

Educational business cycles

The political economy of teacher hiring across German states, 1992–2004

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Received: 21 November 2007 / Accepted: 13 October 2008 / Published online: 25 October 2008
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Abstract Strong institutional constraints and better-informed voters may lead re-election seeking incumbents to *shift* the use of political business cycle mechanisms away from monetary and fiscal policy towards other policy domains that are more easily manipulable, targetable, and timeable. We investigate teacher employment patterns at the state level in Germany and find strong evidence of cycling mechanisms, in the form of electioneering and honeymooning. Against a backdrop of a continuously shrinking *total* teachers' pool, German state-level incumbents accelerate the hiring of *new* teachers during election periods and partly reverse this during politically safer points in the electoral cycle. Cycles are mediated by issue salience: heightened attention to German public schooling after the notorious PISA-2000 tests further strengthens the manipulation of new teacher hiring for electoral purposes.

Keywords Public education · Teacher employment · Political-economic cycles · German federalism · Electioneering · Honeymooning

JEL Classification D72 · I28

1 Introduction

There is strong evidence that incumbents in democracies benefit from favorable economic conditions (Drazen 2000; Hibbs 2006; Tufté 1978). Re-election seeking politicians therefore naturally appear to have powerful incentives to gain votes by increasing the economic

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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), 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. 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 investigates these arguments in the case of employment 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 more strongly tend to reduce the total size of their teacher 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 heightened political salience of education after the German PISA 2000 results further strengthens the cyclical use of this policy domain for electoral purposes. The article proceeds as follows. The second section reviews political-economic business cycle theory, prior empirical studies on Germany, and the institutional context of German federalism to derive testable implications for public education. The third section presents our new dataset and estimation strategy and the fourth section presents and discusses our empirical results. The last section concludes.

2 Political-economic cycles

2.1 Classic political business cycle theory

If voters evaluate candidates on their recent performance, the incentives for the latter to manipulate public policy increase as elections approach. Classic business cycle theory thus starts from the assumption that since politicians primarily care about holding office, they

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 the partisan cycle originally proposed by Hibbs (1977). *Electoral cycle* theory predicts that incumbents use expansionary monetary policy to improve economic performance before elections. In the absence of fully rational expectations, this would help to increase real economic activity and therefore re-election chances (Nordhaus 1975; Berger and Woitek 1997, and Drazen 2000 on fiscal policy). 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).

An important further complication arises from voters' time horizons and their assumed degree of rationality. The literature on political-economic cycles divides into two perspectives here: 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 incumbents' past performance, whereby voters reward good performance and punish bad performance. The empirical literature on voting and popularity functions tends to support the latter assumption (Lewis-Beck and Paldam 2000). Overall, retrospective models have a better empirical fit than prospective models. If the economy goes well, so will the incumbents' popularity (Nannestad and Paldam 1994).

This article therefore assumes that retrospective voters observe public sector provision, judge incumbent performance, and allocate their votes accordingly. Incumbents can then exploit informational advantages in order to signal their competence through pre-election stimulation. While voters in this account are assumed to have full knowledge about incumbents' electoral motivations and preferences, voters are *not* assumed to have either perfect knowledge or complete rationality. Indeed, this sort of competence-signaling electoral cycles is likely to disappear as soon as there are no information asymmetries between voters and incumbents (Alesina et al. 1993; Drazen 2000; Hibbs 2006). In this specific sense, cycles are possible only if voters are not fully rational. It follows that the quality of information available to voters relative to incumbents structures the incidence and nature of cycles. Shi and Svensson (2006) show that the effect of elections on fiscal policy differs between developing and developed countries: cycles tend to be smaller and less robust in Western democracies, where stronger institutional constraints on budget decisions and better-informed voters may prevent politicians from manipulating the economy (see also Alt and Lassen 2006 on fiscal transparency and debt cycles).

2.2 Political-economic cycles in Germany

Using federal-level data, Berger and Woitek (1997) find no evidence for monetary business cycles, though results are very sensitive to data quality and estimation methodology. Seitz (2000) finds no evidence for the impact of government ideology on public spending decisions at the German state level. Jochimsen and Nuscheler (2006) find no evidence of partisan cycles, but suggest that public budget deficits decrease in pre-election years. Investigating eleven western German states with respect to seven different budget categories, Galli and Rossi (2002) find no support for partisan cycles and weak evidence for opportunistic cycles. With respect to education expenditure they find that neither theory is supported by their estimation results. Potrafke (2006) finds weak evidence that politicians increase expenditures

for “schooling” in election years and indicates that government ideology has no effect on any of six expenditure categories. Oberndorfer and Steiner (2006) find contrasting evidence for the classical partisan theory in West German states: conservative governments or social-democratic-conservative coalitions spend more, not less, on public higher education than pure social-democratic governments.

As this review shows, earlier empirical work on Germany has primarily investigated monetary business cycles, while more recent publications focus on budget cycles. The ambiguity of the empirical findings may indicate that German politicians have little scope to manipulate monetary policies or fiscal budget decisions. This article considers dependent variables that differ in a fundamental way from those of prior empirical research, by shifting attention toward concrete employment policies. By studying education, we focus on a policy domain in which there may be more room for political manipulation owing to the particular constitutional context of German federalism.

