Strength in Expectation
Elections, Economic Performance and Authoritarian Breakdown

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Abstract

How do elections and the economy affect authoritarian survival? Distinguishing between (a) non-election periods in autocracies that do not hold competitive elections; (b) election periods in autocracies that hold regular elections; and (c) non-election periods in such autocracies, I argue that bad economic performance makes authoritarian regimes especially likely to break down in election years, but the anticipation of competitive elections should dissuade citizens and elites from engaging in anti-regime behavior in non-election periods, facilitating short-term survival. Thus, compared to regimes that do not hold competitive elections, electoral autocracies should be more vulnerable to bad economic performance in election periods but more resilient to it in non-election years. A study of 258 authoritarian regimes between 1948 and 2011 confirms these expectations. The effect is driven by competitive elections determining the composition of the executive office, and elections-related breakdowns are more likely to result in democratization.

Keywords: authoritarian regimes – authoritarian elections – economic performance – regime breakdown

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How do elections and economic performance affect authoritarian survival? The answer to this question remains hotly contested. While the view that autocracies are more likely to break down in bad economic times is widely shared (Haggard and Kaufman 1995; Bueno de Mesquita et al. 2003; Kennedy 2010; Teorell 2010; Treisman 2015; Houle, Kayser and Xiang 2016), several authors have noted that such regimes can be very resilient to serious economic crises (Pepinsky 2009; Levitsky and Way 2010). Similarly, while some researchers claim that elections foster authoritarian durability by co-opting subnational elites or communicating information about the government's strength (Blaydes 2010; Reuter et al. 2016; Magaloni 2006; Cox 2009; Miller 2013; Little 2012, 2015; Egorov and Sonin 2014; Higashijima 2015; Rozenas 2016), others retort that competitive elections represent a risky endeavor, as victory is not assured and attempts to tinker with the electoral results may trigger mass protests or even coups (Lindberg 2006; Tucker 2007; Kuntz and Thompson 2009; Levitsky and Way 2010; Magaloni 2010; Fearon 2011; Daxecker 2012; Schedler 2013; Egorov and Sonin 2014; Higashijima 2015; Little, Tucker and LaGatta 2015; Bernhard, Edgell and Lindberg 2016; Wig and Rød 2016; Knutsen, Nygård and Wig 2017; Schuler, Gueorguiev and Cantú 2017; Seki 2017).

In this paper I attempt to bridge these literatures by studying how the effect of the economy on breakdown is mediated by competitive elections, and vice versa.[1] Bad economic performance is more likely to result in authoritarian breakdown when exogenous circumstances – like leadership turnover – increase regime weakness (Miller 2012; Treisman 2015; Besley, Persson and Reynal-Querol 2016). I claim that elections can play a similar role. On the one hand, they make it easier to coordinate against the government, increasing vulnerability to breakdown when the economy is doing badly. On the other, the fact that elections are held at regular intervals (Fearon 2011; Pop-Eleches and Robertson 2015; Wilson and Lindberg 2016; Harish and Little 2017; Knutsen, Nygård and Wig 2017) can increase authoritarian resiliency in non-election years because opposition leaders and activists who have invested in electoral strategies have fewer incentives to press for regime change, and the *anticipation* of future elections makes voters are less willing to take to the streets.

[1]Unless otherwise specified, throughout this paper I use the term "elections" to refer to *competitive* elections.

More specifically, authoritarian regimes can be divided into two classes. In *closed autocracies*, competitive elections do not exist, or are restricted to the legislature. Thus, the only way to remove the government is through mass protests, a coup, or an armed uprising. Most of the time, the lack of common knowledge (Chwe 2001) and the high cost of these activities will deter both elites and citizens from engaging in them. When economic conditions are sufficiently bad to precipitate such behavior, however, there is little incentive to back down because nobody knows when (if) there will be another opportunity to overthrow the government. In contrast, in *electoral authoritarian regimes* (henceforth, EARs), both the executive and the legislature are elected in competitive elections that are formally (though not substantively) democratic. This makes such regimes much more vulnerable in election years, because opposition leaders can campaign against the government and citizens who are wary of protesting may nonetheless be willing to cast a vote for them. At the same time, the anticipation of future elections may dissuade people from engaging in anti-government activities in non-election periods, increasing the regime's odds of surviving until the next election.[2]

The point is illustrated in Figure 1, which displays the probability of a regime *breakdown* – defined as the ousting of an authoritarian ruling coalition – on year t conditional on the economic growth rate in $t-1$ for a sample of 258 autocracies between 1948 and 2011. In panel (a) the tiles become lighter as we move upwards along the y-axis, indicating that, on average, better economic performance makes autocracies less likely to break down. Figure 1b shows that the pattern is similar for closed regimes and EARs, though the latter are somewhat more fragile. However, Figure 1c shows that this masks substantial variability within EARs: these regimes are highly vulnerable to bad economic performance in election years, but comparatively more resilient to it in non-election periods. EARs trade off higher vulnerability to economic performance in election years in exchange for higher resiliency to it in non-election periods.

[2] Thus, like Knutsen, Nygård and Wig (2017) I claim that elections can have contradictory effects on authoritarian breakdown, but the underlying mechanisms are different. First, while I agree that elections make autocracies more vulnerable to breakdown in the short term, I argue that this effect also depends on economic performance. Second, I make no claims about elections' effect in the *long run*; rather, I posit that the *anticipation* of future elections may help deflect anti-regime behavior in non-election periods, making a regime more likely to survive but only *until the next election*.

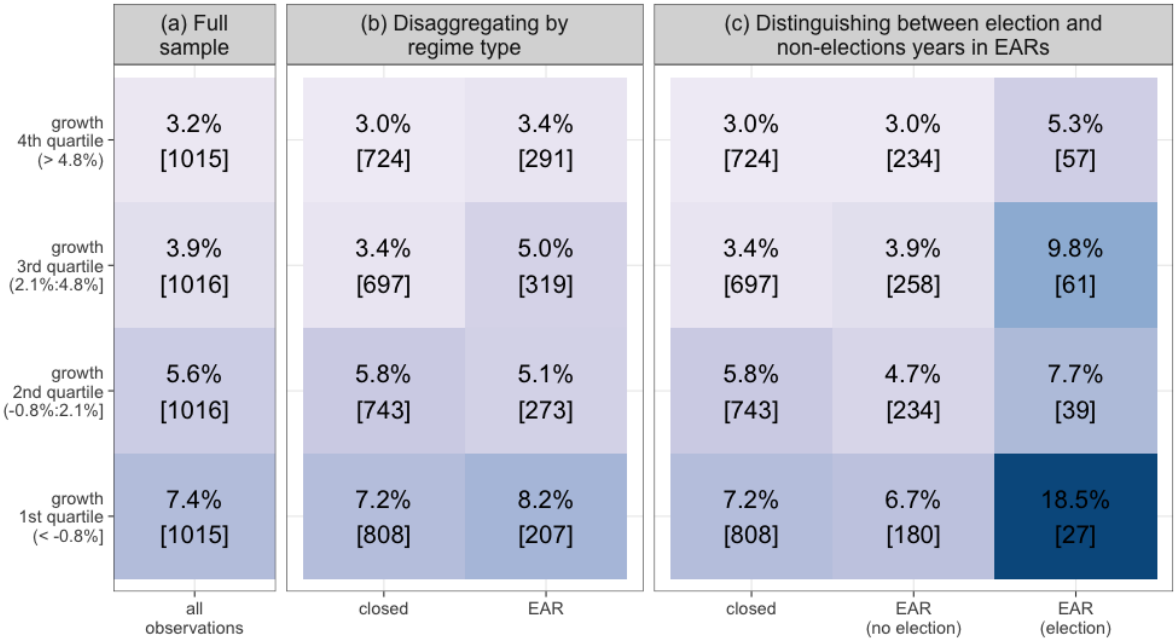


Figure 1: Likelihood of *breakdown*, conditional on economic performance (in the previous year), regime type and election year, 1948-2011. The shades indicate the probability of regime breakdown within each cell; for ease of comparison, the corresponding value is also reported numerically. Values in square brackets indicate the number of country-years in each cell.

The empirical analysis shows that this pattern holds within regimes as well. Furthermore, placebo tests reveal that the results disappear or become substantially weaker when the focus is restricted to non-competitive or legislative elections, vindicating the claim that it is *competitive* elections for the *executive* office that matter. Also in line with the argument, I find that (a) the effect is driven by outsider- rather than insider-driven breakdowns; (b) economic performance affects the likelihood of coup *attempts* and *violent* protests in election years; and (c) election-related breakdowns are more likely to result in democratization. While these results cannot be interpreted causally, their credibility is enhanced by the use of fixed effects – which wipe out a good deal of cross-sectional variation in the data – as well as the fact that election dates can often be taken as fixed (Pop-Eleches and Robertson 2015); indeed, results are stronger for *scheduled* than for *actual* actual election dates. Moreover, to the extent that authoritarian rulers manipulate the economy strategically (Akhmedov and Zhuravskaya 2004; Magaloni 2006; Pepinsky 2007; Blaydes 2010), economic performance should improve in election years, thus providing a tougher test for the argument.

Theoretical argument

I posit an election-driven cycle on the impact of economic performance on authoritarian breakdown, defined as the replacement of an authoritarian ruling coalition by a different set of rulers, whether democratically elected or not. Breakdown captures the inability of a small group of individuals to remain in power at the expense of others, and thus it stands at the heart of what authoritarian rule is all about. In contrast, events like coups, mass protests, leadership turnover or political liberalization do not necessarily compromise a regime's hold on power. Focusing on breakdown also makes more sense than looking at democratization (see also Knutsen, Nygård and Wig 2017:112). First, elections may end with a military coup (Wig and Rød 2016), as in Myanmar in 1990 (Guyot 1991) or Algeria in 1991 (Addi 1996). Second, whether alternation in office at t results in a democratic regime at $t+1$ depends on the behavior of the *upcoming* government: a candidate that defeats an autocrat at the polls and subsequently behaves undemocratically, as Alyaksandr Lukashenka did in Belarus in 1994 (Potocki 2002; Levitsky and Way 2010, ch. 3; Wilson 2011), establishes a new autocracy, not a democracy. Thirdly, minimizing the risk of one kind of transition may maximize the risk of another, and thus all forms of breakdown should be considered.

