

Educational Business Cycles

The Political Economy of Teacher Hiring across German States, 1992-2004

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Abstract³

Strong institutional constraints and better-informed voters may lead re-election seeking incumbents to *shift* the use of cycle mechanisms to those policy domains that are most easily manipulable, targetable, and timeable. We find evidence of cycling mechanisms in teacher employment decisions, notably electioneering and honeymooning. German state-level incumbents reverse their election-period increases in new teacher hiring during politically safer points in the electoral cycle. Cycles are mediated by political context. More heavily indebted states reduce the size of their teachers' pool, and a higher political salience of education strengthens the manipulation of teacher hiring for electoral purposes.

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1. Introduction

There is strong evidence that in democracies, incumbent politicians and parties benefit from favorable economic conditions (Drazen 2000; Hibbs 2006; Tufte 1978). Re-election seeking incumbents therefore naturally appear to have powerful incentives to try and gain votes by increasing the economic well-being of the electorate, or by signaling their ability to do so. Building on this logic, theoretical models of political-economic cycles in the tradition of Nordhaus (1975), McRae (1977), Hibbs (1977) and Tufte (1978) have specified how incumbents manipulate the use of monetary, fiscal and other policy instruments to reap electoral benefits. However, empirical evidence on political manipulation of public policies to create political business cycles is rather mixed and inconclusive.¹ Recently, researchers have made headway in making sense of existing empirical inconsistencies. Franzese and Jusko's (2006) thesis is particularly compelling as it is general yet sensitive to political-institutional context. Political-economic cycles, they argue, should *always* emerge. But, crucially, the degree, character, and effectiveness of these cycles is structured by the political and institutional conditions present in any particular case. Since institutional constraints and informed voters make it difficult for politicians in developed democracies to manipulate monetary and budget cycles, politicians may shift the use of cycle mechanisms towards policies that are easier to manipulate. Franzese and Jusko (2006) propose that incumbents will manipulate policies in proportion to their effectiveness in satisfying their electoral goals. Such a rule would

imply that electoral cycles are more prominent in direct delivery policies and that the degree and character of such manipulations would be context-conditional. Therefore, election-motivated incumbents will prefer policies that are more targetable and timeable to voters, and more manipulable by incumbents. Policies that might meet these criteria include public spending, direct benefit provision and public hiring and firing (Tufte 1978).

This article tests the context conditionality thesis in the case of hiring decisions of public school teachers in Germany. We argue that this policy domain eminently meets the criteria of being targetable, timeable, and palpable for voters, and can therefore be expected to be used for competence signaling electoral cycles. School education in Germany is regulated, financed and administered at the state level and receives much attention from the electorate, which makes it an important field for political party competition. Since voters can be assumed to have less knowledge about educational administration than incumbents, the latter can exploit their information advantage in this domain to create political-economic cycles. We find strong evidence for cycling mechanisms in teacher hiring, in the form of electioneering (more teachers hired before and during election years) and honeymoon effects (more teachers hired by new incumbents in power after elections). As we hypothesize, political context mediates cycling effects. More indebted states generally tend to reduce the total size of their teachers pool. Moreover, incumbents appear to reverse their election-period increases in new

teacher hiring during politically safer points in the election cycle. Lastly, we find that the higher political salience of education after the German PISA 2000 results further strengthens the use of this policy domain for electoral purposes.

The article proceeds as follows. The second section brings together the political-economic business cycles theory and the institutional context of German federalism in order to derive testable implications for public education. Section 3 reviews prior empirical studies on Germany. In the fourth section we present our new dataset and estimation strategy. The fifth section presents and interprets our empirical results. The last section concludes.

2. Political-economic cycles

2.1. Classic business cycle theory

Classic business cycle theory starts from the assumption that politicians primarily care about holding office and therefore choose specific policies in order to maximize their chances of re-election. The literature presents two alternative accounts on this idea: the electoral cycle first put forward by Nordhaus (1975) and McRae (1977), and the partisan cycle originally proposed by Hibbs (1977). If voters evaluate candidates on their recent performance, the incentives for the latter to manipulate public policy increase as elections approach. *Electoral cycle* theory predicts that incumbents use expansionary monetary policy to improve their economic performance before elections. In the absence of rational expectations, this would help

to increase real economic activity and therefore re-election chances (Nordhaus 1975; Berger and Woitek 1997). Drazen (2000) extends this logic formally to fiscal policies. Alternatively, voters may evaluate candidates primarily on the basis of ideological preferences. *Partisan cycle* theory assumes that politicians are not opportunistic but decide according to their political preferences. In this case, parties manipulate economic policy to benefit specific electoral groups. The model predicts that rightwing governments spend more on public administration and infrastructure, while leftwing governments spend more on social security, health care and public education (Hibbs 1977; Boix 1997).

A further complication arises from voter's time horizons and their assumed degree of rationality. The literature on political-economic cycles divides into two perspectives on this issue: prospective and retrospective voters. The prospective view assumes that only the expected future relative performance of candidates' matters. Under pure retrospective voting, elections are referenda on the governing party's past performance, whereby voters reward good performance and punish bad performance. The empirical literature on voting and popularity functions supports this behavioral assumption (Lewis-Beck and Paldam 2000). Moreover, it shows that models with retrospective voters have a better empirical fit than models with prospective voters (Nannestad and Paldam 1994). If the economy goes well, so will the popularity of the government. Thus, we assume that retrospective voters observe

public sector provision, judge incumbent performance, and allocate their votes accordingly.

Following this perspective, politicians can exploit informational advantages in order to signal their competence through pre-election stimulation. Taking into account contextual determinants, this sort of competence signaling electoral cycle is likely to disappear as soon as there are no information asymmetries between voters and incumbents (on which see Przeworski et al. 1999). Thus, the quality of information available to voters relative to incumbents structures the incidence and nature of cycles. For instance, Shi and Svensson (2006) show that the effect of elections on fiscal policy differs between developing and developed countries. They find that political-economic cycles tend to be smaller and less robust in Western democracies, where, they argue, stronger institutional constraints on budget decisions and better-informed voters prevent politicians from manipulating the economy. Alt and Lassen (2006) make a similar argument regarding the effect of fiscal transparency on debt cycles.