2.3 Political-institutional context: teacher hiring in German federalism

The particular allocation of legal competencies among federal and state levels makes public education a fruitful case for the analysis of competence-signaling electoral cycles. One of the distinct features of German federalism, which sets it apart from other systems, is the institutionalized interlocking of three levels of government: federal (Bund), state (Laender) and local (Gemeinden) (Scharpf 2005). The allocation of competences among Bund and Laender follows the subsidiarity principle (Articles 30, 70 and 83 of the Basic Law) but gives the federal government leeway to become active in legislation (Benz 1999, p. 62). The tendency toward centralization became evident soon after the passing of the Federal constitution in 1949 through policies to establish a uniform legal and economic order (Hesse 1962). In the subsequent five decades, federal governments managed to extend their reach through a number of constitutional adjustments (Benz 1999, p. 62). State governments benefited from centralization through fiscal cooperation with the federal government. Today, German federalism is characterized by a relatively strong federal government with legislative power in all major policy areas, whereas the Laender are, in most cases, responsible for implementing the law (Benz 1999, p. 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 have led to a system of interlocking politics (Benz 1999, p. 56), or, more problematically, “joint decision traps” (Scharpf 2005).

Public education and cultural affairs, however, have always been, and continue to be, an exception to 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 govern public education and cultural affairs on their own (Benz 1999). To this day, legislation, financing and administrative competencies in this area are exclusively set at the state level (Article 7 and 30 of the Basic Law). Curricula, funding, and teacher employment are determined directly through the State Ministries of Education. The Conference of State Ministers of Education and Culture (Kultusministerkonferenz), founded in 1948, passes regulatory recommendations for the uniform treatment of school policies but 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 subsidiarity is particularly pronounced and where Laender independence is highest.

In recent years, the political salience of public education has significantly increased in German public discourse. In the so-called “PISA shock” of 2000, German 15-year-olds made a bad showing in literacy, mathematics and science on the internationally comparable Program for International Student Assessment tests. Germany performed well below the

OECD average for literacy and was outflanked not just by neighboring Denmark, France and Belgium, but even by countries such as Spain, the United States, Italy, and the Czech Republic (Allmendinger and Leibfried 2003). These results struck a nerve among politicians and the general public alike, and they strongly boosted public interest in, and media debates about, German education policy. Part of a number of high-profile articles on PISA 2000, the *Frankfurter Allgemeine Zeitung* wrote that “the competences of German children, their knowledge, their schooling, are treated as if the [German] national football team had messed up everything again” (FAZ 2001a, see also FAZ 2000, 2001b, 2001c). As there are substantial differences between the Laender, 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, p. 127).¹ An earlier attempt to reach a reform compromise between Bund and Laender had broken down in 2004 over the federal government’s attempt to increase its influence in public education. Faced with heavy political resistance from state heads irrespective of their political party membership, the federal government eventually withdrew its claims (Scharpf 2005).

2.4 Hypotheses

The lack of room for maneuver in other policy fields may turn public education into a particularly important arena for political 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 ‘means’ for exploiting information asymmetries to gain votes (Tuftte 1978, Franzese and Jusko 2006). We focus on one dimension of education—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 political salience of public education provides some *prima facie* circumstantial evidence for its electoral importance (more on this below). Given their autonomy in this policy domain, state-level incumbents may use teacher employment policy for cycling purposes. Since retirement and maternity leave may over-compensate 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. Cycling theory’s prediction that incumbents 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 and around election years.
- (H2) Election periods 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.

¹The German 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 Data and estimation strategy

Detailed definitions, sources, and summary statistics of all variables are given in [Appendix Tables A.1 and A.2](#). We employ balanced panel data comprising annual data for the 16 German states from 1992 to 2004. New teacher hires 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. Note that our two dependent variables are measured with different scales. For example, one new teacher hire represents one person, while one teacher employment unit can consist of one full-time teacher or two or more part-time teachers. Following Franzese (2000), *Pre-Election Year* and *Election Year* capture the precise timing of the elections and are defined as follows:

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

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

where M represents the month and d 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. Partisan ideology in government is captured by two alternative measures. First, SPD (resp. CDU/CSU) *Absolute Majority* is a dummy variable, which equals one if all members of the cabinet belong to the Sozialdemokratische Partei Deutschlands (SPD), respectively the Christlich Demokratische Union Deutschlands (CDU) or Christlich-Soziale Union Bayerns (CSU). 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. Second, *Left* and *Right* is measured as the share of cabinet members belonging to respectively a leftist (SPD, Greens, PDS) and a rightist (CDU, CSU, FDP) party (Schmidt 2006). During the observation period the Heads of State have been either SPD or CDU/CSU politicians. *New Government*, our variable capturing government change, is defined as 1 if a state's Head changed from left to right or vice versa after elections, and 0 if a sitting government was re-elected.