The argument begins with Svolik's (2012) distinction between internal and external threats to authoritarian governments. Internal threats are those that come from insiders within the ruling coalition, who may exploit their connections and resources to launch a coup, as Saddam Hussein did in Iraq in 1979 (Karsh and Rautsi 1991). External threats are those led by individuals from outside the ruling coalition, such as armed insurgencies (the Cuban Revolution), mass protests (Eastern Europe in 1989) or opposition candidates (the Philippines in 1986; see Thompson 1995). Elections make it easier for outsiders to organize and challenge the regime openly, thus making it more vulnerable to external threats. That said, to the extent that elections are (expected to be) sufficiently competitive, insiders may be less willing to close ranks behind the government, making it less likely to survive an electoral challenge. That is, elections both facilitate outsider coordination against the regime

(Fearon 2011; Little, Tucker and LaGatta 2015), and may make insiders more likely to shirk precisely when the government needs them the most (Rundlett and Svolik 2016). Economic performance matters because it affects both outsiders' capacity (and willingness) to challenge the regime at the polls, and insiders' determination to throw their weight behind it. The electoral calendar enters the picture by shaping beliefs on whether opportunities for coordination will be available in the future: if they will not, it makes little sense to stop collective action when the opportunity arises; otherwise, waiting until the next election may be more attractive than trying to topple the regime via extra-constitutional means (Schuler, Gueorguiev and Cantú 2017). I examine each of these claims in turn.

Elections and coordination. Competitive (executive) elections are risky for autocrats chiefly because they make it easier for outsiders to coordinate against the government. Holding competitive elections forces the government to lift some restrictions on opposition behavior, notably the right to form parties and appear on the ballot, which generally means that opposition leaders also get the opportunity to campaign and voice grievances that would otherwise remain silenced. Since autocracies often try to hide popular discontent behind a façade of unanimity (Kuran 1991), this is not a trivial concession, as it can make people aware of the true extent of popular dissatisfaction with the government. Even if the opposition has limited access to the media, the size of pro- or anti-government rallies can be informative about the regime's actual level of support (Cox 2009; Knutsen, Nygård and Wig 2017). And the winner-takes-all nature of executive elections may induce opposition parties to nominate a common candidate, facilitating voter coordination (Howard and Roessler 2006; Arriola 2012; Donno 2013).

In addition, the secret ballot makes it hard to monitor citizens' behavior. As Venezuela's *Maisanta* shows, even when opposition to the government is publicly voiced, the cost of identifying and punishing millions of individual citizens can be prohibitive (Hsieh et al. 2011).[3] If votes are cast secretly,

[3]There were three petitions calling for a referendum to remove Hugo Chávez from office. After surviving the referendum, Chávez published the signatories' names to have them punished, for example by firing them from government jobs. However, only those who signed the third petition could be easily identified; those who signed the first or second petition but not the third avoided punishment because they were much harder to identify (Hsieh et al. 2011).

such monitoring becomes even more difficult. The point is that the secret ballot lowers the incentives for preference falsification (Kuran 1991), and thus disgruntled citizens who are unwilling to participate in an anti-regime protest may nonetheless turn out to vote for the opposition.

Autocratic governments can cancel or manipulate elections, but neither strategy is without risks. The visibility of elections, coupled with the fact that they follow a pre-established calendar, severely limits autocrats' discretion to decide when (and whether) to hold them: "it is *the commonly understood convention of holding elections at particular times according to known rules*, not the electoral outcome itself, that provides a public signal for coordinating rebellion in the event that elections are suspended or blatantly rigged." (Fearon 2011:1676; original emphasis) For example, the protests and coup that ousted Blaise Compaoré after 27 years in power were triggered by his attempt to change the constitution in order to run for a fifth term.[4] The limitation is even starker for autocracies that depend on foreign aid, which often have little choice but to hold elections under conditions they cannot control (Dunning 2004; Finkel, Pérez-Liñán and Seligson 2007; Levitsky and Way 2010; Dietrich and Wright 2015; Miller forthcoming). The possibility that canceling a scheduled election may be interpreted as a signal of weakness (Egorov and Sonin 2014) further discourages such behavior.

Most autocracies thus prefer to hold elections and manipulate them. Even then, the visibility of elections may transform them into "focal points" for coordination if the regime gives the impression of being weak (Tucker 2007; Fearon 2011; Daxecker 2012; Little, Tucker and LaGatta 2015; Knutsen, Nygård and Wig 2017). If the ruling party wins a relatively clean election (Fearon 2011; Higashijima 2015; Rozenas 2016), or wins by a huge margin that signals widespread support (Magaloni 2006; Simpser 2013), its opponents are unlikely to challenge the results successfully because the government's strength is common knowledge (Chwe 2001): everybody believes that everybody believes that the regime is unassailable, thus dissuading people from taking to the streets in massive numbers. Yet this outcome depends on *perceptions* of who actually won the election, and thus may be affected by minor events that are hard to control and/or anticipate *ex ante* (Kuntz and Thomp-

[4] "Not so pretty now," *The Economist*, 8 November 2014.

son 2009:267-8). Some regimes miscalculate so badly that they have no contingency plan prepared in advance and thus are forced to announce unfavorable results (Higashijima 2015), as happened in Poland in 1989 (Kamiński 1999) or in Gambia in 2016.[5] Alternatively, fabricating false results at the last minute may be impossible either because the opposition reports its own tallies, or because government insiders are reluctant to collaborate. Both factors played a role in the Philippines in 1986, as the opposition had its own monitors, and officials at the electoral commission denounced Ferdinand Marcos' attempt to tamper with the vote count (Thompson 1995, ch. 8). Furthermore, to the extent that some amount of fraud is expected, announcing that the ruling party won by a small margin will send the signal that the government is weak (Egorov and Sonin 2014; Little 2015), thus encouraging anti-government protests, as in Serbia in 2000 (Krnjevic-Miskovic 2001; Birch 2002). The point is that fraud only works when it is either invisible (Simpser 2013; Fearon 2011) – hard to do in an autocracy unless elections are extremely clean, and thus risky (Magaloni 2010; Higashijima 2015; Rozenas 2016) – or so extensive that it signals that the regime is entirely in control and thus protesting is futile (Magaloni 2006; Simpser 2013; Little 2012, 2015; Egorov and Sonin 2014; Seki 2017). But a government that engages in (visibly) fraudulent behavior just to pass the 50 percent mark both offends dissatisfied voters and sends the signal that it is weak, thus triggering massive protests (Tucker 2007; Kuntz and Thompson 2009; Fearon 2011; Daxecker 2012; Little 2012; Little, Tucker and LaGatta 2015; Simpser 2013; Egorov and Sonin 2014).

Elections and insider support. Of course, not all such protests end in breakdown; some governments are able to withstand even massive shows of popular discontent, as the Mexican PRI did in 1988 (Magaloni 2006; Cantú 2017). However, this requires regime insiders to remain united behind the government, which may be problematic when election results are unfavorable. The point is that no government can remain in office without the active support of soldiers and civilian bureaucrats (Wintrobe 1998, ch. 13): even seemingly mundane actions such as stuffing ballot boxes require the active collaboration of hundreds or thousands of government officials, whose willingness and ability

[5] "A shock victory for the underdog in Gambia", *The Economist*, 10 December 2016.

to carry such a task cannot be taken for granted (Rundlett and Svolik 2016), as seen in the Philippines in 1986 (Thompson 1995, ch. 8) or in Mexico in 1988 (Cantú 2017). While these officials may prefer the government to survive, they will also be concerned about their fate in case the regime falls (Svolik 2012; Simpser 2013; Gehlbach and Simpser 2015), especially if they are asked to engage in illegal activities such as repressing protesters (Dragu and Polborn 2013) or tampering with the vote count (Rundlett and Svolik 2016). If lower-than-expected results send the signal that the government is weak, incentives to comply with such orders will be much lower; yet if these officials withdraw their support *en masse*, the regime will be vulnerable even to modestly-sized protests (Levitsky and Way 2010). High-level insiders who (secretly) want the incumbent to go may also take advantage of unfavorable electoral results to withdraw their support (Kuntz and Thompson 2009:260).

Competitive elections also give ambitious insiders the option of joining the opposition and openly challenging the regime at the polls. Although not all such defectors are serious electoral contenders, the political experience, name recognition, financial resources and political connections enjoyed by some of them makes for formidable electoral competitors (Reuter and Szakonyi 2017). Moreover, insiders will be more likely to defect when the government is seen as unpopular (Magaloni 2006; Reuter and Gandhi 2011; Reuter and Szakonyi 2017), i.e. it is precisely the most vulnerable regimes that should face the strongest electoral challenges from within.

Insiders may also choose to stage a coup, as recently seen in Zimbabwe.[6] Indeed, hardliners for whom the prospect of handing power over to the the opposition is unacceptable sometimes take power by force rather than stepping down peacefully (Casper and Tyson 2014; Wig and Rød 2016). Nonetheless, when electoral competition is allowed, insiders – and especially civilian ones – will find the option of defecting more attractive than that of staging a coup. Successful coups often require the support of (some sector of) the military (**?**, fn. 5), yet not all insiders have *entrée* to the top military brass. For the same reason, the military are likely to play a disproportionate role in the new government, making the option less attractive for civilian politicians. Finally, the cost of a

[6] "Fall of the dictator," *The Economist*, 18 November 2017.

failed coup is likely much higher than that of a failed electoral challenge. Unsuccessful coup plotters may be killed, imprisoned, or forced into exile. Defectors who lose an election, on the other hand, may certainly face harassment and lose access to lucrative government contracts, but they are likely to avoid harsher measures, especially if they attract international attention.

