

Partisan conflicts and parliamentary dominance: the Norwegian political business cycle

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Received: 14 June 2009 / Accepted: 28 January 2010
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Abstract The article explores the political business cycle in Norway from the early 1980s onwards. It is shown that unemployment growth is related to uncertainty about likely parliamentary majorities, and to the level of political conflict between such majorities. Data indicate that voter expectations are formed on the basis of likely majority winners in votes, not in seats. Unemployment growth is unrelated to sudden and unpredictable changes in the composition of government. This suggests that the instruments influencing unemployment growth are within the domain of the legislative, not the executive, power.

Keywords Political business cycle · Unemployment growth · Parliamentary majorities · Partisan conflict

1 Introduction

In rational partisan theory (RPT) the economy is described by a Lucas supply side function, rational expectations, and nominal rigidities in wage and price contracting (Alesina 1987, 1988). Partisan differences are assumed to exist. In particular, growth in output and employment above the natural rate is valued more by leftist than rightist parties.¹ After an election, therefore, leftist (rightist) majority-winners inflate the economy at a higher (lower) rate than rightist (leftist) majority-winners, and a post electoral boom (bust) is generated. The effect is temporary and fades away as existing wage and price contracts are replaced. The more surprising the election result is, and the more the parties differ in their valuation of employment, the stronger is the electoral impact.

¹Leftist parties may also care less than rightist parties about the costs of inflation, but such a difference is not necessary to drive the results.

Constructive comments to an early draft was generously provided by Kai Leitemo, Espen Moen, Christian Riis and Rune Sørensen. Helpful suggestions from two anonymous referees is greatly appreciated.

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RPT assume that partisan preferences are stable across electoral periods. Empirically, this is a questionable assumption.² Party manifestos are rewritten prior to each campaign, and are subject to interpretations after elections.³ As a result partisan preferences tend to vary across as well as within electoral periods. The model in Sect. 2 exposes partisan preferences to random exogenous shocks, inducing variance over time. Variation in partisan preferences is controlled for in the empirical specifications of Sect. 3. It turns out that such variation is a powerful predictor of partisan cycles.

A number of tests of RPT exist. One approach is to capture post-electoral business cycles by interventions that are turned on in t periods following an election (Alesina and Rosenthal 1995; Alesina et al. 1993a, 1993b, 1997: chapters 4 and 6). Another approach consists in comparing average growth and unemployment rates the first year(s) after elections with average growth and unemployment rates during the preceding two years (Paldam 1991, 1997). More sophisticated tests of RPT depart from surprise variables based on expectations about future policymakers. A common approach is to extract expectations about majority winners from vote-shares obtained by alternative majorities in polls using an options pricing model (Alesina et al. 1997: chapter 5, Cohen 1993; Carlsen and Pedersen 1999).⁴ An objection to this approach is that policy is decided by a majority of seats, not a majority of votes. Depending on the specifics of electoral rules, seat-shares may deviate substantially from vote-shares.⁵ For this reason we examine, in Sect. 3, the robustness of RPT to changing the expectations argument from votes to seats. It turns out that electoral surprises based on votes, rather than on seats, have the most pronounced effects on unemployment growth in our data. This is somewhat puzzling.

Traditionally, at least since Paldam's seminal article in 1979, the decision maker of interest in RPT is assumed to be the government. For this to give good meaning arbitrary restrictions have to be placed on data generated by parliamentary democracies.⁶ In this paper we take a different view, and assume that the parliamentary majority is the decision-maker of interest. This does away with the need for arbitrary restrictions on the data.

At the same time, governments are routinely replaced outside the electoral cycle in parliamentary systems. Such replacements constitute genuine surprises. For non-electoral replacements to produce political cycles governments must possess some amount of independent policy-making power. We investigate the policy-making powers of governments, by checking for the macroeconomic effects of non-electoral replacements. Such replacements do

²As emphasized by, for instance, Franzese and Jusko (2006).

³Manifestos are naturally viewed as incomplete contracts between the party congress (the principal) and elected representatives that make the decisions (the agents). Considerable variation in manifestos over time has been demonstrated, see for instance, Benoit et al. (2009) with references.

⁴An alternative is to use data from election-markets, as for instance in Fowler (2006). Election market data are not available for Norway over the period in question. Another sophisticated approach is to fit a set of regressions across elections, explaining the incumbent's vote share as a function of both opinion polls and macroeconomic variables. The predictions can then be used to compute election win probabilities for the incumbent. Chappell and Keech (1988) provides the pioneering work of this approach, while Carlsen and Pedersen (1999) represents a more recent application. The approach presumes some kind of retrospective voting based on economic performance of incumbents, which is not a part of RPT. Consequently, we do not use this approach.

⁵For instance, after 1945 eight different governments held a majority of the seats in the Norwegian parliament, none of which had a majority of the popular vote in national elections.

⁶See, for instance, Paldam (1979: 326): "In order to conduct a statistical study some very clear-cut criteria have to be found to decide whether a government is stable. Two rules have been used. They must both be fulfilled. Rule 1: The government should have a parliamentary majority. Rule 2: The government remains in power throughout the normal election period."

not have business cycle effects in our data, indicating that macroeconomic policy is placed firmly within the domain of parliamentary majorities in Norway.