Following Fernandez and Rogerson (1997) and Falch and Rattso (1997), we use a log-transformed specification to examine determinants of new teacher employment. Specifically we start from the following dynamic panel data model:

$$y_{i,t} = \beta_1 y_{i,t-1} + \beta_2 x_{i,t} + \beta_3 z_{i,t} + \mu_i + \varepsilon_{i,t} \quad (1)$$

where $y_{i,t}$ denotes to the log of *New Teacher Employments* or of *Teacher Employment Units* in state i at time t . *Teacher Employment Units* accounts for the overall level of teacher employment measured in full-time equivalents, while *New Teacher Employments* is measured as head counts. The control variables are summarized in vector $x_{i,t}$. It includes *Teacher Employment Units* (only when New Teacher Employment is considered as the dependent variable), *Pupils*, *Tax Revenues* and *Debt*. *Teacher Employment Units* controls for how new teacher employments depend on the current total level of teacher employment. *Pupils* accounts for an opposite social needs effect in classrooms: to maintain a certain pupils/teachers ratio, a rise in the number of pupils is predicted to increase new teacher employments. Since budgetary constraints may influence employment decisions, we include *Tax Revenues* (expected to have a positive effect on teacher employment) and *Debt per GDP* (expected to

have a negative effect). Since it can be argued that there is little room for randomness in a sample that includes all 16 German states, the time invariant effect μ_i is considered to be fixed rather than random. The random effect assumption can be rejected *a priori*, and the Hausman (1978) test for random country effects ($\chi^2(18) = 82.35^{***}$) further supports the use of a fixed effects specification. Hence the random effects estimator can be rejected on both substantive and statistical grounds. However, when the time dimension of the dynamic panel is small, a fixed effect estimator or Least-Square Dummy Variable (LSDV) estimator including a lagged dependent variable generates biased estimates (Nickell 1981).² Instrumental variable methods, which typically consider the first difference, may then be used to improve on the LSDV estimates.

$$\Delta y_{i,t} = \beta_1 \Delta y_{i,t-1} + \beta_2 \Delta x_{i,t} + \beta_3 \Delta z_{i,t} + \mu_i + \varepsilon_{i,t} \quad (2)$$

where Δ is the first-difference estimator. The ‘AH estimator’ developed by Anderson and Hsiao (1982) removes the source of bias by using $y_{i,t-2}$ as an instrument for $\Delta y_{i,t-1}$, while the ‘AB estimator’ developed by Arellano and Bond (1991) uses all valid lags in the dependent variable as instruments for $\Delta y_{i,t}$. Due to the larger set of instruments the AB estimator is more efficient than the AH estimator (Jochimsen and Nuscheler 2006, p. 11–14). The AB estimator can be specified assuming homoscedasticity (one-step) or heteroscedasticity (two-step). Simulation studies by Arellano and Bond (1991), Kiviet (1995) and Judson and Owen (1997) indicate that the two-step AB is in most cases less efficient than the one-step AB. However, the AB estimator is a micro-panel data estimator with poor large-sample properties. Thus, results of the AB estimator (one-step) are used for robustness checks. Judson and Owen (1999) show that with a small time dimension the best estimator is the bias-corrected LSDV estimator developed by Kiviet (1995). LSDVc requires a consistent estimator to initialize the bias correction. This can either be the Anderson-Hsiao (AH), Arellano-Bond (AB) or Blundell-Bond (BB) estimator. Bruno’s (2005) simulation study finds that the AB estimator outperforms the AH estimator and is more robust than the BB estimator. Thus, we choose the AB estimator for initial bias correction with the Kiviet (1995) bias approximation. The standard errors are bootstrapped with 1000 repetitions (see also Jochimsen and Nuscheler 2006).

4 Empirical analysis

4.1 Descriptive analysis

To explore some initial relationships, Fig. 1 presents aggregated figures for the relationship between *New Teacher Employments* and *Teacher employment units* (log-transformed values) and electoral timing. There appears to be a distinct temporal pattern in the case of *New Teacher Employments*, featuring increases before and during election years and decreases thereafter.³

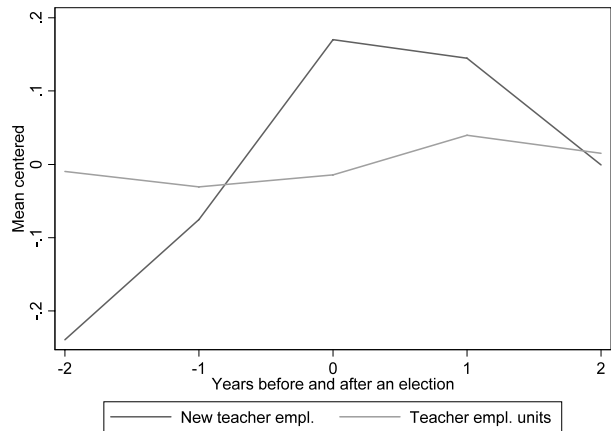
Tables 1 and 2 compare average annual growth rates for new teachers and total teacher employment units for, respectively, election versus non-election years, and new versus non-new governments. Columns 1 and 2 show that the average annual growth rate for new teachers is more than three times higher in election years than in non-election years, and more

²We are grateful to two anonymous reviewers for discussing Nickell bias issues with us.

