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Cabinet Durability and Fiscal Discipline *

David Fortunato
Assistant Professor
Texas A&M University
fortunato@tamu.edu
2010 Allen Building
4348 TAMU
College Station, TX 77843-4348

Matt W. Loftis
Assistant Professor
Aarhus University
mattwloftis@ps.au.dk
Bartholins Allé 7
8000 Aarhus, Denmark

Abstract

Government durability is critically important, yet the supply of research devoted to its causes far exceeds the supply of research devoted to its consequences. We argue that short government durations in parliamentary democracies increase public spending by driving a political budget cycle. We present a revision of the standard political budget cycle model that relaxes the common (often implicit) assumption that election timing is fixed and known in advance. Instead, we allow cabinets to form expectations about their durability and use these expectations to inform their spending choices. The model predicts that 1) cabinets should spend more as their *expected* term in office draws to a close and 2) cabinets that outlive their expected duration should run higher deficits. Using data from 15 European democracies over several decades, we show that governments increase spending as their expected duration withers and run higher deficits as they surpass their forecasted life expectancy.

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Cabinet durability has inspired a vibrant theoretical and empirical literature in comparative political economy. Ostensibly, political economists study government stability, a concept Laver and Shepsle (1998) describe as “self-evidently important,” under the assumption that it is salient to real policy outcomes. However, just which outcomes are conditioned by durability, and how, remain open questions. Here, we take an important step toward answering these questions by presenting theoretical and empirical analyses of the relationship between government durability and public spending. We argue that governments with shorter life expectancies face more immediate pressure to spend at higher rates in order to accrue electoral support — speeding up a political budget cycle which would otherwise see spending crescendo in advance of *scheduled* elections. Our theoretical discussion yields two testable implications: 1) governments spend more as their life expectancy withers and 2) governments that outlive their expected durations will run higher deficits than governments that do not surpass their life expectancy. Our empirical tests reveal support for these hypotheses and imply that, in expectation, government stability has a substantial positive impact on fiscal well-being.

In presenting our arguments and analyses, we make several substantively significant contributions. Our primary empirical findings improve both our understanding of public spending and debt accumulation and take an important and overdue step toward understanding the real policy implications of government durability. Further, by taking into account the uncertainty of cabinet life expectancies, we uncover a possible resolution to a longstanding discord in the literature on political budget cycles where there is theoretical consensus on the central prediction but weak, or institutionally dependent empirical evidence for it in advanced democracies. That is, nearly all previous political budget cycle studies have assumed that elections are fixed, but only about 18% of European cabinets survive the maximum constitutional interelection period (CIEP).¹ By relaxing the assumption of fixed elections and allowing the cabinet to fore-

¹Data taken from (Seki and Williams 2014). We define the maximum as 95% or greater. In countries with a 4 year CIEP, by far the most common length, the remaining 5% corresponds to 73 days. The typical cutoff point for defining early elections is 60 days or more before CIEP

cast its durability, we uncover the evidence of cycling behavior in advanced Western European democracies that has eluded so many of our predecessors. Indeed, models including expected duration provide significantly more explanatory power for observed spending patterns than models including true duration.

Moving forward, we briefly discuss the extant literature on both cabinet durability and public spending, highlighting the opportunity for studying the implications of cabinet stability in the former and the discord between theoretical and empirical studies of political budget cycles in advanced democracies in the latter. We then present our theoretical approach to the question, derive our hypotheses, and move on to describe our research design. Using public spending data from fifteen Western European democracies over a period of roughly fifty years, we find robust empirical evidence for our central predictions.

GOVERNMENT SURVIVAL

As comparativists may recall, beginning in the 1970s and extending through the 1990s the literature on government durability was dominated by debate between the “attributes” and “events” approaches to the question.² In short, the attributes approach conceived of government durability as a function of characteristics which can be observed at the time of the cabinet’s formation (e.g., Warwick 1979; Strøm 1985). That is, governments in general are made more durable by activating certain features (majority status, for example) and coalition governments in particular can be made more stable by selecting partners that are compatible

expiration (e.g., Schleiter and Tavits 2016).

²As Laver (2003) notes, “durability” and “duration” are distinct concepts. Where durability is a latent quality that may be described, but is inherently unobservable, duration, the amount of time a government spent in office, is observable. We think of duration as a realization of a random variable durability. Here, we are interested in durability for the purposes of predicting duration, but we use these terms, and others, such as “stability,” interchangeably.

on salient policy dimensions. The events approach, by contrast, argued that cabinet stability is primarily a function of stochastic shocks to the political environment (e.g., Browne, Frendreis and Gleiber 1984). Warwick (1994) provides a thoughtful and detailed discussion of this debate.

The rift between these competing perspectives eventually gave way to an acceptance that both attributes and events were salient to government longevity and that researchers must integrate both into a hybrid model that considers how cabinets' characteristics make them more or less likely to survive the various shocks they may experience, or, how certain shocks may have the potential to reshape the properties of the status quo government relative to its alternatives. The incorporation of the two approaches into a unified framework is now the dominant theoretical perspective on government stability and exemplified by Lupia and Strøm (1995), Laver and Shepsle (1998), and Diermeier and Stevenson (2000), who argue that the relevant "events" are shocks to the political environment that alter the distribution of bargaining power across legislative parties, making alternatives to the status quo government more or less attractive, and therefore making termination more or less likely.

These theoretical innovations went hand-in-hand with empirical advances that sought to model the attributes or events approaches separately (e.g., Strøm 1985; Browne, Frendreis and Gleiber 1986, respectively), before moving on to harmonize them. One could argue that, in this respect, the empiricists were a step ahead of the theorists, with King, Alt, Burns and Laver (1990, 847; authors' emphasis) presenting a "statistically unified model that can be used to explore the impact of particular attributes upon cabinet *durability* (expected duration), while maintaining the assumption that the *duration* of any particular cabinet will ultimately be determined by a stochastic process, such as the random incidence of terminal events" — a model that correctly predicts government duration within four months on average.

This is not the case in regards to the next major hurdle in the literature: recognizing, and subsequently modeling, the selection problem in cabinet durability. As early as De Swaan (1973), theorists had understood that durability was a critical concern in the formation of

governments, but it would be several decades until empiricists began to engage this issue in earnest (e.g., Merlo 1997; Diermeier, Eraslan and Merlo 2003) and later still before a solution to the selection problem was proposed to jointly model formation and duration was proposed by Chiba, Martin and Stevenson (2014), who find significant differences between the correlates of durability when selection is and is not accounted for. In the interim, we have learned that a government’s durability is a function of events and attributes such as majority status, the complexity of the bargaining environment, the number of and size of antiestablishment parties in parliament, and ideological compatibility within the government, though, the findings of Chiba, Martin and Stevenson (2014) suggest previous estimates of the salience of compatibility may have been overstated as a function of the aforementioned selection bias.³ Importantly, the presence of selection bias provides evidence that cabinets can and do forecast their durability.

What all of this research (and scores of books and articles we have not discussed) has in common is its consistent attention to durability as the dependent variable.⁴ Even when the theoretical focus is not attributes of the cabinet itself, but on alternative political economic phenomena, like, for example, natural resource revenue or interstate transfers, incumbent duration is still nearly always found on the left-hand side of the equation (e.g., Ahmed 2012; Bueno De Mesquita and Smith 2010). This is likely because most scholars perceive the importance of cabinet longevity as self-evident, and on this we do not disagree. Nevertheless, the overwhelming focus on duration as a dependent variable has obscured why scholars were moved to study it in the first place: the belief that rapid government turnover is, on some normative

³We note that cabinets not only terminate in dissolution, but also in replacement (Diermeier and Stevenson 1999; Chiba, Martin and Stevenson 2014). As we discuss below in more detail, we model durability by estimating the risk of dissolution, but not replacement, for two reasons: dissolution is the theoretically salient termination type and because a risk averse governments have little or no incentive to prepare for replacement rather than (or in addition to) dissolution.

⁴We urge interested readers to consult Warwick (1994), Laver (2003), and Woldendorp, Keman and Budge (2013) for excellent reviews of this literature.

level, a net negative, or at the very least salient to democratic representation and governance. That is, the entirety of this literature is motivated by the assumption that cabinet stability has powerful implications for, in the words of King et al. (1990, 846), “democratic stability, policy continuity, or even executive dominance over the legislature” among numerous other, more specific, substantively interesting and normatively significant political economic outcomes. However, in the political science literature, we could find only two empirical studies making a robust connection between cabinet stability, as an *independent variable*, and democratic outcomes (broadly defined) — one links government instability (in terms of executive turnover) to decreased levels of overall satisfaction with democratic governance (Harmel and Robertson 1986) and the other finds that short-term spikes in portfolio volatility decrease the efficiency of policy implementation (Huber 1998).⁵

All of this is to say that, while the supply of research devoted to understanding the causes of government durability is vast and deep, the supply of empirical research devoted to understanding the consequences of government durability is nearly nonexistent. As such, whether or not cabinet stability actually bears any real policy consequences remains an almost entirely open question, one that we begin to provide an answer to by assessing the relationship between government longevity and public spending — perhaps the most significant policy decision that governments must make.

⁵We note that there is a handful of public economics articles presenting evidence that political instability may impede economic growth, increase government consumption, or increase redistribution (e.g., Alesina and Perotti 1996; Annett 2001; Carmignani 2009, respectively). However, these works are overwhelmingly focused on political violence or revolution when referring to “political instability.” As such, none consider primarily modern economies and peaceful transitions of power within consolidated democracies, which is our focus here. We also note that Perry and Robertson (1998) include a type of government durability measure in an index of executive consistency that is regressed on a measure of a state’s bond risk in a sample of advanced, stable democracies.

PUBLIC SPENDING

Interest in public spending in political economic research is pervasive. Though there are a variety of themes within the literature, our interest here is in research devoted to the study of public spending as a realization of the common pool resource problem, particularly the research on the presence of political budget cycles. In short, government parties are accountable to a subset of the electorate that has particular spending priorities. Governments may engage in directed spending to please their supporters, who enjoy the benefits of that spending while bearing only a fraction of its costs. This imbalance between the concentrated benefits accruing to government supporters and the costs of expenditures, which are diffused more evenly across the electorate, means that demand for spending within the government's supporting coalition tends to be greater than it would be otherwise and the commons (national coffers) are at risk of depletion.

Bawn and Rosenbluth (2006) argue that this problem is exacerbated by increasing the diversity of the groups represented by the cabinet, as is the case in coalition governance, so long as the benefits of spending enjoyed by those groups continue to outpace the costs they bear. A similar argument is presented by Persson, Roland and Tabellini (2007). Though subsequent research by Martin and Vanberg (2013) suggests that the temptation to grow public spending as a result of increasing the number of parties in government may be mitigated by institutions constraining the budgeting process, the robust empirical connection between electoral incentives and public spending persists. In the absence of strict institutional barriers, governments will spend excessively to please their supporters.

The notion that governments have strong electoral incentives to spend on their supporters is a special case of the intuitive theoretical argument motivating the search for political budget cycles (PBCs). The classic argument is as follows: governments want to be reelected and voters factor economic performance into their choices at the ballot box. As such, opportunistic governments may stimulate the economy (or at least their supporters' economic prospects) in the short term by increasing spending in hopes that voters will be persuaded of their

managerial competence.⁶ Thus, we should observe greater spending in election years or pre-election years.

Despite the intuitiveness and simplicity of the theoretical account of electoral budget cycles, the literature on the subject has been characterized by intense debate. “The endurance of the debate derives from a stark contrast between the commonsense nature of the opportunistic argument and the paucity of evidence supporting its key implication,” (Clark, Reichert, Lomas and Parker 1998, 87-88). Evidence is particularly weak in the case of consolidated democracies with advanced economies (Brender and Drazen 2005). Many scholars argue that one reason for this is the lack of consideration given to the structure of political and economic institutions — several of which may provide constraints on the ability of governments to stimulate spending in the run-up to election. For example, Persson and Tabellini (2005) find that welfare spending tends to increase in the vicinity of elections to a larger degree in proportional systems than in single member systems, because proportional rules broaden the population parties must appeal to for support. Rose (2006) provides evidence that formal balanced budget rules constrain PBCs in the American states and Alt and Lassen (2006) argue that fiscal policy transparency may similarly constrain governments by exposing their manipulation to voters. Analyzing 19 advanced democracies, they find evidence that cycles exist, but only in opaque fiscal environments.

This institutional approach to the search for political budget cycles sheds light on the discord between the theoretical research, which had reached a near unanimous consensus in the expectation of political budget cycles, and the empirical research that had found inconsistent evidence for them in advanced democracies. Cabinets do not operate in isolation of their institutional constraints, thus, neither should our empirical investigations of their choices.

⁶Our primary concern in this manuscript is on public spending, however, the extant research on political budget cycles has considered not only spending, but also monetary policy, typically focused on the inflation-unemployment tradeoff, as in the canonical works of Nordhaus (1975) and MacRae (1977).

Nonetheless, according to Philips’s (2016) meta analysis of PBC scholarship, 93% of studies ignore institutional variation in the timing of elections by assuming that it is fixed and known *ex ante*, while the remaining 7% assume electoral timing is endogenous — that it is chosen by the incumbent.⁷ This is surprising in light of the recent advances in modeling institutional diversity in the political budget cycle literature and even more surprising given the robust literature on the nature of government durability and the common sense realization that both of these assumptions are unrealistic for parliamentary democracies.

In reality, elections are not fixed in the overwhelming majority of parliamentary democracies and only a very small minority of elections could be described as strategically timed or “opportunistic” (Schleiter and Tavits 2016). Indeed, an accounting of over 608 European governments by Seki and Williams (2014) reveals that *at least* 62% of cabinets terminate in conditions that are not constitutionally mandated elections and are extraordinarily unlikely to be the product of strategic electoral timing — the resignation of the prime minister (for health reasons or otherwise), internal dissent, or loss of parliamentary support. In other words, well over half of all European governments violate the standard assumptions of the extant PBC literature. Government survivability in parliamentary democracies is inherently uncertain and, as such, we cannot presume the timing of elections to be fixed, nor can we assume the timing is purely a function of the cabinet’s tastes.⁸ Our framework relaxes these assumptions by building a model of public spending that incorporates the cabinet’s *expectations* for dissolution. More specifically, we construct a predicted duration for each cabinet in our data based upon its observable characteristics at formation and estimate the effect of the cabinet’s life expectancy on its spending choices.