2.2. Context conditionality: the case of German federalism

One of the distinct features of German federalism, which sets it apart from other systems, is the institutionalized interlocking among different levels of government (Scharpf 2005). The federal structure is reflected by three levels; federal (Bund), state (Laender) and local (Gemeinden). The allocation of competences among the

Bund and the Laender follows the principle of subsidiarity (Articles 30, 70 and 83 of the Basic Law) but gave the federal government leeway to become active in legislation (Benz 1999: 62). The tendency toward centralization became evident early after the passing of the Federal constitution in 1949 through policies to establish a uniform legal and economic order (Hesse 1962). Within the subsequent five decades, federal governments managed to exploit granted competencies and extended them through a number of constitutional adjustments (Benz 1999: 62). Laender governments participated in and benefited from centralization through fiscal cooperation with the federal government. Today, German federalism is characterized by a relatively strong federal government that possesses legislative power in all major policy areas, whereas the Laender are, in most cases, responsible for implementing the law (Benz 1999: 55). The extensive sharing of administrative and financial functions between the federal and state level and a strong political orientation toward unity of living conditions in all regions (Katzenstein 1987) have contributed to the emergence of a system of interlocking politics (Benz 1999: 56), or more problematically, “joint decision traps” (Scharpf 2005).

Public education and cultural affairs, however, have always been an exception from the general tendency toward centralization. This is mainly due to the fact that in 1949, when the Basic Law was drafted, the majority of states had already passed laws to administer and govern public education and cultural affairs on their own (Klein 2006). To this day, legislation, financing and administrative

competencies in public school education and cultural affairs are exclusively set at the state level (Article 7 and 30 of the Basic Law). Curricula, funding and teacher employment are directly set through the State Ministries of Education. The Conference of State Ministers of Education and Culture (Kultusministerkonferenz), already founded in 1948, passes regulatory recommendations for the uniformity treatment of school policies. However, its decisions have no binding character for the Laender, nor are there any possibilities to sanction deviant behavior or poor performance. In other words, public education is a domain where the idea of federalism is particularly pronounced and where the independence of the Laender the highest. The conspicuousness of public education and cultural affairs in the institutional framework of German federalism continues to develop against the general tendency of centralization.

In recent years, the political salience of education policy has significantly increased in German public debate. In the so-called “PISA shock” of 2000, German 15-year-olds made a relatively poor showing in literacy, mathematics and science on the internationally comparable Program for International Student Assessment (PISA) tests. They performed well below the OECD average and were outflanked by neighboring countries and by countries such as Finland, Canada and New Zealand for literacy, and South-Korea for math skills (Allmendinger and Leibfried 2003). These results struck a nerve among politicians and the general public alike and boosted public interest in German education policy. As one reporter wrote: “The

competences of German children, their knowledge, their schooling, are treated as if the [German] national football team had messed up everything again” (FAZ 2001). As there are substantial differences between the Laender, particularly with respect to natural sciences, the decentralization of school education policies has been blamed for German pupils' poor PISA results (Allmendinger and Leibfried 2003; PISA-Konsortium Deutschland 2005). Nevertheless, the latest constitutional amendment, the so-called "Foederalismusreform I" in 2006, consolidates and strengthens the exclusive competences of the Laender in public school education (Stettes 2007: 127).² An earlier attempt to reach a reform compromise between the Bund and the Laender in 2004 had broken down over the federal government's attempt to increase its influence in public education. Faced with heavy political resistance from the Minister Presidents of the Laender, irrespective of their political party membership, the federal government eventually retreated its claims (Scharpf 2005).

2.3. Hypotheses

The lack of room for maneuver in other policy fields may turn public education policy into a particularly important arena for political party competition at the state level in Germany. The constitutional and institutional context of German federalism may provide state-level incumbents with both the “opportunity” and the “weapon” for exploiting information asymmetries to gain votes (Tufte 1979, Franzese and Jusko 2006). We focus on one dimension of education policy – teacher employment

policies. Applying a simple vote and popularity function framework we would initially expect voters to employ this variable as a measure of incumbent performance, rewarding new teacher employments and punishing reductions of the teacher pool. The high salience in public debates of education provides some prima facie circumstantial evidence for the political importance of this domain. Given their autonomy in this policy domain, state-level incumbents may use teacher employment policy for cycling purposes. Since retirement and maternity leave may overcompensate increases in new engagements, we employ two alternative dependent variables: employment figures for *new* teachers measured in head-counts and *total* teacher employment units measured in full-time equivalents. The prediction of electoral cycle theory that politicians manipulate the public in election years in order to get re-elected is captured by (H1) and (H2). The predictions of partisan cycle theory are captured by (H3) and (H4).

- (H1) Employment figures for new public school teachers are higher in election years.
- (H2) Election years have no effect on change in total teacher employment units.
- (H3) Employment figures for new public school teachers are higher with left wing governments.
- (H4) Left wing governments have no effect on change in total teacher employment units.

3. Literature Review

In what follows, we restrict our focus on the empirical literature testing political-economic cycles employing datasets on Germany. Using data at the federal level, Berger and Woitek (1997) find no evidence for monetary business cycles. They detect that results are very sensitive to the quality of the data and the estimation methodology. Seitz (2000) investigates if politics matters for the budget allocation on the state level, but finds no evidence for the impact of government ideology on public spending decisions. Jochimsen and Nuscheler (2006) analyze budget deficits on the state level for the period 1960 to 2005 using a dynamic panel data estimator. Although they find no evidence of partisan cycles, their results indicate that public deficits decrease in pre-election years. Galli and Rossi (2002) test opportunistic and partisan cycles for eleven western German states with respect to seven different budget categories – aggregate expenditure for education is one among them. Their results show no support for partisan cycles and weak evidence for opportunistic cycles in budget allocations. However, with respect to education expenditure they find that none of the theories is supported by their estimation results. Potrafke (2006) applies a SURE panel framework on six different expenditure categories and finds weak evidence that politicians increase expenditures for “schooling” in election years. Moreover, he finds that government ideology has no effect on none of the expenditure categories. Oberndorfer and Steiner (2006) analyze the effects of demographic change and partisan politics on higher education spending. Using panel

data for West German Laender, they find contrasting evidence for the classical partisan theory: governments under conservative parties or coalitions between social democrats and conservatives spend more, not less, on public higher education than governments run by social democrats alone.

