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# Electoral Cycles, Investment, and Institutional Constraints in Developing Democracies<sup>^</sup>

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## Abstract

A longstanding question is whether policy uncertainty reduces private fixed investment in developing democracies. Yet studying the question empirically has proven challenging given that economic activity can cause as well as result from policy uncertainty. We investigate this issue within the context of electoral business cycles, building on research that suggests elections provide an exogenous source of policy uncertainty. As a central part of this analysis, which involves four decades of data from 57 developing democracies, we examine how institutional constraints moderate the relationship. Three main findings emerge. First, on average, elections are associated with a decline in private fixed investment. Second, however, this effect varies according to the level of institutional constraints; as they increase, the electoral cycle becomes less pronounced, including in specifications that account for the potential endogeneity of the institutions. Third, the effects are larger and more robust in systems with fixed elections.

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A longstanding question is whether and how policy uncertainty affects investment in developing democracies. Prior scholarship argues that the prospect of large shifts in policy, let alone the usurpation of private property, can make investors leery to sink large sums into a country (e.g., Rodrik 1991; Stasavage 2002; Kenyon and Naoi 2010). Due to the impact of private investment on long-term growth, this issue has important implications for development. Yet studying the question empirically is challenging. Because economic activity itself can alter political behavior, identifying independent sources of policy uncertainty is not straightforward.

This paper leverages elections and their associated policy uncertainty to offer new evidence on this question. As established in studies of OECD countries and panels dominated by them, elections provide a temporary spike in policy uncertainty given that they can produce large shifts in policy (e.g., Bernhard and Leblang 2006; Canes-Wrone and Park 2012; Julio and Yook 2012). This spike causes private investment to decline in the run-up to an election, thereby creating an electoral cycle in the opposite direction of the canonical opportunistic business cycle, in which the economy expands as an election nears due to incumbent policy manipulation (e.g., Franzese 2002; Golden and Min 2013). Additionally, we build on work that argues institutional constraints should reduce leaders' capacity to alter policies or expropriate property (e.g., Henisz 2000, 2002; Jensen 2008) to examine how the constraints moderate the association between elections and investment. Assuming the constraints indeed reduce the policy uncertainty associated with electoral turnover, they should diminish any observed electoral cycle in investment.

To investigate these relationships, we analyze private fixed investment in a panel of 57 developing democracies from 1975-2017. Private fixed investment, such as telecommunications and construction projects, is a critical component of economic development (e.g., Henisz 2002).

Moreover, the costliness of reversing these projects incentivizes withholding or delaying them when policy uncertainty is high (e.g., Montagnes and Wolton 2017). As part of examining the relationship among private fixed investment, elections, and institutional constraints, we consider multiple measures of institutional constraints, the potential endogeneity of political institutions, and whether effects vary between electoral systems with fixed versus discretionary timing.

Several findings emerge. First, as predicted, there is evidence that the election period is associated with a decline in private fixed investment. Second, however, institutional constraints have a large impact on this relationship. At lower levels of constraints, a strong cycle between elections and investment appears, and as the constraints increase, this cycle becomes substantially weaker, losing significance at the highest levels. Notably, the effect holds even in two-stage least squares analyses that allow for the endogeneity of the political institutions. Finally, the results are mostly driven by regimes with fixed elections as opposed to discretionary ones; in particular, the estimated effects are larger and more robust for the former than the latter. Because the theoretical motivation requires that investors can plan for the policy uncertainty associated with elections, this difference is consistent with the broader arguments. Together, these findings provide considerable evidence that policy uncertainty has an impact on investment in developing democracies.

## **Policy Uncertainty, Elections, and Institutional Constraints**

Within the literature, empirical analysis of the relationship between policy uncertainty and economic outcomes commonly focuses on institutional constraints, particularly with respect to the executive, under the reasoning the constraints will reduce leaders' ability to confiscate property or alter policy in unpredictable ways. For instance, Henisz (2000) develops a measure of institutional constraints and shows it is positively associated with cross-national variation in

economic growth and Henisz (2002) demonstrates a similar association with infrastructure investment. Stasavage (2002) also uncovers a positive relationship between private investment and this measure. Correspondingly, the Polity measure of executive constraints (Gurr 1997; Marshall, Gurr, and Jagers 2019) is associated with lower premiums for political risk insurance in Jensen (2008) and with economic growth in Acemoglu, Johnson, and Robinson (2002).<sup>1</sup>

Scholars have been cognizant of the difficulty in disentangling the causal impact of institutions versus economic outcomes, and various methods have been leveraged to gain traction. For example, Jensen (2008) supplements quantitative analysis with qualitative interviews, while Acemoglu, Johnson, and Robinson (2002) conduct statistical analysis in which the institutional constraints are assumed to be endogenous.<sup>2</sup> Despite these efforts, Abramson and Boix (2019) take issue with the underlying causal claim that institutional constraints affect growth, arguing that in the case of the historical development of Europe, early urbanization spurred both executive constraints and growth; notably, this argument suggests that policy uncertainty, at least as reflected by institutional constraints, may not directly influence economic outcomes. Closer to the approach of this paper, Cox and Weingast (2018) examine how legislative power over the executive mitigates economic downturns at times of turnover in autocracies and democracies. However, their analyses either group together autocracies and democracies or examine autocracies in isolation, and prior scholarship suggests turnover in

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<sup>1</sup> The literature on the relationship between institutional constraints and economic outcomes is vast. For a detailed review, see Arias (2015).

<sup>2</sup> In related work, Kenyon and Naoi (2010) examine firm-level survey data and find that firms' perceptions of policy uncertainty relate to the type of regime as measured by Polity scores. Kenyon and Naoi do not examine how the uncertainty relates to investment decisions or other economic outcomes, however.

autocracies is a function of weak economic performance (e.g., Boix and Svobik 2013).

Therefore, it is unclear whether the results extend to democracies. Moreover, even with respect to democracies, leadership replacement is more likely in economic downturns (e.g., Nadeau, Lewis-Beck, and Bélanger 2013), producing questions of whether it is the downturn leading to the replacement or vice-versa.

Over past decades a series of studies, primarily of OECD countries or panels with them, have examined elections as a source of policy uncertainty without considering the role of institutional constraints. These pieces suggest that the policy uncertainty associated with elections should induce a temporary decline in private investment with high fixed costs, otherwise known as costly-to-undo investment. Canes-Wrone and Park (2012) provide evidence of such a cycle in OECD countries, referring to it as a “reverse electoral business cycle” given that it is in the reverse direction of the classic opportunistic political business cycle. Likewise, Julio and Yook (2012) find corporate investment exhibits such a cycle in a panel dominated by OECD countries.<sup>3</sup> It is worth highlighting that these pieces do not argue that elections discourage investment across all periods; rather, that they produce a cycle in which investment declines in the period prior to the election and then increases when the electoral uncertainty subsides. In related work, Bernhard and Leblang (2006) show that election periods in the US and a set of European countries are associated with higher risk premia, and Bak (2016) provides evidence of electoral cycles in foreign direct investment in a set of countries spanning developed democracies, developing ones, and autocracies. Finally, in an independently developed paper,

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<sup>3</sup> In an alternative approach to examining one type of policy uncertainty, Baker, Bloom, and Davis (2016) develop a measure of economic policy uncertainty in 12 major economies based on newspaper coverage.

Kanyam (2020) shows that electoral cycles in private fixed investment extend to a set of African countries; Kanyam does not examine the role of institutional constraints, however.

The existence of an electoral cycle in private fixed investment should extend to developing democracies broadly. Indeed, one could argue that the cycle should be at least as pronounced as in OECD-type countries. As Lupu and Riedl (2013, 1344) observe, there are “*vastly* greater levels of uncertainty in developing democracies” because of factors including the relative weakness of formal institutional constraints. Following this line of reasoning, we expect the electoral investment cycle to vary with the level of institutional constraints. In particular, the cycle should be larger the lower the level of constraints on the head executive’s capacity to enact new policies unilaterally.

Consider a regime that faces few checks and balances. Newly elected leaders could quickly change regulations, taxes, government subsidies, and other policies that could dramatically alter the profitability of an investment. The policy uncertainty associated with the election should therefore be high. By contrast, when institutional constraints are such that a new executive cannot easily and quickly reverse existing policies, elections should induce lower policy uncertainty and by extension a lower electoral investment cycle. We therefore expect the size of the electoral investment cycle to be inversely associated with the level of institutional constraints; the higher the level of constraints, the smaller the decline in investment in the electoral period.

Furthermore, we anticipate these effects will vary according to the predictability of the election timing. In systems with fixed elections, investors can anticipate the election period with precision and adjust the timing of investment decisions accordingly. If an early election can be called, however, then the snap election may leave little time for investors to adjust their



decisions. Moreover, discretionary election timing opens up the possibility that incumbents schedule elections during times of favorable economic conditions (e.g., Kayser 2005), which may bias results away from finding a negative effect of electoral proximity on investment. Consequently, both the direct effect of the electoral cycle and the interaction of this cycle with the level of institutional constraints should be greater in systems with fixed elections than in ones with discretionary elections.<sup>4</sup>

In sum, there is debate about the extent to which institutional constraints influence private investment and limited evidence on how it relates to policy uncertainty in developing countries. In the following, we leverage elections as an exogenous source of policy uncertainty and argue that institutional constraints should minimize this uncertainty. Moreover, we present analyses that account for the potential endogeneity of these constraints, for confounding factors on investment including urbanization, and for potential differences between systems with fixed elections versus ones where leaders can control the electoral timing.