⁷These figures are not reported in the original article but tallied from the replication materials.

⁸For clarity, in our sample, nearly 75% of cabinets terminate over one month before their possible tenure expires and over 60% terminate over six months before their possible tenure expires.

DURABILITY AND SPENDING

As is common in the literature, we make the following assumptions: (1) incumbent governments wish to be reelected; (2) voters are retrospective, making evaluations on the policy outcomes they have recently observed but not factoring in the future repercussions of these policy choices;⁹ and (3) governments believe that increasing public expenditures will demonstrate competence by stimulating growth, satiating the spending demands of their supporters, or otherwise. The implication of these assumptions is that governments will increase spending as elections approach in order to stimulate electoral support. Setting aside, for the moment, the possibility of opportunistic early elections, if we were to assume that the timing of the election is fixed and known, our expectation would be higher spending in (pre) election years and lower spending in post election election years, all else equal, just as those that have preceded us have predicted (Alt and Lassen 2006; Rose 2006, etc.). However, in the parliamentary democracies that we are interested in here, the survival of the government until the next constitutionally mandated contest is by no means certain and, in fact, it is the exception rather than the rule.

We assume cabinet dissolution is stochastic and model government spending accordingly as a function of the cabinet's electoral *expectations* — when it believes the next election will occur either as a result of expiration of the constitutional interrelation period (CIEP) or premature

⁹How voters are assumed to generate their expectations for future performance, whether rationally or adaptively, has been a subject of debate in the PBC literature — we suggest Alt and Lassen (2006) and Clark et al. (1998) for concise reviews. We believe that our assumption of a retrospective voter (adaptive expectations) is a better match to what we have learned from the economic voting literature, not only about vote choices per se, but also the structure of economic expectations and retrospections and the relative weight of recent (quite high) and distant (quite low) outcomes in determining them (e.g., Duch and Stevenson 2010; Duch and Stevenson 2011; Healy and Lenz 2014).

dissolution (from here on we use the words election and dissolution interchangeably).¹⁰ That is, we assume that governments form beliefs over their durability and grow public spending accordingly. If we believe that governments have a preference for fiscal discipline, all else equal, then this implies a negative relationship between expectations of cabinet durability and public spending. Cabinets should spend less when elections are believed to be distant and spend more when elections are believed to be proximate in order to stimulate electoral support without running burdensome deficits.

Borrowing from Alt and Lassen (2006), a stylized representation of this expectation is given in the left pane of Figure 1 with the x-axis representing the cabinet’s life expectancy and the y-axis representing public spending. As the government’s expected dissolution point approaches (indicated by the 0 hash on the x-axis), it increases spending to engender electoral support. After the election, the government (whether or not the incumbent has returned) lowers spending and the cycle begins again. To reiterate: when governments believe that elections are distant, public spending is more modest. When governments believe that elections are approaching, however, they begin to spend more boldly in an effort to stimulate electoral support. This is the central hypothesis that we test below.¹¹

¹⁰Astute readers realize that dissolution does not trigger immediate elections in all cases and on occasion a dissolved cabinet may remain in government as “caretaker” until elections can be held. We maintain that caretaker cabinets are most often charged as custodians, simply there to shepherd the country to their next cabinet. However, this is not always the case, as Laver and Shepsle (1994) point out, thus, we attempt to account for the time caretakers spend in office in our empirical model.

¹¹Previous readers of the manuscript have asked why governments do not simply wait until the cabinet has dissolved and then spend prodigiously until the election. Our response is that the gears of government grind slowly — meaning that governments are likely incapable of revving up spending over night — and the effects of spending require some time to take effect and to be observed by the electorate. These factors, combined with constitutional limitations

Figure 1: Duration Expectations and Public Spending

Thinking of dissolutions as stochastic and public spending as a function of forecasted durations raises a follow-up question: what happens when the cabinet’s prediction is wrong — either too generous or too miserly — by some significant margin? To the former, when a cabinet forecasts a duration that is too long and terminates earlier than expected, it should lose votes. In this case, the premature termination would preclude the government from ramping up spending to stimulate support and, as a result, its electoral performance should suffer. This prediction is supported by the extant literature on electoral timing and success. For example, Smith (2003) presents compelling evidence that incumbents perform more poorly than expected in early elections.¹²

To the latter, when the cabinet forecasts a duration that is too short and terminates later than expected, there is no impact on the electoral result, but there should be an increase in the government’s propensity to run deficits. A stylized depiction of this expectation is given in the right pane of Figure 1. When the cabinet’s prediction of its duration is accurate, we on the amount of time between dissolutions and elections make forecasting and proactive adjustments essential.

¹²Schleiter and Tavits (2016) also present evidence (their Table 1) that incumbents suffer electoral losses in unforeseen early elections relative to regular elections.

should observe an increase in spending, an election, and then a decrease in spending about the 0-point on the x-axis. This is shown by the lighter line. The darker line, on the other hand, shows the expectation for an underestimate of the cabinet’s durability. In this case, the government increases spending in expectation of elections, but, when the cabinet proves more durable than anticipated, it must continue to spend at the heightened rate until dissolution in order to maintain its electoral support. The longer these protracted periods of heightened spending exceed the cabinet’s expectations, the deeper they will push the state into deficit. This is the second hypothesis we test — the longer a cabinet outlives its expected duration, the greater its deficit spending.

Before moving on to our design, it is important to note that cabinet dissolutions are not entirely stochastic — cabinets *must terminate* at the end of the CIEP and *may choose to terminate* for strategic reasons (given that the institutional context allows) at any time. The first issue is easily accounted for and we explain that below. The second issue requires a bit more contemplation. Recalling Kayser (2005), cabinets may choose to influence potential electoral results by increasing spending (our focus here) or calling for early elections, or they may choose to remain in office without engaging in opportunistic behaviors and this choice is conditioned on exogenous economic shocks and the time remaining in the CIEP.¹³ This presents potential hurdles, both systematic and stochastic in nature, that warrant discussion before moving on.

One possibility is that governments increase spending to stimulate popularity and call for opportunistic elections once estimated support reaches some critical level. Under this condition, we may observe a negative relationship between expected duration and public spending, just as we predict, albeit due to an alternative (though very closely related) mechanism, because expected durations are correlated with true durations. For lack of more clever language

¹³To be clear, Kayser (2005, 21) does not discuss spending in particular, but a generalized and directly unobservable alteration to policy “that shifts resources from the future to the present.” This distortionary policy can take many forms.

we can call this budget cycling under complete electoral endogeneity, as opposed to budget cycling under duration uncertainty — a systematic behavior that may confound our ability assess our arguments. Fortunately, these mechanisms are empirically differentiable with the data on hand. If, as we argue, governments form expectations of their durability and plan their spending accordingly, then *expected* durations should provide more predictive power for observed rates of spending. On the other hand, if elections are, on average, chosen opportunistically after governments have increased spending, then *true* durations should provide more predictive power for observed rates of spending, because, in this case, spending and electoral timing are codetermined. To preview our empirical results, spending patterns are better explained by *expected* durations. We also add that, if cycling under electoral endogeneity was the norm, then past PBC research would not have failed to recover evidence of cycling behavior in these countries as often as it has.

A second possibility is that cabinets privileged by some exogenous shock creating a windfall of popularity may call early elections without having to ramp up spending in the manner we predict — a stochastic occurrence that may confound our ability assess our theoretical argument. Of course, this second possibility would not produce our predicted relationship, rather, this type of opportunistic behavior would bias against recovering support for our hypothesis. And, because our focus is the effect of cabinet durability on public spending, rather than the relationship among economic inputs (e.g. spending, inflation), economic outputs (e.g. growth, unemployment), and electoral timing and performance, opportunistic elections of this type are stochastic nuisances rather than threats to design credibility. We also note that if the true process guiding spending patterns is some combination of systematic cycling under electoral endogeneity and stochastic opportunism, then, again, true durations would provide a better fit to our spending data vis-à-vis expected durations. We return to this issue and other potential confounders in a short section on robustness following our empirical analysis.

RESEARCH DESIGN

To test our hypotheses, we first develop a model of duration expectations for the cabinet. We assume that cabinets take into account the observable characteristics of their government and accordingly generate their expectations for its duration, with greater or lesser degrees of certainty. That is, because governments cannot reasonably be expected to foresee the precise timing of “critical events” such as economic downturns, political scandals, or abrupt fluctuations in public opinion, the best information they have to generate their expectations are the life cycles of governments past, the observable characteristics of those governments, and the attributes of their own cabinet — a form of Muth’s (1961) classic rational expectations.¹⁴

The most obvious starting point for estimating these expectations is the rich political science literature on the durability of governments discussed above. After the work of, for example, King et al. (1990), Laver and Shepsle (1998), Diermeier and Stevenson (2000), and Chiba, Martin and Stevenson (2014), there can be little doubt that political scientists have amassed an impressive understanding of the theoretical foundations and empirical correlates of cabinet stability. Thus, to generate our measure of a government’s life expectancy, which we will impute to the cabinet, we accept the collective wisdom of the discipline and derive our measure from the extant literature. More specifically, we estimate the survival model described in Chiba, Martin and Stevenson (2014), which accounts for potential selection bias induced by strategic formations by jointly modeling formation and duration. Note that, as King et al. (1990) explain, this duration model captures “events” in its stochastic component (distributed Weibull in this case), while including “attributes” as the measured covariates in

¹⁴For the purposes of research design, there is much to recommend estimating durations from the observable characteristics of the cabinet at the time of formation. Most importantly, however, is that these durability estimates are not endogenous to the changing political economic climate (e.g., growth, scandal, militarized dispute, etc.) and are therefore free of potential “feedback effects,” where spending is conditional upon on durability expectations and durability expectations are then updated conditional on the effects of spending and so on.

its systematic component. For all cabinets in our data, we use the model estimates to predict the number of days the cabinet will last before dissolution.

Importantly, we do not pool the risks of dissolution and replacement.¹⁵ We make this choice for two reasons. First, the risk type that is theoretically salient is dissolution and, as such, we do not want replacement terminations contributing to our durability estimates. Second, cabinets have no incentive to prepare for replacement rather than dissolution. In the event of a replacement, the incumbent loses its governing status and is consigned to opposition. Given what we have learned about the collective memory of voters, when the next election arrives, it is not the displaced incumbent that is the focus of competency evaluations, but its successor and any choices made by the displaced incumbent are likely to be irrelevant (or nearly so) to the election outcome (Healy and Lenz 2014).¹⁶ Taken together, these factors imply that a cabinet’s best strategy would be to ignore replacement risk when generating its expectations for durability. Fortunately, the data allow us to bring evidence to bear on the question of expectation formation under competing risks and we discuss this in the robustness section.

Our public spending analysis is conducted at the country-year level. Therefore, to capture each cabinet’s expected remaining time in office when the budget is set, we subtract from the cabinet’s total predicted life the number of days the cabinet has already served at the time it presents its annual budget to the legislature. This estimate of the cabinet’s remaining life expectancy is the covariate of interest in our models of public spending (explained in detail below). We expect this to have a negative relationship to public spending in general — that a cabinet will spend more as its expected duration dwindles — and a negative relationship to

¹⁵In estimation, replacements are right censored.

¹⁶A third consideration would be that, in addition to being much less common than dissolution terminations, replacement hazards are effectively flat, indicating that they are the product of a much more stochastic process (Diermeier and Stevenson 1999). As such, the degree to which cabinets are able to forecast relatively accurate expectations of replacement terminations vis-à-vis dissolution would be impaired.

deficit spending — that a cabinet will spend less responsibly as its expected duration dwindles. Of course, our measure of the cabinet’s duration expectations is estimated with error, and, as such, we take care to model this error structure.

DATA AND MODEL CONSTRUCTION

For our main analyses we gather data on several decades of budgeting in 15 European democracies that allow for parliamentary dissolutions — the same data analyzed by Bawn and Rosenbluth (2006) and Martin and Vanberg (2013).¹⁷ Our dependent variables are the OECD calendar-year estimates of central government spending as a percentage of GDP and this spending figure minus central government receipts as a percentage of GDP, where greater values indicate greater deficits. Budgets in our sample are typically submitted in the latter half of the year for spending in the following calendar year. Most submission dates fall between August and October of the calendar year preceding the budget year, though dates as early as July and as late as April of the budget year appear in the data.¹⁸ The data also include

¹⁷Sample countries include: Austria (1971-2006), Belgium (1971-2007), Denmark (1972-2009), Finland (1971-2007), France (1979-2009), Germany (1971-2009), Greece (1979-2004), Ireland (1971-2009), Italy (1971-2008), Luxembourg (1991-2004), the Netherlands (1971-2006), Portugal (1978-2009), Spain (1980-2009), Sweden (1971-2009), and the United Kingdom (1971-2009). Notably, Norway, which is included in the Bawn and Rosenbluth (2006) sample, is omitted here because its elections are fixed.

¹⁸Budget dates were coded from OECD *Journal on Budgeting* country issues (<http://www.oecd.org/governance/budgeting/oecdjournalonbudgeting.htm>). Each reports the deadline by which the government must present the annual budget to parliament, typically between August and October of the preceding year. For countries not covered by the *Journal on Budgeting*, we refer to the respective constitution or applicable legal framework. In cases in which the budget deadline was unclear or fell within two months of a change of government, the exact date on which the budget was presented to parliament was located in the respective

information on several political economic characteristics salient to budget-making, which we discuss below.

The explanatory variable of interest is the cabinet’s predicted duration — the number of days it expects to remain in office — at the time the budget was submitted. We derive the measure by first reestimating Chiba, Martin and Stevenson’s (2014) model of government duration, which jointly estimates cabinet formation and survival.¹⁹ To account for uncertainty in this estimate, we employ a nonparametric bootstrap.²⁰ At each of the 1,000 bootstrap iterations, we randomly resample the data, with replacement, from our set of 432 cabinets and reestimate the model. We then use the model estimates to predict the duration for each cabinet in our data, record the predictions, and reiterate, generating a distribution of 1,000 predicted survival times for each cabinet.

