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The Fiscal Politics of Turnover and Tenure: Partisan Competition and Interterm Cycles

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ABSTRACT

This paper argues that fiscal policies vary with governmental turnover and tenure in ways that have been overlooked by prior research. It posits a discrepancy between first-term and non-first-term governments: The former consider fiscal adjustments to cultivate partisan reputations, whereas the latter either maintain the status-quo balance or increase deficit spending to buoy their electoral support. The model anticipates first-term heterogeneity, interterm budget cycles, and a last-term effect, in which fiscal deterioration is greater among governments that lose reelection than among those that win another term. An analysis of term-to-term debt trajectories between 1970 and 2019 for twenty-two democracies supports the theory and veto players theory. Fiscal trajectories are most varied among first-term (post-turnover) governments; debt growth is fastest among last-term (pre-turnover) governments;

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multi-term governments frequently follow a restraint-to-expansion cycle; and all three patterns are more pronounced for majority governments than for coalitions or minority governments.

Keywords: Fiscal politics; partisan competition; political budget cycles; veto players

How do electoral dynamics affect fiscal policies? In the voluminous literature on political budget cycles, there is a consensus answer: Many governments do increase deficit spending as elections approach, but others do not for one reason or another (see Alt and Rose, 2009; Streb *et al.*, 2009; Aaskoven and Lassen, 2017; Veiga *et al.*, 2019). By contrast, the literature that links deficits and debt to partisan alternation in government is marked by incertitude, with little empirical support (Franzese Jr., 2002, Potrafke, 2017) for any of the best-known theories (e.g. Hibbs, 1987; Persson and Svensson, 1989; Alesina and Tabellini, 1990). The problem may be the emphasis on certain programmatic motivations—e.g., macroeconomic outcomes, the size of government—and the neglect of other ways that partisan alternation may affect fiscal imbalances. Indeed, scholars have yet to examine several important questions about the fiscal implications of partisan turnover and tenure, including: Do first-term governments make larger fiscal adjustments than later-term governments? Is it common for governments to follow an *interterm* budget cycle, with first-term restraint giving way to expansionism in subsequent terms? And do last-term governments amass larger debts than governments that win another term because they anticipate their vulnerability and use fiscal expansionism in a desperate attempt to avoid defeat?

This study pursues these and other questions about interterm fiscal dynamics. It develops a theory of fiscal politics that, among other things, extends the concept of political budget cycles. The standard (within-term) budget-cycle idea is that deficits swell as elections near, because governments believe that voters will reward fiscal expansionism. An interterm analogue—with fiscal deterioration *across* a government's terms—seems common in at least one country: Britain. The pattern was especially pronounced during Tony Blair's prime ministership (1997–2007) (Johnson, 2016), but something similar occurred in the adjacent two Conservative Party runs, the first under Prime Ministers Margaret Thatcher and John Major (1979–1997) and the second under David Cameron and his successors (2010–2024)—in each case, the government shifted from a first-term austerity program to a looser stance that accommodated new

expenditures or tax cuts.[1] It is unknown, however, whether this interterm trajectory is common elsewhere.

My theory does not expect all multi-term governments to follow a restraint-to-expansion cycle. Rather, it posits that first-term governments may pursue any fiscal policy—including marked consolidations or expansions—to cultivate programmatic and managerial reputations, whereas non-first-term governments are more likely to either maintain the status-quo fiscal balance or increase deficit spending to buoy their electoral support. The model thus anticipates a wider distribution of fiscal trajectories among first-term governments than among non-first-term governments. However, it also anticipates frequent *interterm budget cycles*, because restraint and retrenchment are largely first-term phenomena, whereas later terms tend toward expansionism. The theory also predicts a *last-term effect,* in which deficits and debts increase more under non-first-term governments that lose reelection at term's end than among those that win another term. The phenomenon resembles the Jacobson effect in campaign finance, in which spending by incumbent legislators correlates inversely with their electoral results—not because it is counter-productive, but because it is reactive, used in proportion to the threat of defeat (Jacobson, 1978).

I test these dynamics and their attenuation by partisan veto players with an original dataset of political turnover and interterm debt trajectories. Specifically, I study across-term differences in annualized rates of debt growth using three measures of the debt-to-GDP ratio, each covering a partially different set of countries and terms from 1970 through 2019. In part of the analysis, I estimate the effect of term type (e.g., first term, last term) on debt growth in a dynamic regression model that includes various economic and political controls. In another part, I exclude the term and political variables from the model and examine how the residuals—i.e., the debt fluctuations that are not explained by economic conditions—vary by term type. This approach allows

[1]After the 2010 elections transferred power from the Labour Party to a Conservative-led coalition, the government introduced a pro-cyclical austerity program. In 2015, Cameron's party was reelected with a majority, and it later modestly increased spending (*The Economist*, 2019). When Boris Johnson became prime minister in 2019, and especially when he replaced Sajid Javid with Rishi Sunak as Chancellor of the Exchequer in mid-February of 2020, it seemed as if additional loosening was ahead (Mason and Stewart, 2020). It did subsequently occur, and on a massive scale, but because of an unexpected shock: the coronavirus pandemic. In late 2022, when the pandemic had waned and left Britain with greater debt and inflation, Johnson's successor, Liz Truss, sought additional deficit-financed tax cuts, perhaps because she thought it would boost the economy and her party's approval ratings. But, bond rates spiked in response to the plan, and her agenda and prime ministership came to an abrupt end. The next prime minister, Sunak, heeded the new market constraints and restored the pre-Truss fiscal stance. He also promised a spending-focused consolidation, though not until after the 2024 elections, which his party lost (*The Economist*, 2022).

a more conservative test of the last-term effect and a more thoroughgoing analysis of interterm heterogeneity and dynamics.[2]

The results are consistent with my theory. Three primary and novel findings are: (i) debt growth is greatest among last-term governments, (ii) debt trajectories are most wide-ranging among first-term governments, and (iii) both last-term debt growth and first-term heterogeneity are more pronounced under majority government than non-majority government.[3] The last-term result is consistent with reelection-seeking expansionism, and it complements previous research on that topic (e.g. Schultz, 1995; Aidt *et al.*, 2011; Veiga *et al.*, 2019). The finding that debt deceleration (or reduction) is most strongly associated with first-term, majority government also complements prior research, on fiscal consolidations and their political impediments (e.g. Roubini and Sachs, 1989; Alesina *et al.*, 2006). However, my analysis of fiscal trajectories is broader, and it shows that majority government is associated not only with first-term retrenchment, but also with debt acceleration, in first terms and in non-first-terms.

A fourth main finding is that interterm budget cycles (IBCs) are common, especially when there is majority government. The number of observations in that part of the analysis is small, because the terms data are collapsed into multi-term runs, but the patterns are striking. Specifically, I find that the frequency of IBCs among majority governments well-exceeds the counterfactual in which the observed debt trajectories are randomly distributed within runs. The frequency of IBCs among non-majority governments also exceeds the counterfactual, but by a smaller amount, and their average IBC has a smaller amplitude. In other words, this paper provides evidence for conditional interterm budget cycles, and its broader analysis of interterm heterogeneity shows that partisan turnover and tenure affect national fiscal dynamics in important and previously unrecognized ways.

1 Interterm Fiscal Dynamics

Electoral dynamics are prominent in the literature on comparative fiscal politics, and they come in two varieties: intraterm and interterm. The first relate to the proximity of elections within terms and, typically, the degree to which

[2]To clarify, my analyses compare fiscal trajectories across three types of administrations: first-term governments (FT = 1), non-first-term governments that lose at term's end (i.e., last-term governments, LT = 1), and other non-first-term governments (i.e., that win at term's end, and that I designate FT = LT = 0). The observations are terms, and the variables are constructed from annual-level source data, as in Crivelli *et al.* (2017). There is no attention to year-to-year or within-term dynamics; rather, I study fiscal heterogeneity and dynamics across terms, both of which are "interterm" analyses.