As this literature review shows, earlier empirical work on Germany has primarily investigated monetary business cycles, while more recent publications focus on budget cycles. The ambiguity of empirical findings may appear to indicate that German politicians have little scope to manipulate monetary policies or fiscal budget decisions. This paper contributes to the literature by shifting the attention toward concrete employment policies. It differs in a very fundamental point from prior empirical research: in the dependent variable. By studying education policy, we focus on a policy domain in which there may be more room for political manipulation left due to the particular constitutional context of German federalism. We argue that the particular allocation of legal competencies among the federal and state levels makes public education a fruitful case for the analysis of competency signaling electoral cycles.

4. Data and estimation strategy

We employ balanced panel data that comprises annual data for the 16 German states from 1992 to 2004. Since the unification in 1990, Germany consists of sixteen states; ten states from former West Germany (Schleswig-Holstein, Hamburg, Lower

Saxony, Bremen, North Rhine-Westfalia, Hesse, Rhineland-Platinete, Baden-Wuerttemberg, Bavaria, Saarland) and five states from former East Germany (Brandenburg, Mecklenburg-Westpomeria, Saxony, Saxony-Anhalt and Thuringia). Berlin was divided into East and West Berlin during the Cold War and is now the 16th state. Employment figures for new teacher employments are taken from the statistical office of the Conference of State Ministers of Education and Culture (Kultusministerkonferenz 2005). New teacher employments are measured as head counts for new full-time or part-time contracts. Total teacher employment is measured in terms of full-time equivalent units. Teacher employment units proxy the overall provision of public school teachers. It is important to note that our two dependent variables are measured with different scales. For example, one new teacher employment represents one person, while one teacher employment unit can consist of one full-time teacher or two or more part-time teachers. Data on teacher employment units and the number of pupils in public schools is taken from the German School Statistical Survey (Bundesamt 2005), compiled by the Federal Statistical Office. Macroeconomic control variables such as tax revenues and debt are taken from the Federal Statistical Office Working Group on Macroeconomic Accounting (Bundesamt 2006) and the Federal Statistical Office. A detailed definition and description of the variables is given in Appendix Table 1.

Data for the two independent variables of main interest, electoral timing and government ideology, are computed from the statistics of the Federal Electoral

Supervisor (Bundeswahlleiter 2005). Following Franzese (2000), *Pre-Election Year*, *Election Year* and *Post-Election Year* take into account the exact timing of the elections. They are defined as follows:

$$Election\ Year_{i,t} = [(M-1)+d/D]/12$$

$$Pre-Election\ Year_{i,t} = [12-(M-1)-d/D]/12$$

$$Post\ -Election\ Year_{i,t} = [12+(M-1)+d/D]/12$$

where M accounts for the month of the election, d is the day of the election and D represents the number of days in that month. In all other years the variables are set to zero (Potrafke 2006). Partisan ideology in government is captured by a *SPD Absolute Majority* (resp. *CDU/CSU Absolute Majority*) dummy, which equals one if the social democratic (resp. conservative) party has an absolute state-level majority, and 0 otherwise. Compared to coalitions with minor partners, and even more so grand coalitions, absolute majority governments offer the ideal platform for the dominant party on either side of the ideological spectrum to pursue its most favored policies. In other words, we err on the conservative side in testing the effect of partisanship: if partisanship is not found to affect our dependent variables even in a setting of absolute majorities, the partisanship thesis can be rejected with higher confidence. During the observation period the Minister President of the state has been a SPD or CDU/CSU politician. Thus, government change is defined as change in the Head of

the State as a consequence of elections. The variable *New Government* equals *Election Year* if the Head of the State changed from left to right or vice versa, and 0 if a sitting government was re-elected. Summary statistics for the political variables are given in Appendix Tables 1 and 2.

Following Fernandez and Rogerson's (1997) and Falch and Rattso's (1997) studies of public school expenditure, we use a log-transformed specification to examine determinates of new public school teacher employment. In particular, variants of the following regression model will be estimated:

$$\begin{aligned} \log(\text{New Teacher Employments}_{i,t}) = & \beta_0 + \beta_1 \log(\text{Teacher Employment Units}_{i,t}) \\ & + \beta_2 \log(\text{Pupils}_{i,t}) + \beta_3 \log(\text{Tax Revenues}_{i,t}) + \beta_4 \log(\text{Debt}_{i,t}) \\ & + \beta_5 \log(\text{SPD Absolute Majority}_{i,t}) + \beta_6 \log(\text{CDU/CSU Absolute Majority}_{i,t}) \\ & + \beta_7 \log(\text{Election Year}_{i,t}) + \varepsilon_{i,t} \end{aligned}$$

where *New Teachers Employments* denotes the log of new teacher employments in state *i* at time *t* measured as head counts. Total *Teacher Employment Units* accounts for the overall level of teacher employment measured in full-time equivalents. This variable controls for the effect that new teacher employments should depend on the level of current teacher employment. *Pupils* accounts for the opposite effect. In order to maintain a certain pupils/teachers ratio, a rise in the number of pupils is predicted to increase new teacher employments. In addition, budgetary constraints are likely to

influence employment decisions. We therefore include *Tax Revenues* (expected to have a positive effect on teacher employment) and *Debt* (expected to have the opposite effect). A second type of model investigates change in *Total Teacher Employment Units*.

$$\begin{aligned} \Delta \log(\text{Teacher Employment Units}_{i,t}) = & \beta_0 + \beta_1 \log(\text{Pupils}_{i,t}) + \beta_2 \log(\text{Tax Revenues}_{i,t}) \\ & + \beta_3 \log(\text{Debt}_{i,t}) + \beta_4 \log(\text{SPD Absolute Majority}_{i,t}) \\ & + \beta_5 \log(\text{CDU/CSU Absolute Majority}_{i,t}) + \beta_6 \log(\text{Election Year}_{i,t}) + \varepsilon_{i,t} \end{aligned}$$