For each cabinet-budget year in our spending data, we alter these distributions in two ways: 1) we subtract the number of days the cabinet has served at the time of budget submission and 2) we trim any expected durations that exceed the CIEP back to the expiration of the CIEP. Predicted durations exceeding the CIEP are fairly rare, but must nonetheless be accounted for — no government can reasonably expect to remain in office through the CIEP without holding elections.²¹

By estimating a distribution of expected durations for each of our sample cabinets, we have a straightforward way of accounting for the uncertainty of these predictions. This is

parliamentary archives, ensuring that all budgets are attributed to the correct cabinets.

¹⁹This is a conditional logit model of the government selection stage, joined to a Weibull survival by means of a Gaussian copula function. See Chiba, Martin and Stevenson (2014) for details.

²⁰Bootstrapped model estimates are available in the appendix.

²¹The constitutional interelection period is included in our cabinet duration model and is a powerful duration predictor. We note, however, that model estimates using durations that are not trimmed to CIEP also support our central predictions.

important for both empirical and theoretical reasons. Empirically, this variable is, after all, an estimate with an associated error structure and ignoring this error may bias our estimates of the relationships of interest, and therefore bias our substantive conclusions. Theoretically, expectations rarely take the form of a point when they are generated by individuals and are explicitly distributions when they are generated by a collective, whether it is a system of firms, a betting market, or the ministers composing a cabinet. If we assume that rational expectations are distributions that are, in the aggregate, centered on the most probable (or, most expected, given the state of the world) outcome as Muth (1961) theorized and others have found empirically, then modeling these distributions, rather than merely their central tendency, is critical to hypothesis testing.

We model these distributions by estimating our spending and deficit models 1,000 times — once for each prediction of cabinet survival. Thus, for each iteration, we impute an expected remaining duration for each cabinet-year in our data using a single set of bootstrapped survival predictions, estimate the spending and deficit models, and record the results. This yields 1,000 regression results for the main models which we summarize and interpret below, but we first discuss the construction and estimation of the spending models.

Alongside our focal variable (predicted duration), we include a set of political economic control variables borrowed from Bawn and Rosenbluth (2006) and Martin and Vanberg (2013) to account for potential confounders to our relationship of interest while keeping our substantive results comparable to previous research. The measurement of these covariates and the reasons they are included in the models are described in great detail by Bawn and Rosenbluth and Martin and Vanberg, so we do not reiterate that information here. We provide, instead, a more general discussion of the rationale motivating inclusion of these covariates which break down into three groups: variables capturing the government’s taste for public spending; variables accounting for the state’s revenue supply and entitlement burden; and variables indicating institutional constraints on spending depth and responsibility.

First, consider spending tastes, or the breadth of spending demands within the cabinet.

There is broad theoretical consensus in the literature is that left-leaning parties prefer to spend more than right-leaning parties²² and we account for this by including Powell’s (2000) measure of the cabinet’s ideological positioning: the mean, seat-weighted, left-right stance of each member of the cabinet, where greater values indicate a more right-leaning government.²³ Following the above discussion on the common pool resource problem in budgeting — as the diversity of spending priorities grows, the cabinet’s temptation to spend to please its supporters increases — we include the number of parties in the cabinet. We also include the effective number of legislative parties (Laakso and Taagepera 1979) to account for the possibility that diverse spending priorities outside the cabinet may coax budgets upward in a similar fashion.

Our second set of controls are meant to account for a state’s ability or need to grow its spending obligations. That is, does the state in question have the resources needed to increase spending responsibly, or, are there characteristics of the state that should systematically increase its spending obligations? These variables include: the overall level of economic productivity (measured as per capita GDP), a state’s integration into the modern trade economy (called “trade openness” in the tables below; the export/import fraction of GDP), its unemployment rate, and the percentage of likely non-productive population — those under 15 years of age and those 65 and over (called the “dependency ratio”).

Finally, we include two variables meant to capture domestic and supranational spending constraints. The first is Martin and Vanberg’s (2013) “budgetary constraint index,” a summary of formal rules that “[c]onstrain the ability of parties to push for spending,” and, “generate incentives for parties to oppose spending demands by their partners” (p. 956). This variable is bounded between zero and one and is interacted with the number of parties in the cabinet. The second is a binary variable indicating that the budget was submitted after the adoption of the Maastricht Treaty, which placed limits on the total debt a member state could

²²But see Clark (2009) for opposing evidence.

²³These positions are derived from the Comparative Manifestos Project data following Fortunato, Martin and Vanberg (2017) and others.

carry, as well as the size of the deficit a state could generate in any given year. Interested readers may see summary statistics for all variables in the appendix.

With variables in hand, we now turn to estimation. We are analyzing panel data with substantial cross-sectional variation, but also a great deal of autocorrelation within units. Following Bawn and Rosenbluth (2006) and Martin and Vanberg (2013) we estimate an autoregressive distributed lag model (ADL), including (one year) lags of both the dependent variables and independent variables as well concurrent realizations of the economic variables and estimate panel-corrected standard errors. This is in keeping with Philips’s (2016) conclusion that modeling data dynamics is vital in the analysis of public spending, as ignoring the autoregressive properties of spending patterns can lead to inflated estimates of cycling behavior.²⁴ As our focal explanatory variable is a distribution, rather than a point, we cycle through its 1,000 estimated values, imputing each into our spending and deficit models in turn, estimating, and recording the results. We also generate error estimates on our cabinet ideology measure following Lowe, Benoit, Mikhaylov and Laver (2011) and model these in the same fashion.

The results of our first model, public spending, are summarized in Table 1.²⁵ For our

²⁴An anonymous reviewer correctly points out that ADL models, particularly those with lagged *and* contemporaneous values of covariates, can create multicollinearity and this is certainly the case with our model. However, the construction does not induce collinearity for variable of interest — a variance in factors test reveals that the VIF on expected duration is about 4.3, which is well under the typical level of concern (10).

²⁵We have summarized the 1,000 spending (and deficit) models in a familiar tabular format for the sake of clarity. We note, however, that these models should technically be assessed individually since parameter point estimates and standard errors from regressions using predictions from different bootstrap iterations of the cabinet survival model are not fully comparable — even though the substantive effects we generate from them are. We can make fully reliable comparisons using so-called “pivotal statistics” (e.g. *z*-scores) to draw conclusions about sta-

Table 1: Aggregated results from bootstrapped model of spending as percent of GDP.

Variable	Pooled Model			Fixed Effects		
	Mean	SD	p	Mean	SD	p
Lagged	Expected Duration	-0.0003 (0.0001)	0.03	-0.0004 (0.0002)	0.01	
	Parties in Government	0.2435 (0.1215)	0.02	0.3443 (0.1760)	0.03	
	ENP	0.0353 (0.0745)	0.32	-0.1563 (0.1127)	0.08	
	Caretaker Time	0.8323 (0.7833)	0.14	-0.0399 (0.9191)	0.48	
	GDP Per Capita	1.4593 (0.1805)	0.00	1.4814 (0.1848)	0.00	
	Unemployment Rate	-0.4262 (0.0776)	0.00	-0.4154 (0.0778)	0.00	
	Dependency Ratio	0.1136 (0.3364)	0.37	0.2072 (0.3373)	0.27	
	Trade Openness	0.0194 (0.0177)	0.14	0.0165 (0.0184)	0.19	
	Maastricht Era	-0.4691 (0.2568)	0.03	-0.2645 (0.3440)	0.22	
	Government Ideology	-0.1133 (0.0793)	0.08	-0.0274 (0.0879)	0.38	
	Budgetary Constraint Index (BCI)	0.7499 (0.4616)	0.05	1.1879 (0.7405)	0.06	
	Parties in Government $\times$ BCI	-0.4466 (0.2139)	0.02	-0.6252 (0.2871)	0.01	
Concurrent	Spending	0.9295 (0.0112)	0.00	0.8931 (0.0214)	0.00	
	GDP Per Capita	-1.4092 (0.1766)	0.00	-1.4388 (0.1883)	0.00	
	Unemployment Rate	0.3694 (0.0759)	0.00	0.3799 (0.0774)	0.00	
	Dependency Ratio	-0.0519 (0.3339)	0.44	-0.1740 (0.3379)	0.30	
	Trade Openness	-0.0218 (0.0175)	0.11	-0.0250 (0.0180)	0.08	
	Intercept	1.4287 (1.7487)	0.21			
	N	488		488		
	R^2	0.9626		0.9635		

p = directional certainty, fixed effects not shown

covariate of interest, expected duration, there is a statistically significant negative parameter estimate, indicating, as we predicted, that decreasing expected duration will increase the cabinet's level of public spending. Further, each of our control variables, when reaching statistical significance, are signed in the sensible direction and comport with previous research on spending (e.g., Bawn and Rosenbluth 2006; Martin and Vanberg 2013), giving us confidence that our model is properly specified.

To better illustrate the predicted effect of expected duration on public spending, Figure 2 plots the substantive effect of a reduction in expected cabinet duration from three years

tistical significance of effect parameters across the 1,000 spending or deficit models. Statistics are considered pivotal if their sampling distribution does not depend on unknown parameters, making them a good choice for comparing across models as we do (Shao 2003). The results of this more appropriate comparison lead to exactly the same conclusions but make for a potentially confusing presentation, therefore we have included the appropriate graphic in appendix figure A.3.

Figure 2

to one year aggregated across all of our 1,000 models. Each light density in Figure 2 plots the distribution of predicted changes in spending as a percentage of GDP resulting from this reduction in the expected durability of the cabinet from a single bootstrap iteration. Thus, the shape of each light density illustrates the estimation uncertainty in one of our 1,000 models and the light vertical lines mark the 5th percentile of each distribution (our criterion for statistical significance). The thicker, darker lines give the global density and 5th percentile over *all* bootstrap iterations. Taken together, the figure shows that, not only is our criterion for statistical significance met in virtually all bootstrapped models, but also that there is very little variation in this result across the 1,000 bootstrapped models.

More substantively, on average, the decrease from three years to one year of expected duration (about a 1.5 standard deviation change) increases public spending by 0.26% of GDP. This is a very large spending increase. Using 2010 GDP and spending figures in US dollars we can get a better sense of how salient this effect is: for Denmark (GDP \$320 billion) the increase would be roughly \$844 million, for the Netherlands (GDP \$836 billion) the increase

would be over \$2.2 billion, and for Germany in the same year (GDP \$3.4 trillion) the increase would be just shy of \$9 billion. To contextualize these figures, this increase equates to roughly 11.5%, 16.5%, and 17.6% of the unemployment cash transfers made in these countries, or 18.9%, 19.7%, and 19.5% of defense spending in these countries, respectively. These effects are quite large and we are quite certain of them, indeed, only 1 in 1,000 models yields a certainty estimate that does not meet the $p < 0.05$ criterion and fewer than 1% of all posterior draws are greater than 0. This is very strong support for our first hypothesis: cabinets spend more as their expected time in office dwindles.

Of course, the effects of durability should be larger and much longer lasting than this one-time increase, however. Budgets tend to be remarkably sticky. As such, small increases made here and there have a tendency of becoming effectively permanent. Following Williams and Whitten (2012), we illustrate the compounding nature of cabinet fragility in Figure 3 where we plot the public spending of Austria under two idealized scenarios: one in which Austria always forms 4-year cabinets (dark triangles) and one in which it always forms 2-year cabinets (light circles). To generate the values, we use the parameter estimates summarized in Table 1 to predict Austrian spending levels from 1985 to 2005, using its observed covariate values over that period, with the exception of expected durability and lagged spending. For one case, we count down expected duration from four years to one year and repeat; for the second case, we count down from two years to one year and repeat. Also, for each year (after the first) we use the mean of the previous year’s predicted spending as the lagged spending value. As the figure shows, persistent cabinet instability of this magnitude can substantially increase spending over the long term

We now move on to our deficit models, which are estimated and presented in the same fashion as above in Table 2. As with our spending model, our covariate of interest, expected cabinet duration, is negative and clearly different from zero, indicating that as the cabinet’s expected time in office draws to a close it begins to spend less responsibly — i.e. running ever higher deficits. Further, as before, the control variables with robust estimates are all signed

Figure 3

in the direction that we would expect, making us confident in the model specification. We also note here that there are really three ways to estimate this model: using only the expected remaining duration, folding that expected duration into a binary variable indicating that the cabinet has outlived its expectations ($duration < 0$), or estimating an implied interaction of the duration measure and the indicator. We present the results of the first method in the main text to match the previous analysis, however all three produce very similar results and the remaining two models are given in the appendix.

As predicted, expected duration is negative and the estimate is strongly significant ($p < 0.01$). As above, we interpret these results graphically by predicting the change in deficit size as a function of a change in expected cabinet duration (using the fixed effects model results, once again). The difference here is that we change expected duration from one year to negative one year, indicating that the government has outlasted its life expectancy by a year (there are several such cases in our sample). These results are given in Figure 4, where the densities illustrate the entire distribution of predicted deficit changes and the vertical lines

Figure 4

the wrath of voters for irresponsible spending. A more broad interpretation of this finding is that incumbents are more averse to electoral uncertainty than most current characterizations of the literature may imply — that we have perhaps been too cavalier in our assumptions of an incumbent’s willingness to call for new elections. Of course, there is good reason for this. Though some dissolution models had built in electoral uncertainty (e.g., Kayser 2005), a simplifying assumption of many more formal models of dissolution is that the outcome of the election is common knowledge across all parties (e.g., Diermeier and Stevenson 2000).

ROBUSTNESS

The models presented above reveal strong support for our central predictions, but there are a few issues that we wish to address before concluding. First, the manner in which expected duration enters the model makes, for example, a cabinet with a two-year expected duration in its first year in office equivalent to a cabinet with a four-year expected duration in its

third year in office. It is therefore possible that the effects reported above may not be driven by expiring expected durations, but instead by shorter total expected duration, which may stimulate a need to spend more rapidly in order for the government to achieve its desired policy outcomes within its abbreviated life expectancy. We assess this possibility in the appendix where we include in our models both the cabinet's remaining life expectancy and its entire life expectancy. In these models, the remaining life expectancy remains negative and robust while there is no statistically differentiable effect for total life expectancy.

