alesina: Partisan Politics

Both the Hibbs and the Alesina version of the partisan theory argue that there are ideologically motivated differences in the way "left" and "right" governments conduct fiscal and monetary policy. These differences are explained by differing preferences and assumed to be time invariant, i.e. incumbents do not trade in ideology for opportunism around elections. As a matter of fact, there is some support for the Hibbs and Alesina hypothesis of partisan behaviour in the empirical literature, that is, "left" governments are

found to follow a more expansionary course than "right" governments. Note, however, that these papers did not test directly for the ideological differences assumed by theory: For instance Alesina, Cohen and Roubini (1992) only look for differences in the growth rates of monetary aggregates around elections or other changes of government majorities and not for the fundamental differences in the conduct of policy that should be expected if policy is determined by time-invariant partisan preference sets.[11] However, we will argue that following these approaches instead of the theory does not alter our results.

Our test uses the VAR models described above. The dummy variable has the value 1 if a certain party is in charge of economic policy and is 0 otherwise. We run two different regressions for conservative and social democratic governments. That is to say, we only allow the *Party* index to take two forms: *"right" majority government* or *"left" majority government*. Figure 1 summarizes the political history covered by the dummies.

(Figure 1 about here)

The subperiods with a "right" governments are 1951-1966, and 1982-1989. The subperiod with a "left" government is 1967-1981. These subperiods cleary divide the political history of federal government in Germany. The only possible exception is the subperiod 1967-82 which includes the "Grand Coalition" between the conservative and the social democratic party from December 1966 until October 1969 which preceded the social democratic reign. However, the period definitely marked the end of the conservative era in early post-war Germany and economic policy was quite obviously dominated by the social democrats in that period (Neumann (1993)), so the periodization chosen is well justified.[12]

(Table 1: Partisan Politics about here)

Table 1 summarizes our results for the above model as follows. The estimated coefficients for the political dummies in the VAR models described above are in the first column. In column (2), the results for the VAR models with polynomial trend can be

found. For every dummy variable a separate VAR has been estimated. The significance levels are as noted on the bottom of Table 1. In addition the note hints at the R^2_{adj} and Q^*-statistics of the estimations. The latter provide a test for autocorrelation in the estimated residuals at arbitrary lags. In general we used up to 10 lags to compute them.

Let us take a look at monetary policy first. As already metioned, we use M1 and the *Bundesbank* discount rate as variables. The standard definition of the monetary aggregate M1 is used. It is easier to control than, say, M3, but even the *Bundesbank* is not the only supplier of M1. The discount rate, on the other hand, is solely determined by the monetary authority itself.

As Table 1 reveals, the results clearly do not support the Hibbs hypothesis for the monetary aggregate. For both models, the VAR with (column (2)) and without (column (1)) trend component, the null hypothesis cannot be rejected on conventional levels for M1. It looks as if there is some impact of government ideology on the *Bundesbank* discount rate, but the coefficients have the wrong signs: the central bank seems to have pursued a more expansionary policy with lower discount rates when there were "right" governments. The contrary is the case when there were "left" governments. In summary, it seems that the Hibbs hypothesis cannot explain the course of monetary policy in Germany.

To see whether the results are robust, one can go through a whole set of alternative regressions.[13] One reason for these results might simply be that the *Bundesbank* is truly autonomous. After all, it is one of the most independent central banks in the world (Alesina and Summers (1993), Eijffinger and de Haan (1996)). In this case, however, it might be argued that the *Bundesbank* was autonomously following a partisan course of its own and that it supported governments only if shared the incumbents ideology (Vaubel (1993)). However, using three different test procedures, Berger and Woitek (1996) clearly have to reject the so called party preference hypothesis for the *Bundesbank*.

As far as M1 is concerned, there could be another reason why the predictions by Hibbs and Alesina were rejected. Given full convertibility (officially after 1958) and a notorious trade surplus, it might be sensible to control for the influence of the balance-of-payments on M1. Doing so, we found that the impact on the performance of the relevant dummies was negligible.

Then, to make sure that our results are not due to the fact that we use monthly instead of quarterly data, we re-run all regressions, except M1, with the data transformed to quarterly averages. In the case of M1 we used the money stock in the last month of each quarter. We got comparable results out of these tests.