Monthly unemployment data from the early 1980s onwards are used to check for the presence of a rational partisan business cycle. Existing tests employ yearly or quarterly data. Using monthly data allows us to keep better track of political events, and provides us with statistical power in series of relatively short duration. Series of short duration decreases the probability of drawing data from different politico-economic regimes. This makes short duration data desirable. In addition, Norwegian data are of course particularly suited to a test of the RPT, since Norway is the only western parliament with a fixed election term (as assumed in RPT).⁷

The article is organized as follows. The next section contains the model, while section three describes the data. Section 4 confronts model and data. Section 5 offers some conclusions.

2 Model

The economy is described by a simple supply function:⁸

$$u_t = u^* + w_t - \pi_t \quad (1)$$

In (1) u_t is the realized change in unemployment in period t , u^* is the (time invariant) natural rate of unemployment, π is inflation, and w_t is nominal wage growth. The ruling majority is assumed to control the inflation rate. It is further assumed that t is an election period, and that elections are held every second period. Nominal wage contracts are signed at the beginning of t before the election result is known, and terminate at the end of t .

Inflation is rationally expected and wage setters aim at keeping real wages stable, so that $w_t = \pi_t^e = E(\pi_t | I_{t-1})$, where I is the information set. I includes every decision-relevant fact known at time $t - 1$ (including the structure of the model that follows). Thus, (1) can be rewritten:

$$u_t = u^* + \pi_t^e - \pi_t \quad (2)$$

There are two, ideologically immobile, electoral alternatives—or blocks—for which the votes may be cast; these are denoted $k = (\text{Socialist, Conservative})$.⁹ The alternatives have

⁷RPT was developed as an explanation of the US cycle, in which electoral terms are fixed. Subsequently RPT has been tested on cross country samples, in which many of the countries do not have fixed terms. Apart from Norway, the only western parliamentary democracy that approaches a fixed term is Sweden after 1993. The qualifier “approaches” is due to the fact that, formally, the Swedish Riksdag can be dissolved. However, the new Riksdag only lives for the remainder of the original, fixed, four year term. For this reason, Sweden is often characterized as a fixed term system, despite the fact that parliament may be dissolved. The Norwegian Storting cannot be dissolved during the fixed four year election term. For empirical work on RPT in systems without fixed terms, see Ito (1990), Carlsen and Pedersen (1999), Alesina et al. (1993b).

⁸For micro-foundations and interpretations c.f. Blanchard and Fischer (1989, chapter 8).

⁹The electoral alternatives might be candidates, parties, parliamentary governments or legislative majorities (in seats or in votes). In the empirical analysis of the paper we focus on legislative majorities. The formalization that follows reduces the maximization problem to a one-dimensional choice. One may ask why the parties do not converge on the median position on this dimension. Several answers may be given. A good overview of partial convergence results is given by Alesina and Rosenthal (1995, chapter 2).

loss functions defined over unemployment growth and inflation:

$$\ell_t^k = \sum_{i=0}^{\infty} \delta^i \left[-\theta_t^k u_t - \frac{1}{2} (\pi_t - \pi^k)^2 \right] \tag{3}$$

In (3) $\delta < 1$ is the common discount factor, $\theta_t^k > 0$ is the marginal cost of higher unemployment and π^k is target inflation, with $\pi^{Soc} \geq \pi^{Con} \geq 0$.

As a slight modification of standard RPT, let θ_t^k be a stochastic variable in the range $(\underline{\theta}_t, \bar{\theta}_t)$. We take θ_t^k to be determined by independent and identical draws from a unimodal and symmetric distribution with mean $\tilde{\theta}_t^k$. This ensures that historical inflation rates do not convey information about future inflation rates.

Let $G(\theta_t^k)$ be the cumulative distribution function of θ_t^k , with corresponding density $g(\theta_t^k)$. Assume that $\underline{\theta}_t^{Soc} > \bar{\theta}_t^{Con}$. With this assumption, optimal inflation rates (to be determined) will obey the condition $\pi_t^{Con*} < \pi_t^{Soc*} \forall t$ (while allowing for variation in π_t^{k*} due to random shocks). The larger is $g(\tilde{\theta}_t)$, of course, the smaller are the shocks.

For any pair $(\theta_t^{Soc}, \theta_t^{Con})$ that satisfies $(\theta_t^{Soc} - \underline{\theta}_t^{Soc}) = (\theta_t^{Con} - \underline{\theta}_t^{Con})$, it is assumed that $g(\theta_t^{Soc}) = g(\theta_t^{Con})$. Or in words, the distribution around the mean of the optimal inflation rate is identical for the electoral alternatives. For this reason risk attitudes do not influence wage setting.

The draw of θ_t^k is revealed for every player when a winner sets policy. Policy is set after elections in election periods, and after wage contracts have been signed in non-election periods.

The model is solved using a backwards induction argument.¹⁰ Inserting (2) in (3) and maximizing with respect to π_t we obtain the optimal rates of inflation:

$$\pi_t^{k*} = \theta_t^k + \pi^k = \pi_t \tag{4}$$

In what follows we set $\pi^{Soc} = \pi^{Con} = 0$ without loss of generality. Voters are assumed to have loss functions of the type presented in (3), and to vary in their optimal inflation rates. Votes are cast for the electoral alternative promising the smallest loss. There is uncertainty about the distribution of optimal inflation rates in the voter population, and the electoral outcome is therefore uncertain. Let P signify the probability that the socialist block gains a majority in the upcoming election.¹¹ The expected (post-election) inflation rate for period t is then:

$$\pi_t^\epsilon = P \tilde{\theta}_t^{Soc} + (1 - P) \tilde{\theta}_t^{Con} \tag{5}$$

In period $t + 1$ the majority in charge is common knowledge, and contracts are based on either $\pi_{t+1}^\epsilon = \tilde{\theta}_t^{Soc}$ or $\pi_{t+1}^\epsilon = \tilde{\theta}_t^{Con}$ depending on which block won the election in t .