[3]Non-majority government refers to coalition or minority government or, in the case of the dataset's two presidential systems, divided government.

governments increase spending as their reelection bids approach. The interterm literature has a different focus, and, in fact, it seldom speaks of dynamics. Instead, it concentrates on how certain electoral reconfigurations of power cause fiscal heterogeneity across governmental terms, although term-to-term dynamics are an obvious implication. The most well-known of these theories focus on the differences between left-party and right-party governments, and they offer competing ideas about how shifts from one to the other affect fiscal imbalances.[4] For example, whereas Hibbs's (1987) political-economy theory anticipates tighter fiscal policy (i.e., smaller deficits) by right-leaning governments, Persson and Svensson's (1989) size-of-government theory expects the opposite, because the spending cuts that right-party governments desire are difficult to achieve in the absence of fiscal stress. Alesina and Tabellini (1990) advance another idea: Partisan turnover in either direction causes debt growth, because governing parties that anticipate defeat use debt to limit their successors' fiscal maneuverability.[5]

In another strand of research, interterm fiscal fluctuations relate to political fragmentation and the common-pool problem. These studies argue that more fragmented political environments boost deficits because of their greater number of relevant actors, all of whom externalize the costs of their favored programs (e.g. Kontopoulos and Perotti, 1999; Crivelli *et al.*, 2017). Although much of this literature focuses on cross-sectional variables like electoral systems, the temporal implication is clear: When elections alter party-system fragmentation in the government or legislature, deficits change accordingly.

A third strand of research, rooted in the veto players framework, reaches a different conclusion. The central tenet of veto players theory is that policy changes are less likely or more modest when there are more parties or actors with veto power, as there is under coalition government as opposed to majority government (Tsebelis, 2002). That implies that the prevailing fiscal policy becomes more resilient after an election (say) replaces a majority government with a coalition. However, the fiscal-political context is also relevant, for if deficit control is not urgent or salient, then veto players may choose compromise

[4]Although it is uncommon, partisan control of government *can* change during a term; and the more that that occurs, the less a partisan theory can be depicted as an "interterm" theory. The same goes for analyses that emphasize variables like party-system fragmentation that typically, but not exclusively, assume new values at electoral junctures. Put differently, my depiction of partisan and fragmentation models as "interterm" theories emphasizes their primary and most relevant dynamics.

[5]Analyses of national-level data have not provided strong support for any of the three theories (Potrafke, 2017). That may be because parties' preferences change with economic conditions (e.g. Müller *et al.*, 2016). Or, parties may be office-seeking, rather than strictly policy-seeking. Aghion and Bolton (1990) and Milesi-Ferretti and Spolaore (1994) study office-seeking models, but their parties are asymmetric, with debt growth providing an electoral benefit only to right-wing parties. A different approach, pursued here and in the literature on political budget cycles, is that both types of parties believe that fiscal expansionism can improve their electoral support.

instead of conflict, and their budget negotiations may assume a gains-from-trade, positive-sum dynamic (Cox and McCubbins, 2001). That may cause deficits to swell beyond what a majority government would choose—or it may not, because majority governments also enjoy deficits, and because veto players' negotiations may do less to increase the deficit than to fragment the budget. Thus, there is no clear relationship between deficits and the number of veto players except that which relates to the probability of major fiscal adjustments and the asymmetry between expansions and consolidations. Put simply, non-majority government frustrates fiscal reforms, especially those that reduce deficits.[6] This does imply that deficits will correlate positively with the number of veto players, which resembles the prediction of the common-pool theories. But their fragmentation–deficits relationship is monotonic and about varying degrees of deficit-push, whereas the veto players–deficits correlation relates to occasional consolidations by decisive, undivided governments.

Their differences notwithstanding, these various theories have one thing in common: None anticipates any fiscal shifts when the political variables are unchanged from one term to the next. Thus, none anticipates the British trends noted in this paper's introduction. Also, none considers a salient and basic difference between governing and opposition parties: Whereas the former tend to campaign on their accomplishments, the latter promise to change the status quo. I address these gaps with a partisan theory that emphasizes office-seeking opportunism and policy differentiation.

1.1 Turnover and Tenure

My theory makes two main assumptions about governing parties. The first is that they are office-seeking and pursue fiscal expansionism in proportion to their anticipated electoral vulnerability, because they expect it to bolster their electoral support. This same assumption motivates within-term budget cycles, and it is supported by a substantial literature (e.g. Schultz, 1995; Aidt *et al.*, 2011; Hübscher and Sattler, 2017; Veiga *et al.*, 2019). Also, a similar behavior is well-documented in legislative campaign spending: Incumbents tend to spend reactively, in proportion to the perceived strength of their challengers. That literature further finds that incumbents' spending correlates negatively with their electoral success, not because it is counterproductive, but because it provides only a marginal benefit, so the bivariate relationship is dominated by the reverse effect of vulnerability on spending (Jacobson, 1978; Johnson,

[6]Tsebelis (2002, pp. 187–192) reviews the evidence for the policy inertia account, especially the findings of Franzese Jr. (2002). The literature on fiscal consolidations also supports the theory, for it has found that reforms are more likely after elections concentrate power in one party (e.g. Roubini and Sachs, 1989). Outside that literature, analyses focus on a broader concept: the effective number of veto players. It is a difficult-to-discern quantity, in part because it varies across policy domains (Cox and McCubbins, 2001, pp. 24–26).

2018). I assume the same with fiscal expansionism: The benefit is marginal and incapable of overwhelming the (exogenous) causes of the government's vulnerability. Thus, in the aggregate, deficits and debts correlate negatively with the government's electoral success.[7]

The broader relationship between government tenure and fiscal expansionism is implicated by a relevant dynamic: the tendency for first-term governments to face more comfortable electoral terrain than non-first-term governments. To some degree, the phenomenon can be understood as simple regression to the mean. Generally, first-term governments take office with a popularity advantage over their opponents, including the governments they replaced. As that gap regresses to the mean, the probability of reelection declines; however, if the initial gap is large, then a government may be fairly secure in its *first* reelection bid, which in turn implies that fiscal restraint will be associated with first-term government. Alternatively, the process can be framed in terms of political developments. New governments often enjoy high levels of popularity because they have not yet faced adverse circumstances or made major missteps. Some new governments will continue to enjoy a sizable popularity advantage vis-à-vis the opposition when they wage their first reelection campaign; but at some point events will erode their popularity, and their opponents will improve their leadership, messaging, and coordination. The interterm fiscal implication is the same.

The second assumption is that first-term governments may deviate from the norm of strategic opportunism and introduce fiscal adjustments that cultivate programmatic or managerial reputations. Most significantly, new governments may implement austerity programs that reduce deficits and debts. Austerity is less risky after partisan turnover because new governments can allege that their predecessor's "irresponsible" policies made the adjustments necessary, and because voters who rejected the prior government are receptive to that argument. The new government is especially likely to consolidate if its electoral campaign had criticized its predecessor's deficits, but its measures could also come as a surprise (Stokes, 2001). Either way, a theme in its next campaign will be its fiscal responsibility; and if macroeconomic conditions have improved, the government will credit the adjustments. Of course, non-first-term governments can also initiate austerity programs, and if they do so early in their terms the risks may dissipate by the time of the next election. However, they cannot easily blame their opponents for the adjustments or for any lasting sluggishness

[7]This assumes all else is constant, including economic conditions, which affect both electoral prospects and the fiscal balance. So, the relationship I posit is with cyclically adjusted fiscal measures. Another clarifying point: The relationship reflects an omitted-variable problem, so an analysis that successfully addresses that issue would find a positive relationship between expansionism and electoral success.

in the economy. On the contrary, they make themselves vulnerable to attacks along those lines.[8]

To be clear, this theory does not model the economy, and it does not deny the possibility of expansionary austerity.[9] Rather, the argument is simply that governments generally expect to be rewarded for fiscal expansionism, and that the occasion in which they think that the electorate is most likely to accept (or reward) a consolidation is when it can be framed as an urgent corrective to the policies of the preceding government. This argument emphasizes partisan reputations and policy differentiation, and that is what underlies the parallel claim, inherent in the second assumption, that first-term governments are also more likely than non-first-term governments to significantly *increase* deficit spending in pursuit of programmatic objectives, like tax cuts or spending hikes. In other words, my general claim about turnover is that it correlates with fiscal volatility—i.e., a potential for large fiscal changes, in any direction. Non-first-term governments choose from a narrower range of policies; they either maintain the status quo or increase deficit spending, and the latter option will be more strongly associated with electoral vulnerability and defeat.