Both specifications are completed by a two-way error component, accounting for time and state fixed effects. Coefficients are estimated by OLS and the standard errors are heteroskedasticity-corrected.³ First, we test for unit roots of the dependent variables in order to avoid spurious regressions as a result of nonstationarity. Appendix Table 4 presents the results for panel unit root using an augmented Dickey-Fuller test (Maddala and Wu 1999). According to the test statistic, *New Teacher Employments* appears to be stationary in contrast to total *Teacher Employment Units*. There are two approaches to deal with non-stationarity data; finding cointegrating relationships or to shift to first differences (Kittel 2005). Since panel cointegration is rather difficult to apply and a recent topic of econometric research we use the first difference of teacher employment units. Second, since it can be argued that there is little room for randomness in a sample that includes all 16

German states, we consider a fixed effects model rather than a random effects model. Although random effects could a priori be rejected, the Hausman (1978) test for random country effects ($\chi^2(7)=36.22^{***}$) gives additional evidence that the fixed effects specification is preferable. Hence the random effects solution can be rejected on substantive and on statistical grounds.

5. Empirical Analysis

To explore some initial relationships, Figure 1 present time series for the relationship between *New Teacher Employments* (log-transformed values) and electoral timing. In the majority of cases there appears to be a positive covariance between electoral timing and *New Teacher Employments*.

Figure 1

Tables 1 and 2 compare average annual growth rates for new teachers and total teacher employment units for, respectively, election vs. non-election years, and new vs. non-new government. Columns 1 and 2 show that the average annual growth rate for new teachers is more than three times higher in election years than in non-election years, and more than four times higher if elections carried to power a new Minister President, from a different main party. The descriptive analysis is equally revealing as regards total teacher employment units (columns 3 and 4). The same

period that saw growth of new teacher hiring was actually marked by *reductions* in the total teacher pool. This was the case in all years, but by far most strongly so in non-election years or when elections re-installed to power Minister Presidents from the same party as before. These observations give *prima facie* reasons for searching for a causal relationship between elections and the asymmetric use of teacher hiring for electoral gain.

Tables 1 and 2

5.1 New teacher hiring

Our baseline regression results on new teacher hiring are given in Table 3. The F-tests presented in the lower block of the table confirms that both time and country fixed effects should be included. Since panel data typically exhibits serial correlation and groupwise heteroskedasticity (Greene 2000: 592-608), we also perform a Modified Wald test for groupwise heteroskedasticity (Greene 2000: 598) and a Lagrange-multiplier test for first-order residual serial correlation in panel data (Baltagi 2002: 95). We assume that the observed autocorrelation of the residuals indicates persistency of the dependent variable and therefore include a lagged dependent variable. Since the test for groupwise heteroskedasticity is highly significant it seems reasonable to follow the recommendation by Beck and Katz

(1995: 638) to apply panel corrected standard errors. Nevertheless, the difference between the OLS standard errors and the PCSE is minor.

Models 1, 2, 4 and 5 in Table 3 show that while the signs of the estimated coefficients for *SPD Absolute Majority* and *CDU/CSU Absolute Majority* are in line with the partisan cycle hypothesis that left parties increase teacher supply more than right parties, both coefficients are statistically insignificant. This is consistent with previous studies which found a waning influence since the 1980s of partisanship on total education expenditures (Busemeyer 2007). By contrast, the estimated coefficient for *Election Year* is statistically significant and positive across all models ($\beta = 0.36$). This supports the basic electoral cycle hypothesis (H1). In other words, these baseline results indicate that new teacher appointments are subject to electioneering but not partisanship by incumbents (H3).

Table 3

5.2 Total teacher employment units

Table 4 contains the estimates with respect to the dependent variable ‘change in total *Teacher Employment Units*’. Although the Lagrange-multiplier test does not indicate serial correlation (Baltagi 2002: 95) we include a lagged dependent variable, since employment decisions are made with reference to the previous year. Excluding the lagged dependent variable does not alter the substantial findings for partisanship and

electioneering (Model 1 and 2). With one exception, the coefficients for *SPD Absolute Majority* and *CDU/CSU Absolute Majority* are again statistically insignificant. Interestingly, the same is true now for *Election Year*. This lends support to (H2) and (H4). The baseline regression results in Tables 3 and 4 thus indicate that election years lead to more new teachers being hired, but do not significantly affect the size of the total teacher pool. Note, furthermore, that higher levels of public debt are significantly associated with reductions in the total teacher pool (Table 4), but do not seem to affect new teacher hiring (Table 3). How can we explain this, without watering down the assumption that incumbents face hard budget constraints over the course of the electoral cycle, as the recent literature points out (Shi and Svensson 2006), and as was empirically the case during the post-Maastricht period studied here? At least two political-economic cycle mechanisms –one during and one outside election years- could make sense of these observations. Assume that most young people qualified as teachers want jobs and a significant number of older teachers want to leave the labor force through early retirement. Incumbents could then reap double electoral gains through cycling and political patronage strategies. First, *during* election years they could step up one popular strategy by further accelerating the growth of new teacher hiring (Table 1, column 2). In addition, they could use newly hired teachers to replace old teachers willing to retire early; another (self-selective) popular mechanism. Recall that our data measure new teachers as persons (either new full-time or new part-time

contracts) and total teacher employment as full-time equivalent units. So political patronage could be rendered both more cost-efficient and more electorally rewarding if incumbents created more *part-time* jobs for younger teachers to replace a given number of (full-time) older teachers in election years. This would attract votes from teachers at both extremes of the career cycle, while possibly leaving figures for total teacher employment units unchanged, or even reducing them. Second, *outside* of election years incumbents could further restore fiscal conservatism over the entire cycle, by actually cutting the number of full-time equivalent teacher employment units in order to compensate for their election year extravagance at an electorally safe moment in the cycle (Table 1, column 4). A similar compensation mechanism over the electoral cycle may also be at work with respect to new teacher hiring (see below).