Finally, as mentioned above, there are no signs that our results are significantly altered if we look for partisan differences in the conduct of economic policy only before elections or other polical changes as do Alesina, Cohen and Roubini (1992). In particular, we could not reproduce their findings that right German governments pursued a more expansionary monetary policy before elections.

Looking at the last row of Table 1, we see that the partisan preferences of federal government had no impact on the federal net deficit. Again, our results were not changed by selecting only certain pre-election intervals out of the conservative or social democratic subperiods as in Alesina, Cohen and Roubini (1992).[14]

All in all, we conclude that the partisan hypothesis about a significant difference in the conduct of economic policy due to "right" or "left" idology has to be rejected. Since both the non-rational expectations and the rational expectations version of the partisan school assume a significant difference in policies, we could as well stop here, and turn to the implications of the opportunistic school. However, since the empirical literature up to now suggests that there is evidence for an impact of ideologies on macroeconomic data, we will proceed.

3.2 The Hibbs Hypothesis: Partisan Cycles in the Macroeconomy

Alesina and Roubini (1990) report evidence for permanent partisan effects on unemployment and real GNP elsewhere, especially in Germany. In annual data on 17 OECD countries between 1948 and 1988 Paldam (1991a) uncovers "really convincing evidence" for the Hibbs hypothesis mainly for the US. Here governments succeeded in influencing real growth, unemployment and prices according to their ideology. However, Paldam (1991b) also looks at "left" and "right" German governments and their impact on unemployment and inflation. For "left" governments, he finds weak support for the negative influence on unemployment predicted by the partisan school. The coefficients, however, are only weakly significant. Influence of "left" governments on inflation has the expected positive direction but is not significant at usual levels. While this result favours the Hibbs hypothesis, the performance of "right" governments does not. Following Paldam's regressions, German conservatives reduce unemployment and increase inflation when in government just as "left" governments do. The coefficients for domestic unemployment and (inflation) data are significant at a 5 per cent (10 per cent) level.

The counterintuitive results for "right" governments might give us a hint of the underlying reasons for what Paldam and others reported. The long period of federal conservative government between 1949 and 1966 was characterized economically by the reduction of unemployment from more than 11 per cent to below 1 per cent of the employed workforce (Deutsche Bundesbank (1988)) as a result of the extraordinary expansion of production during Germany's "*Wirtschaftswunder*". This non-stationarity could have been picked up by the regression because the data were not properly filtered.

One way to test this argument is to analyze the results with our data set and the the VAR models introduced above.

(Table 2: Partisan Cycles in the Macroeconomy about here)

Let us first focus on column (1). For NPI and UE, our results definitely do not favour the Hibbs approach. Looking on the results for inflation changes this impression. The growth rate of prices seems to have been significantly lowered by conservative

governments and raised by social democratic ones as predicted by partisan theory. However, the significant result for "right" majorities is not robust. It vanishes if a trend component is included in the model (column (2)). In other words, different assumptions on the non-stationarity of the time series in question might lead, at least in the German case, to a possible misinterpretation of the long run trends of the economy as something like a partisan cycle in economic variables.[15] The results for "left" governments are more robust. However, they seem not to be caused by partisan political behaviour alone: the introduction of an additional dummy variable in both VAR models for the oil price shocks in the 1970s and 1980s renders the coefficients for the political dummies insignificant (see notes in Table 2). Hence we conclude that, despite the empirical literature, we find only insufficient evidence for the partisan theory in the Hibbs' version, both in the macroeconomy and in the policy variables. The results signalling an influence of partisan policies on inflation rates in the predicted direction seem to be an artefact of neglecting the problems of stationarity and missing variables. In general, the impact of ideology has to be rejected.

3.3 The Alesina Hypothesis: Rational Partisan Cycles

The reason why the Hibbs version of the partisan theory fails to convince empirically for a second time might simply be that it wrongly assumes that expectations are non-rational. Therefore the next step has to be a test of the Alesina (1987) model which introduced rational expectations into the the partisan school. The theory has been supported in the literature: Alesina and Roubini (1990, 1992) find that German inflation, and (with qualifications) unemployment are significantly influenced by changes in government majorities. Here again Germany proves to be a better example of the relevant theory than the average country in the panel.