A second possibility is that cabinets are making their spending choices not according to expectations as we conceive of them, but according to the CIEP, assuming, or hoping, the cabinet will last the entirety of its potential duration. This, too, may produce the effects that we uncover above as predicted durations and CIEP are positively correlated. We assess this possibility by including both our measure of expected duration and the maximum possible duration (time left in CIEP) in the model. Again, our substantive results remain, while CIEP has no statistically discernible relationship to spending.

Third, previous readers have suggested that we may introduce bias into our estimates by neglecting to model a cabinet's ability to call early elections. As we discussed above, strategically timed elections following a stochastic windfall in popularity should only induce bias against finding our predicted effect. On the other hand, elections strategically timed to coincide with a planned increase in spending may potentially produce empirical results similar to what we have uncovered. While this is still electoral manipulation and budget cycling, this is not the theoretical story we are telling and we wish to reassure readers that these processes are not driving our results. We do this in two ways.

As mentioned earlier, if cycling under completely endogenous elections is a more accurate description of spending decisions, then true government durations would provide a better fit to the data than expectations as these durations and spending levels are codetermined. To test this, we reestimate our models after replacing our key measure with true durations and then reestimate again with both measures included. In both models, true durations do not provide

statistically significant explanatory power and when both values are included the estimate on expected durations is still large, negative, and statistically robust.

Our second approach is to simultaneously estimate the cabinet’s choice of opportunistic election — using Schleiter and Tavits’ (2016) data on opportunistic elections, which cover our entire sample — and the cabinet’s total public spending and deficit spending, given its early election choice. We do this by instrumenting opportunistic elections with Goplerud and Schleiter’s (2015) index of the assembly dissolution powers vested in the cabinet — a variable that should only affect spending through its impact on early elections. Our substantive results remain in this framework as well.

A fourth concern was noted in our research design section, where we discussed how cabinets should approach competing risks to termination and concluded that they should ignore the potential for replacement because there is simply no expected benefit to anticipating replacement. Our data allow us to get some traction on this question by estimating termination as a function of dissolution (as we did for the models above), replacement, and pooled risk, imputing predicted durations from these different survival models into our spending models, and comparing model fit. As our arguments imply, durations derived from models of dissolution risk perform better than pooled risk predictions, and pooled risk predictions perform better than replacement predictions. Because they are quite highly correlated, all measures produce statistically significant estimates of the predicted effect, but the rank-ordering of empirical fit follows a theoretical rank-ordering where cabinets are concerned with dissolution, but have less (or no) incentive to anticipate replacement.

A final potential concern is our choice of dependent variable as previous research has noted that cycles may be more evident in the composition of spending than in the level of spending and analyzing the level of spending alone may obscure the overall effect of cycling behavior. We have estimated replications of the total spending analysis that only examine social spending (unemployment benefits, pensions, housing transfers, etc.) and these models produce results that are similar in magnitude and a shade more powerful than our total spending models.

There are good reasons to focus exclusively on social spending — most importantly, one could argue that this would be the most direct way stimulate electoral support as these are largely redistributive transfers. However, we contend that total spending is the more appropriate subject of study here because spending is not just a tool of economic stimulus in our theoretical argument, but also a means of satisfying the priorities of core supporters.

CONCLUSION

In this manuscript we set out to learn how cabinet life expectancy conditions the spending choices of incumbent governments. We argued that cabinets have incentives to increase spending in advance of elections, but, because, as Lupia and Strøm note “...governments in parliamentary democracies lead a precarious existence” (1995, 648), exactly when that election will take place is uncertain. We therefore argued that it is the cabinet’s expectations for survival, rather than the time remaining in the CIEP, that are critical to understanding patterns in public spending — as a cabinet’s expected time in office withers, spending should increase. However, even though these expectations are well-informed guesses, they are still guess and therefore prone to error. Given this potential for error, a natural extension of the basic theoretical framework is to consider the implications of expectations that are too conservative — expectations for durability that the government outlives.²⁶ We predicted that governments who increase their spending in expectation of their dissolution, but prove more durable than expected, should run ever higher deficits as they surpass their life expectancy but continue to spend generously to maintain electoral support. Our analysis of public spending data from 15 countries over a 50-year period bore robust evidence for both key predictions.

In crafting and testing our argument we make two contributions to two related but hereto-

²⁶Recall that forecasted durations that are too generous should result in the cabinet underperforming expectations in election as they would have failed to stimulate sufficient electoral support by increasing their spending, just as Smith (2003) found.

fore separate literatures. To the literature on public spending and political budget cycles, we show that the assumption of fixed elections (as well as completely endogenous elections) should be relaxed where the institutional structure allows for variability in government duration and election timing, and particularly where the empirical regularities of cabinet dissolution show that the assumption is frequently violated. Though we cannot say with complete certainty that the lack of evidence for cycling behavior in the extant literature is purely a function of its assumptions regarding electoral timing, our empirical results do point in this direction and a simulation exercise detailed in the appendix reveals that when cabinets spend according to expected durability, assuming a fixed electoral calendar will nearly always obscure the econometric relationship between elections and budgeting. This corroborates our claim that assuming fixed elections, standard in the extant literature, is inappropriate.

To the literature on cabinet duration, we present evidence that government durability has substantive policy consequences: because governments spend more in the shadow of anticipated dissolution, less stable cabinets will spend more on average than their more durable counterparts and they will also spend less responsibly in the event that they prove more durable than expected, running large deficits which can generate increasingly greater long-term borrowing costs. This raises the possibility that cabinet instability may perpetuate a vicious cycle of undisciplined fiscal policy and may add nuance to our understanding of why, for example, Italy's debt continued to increase rapidly through the 1980s, despite strong growth. Likewise, it is possible that the increased stability of Belgian cabinets following 1993's fourth state reform helped the country to curb its increasingly unwieldy debt obligations, which had been rising briskly and steadily for decades marked by rapid government turnover.

Given that we have uncovered robust evidence for cycling behavior in Western European countries, the next step is to ask what may mitigate or exacerbate these behaviors. The results presented above suggest that entering into the Maastricht Treaty (or, more appropriately, striving to meet the Euro Convergence criteria) may help to restrain governments from spending at high rates or running large deficits. Similarly, the evidence suggests that domestic

budgeting institutions can mitigate the common pool resource problems associated with diverse governing coalitions, just as formal rules creating a transparent budgeting environment have been shown to restrain spending in the past (Alt and Lassen 2006). In addition to investigating the institutional context altering the *ability* of governments to spend irresponsibly, we hope that our colleagues will be motivated to analyze the institutional context altering the *willingness* of governments to spend irresponsibly. For example, the structure of electoral institutions and the empirical regularities of electoral competition simply make some governments' expectations for post-electoral reformation more tenuous than others. That is, some Δ decline in popularity can lead to greater or lesser electoral losses in different contexts. Where small changes in vote-share can lead to large changes in seat-share, it seems natural that the expected benefit of cycling behavior would be greater and, indeed, a preliminary comparison of country-level estimates of cycling behavior to Kayser and Lindstädt's (2015) measure of electoral competitiveness, suggests that this is likely the case.²⁷ But a more focused and rigorous study on this, and several other salient institutional variations, is needed.

We close with suggestions for future research regarding cabinet durability. Political scientists have been researching cabinet durability for decades with an almost single-minded focus on the concept as a dependent variable — particularly in the empirical literature. While we agree with Laver and Shepsle that durability is a “self-evidently important topic” (1998, 29), we nonetheless believe that the policy effects of durability are severely understudied and hope that this manuscript will inspire our colleagues to begin to analyze the potential impact of fragile governments. We suggest beginning with their economic implications. For example, instability creates inescapable uncertainty in lending markets such that bond buyers should be skeptical whether the government they are lending to will be same government, or even resemble the government, whose responsibility it will ultimately be to repay the loan. Further, consistent turnover may have a robust impact on the stability of policy outcomes, as scholars have theorized (e.g., Huber and Martinez-Gallardo 2008; Fortunato and Turner 2017) but have

²⁷Interested readers can see details in the appendix.

only begun to test. How does cabinet durability effect long-term electoral strategies and party competition? Does durability condition voters' ability to hold their government accountable for economic outcomes? These critical questions and many more await answers.

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APPENDIX
NOT FOR PRINT PUBLICATION

A APPENDIX: DURATION MODEL RESULTS

	Mean Estimate [95% C.I.]
Minority government	-0.33 [-0.6, -0.039]
Effective number of legislative parties	0.108 [-0.024, 0.291]
Polarization index	-0.066 [-0.126, -0.008]
Ideological divisions in coalition	0.005 [-0.002, 0.015]
Returnability	-0.073 [-0.375, 0.238]
Time remaining in CIEP (Logged)	0.751 [0.515, 0.95]
Intercept	2.03 [0.393, 3.81]
Duration dependence (logged)	0.554 [0.335, 0.786]
Error correlation ($\tanh^{-1}(\theta)$)	1.75 [1.4, 2.19]
Potential coalitions	95576
Formed coalitions	432

Confidence intervals in brackets.

Dependent variable is the duration in days of a given government.

Table A.1: Bootstrapped results from duration portion of bivariate copula selection and duration model of cabinet survival for election risk. Coefficients are expressed in the accelerated failure time metric. Mean estimates are means of coefficient estimates from 1,000 bootstrap iterations, confidence intervals are 2.5 and 97.5 percentiles of distributions of coefficient estimates. Models right-censor replacements.

See figures A.1 and A.2 below for a demonstration of the out-of-sample accuracy of our predictions of cabinet survival. To create this demonstration, we repeatedly randomly selected 90% of the actual cabinets in our data and used them to estimate the pooled copula selection-duration model for election risk. We then predicted the duration of the remaining 10% of cabinets and compared these predictions to their actual durations. This process was repeated 500 times – each time randomly reselecting 90% of cabinets to use in the estimation – to ensure that the random selection of estimation data had no influence on the accuracy of the predictions.

Figure A.1 plots the predicted durations minus the true durations aggregated across all 500 estimations, giving a good summary of the overall predictive performance of the model. The

modal cabinet’s duration is predicted with very little error, and most cabinets are predicted within one calendar year of their actual dissolution. Since the governments in our sample are more likely than not to terminate early and many of these terminations are due to stochastic events (see, again, Seki and Williams 2014), our model more often over- than underpredicts.

Figure A.1: Comparison of predicted durations to true durations: difference.

A further reason for this more frequent overprediction becomes clear in figure A.2. True durations are plotted against their predictions. Points in the scatterplot are binned into hexagons to reduce overplotting and shaded according to the density of observations in each bin – darker bins hold more data. The dark diagonal in this plot represents exact predictions for reference. The plot indicates that our predications also exhibit some conservatism, overestimating more frequently the duration of short-lived cabinets and underestimating the duration of longer surviving cabinets. The model captures the fundamental observable determinants of cabinet survival and makes more errors where we would prefer it to given that these are out-of-sample predictions. These figures were produced using predictions from the election model. The shapes of both plots are quite similar when modeling either pooled or replacement hazards.

Figure A.2: Predicted durations versus true durations (hexagonally binned).

B APPENDIX: SUMMARY STATISTICS

Table A.2: Spending model summary statistics

	Min	Max	Mean	Median	Std Dev	Obs
Government Spending	28.10	71.72	47.73	47.50	7.50	481
Expected Duration	-855.58	1800	740.83	767.55	482.03	481
Parties in government	0.68	6.96	2.10	2	1.23	481
Effective Number of Parties	1.55	9.08	3.68	3.34	1.43	481
Government Ideology	-2.83	2.46	-0.21	-0.23	0.85	481
Caretaker Time	-0.01	0.74	0.04	0.00	0.10	481
GDP Per Capita	10.06	63.95	23.85	22.22	7.85	481
Unemployment Rate	0.70	24.17	7.48	7.21	4.17	481
Dependency Ratio	29.84	42.34	34.07	33.71	2.25	481
Trade Openness	27.91	278.99	76.53	63.52	40.04	481
Maastricht Era	0.00	1.00	0.50	1.00	0.50	481
Budgetary Constraint Index (BCI)	0.05	1.00	0.46	0.45	0.28	481

Table A.3: Deficit spending model summary statistics

	Min	Max	Mean	Median	Std Dev	Obs
Spending Deficits	-7.62	16.01	3.56	3.49	4.32	447
Expected Duration	-855.58	1723	763.04	783.04	783.04	481
Parties in government	0.68	6.00	2.21	2.00	1.21	447
Effective Number of Parties	1.55	9.08	3.81	3.50	1.42	447
Government Ideology	-45.63	38.35	-3.98	-5.73	15.37	447
Caretaker Time	-0.01	0.74	0.05	0.00	0.10	447
GDP Per Capita	9.81	63.95	23.81	22.09	7.99	447
Unemployment Rate	0.57	24.17	7.46	7.19	4.30	447
Dependency Ratio	29.84	42.34	34.00	33.47	2.32	447
Trade Openness	31.35	278.99	78.67	66.37	40.92	447
Maastricht Era	0.00	1.00	0.50	0.00	0.50	447
Budgetary Constraint Index (BCI)	0.05	1.00	0.43	0.35	0.27	447

C APPENDIX: ROBUSTNESS CHECKS

Our first check evaluates the possibility that the spending results we find are not a function of electoral proximity, but of short total life expectancy which may incentivize cabinets to spend a greater amount in order to achieve their policy goals on a shortened time horizon. To this end, we include both the total predicted duration for each cabinet, as well as their remaining time in office. If this alternative explanation was driving our results, we should see the total duration exert a robust negative effect when included. This is not the case. We also estimate this model with the CIEP — the maximum time a cabinet could spend in office. Neither of these variables produce the predicted effect or negate the effect of our focal variable.