Table 4

5.3 Robustness analysis

We test the robustness of our baseline findings on *New Teacher Employments* by generating a one-year-forward and a one-year-lag variable for election year in Table 5. The coefficients for both the forward (Model 1) and the lagged (Model 3) specification are statistically significant and positive. The effect size is smaller than for the exact *Election Year* (Model 2), and larger in the first case (when elections

were soon pending) than in the second (when elections had recently been held). In line with (H1), the electioneering effect is thus at work most strongly in those years when elections were held, although the still sizable effect of *Pre Election Year* indicates a steadily growing "campaign fever" among incumbents. The smaller positive effect for *Post Election Year* in turn lends some support to Franzese & Jusko's (2006) point that election winners can, and do tend to, fulfill their promises, possibly because elections are filters of 'credibly promised largesse.' The empirical pools of pre-electoral candidates contain some candidates who made non-credible promises (and therefore lost) and some incumbents who delivered too little (and therefore lost). In contrast, post-electoral pools contain only winners (either returning incumbents or entering challengers) who have struck a better pre-electoral balance of largesse and credibility, and now need to deliver in order to cement their reputation. To further explore the behavior of post-electoral incumbent pools, Model 4 introduces the variable *New Government* to explore one subset of election winners – newly elected incumbents. The coefficient is positive and statistically significant ($\beta = 0.49$) and the effect size is relatively large. So it is especially newly elected, rather than returning incumbents, who tend to hire new teachers after coming to power. This suggests a honeymoon effect, possibly resulting from the fact that newly elected governments need to act especially quickly to assure voters of the credibility of their pre-electoral promises.

Models 5, 6 and 7 represent the next step in our robustness analysis by testing for the hypothesis that teacher hiring has gained importance as a tool for cycling once the 'PISA shock' in 2000 had raised the political salience of education in German public debate. Model 5 therefore introduces the variable *Post PISA*, which shows a significant and positive effect on new teacher hiring after the year 2000, above and beyond the electioneering effect. Models 6 and 7 are re-estimations of our baseline Model 2 on a split sample. The first sample covers the period 1992-1999, the second sample the years 2000 to 2004. Again confirming our expectations about the increased use of electioneering in teacher hiring since 2000, the *Election Year* coefficient is stronger in the second period by 0.19 percentage points. Again, party ideology does not affect new teacher hiring in any model.

Finally we test the effect of non-election period years on new teacher hiring in Model 8. *Non-Election Period Years* is here defined as a simple dummy variable, which equals zero both in the years before and after an election, and in the election year itself, and one in all other years. *Non-Election Period Years* has a statistically significant, *negative*, and relatively large effect on *New Teacher Employments* ($\beta = -0.13$).⁴ In other words, outside of election periods, incumbents could further restore fiscal conservatism over the entire cycle, by compensating for their election-period excesses (increased new teacher hiring) at a politically safer moment in the electoral cycle (Table 2).⁵

Table 5

5. Conclusions and discussion

This paper aims to contribute to the literature on political-economic cycles in three dimensions. First, it provides a new empirical application of the theory to the case of teacher employment decisions in Germany. Second, we have put in context recent claims by Shi and Svensson (2006), Alt and Lassen (2006) and Keefer (2007) that practices such as cycling and clientelism are prevalent mainly in younger and/or less consolidated democracies. Our study shows considerable evidence pointing both to *electioneering* (but not *partisanship*) by all incumbent governments, and to *honeymooning* by new incumbents at the state level in this well-established, large federal democracy in Europe. Not only is the effect of electoral timing on new teacher employment robust. Our analysis also pointed to potentially significant budget-balancing (or deficit-reducing) mechanisms over the entire election cycle, in that increases in new teacher hiring during election periods might be compensated by reductions in both new teacher hiring and in the total teacher force outside of these periods. The fact that new teacher appointments are subject to political-economic cycles is in line with the thesis that election-motivated incumbents may select the particular policy domain for manipulation according to criteria of targetability and timeability (Tufte 1978; Franzese and Jusko 2006). Our finding that higher issue

salience leads to a more pronounced use of electioneering further strengthens the thesis that the use of cycles is mediated by the political context of the case at hand.

Our finding of electioneering in teacher hiring is also consistent with much empirical evidence indicating politicians' strong preferences for targetable spending and clientelist policies whenever the institutional context allows (Keefer 2007; Keefer and Khemani 2005; Shepsle and Weingast 1981).⁶ For instance, a disproportionately large share of public spending within education budgets tends to go to pay for teaching which teachers tend to value highly - primarily wages and low teaching loads - even though it has been shown that the effectiveness of these inputs is generally lower than the effectiveness of inputs such as textbooks, classroom equipment, writing materials, libraries, and software (Hanushek 1996; 2003; Hoxby 1996). At current levels of spending, teacher-favored inputs have been estimated to be less cost-effective than other education inputs, often by stunning margins of 1-to-5 or 1-to-10 (Pritchett and Filmer, 1998; Pritchett 2004).

Lastly, our finding of the extensive electioneering and honeymooning effects in teacher hiring at the state level sheds new light on recent developments in German federalism. Thus the first attempt to reach a reform compromise between the federal and state levels broke down in 2004 over the federal government's attempt to increase its influence in public education. Discussing this breakdown, Scharpf (2005: 14) claims that "the decision to let the whole reform effort fail was not entirely based on rational calculations, but was emotionally conditioned by disappointment and

frustration." Our study suggests the opposite. The seemingly "irrational" resistance from Minister Presidents of the Laender can be reinterpreted as a rational attempt by vote-seeking politicians to protect their autonomy over a policy domain that can be usefully manipulated for electoral purposes.