Put differently, this theory posits that the prevailing fiscal stance of strategic expansionism may be punctuated by the programmatic and reputation-building opportunities that follow the partisan transfer of power. The framework pre-

[8]The assumption that first-term governments face smaller political risks for consolidations than non-first-term governments suggests another idea: Among governments that consolidate, the implicit costs of *not* consolidating differ in the opposite direction. Consider those costs in a general way. Suppose that exogenous political or fiscal-economic conditions may arise that create pressures for governments to consolidate, and subsequently governments will balance the risks of consolidating against the risks of not consolidating. Further assume that when those pressures arise, a government will prefer to delay retrenchment until after its current term, and if instead the pressure leads it to act, it will prefer to introduce only a modest adjustment in the short term and to save more significant action until after the next election. In either case, the pressure to consolidate is likely to persist at the start of the subsequent term, and the government that is then in place will address the issue more aggressively. If those tended to be non-first-term governments, the pattern would counter the one hypothesized here—i.e., that large consolidations are associated with first-term governments. But that is unlikely to be the case, because the arrival of consolidation pressure in the preceding term, and so too any contemporaneous action to address it, does not bode well for that government's reelection. In other words, when we introduce exogenous pressures that can explain consolidations by non-first-term governments, we may still expect large adjustments to correlate with first-term government, and that expectation holds even though some first-term consolidations will be continuations of programs that were initiated by prior administrations.

[9]Alesina *et al.* (1998) argue that austerity programs can facilitate economic expansions, and they argue that consolidations are not always penalized by voters. My argument is that governments believe otherwise, and that they are more likely to introduce consolidations when they first come to power and are (thus) best-positioned to blame their predecessors for the adjustments. Regarding these types of adjustments, Strobl *et al.* (2021) make a similar argument and find supporting evidence. Mierau *et al.* (2007) also test whether consolidations are associated with new, post-turnover governments. They find no relationship, although their analysis does not differentiate governments by veto players.

sumes that legislative and executive terms are concurrent,[10] and it anticipates three interconnected patterns, plus a few more when we suppose that each may be moderated by veto players.[11] The first pattern, called first-term volatility, was already explained. The FTV hypothesis is that substantial fiscal adjustments, including both consolidations and expansions, are more likely after the partisan transfer of power than after elections that result in continuity, although the magnitude of first-term reforms may be rivaled by the fiscal expansions of vulnerable non-first-term governments. Framed in terms of cyclically adjusted debt growth, the hypothesis can be stated as follows.

> **H-FTV:** The distribution of debt trajectories for first-term governments will be wider and less positively skewed than the distribution for non-first-term governments.

The introduction of veto players affects the range of first-term trajectories, especially the part that is associated with debt reduction.

> **H-FTV-VP:** The distribution of first-term debt trajectories will be narrower for non-majority governments than for majority governments, especially in the portion that is associated with debt control (i.e., the bottom tail).

The second pattern is the interterm budget cycle (IBC), which occurs whenever a multi-term government's least-expansionary term is its first term. IBCs may or may not begin with a fiscal consolidation; so long as the first term is less expansionary than later terms, the cycle is said to occur. My theory does not indicate how many or which governments will follow the cycle because it allows first-term governments to choose expansionism. Even so, IBCs are likely to be more common than alternative multi-term trajectories because restraint and retrenchment are predominantly first-term phenomena. Framed again in terms of debt growth, the hypothesis is:

> **H-IBC:** Debt trajectories will not be randomly distributed across terms of multi-term governments; rather, the IBC pattern, with the least debt growth occurring during the first term, will be disproportionately common.

Veto players may reduce the frequency of IBCs, but their most straightforward consequence regards amplitudes.

[10] Non-concurrent systems require a more complex model because executive-legislative relations may vary within executive terms, and because policymakers' fiscal preferences may vary by election type.

[11] Online Appendix F considers how the fiscal patterns might also relate to early elections and strategic electoral timing. See note 15 for a summary.

> **H-IBC-VP:** The amplitudes of interterm budget cycles—corresponding to the amount of debt deceleration in first terms and/or the amount of debt acceleration in subsequent terms—will be smaller when there is non-majority government.

The third implication is the last-term effect—i.e., a negative correlation between debt growth and electoral success among non-first-term governments.[12]

> **H-LT:** Among non-first-term governments, average debt growth will be greater for those that lose reelection at term's end than for those that win.

Although veto players are more likely to block consolidations than expansions, I also consider the following hypothesis.

> **H-LT-VP**: Among non-first-term governments that lose reelection, average debt growth will be greater for majority governments than for non-majority governments.

Note that Alesina and Tabellini's (1990) model also anticipates something like a last-term effect, because vulnerable, policy-seeking governments use debt to limit their successors' budgetary maneuverability. However, that model does not anticipate fiscal restraint by any insecure, first-term governments, and it does not anticipate expansions by less vulnerable, first-term governments. My theory allows both possibilities.

2 Empirical Model and Data

2.1 Empirical Model

To test the hypotheses, I study three measures of the debt-to-GDP ratio, each of which spans a slightly different set of countries and terms. I analyze debt data over possible alternatives, like the primary deficit, because debt datasets are more comprehensive across countries and time. I use two methods to examine the relationship between debt growth and term type (e.g., first term, last term). One is a dynamic regression model that estimates the effect of term type while controlling for economic and political causes of debt fluctuations. In the other approach, I exclude the term and political variables from the regression and examine how the residuals—i.e., the debt growth that is not explained by a well-fitting economic model—vary by term type. This

[12]First-term governments are excluded to focus on the strategic expansionism of non-first-term governments, not the austerity programs that sometimes follow partisan turnover. In other words, the last-term effect is also likely to occur when first-term governments are included in the analysis, but then its cause would be less clear.

two-stage approach is useful because it is difficult to test for cycles and term heterogeneity in a linear regression model. Also, with the last-term effect, there could be an identification problem that relates to the dual effect of economic stagnation on debt growth, one that occurs via the "automatic" effects on revenues, social expenditures, and deficits, and the other that occurs indirectly via electoral vulnerability and discretionary expansionism. The two-stage approach protects against misattributing some of the former to the latter. It provides a conservative approach to the estimation of the last-term effect.

I treat both sets of variables—political and economic—as exogenous, which is defensible because any simultaneity bias is likely to be modest and related only to the economic variables. Issues that warrant greater attention are dynamic interdependence and unobserved heterogeneity in the cross-section. The latter, along with the focus on within-country dynamics, motivates a first-difference (FD) regression model, given by (1), in which X_{it} is a vector of predictors, D_{it} is the average annualized change in debt-to-GDP in country i during term t, and Δ signifies the first-difference across terms. However, the FD model does not allow a lagged dependent variable, which is useful to account for serial correlation. Therefore, I use the error-correction (EC) model, given by (2). The model, which is equivalent to the autoregressive distributed lag (ADL) model (Beck and Katz, 2011), has two parts: short-run (first-difference) relationships ($\Delta D_{it} = \beta^{EC}\Delta X_{it}$) and long-run or levels relationships ($D_{it-1} = \kappa X_{it-1}$). Essentially, the model "corrects" errors in the short-run component by accounting for the amount that the levels were out of equilibrium in t-1, with ψ as the correction parameter.

$$\Delta D_{it} = \alpha + \beta^{FD}\Delta X_{it} + u_{it} \tag{1}$$

$$\Delta D_{it} = \alpha + \beta^{EC}\Delta X_{it} - \psi(D_{it-1} - \kappa X_{it-1}) + v_{it} \tag{2}$$

In the Online Appendix C, I provide side-by-side comparisons of the FD and EC models, and I consider also the EC/ADL with a second lag of the dependent variable.[13] The analysis shows that the EC model offers a marked improvement over the FD model, with much less serial correlation; and it shows that a second lag of the dependent variable offers little additional improvement.

2.2 Data

Each of the three debt variables is from the International Monetary Fund (IMF). They are described in Table 1. Table 2 lists the countries and years

[13]Beck and Katz (2011) recommend trying the EC/ADL-LDV2 to study dynamics and serial correlation. They also discuss the use of ordinary least squares (OLS) for EC models, and they explain why integration does not occur in most politics-focused time-series datasets. In Online Appendix E, I provide robustness checks of my main regression estimates using both the ADL-LDV2 and a fixed-effects model that accounts for first-order autocorrelation.

covered by each series, which I truncate after 2019 to exclude the massive economic and fiscal shocks of the coronavirus pandemic. The observations are also determined by the availability of the economic variables and the following country-selection criteria: a population of at least one million, a Freedom House political rights score of three or better for the entire 1997–2018 period, a political system with concurrent legislative and executive terms, the absence of a state-controlled petroleum corporation,[14] and a panel with at least six terms of data, one of which is dropped due to first differencing.