In sum, as Fidel Castro presciently warned his friend Daniel Ortega, "Elections are a risky business... If you get into the game, you should be prepared to lose." (quoted in Oppenheimer 1992:207) To be sure, the fact that authoritarian rulers may imprison, proscribe or kill their opponents, harass opposition leaders and supporters, monopolize access to the media, or engage in large-scale fraud means that they are more electorally "successful" than their democratic counterparts: while 29.6% of competitive elections held between 1788 and 2008 (660 out of 2,230) resulted in an electoral defeat for the incumbent (Przeworski 2015:104-5), just 12.7% of competitive executive elections held in autocracies between 1948 and 2011 (34 out of 268) ended in breakdown. Yet this represents an almost threefold increase over the 4.5% failure rate of authoritarian regimes in non-election years (170 our of 3,793). Authoritarian elections may be safer than democratic ones, but they are much riskier than not holding elections at all.

The role of the economy. Economic performance enters into this picture by affecting the level of dissatisfaction with the government among both outsiders and insiders. It is well documented that economic growth is closely related to popular support for the government, both in democracies (Duch and Stevenson 2008; Burke 2012) and in autocracies (Magaloni 2006, ch.5; Treisman 2011; Burke 2012; Matovski 2016). If economic conditions are dire enough, this may suffice to induce coordination against the government (Fearon 2011); indeed, bad economic performance makes protests and riots more likely (Brancati 2016; Aidt and Leon 2016), which sometimes induce the adoption of significant institutional reforms (Burke and Leigh 2010; Teorell 2010; Brückner and Ciccone 2011; Ramsay 2011; Wright, Frantz and Geddes 2015; Rozenas 2016; though see Barron, Miguel and Satyanath 2014 for a discordant view). The fact that competitive elections facilitate coordination should strengthen this result, as the government has fewer resources to buy off potential opponents

(Greene 2007; Kennedy 2010; Treisman 2015), while opposition candidates have an obvious campaign topic that resonates with voters.[7] An ailing economy also makes it harder to retain the loyalty of regime insiders, either because economic downturns often force the introduction of political or institutional reforms that make (some of) them worse off (Haggard and Kaufman 1995; Bueno de Mesquita et al. 2003), or because opportunistic insiders believe the government is less likely to remain in office. Consistent with these claims, both coup attempts (Londregan and Poole 1990; Galetovic and Sanhueza 2000; Kim 2016; though see also Powell 2012) and insider defections (Reuter and Gandhi 2011; Reuter and Szakonyi 2017) are more common when the economy is doing badly.

The electoral calendar. These considerations suggest an interactive relationship between elections and the economy: an economic downturn of a given magnitude will be much more serious in an election year, when coordination is easier, than in a non-election period. Conversely, a booming economy will make elections less dangerous, as genuine support for the regime will be higher, expectations of victory will be stronger, and the government will have more resources to buy off the support of voters and regime insiders.

But elections also matter because of the expectations they generate. Closed regimes lack not only (competitive) elections but also the *expectation* of future elections. This certainly makes coordination more difficult for both insiders and outsiders; but when (if) these players manage to solve their coordination problems, they have little reason to back down, as they cannot know when (if) they will be able to coordinate in the future. In contrast, EARs hold elections at regular and known intervals (Fearon 2011; Pop-Eleches and Robertson 2015), thus generating a common understanding that *future* elections will provide a venue for opposition leaders to challenge the regime, for citizens to vote against it, and for disgruntled insiders to defect and join the opposition. Thus, when a downturn takes place in a non-election year, both voters and opposition leaders have good reasons to wait until the next election rather than engaging in risky behavior like protesting or planning a

[7] Unlike other phenomena that make incumbents look bad, such as corruption scandals, bad economic performance is hard to conceal from voters even when the media is aligned with the government.

coup (see also Harish and Little 2017 and Schuler, Gueorguiev and Cantú 2017). The perception that the success of protests and conspiracies depends on how many people participate in them (Chwe 2001) will further discourage such behavior (Kuntz and Thompson 2009:263-6).

That said, the fact that EARs make it easier for the opposition to organize may facilitate protests and other anti-regime activities, and the knowledge that economic downturns are often transitory may diminish the incentives to wait. Nonetheless, to the extent that protesting is sufficiently costly, voters see elections as inherently legitimate, or opposition leaders had already invested in electoral technologies (Acemoglu and Robinson 2005; Schuler, Gueorguiev and Cantú 2017), these arguments carry less weight. For a start, voters may be fickler than opposition leaders (Fearon 2011; Matovski 2016; Hale and Colton 2017): while the latter may want the regime to go as a matter of course, the former may be less worried about temporary downturns – which may reflect bad luck rather than incompetence – than permanente ones. True, opposition leaders may still find it worthwhile to protest in order to demand an impartial electoral commission or more favorable electoral rules, while high-ranking insiders may defect in non-election periods in order to prepare for the upcoming election. This will make future elections more competitive, but that only strengthens the logic of the argument. To begin with, a breakdown may not occur: by the time the election arrives, the government may have recovered (for example, because the economy has taken off), or the opposition may be extremely fragmented (e.g., if all opposition leaders want to become the new president). Paul Biya barely prevailed in the 1992 Cameroonian presidential election (Arriola 2012, ch. 7), but became unassailable afterwards. Malaysian elections have become more competitive over time, to the extent that the opposition won a majority of votes in 2013; yet malapportionment still gives the ruling party an absolute majority of parliamentary seats (Noh 2014). Even more importantly, if a breakdown effectively occurs, it will take place *at the moment of the election*, not before.

Implications. These considerations suggest three sets of implications. First, consider how economic growth affects the probability of authoritarian breakdown. Compared to closed regimes, which by definition never hold competitive (executive) elections, EARs should be more vulnerable to bad

economic performance in election years, but relatively more resilient to them in non-election periods.[8] Conversely, a booming economic should mitigate the risk of holding competitive elections, though of course this effect can only be studied in EARs.

Second, the argument only applies to *competitive* elections for selecting the incumbent *executive*. Campaigning against the government only makes sense if opposition is allowed, and the announcement of electoral results can only be problematic when losing is theoretically possible. Indeed, only minimally competitive elections have ever resulted in an incumbent defeat (Hyde and Marinov 2012). In the same vein, although they sometimes trigger an authoritarian breakdown – as in Georgia in 2003 or Kyrgyzstan in 2005 (Bunce and Wolchik 2011) –, legislative contests should be less relevant than executive ones (Bernhard, Edgell and Lindberg 2016; Knutsen, Nygård and Wig 2017). Most obviously, breakdown means that the government loses control of the executive office. An opposition-controlled legislature may be able to impeach the incumbent in principle, but this often requires controlling a supermajority of seats, and in any case the police and bureaucracy may have few incentives to obey, as recently seen in Venezuela.[9] Even when losing control of the legislature, the executive may be able to buy off individual legislators or divide the opposition caucus. Concentration of power in the executive also implies that both the stakes of office and clarity of responsibility for economic performance should be higher in executive contests (Powell 2000; Duch and Stevenson 2008). Moreover, the results of legislative elections tend to be noisier than those of executive contests: nationally aggregated data on vote shares is unavailable for many countries, and many EARs have majoritarian electoral systems that produce large distortions in the translation of votes into seats (Higashijima and Chang 2015). Nonetheless, these considerations suggest a "placebo test" for the argument: the impact of growth and elections on breakdown should be limited to com-

[8] The argument cannot say whether closed regimes should be less sensitive to economic performance than EARs *on average* because fixed regime characteristics may affect both a regime's propensity to hold competitive elections and its vulnerability to them (see Harish and Little 2017 for a similar claim applied to election-related violence).

[9] "Fighting their chains," *The Economist*, 29 October 2016.

petitive executive elections; the effect should disappear – or become substantially weaker – when the analysis is restricted to purely legislative or non-competitive executive elections.

Thirdly, while bad economic performance may affect authoritarian breakdown in multiple ways – e.g., by inducing a coup among insiders or by triggering mass protests from outsiders –, election-related breakdowns should be more likely to be driven by outsiders,[10] either directly through an electoral defeat or indirectly via mass protests after a fraudulent election. Furthermore, to the extent that these outsiders won their right to govern at the ballot box, the new government is more likely to qualify as democratic, at least in the short run (see also Brownlee 2009; Teorell 2010; Knutsen, Nygård and Wig 2017; Schuler, Gueorguiev and Cantú 2017). Note, however, that the argument does not suggest any systematic relationship between economic performance and the observed level of electoral manipulation, as the relationship can go both ways. A regime that is electorally weak (e.g., if the economy is doing badly) may be less willing to manipulate in order to produce more informative results (Rozenas 2016) but also less *able* to do it if insiders are dissatisfied or unsure about its electoral prospects. At the same time, when a relatively small amount of fraud may suffice to win the election, the temptation to manipulate is stronger. On the other hand, a strong regime is more able to manipulate in order to signal its strength (Simpser 2013), but has less need to do so, especially if a booming economy increases its popularity and discourages insiders from defecting.

Data and methods

Authoritarian breakdown. I examine these claims on a sample of 258 authoritarian regimes between 1948 and 2011.[11] The unit of observation is the (authoritarian) country-year. The main outcome of interest is $breakdown_{r,t}$, a dummy that takes the value of 1 if regime r broke down before December 31 of year t, and 0 otherwise. Following Geddes, Frantz and Wright (2014; henceforth

[10] I thank an anonymous reviewer for suggesting this possibility.