Finally, inserting (5) and (4) in (2) provides the unemployment-growth equations for the two electoral alternatives:

$$u_t^{Soc} = u^* + (1 - P) (\tilde{\theta}_t^{Con} - \theta_t^{Soc}) \leq 0 \tag{6}$$

$$u_t^{Con} = u^* - P (\theta_t^{Con} - \tilde{\theta}_t^{Soc}) \geq 0 \tag{7}$$

¹⁰Intra-party conflicts due to last term effects for some members are assumed away. For an overlapping generation model where such effects are taken account of, see Alesina and Spear (1988).

¹¹Roemer (1992) derives this probability from primitives (c.f. also Alesina and Cukierman (1990) for some special results).

Thus, a change in the rate of unemployment can either be caused by an electoral surprise (in an election period), or a random shock due to intra-party bargaining over the marginal costs of unemployment (in any period). The central implication is that unemployment increases (decreases) following a conservative (socialist) victory. This effect is stronger: a) the more unlikely (likely) a socialist victory is, and b) the more pronounced the political conflict between the two blocs is.

With added costs in terms of notational complexity the model may be extended to a multi period setting (Carlsen and Pedersen 1999). This formalizes the nature of the post-electoral cycle, where the effect of a surprise gradually fades away as contracts are rewritten. Since this aspect is quite intuitive, we content ourselves by dealing with it in the empirical specifications.

3 Data

Our electoral data cover 342 months, starting with 1976:12 and ending with 2005:05. The electoral alternatives are defined in terms of socialist and non-socialist majorities.¹² As socialist parties we include the Labor party and every party to the left of it on the left-right scale of politics, as tapped by voter self-placements in National election surveys.¹³

Two kinds of expectations are computed: the probability of having a socialist majority in votes after the upcoming election, and the probability of having a socialist majority in seats after the upcoming election.¹⁴ Both computations depart from monthly polls.¹⁵ To calculate the probability of a socialist majority in seats, polls are first transformed into seats using the program CELIUS. The program takes into account various (minor) changes in electoral rules that have been implemented during the period.¹⁶

For any given month the vote-shares (seat-shares) of the socialist parties are added. The probability of a socialist majority vote-share (seat-share) in the election $\tau - t$ months ahead is then computed applying the option pricing method.¹⁷ Norwegian national elections are

¹²Party orderings across studies using various methods (expert placements; voter self placements; manifesto analysis) are generally not stable in the case of Norway. Instability in orderings may indicate significant multi-dimensionality. However, demonstrated instability may also be due to methodological problems. Rasch (2003) departs from a consistent social choice framework, and shows that this produces estimates of ideal-points in the Storting that are stable and ordered on a single dimension. This dimension is interpreted as the standard left-right dimension in politics. In our context, which is limited to partisan views on the desirability of fiscal activism, a traditional ordering on the left-right dimension seems defensible.

¹³Obtainable at Norwegian Social Science Data Services (<http://www.nsd.uib.no/>). We count as socialist parties: Socialist Left Party (Sosialistisk Venstreparti) and Labor Party (Arbeiderpartiet). The remainder is counted as non-socialist: Center Party (Senterpartiet); Liberal Party (Venstre); Christian Peoples Party (Kristelig folkeparti); Conservative Party (Høyre); and Progress Party (Fremskrittspartiet).

¹⁴The Norwegian system is fairly stable in terms of proportionality over the time period in question (approximately score 5 on the simple Gallagher (1991) disproportionality index normalized on 0 to 100). Nonetheless, small deviations in partisan vote shares on a national basis may produce big and surprising shifts in partisan seat shares (due to aggregation of votes from 19 districts with geographical disproportionality in seats through a complicated formula). The result, as is evident from Figs. 1a and 1b below, is that the probability of a socialist majority in seats and votes respectively deviates quite substantially, despite stability in proportionality at the national level. It is the probability of electoral replacement that is of central concern in RPT.

¹⁵Recorded by MMI on a monthly basis since 1976:12.

¹⁶Program developed by Bernt Aardal at the Institute for Social Research, Oslo (<http://home.online.no/b-aardal/>).

¹⁷Pioneered by Black and Scholes (1973), and introduced to political economy by Cohen (1993).