A remark has to be made concerning the specific index of political changes used in the literature up to now. As has been already observed by Neumann (1989) in his recount of Alesina's (1989) evidence on the rational partisan approach, the mere change of a minister within a given coalition government at the end of 1972 does not qualify as a

change in government from "left" to "right".[16] Unfortunately, following Alt (1985), an index containing the 1972 "non-event" was used in Alesina and Roubini (1990, 1992), and in Lang and Welzel (1992) as well. If, however, 1972 is excluded the relative stability of German democracy in the period under scrutiny limits the test cases for the rational partisan cycle even more. There have been just three changes in the composition of the federal government in the whole period 1950-89 that can be interpreted in the desired fashion (compare Alesina, Cohen and Roubini (1992, Table A.1) and Figure 1 above):

1966: Change to the "left", when the social democratic party joined the conservatives in government
1969: Change to the "left", when the conservative party left the government
1982: Change to the "right", when the conservative party took over the government again

The next steps are to see whether there is a rational partisan cycle in our more comprehensive database, with and without the 1972 "non-event". We start using the correct list of government changes, i.e. the one without 1972. We estimate the VAR models described above, but now with two dummies $D_t^{Right \to Left}$ and $D_t^{Left \to Right}$. $D_t^{Right \to Left}$ takes the value 1 after there is a change in the government from a right-wing to a left-wing majority. $D_t^{Left \to Right}$ covers the reverse case. Both are muliplied with a time dummy that allows for two possible lags after an election: *D1* is active 6 and *D2* is active 12 months after a change in the majority. This is the procedure used in he empirical literature mentioned above. Table 3 reports on our results.

(Table 3: Rational Partisan Cycles about here)

There seems to be a mildly significant coefficient in the case of NPI within one year after changes from left to right governments that has the expected negative sign. However, the result is not robust and vanishes completely if a trend is introduced into the model (column (2)). All in all there is almost no way to avoid rejecting the Alesina approach to political business cycle theory for the macro data under consideration.[17] Taking into account the whole of Section 3, our data gives very little support to the predictions of the partisan school in general.

4. Opportunistic Theories

4.1 The Nordhaus Hypothesis: Political or Opportunistic Cycles

In general, the Nordhaus hypothesis failed to convince researchers. Neither McCallum (1978) for the US, nor Paldam (1979) for a number of OECD countries could support it. The same is true for more recent studies (see Alesina and Roubini (1992)).

For the German case, however, results are mixed. Freyer (1980) and Schmidt (1983) argue against the impact of elections on both business cycles and policy instruments but do not present quantitative tests. On the other hand, Alesina and Roubini (1990, Tab. 9 ff. and A3 ff., 1992) provide such tests and cannot reject the Nordhaus hypothesis in terms of output (on a 5 per cent level) and on post-election inflation (only on a 10 per cent level). In both cases the majority of countries in the panel does provide no support for the theory. There is no report of a Nordhaus cycle in German unemployment in the literature.

To test for the Nordhaus hypothesis, we again fitted our VAR models to the series under consideration. In order to capture the possible impact of elections we included an electoral dummy. We used three different lags for the pre- and the post-election dummy covering the periods 18 months, 12 months, and 6 months (election month included). The Nordhaus (1975) hypothesis leads us to expect that the parameters for the pre-election dummies are significant and have a positive (negative) sign in the regression on prices and net production (unemployment) because of the efforts of incumbent politicians to get re-elected. The opposite should be true for post-election period.

(Table 4: Opportunistic Cycles in the Macreconomy about here)

As a matter of fact, the German macroeconomy shows some impact of federal elections. However, the results listed in Table 4 are not always the ones predicted by theory or reported by the literature mentioned above. Whereas the NPI is totally unaffected by the ballot dates, the inflation rate even declines significantly before elections. Unemployment (UE) is the one target variable where the coefficients show the expected signs and are significant on conventional levels. However, since neither the coefficient for the *D-2* nor

the one for the *D2* dummy is significant, both phenomena, the lowering of unemployment before and its rise after elections, seem to be limited to periods of more than one year before and after the actual election dates. If they are indeed the result of opportunistic political action, one wonders why the decline in the number of unemployed should fail to proceed right into the election year. All in all we find the evidence in favour of the Nordhaus approach not convincing.[18]

4.2. The Rogoff-Sibert Hypothesis: Rational Opportunistic Cycles

Monetary Policy

Why should the fiscal or monetary authorities react to elections if, in so doing, they can hardly influence the economy? If we assume, in the spririt of the Nordhaus approach, that they just "repeatedly tried", we would have to believe that the the government and/or the *Bundesbank* were less rational than the public which made the effort fail in the first place. As argued above, a theory can be found in Rogoff and Sibert (1988) and in Rogoff (1990), which explains why monetary authorities might try to manipulate their instruments despite the lack of convincing evidence of an impact on the real economy. Here it is argued that politicians will increase seigniorage income and/or the deficit before elections so they can produce a signal of competence to their voters.