[11] Online appendix B lists all regimes included in the analysis. Note that the handful of regimes that appeared after January 1st of a given year and broke down before December 31 are excluded from the sample.

GWF), I define a regime breakdown as a change in the "set of formal and/or informal rules for choosing leaders and policies."[12] In practice, this means that the incumbent executive and his ruling coalition were replaced by a different set of rulers, and thus it captures the main theoretical concept of interest, which is an autocracy's (in)capacity to remain in power (see Hollyer, Rosendorff and Vreeland 2015; Wright, Frantz and Geddes 2015; Knutsen, Nygård and Wig 2017 and Schuler, Gueorguiev and Cantú 2017 for a similar approach).[13] In contrast, other measures of regime type – such as Polity (Marshall, Gurr and Jaggers 2014), Freedom House, V-Dem (Coppedge et al. 2017) or the Democracy and Dictatorship (DD) dataset (Cheibub, Gandhi and Vreeland 2010) – either ignore transitions from one authoritarian regime to another, or identify instances of regime change even when there was no turnover at the top (Wright, Frantz and Geddes 2015:290-2). The replacement of the Shah by Ayatollah Khomeini does not show up in DD because both regimes were authoritarian, while the introduction of multiparty elections in several African countries in the 1990s improved their Polity scores (Bratton and van de Walle 1997) even though most incumbents remained in office. GWF's dataset also accounts for the possibility that a regime may be neither democratic nor authoritarian, for example if no group controlled most of the country's territory, or if there was a provisional government in charge of organizing transitional elections. Excluding such cases is important because the argument only applies when an authoritarian regime effectively governs and has some aspiration to endure. Moreover, since provisional governments rarely last more than two years and step down following an election, classifying them as autocracies would overstate the effect of elections on breakdown.

The main concern with GWF's data is that a substantial share of regime breakdowns are coded as occurring in election days, thus potentially introducing a mechanical correlation between elections and breakdown (see Schuler, Gueorguiev and Cantú 2017). In practice, however, this is less worrisome than it seems at first sight. On the one hand, the removal of an authoritarian ruling coalition

[12] I extended GWF's data until 2015 and made a few changes in their coding; see Online appendix A for details.
[13] Foreign occupation, a country's breakup, or loss of effective control over its territory also count as breakdown.

is a prerequisite for a transition to occur, and elections matter precisely because they make such replacement more likely. On the other, coding breakdowns on election days is most problematic when an election signals the end of a regime that had been decided beforehand. For example, GWF code the end of the Uruguayan dictatorship on 25 November 1984, when the military organized competitive presidential elections as a means of devolving power to civilians. Attributing this breakdown to the election would bias the results because the military had indicated its willingness to return to the barracks well before election day, and indeed no member of the government was running for the presidency. To avoid this problem, I only include elections in which a high-ranking regime insider – either the incumbent or a designated successor – was running for office. That is, Uruguay in 1984 is coded as an instance of breakdown, but the corresponding country-year is coded as having no election. Similarly, when the incumbent executive was effectively deprived of his powers before the election (as in several African countries in the 1990s; see Bratton and van de Walle 1997), the breakdown is coded as having taken place before the election. My list of election-driven breakdowns is thus very similar to that of Schuler, Gueorguiev and Cantú (2017; see Table A2), though I take a less aggressive approach at pre-dating breakdowns preceded by institutional reforms when the ruling party participated in the election.[14]

To shed some light on the mechanisms through which elections and the economy affect authoritarian breakdown, I look at ten additional outcomes. All of them are dummies that take the value of 1 when the event of interest (i.e., a coup attempt) occurred in a given country-year, and 0 otherwise. First, I rely on GWF's coding of how a regime ended to distinguish between different *sources* of breakdown. Specifically, I discriminate between breakdowns driven by *insiders* (i.e., a coup); those driven by *outsiders* (a category that includes mass protests as well as electoral defeats); and cases in which the authorities stepped down *voluntarily* (as in Uruguay in 1984).[15] Second, I iden-

[14] Tracing breakdown dates back to the adoption of institutional reforms that made elections cleaner, as Schuler, Gueorguiev and Cantú (2017) do, is problematic because it ignores regimes that introduced similar reforms and survived. In any case, Table A24 shows that the results are robust to re-coding these cases.

[15] I ignore foreign invasions and instances of state disappearance. Note that GWF code cases in which the government tried to manipulate the elections but stepped down following mass protests as protest-driven breakdowns.

tify anti-government protests with data from the Social, Political, and Economic Event Database (SPEED), which compiles information from thousands of news sources about politically relevant events – from statements and demonstrations to (attempted) assassinations and acts of repression – between 1946 and 2005 (Nardulli, Althaus and Hayes 2014).[16] Specifically, $protest_{r,t}$ takes the value of 1 if in regime r during year t there was at least one demonstration, strike or riot organized by a non-governmental actor.[17] Following **?**, *violent protest*$_{r,t}$ indicates if there was at least one protest in which a weapon was used. Third, to see whether the results are driven by insiders' incentives to conspire, I use data from Powell and Thyne (2011) to look at coup attempts as well as successful and unsuccessful coups.[18] Finally, to determine whether election-driven breakdowns are more likely to result in democratization, I look at whether GWF code the regime in place at $t+1$ as another autocracy, or as a democracy or provisional government.

Explanatory variables. The argument posits that authoritarian breakdown is a function of three factors. I measure short-term economic performance with $growth_{r,t-1}$, the country's lagged change in per capita income. I constructed this variable with data from the Maddison Project.[19] I employ the lagged rather than the contemporaneous growth rate because an ailing economy may be the consequence of regime breakdown rather than its cause.

The distinction between EARs and closed regimes[20] is based on whether the executive and the legislature have been elected in formally competitive elections (as in Mexico under the PRI or Zim-

[16] http://www.clinecenter.illinois.edu/data/event/speed/. I thank an anonymous reviewer for bringing this dataset to my attention.

[17] I first coded whether an event was a "demonstration" (PE_TYPE = 7), a "strike" (PE_TYPE = 8) or a "riot or brawl" (ATK_TYPE = 1), and then restricted the sample to those that were initiated by non-governmental actors (INI_TYPE = 1). Note that **?** employ a much loser definition of "protest" that also includes verbal attacks against the government and the formation of opposition organizations. I ignored protests that occurred after a breakdown. I use a dummy rather than the total number of protests in a given year because protests that are repeated over several consecutive weeks or days should be coded as a single event.

[18] Naturally, I only include coups that took place before a breakdown.

[19] http://www.ggdc.net/maddison/maddison-project/home.htm, 2013 version.

[20] Specific definitions vary, but the conceptual distinction between closed regimes and EARs is common (see Howard and Roessler 2006; Schedler 2006, 2013; Brownlee 2009; Gandhi and Lust-Okar 2009; Levitsky and Way 2010; Magaloni and Kricheli 2010; Morse 2012; Svolik 2012; Mainwaring and Pérez-Liñán 2014; Knutsen, Nygård and Wig 2017).

babwe under Mugabe) or not, either because there are no elections at all (China, Saudi Arabia), because elections are not formally competitive (the Soviet Union), or because only the legislature is competitively elected (Jordan).[21] Thus, $EAR_{r,t}$ is a dummy that takes the value of 1 if at the beginning of year t, regime r had an executive and a legislature that had been elected in formally competitive elections.[22] As explained in Appendix D, I coded this variable by crossing data from executives and legislatures from Cheibub, Gandhi and Vreeland (2010) with the National Elections Across Democracy and Autocracy dataset (Hyde and Marinov 2012; henceforth NELDA), which provides a list of national-level elections around the world between 1946 and 2012. I also relied on NELDA to create $election_{r,t}$, a dummy that indicates if regime r held a competitive executive election in year t, the election took place before a breakdown, *and* (a) some member of the outgoing government was running for office, or (b) the outgoing government clearly backed one of the candidates. Following Hyde and Marinov (2012), I classified an election as (minimally) competitive if (a) there existed at least one non-government group that might have participated in the election; (b) opposition parties were legally allowed; and (c) there was a choice of candidates in the ballot.[23]

A potential concern with election dates is that they may be endogenous (Knutsen, Nygård and Wig 2017): elections in parliamentary regimes are often held ahead of schedule, and unelected autocrats are sometimes forced to hold elections under pressure from popular protests (Kim 2017) or international donors (Dietrich and Wright 2015; Miller forthcoming). Thus, in some specifications I look at *scheduled* rather than actual elections (Pop-Eleches and Robertson 2015), coding an observation as 1 if an election was scheduled to take place on year t at the beginning of the year, even if

[21] Two other regime types are possible. In *indirect* autocracies, formal democratic institutions coexist with an unelected body that holds effective political power, e.g. the military (Guatemala 1970-85) or a religious council (Iran after 1979). *Competitive oligarchies* hold multiparty elections but restrict the suffrage to a small subset of the population (South Africa until 1994). I ignore these regimes (just 3.8% of authoritarian country-years) because the logic of the argument does not apply to them: in the former elections do not determine the effective allocation of political power, while in the latter most citizens are disenfranchised. In any case, including them does not change the results (see Table A29).

[22] A regime may qualify as closed and EAR at different moments during its lifetime – typically when a closed autocracy introduces elections and wins them, as in Africa in the 1990s (Bratton and van de Walle 1997). Such regimes are coded as electoral authoritarian (closed) since the year *after* the introduction (cancellation) of competitive elections.