³A similar pattern (not shown, but available on demand) has been obtained when disaggregating these data to the state level in the vast majority of our sixteen cases.

Fig. 1 Teacher employment and electoral timing (1992–2004).

Note: Average values of *New teacher employment* (log) and *Teacher employment units* (log).
 -2 = two years before an election, -1 = one year before an election, 0 = election year, 1 = first year after an election, 2 = second year after an election

**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

than four times higher if elections carried to power a new Head of State, 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 witnessed 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 Heads of State from the same party as before. These observations give *prima facie* reasons for searching for a

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

causal relationship between elections and the asymmetric use of teacher hiring for electoral gain.

4.2 New teacher employment

Our baseline regression results on new teacher hiring are given in Table 3. None of the signs of the estimated coefficients for the political ideology variables are statistically significant—whether measured as absolute majority or as relative cabinet share. This is broadly consistent with recent studies on total education expenditures, which found a waning influence since the 1980s of political partisanship (Busemeyer 2007). By contrast, the number of pupils in a state is positively and significantly associated with new teacher hiring. The estimated coefficient for our main business cycle variable of interest, *Election Year*, is statistically significant and positive across all four models ($\beta = 0.32$ – 0.36). This supports the basic political business cycle hypothesis. In other words, these baseline results indicate that new teacher appointments are subject, on the part of incumbents, to electioneering (H1), but not partisaneering (H3).

4.3 Total teacher employment units

Table 4 reproduces the same models, this time with changes in total teacher employment units as the dependent variable. As before, *Pupils* shows a positive and statistically significant association, while the political ideology coefficients are statistically insignificant. Interestingly, however, the latter is true now also for *Election Year*. This lends support to (H2) and (H4).

Jointly, 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 changes in 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 to explain this, without watering down the assumption that incumbents

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

	Model 1	Model 2	Model 3	Model 4
	LSDVc	LSDVc	LSDVc	LSDVc
New teacher employments (lag)	0.48 ^{***} [0.07]	0.48 ^{***} [0.07]	0.49 ^{***} [0.07]	0.48 ^{***} [0.07]
Teacher employment units	−2.02 [1.5]	−2.09 [1.5]	−2.06 [1.5]	−1.97 [1.5]
Pupils	1.86 ^{**} [0.8]	1.80 ^{**} [0.8]	1.78 ^{**} [0.8]	1.74 ^{**} [0.8]
Tax revenues	0.12 [0.4]	0.20 [0.4]	0.19 [0.4]	0.19 [0.4]
Debt	−0.022 [0.3]	−0.072 [0.3]	−0.099 [0.3]	−0.13 [0.3]
Election year	0.32 ^{**} [0.1]	0.36 ^{***} [0.1]	0.36 ^{***} [0.1]	0.36 ^{***} [0.1]
SPD absolute majority	−0.14 [0.1]			
CDU/CSU absolute majority	−0.11 [0.1]			
Left cabinet share		−0.067 [0.1]		
Right cabinet share			0.043 [0.2]	
Observations	192	192	192	192
Number of id	16	16	16	16

Note: Bias corrected Least-Squares Dummy Variable (LSDVc) using Arellano and Bond (1991) for initial bias correction and $O(1/TN)$ to determine the accuracy of the approximation (Kiviet 1995), standard errors are bootstrapped with 1000 repetitions, standard errors are in brackets, ^{***} $p < 0.01$, ^{**} $p < 0.05$, ^{*} $p < 0.1$

face hard budget constraints over the course of the electoral cycle, as the recent literature points out?

At least two plausible political-economic 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 1). In addition, they could use newly hired teachers to allow more elderly teachers eager to retire early to do so, which would be another politically popular and self-selective 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 po-

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

	Model 1 LSDVc	Model 2 LSDVc	Model 3 LSDVc	Model 4 LSDVc
Teacher employment units (lag)	0.92*** [0.05]	0.91*** [0.04]	0.91*** [0.04]	0.92*** [0.04]
Pupils	0.065*** [0.02]	0.066*** [0.02]	0.067*** [0.02]	0.066*** [0.02]
Tax revenues	−0.0043 [0.01]	−0.0039 [0.01]	−0.0038 [0.01]	−0.0041 [0.01]
Debt	−0.024*** [0.008]	−0.024*** [0.009]	−0.024*** [0.008]	−0.025*** [0.007]
Election year	0.0035 [0.004]	0.0035 [0.004]	0.0034 [0.004]	0.0035 [0.004]
SPD absolute majority	−0.00097 [0.003]			
CDU/CSU absolute majority	0.0013 [0.004]			
Left cabinet share		−0.00076 [0.004]		
Right cabinet share			0.001 [0.004]	
Observations	192	192	192	192
Number of id	16	16	16	16

Note: Bias corrected Least-Squares Dummy Variable (LSDVc) using Arellano and Bond (1991) for initial bias correction and $O(1/TN)$ to determine the accuracy of the approximation (Kiviet 1995), standard errors are bootstrapped with 1000 repetitions, standard errors are in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

litical 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 potentially 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 at an electorally safe moment in the cycle for their earlier election-period extravaganza (Table 1, column 4). A similar compensation mechanism over the electoral cycle may also be at work with respect to new teacher hiring, on which more below.