Table A.4: Spending model with total possible duration (CIEP) and total predicted duration

	Variable	Total predicted duration			CIEP		
		Coef.	(SE)	<i>p</i>	Coef.	(SE)	<i>p</i>
Lagged	Remaining Predicted Duration	-0.0004	(0.0002)	0.0430	-0.0003	(0.0001)	0.0130
	Total Predicted Duration	0.0001	(0.0002)	0.7510			
	CIEP				0.1036	(0.6928)	0.8810
	Government Ideology	-0.0060	(0.0046)	0.1970	-0.0060	(0.0047)	0.1970
	Parties in Government	0.3694	(0.1722)	0.0320	0.3677	(0.1718)	0.0320
	Budgetary Constraint Index (BCI)	1.1359	(0.7574)	0.1340	1.1275	(0.7548)	0.1350
	Parties in Government $\times$ BCI	-0.6630	(0.2910)	0.0230	-0.6611	(0.2905)	0.0230
	Effective Number of Parties	-0.1034	(0.1146)	0.3670	-0.0980	(0.1132)	0.3870
	Caretaker Time	-0.4210	(0.8670)	0.6270	-0.3701	(0.8399)	0.6590
	GDP Per Capita	1.4559	(0.1810)	0.0000	1.4562	(0.1808)	0.0000
	Unemployment Rate	-0.4117	(0.0779)	0.0000	-0.4098	(0.0780)	0.0000
	Dependency Ratio	0.2623	(0.3300)	0.4270	0.2589	(0.3302)	0.4330
	Trade Openness	0.0147	(0.0184)	0.4240	0.0154	(0.0183)	0.4010
	Spending	0.8916	(0.0207)	0.0000	0.8915	(0.0207)	0.0000
	Maastricht Era	-0.2789	(0.3459)	0.4200	-0.2771	(0.3451)	0.4220
Concurrent	GDP Per Capita	-1.3999	(-1.4065)	0.1840	-1.4057	(0.1838)	0.0000
	Unemployment Rate	0.3840	(0.0776)	0.0000	0.3815	(0.0773)	0.0000
	Dependency Ratio	-0.2205	(0.3345)	0.5100	-0.2162	(0.3348)	0.5180
	Trade Openness	-0.0233	(0.0178)	0.1910	-0.0239	(0.0177)	0.1770
Country Effects	Belgium	0.7387	(0.7819)	0.3450	0.7491	(0.7809)	0.3370
	Denmark	0.5160	(0.3798)	0.1740	0.5099	(0.3802)	0.1800
	Finland	-0.2755	(0.5146)	0.5920	-0.2782	(0.5196)	0.5920
	France	-0.0685	(0.6306)	0.9130	-0.0579	(0.6281)	0.9270
	Germany	-0.8285	(0.4595)	0.0710	-0.8216	(0.4570)	0.0720
	Greece	-0.7878	(0.5892)	0.1810	-0.7737	(0.5869)	0.1870
	Ireland	-0.1632	(0.6710)	0.8080	-0.1453	(0.6655)	0.8270
	Italy	-0.1572	(0.5281)	0.7660	-0.1638	(0.5287)	0.7570
	Luxembourg	0.5067	(1.2249)	0.6790	0.4763	(1.2186)	0.6960
	Netherlands	0.2795	(0.4726)	0.5540	0.2804	(0.4708)	0.5520
	Portugal	-0.6625	(0.4879)	0.1750	-0.6664	(0.4876)	0.1720
	Spain	-0.9108	(0.7621)	0.2320	-0.8874	(0.7494)	0.2360
	Sweden	0.5183	(0.4199)	0.2170	0.5064	(0.4216)	0.2300
	United Kingdom	-1.2437	(0.5741)	0.0300	-1.2316	(0.5683)	0.0300
	Intercept	4.5522	(2.7375)	0.0960	4.5378	(2.7364)	0.0970
	<i>N</i>		487			487	
	<i>R</i> ²		0.9615			0.9615	

Next, we evaluate potential bias induced by endogenous election timing. Following Schleiter and Tavits (2016), we instrument opportunistic elections with the cabinet's formal dissolution powers coded by Goplerud and Schleiter (2015) — country fixed effects must be omitted as several countries in our sample have never had an opportunistic election. In the first stage, we predict the probability of an opportunistic election for each country year, then impute these predicted probabilities into our total spending models. Note that predicted duration has a very large negative effect on opportunistic election timing, just as Diermeier and Stevenson (2000) would predict. However, adding the probability of opportunistic election into our spending model does not negate the effect of predicted duration.

Table A.5: Instrumental variable model accounting for opportunistic elections in spending. Dissolution power instruments opportunistic elections in the first stage.

Election Model					Spending Models					
Variable	Logistic Regression			Coef.	Pooled		Fixed Effects			
	Coef.	(SE)	<i>p</i>		(SE)	<i>p</i>	Coef.	(SE)	<i>p</i>	
Lagged	Opportunistic Election				0.4692	(1.3691)	0.7320	-0.1668	(1.6744)	0.9210
	Dissolution Power	0.1800	(0.0899)	0.0450						
	Expected Duration	-0.0012	(0.0005)	0.0130	-0.0003	(0.0002)	0.0860	-0.0004	(0.0002)	0.0160
	Parties in Government	-0.9586	(0.5954)	0.1070	0.2496	(0.1283)	0.0520	0.3483	(0.1805)	0.0540
	ENP	0.2655	(0.2602)	0.3080	0.0348	(0.0756)	0.6460	-0.1448	(0.1106)	0.1900
	Caretaker Time	-16.1787	(10.5761)	0.1260	0.9403	(0.7807)	0.2280	0.0222	(0.9143)	0.9810
	GDP Per Capita	-0.4272	(0.5835)	0.4640	1.4721	(0.1808)	0.0000	1.4895	(0.1870)	0.0000
	Unemployment Rate	0.0153	(0.2240)	0.9460	-0.4244	(0.0776)	0.0000	-0.4100	(0.0782)	0.0000
	Dependency Ratio	1.5652	(1.1260)	0.1650	0.1044	(0.3478)	0.7640	0.2550	(0.3530)	0.4700
	Trade Openness	0.0413	(0.0497)	0.4060	0.0183	(0.0177)	0.3020	0.0160	(0.0186)	0.3880
	Spending	0.0140	(0.0339)	0.6800	0.9294	(0.0112)	0.0000	0.8928	(0.0214)	0.0000
	Maastricht Era	0.4466	(0.6469)	0.4900	-0.4646	(0.2559)	0.0690	-0.2695	(0.3426)	0.4320
	Budgetary Constraint Index (BCI)	-2.3229	(1.8103)	0.1990	0.7201	(0.4722)	0.1270	1.1858	(0.7731)	0.1250
Parties in Government × BCI	1.4162	(0.9869)	0.1510	-0.4374	(0.2155)	0.0420	-0.6212	(0.2944)	0.0350	
Government Ideology	0.0757	(0.2493)	0.7610	-0.1259	(0.0790)	0.1110	-0.0328	(0.0864)	0.7040	
Concurrent	GDP Per Capita	0.4186	(0.5765)	0.4680	-1.4219	(0.1774)	0.0000	-1.4443	(0.1909)	0.0000
	Unemployment Rate	0.0559	(0.2247)	0.8030	0.3658	(0.0756)	0.0000	0.3793	(0.0779)	0.0000
	Dependency Ratio	-1.5018	(1.1070)	0.1750	-0.0441	(0.3444)	0.8980	-0.2191	(0.3538)	0.5360
	Trade Openness	-0.0515	(0.0504)	0.3070	-0.0207	(0.0176)	0.2390	-0.0250	(0.0183)	0.1710
Fixed Effects	Belgium							0.7675	(0.7941)	0.3340
	Denmark							0.4851	(0.3884)	0.2120
	Finland							-0.2468	(0.5183)	0.6340
	France							-0.0646	(0.6399)	0.9200
	Germany							-0.9175	(0.4652)	0.0490
	Greece							-0.8888	(0.6000)	0.1390
	Ireland							-0.0426	(0.6768)	0.9500
	Italy							-0.2303	(0.5507)	0.6760
	Luxembourg							0.7693	(1.2427)	0.5360
	Netherlands							0.1963	(0.4737)	0.6790
	Portugal							-0.7027	(0.5012)	0.1610
	Spain							-1.0103	(0.7545)	0.1810
	Sweden							0.5343	(0.4313)	0.2150
	United Kingdom							-1.3515	(0.5768)	0.0190
Intercept	-5.5643	(5.6216)	0.3220	1.4785	(1.7331)	0.3940	5.1605	(2.8299)	0.0680	
<i>N</i>		487			487		487			
<i>ln(likelihood)</i>		-83.2178			—		—			
<i>R</i> ²		—			0.9599		0.9623			

Here, we repeat the instrumental variable analysis for our deficit spending models. Our results hold.

Table A.6: Instrumental variable model accounting for opportunistic elections in deficits. Dissolution power instruments opportunistic elections in the first stage.

Election Model					Deficit Spending Models					
Variable	Logistic Regression			Pooled			Fixed Effects			
	Coef.	(SE)	<i>p</i>	Coef.	(SE)	<i>p</i>	Coef.	(SE)	<i>p</i>	
Lagged	OpportunisticElection			-0.9864	(1.7496)	0.5730	-0.9652	(1.9573)	0.6220	
	Dissolution Power	0.1838	(0.0929)	0.0480						
	Expected Duration	-0.0011	(0.0005)	0.0490	-0.0006	(0.0002)	0.0010	-0.0007	(0.0002)	0.0000
	Parties in Government	-1.0007	(0.6013)	0.0960	0.0885	(0.1587)	0.5770	0.4375	(0.1912)	0.0220
	ENP	0.2590	(0.2555)	0.3110	-0.2129	(0.0824)	0.0100	-0.1287	(0.1234)	0.2970
	Caretaker Time	-13.2356	(10.3432)	0.2010	0.5995	(0.8540)	0.4830	0.6674	(0.9775)	0.4950
	GDP Per Capita	-0.4022	(0.6262)	0.5210	1.1192	(0.2046)	0.0000	1.0825	(0.2021)	0.0000
	Unemployment Rate	0.0386	(0.2343)	0.8690	-0.5037	(0.0881)	0.0000	-0.5122	(0.0832)	0.0000
	Dependency Ratio	0.7094	(1.2921)	0.5830	-0.3775	(0.4050)	0.3510	-0.3627	(0.3991)	0.3640
	Trade Openness	0.0460	(0.0516)	0.3720	0.0176	(0.0182)	0.3320	0.0164	(0.0183)	0.3700
	Spending	-0.0405	(0.0825)	0.6240	0.8293	(0.0281)	0.0000	0.6984	(0.0422)	0.0000
	Maastricht Era	0.5364	(0.7121)	0.4510	-0.4265	(0.2945)	0.1480	-0.2607	(0.3532)	0.4600
	Budgetary Constraint Index (BCI)	-2.8251	(2.1010)	0.1790	0.9568	(0.6835)	0.1620	0.9271	(0.8782)	0.2910
Parties in Government × BCI	1.5630	(1.0017)	0.1190	-0.1401	(0.2886)	0.6270	-0.8680	(0.3191)	0.0070	
Government Ideology	0.0258	(0.2828)	0.9270	0.1115	(0.1017)	0.2730	0.0731	(0.1043)	0.4830	
Concurrent	GDP Per Capita	0.3510	(0.6231)	0.5730	-1.1633	(0.2032)	0.0000	-1.1241	(0.2089)	0.0000
	Unemployment Rate	0.0397	(0.2366)	0.8670	0.5183	(0.0868)	0.0000	0.5899	(0.0864)	0.0000
	Dependency Ratio	-0.6647	(1.2631)	0.5990	0.3274	(0.3964)	0.4090	0.3469	(0.3911)	0.3750
	Trade Openness	-0.0516	(0.0533)	0.3340	-0.0052	(0.0182)	0.7740	-0.0021	(0.0184)	0.9090
Fixed Effects	Belgium						-0.7106	(0.8978)	0.4290	
	Denmark						-0.2860	(0.4311)	0.5070	
	Finland						-2.3720	(0.6585)	0.0000	
	France						0.5078	(0.6844)	0.4580	
	Germany						-0.5445	(0.4544)	0.2310	
	Greece						0.7382	(0.5312)	0.1650	
	Ireland						-0.1720	(0.7571)	0.8200	
	Italy						1.3357	(0.5466)	0.0150	
	Luxembourg						-0.8831	(1.2487)	0.4790	
	Netherlands						-0.0980	(0.5587)	0.8610	
	Portugal						-0.4305	(0.4894)	0.3790	
	Spain						-0.5882	(0.6157)	0.3390	
	Sweden						-0.6370	(0.4533)	0.1600	
Intercept	-3.3943	(6.1585)	0.5820	4.0059	(2.0989)	0.0560	2.4323	(2.8218)	0.3890	
<i>N</i>		448		448			448			
<i>ln(likelihood)</i>		-72.712		—			—			
<i>R</i> ²		—		0.8613			0.8734			

The tables below summarize results from regressions modeling predicted duration in different ways for our deficit models. As noted in the main text, there are three ways to estimate these models: using the total remaining predicted duration, using a binary indicating that the cabinet has surpassed its predicted time in office, or using an implied interaction of the two. In the main text we present results using the total remaining predicted duration to maintain comparability with our total spending models. However, all three specifications support our prediction.

Table A.7: Implied interaction and indicator only model of deficit spending where “outlived expectations” is a binary indicating the cabinet has surpassed its predicted duration.