[23] I changed their coding in a handful of instances, e.g. if there was a single candidate because of an opposition boycott.

the election eventually did not take place. This both excludes elections that are held on the basis of short-term political considerations,[24] and allows for the possibility that a regime's demise may be triggered by an attempt to cancel or postpone a scheduled election. Specifically, I distinguish between three cases: (a) no competitive (executive) election was scheduled to take place at any time in the future; (b) a competitive (executive) election was scheduled to take place in a future year; or (c) a competitive (executive) election was scheduled to take place during the year. Case (a) corresponds to closed regimes, while cases (b) and (c) correspond to EARs. To determine the date of scheduled elections, I looked at the date in which the executive's term was set to end, as even authoritarian executives are elected for a fixed (or maximum) term (Fearon 2011; Baturo 2014). For example, if an incumbent was elected for a five-year term at t, the next election is scheduled to take place at $t+5$.[25]

Specification. I estimate linear probability models[26] of the form

$$breakdown_{r,t} = f\left(growth_{r,t-1},\, election_{r,t},\, EAR_{r,t}\right) + \theta \cdot \mathbf{C}_{r,t} + \mu_r + \delta_t + \sum_{d=1}^{3} \gamma_d \cdot duration_{r,t}^{d} + \varepsilon_{r,t},$$

where $f(\cdot)$ is some function of the explanatory variables of interest (for example, an interaction between the three), $\mathbf{C}_{r,t}$ is a vector of time-varying controls, μ_r and δ_t are regime and year fixed effects, and the γ_d's are the coefficients for a cubic duration trend. I include regime fixed effects to account for both country-specific characteristics as well as for factors that remain constant over a regime's lifetime – like its founding episode (Albertus and Menaldo 2012), or whether the regime is military or personalist (Geddes, Wright and Frantz 2014) – or change little within regimes – e.g., its

[24] Early elections are often announced in the same year in which they occur, and thus are often coded as non-scheduled.

[25] When applicable, I accounted for constitutional changes that lengthened an incumbent's term. NELDA does not provide information on term lengths, so I relied on the Nohlen handbooks (Nohlen, Krennerich and Thibaut 1999; Nohlen, Grotz and Hartmann 2001*a*,*b*; Nohlen 2005*a*,*b*; Nohlen and Stover 2010) and other country sources.

[26] I estimate linear probability models instead of survival models because the latter cannot account for regime-specific characteristics: frailty models require some clusters to experience multiple events (Box-Steffensmeier, Boef and Joyce 2007) but regimes can break down only once. Generalized linear models also behave poorly when an irrelevant variable is included (or a relevant one is excluded), even if that variable is uncorrelated with the explanatory variable(s) of interest (Angrist and Pischke 2009). Moreover, when the goal is to estimate marginal effects, LPMs and logit/probit produce similar results (Angrist and Pischke 2009:102-7).

constitutional structure (see Roberts 2015). This ensures that the results will be driven by variation in elections and growth rates *within* regimes rather than between them. That is, while institutions (Pepinsky 2014) or average growth rates may be endogenous to regime type, the fixed effects ensure that only variation over the regime's baseline will be taken into account. The year dummies account for world trends that are common to all regimes, such as the end of the Cold War. The duration trends account for the possibility that younger regimes may differ systematically from older ones (Carter and Signorino 2010). In some specifications I also control for factors that may affect both the decision to hold elections and the probability of breakdown: *GDP per capita*$_{r,t-1}$ (from Maddison), the availability of natural resources (*oil and gas per capita*$_{r,t-1}$, from Ross and Mahdavi 2015), and the *proportion of democratic neighbors*$_{r,t}$ in a country's region (excluding the country itself).[27] I report robust standard errors clustered by regime.

Results

Figure 1 shows that the relationship of interest is present in the data: EARs are more vulnerable to breakdown in election years, and the effect is specially marked when the economy is doing badly. However, this relationship may be spurious. For example, both EARs and breakdowns may be more common after the Cold War, or EARs may grow more slowly than closed regimes. Thus, in this section I examine whether the relationship holds within individual regimes and after accounting for worldwide trends.

[27] I distinguish between eight regions: East Asia; Former Communist; Latin America; MENA; South Asia; South-East Asia and the Pacific; Sub-Saharan Africa; and Western Europe plus former British settler colonies (see Miller 2015).

Table 1: Elections, economic performance and authoritarian breakdown, 1948-2011

		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.15		-0.12	-0.12	-0.13		-0.11	-0.12	-0.16	-0.17	-0.14	-0.11
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
$election_t$		0.07	0.09	0.10	0.10	0.10	0.12	0.12	-0.01	0.02	0.01	0.04
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.74	-0.31	0.03		-1.15	-0.93	0.41	-0.27	-0.06	-0.41
			(0.36)	(0.76)	(0.83)		(0.43)	(0.45)	(0.58)	(0.37)	(0.19)	(0.27)
$growth_{t-1} \times EAR_t$				-0.01	0.05				-0.01		-0.11	
				(0.17)	(0.17)				(0.17)		(0.18)	
$growth_{t-1} \times election_t$				-0.66	-0.90				-0.22		0.38	
$\times EAR_t$				(0.90)	(0.97)				(0.59)		(0.35)	
EAR_t				0.01	0.02				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.01	-0.02				-0.03		-0.04	
				(0.06)	(0.06)				(0.05)		(0.03)	
$election\ (other\ year)_t$						-0.00	-0.01	-0.00		-0.01		0.00
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.08	0.12		0.18		0.08
$\times\ election\ (other\ year)_t$							(0.14)	(0.15)		(0.15)		(0.14)
Marginal effect of growth_{t-1} *on* $\Pr(\text{breakdown}_t = 1)$												
no election (closed)			-0.12	-0.12	-0.13		-0.11	-0.12	-0.16	-0.17	-0.14	-0.11
			(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
no election (EAR)			-0.12	-0.13	-0.07		-0.04	0.00	-0.17	0.00	-0.24	-0.03
			(0.06)	(0.16)	(0.16)		(0.13)	(0.13)	(0.16)	(0.14)	(0.17)	(0.13)
election (closed)			-0.86	-0.43	-0.10				0.24		-0.19	
			(0.36)	(0.76)	(0.83)				(0.58)		(0.19)	
election (EAR)			-0.86	-1.09	-0.95		-1.27	-1.05	0.01	-0.44	0.08	-0.52
			(0.36)	(0.48)	(0.49)		(0.43)	(0.45)	(0.09)	(0.36)	(0.26)	(0.27)
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

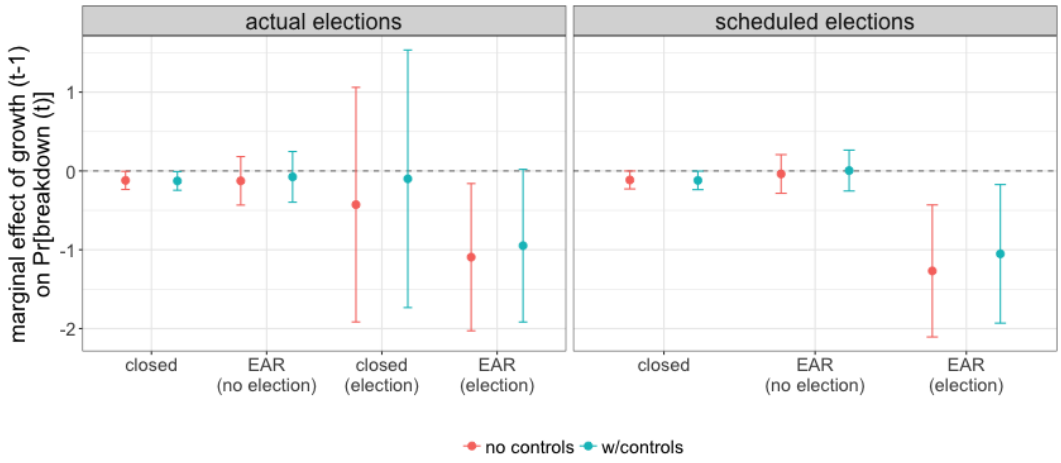
OLS regression estimates. The dependent variable is $breakdown_t$. Robust standard errors clustered by regime in parentheses. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Models 5 and 8 control for $GDP\ per\ capita_{t-1}$ (log), $oil\ and\ gas\ per\ capita_{t-1}$ (log) and $proportion\ of\ democratic\ neighbors_t$, while model 12 includes a dummy and interaction term for years that featured both an executive *and* a legislative election; see Table A6 for the full set of coefficients.

Main results. The first model of Table 1 shows that better economic performance makes breakdowns less likely, though the effect is modest in magnitude: the point estimate of −0.15 (SE: 0.06) implies that a 5 percentage point increase in *growth* at $t-1$ (equivalent to the within-regime standard deviation in the data; see Table A5) reduces the probability of breakdown at t by just 0.77 percentage points. Although far from irrelevant (the unconditional probability of breakdown in the data is 5%; see Table A5), the effect is much smaller than simply holding a competitive election, which increases the probability of breakdown by between 7 (SE: 2) and 10 (SE: 3) percentage points, depending on whether we consider actual elections (model 2) or scheduled ones (model 6).