The rational version of the opportunistic approach has found some support in the empirical literature. While Lang and Welzel (1992) do not find a rational opportunistic cycle in annual data for monetary aggregates, Alesina, Cohen, and Roubini (1992, Tab. 8) do find such regularities in the quarterly data for M1. According to the latter paper, the *Bundesbank* reacts to upcoming elections by an expanding M1. Given the traditional view of German monetary policy, the result is interesting.

To test the Rogoff-Sibert hypothesis for the discount rate and M1, we analyze the influence of the election dummies on the monetary policy variables in the same VAR model estimated in Section 4.1. Table 5 shows that monetary aggregates for the period 1951-89 are much closer to the idea of a political cycle than the macroeconomic data under review so far.

(Table 5: Rational Opportunistic Cycles, Monetary Policy about here)

There is no evidence for an impact of elections on the *Bundesbank's* discount rate between 1951 and 1989. But then the central bank might simply have avoided compensating for exogenous variations in monetary aggregates like M1 ahead of elections. As a matter of fact, for M1 the coefficients for the dummy variables *D-1* and *D2* are significant at least on the 10 per cent level in columns (1) and (2). Hence, we can conclude that on a fairly regular basis M1 expanded in the six months before elections and contracted in the twelve months afterwards. The impact of elections is small, however. The growth rate of M1 was only about 0.4 percentage points higher than usual before the election and about 0.2 percentage points lower afterwards.[19]

The evidence in Table 5 is robust and for once our results are in line with the empirical literature so far. The pre-election expansion and post-election contraction of M1 clearly looks like the one predicted by Rogoff and Sibert (1988). Or does it? There are arguments that prevent an easy interpretation along these lines. One argument might be that, since § 20 of the *Bundesbank* law more or less rules out a direct transfer of money from the central bank to the federal government, an increase in M1 does not directly increase the seigniorage to be included in the budget.[20] But the federal government is the only shareholder of the *Bundesbank* (§ 27), and increasing the money stock might also increase the profit the bank makes by lending it to the private sector. Note, however, that the possible interest flow resulting from the monetary expansion is only a fraction of the increase in the money stock.[21] Nevertheless, even an empirically small increase in the growth rate of M1 might be interpreted in the spirit of the model.

A second argument is harder to deny, however. Following our results for *D-1* above, the additional (indirect) seigniorage might be produced half a year before elections. For the Rogoff-Sibert idea to work, there should be enough time to effect transfer to the government prior to a typical election. In Germany, the lag between the time the seigniorage might accrue and the time it is transfered to the federal government is usually too long to help the government before elections because all elections between 1951 and

1989 were held between September and March and the *Bundesbank*'s profits, as a rule, are not transferred before spring of the following year.

But what about the significant *D-2* variable for the VAR model with a trend (column (2))? The above argument seems less strong here, since, with the seigniorage coming in 12 months ahead of elections, at least a quarter of the seigniorage will reach the federal budget in time. This leads us to the question as to how the transfer of seigniorage profits would actually happen in the German institutional setting. As a matter of fact, the *Bundesbank* profit is published before it is included in the budget. This procedure ultimately violates a necessary assumption in Rogoff and Sibert (1988). For a "signal of competence" by reducing taxes to make sense, they have to assume that voters have at least temporarily less information than the government about the way the budget is financed. Voters are not supposed to know that the tax cuts were financed by seigniorage. Since this is not the case, there is a second argument against a straightforward interpretation of our results in the light of the rational opportunistic cycle. All in all, we conclude that there must be other explanations for the election cycle in monetary aggregates than the one proposed by Rogoff and Sibert (1988).[22]

Fiscal Policy

Rogoff and Sibert (1988) and Rogoff (1990) implicate that governments will allow for a higher net deficit in election years as compared to non-election years. Alesina, Cohen, and Roubini (1992, Tab. 8) do find that the election dummies for the net deficit of the public sector have a positive sign but are not significant. However, these results might be due to the fact that there are more agents (social security, *Länder* governments etc.) in the German public sector than just the federal government which is the only one interested in federal elections. Looking at the influence of elections on the federal net deficit, we use the same model as in Section 4.1. Table 6 presents the results of this exercise.