## **Empirical Strategy**

Our dataset encompasses 57 developing democracies across Africa, Asia, Eastern Europe, and Latin America for which there are data on private fixed investment from the World Bank World Development Indicators (WDI). Appendix Table A1 lists these countries. The WDI economic data are annual and encompass the years 1975 through 2017. Following prior research (e.g., Acemoglu et al. 2019), we focus on country-year observations with a positive Polity score in the Marshall, Gurr, and Jaggers (2019) database of countries' democratic characteristics and a

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<sup>4</sup> Bak (2016) establishes such a difference for a direct effect in foreign direct investment (FDI) but does not examine how institutional constraints relate to such an electoral cycle.

Freedom House (2020) score of “free” or “partially free” on the basis of political rights and civil liberties.<sup>5</sup> Furthermore, we require that the country held elections that determine (directly or indirectly) the chief executive and, due to the inclusion of country-level fixed effects, at least three years of data.

The dependent variable *Private Fixed Investment Growth* captures the real year-over-year change (from year  $t-1$  to year  $t$ ) in country  $i$ . Although the OECD collects data on quarterly private fixed investment, the available data for a sufficient sample of developing countries is only at the annual level. For this reason, prior scholarship that analyzes fixed investment in these countries also uses annual data (e.g., Stasavage 2002; Cox and Weingast 2018). Descriptive statistics on all variables are given in Appendix Table A2. For the dependent variable and other economic variables, there are extreme outliers as well as a number of negative values, and we therefore follow common practice by winsorizing (e.g., Watson and Arunachalam 2018; Dipoppa and Grossman 2020). Specifically, the economic data are winsorized at the 1% level (0.5% on each tail) in the main specifications. As discussed subsequently, the results are robust to the IHS log-transformation (e.g., Burbidge, Magee, and Robb 1988), which was created for log-transformations of data with many negative values, as well as to other types of log-transformations of the economic data.

The two key independent variables of interest capture proximity to elections and constraints on the ability of a regime to alter policy without checks or oversight. In line with existing scholarship, we rely on the key identifying assumption that policy uncertainty varies across the electoral cycle (e.g., Bernhard and Leblang 2006; Julio and Yook 2012), with

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<sup>5</sup> To avoid concerns that data availability may be skewed towards observations that have high levels of democratic development, we exclude countries that join the OECD during the time series.

uncertainty increasing as the election approaches. Also following prior work, we operationalize the election variable as an indicator that depends on when in the calendar year the election that determines the head executive occurred. Specifically, *Election* equals 1 for an observation of private fixed investment in year  $t$  if the election occurred in the second half of year  $t$  or the first half of year  $t+1$  (e.g., Alesina, Cohen, and Roubini 1993). This coding reduces post-electoral effects in comparison to an indicator of whether the election occurred in the same calendar year.<sup>6</sup> In the online supplemental materials (Table S1), we present robustness checks with alternative measures, including an indicator for the calendar year and two separate indicators that capture the first and second half of the calendar year (Brender and Drazen 2005). These results support those in the main text. Because the focus is on elections for the head executive, *Election* is based on parliamentary elections in parliamentary regimes, and presidential elections in presidential regimes. In semi-presidential regimes, the variable reflects the election of the dominant (or head) executive.<sup>7</sup>

To capture institutional constraints on the head executive's ability to alter policy unilaterally, the main analyses rely on the widely used measure PolConV from the Political Constraint Index (Henisz 2000, 2017), which is based on the number of independent branches of government that can veto policy change, including at the national and subnational levels, and the preference distribution of the actors leading these institutions. The resulting measure *Institutional*

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<sup>6</sup> We collected data on the year and month of relevant elections from the Database on Political Institutions (Cruz, Keefer, and Scartascini 2018) and cross-checked the information against additional sources such as the Inter-Parliamentary Union's archive of parliamentary election results (<http://archive.ipu.org/parline-e/RecentElections.asp>) and online news searches.

<sup>7</sup> Please see the online supplemental materials (Section B) on regime coding for further description of how we coded parliamentary, presidential, and semi-presidential regimes, as well as the coding decisions for determining the head executive in semi-presidential regimes.

*Constraints* ranges from 0 to 1, with higher values indicating greater constraint (see e.g., Gray and Kucik 2017; Jeong and Peksen 2019). Later in the paper, we also present results from other measures, including from the Polity (Marshall, Gurr, and Jagers 2019) and Varieties of Democracy (otherwise known as V-Dem, Coppedge et al. 2020) databases. For the main results, we focus on PolConV for several reasons. First, relative to the alternative measures, it is less dependent on ex-post evaluations of events (e.g., Gleditsch and Ward 1997; Glaeser et al. 2004, 272-3). Further, it incorporates a large number of potentially constraining institutions, including subnational ones.<sup>8</sup> Some earlier research on institutional constraints (e.g., Stasavage 2002) suggests that the effects may be nonlinear and examines specifications with a log transformation of this factor. We present findings with both the (non-transformed) linear measures and with log-transformed ones.<sup>9</sup>

The control variables fall into two broad categories: political and economic. Prior work suggests higher overall economic growth is associated with the ideology of the government (e.g., Bjørnskov 2005), and although we are examining private fixed investment rather than overall growth, we still control for government ideology. Specifically, we use a set of indicator variables based on the 2017 Database of Political Institutions (DPI) (Beck et al. 2001), a standard source that codes the head executive as left, right, center, or non-ideological (e.g., Leblang 2003; Bjørnskov 2005). In the regressions, the omitted category is the non-ideological governments.

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<sup>8</sup> In addition to blocking national-level policies in federal systems, actors in subnational institutions can also impose constraints on the national executive by carrying out policies in their territory that subvert or counteract the national policy (Mainwaring and Samuels 1999).

<sup>9</sup> For the log transformations of the Henisz (2017) and V-dem (Coppedge et al. 2020) measures  $x$ , each of which range from 0 to 1, we use the transformation  $\ln(1+x*100)$ . Because the Polity data range from 1 to 7 in our data, the transformation is simply the natural log of this factor.

Similarly, the rational partisan theory (e.g., Alesina, Roubini, and Cohen 1997) predicts a temporary increase (decrease) in total output and inflation following a shift from a right (left) to a left (right) government, and the analysis accounts for the possibility that this pattern extends to private fixed investment. *Rational Partisan Theory* equals -1 if the government shifts from left to right in the year after the election (as measured by the elections indicator), 1 for a shift from right to left, and 0 otherwise. It is worth reiterating that for the main election effect on investment, we predict the sign opposite to that predicted by the canonical business cycle for total output. Accordingly, it should not be surprising if the effects of these ideological controls carry the opposite signs from those one might theoretically expect if the dependent variable were total output.

The economic controls encompass potential macroeconomic influences including prior economic growth, inflation, and interest rates. All of these variables are available from the World Bank WDI. Following earlier scholarship, we measure economic growth with GDP per capita (e.g., Jensen 2008). Specifically, it equals lagged year-over-year change in real GDP per capita (in US\$). Inflation is measured as the year-over-year change in the consumer price index, and the interest rate variable equals the year-over-year change in the lending interest rate, adjusted for inflation. Because the interest rate data are available for only a subset of countries and years, and because the World Bank's methodological notes caution about the cross-country comparability of lending rates, we present results both with and without this control variable. As mentioned previously, in the main specifications the economic variables are winsorized at 1% and the results are robust to log transformations. Appendix Table A2, which reports the descriptive statistics, provides further details including the official WDI names.

Finally, and as specified further in the description of methods, we include in the main analyses a set of fixed effects for the country and year. The fixed effects capture private investment patterns that are specific to a country across time and to a year across countries.<sup>10</sup>

### **Estimation and Inference**

Equation (1) estimates for country  $i$  and year  $t$  the effect of election-induced uncertainty on private fixed investment and the extent to which it is moderated by institutional constraints:

$$(1) \text{ Private Fixed Investment Growth}_{it} = \alpha_{it} + \beta_1 \text{ Election}_{it} + \beta_2 \text{ Election}_{it} \times \text{ Institutional Constraints}_{it} + \beta_3 \text{ Institutional Constraints}_{it} + \gamma_i + \tau_t + \rho \mathbf{X}_{it} + \varepsilon_{it}$$

The key coefficients are  $\beta_1$  and  $\beta_2$ , the former capturing the effect of elections on private investment absent institutional constraints and the latter reflecting the additional impact of institutional constraints on this relationship. If our expectations are correct,  $\beta_1$  will be significantly negative and  $\beta_2$  significantly positive, given that the policy uncertainty associated with elections should decrease private investment and that institutional constraints should mitigate this effect.<sup>11</sup>

The main specification employs the commonly used approach recommended by Beck and Katz (1995) of ordinary least squares (OLS) with panel-corrected standard errors (PCSE) to address panel-level heteroskedasticity, and a first-order autoregressive process to address serial

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<sup>10</sup> Country-year effects cannot be included because the unit of analysis is the country-year. The results are robust to including a set of region-year effects, as shown in the online supplemental materials (Table S2).

<sup>11</sup> Following current best practices for analyses with interaction effects (Hainmueller, Mummolo, and Xu 2019), we use the *interflex* estimation procedure in STATA to assess the validity of the functional form and the extent to which there is common support of the moderator institutional constraints for election and non-election years. The results, presented in the online supplemental materials (Figure S1), provide support for the specification.

correlation (e.g., Karaman and Pamuk 2013). In further specifications in the text and supplemental materials, results are shown with pooled OLS without the fixed effects, a fixed effects OLS model with Driscoll-Kraay standard errors (Driscoll and Kraay 1998), and a fixed effects OLS model with multi-way clustered standard errors (Correia 2016). The last two approaches represent different ways to deal with heteroskedasticity and serial correlation, in addition to addressing contemporaneous correlation across the panels.<sup>12</sup> As subsequently shown, the results from these alternative methods are consistent with those from the main specification.