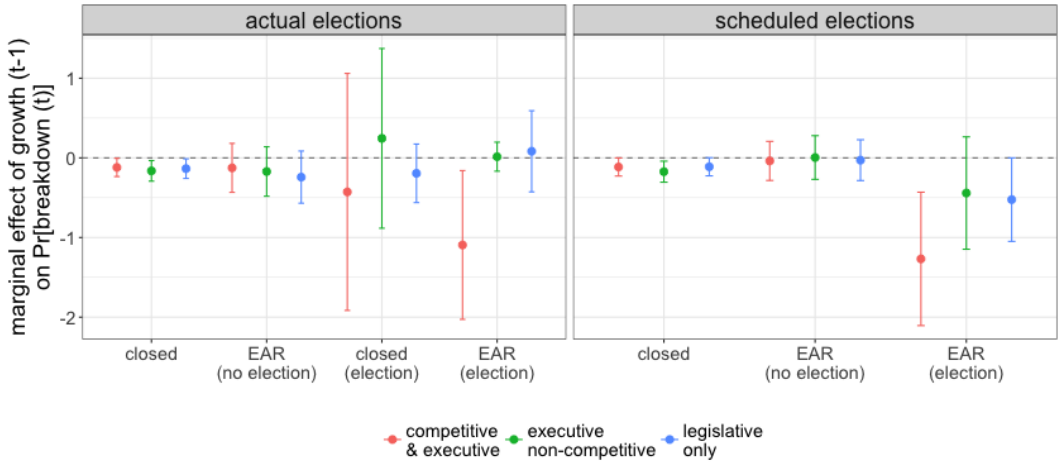
Model 3 confirms that the economy matters more in election years. The estimate of −0.12 (SE: 0.06) for $growth_{t-1}$ means that in a year without elections, increasing the growth rate by 5 percentage points changes the probability of breakdown by $-0.12 \times 5 \approx -0.6$ pp., whereas in a non-election year a similar change would have an effect of $(-0.12-0.74) \times 5 \approx -4.3$ pp. The reverse is also true: holding an election increases the probability of breakdown by 9 (SE: 2) percentage points when the growth rate is zero, but the effect is cut to 5.3 pp. if the economy is growing at 5% per year. Elections are a risky venture, but high growth rates make them safer.

A potential problem with these results is that they conflate EARs and closed regimes: the former hold elections regularly, while the latter typically do it only under pressure (Kim 2017), i.e. when they are weak and thus more likely to lose (see also Rozenas 2016). Thus, model 4 includes a triple interaction term between growth, elections and authoritarian regime type. To facilitate interpretation of the results, the bottom of Table 1 shows the marginal effect of $growth_{t-1}$ on $breakdown_t$ (a) in non-election years in closed regimes; (b) in non-election years in EARs; (c) in election years in closed regimes; and (d) in election years in EARs. The marginal effect of growth in election years is more than twice as large in EARs than in closed regimes (−1.09, SE: 0.48 vs. −0.43, SE: 0.76). These estimates are not statistically different from one another (see also the left panel of Figure 2a), but there is no theoretical reason why they should be; rather, the goal of the exercise is to demonstrate

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

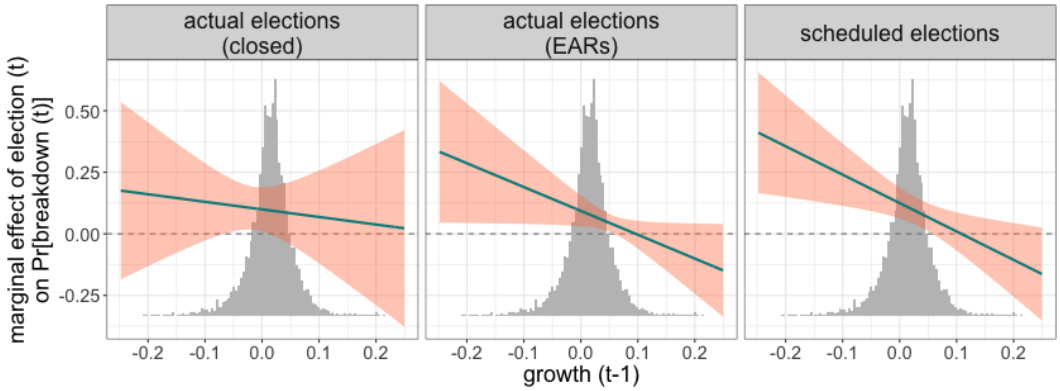


Figure 2: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table 1. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

that the results in model 3 are not an artifact of closed regimes holding competitive elections under unfavorable circumstances.[28]

Conversely, higher growth rates make elections safer. For example, the middle panel of Figure 2c indicates that under average growth rates (1-2% per year; see Table A5), elections increase the probability of breakdown by 7-8 pp. This makes sense, as elections are especially good at facilitating coordination among an aggrieved populace (Fearon 2011), and such grievances may have multiple sources besides economic performance – such as corruption (Tucker 2007) or an infelicitous statement during the campaign. Even then, a booming economy clearly helps the government: for growth rates of 6 pp. a year of higher, the effect of elections on breakdown is not longer statistically significant at the 0.05 level (see Figure 2c). As for non-election years, the effect of economic performance remains negative and statistically significant in closed regimes, and similar in magnitude but imprecisely estimated in EARs. Again, these estimates are not statistically different from one another, but the fact that growth only matters in non-election years in closed regimes is consistent with the argument.[29] Model 5 shows that accounting for GDP per capita, natural resources, and the proportion of democracies in a country's region does not change the gist of the results, save for the marginal effect of growth in election years in closed regimes.[30] Among the control themselves, Table A6 shows that increasing the *proportion of democratic neighbors*$_t$ makes breakdowns more likely, but income and natural resources have no independent effect on the outcome.

Model 7 distinguishes between autocracies in which no election was scheduled at any time in the future (closed regimes), those in which an election was scheduled for a different year (EAR, non-election year), and those in which an election was scheduled to take place in that same year (EAR,

[28] The theoretical argument assumes that there only two kinds of autocracies: EARs, in which elections are expected as a matter of course; and closed regimes, in which (competitive) elections are never held. Reality is more complicated, however, because closed regimes sometimes hold competitive elections for the first time (Kim 2017). While I make no theoretical claim about the consequences of these elections, it would be worrying to find that the conditional effect of economic growth on breakdown is entirely driven by such founding elections.

[29] Since the effect of $growth_{t-1}$ in non-election years in closed regimes is so small (−0.12, SE: 0.06), distinguishing it from an effect that were *estimated* (rather than *assumed*) to be zero would be almost impossible.

[30] This is a pure "sample effect:" restricting the analysis to observations with data on natural resources but not including the variable in the equation produces almost identical estimates and standard errors (see Table A20).

election year). This only increases the strength of the results: the effect of $growth_{t-1}$ on regimes with no scheduled elections remains unchanged (−0.11, SE: 0.06, t-score: −1.97), but is essentially zero when an election is scheduled for another year (−0.04, SE: 0.13). Again, these two estimates are not statistically distinguishable from one another, thought their relative magnitude is consistent with the argument, and distinguishing between them would be hard anyway (see fn. 29). When an election is scheduled to take place at t, in contrast, the marginal effect of $growth_{t-1}$ goes up by an order of magnitude (−1.27, SE: 0.43), implying that an increase in the growth rate of 5 percentage points decreases the probability of breakdown by a whopping 6.35 pp. Moreover, Figure 2a shows that the confidence intervals for the marginal effect of $growth_{t-1}$ on election years in EARs do not overlap with those from closed regimes, implying that the estimates are significantly different from one another. Looking at the relationship between growth and elections the other way round, Figure 2c confirms that the impact of elections also depends on the economy: simply holding an election increases the risk of breakdown by 10-12 pp. when the economy is barely growing, and can be disastrous under a serious recession – if the economy contracts by 10 pp. at $t-1$, the model predicts that holding an election will increase the probability of breakdown by around 25 pp. Conversely, if the economy expands by 6 pp. per year or more, holding an election no longer has a statistically significant effect on the probability of breakdown. Autocrats trying to engineer electoral business cycles (Akhmedov and Zhuravskaya 2004; Magaloni 2006; Pepinsky 2007; Blaydes 2010) clearly know what they are doing. Model 8 confirms that adding controls does not change the results.

Placebo tests. The last four columns of Table 1 examine whether these effect are being driven by *competitive* elections for an *executive* office. Models 9 and 10 look at non-competitive executive elections only.[31] Higher growth rates still reduce the likelihood of breakdown in non-election years, but the interaction between growth and elections disappears completely or becomes much smaller in magnitude, corroborating the claim that non-competitive elections have no short-term effects on authoritarian survival. The point is especially evident in Figure 2b, where the contrast between com-

[31] That is, $election_t$ takes the value of 1 in years with (scheduled) non-competitive executive elections, and 0 otherwise.

petitive and non-competitive elections could not be starker. Models 11 and 12 show a very similar story for years with competitive *legislative* elections but no competitive *executive* ones.[32] The estimate for scheduled (competitive) legislative-only elections is close to being statistically significant, but Figure 2b shows that its 95% confidence interval does not include the point estimate for the marginal effect of $growth_{t-1}$ in (competitive) executive elections. Georgia and Kyrgyzstan notwithstanding, authoritarian regimes are most vulnerable when the *executive* office is contested; taken by themselves, competitive legislative elections do not matter that much.

Robustness. These results are robust to a wide variety of sample and specification changes. To begin with, closed regimes that hold single-party elections may be stronger than those that do not hold elections at all (Cox 2009). Thus, Table A7 and Figure A1 in Online appendix F distinguish between closed regimes that do not hold executive elections at all, closed regimes that hold single-party executive elections only, and EARs. The first two are indistinguishable from one another; the results are clearly driven by EARs.

Tables A19 to A23 show that controlling for the logarithm of *GDP per capita*$_{t-1}$, *oil and gas per capita*$_{t-1}$ or the *proportion of democratic neighbors*$_t$, either jointly or separately, has no impact on the results. The last variable has a positive effect on the outcome, as expected, but it does not affect the rest of the estimates. The null results for *GDP per capita*$_{t-1}$ (log) are not that surprising considering that the regime effects already account for large differences in levels between countries – e.g., between Singapore and Zambia. What remains is variation within regimes, which is likely correlated with growth rates anyway (see Acemoglu et al. 2008; Treisman 2015 for similar null results in fixed-effects specifications). Natural resources do vary somewhat within countries, but if elections follow a pre-established calendar, natural resource shocks are unlikely to affect specific election dates.