ENDNOTES

- ¹ For general reviews, see Alesina & Roubini (1992), Blais, Blake & Dion (1996), Cusack (1999), Clark (2003), Drazen (2000), Hibbs (2006), and Franzese & Jusko (2006). For a review of the business cycle literature on Germany, see section 3.
- ² The federal government retreats from financing the building of universities and public schools. Its remaining competence lays in the definition of admission requirements for university studies.
- ³ Recent studies on political budget cycles use dynamic panel data models to account for non-stationarity in expenditure variables. Shi & Svensson (2006) use the GMM estimator developed by Arellano and Bond (1991), while Jochimsen & Nuscheler (2006) rely on the bias corrected LSDV estimator developed by Kiewit (1995). The use of these estimators in political economy datasets (relatively small T and N) is little known and raises additional methodological problems (Kittel & Winner 2005).
- ⁴ For comparison, the estimated coefficients (not shown) of the other electoral timing variables when these are also defined as simple dummy variables, were respectively *Pre-Election Year* = 0.04 (0.07); *Election Year* = 0.20*** (0.07); *Post-Election Year* = -0.07 (0.08) (based on Models 1, 2 and 3 in Table 3, robust standard errors in parenthesis, *** p<0.01, ** p<0.05, * p<0.1).
- ⁵ We also performed a Jackknife analysis on Model 2. Results are presented in Appendix Table 4. Estimated coefficients for *Election Year* range from 0.25 to 0.37

omitting one cross section at a time, indicating that the results for *Election Year* are not driven by outliers.

⁶ For instance, in developing democracies, electoral accountability leads politicians to allocate more resources to disadvantaged groups, but mainly in the form of government jobs and targeted transfers, rather than broad-based social services (Pande 2003). In the case of education, the attractiveness to incumbents of new job creation is further enhanced by the fact that teachers as a group tend to be highly unionized and to record much higher-than-average voting turnout rates (Moe 2006).

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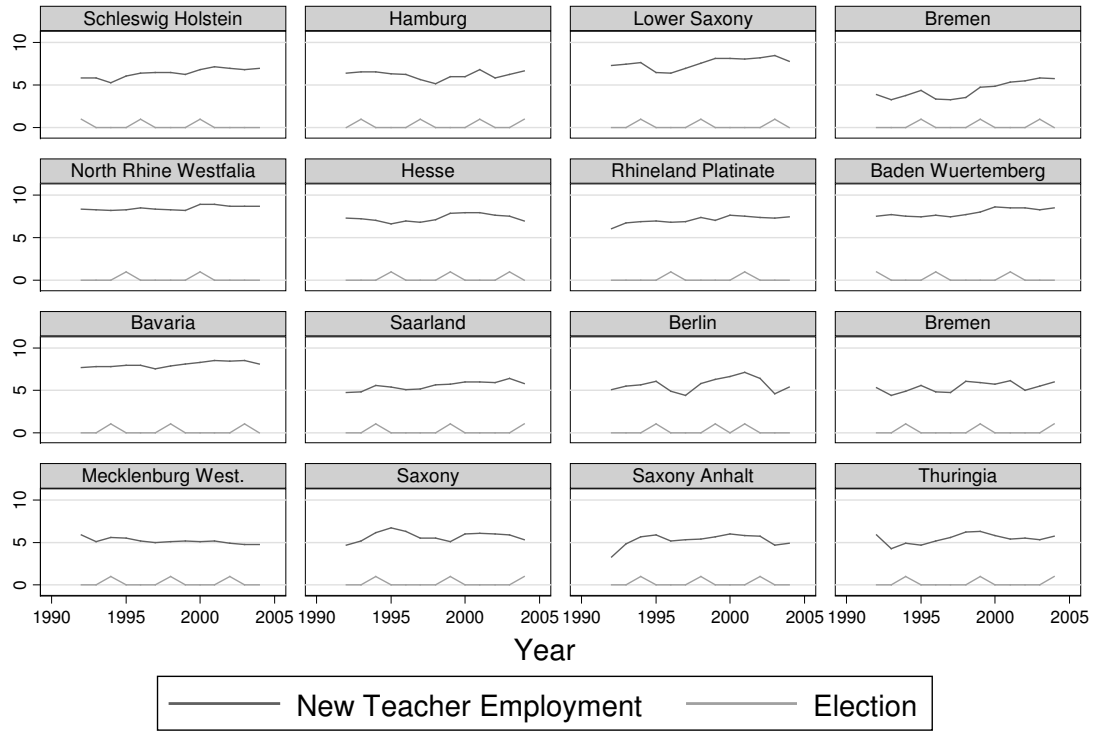
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Figure 1. New teacher employment (log) and electoral timing (1992-2004)



Graphs by id

Note: Election is dummy coded and equals 1 in election years.

Table 1. Descriptive analysis: Average annual growth rate of *New teacher employments* and *Total teacher employment units* in election years and non election years.

State	New teacher employments		Total teacher employment units	
	Election years	Non election years	Election years	Non election years
Schleswig-Holstein	55.53	10.24	0.91	0.49
Hamburg	38.13	5.84	0.43	0.42
Lower-Saxony	38.25	8.13	1.51	0.31
Bremen	126.43	6.77	-0.75	-0.73
North Rhine-Westfalia	59.51	-3.55	0.9	0.6
Hesse	19.34	-0.59	1.88	0.43
Rhineland-Palatinate	-11.51	23.2	1.52	1.55
Baden-Wuerttemberg	2.44	13.32	1.33	1.07
Bavaria	14.75	2.65	1.24	0.83
Saarland	28.44	12.59	-0.41	0.08
Berlin	61.85	29.02	-0.5	-1.44
Brandenburg	35.33	32.12	-1.86	-2.34
Mecklenburg-West.	10.12	-10.53	-2.54	-2.35
Saxony	31.53	19.41	-1.17	-1.83
Saxony-Anhalt	37.32	44.24	-0.98	-2.07
Thuringia	51.63	3.03	-0.75	-1.99
Average	38.43	12.25	-0.04	-0.4

Note: Election is dummy coded and equals 1 in election years.

Table 2. Descriptive analysis: Average annual growth rate of *New teacher employments* and *Total teacher employment units* in years in which a new government was (not) elected.

State	New teacher employments		Total teacher employment units	
	New government	Non new government	New government	Non new government
Hamburg	135.19	5.83	0	0.46
Lower Saxony	31.38	14.23	3.59	0.34
Hesse	108.3	-5.05	2.09	0.67
Saarland	12.31	16.94	-0.26	-0.03
Berlin	66.53	34.56	0.44	-1.36
Mecklenburg-West.	3.92	-6.21	-4.46	-2.21
Saxony-Anhalt	54.83	40.05	-0.83	-1.99
Average	58.41	14.33	-0.03	-0.59

Note: Only states in which the government has changed between 1992 and 2004 once or more. New government is dummy coded and equals 1 if the Head of State has changed from left to right or vice versa as a consequence of elections.