To construct terms from annual data requires choices about bracketing. In particular, the challenge is to appropriately link annual fiscal data to particular governments, especially in post-election periods. Note that it is rare for a government elected in year y to be able to significantly alter that year's fiscal balance; much more frequently, a government elected in y can only affect the data for $y + 1$, and sometimes it does not shape fiscal outcomes until $y + 2$. Accordingly, I adopted the following coding scheme: A term expires at the end of y if there is an election that year, unless it is held in the last quarter of y and the fiscal year begins in January, in which case the expiring term continues through $y + 1$. Put differently, the economic and fiscal data for y are never linked to a government that comes to power in y. Instead, they are linked to the government that was in place at the start of the year or, if there had been a late-year election in the previous year and the country's fiscal year begins in January, to the pre-election government in $y - 1$. To assess this scheme, I researched fiscal and news reports for many new governments and their first-year budgets. I found that it was appropriate in most cases, but there were two in which a government that first assumed power after a late-year election was able to introduce immediate changes to the budget (i.e., for $y + 1$). In those cases, which are noted in Online Appendix A, I did not extend the end of the expiring term past the election year.

[14] Countries with a state-controlled oil or gas company may be able to use its resources (or an associated sovereign wealth fund) to make large, unexpected adjustments to the government's debt stock.

Table 1: Description of variables.

Variable	Description and source	Value
D1	General government gross debt, percent of GDP. (World Economic Outlook 2022, IMF)	Change over term, annual average
D2	General government debt, percent of GDP. (Global Debt Database 2022, IMF)	Change over term, annual average
D3	Gross public debt, percent of GDP. (Public Finances in Modern History Database 2023, Mauro *et al.* (2015), and IMF)	Change over term, annual average
GROWTH	Growth in GDP per capita, percent. (World Bank)	Average during term
UNEMPLOYMENT	Unemployment rate, percent of labor force. (IMF)	Average during term
INFLATION	Inflation rate. Annual percent change in consumer price index. (World Bank)	Average during term
BANK CRISIS	Estimated debt-to-GDP growth due to a banking crisis. (Laeven and Valencia, 2020)	Amount, split over terms by crisis years
EURO	For pre-1998 terms, Euro = 0. For EU member states in 1997, the first term that includes 1998 is Euro = 1; all subsequent terms are Euro = 2, unless the country never adopted the euro, then Euro = 1. For later adopters, Euro = 1 for the term that includes the two years prior its introduction, and =2 for subsequent terms.	0, 1, or 2
FT	Dummy for first-term government.	
LT	Dummy for last-term government.	
MAJ	Dummy for majority government.	
NMAJ	Equal to 1–MAJ.	
GFRAG	Party-system fragmentation in the governing coalition. GF = 0 if MAJ = 1, and values above 0 correspond to the number of within-sample standard deviations in the source variable, which is the "probability that two deputies picked at random from among the government parties will be of different parties" (Cruz *et al.*, 2021, p. 10).	Value in last year
LEFT	Dummy for left-leaning government. (Cruz *et al.*, 2021).	Value in last year
NFRULES	Number of all fiscal rules in place. (Schaechter *et al.*, 2012)	Value in last year

Note: Unless otherwise stated, the variable uses the author's coding, based on sources listed in Online Appendix B.

Table 2: Countries and terms by debt series.

Country	D1	D2	D3
Australia	1991–2019 (10)	1991–2019 (10)	1970–2019 (17)
Belgium	1983–2019 (10)	1973–2019 (13)	1969–2019 (14)
Canada	1981–2019 (11)	1975–2019 (12)	1975–2019 (12)
Costa Rica	n/a	n/a	1975–2018 (11)
Czech Republic	1997–2018 (6)	1997–2018 (6)	1997–2018 (6)
Denmark	1995–2019 (7)	1972–2019 (16)	1969–2019 (17)
Estonia	1996–2019 (6)	1996–2019 (6)	1996–2019 (6)
Finland	1984–2019 (9)	1967–2019 (14)	1967–2019 (14)
Germany	1992–2017 (7)	1978–2017 (11)	1978–2017 (11)
Greece	1983–2019 (11)	1979–2019 (12)	1979–2019 (12)
Hungary	n/a	1995–2018 (6)	1995–2018 (6)
Ireland	n/a	1982–2019 (10)	1974–2019 (12)
Israel	2002–2019 (6)	1997–2019 (8)	1997–2019 (8)
Italy	1993–2018 (7)	1993–2018 (7)	1993–2018 (7)
Japan	1994–2017 (8)	1994–2017 (8)	1994–2017 (8)
Netherlands	1983–2019 (11)	1968–2019 (14)	1968–2019 (14)
New Zealand	1988–2019 (11)	n/a	1979–2019 (14)
Slovenia	1998–2018 (6)	1998–2018 (6)	1998–2018 (6)
Spain	1984–2016 (9)	1984–2016 (9)	1980–2016 (10)
Sweden	1995–2018 (6)	1971–2018 (14)	1969–2018 (15)
United Kingdom	1980–2019 (10)	1967–2019 (13)	1967–2019 (13)
Uruguay	n/a	n/a	1991–2019 (6)

Note: The table lists the period of coverage by dependent variable. In parentheses is the number of terms.

As noted in Table 1, I construct each debt variable as the annualized change in debt-to-GDP over the term.[15] The regressions fit those variables

[15]The annualization obviates the need to control for the number of years in each term; but to ensure that the results are not an artifact of term duration, early elections, or strategic electoral timing, Online Appendix F provides additional analysis. The results are consistent with what is reported here, and they point to some interesting and complementary patterns. They indicate: (1) terms that are shortened by early elections are associated with debt growth (or, conversely, that full-term government is associated with debt control), (2) early elections are associated with non-majority governments, and (3) among majority governments, early elections are least strongly associated with last-term government. The results suggest that governments that pursue austerity are averse to early elections, and that majority governments that anticipate electoral defeat are similarly disposed. The results are also consistent with the idea that non-majority governments have difficulty with both the introduction of fiscal consolidations and with the completion of their mandates. In short, the analysis suggests that a government's choice and ability to complete a full term is related to this paper's turnover-and-veto-players model of fiscal trajectories. And therefore, as a helpful reviewer emphasized, future research might explore a multifaceted model, in which choices about fiscal policies and early elections are interconnected.

to five economic and structural controls, three of which are average annual values over the term. Those are the average GDP-per-capita growth rate, the average unemployment rate, and the average inflation rate. The fourth variable is the estimated growth in debt-to-GDP that accompanied a banking crisis. The variable equals zero when there is no crisis; otherwise, for twenty-two observations, it ranges from 1.1, for the Czech term ending in 2002, to 76.5, for the Irish term ending in 2011.[16] The final variable, EURO, is meant to account for fiscal restraint in the period surrounding the introduction of the euro currency.

I mark first terms and last terms with two dummy variables, FT and LT. I reserve $\text{FT}_t = 1$ for cases in which the governing coalition in term t does not include the main governing party (i.e., of the prime minister) from term $t - 1$. So, an election that introduces a new coalition does not necessarily introduce FT = 1; if the primary governing party is unchanged, FT = 0, even if the new coalition has a different number of parties or ideological bent. (Other variables capture those shifts.) In all cases, $\text{LT}_t = 1$ if both $\text{FT}_{t+1} = 1$ and $\text{FT}_t = 0$; otherwise, $\text{LT}_t = 0$. This coding means that no term can be both LT = 1 and FT = 1.

My veto players variable is a simple dummy, MAJ, that differentiates between majority and non-majority governments (i.e., coalitions or minority governments) or, in the case of the two presidential systems in D3 sample, between unified and divided governments.[17] I also use NMAJ = 1 − MAJ, so that larger values indicate more partisan veto players. And, to account for differences in party-system fragmentation among non-majority governments, I use GFRAG, which is equal to zero for single-party governments, including both majority and minority governments, and each unit above zero corresponds to a one-standard deviation increase in the source variable, which measures party-system fragmentation in governing coalitions.