Coding a regime's end as the moment in which the first move toward liberalization begins results in somewhat weaker but substantively identical estimates (Table A24). Survival models with either

[32] Model 12 in Table A6 shows that in years with both executive and legislative elections, the results are very similar to those of model 7, further vindicating this claim.

a cloglog or a probit link produce qualitatively identical estimates, even when including the mean of the explanatory variables in the specification (Tables A25 to A27). Replacing regime with country fixed effects (Table A28), adding indirect regimes and oligarchies, or coding EARs using LIED (Skaaning, Gerring and Bartusevičius 2015) or V-Dem (Coppedge et al. 2017) rather than Cheibub, Gandhi and Vreeland's (2010) data produces even stronger estimates (Tables A29 to A31). Replacing $growth_{t-1}$ with a $recession_{t-1}$ dummy that takes the value of 1 when the growth rate falls below −5% per year, or using growth data from the Penn World Tables or the World Development Indicators does not change the gist of the results (Tables A32 to A34), though the latter estimates are somewhat weaker – probably because they are more variable than the rest (see Table A5). The results are somewhat weaker for regimes that GWF code as party-based (Tables A35 and A36), and the relationship appears to be stronger in poorer countries, though this may reflect differences in sample sizes (Tables A37 and A38).

Potential mechanisms. Table 2 replicates models 4 and 7 from Table 1 for a variety of alternative outcomes. To save space, I focus on the estimates and standard errors for the marginal effect of $(growth_{t-1} \mid election_t)$, though the full results can be found in Tables A8 through A17.

Three results are worth noting. First, economic performance seems to have little effect on insider-driven takeovers or voluntary withdrawals from office: although the estimates are generally negative, the coefficients are very small in magnitude and far from statistically significant. But when the outcome of interest is $end\ (outsider)_t$, the results are very similar to those reported in Table 1: the effect of growth in non-election years in closed regimes is still negative and significant, though quite small in magnitude, while its impact on election years in EARs is more than an order of magnitude larger. This supports the claim that elections and the economy are especially relevant for understanding outsider-driven transitions. Furthermore, recall that the outcome of interest is loss of power by an autocratic ruling coalition, not the introduction of political or economic reforms; while the latter may harm (some) regime insiders and thus weaken their allegiance to the government, this need not result in a complete overhaul of the regime, at least in the short term.

Table 2: Examining potential mechanisms: Marginal effect of $growth_{t-1}$ on alternative outcomes

	end (insider)$_t$		***end (outsider)***$_t$		***end (voluntary)***$_t$		***protest***$_t$		***violent protest***$_t$	
	actual	sched.	actual	sched.	actual	sched.	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
no election (cl.)	-0.04	-0.04	-0.07	-0.06	-0.02	-0.01	-0.38	-0.38	-0.06	-0.07
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.13)	(0.13)	(0.06)	(0.06)
no election (EAR)	-0.02	0.02	-0.16	-0.10	0.05	0.05	-0.36	-0.44	-0.03	-0.08
	(0.06)	(0.04)	(0.14)	(0.11)	(0.05)	(0.03)	(0.30)	(0.33)	(0.09)	(0.11)
election (closed)	-0.45		-0.07		0.10		-0.17		-0.87	
	(0.27)		(0.69)		(0.23)		(0.82)		(0.55)	
election (EAR)	0.02	-0.25	-1.04	-0.96	-0.07	-0.04	-1.00	-0.26	-1.04	-0.63
	(0.05)	(0.20)	(0.47)	(0.42)	(0.11)	(0.09)	(0.68)	(0.40)	(0.51)	(0.35)
	coup attempt$_t$		***successful coup***$_t$		***unsuc. coup***$_t$		***other autocracy***$_{t+1}$		***democracy***$_{t+1}$	
	actual	sched.	actual	sched.	actual	sched.	actual	sched.	actual	sched.
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
no election (cl.)	-0.13	-0.13	-0.06	-0.06	-0.08	-0.08	-0.08	-0.08	-0.04	-0.03
	(0.10)	(0.10)	(0.05)	(0.05)	(0.09)	(0.09)	(0.05)	(0.05)	(0.03)	(0.03)
no election (EAR)	-0.29	-0.21	-0.08	0.02	-0.18	-0.20	-0.08	-0.11	-0.00	0.08
	(0.21)	(0.19)	(0.12)	(0.05)	(0.17)	(0.18)	(0.09)	(0.09)	(0.13)	(0.08)
election (closed)	-0.46		-0.48		-0.01		0.07		-0.49	
	(0.37)		(0.29)		(0.24)		(0.14)		(0.75)	
election (EAR)	-0.47	-0.82	-0.04	-0.61	-0.42	-0.24	0.00	0.10	-1.10	-1.23
	(0.21)	(0.37)	(0.09)	(0.37)	(0.21)	(0.16)	(0.08)	(0.08)	(0.47)	(0.40)

Values based on OLS regression estimates reported in columns 4 and 7 of Tables A8 to A17. Robust standard errors clustered by regime in parentheses. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3.

The impact of growth on intermediate outcomes such as protests and coups is less straightforward. Higher growth rates make protests less likely in non-election years, and the effect is substantial: a 5 pp. increase in $growth_{t-1}$ decreases the probability of observing at least one protest at t by $5 \times 0.38 \approx 1.9$ pp. The effect is similar for closed regimes and EARs, though it is only significant for the former (probably due to sample size). More surprisingly, the effect does not increase in election years. This may reflect the fact that governments are more sensitive to social unrest during election years, and thus more willing to offer concessions and negotiate with opposition leaders. Furthermore, if the government concedes an electoral defeat, there is little reason to protest in the first place. But when the government steals an election – or there is widespread perception that

the government tried to steal an election –, protests should be more likely (Tucker 2007; Kuntz and Thompson 2009), and since the stakes are higher, the likelihood of violence increases. In line with this admittedly speculative interpretation, models 9-10 show that economic performance only increases the likelihood of *violent* protests in election years.

Models 11-16 indicate that the evidence for coups is not straightforward either. Coup *attempts* are much more sensitive to economic conditions in elections years, perhaps because hardliners are less likely to stage a coup when electoral results are favorable to the government (Wig and Rød 2016). But whether this results is driven by successful or unsuccessful coups is unclear: looking at scheduled elections suggests the former, while examining actual elections implies the latter.

Finally, models 17-20 suggest that growth-driven breakdowns in closed regimes are more likely to result in another autocracy, while election-related ones are much more likely to result in democratization (see also the estimates for $election_t$ in Table A17). These results should be taken with a grain of salt, however, as they are purely short-term effects – they simply reflect a country's status at $t+1$ –, and may be an artifact of the fact that when an authoritarian government steps down following an election, researchers are more likely to give the new government the benefit of the doubt. But in line with the claim that the effect of economic growth on electoral *manipulation* is indeterminate, the estimates in Table A18 are often statistically insignificant and change wildly between specifications.

Conclusion

This paper extends the literature on authoritarian elections in three ways. First, it examines how the effect of competitive elections may be mediated by the economy and vice versa, an issue that has received surprisingly little attention so far (though see Fearon 2011, Reuter and Gandhi 2011 and Rozenas 2016 for partial exceptions). Second, it explains why, considered in isolation, economic performance may have a weak impact on breakdown. And thirdly, it proposes a novel mechanism through which elections may affect authoritarian survival: not by generating or communicating

information, but rather by defusing anti-regime behavior in non-election years – though this comes at the cost of increased vulnerability in election periods (see also Harish and Little 2017).

In line with this story, economic performance has a modest impact on breakdown in closed regimes, but EARs are extremely vulnerable to bad economic performance in election periods. That said, the effect of economic growth in non-election years in EARs, while statistically indistinguishable from zero, is not statistically different from the corresponding effect in closed autocracies. Nonetheless, this is not inconsistent with the argument, and other pieces of evidence are in line with it. First, the results are driven by *competitive* elections for selecting the incumbent *executive*, highlighting the importance of distinguishing between different kinds of authoritarian elections (see also Morse 2012; Bernhard, Edgell and Lindberg 2016 and Knutsen, Nygård and Wig 2017). Second, election-related breakdowns are more likely to be driven by outsiders, and more likely to result in democratization. These results cannot be interpreted causally, but their credibility is enhanced by three factors. First, the fixed effects account for a wide variety of time-invariant regime characteristics that may simultaneously affect the growth rate, the decision to adopt elections, and the likelihood of breakdown. Second, the findings are stronger when looking at the date of the next scheduled election, which can be taken as fixed, rather than the actual election date, which may be manipulated strategically (Pop-Eleches and Robertson 2015). Lastly, if the government manipulates the economy for electoral reasons (Akhmedov and Zhuravskaya 2004; Magaloni 2006; Pepinsky 2007; Blaydes 2010), growth rates should be especially high in election years, thus stacking the deck against finding any results. Still, some time-changing factors may simultaneously affect both economic performance and regime breakdown, e.g. if economic elites reduce investment in anticipation of election-driven turmoil. Lagging growth rates mitigates the impact of this possibility, but cannot eliminate it. In any case, such behavior would be consistent with the logic of the argument, as it implies that authoritarian rulers are safer when elections are far away into the future.

It is also worth noting what this paper does *not* say. First, the argument is agnostic about the impact of elections on long-term survival: the increased risk of breakdown in election periods may

more than compensate for EARs' increased resiliency in non-election years. Second, I do not claim that the short-term benefits of the electoral cycle are the reason why autocracies introduce elections in the first place; both domestic (Acemoglu and Robinson 2005; Kim 2017) and international (Dunning 2004; Finkel, Pérez-Liñán and Seligson 2007; Levitsky and Way 2010; Dietrich and Wright 2015; Miller forthcoming) pressure probably play a much bigger role. That said, autocracies that expect to deliver consistently high growth rates – China or Vietnam come to mind – should be especially wary of electoral competition: they have little reason to fear a coup or a popular uprising, but elections could make them vulnerable to even moderate slowdowns. Indeed, the Chinese government is extremely suspicious about citizens coordinating for *any* reason (King, Pan and Roberts 2013).