In the main analyses, we address concerns related to the endogeneity of executive-constraining institutions through a combination of two-stage least squares (2SLS) regressions and additional control variables. Specifically, we build on Acemoglu, Johnson, and Robinson (2001) and Acemoglu and Johnson (2005) by using log population density in the year 1500 and the country's legal origin (civil-law vs. common-law origin) to instrument for institutional constraints in the subset of countries that were colonized by European nations between 1500 and 1900.<sup>13</sup> Because the instruments do not vary within countries, we omit country fixed effects in the 2SLS analyses and replace them with region fixed effects, where the region is defined by the continent. The second-stage equation interacts institutional constraints with the elections

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<sup>12</sup> The PCSE specification does not correct for contemporaneous correlation of the errors, since this procedure requires at least one common time period across all panels, and as mentioned previously, we have an unbalanced panel. The Driscoll and Kraay (1998) estimator corrects for contemporaneous correlation, heteroskedasticity, and serial correlation by applying a correction in the style of Newey-West to the sequence of cross-sectional averages of moment conditions. The specific implementation by the xtsc program (Hoechle 2007) permits the analysis of unbalanced panels by allowing the number of observations to vary by time period when calculating the sum of the individual time  $t$  moment conditions. In our specifications, we keep the program's default for the maximum lag to be considered in the autocorrelation structure (Hoechle 2007, 286).

<sup>13</sup> Acemoglu and Johnson (2005) analyze executive-constraining institutions as well as contracting ones.

indicator, and we therefore include as additional instruments interactions between the elections indicator and each of the population density and legal origins variables. This strategy for instrumenting interactions involving endogenous variables is standard (e.g., Wooldridge 2002). The key assumptions for the 1<sup>st</sup>-stage equations are that the instruments are correlated with institutional constraints, but only affect investment growth through their effect on these institutions. As discussed subsequently, specification testing supports this assumption in that the testing fails to reject the null that the instruments are not directly associated with private fixed investment.

In recent research, Abramson and Boix (2019) advance the argument that in Europe from 1200 to 1900 both economic growth and the development of executive constraints resulted from urban agglomeration that fostered technical knowledge among urban-dwelling artisans. Although the analysis here focuses on a comparatively brief span of time of annual changes in investment, we nevertheless follow their approach and account for the possibility that urbanization is an omitted variable. Therefore, we include the percentage of the population living in urban areas, lagged by one year, as an additional control in the instrumental variables regression and, separately, as a control in an additional analysis of the main fixed effects specification.

Finally, we conduct analyses to address the potential for heterogeneous effects across systems with fixed election dates versus ones that allow for the calling of early elections. In the latter, elections are more difficult to anticipate and therefore in practice, the pre-electoral period during which investment decisions can be altered becomes shorter, possibly inducing lower levels of change in investment. Moreover, in systems with discretionary elections, politicians have incentives to call them during strong economies (e.g., Beckman and Schleiter 2020), which may include higher levels of investment. As highlighted earlier, these circumstances will make it

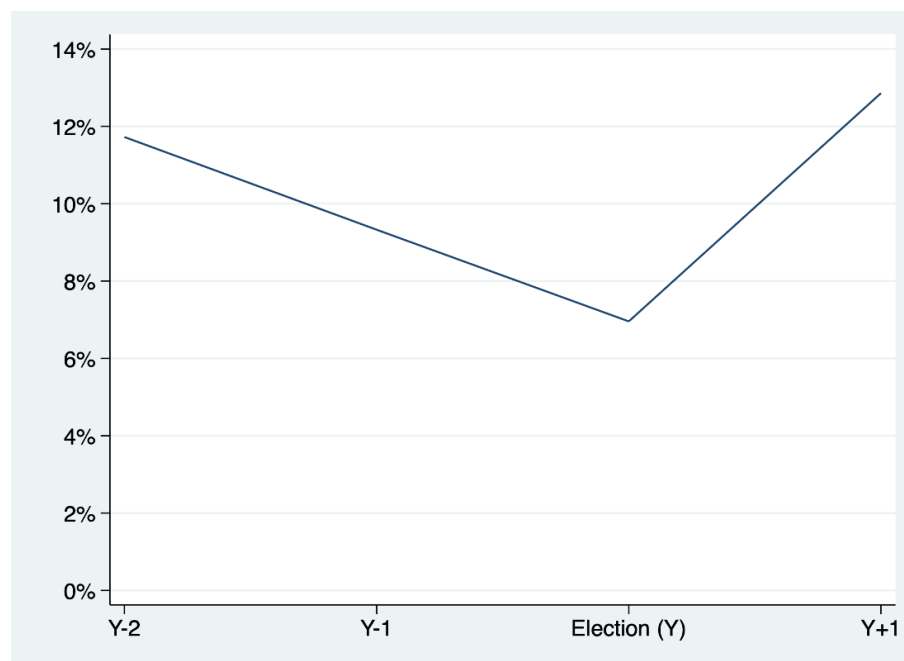


more difficult to detect a negative effect in countries with flexible election dates. We consequently conduct analyses that distinguish between the 38 systems with non-flexible (or fixed) election calendars and the remaining ones with discretionary elections.<sup>14</sup> The former include countries with presidential or semi-presidential systems in which the president is the head executive and for which the election cannot be called early.

## Results

We first show the basic patterns of the electoral cycle in the pooled sample, absent any accounting for institutional constraints. Figure 1 shows the mean of (real) private fixed investment growth in the data for *Election*, the two preceding years, and the following year. As Figure 1 shows, in the pre-election period that is captured by *Election*, average private fixed

**Figure 1. Average Private Fixed Investment Growth**



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<sup>14</sup> Because Moldova and Sri Lanka switch systems, they are included, with different years, in both samples.

investment growth is lower than in either of the two preceding years or the following year. On average, (real) private fixed investment growth is around 7.0 percent in the pre-election year and ranges between averages of 9.3 to 12.9 percent in the surrounding years.

This relationship is similar to that from a pooled OLS analysis, as shown in Column [1] of Table 1. The estimate on the election variable suggests a decline in private fixed investment of approximately 3.3 percentage points in the pre-election year relative to all other variables. Notably, however, Column [2] highlights that this effect is not evenly distributed. The coefficient on *Election* remains significantly negative and the coefficient on the interaction *Election x ln Institutional Constraints* is significantly positive, suggesting that the size of the electoral investment cycle is lower when there are higher levels of institutional constraints.

Columns (3) and (4) show this finding for our main specification, which incorporates the country and year fixed effects along with panel-corrected standard errors. In Column (3), the log-transformed institutional constraints variable is included while Column (4) presents results with the linear (non-transformed) variable. In either case, the estimates suggest the electoral cycle is mitigated substantially by the presence of formal limits on the executive's capacity to move policy unilaterally. Column (4) suggests that at the lowest levels of institutional constraints, private fixed investment declines 12.7 percentage points in the pre-election period, but that for each standard deviation increase in the constraints index, the effect subsides by approximately 4.8 percentage points. Likewise, Column (3) suggests a baseline electoral cycle of 22.5 percentage points, with a standard deviation increase in the log transformed constraints index mitigating the decline by 4.5 percentage points. In each column, at larger values of the constraints index (e.g., above 3.2 for the logged measure), the electoral investment cycle is no longer statistically significant. Comparing these findings to those on OECD countries, which

**Table 1.** Electoral Investment Cycles and Institutional Constraints

	Pooled OLS (1)	Pooled OLS (2)	PCSE (3)	PCSE (4)	PCSE (5)	Two-way Clustered SEs (6)	Driscoll- Kraay (7)	PCSE, Interest Rate (8)
Election	-3.28* (1.85)	-26.60*** (7.80)	-22.50** (10.28)	-12.72*** (5.67)		-22.21** (8.29)	-22.21** (9.76)	-21.74*** (9.70)
Election × In Institutional constraints		6.40*** (2.07)	5.59** (2.61)			5.53** (2.08)	5.53** (2.57)	5.48** (2.38)
In Institutional constraints		-2.47 (1.52)	0.42 (1.90)		3.05* (1.70)	0.06 (1.99)	0.06 (1.76)	-4.78*** (2.12)
Election × Institutional constraints				22.06** (9.47)				
Institutional constraints				-4.88 (6.63)				
Δ Interest rate								-0.44** (0.17)
Standard controls	✓	✓	✓	✓	✓	✓	✓	✓
Country FEs			✓	✓	✓	✓	✓	✓
Year FEs			✓	✓	✓	✓	✓	✓
N	1069	1069	1069	1069	1069	1069	1069	810

Note: \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed. The dependent variable is Private Fixed Investment Growth. Standard errors are in parentheses. Columns 1 and 2 report Huber-White standard errors. Columns 3, 4, 5, and 8 report panel-corrected standard errors. Column 6 reports multi-way clustering at the country and year level and Column 7 reports Driscoll-Kraay standard errors. Standard controls include Left, Center, and Right government, Rational Partisan Theory, lagged change in real GDP per capita (\$US) and the inflation rate. Coefficient estimates for the standard control variables are presented in Appendix Table A3.

have high levels of institutional constraints and where the electoral investment cycle is significant for the pre-election quarter, it is worth noting no significant effect exists two or more quarters before the election (e.g., Canes-Wrone and Park 2012) and our annual data by necessity include multiple quarters. The fact that we find a clear effect in systems with lower levels of institutional constraints, even with the annual data, highlights the strength of the effect for these systems in addition to the impact of the constraints.