4.4 Robustness analyses and further interpretation

Table 5 presents a number of further specifications and robustness tests for our baseline findings on new teacher employments. In Model 1 we generate a *Pre-Election Year* variable.

Table 5 Robustness analysis: Determinants of New Teacher Employment

	Model 1 LSDVc (AB)	Model 2 LSDVc (AB)	Model 3 LSDVc (AB)	Model 4 LSDVc (AB)	Model 5 LSDVc (AB)	Model 6 LSDVc (AB)	Model 7 LSDVc (AB)
New teacher employments (lag)	0.49*** [0.07]	0.48*** [0.07]	0.49*** [0.07]	0.59*** [0.07]	0.50*** [0.11]	0.25 [0.2]	0.49*** [0.07]
Teacher employment units	-2.11 [1.5]	-2.02 [1.5]	-2.09 [1.5]	-2.06 [1.6]	-2.84 [3.5]	-0.91 [5.7]	-2.07 [1.5]
Pupils	1.87*** [0.8]	1.86*** [0.8]	1.78*** [0.8]	1.78*** [0.8]	3.24 [2.4]	1.85 [3.4]	1.87*** [0.8]
Tax revenues	0.15 [0.4]	0.12 [0.4]	0.22 [0.4]	0.24 [0.4]	0.16 [0.6]	1.15 [1.4]	0.14 [0.4]
Debt	-0.035 [0.3]	-0.022 [0.3]	-0.1 [0.3]	0.074 [0.2]	0.38 [0.5]	-1.24 [2.8]	-0.032 [0.3]
SPD absolute majority	-0.14 [0.1]	-0.14 [0.1]	-0.11 [0.1]	-0.13 [0.1]	-0.33* [0.2]	-0.15 [0.3]	-0.13 [0.1]
CDU/CSU absolute majority	-0.11 [0.1]	-0.11 [0.1]	-0.1 [0.1]	-0.049 [0.1]	-0.26 [0.3]	-0.1 [0.4]	-0.1 [0.1]
Pre-election year	0.25*** [0.1]						
Election year		0.32** [0.1]		0.41*** [0.1]	0.24 [0.2]	0.47* [0.3]	
New government			0.30* [0.2]				
Post-PISA				0.19** [0.09]			-0.18** [0.07]
Non-election-period years							
Observations	192	192	192	192	112	64	192
Number of id	16	16	16	16	16	16	16
Period	1993–2004	1993–2004	1993–2004	1993–2004	1993–2004	1993–1999	1999–2004

Note: Bias corrected Least-Squares Dummy Variable (LSDVc) using Arellano and Bond (1991) for initial bias correction and $O(1/TN)$ to determine the accuracy of the approximation (Kiviet 1995), standard errors are bootstrapped with 1000 repetitions, standard errors are in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The coefficient is statistically significant and positive, though the effect size is smaller than for the exact *Election Year* in Model 2. Consistent with the picture painted by Fig. 1, this indicates steadily growing “election period fever” among incumbents. But how to make sense of the post-election year effect, which is also apparent in Fig. 1? The empirical pools of pre-electoral candidates contain some candidates who made incredible promises (and therefore lost) and some incumbents who delivered too little (and therefore lost). 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 want to deliver, to cement their reputations. Model 3 therefore introduces the variable *New Government* to explore one subset of election winners—*newly* elected incumbents. The coefficient is positive and statistically significant ($\beta = 0.30$). This suggests a honeymooning effect, possibly resulting from the fact that newly elected parties in government need to act especially quickly to assure voters of the credibility of their pre-electoral promises.

Models 4, 5, and 6 in Table 5 represent the next step in our robustness analysis. We test the hypothesis that teacher hiring may have gained importance as a tool for cycling once the ‘PISA 2000 shock’ had raised the political salience of public education among the German electorate. For instance, the main German teachers’ union, GEW, explicitly resisted the PISA 2000 study in a strongly worded strategy paper, and it threatened to mobilize students and parents against the tests. The President of the Conference of State Ministers of Education and Culture—who was a senator, incidentally, from the single worst-performing state, Bremen—pointed out that individual states or schools could not be blamed for failure (FAZ 2000; Allmendinger and Leibfried 2003, pp. 75–76). In this political context, the number of new teachers may have been used by German voters and incumbents alike as a heuristic, or a proxy, for a deeper variable of interest—the quality of public schooling. At least two mechanisms could be at play in voters’ minds. First, a larger number of newly trained (younger) teachers could improve education quality directly, because they are presumed to be better motivated and better skilled than the average in the teachers’ pool. Second, all else equal, more newly hired teachers could improve education quality indirectly by reducing average class size—a variable commonly assumed by voters, and touted by teachers’ unions, as being crucial for educational quality. Model 4 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 5 and 6 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 after PISA 2000, the *Election Year* coefficient is statistically significant only in the second period, and the effect size is larger by 0.23 percentage points.