	Variable	Implied interaction			Indicator only		
		Coef.	(SE)	<i>p</i>	Coef.	(SE)	<i>p</i>
Lagged	Outlived Expectations	0.2264	(0.3305)	0.4930	0.5727	(0.2665)	0.0320
	Expected Duration	-0.0003	(0.0002)	0.0950			
	Parties in Government	0.5591	(0.1918)	0.0040	0.5612	(0.1925)	0.0040
	Effective Number of Parties	-0.1214	(0.1272)	0.3400	-0.1427	(0.1263)	0.2590
	Government Ideology	-0.0057	(0.0056)	0.3090	-0.0060	(0.0056)	0.2800
	Caretaker Time	-0.6361	(0.9368)	0.4970	-0.4644	(0.9314)	0.6180
	GDP Per Capita	1.0292	(0.2021)	0.0000	1.0225	(0.2042)	0.0000
	Unemployment Rate	-0.4947	(0.0850)	0.0000	-0.4896	(0.0859)	0.0000
	Dependency Ratio	-0.3642	(0.3834)	0.3420	-0.3192	(0.3861)	0.4080
	Trade Openness	0.0186	(0.0184)	0.3130	0.0178	(0.0186)	0.3400
	Deficit Spending	0.7126	(0.0417)	0.0000	0.7130	(0.0421)	0.0000
	Maastricht Era	-0.3050	(0.3689)	0.4080	-0.3160	(0.3718)	0.3950
	Budgetary Constraint Index (BCI)	1.1086	(0.8489)	0.1920	1.1719	(0.8475)	0.1670
	Parties in Government $\times$ BCI	-1.0415	(0.3271)	0.0010	-1.0379	(0.3269)	0.0020
Concurrent	GDP Per Capita	-1.0414	(0.2084)	0.0000	-1.0297	(0.2104)	0.0000
	Unemployment Rate	0.5785	(0.0880)	0.0000	0.5742	(0.0888)	0.0000
	Dependency Ratio	0.3750	(0.3773)	0.3200	0.3245	(0.3797)	0.3930
	Trade Openness	-0.0060	(0.0181)	0.7390	-0.0078	(0.0183)	0.6710
Country Effects	Belgium	-0.6313	(0.8996)	0.4830	-0.5627	(0.9022)	0.5330
	Denmark	-0.2540	(0.4234)	0.5490	-0.2681	(0.4220)	0.5250
	Finland	-2.5780	(0.6992)	0.0000	-2.5900	(0.7040)	0.0000
	France	0.3823	(0.6843)	0.5760	0.2654	(0.6743)	0.6940
	Germany	-0.4218	(0.4595)	0.3590	-0.4839	(0.4603)	0.2930
	Greece	0.8372	(0.5274)	0.1120	0.8037	(0.5321)	0.1310
	Ireland	-0.3180	(0.7570)	0.6740	-0.2121	(0.7654)	0.7820
	Italy	1.5076	(0.5345)	0.0050	1.4506	(0.5316)	0.0060
	Luxembourg	-1.4244	(1.2767)	0.2650	-1.1595	(1.2811)	0.3650
	Netherlands	-0.0009	(0.5614)	0.9990	0.0443	(0.5673)	0.9380
	Portugal	-0.1396	(0.4936)	0.7770	-0.0935	(0.4924)	0.8490
	Spain	-0.5328	(0.6226)	0.3920	-0.6300	(0.6227)	0.3120
	Sweden	-0.6815	(0.4531)	0.1330	-0.6282	(0.4628)	0.1750
	Intercept	0.2647	(2.7849)	0.9240	0.2966	(2.8442)	0.9170
	<i>N</i>	448			448		
	<i>R</i> ²	0.8724			0.8717		

D APPENDIX: BOOTSTRAPPED z -SCORE PLOTS

Figure A.3 summarizes the z -scores for each parameter estimate from all 1,000 spending models; both pooled and fixed effects. We plot the z -scores because they are pivotal statistics — i.e. their sampling distribution does not depend on unknown parameters — making them a good choice for comparing across models as we do here (Shao 2003).

Figure A.3: Aggregated results from bootstrapped model of spending as percent of GDP.

Note: Lagged spending variable is excluded from the plot to preserve the scale. Its z -scores are much larger relative to the other variables.

Table A.8: Aggregated results from bootstrapped model of social transfers as percent of GDP.

	Variable	Pooled Model			Fixed Effects		
		Mean	SD	<i>p</i>	Mean	SD	<i>p</i>
Lagged	Expected Duration	-0.0002	(0.0001)	0.0058	-0.0002	(0.0001)	0.0027
	Government Ideology	-0.0255	(0.0503)	0.3061	0.0007	(0.0555)	0.4971
	Parties in Government	0.0890	(0.0771)	0.1234	-0.0561	(0.0924)	0.2714
	ENP	0.0062	(0.0391)	0.4376	-0.1159	(0.0711)	0.0519
	Caretaker Time	0.2579	(0.3604)	0.2374	-0.3209	(0.4859)	0.2545
	GDP Per Capita	0.6308	(0.0875)	0.0000	0.6587	(0.0887)	0.0000
	Unemployment Rate	-0.2312	(0.0423)	0.0000	-0.2185	(0.0429)	0.0000
	Dependency Ratio	-0.3247	(0.1563)	0.0191	-0.4667	(0.1703)	0.0029
	Trade Openness	0.0183	(0.0082)	0.0123	0.0156	(0.0089)	0.0392
	Social Transfers	0.9412	(0.0096)	0.0000	0.8926	(0.0277)	0.0000
	Maastricht Era	-0.1805	(0.1218)	0.0690	-0.0523	(0.1743)	0.3824
	Budgetary Constraint Index (BCI)	0.6981	(0.2697)	0.0050	0.3605	(0.3286)	0.1372
	Parties in Government x BCI	-0.1973	(0.1198)	0.0497	-0.0246	(0.1302)	0.4256
Concurrent	GDP Per Capita	-0.6013	(0.0857)	0.0000	-0.6341	(0.0875)	0.0000
	Unemployment Rate	0.2111	(0.0416)	0.0000	0.2166	(0.0423)	0.0000
	Dependency Ratio	0.3426	(0.1560)	0.0140	0.4396	(0.1661)	0.0039
	Trade Openness	-0.0207	(0.0082)	0.0056	-0.0181	(0.0087)	0.0186
Fixed Effects	Belgium				0.5186	(0.4830)	0.1423
	Denmark				-0.0066	(0.1573)	0.4857
	Finland				0.1423	(0.2829)	0.3087
	France				-0.0193	(0.3298)	0.4779
	Germany				-0.3957	(0.2101)	0.0296
	Greece				-1.0746	(0.4114)	0.0047
	Ireland				-0.4885	(0.4937)	0.1607
	Italy				-0.2098	(0.2817)	0.2288
	Luxembourg				-0.3076	(0.5538)	0.2904
	Netherlands				-0.4627	(0.2739)	0.0460
	Portugal				-0.7177	(0.3571)	0.0227
	Spain				-0.8390	(0.4452)	0.0300
	Sweden				0.1571	(0.2131)	0.2302
	Intercept	0.8209	(0.8536)	0.1674	4.4586	(1.3551)	0.0005
	<i>N</i>		449			449	
	<i>R</i> ²		0.8601			0.8725	

Here, we examine the possibility that true durations outperform expected durations in predicting public spending. This is to evaluate the fit of a cycling under completely endogenous elections model that we discussed in the main text of the manuscript. In a world where spending and elections are co-determined, true durations should provide better fit than expected durations. Table A.9 suggests that this is not the case as the true durations covariate is of moderate statistical significance in the pooled model only and the fit is poorer than our expected durations model in the main text. Indeed, in every iteration of our bootstrap, expected durations predict spending more accurately than true durations, which supports our theoretical model over this alternative.

Table A.9: Aggregated results from bootstrapped model of public spending as percent of GDP, replacing expected duration with true duration.

	Variable	Pooled Model			Fixed Effects		
		Mean	SD	<i>p</i>	Mean	SD	<i>p</i>
Lagged	True Remaining Duration	-0.0003	(0.0002)	0.0766	-0.0002	(0.0002)	0.1154
	Government Ideology	-0.0999	(0.0850)	0.1198	-0.0499	(0.0933)	0.2968
	Parties in Government	0.2799	(0.1284)	0.0146	0.3106	(0.1760)	0.0380
	ENP	-0.0068	(0.0747)	0.4631	-0.1719	(0.1147)	0.0673
	Caretaker Time	0.2335	(0.7762)	0.3825	-0.4302	(0.9132)	0.3192
	GDP Per Capita	1.3886	(0.1907)	0.0000	1.4030	(0.1945)	0.0000
	Unemployment Rate	-0.4361	(0.0801)	0.0000	-0.4167	(0.0818)	0.0000
	Dependency Ratio	0.1021	(0.3519)	0.3850	0.1850	(0.3677)	0.3078
	Trade Openness	0.0252	(0.0180)	0.0801	0.0238	(0.0194)	0.1102
	Spending	0.9300	(0.0114)	0.0000	0.8987	(0.0234)	0.0000
	Maastricht Era	-0.5236	(0.2698)	0.0264	-0.3198	(0.3679)	0.1912
	Budgetary Constraint Index (BCI)	1.2281	(0.5318)	0.0106	1.0220	(0.7523)	0.0873
	Parties in Government x BCI	-0.5566	(0.2367)	0.0091	-0.4957	(0.2922)	0.0444
Concurrent	GDP Per Capita	-1.3432	(0.1873)	0.0000	-1.3701	(0.2013)	0.0000
	Unemployment Rate	0.3763	(0.0784)	0.0000	0.3898	(0.0812)	0.0000
	Dependency Ratio	-0.0479	(0.3472)	0.4451	-0.1688	(0.3631)	0.3214
	Trade Openness	-0.0270	(0.0179)	0.0657	-0.0292	(0.0189)	0.0617
Fixed Effects	Belgium				0.6560	(0.8935)	0.2315
	Denmark				0.4167	(0.3843)	0.1388
	Finland				-0.1611	(0.5257)	0.3792
	France				-0.0941	(0.6593)	0.4439
	Germany				-0.8080	(0.5048)	0.0543
	Greece				-0.7745	(0.6172)	0.1042
	Ireland				-0.2712	(0.7377)	0.3567
	Italy				-0.2372	(0.5624)	0.3360
	Luxembourg				0.7994	(1.2440)	0.2594
	Netherlands				0.1305	(0.5341)	0.4047
	Portugal				-0.5525	(0.5162)	0.1412
	Spain				-0.8615	(0.8250)	0.1484
	Sweden				0.5907	(0.4330)	0.0863
	Intercept	1.7349	(1.8436)	0.1735	5.4654	(2.9521)	0.0318
	<i>N</i>		449			449	
	<i>R</i> ²		0.9623			0.9631	

These models examine expected durations from alternative survival model, (Saalfeld 2011), to make certain that our results are robust to alternate measures of the IV. Our results hold under the alternative measure.

Table A.10: Aggregated results from bootstrapped model of public spending as percent of GDP, replacing CMS durability measure with Saalfeld.

	Variable	Pooled Model			Fixed Effects		
		Mean	SD	<i>p</i>	Mean	SD	<i>p</i>
Lagged	Expected Duration	-0.0003	(0.0002)	0.0475	-0.0003	(0.0002)	0.0536
	Government Ideology	-0.1027	(0.0848)	0.1133	-0.0599	(0.0934)	0.2609
	Parties in Government	0.2525	(0.1296)	0.0257	0.3026	(0.1757)	0.0424
	ENP	-0.0048	(0.0751)	0.4733	-0.1674	(0.1140)	0.0712
	Caretaker Time	0.4854	(0.7680)	0.2649	-0.1777	(0.9037)	0.4230
	GDP Per Capita	1.4241	(0.1929)	0.0000	1.4395	(0.1964)	0.0000
	Unemployment Rate	-0.4358	(0.0801)	0.0000	-0.4129	(0.0817)	0.0000
	Dependency Ratio	0.0805	(0.3526)	0.4097	0.1621	(0.3669)	0.3299
	Trade Openness	0.0232	(0.0181)	0.1011	0.0220	(0.0195)	0.1301
	Spending	0.9320	(0.0116)	0.0000	0.8996	(0.0234)	0.0000
	Maastricht Era	-0.5450	(0.2702)	0.0220	-0.3066	(0.3667)	0.2006
	Budgetary Constraint Index (BCI)	1.1997	(0.5292)	0.0120	1.0104	(0.7464)	0.0882
	Parties in Government x BCI	-0.5548	(0.2350)	0.0089	-0.5143	(0.2890)	0.0372
Concurrent	GDP Per Capita	-1.3825	(0.1896)	0.0000	-1.4146	(0.2036)	0.0000
	Unemployment Rate	0.3747	(0.0784)	0.0000	0.3875	(0.0811)	0.0000
	Dependency Ratio	-0.0299	(0.3481)	0.4655	-0.1521	(0.3621)	0.3379
	Trade Openness	-0.0246	(0.0181)	0.0874	-0.0268	(0.0191)	0.0804
Fixed Effects	Belgium				0.5633	(0.8974)	0.2657
	Denmark				0.4205	(0.3836)	0.1362
	Finland				-0.2887	(0.5224)	0.2882
	France				-0.1027	(0.6533)	0.4376
	Germany				-0.8149	(0.5028)	0.0518
	Greece				-0.8481	(0.6169)	0.0845
	Ireland				-0.3288	(0.7415)	0.3292
	Italy				-0.2843	(0.5619)	0.3066
	Luxembourg				0.8988	(1.2426)	0.2342
	Netherlands				0.0952	(0.5367)	0.4309
	Portugal				-0.6034	(0.5158)	0.1207
	Spain				-0.9407	(0.8235)	0.1268
	Sweden				0.5570	(0.4331)	0.0997
	Intercept	1.8805	(1.8414)	0.1533	5.8118	(2.9737)	0.0256
	<i>N</i>		449			449	
	<i>R</i> ²		0.9624			0.9632	

Here, we compare the of models employing expected durations derived from a survival model estimating risk as a function of replacement. Our predicted results hold with this measure (the two duration estimate share a strong, positive correlation), but the explanatory power is significantly reduced. Indeed, in every iteration of the bootstrap, dissolution risk predicted durations outperform pooled risk predicted durations, and pooled risk predicted durations outperform replacement risk predicted durations — just as we would expect if cabinets were concerned with preparing for elections, rather than replacements.

Table A.11: Aggregated results from bootstrapped model of public spending as percent of GDP, replacing election durability with replacement durability.