Continuing with Table 1, Column (5) shows the results on institutional constraints if no election variables are included. Interestingly, while the main effect of institutional constraints is not significantly positive in Columns (2) – (4), in Column (5) it is. This result in Column (5) is consistent with earlier work that does not examine electoral investment cycles and finds a direct positive relationship between institutional constraints and investment (e.g., Henisz 2002; Stasavage 2002). Indeed, arguably Column (5) is a tougher test of this direct relationship given the comparably short time span of the data combined with the inclusion of country fixed effects; for instance, Henisz examines a period of two hundred years and Stasavage acknowledges his results are not generally significant once country effects are included. Possibly with a multi-century time span such as in Henisz, we would find effects outside the pre-election period as well.

Columns (6) and (7) show that the results extend to alternative specifications, including with multi-clustered standard errors by country and year and with Driscoll-Kraay standard errors. Column (8) returns to the main PCSE model but includes the interest rate control, which reduces the sample size due to data availability. The control itself has the predicted negative relationship with investment. At the same time, the estimates on the election and institutional constraints variables are remarkably consistent between the specifications with and without this control,

suggesting it does not alter the main findings. Because this variable is not available for the full set of observations, and because its inclusion does not alter the main substantive findings, we present future results in the text without it.

Appendix Table A3 presents the estimates for the other controls. The inflation rate has a significantly negative relationship with investment in multiple models and a negative relationship in all specifications other than that with the interest rate control. Higher rates of inflation tend to be associated with higher interest rates, and therefore in the models without the interest rate control, the inflation control may be capturing these effects. The political ideology variables generally do not have significant effects, and when the rational party theory control is significant the relationship with investment is negative. As mentioned earlier, the theories regarding these variables are focused on total output rather than investment, and given that electoral business cycles operate differently for investment versus total output, arguably it is unsurprising that other political variables operate differently as well. Moreover, the results are robust to excluding the ideology controls, as shown in the online supplemental materials (Table S2).

Overall, Table 1 suggests that even controlling for a variety of potential influences on investment, including factors specific to the country and year, elections induce a decline in private investment that is mitigated by institutional constraints on the executive. These results support the argument that policy uncertainty reduces investment in developing countries. The policy uncertainty induced by elections is associated with a temporary decline in private fixed investment. And institutional constraints, which should reduce the uncertainty from potential turnover, serve to moderate the temporary decline.

To assess the robustness of these findings, we analyze a number of alternative specifications. In the online supplemental materials, Table S1 shows robustness to different operationalizations of the electoral cycle, and Supplemental Table S3 to using a log transformation of the economic variables (where a constant based on the lowest negative value is added to ensure all values are above 0) or the log-based IHS transformation (e.g., Burbidge, Magee, and Robb 1988). Likewise, Supplemental Table S3 demonstrates that the key effects remain significant when high-leverage outlying observations are simply removed. In Supplemental Table S4, the analyses establish the persistence of a moderating effect of institutional constraints when controlling for conceptually distinct components of democratic development, including electoral accountability as reflected in V-Dem's Free and Fair Elections measure (Coppedge et al. 2020) and the updated continuous Machine Learning Democracy Index (Gründler and Krieger 2021).

A related concern might be that the findings are particular to the measure of institutional constraints. Accordingly, in Table 2, we replace PolConV with the Coppedge et al. (2020) Varieties of Democracy (V-Dem) Liberal Component index (`v2x_liberal` in the V-Dem database) and the Marshall, Gurr, and Jagers (2019) Polity Executive Constraints score (`XCONST` in the Polity database). The former, which we label *Liberal Component Index*, averages over indices that measure judicial constraints on the executive, legislative constraints on the executive, individual liberties and equality before the law; the raw values range from 0 to 1. The Polity-based variable *XCONST* ranges from 1 to 7 and is set up to capture constraints on the chief executive from groups that provide accountability, including from the legislature and judiciary as well as, where applicable, the ruling party or military. As with the main measure of institutional constraints, we present results with the linear and logged versions of the variables.

**Table 2.** Alternative Measures of Institutional Constraints

	ln XCONST	Linear XCONST	ln Liberal Component Index	Linear Liberal Component Index
	(1)	(2)	(3)	(4)
Election	-44.63** (18.44)	-28.27** (12.56)	-38.56 (24.50)	-14.59* (7.52)
Election × ln XCONST	24.11** (10.01)			
ln XCONST	-28.76*** (7.58)			
Election × XCONST		4.40** (1.96)		
XCONST		-6.00*** (1.58)		
Election x ln Liberal component index			8.77 (5.87)	
ln Liberal component index			-3.66 (5.60)	
Election x Liberal component index				18.90* (10.70)
Liberal component index				-12.47 (11.76)
Standard controls	✓	✓	✓	✓
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
N	1057	1057	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. All columns report panel-corrected standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , two-tailed. Estimates on control variables are provided in the online supplemental materials (Table S5).

Table 2 presents the results. Notably, regardless of the measure of institutional constraints, the interaction effect is positive and in three of the four cases it reaches conventional levels of significance. Moreover, in the one case where it is below conventional levels (being only significant at  $p < 0.1$ , one-tailed), the estimates reach significance in alternative specifications including with multi-clustered standard errors or Driscoll-Kraay standard errors, as shown in the online supplemental materials (Table S6). Furthermore, with each measure the magnitudes are comparable to those with *PolConV*. For example, according to models with the

linear institutional constraints variables, an increase of one standard deviation in either *Liberal Component Index* or *XCONST* increases the marginal effect of *Election* by 4.7 or 3.2 percentage points, respectively (compared to 4.8 percentage points for *PolConV*). Table 2 thus provides further evidence that the policy uncertainty associated with elections induces electoral investment cycles that are mitigated by the level of institutional constraints.

Arguably an even larger concern about the estimation is the potential for endogenous institutions. Although the concern may be smaller here than for studies of longer periods of time, there remains the possibility that the institutions could shift across the electoral cycle in ways that conflate with an investment cycle. For instance, executives might try to reduce constraints to increase their own policymaking capacity in pre-election periods. Accordingly, Table 3 addresses the potential for endogenous institutions with the previously described specification and instruments, which include the legal origin and population density in the year 1500 (Acemoglu, Johnson, and Robinson 2001; Acemoglu and Johnson 2005). As noted earlier, because these instruments were designed for former colonies, the analysis is limited to these countries, and because the instruments do not vary within a country, the country fixed effects are excluded (although region fixed effects are included).

Columns (1) and (2) present the results from a pooled OLS regression as a baseline comparison. These results are substantively similar to those in the pooled OLS analysis of Table 1, suggesting that the sample of former colonies does not materially alter the findings. Columns (3) and (4) describe the 2<sup>nd</sup>-stage results from the 2SLS analysis, which uses the instruments from Acemoglu and Johnson (2005) and then builds on the Abramson and Boix (2019) critique by including in both stages of the specification lagged urbanization and the interaction of lagged urbanization with the elections. Notably, each column of results suggests there is a significant



**Table 3.** Endogenous Institutions and Urbanization

	Pooled OLS (1)	Pooled OLS (2)	2SLS 2 <sup>nd</sup> Stage (3)	2SLS 2 <sup>nd</sup> Stage (4)	PCSE (5)	PCSE (6)
Election	-28.58** (11.49)	-15.39*** (5.426)	-117.98** (57.71)	-47.07** (21.48)	-26.67** (11.82)	-17.35** (8.28)
Election × ln Institutional constraints*	7.16** (2.999)		31.40** (15.92)		5.57** (2.60)	
ln Institutional constraints*	-2.78 (2.452)		-14.79 (13.48)		0.22 (1.92)	
Election × Institutional constraints*		28.21*** (10.41)		94.37* (50.28)		22.05** (9.43)
Institutional constraints*		-15.91** (6.565)		-24.52 (25.23)		-6.24 (6.82)
Election × Lagged % urban			-0.13* (0.08)	-0.14* (0.07)	-0.36 (0.23)	-0.41* (0.23)
Lagged % urban			0.07 (0.11)	0.11 (0.14)	0.09 (0.10)	0.10 (0.10)
Standard controls	✓	✓	✓	✓	✓	✓
Country Fes					✓	✓
Region Fes			✓	✓		
Year Fes			✓	✓	✓	✓
N	813	813	813	813	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Robust standard errors are in parentheses. Columns 1-4 report Huber-White standard errors. Columns 5 and 6 report panel-corrected standard errors. The first stage results for Columns 3 and 4 are reported in the supplemental materials (Table S7). The instruments for the *Institutional Constraints* variable are the log of population density in 1500, the country's legal origin, and the interactions of both variables with *Election*. Control variable estimates are presented in online Supplemental Table S8. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

and positive moderating effect of institutional constraints on investment in the election period.

Depending on the specification, the magnitudes of the coefficients on the interaction involving institutional constraints and the main effect of elections are a bit more than quadruple (logged institutions) or double (linear institutions) the size of the analogous estimates from specifications that assume exogeneity of the constraints. The results accordingly suggest that, if anything, accounting for the endogeneity of the institutions increases the size of the estimated impacts.

The online supplemental materials (Table S7) present the first-stage results. As this table and associated description details, the instruments are jointly significant in each first-stage equation at  $p < 0.05$ , two-tailed. Furthermore, overidentification tests cannot reject the null that the instruments lack a direct impact on private fixed investment. Finally, specification testing cannot reject the null that the institutional constraints are exogenous, suggesting that the one-equation models are an appropriate means of analyzing the data. Full details are given in the supplemental materials.