(Table 6, Rational Opportunistic Cycles, Net Deficit about here)

The Rogoff-Sibert hypothesis leads us to expect an increase in the federal deficit prior to elections due to the tax cuts financed by seigniorage money. However, albeit most coefficients have the signs predicted by theory, none of them is significant. Hence we find no evidence of an impact of federal elections on the variable in question in the period 1951-89.[23]

5. Summary

One of the most surprising results of the revitalised empirical literature on the politcal economy of stabilisation policy is that there are signs of a political business cycle in Germany, despite its independent central bank and conservative fiscal behaviour (see e.g. Alesina and Roubini (1992) and Alesina, Cohen and Roubini (1992)). To see whether these results are robust, we use a more comprehensive data base (monthly data for the period 1950-89 for all but one variable) and standard methods of econometric time series analysis. In general, the results for our data do not support the findings in the literature, with the possible exception of the evaluation of the Rogoff-Sibert hypothesis for the monetary aggregate M1.

More specifically, (i) we find almost no support for the predictions of the partisan school, neither in its non-rational (Hibbs) nor rational expectations versions (Alesina). It appears that to reproduce the evidence reported by literature, in some cases the implications of non-stationarity have to be ignored. Also, (ii) there is no significant impact of federal elections on net production and prices as the Nordhaus hypothesis on opportunistic cycles predicts. Unemployment might qualify as an exception to the rule, but again the evidence is not totally convincing. (iii) There could be opportunistic cycles in some policy instruments. We do not find signs of pre-election activity in the Rogoff-Sibert fashion in neither fiscal policy nor the discount rate, but we do find them in the monetary aggregate M1. There are, however, arguments about the institutional setting in which German seigniorage income is channelled, which makes the Rogoff-Sibert approach an unlikely explanation for the phenomenon.

Notes

[1] For a more extensive introduction to the theory see e.g. Nordhaus (1989), Persson and Tabellini (1990), Paldam (1991a and 1991b), Alesina, Cohen and Roubini (1992), and Gärtner (1994).

[2] In general the exclusion of the 1950s does not change our empirical findings significantly.

[3] Our series have the following *Bundesbank* codes: CPI: UU0062, NPI: UU034, UE: UU0289, M1: TU0047. r is published monthly by Deutsche Bundesbank (January 1950 ff.).

[4] As the German economic council writes the surprisingly large net surplus of more than DM 12 billion in 1953 reported in the national accounts has to be adjusted for the transfer of DM 9.3 billion of federal debt to a sub-budget excluded in the national account (*Sachverständigenrat* (1965: 114)).

[5] Freyer (1980) uses budget figures. Just listing the numbers he finds no election-specific behaviour.

[6] However, as already shown in Berger and Woitek (1995), most of the VAR results can be reproduced in an simple AR setting as well.

[7] The maxiumum lag p is determined using standard time series analysis methods.

[8] Not taking care of the correct type of non-stationarity/seasonlity, one faces the problem of *spurious* or *artificial cycles*. Beginning with the seminal papers of Chan, Hayya and Ord (1977) and Nelson and Plosser (1982), there is a body of literature discussing this issue.

[9] The different types of (seasonal) integration indicate that we can neglect the problem of seasonal cointegration (see Hylleberg (1992)).

[10] For the same reasons it is impossible to apply standard conintegration/error-correction procedures. See Berger and Woitek (1995) for stationarity tests for the series under consideration.

[11] This brings the tested theory close to models where governments have partisan preferences but face opportunistic re-election constraints. See the survey by Frey and Schneider (1988).

[12] Our results in this Section are very much the same if we exclude the Grand Coalition from the "left" period. In order to analyse the effects of political changes later on we will, however, consider the end of the Grand Coalition a political change (see Section 3.3).