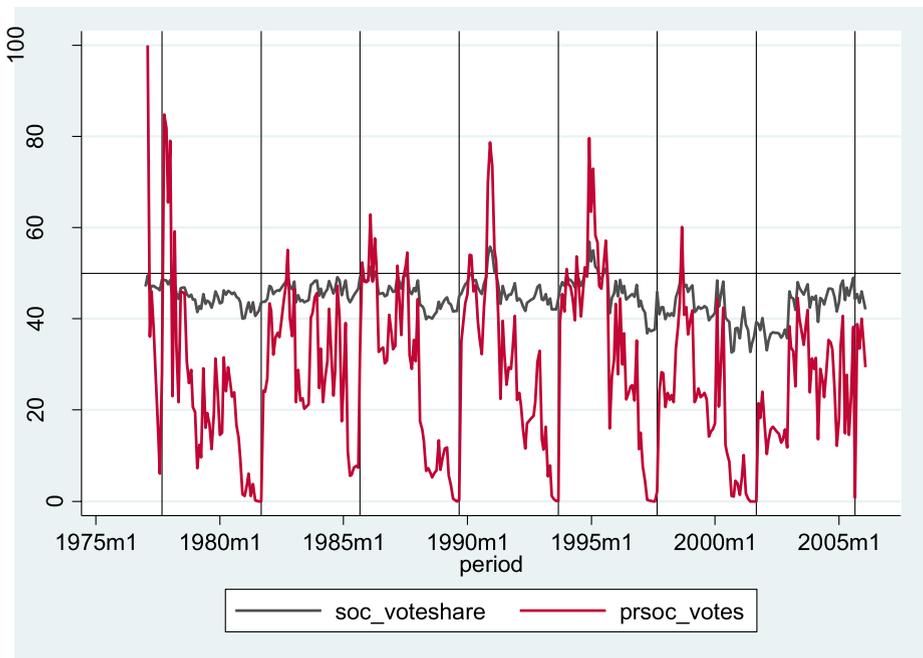


Fig. 1a Socialist share of votes and probability of a socialist majority in votes

fixed, and occur in September at four-year intervals (so $\tau - t$ ranges between 0 and 48 months).

The probability of a socialist majority in the upcoming election, P_t , is contingent on the number of months remaining before the election, the current vote-share (seat-share) of the socialist block ($x_t^{Soc} = \text{Socialist seatshare, Socialist voteshare}$), the mean monthly change in the polls μ_t , and the standard deviation of month-to-month changes in the polls σ_t . The mean and standard deviation of changes are calculated using a cumulative moving average technique, utilizing data from the first available poll (1976:12) up to and including the present month (t).

Vote-shares (seat-shares) are converted into probabilities for a socialist majority by the following formula, where Φ signifies the standardized, cumulative, normal distribution (and movements in the polls are assumed to be i.i.d.):

$$P_t = \Phi\left(\frac{x_t^{Soc} + \mu_t \tau - 0.5}{\sigma_t \sqrt{\tau}}\right) \quad (8)$$

Figure 1a displays the probability of a socialist majority in votes, and the socialist vote share on a month-to-month basis. Figure 1b displays the corresponding probability of a socialist majority in seats, and the socialist seat share on a month-to-month basis.

From the estimated probabilities of a socialist majority in the upcoming election our surprise variable is constructed in the following way:

$$\text{Electoral Surprise} = D_t^{Soc} - \frac{1}{N} \sum_{i=0}^{N-1} P_{t-i} \quad (9)$$

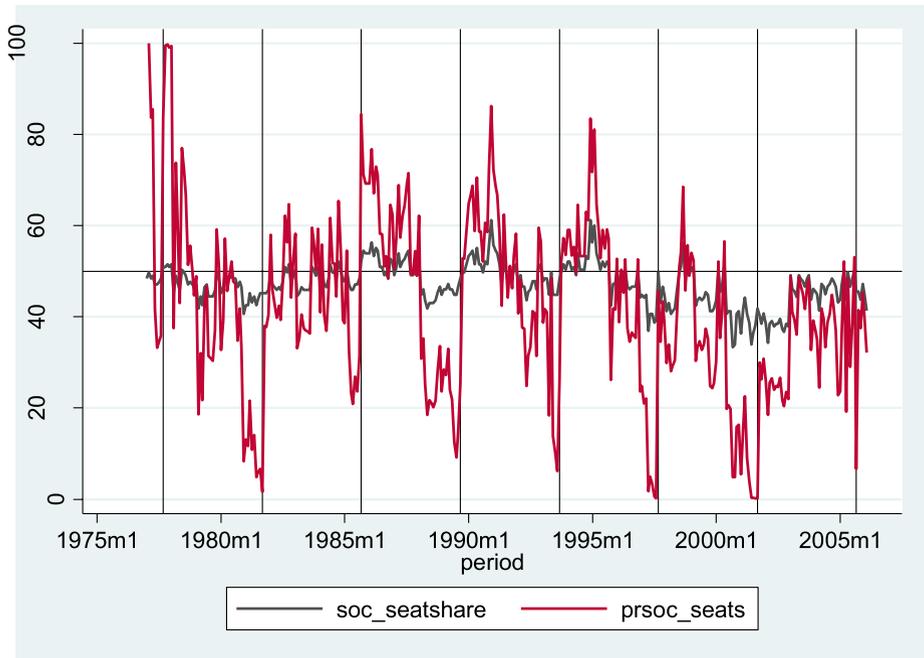


Fig. 1b Socialist share of seats and probability of a socialist majority in seats

In (9) N is the length of a nominal wage contract and D_t^{Soc} is a dummy that takes the value one after a socialist victory, and zero after a non-socialist victory. Since we lack firm knowledge about the precise term structure of wage contracts, we assume that they are signed uniformly across time. The surprise variable captures the expected post-electoral change in employment, with a positive sign for socialist majorities and a negative sign for non-socialist majorities. The magnitude of the surprise determines the magnitude of the effect on employment, and as contracts are rewritten the effects of the electoral surprise on employment fades away.

We follow the convention of denoting a surprise variable calculated on the probability of a socialist vote share (*Vote Surprise*) $_t$, while a surprise variable calculated on the probability of a socialist seat share is denoted (*Seat Surprise*) $_t$. The surprise variables are based on nominal wage contracts of 24 months duration, and 12 months lag for policy to work. Both choices are based on best fit in our data.