Columns (5) and (6) of Table 3 return to the main PCSE specification but also include lagged urbanization and the interaction of lagged urbanization with the elections indicator as additional controls. Again, the inclusion of these controls does not substantively alter the main findings regarding institutional constraints. In each specification, elections induce a decline in private fixed investment and this decline is mitigated by the level of institutional constraints. Thus, even accounting for the endogeneity of institutions and urbanization, the results support our expectations.

In sum, Table 3 pushes the bounds of establishing a causal relationship between policy uncertainty and investment. Elections themselves serve as a relatively exogenous source of policy uncertainty. Furthermore, the 2SLS findings indicate that even when institutions are modeled as endogenous, they mitigate the investment cycles induced by elections. Overall, Tables 1, 2 and 3 demonstrate that across a variety of measures and statistical models, institutional constraints reduce electoral investment cycles, consistent with the argument that policy uncertainty affects investment in developing countries.

Table 4 builds on these findings to test the prediction that these effects should be stronger in systems with fixed election dates than ones with snap elections. When the timing of an

election is predictable, the policy uncertainty associated with it should be better incorporated into investment decisions. Accordingly, we compare results between systems where a president with a fixed term is the head executive to those where this is not the case.

**Table 4.** Fixed versus Discretionary Elections

	Fixed (1)	Disc. (2)	Fixed (3)	Disc. (4)	Fixed (5)	Disc. (6)
Election	-31.10* (15.97)	-9.43 (12.01)	-15.57** (8.05)	-3.04 (6.96)	-34.70** (16.06)	-19.06** (9.47)
Election × ln Institutional constraints	7.53* (4.22)	2.87 (2.96)			8.47** (4.23)	5.22** (2.35)
ln Institutional constraints	-1.45 (2.51)	4.27 (3.64)			-3.14 (2.33)	-1.83 (1.91)
Election × Institutional constraints			25.90* (15.17)	7.83 (11.06)		
Institutional Constraints			-10.01 (8.98)	6.72 (9.68)		
Standard controls	✓	✓	✓	✓	✓	✓
Country FEs	✓	✓	✓	✓		
Year FEs	✓	✓	✓	✓	✓	✓
N	627	436	627	436	627	436

Note: The dependent variable is Private Fixed Investment Growth. All columns report panel-corrected standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed. Control variable estimates are presented in the online supplemental materials (Table S9).

Columns (1)-(4) compare the systems with fixed and discretionary elections for the logged and linear measures of institutional constraints with the standard specification. In each case, for the systems with fixed elections (Columns 1 and 3), the coefficient on *Election* is significantly negative and, as before, the coefficient on the interaction between *Election* and *Institutional Constraints* is significantly positive. Moreover, the magnitudes of the effects are more than double those for discretionary elections in Columns (2) and (4). The magnitudes are also higher than those in the analogous analyses for the full sample of both types of elections, back in Table 1 (Columns 3 and 4). For instance, whereas those results suggested that the

marginal effect of electoral proximity increases by 4.8 percentage points with a one standard deviation increase in the linear institutional constraints measure, the analogous impact for fixed elections is 5.6 percentage points (Column 3 of Table 4).

Table 4 further suggests that when the country and year effects are included in the analysis of the systems with discretionary elections, there is not a significant effect of either the main effect of the election cycle or the interaction involving institutional constraints.<sup>15</sup> Because the number of observations for the discretionary elections sample is arguably low relative to the full set of predictors including the country and year effects, we also show results excluding the country effects, in Columns (5) and (6). These results again suggest that the effects are larger in countries with fixed elections, but here significant coefficients emerge in the systems with discretionary elections as well. These findings, combined with the similar qualitative results in the other columns, indicate that if more years of data were available, the estimates for the discretionary systems might be significant at conventional levels even with country fixed effects. The fact that the magnitudes are lower for discretionary elections across all specifications, however, suggests a stronger impact when investors can predict with accuracy the timing of elections.

Overall, Table 4 offers a good deal of evidence in support of the theoretical expectations. When firms and investors can anticipate the electoral period with regularity, an electoral investment cycle emerges whereby they are less apt to take on costly-to-undo investments prior to the election. At the same time, institutional constraints counteract this cycle such that checks

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<sup>15</sup> The online supplemental materials (Table S10) show that the results for systems with discretionary elections hold even with 2SLS analyses that account for the endogeneity of these elections; full details are given in the supplemental materials.

and balances on the executive's ability to move policy unilaterally diminish the effect of elections on investment. In systems with discretionary elections, the effects are weaker and less robust to the choice of specification, but there is still evidence of a smaller electoral investment cycle that is counteracted by the level of institutional constraints. This difference is in keeping with expectations in that snap elections are harder to predict and when called unexpectedly, leave investors with little lead time to adjust fixed investment decisions. In sum, the relationship among the election type, institutional constraints, and private fixed investment is consistent with a world in which policy uncertainty reduces private investment.

## **Discussion and Conclusion**

Despite the importance of private investment to development, understanding the political and policy-related factors that affect it in developing countries has proven difficult given that these factors are both a cause and result of investors' behavior. Thus, while the idea that policy uncertainty dampens investment is longstanding, empirical evidence for this impact is scarce and existing scholarship has been subject to critiques about alternative causes and reverse causality. This paper provides new evidence by analyzing the relationship among elections, institutional constraints, and investment in 57 developing democracies for over four decades. Elections, particularly when fixed in timing, offer a reasonably exogenous source of policy uncertainty, which should be lower the higher the level of institutional constraints. To examine these relationships, we have analyzed a range of specifications, including ones that account for the endogeneity of institutional constraints, the potential impact of urbanization, and differential effects between systems with fixed and discretionary elections.

Three main findings emerge. First, as expected, an electoral investment cycle occurs, whereby on average private fixed investment growth declines during the pre-election period. Second, however, the electoral cycle is mitigated by institutional constraints regarding the executive's ability to change policy unilaterally. At lower levels of constraints, elections are associated with a substantial decline in private fixed investment growth. However, this effect subsides gradually and indeed, at higher levels, the marginal effect of electoral proximity is no longer statistically significant. Thus, electoral investment cycles materialize from those elections in which policy may change the most substantially. Third, these patterns are strongest in systems with fixed elections. When investors know that an election is impending, they can adjust the timing of investment decisions to minimize the policy uncertainty.

Notably, although we have leveraged elections as a source of policy uncertainty, the analysis does not suggest that elections, let alone democratic development, is bad for investment. Rather, elections serve as a temporary spike in policy uncertainty that abates after a new government is formed. Moreover, the evidence on the interaction of institutional constraints and elections suggests that at least this component of democratic development reduces the policy uncertainty associated with leadership turnover. As such, the results are consistent with the argument that efforts to increase capital investment should benefit from strengthening institutions designed to constrain unilateral executive policymaking.

More broadly, the findings present numerous avenues for future research. First, they suggest that studies of the classic electoral business cycle in developing democracies may be understating the expansionary effect of electoral proximity on total economic output. Treisman and Gimpelson (2001) point out that studies of developing democracies have often failed to find any effect in real economic outcomes due to focusing on one policy instrument, such as total

GDP, at a time. This study offers another reason for null effects of such studies — namely, that one part of GDP, private fixed investment, is experiencing a decline while other parts may be expanding in the run-up to the election. Second, subsequent scholarship might focus on a narrower subset of developing democracies for which higher frequency data may be available. Such data, if obtainable, could offer greater insight into the dynamics of electoral investment cycles, particularly for systems with discretionary timing given the short window investors often have between the calling of an election and the election itself. Third, our analysis raises questions about how constraints on the executive affect electoral cycles and investment in OECD countries. These countries tend to have higher levels of economic development and although institutional constraints are stronger on average, some governments, such as in Hungary, have experienced backsliding. Future research might examine whether such backsliding has the effects revealed here for developing democracies.

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## Appendix

**Appendix Table A1.** List of Countries

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Albania	El Salvador	Liberia	Pakistan
Bangladesh	The Gambia	Madagascar	Panama
Bhutan	Georgia	Malawi	Papua New Guinea
Bolivia	Ghana	Malaysia	Peru
Botswana	Guatemala	Mali	Philippines
Brazil	Guinea-Bissau	Mauritius	Romania
Bulgaria	Guyana	Moldova	Russia
Burundi	Haiti	Mongolia	Senegal
Colombia	Honduras	Mozambique	Sierra Leone
Republic of Congo	India	Namibia	South Africa
Costa Rica	Jamaica	Nepal	Sri Lanka
Côte d'Ivoire	Kenya	Nicaragua	Thailand
Croatia	Kyrgyz Republic	Niger	Ukraine
Dominican Republic	Lebanon	North Macedonia	Uruguay
Ecuador			

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**Appendix Table A2. Descriptive Statistics**

<b>Variable Name</b>	<b>N</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min.</b>	<b>Max.</b>
Private fixed investment growth (%)	1069	9.476	32.503	-53.358	295.537
Private fixed investment growth (%), unwinsorized	1069	11.810	71.689	-75.755	1625.817
Election	1069	0.221	0.415	0	1
Institutional Constraints (PolConV)	1069	0.476	0.217	0	0.855
ln Institutional Constraints (PolConV)	1069	3.697	0.808	0	4.460
XCONST (Polity)	1057	5.854	1.064	3	7
ln XCONST (Polity)	1057	1.748	0.203	1.100	1.946
Liberal component index (V-Dem)	1069	0.641	0.171	0.187	0.934
ln Liberal component index (V-Dem)	1069	4.119	0.304	2.929	4.537
lagged per capita GDP growth (in \$US)	1069	0.064	0.130	-0.373	0.600
lagged per capita GDP growth (in \$US), unwinsorized	1069	0.064	0.135	-0.522	0.983
Inflation rate	1069	0.282	1.596	-0.023	19.274
Inflation rate, unwinsorized	1069	0.385	3.222	-0.078	74.817
Interest rate change	810	0.036	7.574	-34.347	33.694
Interest rate change, unwinsorized	810	0.151	9.100	-57.534	99.800
Left government	1069	0.335	0.472	0	1
Center government	1069	0.085	0.279	0	1
Right government	1069	0.285	0.452	0	1
Non-ideological government	1069	0.295	0.456	0	1
Rational partisan theory	1069	0.001	0.159	-1	1
lagged % Urban	1069	45.694	19.344	8.541	95.045
Legal origin	813	0.445	0.497	0	1
ln Population density in 1500	813	0.482	1.562	-2.211	3.219

Note: World Development Indicator (WDI) labels and base years for the economic variables are as follows. Private fixed investment growth is from Gross fixed capital formation, private sector (current LCU). Per capita GDP growth is from WDI GDP per capita (constant 2015 US\$). Rate of inflation is based on the WDI Consumer price index (2010=100), and Interest rate change is based on the WDI Real interest rate.