Two additional political variables are LEFT, a dummy for left-of-center governments, and NFRULES, which is the number of fiscal rules in place at the end of the term, including both international (European) rules and domestic rules. The addition of a new rule is expected to correlate with debt control, but it may not be causal, because a rule could be added by a government that is already intent on fiscal restraint.

[16]When a crisis straddles terms, according to the crisis years in the source dataset (Laeven and Valencia, 2020), I divide the measure across adjacent terms in proportion to the number of crisis years in each term.

[17]More precisely, MAJ = 1 when the president's party controls a majority of seats in the legislature. Throughout, I refer to MAJ = 1 as majority government, and do not emphasize that it means unified government in the case of the two presidential systems. This is to be concise, which is warranted because there are so few observations from presidential systems, and because this paper focuses on temporalities, not differences across systems.

3 Data Analysis

My analysis begins with Table 3 and Figure 1. The former provides OLS results of baseline economic models. The regressions show the same result: Debt growth is strongly countercyclical and well-explained by the economic

Table 3: Economic determinants of debt growth.

	(3.1) ΔD1	(3.2) ΔD2	(3.3) ΔD3
ΔGROWTH	−0.82∗	−0.82∗	−0.62∗
	(0.20)	(0.16)	(0.16)
ΔUNEMPLOYMENT	0.16	0.24*	0.41*
	(0.15)	(0.11)	(0.10)
ΔINFLATION	−0.16	−0.03	−0.06
	(0.19)	(0.10)	(0.09)
ΔBANK CRISIS	0.13*	0.16*	0.15*
	(0.04)	(0.04)	(0.03)
ΔEURO	−1.21∗	−1.04∗	−1.03∗
	(0.48)	(0.48)	(0.48)
L.D†	−0.70∗	−0.68∗	−0.69∗
	(0.10)	(0.08)	(0.07)
L.GROWTH	−0.50∗	−0.56∗	−0.44∗
	(0.23)	(0.19)	(0.17)
L.UNEMPLOYMENT	−0.01	0.03	0.04
	(0.06)	(0.05)	(0.05)
L.INFLATION	0.10	0.13*	0.03
	(0.09)	(0.06)	(0.05)
L.BANK CRISIS	0.17*	0.11*	0.08
	(0.06)	(0.05)	(0.05)
L.EURO	−0.25	−0.24	−0.19
	(0.34)	(0.31)	(0.30)
Constant	1.21	1.13	1.16
	(0.93)	(0.83)	(0.75)
N	133	176	217
R^2	0.69	0.75	0.72

Note: Each column shows OLS estimates of the EC model. Δ signifies first difference; L signifies lag. † D1, D2, or D3. Robust standard errors in parentheses. $*p < 0.05$ (two-tailed).

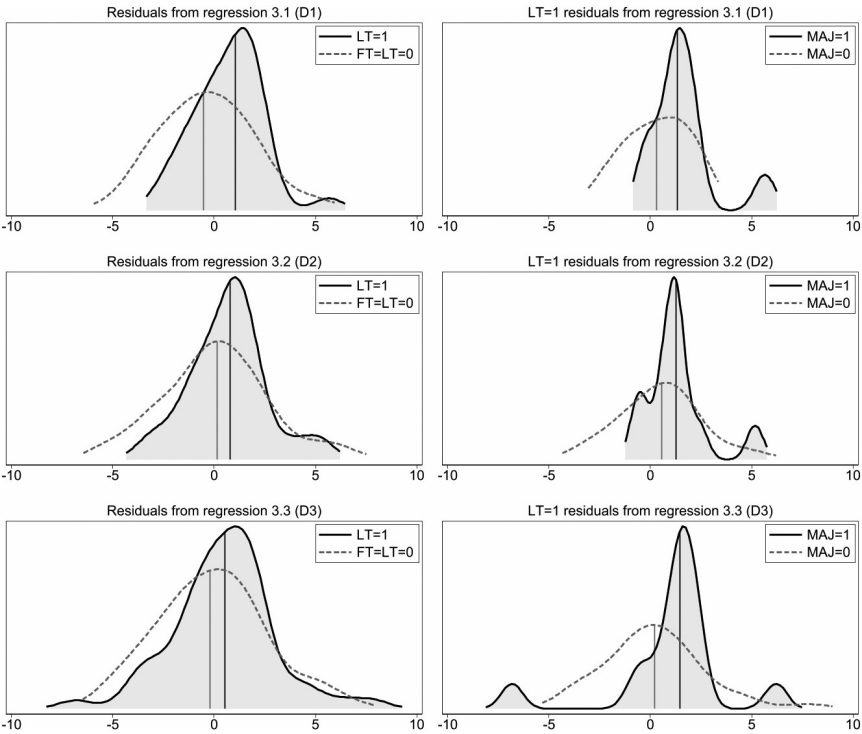

Figure 1: Last-term effect and majority government.

Note: Each panel shows kernel distributions of groups of residuals from regressions 3.1–3.3 in Table 3. The vertical lines indicate group medians. The panels on the left compare last-term governments with other non-first-term governments. The panels on the right compare the last-term trajectories of majority and non-majority governments.

variables.[18] Figure 1 plots the residuals from the three regressions to provide initial tests of (a) the last-term effect (H-LT), which anticipates greater debt growth in last terms than in other non-first terms, and (b) the moderation of last-term expansionism by veto players (H-LT-VP). The first hypothesis is tested by the left column, which shows kernel densities of the two groups of residuals (LT = 1 versus FT = LT = 0); and the three panels, one for each regression, show that, as expected, debt growth is more pronounced in last terms. The right column of Figure 1 tests the moderation hypothesis by subdividing the last-term residuals by the majority government dummy (MAJ), and it illustrates that majority governments indeed exhibit greater

[18]Notice that the estimate on the growth variable differs in regression 3.3. The discrepancy has less to do with the measure, D3, than with the more heterogeneous sample of countries. Indeed, if the two Latin American countries (and the only countries with presidential systems) are excluded, the coefficient on the growth variable is .79.

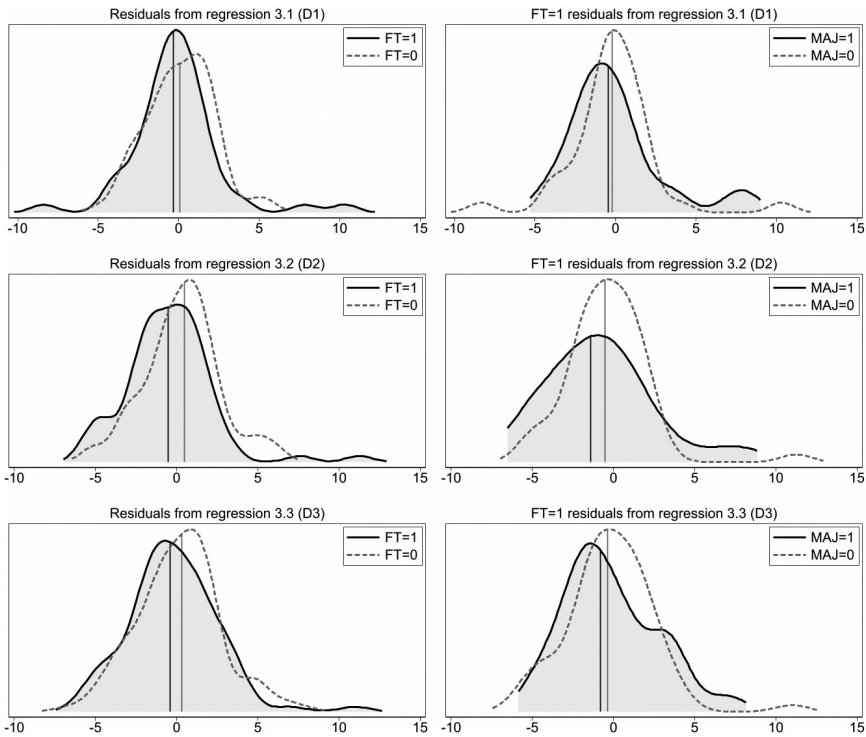

Figure 2: First-term volatility and majority government.