According to (6) and (7) the effect on employment also depends (multiplicatively) on the magnitude of partisan disagreements.¹⁸ We capture the magnitude of such disagreements by a measure based on fractional statements by the Labor party and the Conservative party—

¹⁸Since the theoretical model suggests a multiplicative term, we include this product alone rather than specifying the full interaction model. This is in line with for instance the specifications used by Alesina et al. (1997: 198) to model the multiplicative effects of partisanship and election in business cycle models. Further discussion on the appropriateness of including the product alone is given by Kam and Franzese (2007: 99–102).

the two major parties in each bloc—on budgetary matters in parliament.¹⁹ Only statements recorded in the Finance committee are used.²⁰ The Finance committee is *the* coordinating committee in the parliaments economic policy making, and economic policy making is primarily made in the budget.²¹

Denote fractional statements (statements from the majority and minorities) that both parties participate in by S_{ij} , while statements that only one of the two parties participates in is denoted S_i and S_j respectively. Define the statement score of party i as $\frac{S_i}{S_i+S_{ij}} \equiv f_i$, and the corresponding statement score of party j as $\frac{S_j}{S_j+S_{ij}} \equiv f_j$. The statement scores are simply the fraction of statements that one party did not have in common with the other party. The disagreement score of the two parties is defined as:

$$0 \leq \frac{1}{2}(f_{i,t} + f_{j,t}) \equiv (Disagreement)_t \leq 1 \quad (10)$$

The score in (10) is calculated on an annual basis, but follows parliamentary sessions from June to June rather than the calendar year.

One may of course suspect that the disagreement score varies with the parliamentary base of governments, through the opposition's incentives to produce statements. To check for this suspicion we regress the disagreement score on dummies for coalition governments and majority governments, as well as a time trend:

$$Disagreement = - .411 + .016 Coalition - .035 Majority + .001t \quad (11)$$

[-4.91]
[1.08]
[-1.36]
[10.88]

The estimate is based on robust regression for years greater than 1978 (the first year OECD data on changes in G7 unemployment are made available, and where our analysis starts). As can be seen, only the constant and the time trend are significantly different from zero at conventional levels (t -values are reported in brackets). The F -statistic for the model is highly significant.

With this as a background, we use the product of the disagreement score in (10) and the surprise variable in (9) to capture the implications of RPT, as they are stated in (6) and (7).

There are four replacements of governments outside of elections in our data.²² They all go in a leftist direction as measured by party-voter's self-placements in National Election Surveys. In June 1983 the Conservative Party's (CoP) minority government was supplemented with the Center Party (CeP) and the Christian Peoples Party (CPP). In May 1986 a non-socialist minority coalition composed of CoP, CeP and CPP was replaced by a minority government consisting of the Labor Party (LP). In October 1990 a non-socialist minority coalition composed of the CoP, CeP and the CPP was replaced by a minority government consisting of the LP. Finally, in April 2000 a non-socialist minority coalition composed of the CeP, CPP and the Liberal Party was replaced by a minority government consisting of LP.

Replacements of this kind are genuine surprises that cannot be predicted with any confidence *ex ante*. To the extent that governments possess discretionary powers to inflate

¹⁹Data on fractional statements have been used in previous studies by, amongst others, Schaffer (1998) and Rommetvedt (2003), to describe distances between legislative parties and coalitions in the Storting.

²⁰Obtainable at Norwegian Social Science Data Services (<http://www.nsd.uib.no/>).

²¹Norwegian parliamentary committees are organized along party lines, and deviations from the party line are very rare occurrences.

²²Remember that there are only five elections in the data set, so the data are not particularly biased in favor of governmental change in elections.

Table 1 Descriptive statistics 1982:01–2005:05

	Observations	Mean	STD	Min	Max
u_t	281	.046	.242	−.310	1.042
U_t	281	4.26	1.37	1.90	6.80
u_t^{G7}	281	−.005	.066	−.113	.220
Seat-surprise $_t$	281	−.015	.036	−.207	.000
Vote-surprise $_t$	281	−.005	.014	−.103	.000
Gov-change $_t$	281	.036	.106	.000	.500
Disagreement $_t$	281	.536	.124	.300	.810

(deflate) the economy and increase (decrease) employment, one would expect them to exploit genuine surprises created by replacements outside of elections. Denote the measure of replacement-surprises ($Gov-change$) $_t$. Since all replacements in our data go to the left we may define this measure as:

$$(Gov-Change)_t = \sum_{t=T}^{T+N-1} \frac{1}{N} \quad (12)$$

In (12) $t = T$ is the month of government replacement. The measure starts out with unity in $t = T$, and is reduced by increments of $1/N$ each month until month $t = T + N$ where all contracts have been rewritten and the measure reaches a steady state of zero. The measure captures the idea that a surprise government can fully exploit existing contracts until they are rewritten, provided that it has the discretionary powers to set policy independent of the current parliamentary majority. Since we are looking at a socialist surprise, the effect on changes in employment is taken to be positive. To ensure comparability with the electoral surprise variables calculated from (9), Government change is also based on contract lengths of 24 months and a lag of 12 months for policy to work.

Our last political variables are a set of intervention terms that are turned on in the twelve, nine, and six months preceding an election respectively. The intervention terms are labeled $D_t(Election-12)$, $D_t(Election-9)$ and $D_t(Election-6)$ (with obvious reference). They represent controls for an opportunistic cycle, in which any ruling majority will try to create a pre electoral boom in order to get reelected (Nordhaus 1975; Lindbeck 1976; Clark et al. 1998; Clark and Hallerberg 2000).