Table 3. Regression analysis: Determinants of *New Teacher Employments*

	Model 1 FE(CT)	Model 2 FE(CT)	Model 3 FE(CT)	Model 4 FE(CT)	Model 5 PCSE
New teacher employments (lag)		0.38*** [0.07]	0.38*** [0.07]	0.38*** [0.07]	0.38*** [0.09]
Teacher employment units	-1.7 [1.5]	-2.14 [1.5]	-2.09 [1.4]	-2.16 [1.6]	-2.14 [1.5]
Pupils	1.85** [0.9]	1.88** [0.8]	1.87** [0.8]	1.82** [0.8]	1.88*** [0.7]
Tax revenues	0.68 [0.5]	0.25 [0.5]	0.22 [0.5]	0.29 [0.5]	0.25 [0.4]
Debt	-0.39 [0.3]	-0.15 [0.4]	-0.14 [0.3]	-0.2 [0.4]	-0.15 [0.3]
Election year	0.36** [0.2]	0.36*** [0.1]	0.35*** [0.1]		0.36*** [0.1]
SPD absolute majority	0.016 [0.1]	0.066 [0.1]		0.029 [0.1]	0.066 [0.1]
CDU/CSU absolute majority	-0.23 [0.3]	0.031 [0.2]		0.0049 [0.2]	0.031 [0.2]
Observations	208	192	192	192	192
Number of id	16	16	16	16	16
R-squared	0.43	0.54	0.54	0.52	0.93
F(Time effects)	11.04***	7.04***	7.72***	6.69***	
F(Country effects)	4.18***	2.96***	3.31***	2.27***	
Mod. Wald (GH), chi2(16)	378.96***	88.04***	87.86***	79.15***	
LM(AR1), chi2(1)	37.33***	1.78	1.83	1.6	

Note: FE(CT) = fixed country & time effects, PCSE = Panel corrected standard errors, robust standard errors in brackets, F(Time effects) = F-test for the inclusion of year dummies, F(Country effects) = F-test for the inclusion of country dummies, Mod. Wald (GH) = Modified Wald test for groupwise heteroskedasticity (Greene 2000: 598), LM(AR1) = Lagrange-multiplier test for first-order residual serial correlation in panel data (Baltagi 2002: 95) *** p<0.01, ** p<0.05, * p<0.1

Table 4. Regression analysis: Determinants of change in total *Teacher Employment Units*

	Model 1 FE(CT)	Model 2 FE(CT)	Model 3 FE(CT)	Model 4 FE(CT)	Model 5 PCSE
Teacher employment units (lag)		-0.11 [0.09]	-0.1 [0.09]	-0.11 [0.09]	-0.11 [0.1]
Pupils	0.028** [0.01]	0.038*** [0.01]	0.039*** [0.01]	0.038*** [0.01]	0.038*** [0.01]
Tax revenues	-0.008 [0.01]	-0.016 [0.01]	-0.018 [0.01]	-0.016 [0.01]	-0.016 [0.01]
Debt	-0.025*** [0.007]	-0.028*** [0.010]	-0.022** [0.009]	-0.029*** [0.010]	-0.028** [0.01]
Election year	0.004 [0.004]	0.003 [0.004]	0.003 [0.004]		0.003 [0.003]
SPD absolute majority	0.003 [0.004]	0.003 [0.005]		0.003 [0.005]	0.003 [0.003]
CDU/CSU absolute majority	-0.002 [0.005]	-0.008 [0.006]		-0.008 [0.006]	-0.008* [0.005]
Observations	192	176	176	176	176
Number of id	16	16	16	16	16
R-squared	0.34	0.32	0.31	0.32	0.68
F(Time effects)	1.77***	2.17**	2.04**	2.23**	
F(Country effects)	7.73***	5.55***	7.53***	5.48***	
Mod. Wald (GH), chi2(16)	327.71***	211.28***	241.84***	191.55***	
LM(AR1), chi2(1)	2.03	0.35	0.04	0.11	

Note: FE(CT) = fixed country & time effects, PCSE = Panel corrected standard errors, robust standard errors in brackets, F(Time effects) = F-test for the inclusion of year dummies, F(Country effects) = F-test for the inclusion of country dummies, Mod. Wald (GH) = Modified Wald test for groupwise heteroskedasticity (Greene 2000: 598), LM(AR1) = Lagrange-multiplier test for first-order residual serial correlation in panel data (Baltagi 2002: 95) *** p<0.01, ** p<0.05, * p<0.1

Table 5. Robustness analysis: Determinants of *New Teacher Employment*

	Model 1 FE(CT)	Model 2 FE(CT)	Model 3 FE(CT)	Model 4 FE(CT)	Model 5 FE(C)	Model 6 FE(CT)	Model 7 FE(CT)	Model 8 FE(CT)
New teacher employments (lag)	0.39*** [0.07]	0.38*** [0.07]	0.38*** [0.07]	0.38*** [0.07]	0.46*** [0.07]	0.32*** [0.1]	0.00071 [0.2]	0.38*** [0.07]
Teacher employment units	-2.28 [1.5]	-2.14 [1.5]	-2.22 [1.5]	-2.27 [1.5]	-1.72 [1.5]	-2.56 [3.0]	-1.41 [3.9]	-2.20 [1.5]
Pupils	1.90** [0.8]	1.88** [0.8]	1.91** [0.8]	1.82** [0.8]	1.66** [0.8]	3.09 [2.6]	1.98 [2.0]	1.85** [0.8]
Tax revenues	0.28 [0.5]	0.25 [0.5]	0.25 [0.5]	0.32 [0.5]	0.25 [0.5]	0.28 [0.5]	1.44 [1.1]	0.36 [0.5]
Debt	-0.21 [0.4]	-0.15 [0.4]	-0.17 [0.4]	-0.25 [0.4]	0.16 [0.3]	0.48 [0.6]	-2.50* [1.4]	-0.18 [0.4]
SPD absolute majority	0.051 [0.1]	0.066 [0.1]	0.067 [0.1]	0.055 [0.2]	-0.041 [0.1]	0.039 [0.2]	-0.43 [0.3]	0.049 [0.1]
CDU/CSU absolute majority	-0.031 [0.2]	0.031 [0.2]	0.006 [0.2]	-0.009 [0.2]	0.14 [0.2]	0.077 [0.3]	0.43** [0.2]	-0.002 [0.2]
Pre election year	0.25** [0.10]							
Election year		0.36*** [0.1]			0.41*** [0.1]	0.29* [0.2]	0.48** [0.2]	
Post election year			0.14*** [0.05]					
New government				0.49* [0.3]				
Post PISA					0.20** [0.09]			
Non election period years								-0.13** [0.06]
Observations	192	192	192	192	192	112	80	192
Number of id	16	16	16	16	16	16	16	16
R-squared	0.54	0.54	0.55	0.53	0.42	0.33	0.42	0.55
Period	1993-2004	1993-2004	1993-2004	1993-2004	1993-2004	1993-1999	1999-2004	1993-2004