Note: Each panel shows kernel distributions of groups of residuals from regressions 3.1–3.3 in Table 3. The vertical lines indicate group medians. The panels on the left compare first-term governments and non-first-term governments. The panels on the right compare the first-term trajectories of majority and non-majority governments.

last-term debt growth than non-majority governments. That result is not just substantively important; it also undermines a plausible alternative explanation for the differences in column one—i.e., that the regressions underestimate the effects of economic troubles on debt growth. That could manufacture a last-term effect, but it would not explain the differences in column two. And notice one more thing about Figure 1: In the bottom-right panel there is a large negative outlier in the MAJ = 1 group. That it is a majority government fits comfortably in the veto players framework, but its direction is at odds with the last-term hypothesis. The observation is noteworthy because it proves very influential in the D3 regressions below.[19]

Figure 2 provides similar tests of the first-term hypotheses. The first column compares FT = 1 and FT = 0 residuals from the economic regressions,

[19]The term is New Zealand 1988–1990, during which the Labour government advanced an austerity program.

and it shows that the first-term distributions are wider and less positively skewed, as H-FTV anticipates. However, the differences are small and are at least partially attributable to outliers in the FT = 1 distributions. Of course, H-FTV expects outliers to be first-term governments, but those observations do not make a trend, so it is appropriate to emphasize the parity between the two types of governments. Yet, even that is meaningful for the hypothesis, given what is shown in Figure 1—i.e., marked heterogeneity within the FT = 0 group, between the LT = 1 and FT = LT = 0 subtypes. In other words, it is noteworthy that the first-term distributions are not thinner than the non-first-term distributions; and it is apparent that first-term trajectories are considerably more varied than those of either of the two FT = 0 sub-groups.

The second column of Figure 2 shows only the first-term residuals, and it subdivides them by MAJ. It reveals two important differences between majority governments and non-majority governments: The former exhibit a wider range of first-term trajectories, and the difference is more pronounced in the bottom tails of the distributions. The second discrepancy is consistent with the idea that non-majority government is a greater hindrance to consolidations than to expansions, and both patterns are anticipated by the moderation hypothesis (H-FTV-VP).[20]

3.1 Full Regression Model

Table 4 provides statistical tests of the last-term hypotheses using the dynamic regression model. The table shows partial results of nine regressions, corresponding to three sets of less-restricted versions of regressions 3.1–3.3. (Online Appendix D provides the full results, including the estimates on the economic variables.) The first trio of regressions do not control for differences between majority and non-majority governments; instead, they simply estimate how the average shift to first-term government ($\Delta FT = 1$) or last-term government ($\Delta LT = 1$) compares to the excluded category of terms ($\Delta FT = \Delta LT = 0$). The regressions show large standard errors on the ΔFT coefficient, as the first-term hypothesis anticipates. By contrast, the coefficients on ΔLT are

[20]See also Table E1 and Figure E1 in the Online Appendix, part E. The former provides statistics to accompany Figures 1 and 2, and it reinforces the evidence for H-FTV, H-LT, and the two moderation hypotheses. Figure E1 illustrates that the first-term difference by majority government is even larger when the analysis is limited to panels that have at least one term with majority government.

markedly positive, per the last-term hypothesis, although only one of them reaches $p < 0.05$ in a one-tailed test.[21]

The second trio of regressions introduces the dummy for non-majority government, and it interacts that variable with both term variables. The dual interaction is necessary to capture the differential effects of non-majority government on first-term and last-term trajectories, and the estimates illustrate that divergence. But first, ignore the interaction terms, focus on majority governments (NMAJ = 0), and notice that estimates on ΔFT and ΔLT remain consistent with the two main hypotheses: The coefficients on the first-term dummy ($\beta_{\Delta FT}$) continue to have large standard errors, and the coefficients on the last-term dummy ($\beta_{\Delta LT}$) are large and positive. The latter are more than twice the size of the coefficients in regressions 4.1–4.3, and in regressions 4.4 and 4.5 they are statistically significant ($p < 0.05$, one-tailed).

Next, consider the estimates for non-majority governments. When NMAJ = 1, the estimated differences between last terms and the baseline category (ΔFT = ΔLT = 0) remain positive, but they are much smaller. The negative coefficients on the interactions ($\beta_{\Delta(LT*NMAJ)}$) offset about eighty percent of the main effect ($\beta_{\Delta LT}$). Note also that less debt growth during last terms implies less debt control during first terms, because most observations that are ΔFT = 1 are also ΔLT = −1.[22] In other words, the last-term differential between majority and non-majority governments implies an inverse first-term differential, with greater debt control by majority governments, though the latter difference is also a function of the coefficient on the first-term interaction ($\beta_{\Delta(FT*NMAJ)}$), and in regressions 4.5 and 4.6 that coefficient receives a positive estimate, which widens the gap between the two types of first-term governments. Yet, there is still uncertainty about the trajectories of first-term, non-majority governments—the point estimates are close to zero, and the standard errors are large. In all, therefore, regressions 4.4–4.6 show: (i) a large last-term effect among majority governments, (ii) a small and insignificant last-term effect among non-majority governments, (iii) widely varying first-term

[21]There are co-dependencies between ΔFT and ΔLT that are important to recognize. Recall that some observations that are ΔLT = 1 are ΔFT = −1 (i.e., the last terms of two-term governments), whereas other ΔLT = 1 terms are ΔFT = 0. The estimated debt growth of the latter is captured by $\beta_{\Delta LT}$, but for last terms that are ΔFT = −1, the point estimate is given by $-\beta_{\Delta FT} + \beta_{\Delta LT}$. Thus, even though $\beta_{\Delta FT}$ should be poorly identified, its value matters for $\beta_{\Delta LT}$. If the model returns $\hat{\beta}_{\Delta FT} < 0$, then $\hat{\beta}_{\Delta LT}$ will be smaller and, in all likelihood, less statistically significant. More generally, the identification of $\beta_{\Delta LT}$ in the full regression model is challenged by two issues: the covariance between the last-term and first-term dummies (i.e., many observations that are ΔLT = 1 follow first terms, and most observations that are ΔFT = 1 are also ΔLT = −1), and the wide variance in first-term trajectories.

[22]This is by construction. The only ΔFT = 1 observations that are not also ΔLT = −1 (and vice versa) are first-term governments at the beginning of a panel or last-term governments at the end of a panel.

Table 4: Debt growth by term and majority government.

	(4.1) ΔD1	(4.2) ΔD2	(4.3) ΔD3	(4.4) ΔD1	(4.5) ΔD2	(4.6) ΔD3	(4.7) ΔD1	(4.8) ΔD2	(4.9) ΔD3
ΔLT	1.24*	0.75	0.61	2.84*	2.01*	1.70	2.73*	1.86*	1.57
	(0.52)	(0.50)	(0.49)	(0.96)	(0.89)	(1.15)	(1.07)	(0.98)	(1.21)
Δ(LT*NMAJ)				−2.19∗	−1.54	−1.41	−3.30∗	−2.86∗	−3.10∗
				(1.09)	(1.08)	(1.32)	(1.42)	(1.63)	(1.61)
Δ(LT*GFRAG)							0.89	0.79	1.13*
							(0.71)	(0.72)	(0.66)
ΔFT	0.10	−0.46	0.19	1.50	−0.06	0.80	1.42	−0.53	0.77
	(0.96)	(0.75)	(0.70)	(1.35)	(1.33)	(1.03)	(1.51)	(1.53)	(1.24)
Δ(FT*NMAJ)				−1.64	−0.49	−0.78	−2.05	−2.19	−2.15
				(1.47)	(1.39)	(1.12)	(1.65)	(1.62)	(1.42)
Δ(FT*GFRAG)							1.04	1.83*	1.57*
							(0.76)	(0.78)	(0.73)
ΔNMAJ				1.52	1.28	0.88	2.50*	2.91*	2.28*
				(0.93)	(0.86)	(0.84)	(1.15)	(1.18)	(1.08)
ΔGFRAG							−1.15∗	−1.60∗	−1.30∗
							(0.63)	(0.70)	(0.63)
ΔLEFT	−0.26	−0.31	−0.05	−0.15	−0.28	−0.02	−0.40	−0.51	−0.21
	(0.51)	(0.46)	(0.40)	(0.51)	(0.48)	(0.42)	(0.52)	(0.57)	(0.46)
ΔNFRULES	−0.44∗	−0.21	−0.15	−0.35	−0.20	−0.14	−0.38	−0.30	−0.24
	(0.22)	(0.20)	(0.21)	(0.24)	(0.21)	(0.21)	(0.24)	(0.22)	(0.22)
N	133	176	217	133	176	217	128	164	200
R^2	0.72	0.77	0.73	0.73	0.77	0.73	0.74	0.79	0.75