Table 1 presents descriptive statistics for relevant variables. A few things are worth noting. Firstly, the surprise variables (calculated on the basis of votes as well as seats) have maximums at zero. Thus, there where no surprising socialist majorities during the period in question. The reason is simply that no socialist majority in either seats or votes materialized in the period analyzed. Secondly, the variable capturing extra-electoral government changes has a maximum of 0.5 and a minimum of zero. The reason is that such changes are assumed to be genuinely surprising, that all such changes in the period go in the left direction (+1), and that a contract length is 24 months, but policy only starts working 12 months after an election. Lastly, Norwegian unemployment growth has wider extreme bounds and larger standard deviations than unemployment growth in the G7 countries. This is so firstly because we calculate percentage growth, and because the unemployment level in the G7 countries far exceeds the Norwegian unemployment level. Secondly, the standard deviation of the G7 growth is the standard deviation of an average, which tends to bring it down.

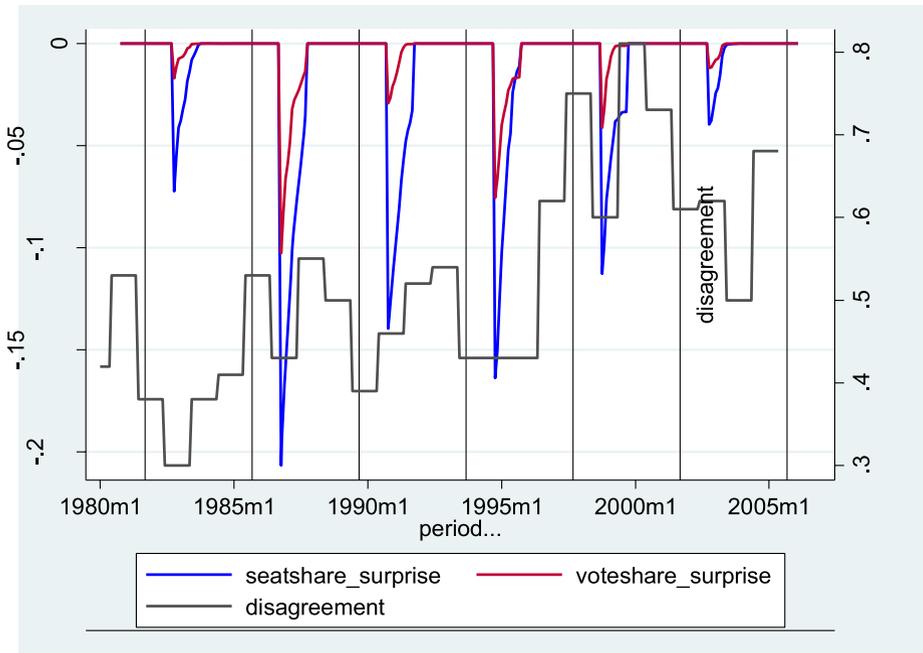


Fig. 2 Surprise variables and partisan disagreement

Figure 2 displays timelines for our three central independent variables: the surprise variables calculated on the basis of (9) for seat-shares and vote-shares respectively, and the disagreement score calculated on the basis of (10). The vertical lines represent election dates.

4 Results

The following econometric specification is used:

$$u_t = \alpha_0 + \beta U_t + \sum_{i=1}^{48} \lambda_i u_{t-i} + \sum_{i=0}^{48} \delta_i u_{t-i}^{G7} + \varphi POLITICS + \varepsilon_t \quad (13)$$

In the specification U_t is the level of Norwegian unemployment, u_t is the change in Norwegian unemployment, u_t^{G7} is the change in unemployment in the G7 countries, $\varphi POLITICS$ represents the vectors of coefficients and variables capturing politically relevant variables, and ε_t is an error term assumed to be iid with zero expectation. Both Norwegian and G7 u 's are measured as annualized changes in the unemployment rate, that is, $u_t \equiv \frac{U_t - U_{t-12}}{U_{t-12}}$.

Since G7 unemployment data start in 1978:01, and since we employ a lag of 48 months, our analysis starts in 1982:01. Observations end in 2005:05. The data set contains five elections (1985, 1989, 1993, 1997, and 2001). Monthly polls back to 1976:12 are utilized in the calculation of win-probabilities.

The vector of politics includes measures of electoral surprise based on socialist vote-shares and seat-shares respectively, genuine surprises following government replacements

Table 2 Maximum likelihood estimates (p -values). Dependent variable: annualized change in Norwegian unemployment rates. 48 months lagged annualized change in Norwegian and G7 unemployment rates included but not reported

	Model						
	I	II	III	IV	V	VI	VII
<i>Seat-surprise_t</i>	-.140 (.002)						
<i>Seat-surprise_t × Disagreement_t</i>		-.274 (.049)					
<i>Vote-surprise_t</i>			-.331 (.024)				
<i>Vote-surprise_t × Disagreement_t</i>				-.660 (.051)	-.581 (.093)	-.581 (.093)	-.703 (.047)
<i>D_t(Election-12)</i>					.004 (.430)		
<i>D_t(Election-9)</i>						.004 (.411)	
<i>D_t(Election-6)</i>							.003 (.651)
<i>Gov-change_t</i>	.017 (.530)	.022 (.414)	.014 (.601)	.020 (.471)	.016 (.557)	.025 (.348)	.016 (.554)
<i>U_t</i>	-.002 (.157)	-.002 (.249)	-.003 (.111)	-.003 (.184)	-.002 (.232)	-.002 (.232)	-.003 (.148)
<i>Constant</i>	.009 (.252)	.008 (.359)	.012 (.162)	.010 (.241)	.010 (.230)	.009 (.261)	.010 (.211)
<i>N</i>	281	281	281	281	281	281	281
<i>Log-L</i>	586	573	586	573	573	573	573

outside elections, disagreement scores based on fractional statements, and intervention terms capturing pre-electoral months.