Model 7 in Table 5 tests the effect on new teacher hiring of *Non-Election-Period Years*, defined as a simple dummy variable equaling zero both in the years before and after an election and in the election year itself, and one in all other years. In a mirror image to our electioneering result, *Non-Election-Period Years* has a statistically significant, relatively large and *negative* effect on new teacher employments ($\beta = -0.18$). To further check our findings on the temporal dimensions of new teacher hiring, we have defined each year in the electoral cycle as a simple dummy variables in appendix Table A.4. Results further corroborate the ‘growing election period fever’ interpretation regarding Fig. 1 and Table 5 above. Only the coefficient for *Election Year* is positive and significant. The coefficient for *Pre-Election Year* is positive, while those of the other years in the electoral cycle are negative in Appendix Table A.4. In other words, outside of election periods, incumbents could further restore fiscal conservatism over the entire cycle, by compensating for their election-period extravaganza (increased new teacher hiring) at a politically much safer moment in the electoral cycle.

One last methodological test of the robustness of our main findings is in order here.⁴ Appendix Tables A.5 and A.6 compare results of different dynamic panel estimators. While Model 1 still employs the Least-Squares Dummy Variable estimator (fixed effect estimator), Models 2 and 3 use, respectively, the Arellano and Bond (1991) one-step approach, and the Anderson and Hsiao (1982) estimator. Since the AB and AH estimators are micro-panel data estimators with poor large-sample properties, both are used for robustness checks. Model 4 in Appendix Tables A.5 and A.6 uses the LSDVc estimator with the Arellano and Bond (1991) estimator for initial bias correction and with $O(1/T)$ to determine the accuracy of the approximation.⁵ We choose to use the Nickell (1981) approach to determine the accuracy of the approximation in these Appendix tables, rather than the Kiviet (1995) approximation presented in Tables 3 and 4 above. Model 5, lastly, uses the LSDVc estimator with the Anderson and Hsiao (1982) estimator for initial bias correction and $O(1/T)$ to determine the accuracy of the approximation (Nickell 1981). With the sole exception of the AH specification, the findings in our Appendix Tables A.5 and A.6 confirm that the main findings discussed above are indeed robust. This increases our confidence that the cyclical patterns observed in German teacher employment are more than a statistical artifact.

5 Conclusions and discussion

This article aims to contribute to the literature on political-economic cycles in three respects. First, it provides a novel empirical application of business cycle theory to the case of teacher employment in Germany. Second, we have qualified recent claims that practices such as cycling and clientelism are prevalent mainly in younger and/or less consolidated democracies (Shi and Svensson 2006; Alt and Lassen 2006; Keefer 2007). Our study of this large and well-established federal democracy in Europe shows considerable evidence of *electioneering* (but not *partisaneering*) by all incumbent parties in government, and of *honeymooning* by new incumbents. The effect of electoral timing on new teacher employment proved to be robust. But beyond this, our analysis also pointed to potentially significant budget-balancing (or deficit-reducing) mechanisms over the entire electoral cycle, in that increases in new teacher hiring during election periods might be compensated by concomitant reductions both in new teacher hiring and in the total teacher pool 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 (Tuftte 1978; Franzese and Jusko 2006).

Our finding of electioneering and honeymooning in teacher hiring is in line with recent evidence indicating politicians' strong preferences for targetable spending and clientelist policies whenever the institutional context allows (e.g. Keefer 2007). It may also inform wider theoretical debates about democratic efficiency (e.g. Vanhuysse 2002). For instance, in education policy, a disproportionately large share of public spending tends to go to inputs which teachers value highly (primarily higher wages and teacher-to-pupil ratios), even though the marginal effectiveness of these inputs is often dramatically lower than that of

⁴We are grateful to two anonymous reviewers for urging us to do so.

⁵The LSDV estimator is not consistent for N large and T finite (Bruno 2004). For $N \rightarrow \infty$, Nickel (1981) expresses the inconsistency as $O(1/T)$. Using asymptotic expansion techniques, Kiviet (1995) expresses the inconsistency as $O(1/NT)$. Approximating the bias by $O(1/NT)$ offers a method to correct the LSDV estimator for samples where N is small or only moderate large (Bruno 2004, p. 2).

inputs such as textbooks, classroom equipment, writing materials, libraries, and software (Hanushek 2003; Pritchett and Filmer 1999). Rather than stemming from voter irrationality, such seeming inefficiencies might, alternatively, be explained by standard rational voter ignorance. Narrow-focus interest groups such as teachers substantially and directly benefit from more jobs, higher wages and smaller classes. By contrast, average voters may simply face too high a cost/benefit ratio for them to become fully informed regarding the marginal productivities of education spending and hiring-and-firing. Electorally, the attractiveness to incumbents of teacher-favorable policy bias is further enhanced by teachers' high recorded levels of unionization and voting turnout rates (Vanhuysse and Sulitzeanu-Kenan 2009). Relative cost/benefit ratios of information gathering to voters and pressure groups might also suggest alternative explanations for the variable prevalence of political-economic cycles across different policy domains. Regarding monetary and fiscal policies, comparatively much information is cheaply available to voters today from government-independent sources such as central banks, economic think tanks and the financial press. On the benefit side, inefficient monetary and fiscal policies can evidently have macro-economic consequences directly palpable to voters. By contrast, in education policy, the costs of information gathering may be noticeably higher (less trustworthy information readily available) and the benefits lower (efficiency gains less directly palpable). To the extent that this is the case, rational voter ignorance would provide a theoretical alternative to voter irrationality for explaining why cycles could be more prominent in targetable, narrow-focus policies such as education than in standard macro-economic policies.