Note: Each column shows partial OLS estimates of the EC model, and each regression includes the economic variables in Table 3. Full results are in Online Appendix D. Robust standard errors in parentheses. * $p < 0.05$ (one-tailed).

trajectories for both types of governments, and (iv) greater debt control by first-term majority governments than by first-term non-majority governments.[23]

The third trio of regressions (4.7–4.9) add the government fragmentation variable (GFRAG) and its interaction with the two term variables. Recall that majority governments are coded GFRAG = 0 and NMAJ = 0, so the main term coefficients ($\beta_{\Delta LT}$ and $\beta_{\Delta FT}$) still correspond to majority governments. Unsurprisingly, those coefficients receive estimates that are very similar to the estimates in the previous trio of regressions. The addition of GFRAG serves to differentiate among coalitions (NMAJ = 1, GFRAG > 0) and between coalitions and single-party minority governments (NMAJ = 1, GFRAG = 0). Regarding those differences, there are four results to note. First, the regressions show $\beta_{\Delta GFRAG} < 0$, so an increase in fragmentation in associated with debt control, not debt growth as the common-pool theories anticipate. Second, the large, negative coefficients on Δ(LT*NMAJ) indicate a large difference in last-term debt growth between majority governments and non-majority governments with low levels of fragmentation (like single-party minority governments). Third, that gap reduces with fragmentation. For example, if GFRAG = 1, which corresponds to party-system fragmentation that is one standard deviation above single-party government, the gap in last-term debt growth is reduced by one-quarter to one-third. At increasing levels of fragmentation, the last-term effect for coalition governments approaches that of majority governments, but it only does so when fragmentation is three or four standard deviations above zero (and when $\Delta\text{GFRAG} \not> 0$, which is unlikely at very high levels of GFRAG).

The fourth result is that first-term trajectories continue to be poorly identified, and for all types of non-majority governments. The easiest way to see that is to hold NMAJ and GFRAG constant at specific values and sum the various first-term and last-term coefficients. For single-party minority governments, the first-term estimate is given by $\beta_{\Delta FT} + \beta_{\Delta(FT*NMAJ)} - \beta_{\Delta LT} - \beta_{\Delta(LT*NMAJ)}$, the sum of which is close to zero. For a coalition with GFRAG = 1, one computes the same sum with two additions, $\beta_{\Delta(FT*GFRAG)} - \beta_{\Delta(LT*GFRAG)}$. Again, the sum is close to zero.

Overall, the regressions in Table 4 are consistent with first-term volatility and the idea that veto players block or moderate first-term consolidations. The regressions have also provided evidence for the last-term effect and its attenuation by non-majority government. However, the last-term effect among majority governments has only been significant with the D1 and D2 series, and not also with D3. Of course, non-significant estimates indicate that there is sufficient variation in either last-term trajectories or non-last-term trajectories (or both) to make the difference between the two difficult to discern, and

[23]The last difference is also implicated by the estimate on ΔNMAJ, because $\Delta\text{NMAJ} \neq 0$ correlates with ΔFT = 1. In the D3 series, 45% of observations that are $\Delta\text{NMAJ} \neq 0$ are ΔFT = 1.

that is not surprising on a theoretical level given that last-term governments are not the only ones to pursue fiscal expansionism. There are also two empirical challenges. One is that first-term trajectories are highly variable, which complicates estimation of adjacent last terms. The other is specific to the D3 series and was noted above: There is an outlier that reduces the estimate on $\beta_{\Delta LT}$. Indeed, if New Zealand (the panel with the outlier) is removed from the D3 regressions, then the null hypothesis $\beta_{\Delta LT} \leq 0$ can be rejected at the $p < 0.05$ level in both regressions 4.6 and 4.9.[24]

3.2 Multi-term Governments

In this section, I focus on multi-term governments and examine how many follow the IBC pattern, with the least debt growth occurring in the first term. As before, I study debt growth that is unexplained by the economic models in Table 3. Thus, I identify a multi-term run as an IBC if its smallest residual occurs in its first term. The first question to answer is whether IBCs are common—or, as H-IBC puts it, whether the number of observed IBCs exceeds that which would obtain if debt-growth residuals were randomly distributed across terms in each run. This counterfactual is simple to calculate. Under a random distribution, the expected proportion of runs with the smallest residual in the first term (as opposed to a later term) would be one-half of two-term governments, one-third of three-term governments, and so on. So, if we take a set of runs consisting of ten two-term governments, six three-term governments, and two four-term governments, the counterfactual is: $10/2 + 6/3 + 2/4 = 7.5$.

Table 5 lists for each regression sample the observed and counterfactual number of IBCs for three groups of runs—those with no majority government, those with at least one term of majority government, and those with only majority government. Note that in the first group (no majority government) the observed number of IBCs exceeds the counterfactual. In the D3 sample, for example, 52% of runs are IBCs, whereas the counterfactual percentage is 41%. In the D1 and D2 samples, the disparity is greater. In the second group of runs (at least one term of majority government), IBCs are more frequent, and the contrast with the counterfactual is greater. And the trend continues when we move to the third group (runs with only majority government).[25] Among those runs, the percent of IBCs is very high—between 67% and 100%. On a cautionary note, those groups do have a very small number of observations; and, for that matter, the small-N caveat applies to all the statistics in Table 5.

[24]With that change, the estimate on ΔLT receives p = .002 (two-tailed) in regression 5.6 and p = .020 in regression 5.9. See also Online Appendix E, which provides robustness checks with alternative statistical models.

[25]The differences between the "1+ terms" rows and the "every term" rows indicates that mixed runs—i.e., with shifts to or from majority government—have a low percentage of IBCs. However, the percentages remain above the counterfactual. In the D3 series, for example, the observed percent of IBCs is 44%; the counterfactual is 33%.

Nevertheless, the data are consistent with the hypothesis that interterm cycles are disproportionally common, and they are consistent with the corollary that IBCs are more common among majority governments than non-majority governments.

Another look at the runs data from regression 3.3—i.e., the regression with the most observations—is given by Figure 3. It displays runs by their first-term and last-term residuals. Runs that have at least one instance of majority government are in bold, and the marker indicates the number of terms in the run. Notice that the runs are not randomly distributed. Instead, there is a disproportionate number of runs in the upper half of the graph, in which last-term debt growth exceeds expectations. Overall, 60% of runs have $y > 0$, including 72% of runs with majority government and 56% of runs without majority government. Similarly, there are disproportionately many runs in quadrant II, with unexpectedly low debt growth in the first term and unexpectedly high debt growth in the last term. In that quadrant are 29% of runs, instead of the 25% that a random distribution would produce. Note also that some of the runs in quadrant II have large negative first-term residuals, which suggests a marked fiscal consolidation; however, most are characterized by modest departures from the economic model's predictions, which implies that first-term restraint is a general trend. Another feature of Figure 3 is that quadrant IV ($x > 0$, $y < 0$) is relatively empty, with only 13% of the runs, and only 6% of the runs with majority government. The emptiness is expected, because last terms are hypothesized to be expansionary.[26] There are, by contrast, several runs in quadrant III ($x < 0$, $y < 0$), although many of them are near the $y = x$ line. That placement indicates that debt growth was consistently (for each term) overestimated by the economic model, which suggests that the government maintained a restrained fiscal policy throughout its time in office.