[13] For several reasons an adaption of the VAR models to all of the alternative tests applied in the paper is not possible. In these cases we use standard AR models of the series under consideration instead (cf. Berger and Woitek (1995)). All results to be mentioned are available on request.

[14] All results are available on request.

[15] The results were robust against a change from monthly data to quarterly averages.

[16] In June 1972 Karl Schiller, a social democrat and head of the "*Superministerium*", comprising the department of commerce and the more important treasury department, resigned because the coalition government comprised of social democrats and the small liberal party would not follow his anti-inflationary course. In November 1972 the coalition government was re-elected with gains for both parties. The new cabinet split the former "*Superministerium*". The department of commerce went to the liberals. The treasury department was kept by the social democrats. All in all, the event does not qualify as a major political change, especially not one from "left" to "right".

[17] To see whether the assumption of a change from "left" to "right" in 1972 has a impact on the outcome of our regressions, we re-run all tests listed in Table 3 with a list of changes including 1972. The results were not altered significantly. In addition, we found that our results are robust against a change from monthly data to quarterly averages.

[18] To see whether there is partisan behaviour behind our results for the period as a whole, we ran a set of univariate regressions for subperiods of "right" and "left" governments as determined by German political history (see Figure 1 in Section 3.1 above). For the variables under test, we find no systematic partisan influence in any subperiod. Again, we found our results robust against a change from monthly data to quarterly averages. The results for the political subperiods are robust against different treatments of the Grand Coalition. All results are available on request.

[19] We repeated the regressions with M2 and M3 and found a similar pattern. Adding the balance-of-payments surplus or dummies for changing exchange-rate regimes to the VAR model changes the significance level of the political dummies a little, but not fundamentally. Our results were robust against a change from monthly data to quarterly averages and did not change much in the partisan subperiods introduced above. Berger and Woitek (1996) reject the hypothesis that controling for the partisan majorities within the *Bundesbank's* council improves the performance of the model.

[20] The overall line of credit of the federal government is just DM 3 bill.

[21] The additional money stock is equal to the present value of the increase in interest payments, assuming that the bank earns more interest out of the additional money than it looses on the existing stock.

[22] The Bundesbank, for example, might want to signal its support for the government. It could also be the case that the central bank simply accommodates the expectations of the public that already anticipates opportunistic behaviour on the side of the bank to avoid a slump in real variables.

[23] There are indications, however, that there was significant opportunistic behaviour in the 1950-66 subperiod in an univariate approach, i.e. that especially the Adenauer/Erhard governments increased the full employment deficit before elections. Results are available on request.

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Table 1: Partisan Politics

	(1) Raw Series		(2) Trend	
	"Left" Majority	"Right" Majority	"Left" Majority	"Right" Majority
M1	-0.001	0.001	0.004	-0.003
Discount Rate	0.100**	-0.081**	0.162*	-0.180***
Federal Deficit	0.420	-0.365	0.302	0.055

Notes:

- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null-hypothesis oft the Q-test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)

Table 2:
Partisan Cycles in the Macroeconomy

	(1) Raw Series		(2) Trend	
	"Left" Majority	"Right" Majority	"Left" Majority	"Right" Majority
Net Production Index	0.001	-0.004	0.005	-0.008
Unemployment	0.003	0.005	0.016	0.010
Consumer Price Index	0.002***	-0.001**	0.002**	0.000

Notes:

- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null-hypothesis oft the Q-test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)
- Adding a dummy to the VAR model with trend (column (2)) to capture the impact of the oil price shocks (1974:1 to 1975:12 and 1978:1 to 1981:12) on our variables has the effect that the coefficient for "left" majorities becomes insignificant for the CPI (Detailed results are available on request)

Table 3:
Rational Partisan Cycles

		(1) Raw Series		(2) Trend	
		Left → Right	*Right → Left*	*Left → Right*	*Right → Left*
Net Production Index	D1	-0.005	0.000	-0.002	0.002
	D2	-0.009*	-0.001	-0.005	0.002
Consumer Price Index	D1	0.001	0.000	0.001	-0.001
	D2	0.000	0.000	0.000	-0.001
Unemployed	D1	0.012	0.007	0.016	0.003
	D2	0.011	0.024	0.012	0.023

- Notes:
- D1/D2 is 1 in the 6/12 months after a change from left to right (right to left) and 0 otherwise.
- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null hypothesis of the Q-Test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)