	Variable	Pooled Model			Fixed Effects		
		Mean	SD	<i>p</i>	Mean	SD	<i>p</i>
Lagged	Expected Duration	-0.0002	(0.0001)	0.0450	-0.0002	(0.0002)	0.0545
	Government Ideology	-0.0906	(0.0846)	0.1429	-0.0468	(0.0932)	0.3070
	Parties in Government	0.3103	(0.1278)	0.0076	0.3382	(0.1742)	0.0259
	ENP	0.0054	(0.0743)	0.4734	-0.1534	(0.1131)	0.0877
	Caretaker Time	0.5901	(0.7662)	0.2202	-0.0892	(0.9056)	0.4613
	GDP Per Capita	1.4056	(0.1914)	0.0000	1.4168	(0.1942)	0.0000
	Unemployment Rate	-0.4399	(0.0801)	0.0000	-0.4210	(0.0813)	0.0000
	Dependency Ratio	0.1297	(0.3550)	0.3588	0.1815	(0.3698)	0.3114
	Trade Openness	0.0246	(0.0181)	0.0870	0.0243	(0.0192)	0.1029
	Spending	0.9309	(0.0113)	0.0000	0.8984	(0.0232)	0.0000
	Maastricht Era	-0.5405	(0.2709)	0.0232	-0.2991	(0.3688)	0.2091
	Budgetary Constraint Index (BCI)	1.3806	(0.5388)	0.0052	1.0800	(0.7493)	0.0744
	Parties in Government x BCI	-0.6203	(0.2364)	0.0041	-0.5683	(0.2895)	0.0248
Concurrent	GDP Per Capita	-1.3586	(0.1877)	0.0000	-1.3895	(0.2009)	0.0000
	Unemployment Rate	0.3782	(0.0784)	0.0000	0.3933	(0.0806)	0.0000
	Dependency Ratio	-0.0635	(0.3506)	0.4280	-0.1580	(0.3656)	0.3331
	Trade Openness	-0.0272	(0.0180)	0.0651	-0.0278	(0.0189)	0.0701
Fixed Effects	Belgium				0.4541	(0.9070)	0.3086
	Denmark				0.4414	(0.3836)	0.1255
	Finland				-0.2328	(0.5182)	0.3266
	France				0.0506	(0.6676)	0.4687
	Germany				-0.7700	(0.5015)	0.0616
	Greece				-0.8026	(0.6170)	0.0969
	Ireland				-0.3984	(0.7523)	0.2986
	Italy				-0.1846	(0.5613)	0.3713
	Luxembourg				0.5173	(1.2645)	0.3410
	Netherlands				0.0159	(0.5445)	0.4876
	Portugal				-0.6933	(0.5185)	0.0902
	Spain				-0.8846	(0.8225)	0.1407
	Sweden				0.5353	(0.4358)	0.1096
	Intercept	1.2259	(1.8420)	0.2517	5.2050	(2.9658)	0.0395
<i>N</i>			449			449	
<i>R</i> ²			0.9623			0.9632	

Table A.12: Jackknifed models of public spending as percent of GDP, where each country is dropped out in succession. No model yields a z -statistic on our focal variable of greater than -1.984 (when Germany is dropped). The mean z -statistic is -2.664 and the smallest is -3.181 (when Ireland is dropped).

		Omitted Country														
	Variable	Austria	Belgium	Denmark	Finland	France	Germany	Greece	Ireland	Italy	Luxembourg	Netherlands	Portugal	Spain	Sweden	UK
Lagged	Expected Duration	-0.0004 (0.0002)	-0.0005 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)	-0.0003 (0.0002)	-0.0004 (0.0002)	-0.0005 (0.0002)	-0.0004 (0.0002)	-0.0005 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)	-0.0004 (0.0002)
	Government Ideology	-0.0379 (0.0904)	-0.0333 (0.0871)	0.0179 (0.1026)	-0.0265 (0.0901)	-0.0315 (0.0914)	-0.0133 (0.0858)	-0.0088 (0.0872)	-0.0063 (0.0880)	-0.0088 (0.0885)	-0.0134 (0.0860)	-0.0369 (0.0870)	-0.0589 (0.0889)	-0.0217 (0.0915)	-0.1011 (0.0856)	-0.0497 (0.0915)
	Parties in Government	0.3537 (0.1765)	0.4227 (0.2060)	0.3794 (0.1749)	0.3807 (0.1798)	0.3676 (0.1745)	0.3229 (0.1710)	0.2726 (0.1762)	0.3052 (0.1729)	0.2606 (0.1895)	0.3320 (0.1718)	0.4061 (0.1754)	0.4077 (0.1878)	0.3793 (0.1762)	0.4092 (0.1821)	0.3334 (0.1744)
	ENP	-0.1509 (0.1191)	-0.1375 (0.1117)	-0.1605 (0.1180)	-0.1606 (0.1159)	-0.1771 (0.1243)	-0.1427 (0.1104)	-0.1473 (0.1099)	-0.1250 (0.1110)	-0.1461 (0.1150)	-0.1294 (0.1086)	-0.0794 (0.1113)	-0.1529 (0.1287)	-0.1442 (0.1133)	-0.1580 (0.1110)	-0.1472 (0.1127)
	Caretaker Time	0.1098 (0.9650)	-0.0016 (0.9105)	0.0864 (0.9072)	0.4239 (0.9235)	-0.1417 (0.9177)	-0.3617 (0.9101)	-0.1101 (0.8987)	0.0115 (0.8788)	0.1634 (1.0205)	0.0557 (0.8816)	-0.1631 (0.9753)	0.5072 (0.9863)	0.0668 (0.9146)	-0.1337 (0.8999)	0.0771 (0.9043)
	GDP Per Capita	1.4932 (0.1896)	1.4663 (0.1872)	1.4422 (0.1856)	1.4232 (0.1889)	1.4978 (0.1869)	1.5426 (0.1889)	1.5045 (0.1901)	1.5519 (0.1956)	1.4391 (0.1886)	1.8211 (0.2008)	1.4607 (0.1835)	1.5255 (0.1863)	1.4596 (0.1882)	1.4623 (0.1875)	1.4382 (0.1951)
	Unemployment Rate	-0.4129 (0.0793)	-0.4137 (0.0806)	-0.4152 (0.0807)	-0.3679 (0.0807)	-0.4089 (0.0786)	-0.4129 (0.0777)	-0.4195 (0.0786)	-0.4139 (0.0767)	-0.4366 (0.0804)	-0.3099 (0.0799)	-0.4214 (0.0768)	-0.4140 (0.0787)	-0.4676 (0.0953)	-0.3861 (0.0787)	-0.4197 (0.0810)
	Dependency Ratio	0.1636 (0.3555)	0.1605 (0.3461)	0.0702 (0.3325)	0.2782 (0.3365)	0.2323 (0.3357)	0.7133 (0.3343)	0.2575 (0.3293)	0.2043 (0.3480)	0.1975 (0.3735)	0.3721 (0.3316)	0.1820 (0.3227)	0.2006 (0.3351)	0.1757 (0.3353)	0.3096 (0.3433)	0.1706 (0.3670)
	Trade Openness	0.0124 (0.0186)	0.0163 (0.0201)	0.0178 (0.0188)	0.0199 (0.0186)	0.0163 (0.0185)	0.0136 (0.0185)	0.0170 (0.0188)	0.0040 (0.0201)	0.0170 (0.0185)	0.0154 (0.0200)	0.0142 (0.0183)	0.0099 (0.0187)	0.0168 (0.0184)	0.0188 (0.0187)	0.0228 (0.0193)
	Spending	0.8944 (0.0217)	0.8968 (0.0216)	0.8939 (0.0213)	0.8831 (0.0220)	0.8918 (0.0210)	0.9011 (0.0206)	0.8913 (0.0210)	0.8954 (0.0226)	0.8894 (0.0217)	0.8853 (0.0207)	0.8898 (0.0211)	0.8941 (0.0215)	0.8951 (0.0218)	0.8808 (0.0229)	0.8962 (0.0232)
	Maastricht Era	-0.3353 (0.3518)	-0.2663 (0.3445)	-0.3127 (0.3464)	-0.2096 (0.3523)	-0.3332 (0.3544)	-0.2420 (0.3318)	-0.3515 (0.3544)	-0.4476 (0.3343)	-0.2122 (0.3469)	-0.0979 (0.3508)	-0.1493 (0.3250)	-0.2354 (0.3454)	-0.2200 (0.3459)	-0.3229 (0.3544)	-0.2982 (0.3664)
	Budgetary Constraint Index (BCI)	1.2743 (0.7491)	1.3039 (0.7689)	1.2163 (0.7544)	1.1567 (0.7448)	1.3714 (0.8101)	1.2813 (0.7653)	0.2935 (0.8266)	1.2242 (0.7439)	1.1117 (0.7454)	1.5102 (0.7262)	0.8706 (0.7802)	1.3714 (0.7693)	1.4876 (0.8088)	1.4857 (0.7733)	1.0988 (0.7451)
Parties in Government x BCI	-0.6234 (0.2866)	-0.8372 (0.3326)	-0.6294 (0.2927)	-0.5870 (0.2835)	-0.6793 (0.3035)	-0.6127 (0.2783)	-0.4571 (0.2924)	-0.6181 (0.2799)	-0.5365 (0.2921)	-0.6350 (0.2812)	-0.7163 (0.2793)	-0.6703 (0.2965)	-0.6829 (0.2911)	-0.7027 (0.2846)	-0.5659 (0.2898)	
Concurrent	GDP Per Capita	-1.4479 (0.1929)	-1.4280 (0.1909)	-1.3944 (0.1888)	-1.3949 (0.1928)	-1.4499 (0.1897)	-1.4970 (0.1921)	-1.4449 (0.1924)	-1.4735 (0.1952)	-1.4067 (0.1922)	-1.7821 (0.2059)	-1.4095 (0.1863)	-1.4833 (0.1891)	-1.4229 (0.1920)	-1.3985 (0.1908)	-1.4113 (0.2013)
	Unemployment Rate	0.3822 (0.0786)	0.3776 (0.0799)	0.3621 (0.0817)	0.3339 (0.0801)	0.3816 (0.0781)	0.3718 (0.0769)	0.3837 (0.0782)	0.4062 (0.0765)	0.3999 (0.0809)	0.2819 (0.0790)	0.3796 (0.0767)	0.3838 (0.0782)	0.4248 (0.0969)	0.3936 (0.0781)	0.3940 (0.0803)
	Dependency Ratio	-0.1266 (0.3578)	-0.1296 (0.3456)	-0.0178 (0.3347)	-0.2555 (0.3386)	-0.1956 (0.3371)	-0.6975 (0.3385)	-0.2099 (0.3324)	-0.1352 (0.3513)	-0.1557 (0.3762)	-0.3816 (0.3295)	-0.1327 (0.3247)	-0.1634 (0.3382)	-0.1441 (0.3375)	-0.2443 (0.3418)	-0.1465 (0.3628)
	Trade Openness	-0.0196 (0.0182)	-0.0218 (0.0195)	-0.0259 (0.0183)	-0.0268 (0.0181)	-0.0251 (0.0180)	-0.0246 (0.0179)	-0.0251 (0.0183)	-0.0238 (0.0197)	-0.0231 (0.0181)	-0.0367 (0.0195)	-0.0227 (0.0180)	-0.0189 (0.0182)	-0.0243 (0.0179)	-0.0284 (0.0183)	-0.0265 (0.0189)
Fixed Effects	Austria	—	4.9937 (2.8392)	4.4972 (2.9515)	6.2347 (3.0282)	5.1692 (2.8283)	5.4735 (2.8527)	4.6992 (2.8701)	3.9621 (2.8751)	5.3450 (2.8538)	8.1995 (3.2242)	4.4545 (2.7118)	4.9572 (2.9015)	5.1958 (2.8507)	4.1459 (2.9626)	5.4636 (2.9539)
	Belgium	5.5963 (3.0807)	—	0.8107 (0.7984)	0.6359 (0.8057)	0.8558 (0.8072)	1.0195 (0.8213)	0.9007 (0.7891)	1.2595 (0.8253)	0.8019 (0.8009)	1.4234 (0.8164)	0.5020 (0.7881)	0.6539 (0.8582)	0.6829 (0.7896)	0.5646 (0.8047)	0.4440 (0.9025)
	Denmark	-0.2102 (0.8243)	0.5831 (0.3853)	—	0.5425 (0.3847)	0.4966 (0.3973)	0.4790 (0.3957)	0.7196 (0.3889)	0.3283 (0.3792)	0.5271 (0.3956)	0.3130 (0.3798)	0.5371 (0.3967)	0.4392 (0.4014)	0.4243 (0.3860)	0.3433 (0.3861)	0.4710 (0.3832)
	Finland	-0.8844 (0.7355)	-0.2947 (0.5420)	-0.1368 (0.5264)	—	-0.2001 (0.5252)	-0.0820 (0.5179)	-0.0102 (0.5166)	-0.2603 (0.5101)	-0.0513 (0.5426)	-0.4907 (0.5206)	-0.4022 (0.5231)	-0.3671 (0.5666)	-0.2517 (0.5218)	-0.4711 (0.5253)	-0.2666 (0.5189)
	France	-0.7295 (1.1715)	0.2863 (0.6681)	0.0688 (0.6559)	0.0217 (0.6355)	—	-0.1424 (0.6553)	0.4845 (0.6545)	-0.5207 (0.6380)	0.0131 (0.6438)	-0.7069 (0.6579)	0.3070 (0.6659)	-0.1601 (0.6559)	-0.1288 (0.6414)	-0.4311 (0.6697)	0.0492 (0.6581)
	Germany	-1.5445 (1.0416)	-0.7918 (0.4614)	-0.8438 (0.4663)	-0.9104 (0.4495)	-0.9486 (0.4528)	—	-0.7706 (0.4514)	-1.1845 (0.4555)	-0.8331 (0.4542)	-1.3464 (0.4687)	-0.8125 (0.4473)	-0.9293 (0.4638)	-0.8877 (0.4608)	-1.0855 (0.4646)	-0.8088 (0.4978)
	Greece	-1.4882 (1.0615)	-0.7727 (0.5912)	-0.6857 (0.6017)	-0.9715 (0.5960)	-0.9148 (0.5871)	-0.8402 (0.5835)	—	-1.0314 (0.5922)	-0.9367 (0.5886)	-1.4742 (0.6260)	-0.7351 (0.5850)	-0.8672 (0.5924)	-0.8477 (0.6109)	-1.0670 (0.6054)	-0.8815 (0.6175)
	Ireland	-0.8053 (0.7488)	-0.0424 (0.6887)	0.0046 (0.6785)	-0.2300 (0.6918)	-0.1091 (0.6831)	0.1291 (0.7304)	0.1477 (0.6789)	—	-0.2459 (0.6939)	0.5382 (0.6950)	0.1058 (0.6731)	-0.0870 (0.6886)	-0.1151 (0.6909)	-0.4512 (0.6896)	-0.3149 (0.7468)
	Italy	-0.8625 (1.0008)	-0.0056 (0.5469)	-0.1146 (0.5805)	-0.3680 (0.5438)	-0.2180 (0.5365)	-0.1332 (0.5380)	0.0491 (0.5326)	-0.5043 (0.5350)	—	-0.8511 (0.5861)	-0.0470 (0.5392)	-0.3496 (0.5667)	-0.2061 (0.5506)	-0.4418 (0.5500)	-0.1856 (0.5587)
	Luxembourg	-0.1897 (1.1089)	0.4945 (1.2749)	0.5101 (1.2677)	0.7654 (1.2387)	0.6990 (1.2400)	1.2989 (1.2890)	0.5210 (1.2396)	1.6017 (1.3666)	0.5879 (1.2489)	—	0.4612 (1.2262)	0.8533 (1.2486)	0.6626 (1.2348)	0.2262 (1.2363)	0.4958 (1.2535)
	Netherlands	-0.5667 (0.6218)	0.1870 (0.4886)	0.2168 (0.4716)	0.1006 (0.4741)	0.2060 (0.4737)	0.3048 (0.4895)	0.4143 (0.4740)	0.4959 (0.5041)	0.1556 (0.4729)	0.4561 (0.4722)	—	0.1173 (0.4797)	0.1025 (0.4725)	0.0696 (0.4694)	-0.0119 (0.5421)
	Portugal	-1.3012 (0.8467)	-0.6324 (0.4975)	-0.5391 (0.4996)	-0.8537 (0.5178)	-0.6550 (0.4965)	-0.5913 (0.4881)	-0.4630 (0.4909)	-0.6129 (0.5070)	-0.7948 (0.4964)	-1.1437 (0.5241)	-0.6476 (0.4827)	—	-0.7026 (0.4983)	-0.7853 (0.5066)	-0.7399 (0.5154)
	Spain	-1.6360 (1.2371)	-0.8028 (0.7687)	-0.6443 (0.7866)	-1.0478 (0.7476)	-1.1119 (0.7563)	-0.9447 (0.7498)	-0.6451 (0.7468)	-1.4867 (0.7584)	-0.9911 (0.7550)	-1.7411 (0.7842)	-0.7083 (0.7466)	-1.0410 (0.7739)	—	-1.6358 (0.8149)	-0.9720 (0.8226)
	Sweden	-0.1565 (0.8726)	0.5372 (0.4246)	0.5305 (0.4277)	0.6787 (0.4337)	0.5323 (0.4312)	0.5568 (0.4339)	0.6649 (0.4275)	0.3464 (0.4440)	0.5615 (0.4316)	0.4865 (0.4197)	0.5550 (0.4301)	0.5098 (0.4280)	0.4920 (0.4268)	—	0.5280 (0.4327)
	UK	-2.0247 (1.1213)	-1.1864 (0.5981)	-1.3131 (0.5870)	-1.3373 (0.5743)	-1.4720 (0.5950)	-1.3232 (0.5852)	-0.9070 (0.5683)	-1.6981 (0.5727)	-1.3942 (0.5865)	-1.8746 (0.5865)	-1.0312 (0.5897)	-1.3710 (0.5961)	-1.4268 (0.5864)	-1.6726 (0.6108)	—
N		452	451	450	455	460	449	463	449	459	474	456	458	458	449	449