Note: FE(CT) = fixed country & time effects, FE(C) = fixed country effects, robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1. Definition and Source of Variables

Variable	Definition	Source
New teacher employments	Full time or part time new employed teachers measure in head-counts	Statistische Veroeffentlichungen der Kultusministerkonferenz Dokumentation Nr. 175 Tab. 1.5
Teacher employment units	Full time and part time teacher employments measured in full time equivalent units	Statistisches Bundesamt Schulstatistik Fachserie 11 / Reihe 1 Tab. 7.1
Pupils	Pupils in public schools measure in head-counts	Statistisches Bundesamt Schulstatistik Fachserie 11 / Reihe 1 Tab. 3.1
Tax revenue	Tax revenue before tax adjustment	Statistisches Bundesamt GENESIS (temp. Tabelle)
Debt	Capital market debt per GDP	Statistisches Bundesamt GENESIS (temp. Tabelle)
SPD absolute majority	Dummy variable taking 1 if the SPD is in the absolute majority. Based on a 1 to 5 scale for government ideology. 1 = SPD absolute majority, 2 = SPD in a coalition with a smaller coalition partner, 3 = Grand coalition, 4 = CDU/CSU in a coalition with a smaller coalition partner, 5 = CDU/CSU absolute majority	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1, http://www.wahlergebnisse.info
CDU/CSU absolute majority	Dummy variable taking 1 if the CDU/CSU is in the absolute majority. Based on a 1 to 5 scale for government ideology (see SPD absolute majority)	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1, http://www.wahlergebnisse.info
Election year	Defined as $((M-1)+d/D)/12$, where M is the month of election, d is the day of election and D is the number of days in that month	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1
Pre-Election year	Defined as $(12-(M-1)-d/D)/12$, where M is the month of election, d is the day of election and D is the number of days in that month	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1
Post-Election year	Defined as $(12+(M-1)+d/D)/12$, where M is the month of election, d is the day of election and D is the number of days in that month	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1
Non election period years	Dummy variable taking 0 in the years before and after an election and in election years and 1 otherwise	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1
New government	Dummy variable taking the values of the Election variable if a new head of the state was elected	Der Bundeswahlleiter Wahl zum 16. Deutschen Bundestag Heft 1 Tab. 3.1
Post PISA	Dummy variable taking 1 after 1999 and 0 otherwise	

Appendix Table 2. Frequencies for political business cycle variables (in years)

State	Election years	New governments	SPD absolute majority	CDU/CSU absolute majority
Schleswig-Holstein	3	0	4	0
Hamburg	4	1	5	1
Lower-Saxony	3	1	9	0
Bremen	3	0	0	0
North Rhine-Westfalia	2	0	13	0
Hesse	3	1	0	0
Rhineland-Platinat	2	0	13	0
Baden-Wuerttemberg	3	0	0	0
Bavaria	3	0	0	13
Saarland	3	1	7	6
Berlin	3	1	0	0
Brandenburg	3	0	7	0
Mecklenburg West.	3	1	0	2
Saxony	3	0	0	13
Saxony-Anhalt	3	2	0	2
Thuringia	3	0	0	13
Total	47	8	58	50

Appendix Table 3. Summary statistics

Variable	Obs.	Mean	Std.Dev.	Min	Max
New teacher employments (log)	208	6.35	1.31	3.26	8.94
Total teacher employment units (log)	208	10.29	0.85	8.62	11.91
Pupils (log)	208	12.95	0.89	11.16	14.67
Tax revenue (log)	208	13.49	0.99	11.58	15.34
Debt (log)	208	-1.68	0.57	-3.15	-0.37
SPD absolute majority	208	0.28	0.45	0	1
CDU/CSU absolute majority	208	0.24	0.43	0	1
Pre-Election Year	208	0.14	0.28	0	1
Election year	208	0.11	0.24	0	0.81
Post-Election Year	208	0.34	0.63	0	1.81
Non-Election Period Years	208	0.36	0.48	0	1

Appendix Table 4. Non-Stationarity

	New teacher employment (log)	Teacher employment units (log)
chi2(32)	45.68	29.51
Prob > chi2	0.05	0.59

Note: Fisher Test for panel unit root using an augmented Dickey-Fuller test (1 year lag)

Appendix Table 5. Panel jackknife coefficient estimates omitting one cross section at a time

	Minimum	Estimate	Maximum	Range
New teacher employments (lag)	0.60	0.64	0.67	0.08
Teacher employment units	-1.26	-1.04	-0.89	0.37
Pupils	0.79	0.93	1.16	0.37
Tax revenues	0.39	0.49	0.59	0.19
Debt	-0.01	0.08	0.30	0.31
Election year	0.25	0.31	0.37	0.12
SPD absolute majority	-0.02	0.07	0.11	0.13
CDU/CSU absolute majority	-0.06	0.04	0.09	0.15

Note: Minimum = Minimum coefficient, Maximum = Maximum coefficient, Estimate = Coefficient estimate using all cross sections