Table 4:
Opportunistic Cycles in the Macroeconomy

		(1) Raw Series	(2) Trend
Net Production Index	D-3	0.001	0.000
	D-2	0.001	0.000
	D-1	-0.004	-0.005
	D1	-0.004	-0.005
	D2	-0.002	-0.003
	D3	-0.002	-0.003
Consumer Price Index	D-3	-0.001*	-0.001**
	D-2	-0.001	-0.001*
	D-1	0.000	0.000
	D1	0.001	0.001
	D2	0.001	0.001
	D3	0.000	0.000
Unemployed	D-3	-0.021**	-0.021**
	D-2	-0.015	-0.014
	D-1	0.001	0.002
	D1	0.014	0.015
	D2	0.009	0.010
	D3	0.022**	0.023**

Notes:

- D-3/D-2/D-1 is 1 in the 18/12/6 months before an election and 0 otherwise; D1/D2/D3 is 1 in the 6/12/18 months after an election and 0 otherwise.
- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null-hypothesis oft the Q-test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)

Table 5:
Rational Oportunistic Cycles
Monetary Policy

		(1) Raw Series	(2) Trend
Discount Rate	D-3	-0.024	-0.027
	D-2	-0.020**	-0.024
	D-1	0.009	0.007
	D1	-0.051	-0.052
	D2	-0.012	-0.008
	D3	-0.031	-0.023
M1	D-3	0.001	0.001
	D-2	0.001	0.001**
	D-1	0.004**	0.004***
	D1	0.001	0.001
	D2	-0.002***	-0.003*
	D3	-0.002	-0.002

Notes:

- D-3/D-2/D-1 is 1 in the 18/12/6 months before an election and 0 otherwise; D1/D2/D3 is 1 in the 6/12/18 months after an election and 0 otherwise.
- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null-hypothesis oft the Q-test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)

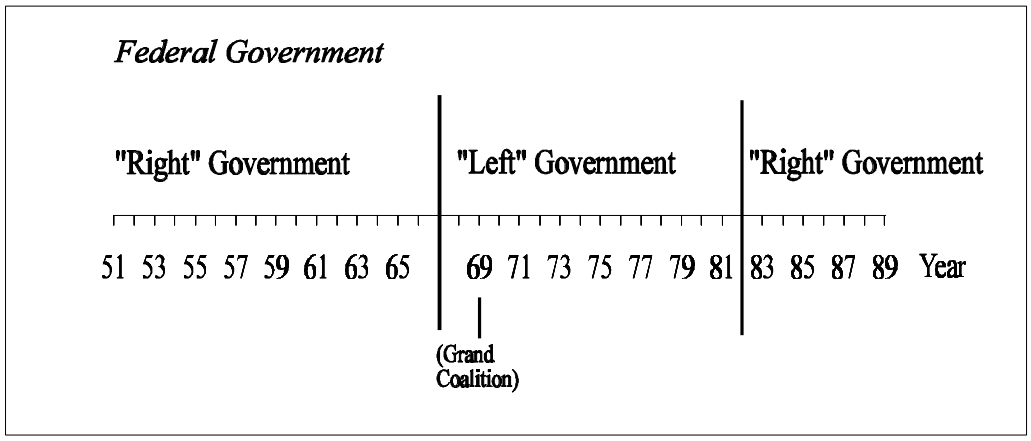
Table 6:
Rational Opportunistic Cycles
Net Deficit

	(1) Raw Series	(2) Trend
D-3	0.269	0.205
D-2	0.150	0.077
D-1	0.062	-0.036
D1	-0.290	-0.407
D2	-0.139	-0.255
D3	-0.010	-0.122

Notes:

- D-3/D-2/D-1 is 1 in the 18/12/6 months before an election and 0 otherwise; D1/D2/D3 is 1 in the 6/12/18 months after an election and 0 otherwise.
- Superscript `*´/`**´/`***´: Estimate is significant at the 10/5/1 per cent level.
- The null-hypothesis oft the Q-test ("The residuals are white noise") could not be rejected in any case. R^2_{adj} is about 0.7-0.9 (Detailed results are available on request)

Figure 1


Federal Government
"Right" Government
"Left" Government
"Right" Government
51 53 55 57 59 61 63 65
69 71 73 75 77 79 81
83 85 87 89
Year
(Grand Coalition)