E ANALYSIS OF CONTEXTUAL EFFECT VARIABILITY

In this section, we attempt to discover the roots of country level variability in cycling. We begin by deriving country-specific estimates of cycling behavior by reestimating our main spending model (without country fixed-effects) while allowing for random intercepts and coefficient estimates on expected duration at the country-level. As in the main text, we iterate this estimation through each of our 1,000 predicted cabinet durations and we save each country-level random coefficient in each iteration of the loop.

These country-level parameter estimates are then regressed on Kayser and Lindstädt’s (2015) country-level mean measure of electoral competitiveness. These estimates use the institutional context (the votes to seats conversion in particular) and the empirical regularities of party competition to capture the probability of the plurality party losing its status (and therefore changing the expected formateur), given a 1% change in vote-share. For our purposes, these loss probabilities represent the electoral incentive to engage in cycling behavior — the higher the loss probability, the greater the marginal return on small changes in voter share, and therefore the greater the incentive for manipulation. In Figure A.4, we plot the standardized country-level cycling estimates (where more negative values indicate a greater propensity to increase spending as expected duration draws to a close) against the Kayser and Lindstädt (2015) loss probabilities, where the dark red triangles are the means of both measures and the light blue circles are the entire distribution of estimates — we draw the loss probabilities from a normal distribution defined by Kayser and Lindstädt’s (2015) country-level mean and standard deviation estimates. We also provide the results of a bivariate regression and draw the fitted line.

The results are encouraging and suggest that the temptation to engage in cycling is driven by a history of incumbent vulnerability — that cabinets are more likely to manipulate where their grasp on power is more tenuous. However, these results are preliminary and should be taken with a grain of salt. More data and a more focused analysis is needed to draw stronger conclusions.

Figure A.4: Regressing Kayser and Lindstädt's measure of incumbent loss probability on country-specific estimates of cycling behavior.

F IDENTIFYING POLITICAL BUDGET CYCLES

A salient question for our analysis is exactly how political budget cycles in Europe went undetected despite the research attention dedicated to uncovering them. In C we partly addressed this issue by replacing our measure of expected cabinet duration with the cabinet’s true duration and finding that the results when using true duration are weaker and less consistent than those in evidence when using our predictions of expected cabinet duration.

We provide here a deeper two-part discussion of the important question of how political budget cycles escaped detection. First, we consider the role of transparent budget practices in moderating the relationship between election proximity and public spending following Alt and Lassen (2006). Second, we present a simulated data experiment aimed at examining directly whether flexible election timing might – in an idealized setting – mask the relationship between cabinets’ expectations of imminent elections and increases in public spending.

In the main text, we review previous research suggesting that budget cycles should only persist in settings lacking budget transparency, but all of the cases in our data have fairly transparent budgeting practices. To assess how much bearing this has on our results, we had to first derive a measure of transparency and we do so following Alt and Lassen (2006). Alt and Lassen’s (2006) transparency measures do not perfectly overlap our data (about 30% of our cases are omitted) and we could not access the original OECD budget practices survey that they use to derive the measure so we accessed the most recent survey and estimated a factor analysis of the 23 transparency components in the instrument. Our measure results in a very close match to the original Alt and Lassen (2006) scale as shown in Table A.13, where our measure is regressed on the measure that Alt and Lassen (2006) use for our 11 overlapping cases. We then replicate our main model including an interaction of this transparency measure and our durability measure. We find no moderating effect of transparency on our variable of interest (the interaction parameter is signed in the wrong direction and insignificant), although transparency does seem to constrain total spending. These models are in Table A.14.

Table A.13: Regression of our transparency measure on Alt and Lassen’s original measure

Variable	Coefficient	(Std. Err.)
Our measure	0.415	(0.186)
Intercept	-1.916	(0.748)

This indicates that our results are driven by something other than the budget transparency of the cases in our sample. There are important differences between our study and Alt and Lassen (2006) that may explain the empirical difference. Importantly, our sample of countries is quite different from theirs. But it is also salient that Alt and Lassen (2006) assume (at least implicitly) that elections are fixed and we do not. We have argued that the assumption of fixed elections is likely to be driving the difference between our reported findings and those in previous research. Such a claim is difficult to assess directly from the data, however. After all, the field only has access to data from a finite and slowly expanding number of cabinets across all of Europe.

Table A.14: Replication of main model interacting expected durations with transparency.

	Variable	Fixed Effects		Pooled Model	
		Mean	SE	Mean	SE
Lagged	Expected Duration	-0.0004	(0.0002)	-0.0003	(0.0001)
	Transparency	-0.6845	(0.3229)	-0.0638	(0.1400)
	Expected Duration x Transparency	-0.0001	(0.0001)	-0.0001	(0.0001)
	Government Ideology	-0.0334	(0.0858)	-0.1283	(0.0781)
	Parties in Government	0.3681	(0.1739)	0.1859	(0.1240)
	ENP	-0.1401	(0.1091)	0.0728	(0.0778)
	Caretaker Time	-0.0142	(0.8911)	0.6014	(0.7890)
	GDP Per Capita	1.4857	(0.1844)	1.4781	(0.1813)
	Unemployment Rate	-0.4142	(0.0780)	-0.4248	(0.0774)
	Dependency Ratio	0.2479	(0.3310)	0.1802	(0.3300)
	Trade Openness	0.0166	(0.0185)	0.0193	(0.0176)
	Spending	0.8929	(0.0208)	0.9280	(0.0112)
	Maastricht Era	-0.2744	(0.3430)	-0.4521	(0.2554)
	Budgetary Constraint Index (BCI)	1.2601	(0.7432)	0.8726	(0.4876)
	Parties in Government x BCI	-0.6502	(0.2799)	-0.4847	(0.2183)
Concurrent	GDP Per Capita	-1.4392	(0.1875)	-1.4236	(0.1773)
	Unemployment Rate	0.3825	(0.0774)	0.3721	(0.0757)
	Dependency Ratio	-0.2090	(0.3327)	-0.1169	(0.3280)
	Trade Openness	-0.0258	(0.0180)	-0.0228	(0.0175)
Fixed Effects	Belgium	0.4733	(0.7716)		
	Denmark	1.1364	(0.3540)		
	Finland	-0.5336	(0.5470)		
	France	0.6951	(0.4187)		
	Germany	-0.9061	(0.4486)		
	Greece	0.2384	(0.5308)		
	Ireland	-0.5279	(0.7213)		
	Italy	-1.6506	(0.9840)		
	Luxembourg	1.2394	(1.3073)		
	Netherlands	0.1736	(0.4681)		
	Portugal	0.3303	(0.5206)		
	Spain	0.5205	(0.5892)		
	Sweden	0.7806	(0.4169)		
	Intercept	4.6347	(2.8227)	1.4102	(1.7319)
	N		488		488
	R^2		0.9628		0.9613

We propose one way of probing this argument is to investigate simulations of political budget cycles to determine whether it is *possible* for the assumption of fixed elections to mask a simulated true relationship between *expected* election timing and budget increases. To that end, we describe a simulated data experiment designed to investigate whether stochastic early terminations allowed by the flexible election calendar in the countries we study could mask a real relationship between election timing and increases in public spending.

Our simulation proceeds by generating simulated data sets about the size of the one we analyze. We generate data for 432 “cabinets” by sampling from the observed lifespans in our data. For each of our 432 real cabinets, we have generated 1,000 bootstrapped forecasts of cabinet lifespan, yielding 432,000 matched pairs of actual and predicted lifespans. From these pairs, we simulate new datasets by repeatedly resampling 432 matched pairs of actual and predicted lifespans without replacement.

Simulated cabinets pass annual budgets while they survive, so each simulated data set consists of around 1,000 cabinet-budget year observations. Budgets passed within 365 days of the simulated cabinet’s expected dissolution are inflated to 104% (we vary this later) of the cabinet’s normal budget size. All of each cabinet’s other budgets are otherwise the same

size. Budget sizes vary across cabinets; we randomly assign a budget size for each simulated cabinet from the range of observed budget sizes in our real data.

In this way, the simulations create an imaginary setting in which our theory holds true in a very blunt way. Since the simulations draw randomly and evenly from our data on actual duration and expectations, these results are not necessarily indicative of the magnitude of relationships we should observe in real life. However, they can demonstrate whether the econometric relationship between expected durations and budgets is generally stronger than that between actual durations and budgets in a perfectly controlled setting in which we know our theory holds. If we find this is the case in the simulated world, then we believe this supports our argument that the assumption of fixed election timing has contributed to masking evidence for political budget cycles in previous work.

We test our expectations using simple bivariate analyses of the simulated data. For each simulated data set, we regress the size of the budget on the cabinet’s remaining “actual” duration and separately on its “expected” remaining duration. Results presented here use runs of 2,000 data sets of 435 cabinets each to examine the performance of each measure in turn. As anticipated, when measuring time remaining to elections using cabinet expectations, many more simulated data sets produce t-statistics below the significance threshold of -1.96 on our expected negative relationship between time remaining and the size of budgets.

Figure A.5 plots histograms of t-statistics from 2,000 bivariate regressions of budgets on both actual and expected simulated cabinet duration. Figure A.6 plots histograms of the same p-values (excluding any that are statistically significant in the wrong direction).

Figure A.5: Comparison of t-statistics in regressions on simulated cabinet data (4% budget increase). Vertical dashed line indicates significance threshold of -1.96.

As the figures illustrate, significant findings in the correct direction are much more common when measuring election proximity via cabinets’ expectations of their duration. Indeed, p-values in regressions using actual remaining cabinet duration appear to be distributed uniformly between zero and one. Furthermore, this general pattern remains the same for smaller increases in budgets, as we see in figures A.7 and A.8.

Figure A.6: Comparison of p-values in regressions on simulated cabinet data (4% budget increase). Vertical dashed line indicates significance threshold of 0.05.

Figure A.7: Comparison of p-values in regressions on simulated cabinet data (3% budget increase). Vertical dashed line indicates significance threshold of 0.05.

Figure A.8: Comparison of p-values in regressions on simulated cabinet data (2% budget increase). Vertical dashed line indicates significance threshold of 0.05.

Although these simulations are based on a brutally simplified ideal world, we believe that they capture the essence of how cycles can evade econometric detection if one treats election timing as fixed. The hypothesized relationship between election proximity and budget increases is far more likely to be apparent in the simulations when one replaces the measure of actual time to the next election with the cabinet's expectation of the proximity of the next election. In the simulation, as we argue it is in the real world, it is the cabinet's expectation about the timing of the next election that determines their decision to inflate spending. Even in our highly simplified simulation setting, this crucial distinction is almost always sufficient to wash out an econometric relationship between actual election timing and budget shifts.