Lastly, our findings may help to shed new light on recent political developments in German federalism. Consider the first attempt to reach a reform compromise between the federal and state levels, which broke down in 2004 over the federal government's attempt to increase its influence in public education. Discussing this breakdown, Scharpf (2005, p. 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 State Heads may be usefully interpreted as a rational attempt by vote-seeking state-level politicians to protect their autonomy over a local policy domain that can be usefully manipulated for electoral purposes.

Acknowledgements We gratefully acknowledge research funding from Haifa University's Centre for German and European Studies (HCGES) and support from Yale University's Leitner Program for International and Comparative Political Economy, the Max Planck Institute in Cologne, and the European Centre for Social Welfare Policy and Research in Vienna. Helge Berger, Marius Busemeyer, Bernhard Kittel, Frank Neher, Frances Rosenbluth, Frieder Wolf, and seminar participants at the Verein fuer Socialpolitik's 2007 Meeting, Haifa University's Faculty of Education, the Max Planck Institute, and the European University Institute in Florence provided helpful comments.

Appendix

Table A.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 (2005) Dokumentation Nr. 175 Tab. 1.5
Teacher employment units	Full time and part time teacher employments measured in full time equivalent units	Statistisches Bundesamt (2005) Schulstatistik Fachserie 11 / Reihe 1 Tab. 7.1
Pupils	Pupils in public schools measure in head-counts	Statistisches Bundesamt (2005) Schulstatistik Fachserie 11 / Reihe 1 Tab. 3.1
Tax revenue	Tax revenue before tax adjustment	Statistisches Bundesamt (2006) GENESIS (temp. Tabelle)
Debt	Capital market debt per GDP	Statistisches Bundesamt (2006) GENESIS (temp. Tabelle)
SPD absolute majority	Cabinet consists exclusively of SPD ministers	Schmidt (2006)
CDU/CSU absolute majority	Cabinet consists exclusively of CDU/CSU ministers	Schmidt (2006)
Left cabinet share	Share of left wing ministers in the cabinet (Left is defined as SPD, Green, PDS)	Schmidt (2006)
Right cabinet share	Share of right wing ministers in the cabinet (Right is defines as CDU, CSU, FDP)	Schmidt (2006)
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	Bundeshwahlleiter (2005) 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	Bundeshwahlleiter (2005) 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	Bundeshwahlleiter (2005) 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	Bundeshwahlleiter (2005) Heft 1 Tab. 3.1
Post-PISA	Dummy variable taking 1 after 1999 and 0 otherwise	

Table A.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	12	0
Hamburg	4	1	5	0
Lower-Saxony	3	1	3	1
Bremen	3	0	0	0
North Rhine-Westfalia	2	0	12	0
Hesse	3	1	7	5
Rhineland-Platinat	2	0	0	0
Baden-Wuerttemberg	3	0	0	4
Bavaria	3	0	0	13
Saarland	3	1	7	0
Berlin	3	1	2	0
Brandenburg	3	0	0	0
Mecklenburg West.	3	1	3	2
Saxony	3	0	0	10
Saxony-Anhalt	3	2	7	2
Thuringia	3	0	0	6
Total	47	8	58	43

Table A.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	0.21	0.41	0	1
Left cabinet share (ratio)	208	0.55	0.42	0	1
Right cabinet share (ratio)	208	0.42	0.41	0	1
Election year	208	0.11	0.24	0	0.81
Pre-Election Year	208	0.12	0.25	0	0.91
Non-Election Year	208	0.36	0.48	0	1
New government	208	0.04	0.19	0	1
PISA	208	0.31	0.46	0	1

Table A.4 Electioneering in *New Teacher Employments* measured via year dummies

	Model 1 LSDVc	Model 2 LSDVc	Model 3 LSDVc	Model 4 LSDVc	Model 5 LSDVc
New teacher employments (lag)	0.47*** [0.08]	0.50*** [0.08]	0.49*** [0.07]	0.51*** [0.07]	0.53*** [0.08]
Teacher employment units	-2.02 [1.8]	-1.45 [1.7]	-2.04 [1.5]	-2.03 [1.5]	-1.43 [1.6]
Pupils	2.06** [1.0]	1.65* [0.9]	1.76** [0.8]	1.73** [0.8]	1.59* [0.8]
Tax revenues	0.056 [0.5]	0.19 [0.5]	0.2 [0.4]	0.22 [0.4]	-0.026 [0.4]
Debt	0.19 [0.3]	-0.10 [0.3]	-0.13 [0.3]	-0.15 [0.3]	-0.32 [0.3]
2 years before election year	-0.14* [0.07]				
1 year before election year		0.049 [0.07]			
Election year			0.20*** [0.07]		
1 year after election year				-0.085 [0.07]	
2 years after election year					-0.12 [0.07]
Observations	160	176	192	192	176
Number of id	16	16	16	16	16