One of the questions posed in this paper's introduction was whether the IBC pattern is common in countries other than the United Kingdom. Unfortunately, the data do not allow an answer. There are simply too few relevant observations—i.e., complete, multi-term, majority-government runs—to say

[26]The majority government was the Australian Labor run of 1983–1996, which in the 1990s turned toward fiscal retrenchment and pro-market reforms. Four of the five other runs in quadrant IV straddled an economic crisis. Three were governments that came to power near the onset of the global financial crisis, including the three-term National Party minority government in New Zealand (2009–2017), the three-term, 2005-to-2019 run of Uruguay's Broad Front, and the two-term, Civic Democratic Party-led coalitions in the Czech Republic (2007–2014). In each case, the first-term exhibited high debt growth, and the last term exhibited better-than-expected debt control. The fourth, crisis-stricken run was the series of Swedish Social Democratic-coalitions in 1983–1991, which came to an end after the banking crisis and recession of 1990. It is noteworthy that there are crisis-stricken runs in quadrant IV, for it might be even more sparsely populated if not for unusual circumstances. Also, those observations illustrate that economic crises do not necessarily put multi-term governments in the top half of the chart.

Table 5: Interterm cycles and majority government.

Regression sample	Majority government in run?	Number of complete multi-term runs	Frequency of interterm budget cycles compared to counterfactual frequency in which runs are randomly distributed within terms (H-IBC)		Are the amplitudes of interterm budget cycles more pronounced when there is majority government? (H-IBC-VP)
			Counterfactual IBC frequency	Observed runs with IBC pattern	Mean difference in residuals (LT residual – FT residual) in IBC sub-group
3.1 D1	None	12	5.3 (44%)	7 (58%)	1.74
	1+ terms	11	4.1 (37%)	8 (72%)	2.19
	Every term	4	1.7 (42%)	4 (100%)	3.36
3.2 D2	None	21	8.7 (41%)	13 (62%)	2.47
	1+ terms	12	4.5 (38%)	8 (67%)	2.96
	Every term	6	2.4 (40%)	5 (83%)	3.85
3.3 D3	None	27	11.2 (41%)	14 (52%)	2.60
	1+ terms	18	6.7 (37%)	10 (56%)	2.71
	Every term	9	3.7 (41%)	6 (67%)	3.61

Note: The table reports summary statistics for groups of residuals from the regressions in Table 3. The data are limited to terms in complete, multi-term runs. The final column lists the median difference in residuals (last-term residual minus first-term residual) in the IBC sub-group.

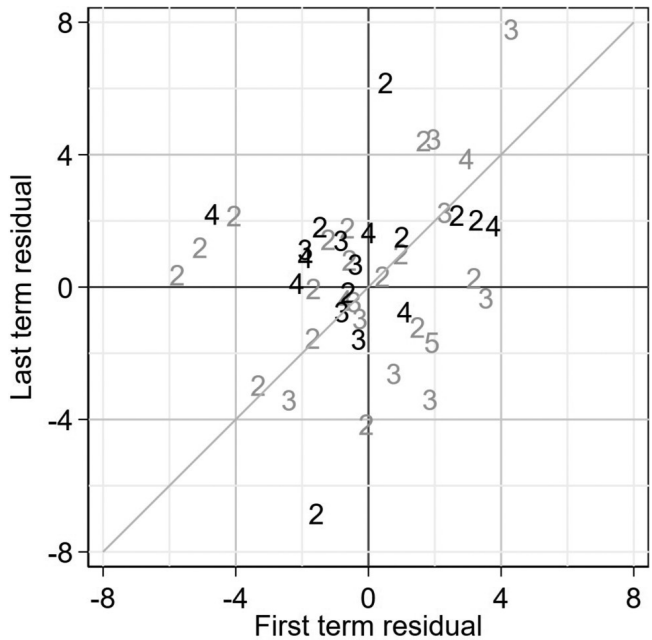

Figure 3: Multi-term governments by first-term and last-term residuals.

Note: The graph shows the first-term and last-term residuals for complete, multi-term runs in regression 3.3. Marker numbers indicate the number of terms in the run. Boldface indicates at least one term of majority government in the run. The diagonal line is $y = x$.

anything about within-country trends. However, different groups of IBCs can be compared to assess H-IBC-VP, which stipulates that the first-term decelerations and/or later-term accelerations in IBCs will be larger when the number of veto players is fewer. The last column in Table 5 provides statistics to test that hypothesis. For each IBC, I subtract the first-term residual from the last-term residual, thus calculating the difference in unexplained debt growth between those two terms. The mean in each group is reported. Reading vertically across the rows for each regression, notice that the mean difference is always smaller for non-majority governments than for majority governments. In the residuals from regression 3.2, for example, the mean difference in the non-majority-government cycles is 2.47, whereas it is 3.85 in the majority-government cycles. Thus, the data are consistent with the idea that interterm cycles are more pronounced when there is majority government. It is important that cycle amplitudes and frequencies exhibit the same associations with government types. Both are small-sample results, but each helps to reinforce the other, and together they help to support the broader, central ideas of this study: That governments, especially if unencumbered, pursue different fiscal policies

in different types of terms, and that governmental turnover and tenure affect national fiscal dynamics.

4 Conclusion

In this article, I examined fiscal politics from an interterm perspective, and I argued that first-term governments are more inclined toward adjustments that modify the status-quo fiscal balance, whereas later-term governments hew more closely to a bloated version of the status quo, with deficit growth that relates to the government's electoral vulnerability. I found considerable support for the theory in an examination of debt trajectories in twenty-two democracies, all with concurrent legislative and executive terms. The data show that first terms are more varied than other types of terms, that last terms exhibit more debt growth than other non-first terms, and that interterm cycles are common among multi-term governments. Also, all three trends are more pronounced among majority governments than non-majority governments.

The results are consistent with prior research on veto players and fiscal adjustments (Roubini and Sachs, 1989; Cox and McCubbins, 2001; Franzese Jr., 2002; Alesina *et al.*, 2006), and the last-term effect complements research that links fiscal expansionism to electoral vulnerability (Schultz, 1995; Aidt *et al.*, 2011; Veiga *et al.*, 2019). My analysis also highlighted two issues that may obfuscate the vulnerability–expansionism relationship: veto players and first terms. The latter warrants attention in future research. Previous studies of within-term budget cycles have considered many types of moderating variables, including veto players, fiscal rules, and term limits (see Veiga *et al.*, 2019). But they have not considered how first-term governments often eschew expansionism and, more generally, how fiscal trajectories differ across terms with governmental turnover and tenure.

A similar point can be made regarding the research that links electoral reconfigurations of power to deficits and debt. Most of that work does not consider the fiscal shifts that can occur when the key government attributes (e.g., ideology, fragmentation) are unchanged from one term to the next. There is, however, some research on fiscal-policy interactions between party ideology and the state of the economy (Carlsen, 1997; Cusack, 1999; Franzese Jr., 2002; Müller *et al.*, 2016). This study did not directly engage with that work, and it did not examine interactions between term type and governing party ideology, but both are worthwhile avenues for future research. One open question is whether ideology–economy interactions strengthen or weaken when first-term restraint and later-term expansionism are taken into account. The question's relevance is highlighted by the fact that economic stagnation causes both electoral vulnerability and political turnover, and its investigation would

bring office-seeking opportunism into models that assume only policy-seeking behavior.

An interterm budget cycle occurs when growth in the cyclically adjusted deficit increases after a government's first term. The cycle need not begin with fiscal restraint or retrenchment, but both appear common. And the cycles are themselves common. More than fifty percent of non-majority governments—and a larger percentage of majority governments—accelerated debt growth after their first terms. The number of observations in that analysis was small, because even a fifty-year span contains only a few complete, multi-term runs per country. So, the data could not speak to within-country tendencies, but they did illustrate the conditions for future interterm cycles, and they underscored the value of additional research, and of two steps in particular. One is to expand the amount of data available for analysis, especially by widening the cross-section to include more countries. The other, related task is the theoretical work that must accompany an expansion to countries with non-concurrent executive and legislative terms. In those settings, fiscal dynamics may be more complex because policymakers' preferences in any period may depend upon which type of election is next to occur. A related issue is that a wider cross-section would introduce greater heterogeneity in the fiscal response to macroeconomic conditions by incorporating more developing economies, in which fiscal policy is often procyclical rather than countercyclical. That could translate into greater error in attempts to control for automatic stabilizers, which in turn could frustrate the identification of politics-driven dynamics.

There are several other worthwhile next steps. One is to use polling data to study office-seeking expansionism more directly. And another is to model first-term adjustments as a function of antecedent economic and fiscal trajectories. Although that approach is limited in what it could achieve, because there is always a degree of contingency in fiscal policy, it could allow a finer differentiation between the economic and political causes of national fiscal dynamics.

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