**Appendix Table A3.** Electoral Investment Cycles and Institutional Constraints, Control Variable Estimates

	Pooled		Two-way			Driscoll-Kraay		PCSE, Interest Rate
	OLS (1)	Pooled OLS (2)	PCSE (3)	PCSE (4)	PCSE (5)	Clustered SEs (6)	Kraay (7)	(8)
Left government	1.92 (2.47)	2.27 (2.55)	-0.23 (3.94)	0.10 (3.94)	-0.36 (3.98)	0.08 (3.55)	0.08 (4.05)	-1.21 (3.81)
Center government	0.51 (3.90)	0.57 (3.88)	2.04 (5.11)	2.57 (5.12)	1.80 (5.12)	2.24 (4.78)	2.24 (4.88)	-1.01 (4.28)
Right government	-0.38 (2.20)	-0.10 (2.19)	-1.67 (3.92)	-1.35 (3.94)	-2.08 (3.96)	-1.55 (2.61)	-1.55 (2.37)	-6.00 (4.51)
Rational partisan theory	-10.37** (4.60)	-9.94** (4.69)	-8.32 (5.87)	-8.27 (5.91)	-8.44 (5.85)	-9.12* (4.86)	-9.12 (5.47)	-4.83 (8.35)
lagged $\Delta$ GDP per capita	14.95 (11.86)	13.71 (11.94)	6.39 (10.79)	6.65 (10.81)	6.79 (10.82)	8.54 (9.82)	8.55 (9.50)	19.31 (13.96)
Inflation rate	-0.44 (0.89)	-0.45 (0.89)	-0.71* (0.43)	-0.66 (0.43)	-0.69 (0.43)	-0.73 (0.46)	-0.73* (0.39)	1.12 (1.04)
Country FEs			✓	✓	✓	✓	✓	✓
Year FEs			✓	✓	✓	✓	✓	✓
N	1069	1069	1069	1069	1069	1069	1069	810

Note: \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed. The dependent variable is Private Fixed Investment Growth. Standard errors are in parentheses. Columns 1 and 2 report Huber-White standard errors. Columns 3, 4, 5, and 8 report panel-corrected standard errors. Column 6 reports multi-way clustering at the country and year level, and Column 7 reports Driscoll-Kraay standard errors. Coefficient estimates for the key independent variables are presented in Table 1.

## Online Supplemental Materials

## Section A. Tables and Figures

Supplemental Table S1 considers two alternative measures of the election period. The half-year measures reflect whether the election occurs in the first or second half of the same calendar year as the investment data. The calendar year indicator groups together these half-years into one indicator. (Recall that the election indicator in the main text groups together elections that occurred in the second half of the calendar year of the investment data with the first half of the following calendar year, in order to minimize post-election effects.) Supplemental Table S1 supports the main results of the paper by showing that when the election occurs in the second half of the year, such that the investment data primarily reflect the pre-election period, the main results hold. As well, the results hold with the calendar year indicator.

**Supplemental Table S1.** Alternative Election Indicators

	Half-years (1)	Half-years (2)	Calendar year (3)	Calendar year (4)
Election 2 <sup>nd</sup> half-year	-33.32** (13.25)	-23.46*** (7.68)		
Election 1 <sup>st</sup> half-year	-10.29 (13.67)	-7.82 (7.07)		
Election 2 <sup>nd</sup> half-year × ln Institutional constraints	7.71** (3.35)			
Election 1 <sup>st</sup> half-year × ln Institutional constraints	3.31 (3.57)			
Election 2 <sup>nd</sup> half-year × Institutional constraints		37.56*** (12.36)		
Election 1 <sup>st</sup> half-year × Institutional constraints		20.24 (12.39)		
Election calendar year			-22.10** (9.76)	-15.53*** (5.34)
Election calendar year × ln Institutional constraints			5.57** (2.51)	
Election calendar year × Institutional constraints				28.55*** (8.89)
ln Institutional constraints	0.41 (1.73)		0.58 (1.73)	
Institutional constraints		-7.52 (6.48)		-6.69 (6.46)
Standard controls	✓	✓	✓	✓
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
N	1069	1069	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Columns report panel-corrected standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.



**Supplemental Table S2.** Alternative Combinations of Control Variables

	Region-Year effects (1)	Region-Year effects (2)	Excluding ideology controls (3)	Excluding ideology controls (4)
Election	-27.06** (10.79)	-14.52** (5.99)	-22.94** (10.34)	-12.70** (5.67)
Election × ln Institutional constraints	6.49** (2.76)		5.73** (2.63)	
ln Institutional constraints	-1.09 (1.89)		0.28 (1.91)	
Election × Institutional constraints		23.61** (10.55)		22.15** (9.47)
Institutional constraints		-5.76 (6.73)		-5.00 (6.59)
lagged Δ GDP per capita	3.68 (11.21)	4.04 (11.23)	5.91 (10.76)	6.09 (10.77)
Left government	-2.01 (4.28)	-1.77 (4.24)		
Center government	-0.44 (4.70)	-0.09 (4.68)		
Right government	-2.18 (3.99)	-2.10 (3.99)		
Rational partisan theory	-9.64** (4.70)	-9.70** (4.72)		
Inflation rate	-1.89*** (0.58)	-1.88*** (0.58)	-0.66 (0.43)	-0.61 (0.44)
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Region-Year FEs	✓	✓	✓	✓
N	1069	1069	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Columns report panel-corrected standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

Supplemental Table 3 provides results for alternative approaches to dealing with outlying observations. In particular, Columns 1 and 2 show results of the main specifications, but with all economic variables transformed by adding the minimum value for each variable and taking the natural logarithm of all values. Columns 3 and 4 transform the economic variables according to the inverse hyperbolic sine (IHS) log-transformation (e.g., Burbidge, Magee, and Robb 1988), which takes the natural logarithm of the sum of the original value and the square root of the sum of the squared original value plus 1. Further, Columns 5 and 6 remove high-leverage outlying observations that might skew the results. In particular, observations were removed from the analysis if both leverage (the diagonal elements of the hat matrix) and the normalized (through min-max normalization) squared residual value were greater than 0.1.

**Supplemental Table S3.** Alternative Approaches to Outlying Observations

	ln(x+min x) (1)	ln(x+min x) (2)	IHS (3)	IHS (4)	Outliers removed (5)	Outliers removed (6)
Election	-0.55*** (0.15)	-0.26*** (0.08)	-1.14** (0.51)	-0.55** (0.27)	-22.35** (9.48)	-9.91** (4.65)
Election × ln Institutional constraints	0.14*** (0.04)		0.31** (0.13)		5.85** (2.45)	
ln Institutional constraints	0.01 (0.02)		0.10 (0.10)		-0.00 (1.79)	
Election × Institutional constraints		0.47*** (0.13)		1.15** (0.50)		18.34** (8.18)
Institutional constraints		-0.04 (0.08)		0.09 (0.37)		-6.60 (6.21)
lagged Δ GDP per capita	0.03 (0.04)	0.03 (0.04)	0.54 (0.42)	0.56 (0.42)	-9.06 (9.81)	-8.72 (9.82)
Left government	-0.02 (0.06)	-0.02 (0.06)	0.07 (0.20)	0.09 (0.20)	-0.22 (3.77)	0.19 (3.79)
Center government	0.02 (0.06)	0.04 (0.06)	-0.15 (0.28)	-0.12 (0.28)	1.32 (5.09)	1.90 (5.10)
Right government	-0.03 (0.05)	-0.02 (0.05)	-0.31 (0.22)	-0.29 (0.22)	-1.03 (3.79)	-0.65 (3.81)
Rational partisan theory	-0.09* (0.05)	-0.09* (0.05)	-0.47 (0.29)	-0.46 (0.29)	-3.68 (5.39)	-3.58 (5.42)
Inflation rate	-0.04* (0.02)	-0.03 (0.02)	-0.35** (0.14)	-0.32** (0.14)	-1.90*** (0.38)	-1.82*** (0.38)
Country FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
N	1069	1069	1069	1069	1059	1059

Note: The dependent variable is Private Fixed Investment Growth. Columns report panel-corrected standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

Supplemental Table S4 includes controls that aim to capture the level of electoral democracy. Columns (1) and (3) include the V-Dem (Coppedge et al. 2020) variable Free and Fair Elections (v2xel\_frefair), which captures “to what extent elections are free and fair.” Columns (2) and (4) include the continuous Machine Learning (ML) Democracy measure (Gründler and Krieger 2021) which captures political participation, political competition, and freedom of opinion.