The usual time series techniques were employed. Dicky-Fuller tests for stationarity were carried out, and autocorrelation was accounted for by lagging the dependent variable. The optimal number of lags was determined by the Breusch-Goodfrey Lagrange multiplier test. Finally, we estimated the regressions by maximum likelihood, using robust standard errors, in order to weed out heteroscedasticity. Results are presented in Table 2.

Note first that the coefficients of both surprise variables have signs in accordance with RPT, and are significant at conventional levels (models I and III): The more the election result deviates from expectations, the more unemployment growth changes. This is in opposition to Carlsen and Pedersen (1999), who fails to find significant effects of RPT using quarterly Norwegian output data from the late 1970s to the early 1990s. In our data a non-socialist surprise significantly accelerates unemployment growth, while a socialist surprise significantly decelerates it. The coefficient of surprises calculated on the basis of vote-shares are almost two and a half times as strong as the coefficient of surprises calculated on the basis of seat-shares. This is somewhat odd, given that policy is decided by a majority of seats, not a majority of votes.

A possible explanation lies in a particular assumption underlying the calculation of seat-shares from polls in CELIUS, namely that the party vote registered in any monthly poll follows the geographical distribution from the last election. Direct empirical evaluation of the assumption is not feasible since monthly polls are drawn from a national sample that is unrepresentative when broken down by election districts. Nevertheless, assuming a stable geographical distribution over 48 months at a time seems excessively restrictive. This is all the more so since the geographical distribution of the party vote tends to change quite a bit over elections.

A more fundamental challenge is that seat-shares may respond quite violently to minor changes in vote-shares. For example, transferring one percentage point of the popular vote from the Labor party to the Conservative party in the election of 2005, would induce a loss of five Labor party seats. These seats, however, would be distributed to the non-socialist parties with one seat each for the Christian Democratic Party and the Progress party, and three seats for the Conservative Party. The example illustrates that aggregating seat-shares from 19 electoral districts that use a complicated PR system, may produce surprising results.²³ Consequently, requiring agents to be able to transform vote-shares from polls into consistent beliefs about the probability of a socialist majority in seats, may well be asking too much. Basing beliefs directly on vote-shares may constitute a workable proxy for the agents.

Second, we notice from Table 2 that the products of the surprise variables and the political disagreement variable have signs in accordance with the expectations from RPT, and are significantly different from zero at conventional levels (models II and IV). Thus, as before, unemployment accelerates after a non-socialist surprise, and decelerates after a socialist surprise. However, a higher level of disagreement now induces a greater change in the unemployment growth for a given electoral surprise. As can be seen, the absolute values of the coefficients on the surprise variables are approximately doubled, when multiplied by the level of disagreement (compare models I and II, and models III and IV respectively). Since the average level of disagreement is approximately 1/2 (c.f. Table 1), however, the coefficient estimates of the products are in broad agreement with the coefficient estimates of the surprise variables (models I and III).

Third, we observe that the pre-electoral dummies are not significantly different from zero at conventional levels in any of the specifications of Table 2 (models V, VI and VII). Thus, once we check for the determinants of rational partisan cycles, there is no indication of adaptive cycles of the opportunistic kind.²⁴

Finally, an interesting finding is that genuinely surprising changes in the partisan composition of government do not significantly alter the growth of unemployment for any of the models in Table 2. We interpret this in the following way: The crucial policies affecting unemployment are effectively set by parliamentary majorities, indicating that parliament has overcome agency problems in this policy area. We note that Alesina et al. (1997: 148–163) fail to find statistically valid evidence of a Norwegian rational partisan cycle in quarterly unemployment data over the period 1972:1–1993:4. The authors employ a crude test with

²³The current system allocates seats by use of the St. Lägues method with 1.4 as first divisor in 19 electoral districts with magnitudes between 4 and 17 mandates. The first 150 mandates are allocated in the electoral districts, while the last 19 mandates are allocated on a national basis using the same formula. There is also pronounced geographical disproportionality in the system, with the ratio [seat-share/vote-share] ranging from 0.8 to 1.4 over the electoral districts. The system has been subjected to various changes over the period in question. Such changes are taken account of in CELIUS.

²⁴Opportunistic cycles with adaptive expectations where originally conceived by Nordhaus (1975) and Lindbeck (1976). Recent refinements and tests are found in Clark and Hallerberg (2000) for debt and monetary aggregates, and in Clark et al. (1998) for unemployment and growth.