Note: Bias corrected Least-Squares Dummy Variable (LSDVc) using Arellano and Bond (1991) for initial bias correction and $O(1/TN)$ to determine the accuracy of the approximation (Kiviet 1995), standard errors are bootstrapped with 1000 repetitions, standard errors are in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5 Determinants of *New Teacher Employments* using different dynamic panel estimators

	Model 1 LSDV	Model 2 GMM(AB)	Model 3 AH	Model 4 LSDVc (AB)	Model 5 LSDVc (AH)
New teacher employments (lag)	0.37*** [0.07]	0.40*** [0.08]	1.51 [1.5]	0.47*** [0.07]	0.54*** [0.08]
Teacher employment units	-2.17 [1.4]	-2.62 [2.0]	-1.31 [8.0]	-2.04 [1.5]	-2.42 [1.8]
Pupils	2.02** [0.8]	1.95* [1.1]	-0.75 [3.6]	1.88** [0.8]	2.20** [1.0]
Tax revenues	0.15 [0.5]	0.53 [0.5]	0.83 [1.0]	0.13 [0.4]	0.13 [0.5]
Debt	-0.031 [0.3]	-0.023 [0.3]	-1.1 [1.9]	-0.024 [0.3]	-0.023 [0.3]
Election year	0.31** [0.1]	0.29** [0.1]	0.17 [0.2]	0.32** [0.1]	0.34** [0.2]
SPD absolute majority	-0.14 [0.1]	-0.13 [0.1]	-0.15 [0.2]	-0.14 [0.1]	-0.14 [0.1]
CDU/CSU absolute majority	-0.12 [0.1]	-0.14 [0.1]	-0.29 [0.4]	-0.11 [0.1]	-0.095 [0.1]
Observations	192	176	160	192	192
Number of id	16	16	16	16	16
R-squared	0.55				
LM(AR1), chi2(1)	1.78				

Note: LSDV = Least-Squares Dummy Variable estimator (fixed effect estimator), LM(AR1) = Lagrange-multiplier test for first-order residual serial correlation in panel data (Baltagi 2001, p. 95), GMM(AB) = Arellano and Bond (1991) one step approach, AH = Anderson and Hsiao (1982) estimator using 2nd lag of difference, LSDVc = Bias-corrected Least-Squares Dummy Variable estimator (LSDVc) with standard errors bootstrapped with 1000 repetitions and $O(1/T)$ to determine the accuracy of the approximation (Nickel 1981), AB = Arellano and Bond (1991) for initial bias correction, AH = Anderson and Hsiao (1982) for initial bias correction, standard errors are in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.6 Determinants of change in total *Teacher Employment Units* using different dynamic panel estimators

	Model 1 LSDV	Model 2 GMM(AB)	Model 3 AH	Model 4 LSDVc (AB)	Model 5 LSDVc (AH)
Teacher employment units (lag)	0.83*** [0.05]	0.72*** [0.06]	0.64* [0.4]	0.92*** [0.05]	0.92*** [0.05]
Pupils	0.10*** [0.02]	0.16*** [0.03]	0.18 [0.2]	0.066*** [0.02]	0.066*** [0.02]
Tax revenues	-0.0022 [0.01]	-0.01 [0.01]	-0.011 [0.01]	-0.0044 [0.01]	-0.0046 [0.01]
Debt	-0.022*** [0.007]	0.0024 [0.01]	-0.021 [0.03]	-0.024*** [0.008]	-0.023*** [0.008]
Election year	0.003 [0.004]	0.0037 [0.004]	0.0019 [0.003]	0.0035 [0.004]	0.0033 [0.004]
SPD absolute majority	-0.001 [0.003]	0.0017 [0.003]	-0.00011 [0.004]	-0.00096 [0.003]	-0.00076 [0.003]
CDU/CSU absolute majority	0.0016 [0.003]	-0.0012 [0.004]	-0.0057 [0.006]	0.0013 [0.004]	0.0016 [0.004]
Observations	192	176	160	192	192
Number of id	16	16	16	16	16
R-squared	0.96				
LM(AR1), chi2(1)	0.13				

Note: LSDV = Least-Squares Dummy Variable estimator (fixed effect estimator), LM(AR1) = Lagrange-multiplier test for first-order residual serial correlation in panel data (Baltagi 2001, p. 95), GMM(AB) = Arellano and Bond (1991) one step approach, AH = Anderson and Hsiao (1982) estimator using 2nd lag of difference, LSDVc = Bias corrected Least-Squares Dummy Variable estimator (LSDVc) with standard errors bootstrapped with 1000 repetitions and $O(1/T)$ to determine the accuracy of the approximation (Nickel 1981), AB = Arellano and Bond (1991) for initial bias correction, AH = Anderson and Hsiao (1982) for initial bias correction, standard errors are in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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