**Supplemental Table S4.** Controlling for Electoral Democracy, Main Variables

	Free & fair elections (1)	ML democracy (2)	Free & fair elections (3)	ML democracy (4)
Election	-40.49** (15.98)	-38.98* (21.34)	-21.90*** (8.09)	-22.52** (9.47)
Election × ln Institutional constraints	4.93* (2.64)	4.86* (2.62)		
ln Institutional constraints	0.62 (1.92)	0.47 (1.95)		
Election × ln Free & fair elections	5.11 (3.23)			
ln Free & fair elections	-1.69 (2.70)			
Election × ln ML democracy		4.43 (4.64)		
ln ML democracy		0.06 (2.83)		
Election × Institutional constraints			16.68* (9.26)	17.40* (9.67)
Institutional constraints			-3.13 (6.62)	-4.40 (6.66)
Election × Free & fair elections			19.52** (9.05)	
Free & fair elections			-11.27 (11.40)	
Election × ML democracy				14.84 (10.51)
ML democracy				-1.66 (7.84)
Standard controls	✓	✓	✓	✓
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
N	1069	1069	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Columns report panel-corrected standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

**Supplemental Table S5.** Control Variable Estimates for Table 2

	In XCONST (1)	Linear XCONST (2)	In Liberal Component Index (3)	Linear Liberal Component Index (4)
Left government	1.30 (4.05)	1.24 (4.05)	0.68 (4.03)	0.46 (4.03)
Center government	3.01 (5.26)	3.01 (5.26)	2.75 (5.13)	2.74 (5.14)
Right government	-0.82 (4.15)	-0.98 (4.16)	-1.45 (3.94)	-1.31 (3.92)
Rational partisan theory	-8.37 (5.83)	-8.32 (5.86)	-8.10 (5.89)	-8.23 (5.90)
lagged $\Delta$ GDP per capita	6.22 (10.76)	6.44 (10.78)	6.22 (10.84)	6.58 (10.85)
Inflation rate	-0.54 (0.45)	-0.52 (0.45)	-0.58 (0.44)	-0.60 (0.44)
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
N	1057	1057	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. All columns report panel-corrected standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , two-tailed.

**Supplemental Table S6.** Alternative Specifications for *ln Liberal Component Index*

	Driscoll-Kraay standard errors (1)	Two-way clustered standard errors (2)
Election	-39.49** (18.90)	-39.49* (21.15)
Election × ln Liberal component index	9.02** (4.32)	9.02* (5.03)
ln Liberal component index	-3.25 (7.00)	-3.25 (4.65)
Standard controls	✓	✓
Country FEs	✓	✓
Year FEs	✓	✓
N	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

**Supplemental Table S7.** First-Stage Estimates from 2SLS Analysis of Endogenous Institutions

	1 <sup>st</sup> -stage estimates Column 3 of Table 3		1 <sup>st</sup> -stage estimates Column 4 of Table 3	
	In Institutional constraints (1)	Election × In Institutional constraints (2)	Institutional constraints (3)	Election × Institutional constraints (4)
In Population density 1500	-0.104*** (0.024)	-0.025** (0.010)	-0.053*** (0.008)	-0.008*** (0.002)
Election × In Pop. Density 1500	-0.031 (0.047)	-0.050 (0.043)	0.000 (0.013)	-0.019 (0.012)
Legal origin	0.061 (0.086)	-0.024 (0.031)	0.006 (0.023)	-0.015** (0.007)
Election × Legal origin	0.242 (0.190)	0.430** (0.182)	0.022 (0.041)	0.099*** (0.037)
Election	-0.426 (0.315)	3.147*** (0.297)	-0.065 (0.062)	0.373*** (0.056)
lagged % Urban	-0.003 (0.002)	-0.002** (0.001)	-0.001** (0.001)	-0.000*** (0.000)
Election × lagged % Urban	0.003 (0.005)	0.004 (0.005)	0.001 (0.001)	0.000 (0.001)
lagged Δ GDP per capita	0.109 (0.218)	0.264** (0.130)	0.037 (0.061)	0.050* (0.030)
Left government	0.192** (0.090)	0.035 (0.056)	0.041** (0.020)	0.011 (0.010)
Center government	-0.016 (0.120)	0.028 (0.063)	-0.066** (0.033)	-0.010 (0.014)
Right government	0.109 (0.100)	0.025 (0.057)	-0.007 (0.023)	0.001 (0.012)
Rational partisan theory	-0.025 (0.093)	-0.028 (0.043)	-0.010 (0.044)	-0.001 (0.010)
Inflation rate	0.045*** (0.009)	0.019** (0.007)	0.017*** (0.004)	0.006 (0.003)
Region FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
N	813	813	813	813

Note: The estimates correspond to the 2<sup>nd</sup>-stage results reported in Table 3 in the main text. The dependent variables are In Institutional constraints in Column 1, Election × In Institutional constraints in Column 2, Institutional constraints in Column 3, and Election × Institutional constraints in Column 4. All columns report Huber-White standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

Additional Notes on Supplemental Table S7: The p-values and F-statistics reported by Stata for the joint significance of the instruments in the first stage are: (Column 1) Robust  $F(4,756)=8.1868$ ,  $p=0.0000$ ; (Column 2) Robust  $F(4,756)=3.3448$ ,  $p=0.0100$ ; (Column 3) Robust  $F(4,756)=15.8961$ ,  $p=0.0000$ ; (Column 4) Robust  $F(4,756)=4.93238$ ,  $p=0.0006$ . The p-values for the robust score tests of overidentification (Wooldridge 1995) are  $p = 0.5279$  for Columns 1 and 2 and  $p = 0.4143$  for Columns 3 and 4. Finally, the robust score values for tests of endogeneity of the instruments are  $p = 0.2137$  (Columns 1 and 2) and  $p = 0.3009$  (Columns 3 and 4).

**Supplemental Table S8.** Estimates for 2<sup>nd</sup>-stage Control Variables, Table 3

	Pooled OLS		2SLS		PCSE with % Urban	
	In Constraints (1)	Linear Constraints (2)	In Constraints (3)	Linear Constraints (4)	In Constraints (5)	Linear Constraints (6)
Left government	2.08 (3.00)	2.10 (2.98)	4.31 (4.37)	2.57 (3.33)	0.37 (4.02)	0.79 (4.03)
Center government	-3.07 (2.66)	-3.37 (2.67)	-1.11 (3.39)	-0.86 (3.30)	2.52 (5.13)	3.12 (5.14)
Right government	-1.62 (2.30)	-1.98 (2.32)	-0.68 (3.40)	-1.82 (2.91)	-1.34 (3.93)	-0.96 (3.95)
Rational partisan theory	-11.83** (5.41)	-11.67** (5.26)	-9.74* (5.18)	-10.41** (5.14)	-8.03 (5.85)	-7.97 (5.89)
Lagged $\Delta$ GDP per capita	7.29 (9.77)	8.95 (9.70)	3.43 (11.03)	5.94 (11.00)	6.06 (10.83)	6.28 (10.84)
Inflation rate	0.10 (0.96)	0.23 (0.94)	0.30 (1.09)	0.08 (1.06)	-0.73* (0.43)	-0.67 (0.44)
Country FEs					✓	✓
Region FEs			✓	✓		
Year FEs			✓	✓	✓	✓
N	861	861	813	813	1069	1069

Note: The dependent variable is Private Fixed Investment Growth. Robust standard errors are in parentheses. Columns 1, 2, 3 and 4 report Huber-White standard errors. Columns 5 and 6 reports panel-corrected standard errors. The first stage results for Columns 3 and 4 are reported in Supplemental Table S7. The instruments for the *Institutional Constraints* variable are the log of population density in 1500, the country's legal origin, and the interactions of both variables with *Election*. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.



**Supplemental Table S9.** Control Variable Estimates for Table 4

	Ln Constraints		Linear constraints		No country FEs	
	Fixed (1)	Disc. (2)	Fixed (3)	Disc. (4)	Fixed (5)	Disc. (6)
Left Government	3.88 (6.59)	-7.56** (3.60)	3.40 (6.61)	-6.23* (3.61)	2.54 (4.78)	-0.38 (3.13)
Center Government	4.07 (5.98)	-9.56 (7.25)	4.15 (5.99)	-8.49 (7.16)	-0.87 (4.66)	-3.31 (5.70)
Right Government	0.61 (5.92)	-5.15 (4.82)	0.35 (5.92)	-4.26 (4.86)	-3.06 (3.82)	2.60 (3.70)
Rational partisan theory	-16.47*** (5.17)	3.29 (10.52)	-16.48*** (5.16)	4.15 (10.71)	-17.63*** (5.39)	5.25 (10.85)
Lagged $\Delta$ GDP per capita	6.14 (13.96)	-9.75 (16.22)	7.14 (13.96)	-10.09 (16.37)	2.50 (13.98)	-4.58 (16.44)
Inflation rate	-0.56 (0.43)	-3.14 (8.99)	-0.50 (0.43)	-4.15 (9.75)	-0.71 (0.43)	-9.58 (8.01)
Country FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓		
N	627	436	627	436	627	436

Note: The dependent variable is Private Fixed Investment Growth. All columns report panel-corrected standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , two-tailed.