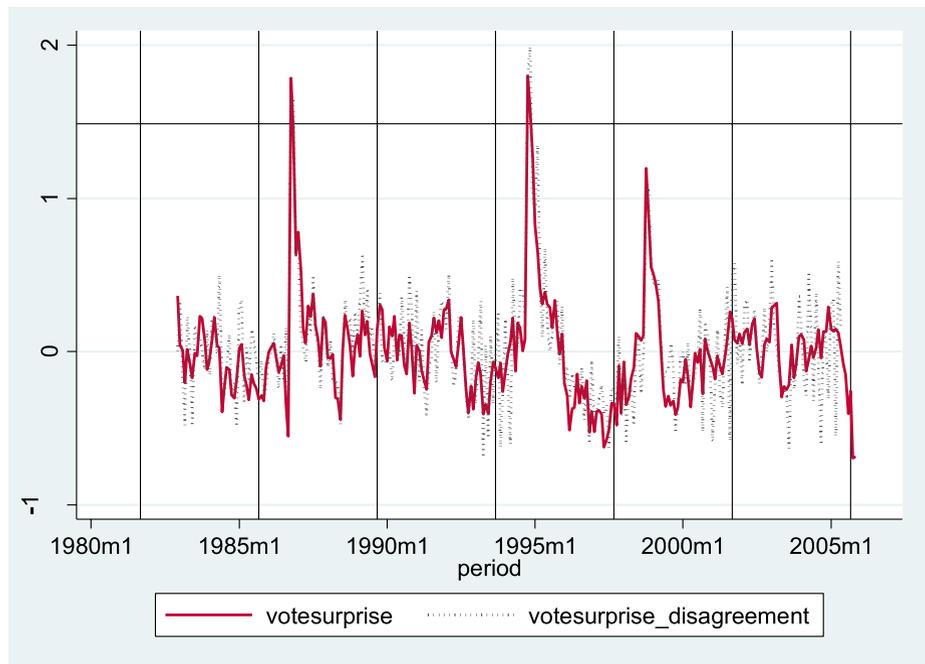


Fig. 3 Effects of vote surprise and disagreement. Percentage change in annualized unemployment

interventions that are turned on in a specified number of quarters after a change in government, whether such a change takes place in elections or not. Our findings indicate that their result may have been produced by a badly specified political variable that confuses electoral surprise related to relevant majoritarian decision makers with non-consequential changes in government.

What, then, are the substantive effects of electoral surprises on unemployment growth? Figure 3 shows two differences. First, the difference between the values predicted by model III and the values predicted by (13) with no political variables included (*votesurprise*). Second, the difference between the values predicted by model IV and the values predicted by (13) with no political variables included (*votesurprise-disagreement*). These differences convey the substantial effects of policy surprises on unemployment.

The strongest effects are found after the elections of 1985 and 1993. About a year after these elections (when policy starts to work), unemployment grows by approximately 2%, and thereafter gradually returns to trend (as contracts are replaced). As is evident from Fig. 2, both of these elections saw sizeable socialist surprises (a vote share surprise of about 8 to 10%), while the level of disagreement was fairly low.

The elections of 1989 and 1997 both saw moderate, and comparable, socialist surprises (again calculated in vote-shares). While the 1997 election was followed by an unemployment growth above 1% a year after election, no policy effect is evident after the 1989 election. Figure 2 indicates why. While the level of disagreement in the 1989 election was slightly below average, disagreements in the 1997 election reached the highest level in the period.

The elections of 1981 and 2001 were equally unsurprising (calculated in vote-shares). The absence of a deviation between expectations and realizations in these elections hindered

a partisan cycle in unemployment growth (notwithstanding the fact that partisan differences were quite pronounced in the 2001 election).

5 Conclusions

Tests of RPT in parliamentary systems have commonly assumed that the authority to set policy in the macro-economic sphere rests with the government. The present article questions this assumption on empirical grounds. Having accounted for the electoral surprises relating to parliamentary majorities, extra-electoral changes in the composition of governments add nothing to the explanation of a Norwegian rational partisan cycle in unemployment growth. A conjecture that should be put to the test is that the same holds true for other parliamentary systems.

A possibility suggested by Alesina and Rosenthal (1995: 249–153) is that voters engage in strategic balancing pitting a minority government against a legislative majority, or voting in order to produce a multiparty government, in order to create checks and balances that render manipulation difficult. However, strategic voting in a proportional system like the Norwegian one is unlikely to occur. Due to the complexities of the system, voters are likely to cast their votes sincerely, and leave the composition of governments to the elected representatives (Cox 1997). In support of this, controlling the regressions for minority governments and coalition governments does not alter the results presented.

Given that macro-economic policy is the domain of majorities in parliamentary systems, agent's expectations ought to be based on likely majority winners in seats, not in votes. This is so simply because policy is set by a majority in seats. However, our data does not support the conjecture that voters form beliefs about likely majorities in seats. This finding may certainly result from measurement errors in our seat-surprise variable. More fundamentally, however, we contend that difficulties in forming consistent beliefs about likely majority-seat winners are severe in multi-constituency PR systems. Our conjecture is that rational agents may instead use vote-shares from the polls as a proxy in forming such beliefs. More research should be directed towards gaining a firmer understanding of belief formation in multi-constituency PR systems.

A cornerstone in RPT is the implication that the more electoral alternatives deviate in terms of policy preferences for a given electoral surprise, the more should output and unemployment react to a change of policy makers. Surprisingly, this implication has not been tested previously. Using disagreement scores from Norwegian political history, we obtain support for this important implication in our data. Thus, the magnitude of rational political cycles is contingent on the disagreements between electoral alternatives. At least this holds for fluctuations in Norwegian unemployment growth since the early 1980s. If this finding is general, as theory claims it is, electoral surprises of comparable magnitudes could lead to widely different fluctuations in real variables like unemployment growth. Future research in RPT should explore such contingencies.

This being said, there are several reasons why one might expect particular political and institutional circumstances of Norway in the period analyzed to drive results. First, compared to other countries Norway scores high on general indexes of opposition influence (Strøm 1990; Bingham-Powell 2000). Second, the Norwegian Storting has wide amendment powers in general, and particularly in the fiscal budget (Helland 2000). Third, the committee system of the Storting is strong compared to many western national assemblies (Mattson and Strøm 1996; Bingham-Powell 2000). Qualifications such as these, of course, only strengthens the argument for including a parliamentary perspective in future research on RPT. It

might well be that the real effects of electoral uncertainty about legislative majorities are weaker in countries with stronger governments than the Norwegian one.

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