### Supplemental Table S10. Endogenous Discretionary Elections

Below we present results from two-stage least squares models that account for the potential endogeneity of *Election* in systems with discretionary elections. Accordingly, in the 1<sup>st</sup>-stage equations, *Election* and *Election* interacted with institutional constraints (whether logged or linear) are the dependent variables. The instrument for *Election* is *Term Expires*, which equals 1 if the term is scheduled to expire that year and 0 otherwise. This approach follows prior scholarship that uses the maximum term expiration as an instrument for elections (e.g., Heckelman and Berument 1998; Canes-Wrone and Park 2012). As is common in models with potentially endogenous factors in the interaction term (Wooldridge 2002), the first-stage equations also include an interaction between term expires and institutional constraints, given that the latter is interacted with the election indicator in the 2<sup>nd</sup>-stage equation. All standard controls from the text are also included in both the 1<sup>st</sup>- and 2<sup>nd</sup>-stage equations. These results are consistent with those in Table 4 for the main specifications (with the country fixed effects). Moreover, specification testing fails to reject the null of the exogeneity of *Election*.

Specifically, the robust score values for tests of endogeneity of the instruments (Wooldridge 1995) are  $p = 0.2099$  (logged institutional constraints) and  $p = 0.2296$  (linear institutional constraints). The p-values and F-statistics for the joint significance of the instruments in the first stage estimates presented in Panel B below are: (Column 1) Robust  $F(2,365) = 13.7342$ ,  $p=0.0000$ ; (Column 2) Robust  $F(2,365)= 14.0958$ ;  $p=0.0000$ ; (Column 3) Robust  $F(2,365)= 14.1039$ ;  $p=0.0006$ ; and (Column 4) Robust  $F(2,365)= 12.7861$ ;  $p=0.0000$ .

**Supplemental Table S10, Panel A. 2<sup>nd</sup>-stage Estimates.**

	In Institutional Constraints	Linear Institutional Constraints
	(1)	(2)
Election	-67.72 (45.69)	-37.03 (22.88)
Election × In Institutional constraints	14.87 (11.05)	
In Institutional constraints	-4.07 (4.76)	
Election × Institutional constraints		49.05 (35.38)
Institutional constraints		-8.17 (12.31)
Left government	-8.31* (4.53)	-7.98* (4.36)
Center government	-11.45* (6.85)	-10.61 (6.78)
Right government	-7.77** (3.68)	-7.47** (3.70)
Rational partisan theory	0.07 (8.35)	-0.78 (8.07)
Lagged Δ GDP per capita	1.91 (28.31)	-0.75 (28.61)
Inflation rate	2.53 (11.96)	3.29 (11.09)
Country FEs	✓	✓
Year FEs	✓	✓
N	436	436

Note: The dependent variable is Private Fixed Investment Growth. Columns report Huber-White standard errors in parentheses. \* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01, two-tailed.

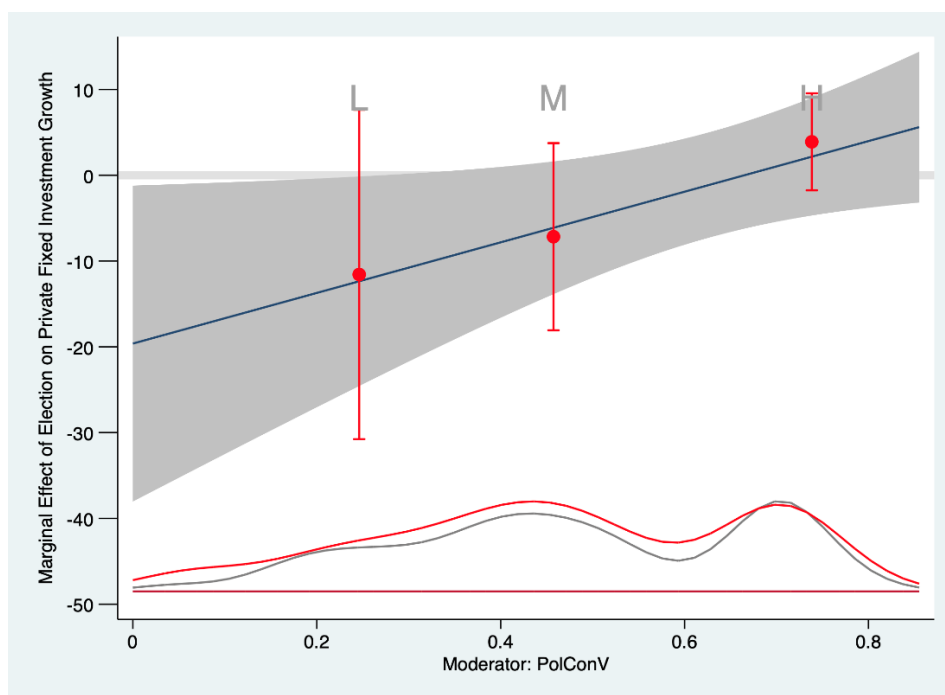
**Supplemental Table S10, Panel B. 1<sup>st</sup>-stage Estimates**

	Election (1)	Election × ln Institutional constraints (2)	Election (3)	Election × Institutional constraints (4)
Term expires	0.204 (0.315)	- 0.594 (0.672)	0.265 (0.186)	- 0.078 (0.075)
Institutional constraints × Term expires	0.029 (0.080)	0.475** (0.186)	0.100 (0.314)	0.462** (0.159)
ln Institutional constraints	- 0.085 (0.062)	0.065 (0.143)		
Institutional constraints			- 0.145 (0.179)	0.100 (0.090)
Lagged Δ GDP per capita	0.046 (0.249)	0.071 (0.966)	0.050 (0.250)	0.049 (0.144)
Left government	- 0.028 (0.095)	- 0.044 (0.372)	- 0.046 (0.096)	- 0.001 (0.055)
Center government	- 0.036 (0.200)	0.003 (0.805)	- 0.043 (0.201)	0.016 (0.126)
Right government	- 0.017 (0.104)	- 0.036 (0.409)	- 0.024 (0.105)	0.002 (0.062)
Rational partisan theory	- 0.058 (0.147)	- 0.336 (0.547)	- 0.068 (0.146)	- 0.021 (0.081)
Inflation rate	0.068 (0.200)	0.697 (0.587)	0.091 (0.185)	0.128 (0.085)
Country FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Joint significance of instruments	$F_{(2, 365)} = 13.73$ $p < 0.01$	$F_{(2, 365)} = 14.10$ $p < 0.01$	$F_{(2, 365)} = 14.10$ $p < 0.01$	$F_{(2, 365)} = 12.79$ $p < 0.01$
N	436	436	436	436

Note: The dependent variables are (1) Election; (2) Election x ln Institutional constraints; (3) Election; and (4) Election x Institutional constraints. All columns report Huber-White standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , two-tailed.

### Supplemental Figure S1. Linear Interaction Effect and Common Support Assumptions

Hainmueller, Mummolo, and Xu (2019) provide persuasive evidence that diagnostics checks should be used to validate the assumptions of a linear interaction effect and common support of the moderator for different values of the main independent variable. We rely on the authors' *interflex* package in STATA to examine the assumption of common support and to compare conditional marginal effects of *Election* from a linear interaction model to those from the binned estimator model. As can be seen in the figure, the relative proximity of the binned estimates (red dots) to the estimate from the linear interaction model (blue line) provides initial evidence in favor of the linear interaction effect assumption. Further evidence is provided by a Wald test, which rejects the null hypothesis that the linear interaction model and the three-bin model are statistically equivalent ( $p=0.26$ ). Supplemental Figure 1 also highlights via density plots of the distributions that the moderator *PolConV* has common support across both values of the *Election* indicator (gray for the value of 0, red for the value of 1). The *interflex* package does not facilitate using multiway clustering or our main specification, which includes an AR(1) process to model the errors. Therefore, we rely on a specification with country and year fixed effects with standard errors clustered at the country-level.



Note: The plot shows the estimated marginal effect of *Election* on Private Fixed Investment Growth for different values of the moderator *PolConV* (*Institutional constraints*). The blue line shows the effect from a linear interaction model; the red dots show the estimates from the binning estimator, with three bins. The gray and red density plots show the distribution of the moderator for values of 0 and 1 (respectively) of the *Election* indicator. The models include country and year fixed effects. Standard errors are clustered at the country level.

## **Section B. Coding of Political System Variables**

To code years with elections of the head executive as well as systems with fixed vs. discretionary election schedules, we first determined whether a regime was parliamentary, presidential, or semi-presidential. Further, for semi-presidential systems, we determined whether the head executive is the prime minister or the president/head of state since the latter tend to have fixed election schedules. We coded systems as parliamentary, presidential, and semi-presidential based on the DPI (Cruz, Keefer, and Scartascini 2018), Bjørnskov and Rode (2020) regime data, as well as from the V-Dem (Coppedge et al. 2020) data on regime type and the ability of the head of state to propose legislation. From the V-Dem data, regimes were coded as parliamentary if the head of state is not directly elected, is not the same as the head of government, and the head of government could at least potentially be removed by the legislature. The criteria for presidential systems are that at least one of the following holds: the head of government cannot be removed by the parliament and is not appointed by parliament, the head of state is the same as the head of government, or that the head of government is directly elected. For semi-presidential systems, either 1) the head of state is directly elected, is not the same as the head of government, and the government could potentially be removed by the legislature, or, 2) the head of state is directly elected, is not the same as the head of government, and the head of government is appointed by the legislature. Where the three sources yielded inconsistent results, we further examined the country's constitution or country data from the CIA World Factbook to determine the correct regime. We coded semi-presidential regimes as having a dominant president if the president can propose policy in all policy areas, based on the V-Dem data. With the exception of Guyana, all presidential systems and semi-presidential systems with a president as the head executive were determined to have fixed election schedules according to their constitutions as well as the pattern of elections held as recorded by the DPI data.

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