In terms of further research, while the argument emphasized the possibility that elections allow citizens and opposition leaders to coordinate, the electoral cycle should affect the behavior of other players as well. For example, elections may strengthen the hand of moderate opposition leaders who advocate challenging the regime at the polls at the expense of extremists who want to take to the streets, as in Venezuela after 2013.[33] Thus, one may examine the rhetoric of moderate and extremist opposition leaders when scandals or economic crises erupt in non-election years. Elections may also induce regime insiders to invest in election-related activities or change the *type* of insider that reaches top-level positions or defects (see Reuter and Szakonyi 2017). To the extent that this is the case, insiders who have invested in election-related activities should be more willing to concede an electoral defeat, because they can expect to return to power in the future. The argument also assumes that defecting from the regime to challenge it at the polls is less risky than planning a coup; accordingly, defectors who lost an election (in EARs) should fare better than unsuccessful coup plotters (in both EARs and closed regimes). Lastly, to the extent that elections are risky, incumbents in EARs should maximize their chances of survival by extending the term lengths of executive officials and thus reducing the frequency of elections, as Chávez did in Venezuela in 1999 (Corrales and Penfold-Becerra 2011) or Medvedev in Russia in 2008 (Baturo 2014).

[33] "A tale of two prisoners," *The Economist*, 22 February 2014; "Tyranny looms," *The Economist*, 28 February 2015.

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Online Appendix for "Strength in Expectation"

- Section A presents an overview of the Geddes, Wright and Frantz (2014) dataset and documents how I extended it until 31 December 2015.
- Section B lists all authoritarian regimes included in the sample, specifying how they ended according to Geddes, Wright and Frantz (2014).
- Section C lists all regimes that broke down in a year in which there was an election, distinguishing between elections that occurred *before* the breakdown (and thus are included in the analysis) or *after* it (and thus are excluded).
- Section D explains how I coded EARs and closed regimes.
- Section E presents the descriptive statistics.
- Section F reports the full results from Tables 1 and 2 as well as the robustness checks.

A Extending the Autocratic Regimes dataset

Description. The Autocratic Regimes dataset (Geddes, Wright and Frantz 2014; henceforth GWF) is a dataset of authoritarian regimes around the world between 1946 and 2010. The sample is restricted to countries that (a) had at least one autocratic country-year between 1946 and 2010; and (b) had at least one million inhabitants in 2009.

GWF define a *regime* as "a set of formal and/or informal rules for choosing leaders and policies. An important element of this set of rules is the identity of the group from which leaders can be chosen (e.g., in a professionalized military regime, the group from which leaders can be chosen is officers of very high rank)." (Geddes, Wright and Frantz 2014:codebook) Each regime has a specific start and end dates. A regime may be replaced by another regime of a different kind (i.e., a democracy) or by another authoritarian regime. The codebook provides a brief description of the events that led to each authoritarian regime's establishment and breakdown.

A regime qualifies as authoritarian if either

(1) The executive achieves power through nondemocratic means and changes the rules for choosing leaders and policies. The executive is considered to have been chosen democratically if (a) she is directly elected by voters, and at least 10 percent of the total population is eligible to vote; or (b) she is chosen indirectly by a body of which at least 60 of its members were elected; or (c) she is the constitutional successor of a directly elected executive, even if the latter was removed undemocratically; and (d) elections are reasonably free and fair, meaning that all major parties are allowed to participate, there is no systematic harassment of opposition leaders or supporters, international observers do not report extensive vote fraud, and the incumbent does not "so dominate political resources and the media that observers do not consider elections fair;" or

(2) An executive that achieved power through democratic means subsequently changes the formal or informal rules for getting into office so that these cease to be democratic. This includes actions

such as closing the legislature unconstitutionally, banning major opposition parties, annulling unfavorable electoral results, or engaging in systematic vote fraud; or

(3) Elections are democratic, but the military (or some other unelected body) prevents major parties/candidates from competing, or dictates policy in major areas.

A regime is not authoritarian if either

(1) It is democratic; or

(2) It has a provisional government in charge of holding democratic elections and withdrawing from office afterwards. A government is only coded as provisional if (a) the majority of its top members were not part of an immediately preceding authoritarian regime; (b) the government actually holds democratic elections; and (c) the elections are held and the winner(s) take office; or

(3) It is not independent or is under foreign occupation; or

(4) No government controls most of the resources of the state.

An authoritarian regime may break down for one of the following reasons:

(1) Insiders change rules of regime; or

(2) Electoral defeat; or

(3) Transitional election in which no high-ranking member of the regime runs for office; or

(4) Popular uprising; or

(5) Military coup; or

(6) Military defeat by insurgents, revolutionaries, or combatants fighting a civil war; or

(7) Foreign invasion; or

(8) A new autocratic leader changes the rules for gaining office; or

(9) The state ceases to exist, or loses control of most of the country's territory.

Extension. The dataset only covers the 1946-2010 period, so I followed the authors' coding rules to extend it until 31 December 2015. I changed the authors' original coding in a few instances in which I found reasons to document that a democratic regime had become authoritarian. Below I present a brief description of the episodes that led to the establishment or demise of a new regime; if a country does not appear in this list, it means that I simply extended GWF's original coding until 2015.

Provisional governments

Burkina Faso (2014-2015)

Start: 10/30/2014 President Blaise Compaoré removed by the military, who handed over power to a civilian-led transitional government shortly afterwards.

End: 11/29/2015 Competitive presidential election won by Roch Marc Christian Kaboré.

Central African Republic (2013-)

Start: 03/24/2013 President Bozizé removed by insurgents, who handed over power to a civilian-led transitional government shortly afterwards. The new government held presidential elections in 2015 (with a second round in 2016), with the incumbent president barred from running as a candidate.

End: Regime continued in power as of December 31, 2015.

Czechoslovakia (1989-1990)

Start: 12/04/1989 Resignation of the communist regime leadership in response to massive demonstrations and strikes.

End: 06/08/1990 Multiparty legislative elections in which the anti-Communist opposition won by a landslide.

Egypt (2011-2012)

Start: 02/11/2011 President Hosni Mubarak resigned and handed over power to a provisional military government.

End: 06/16/2012 Second round of the presidential election, won by Mohamed Morsi of the Muslim Brotherhood.

Guinea (2010-2013)

End: 09/28/2013 Multiparty legislative elections in which the incumbent party fell short of an absolute majority of seats.

Guinea-Bissau (2012-2014)

Start: 04/12/2012 Interim president Raimundo Pereira ousted in a military coup.

End: 05/18/2014 Second round of the presidential election, won by José Mário Vaz of the PAIGC.

Kyrgyzstan (2010-2011)

Start: 04/08/2010 Ouster of Bakiyev in popular uprising (Economist 2010, 44).

End: 10/30/2011 Competitive presidential election, completing the transition.

Madagascar (1991-1993)

Start: 10/30/1991 President Didier Ratsiraka signed the Panorama Convention, which stripped him of most of his powers and placed executive authority in the High State Authority (HAE) and the Committee for Economic and Social Recovery (CRES).

End: 2/10/1993 Second round of the presidential election.

Mali (2012-2013)

Start: 03/22/2012 President Amadou Toumani Touré was ousted by a military coup.

End: 08/11/2013 Second round of the free and fair elections in which the incumbent president did not participate.

Niger (2010-2011)

Start: 02/18/2010 The military ousted the incumbent president in a coup.

End: 03/12/2011 Second round of the free and fair elections in which the incumbent president did not participate.

Tunisia (2011-2011)

Start: 02/27/2011 The long-ruling dictator, Ben-Ali, resigned in response to massive popular protests on January 14, 2011. He was succeeded briefly by his PM, but all members of the cabinet associated with the former ruling party resigned on February 27, 2011, ending the regime.

End: 12/12/2011 The newly elected Constituent Assembly selected Moncef Marzouki as the country's new president.

Authoritarian regimes

Afghanistan (2009-2014)

End: 04/05/2014 Incumbent president Karzai could neither run nor impose a successor in the presidential election.

Bolivia (2009-)

Start: 02/07/2009 Promulgation of a new constitution that violated established rules. In particular, (a) opposition candidates were prevented from attending the Congressional session that submitted the new constitution to voters; (b) Congress had to submit contentious issues to a referendum, *after* which the Constituent Assembly would submit the final draft of the constitution to voters, but instead sent everything at the same time; (c) the agreement with the opposition to approve the new constitution with a two-thirds majority of the Assembly was not respected – a secret meeting in which opposition members were not present changed the rules to single majority; the government later backtracked and established a two-thirds majority, but in the end avoided the requirement by allowing Congress to decide contentious issues (i.e., those without a two-thirds majority in the Assembly) by simple majority – ; and (d) Congress initiated impeachment proceedings against the members of the Constitutional Tribunal, thus preventing the opposition from having its complaints heard (Lehoucq 2008; see also Levitsky and Way 2010, ch. 4). The Morales government also arrested or threatened to arrest several opposition politicians (Levitsky and Way 2010, ch. 4; Lansdorf 2012).

End: Regime continued in power as of December 31, 2015.

Burkina Faso (1987-2014)

End: 10/30/2014 Popular protests following president Blaise Compaoré's decision to change the constitution and run for an additional term ended with the military taking power. Compaoré left the presidency and fled to Ivory Coast.

Central African Republic (2003-2013)

End: 03/24/2013 Rebel troops entered the capital, forcing president Bozizé to resign.

Ecuador (2007-)

Start: 01/15/2007 The newly elected president called a constituent assembly with sweeping powers; when Congress tried to restrict the assembly's powers, the electoral authority responded by sacking a majority of legislators (57 over 100), replacing them with their substitutes, who complied with the president's wishes. Once elected, the constituent assembly – which was controlled by the president's party – suspended Congress and sacked several officials appointed by it. Subsequently, president Correa restricted media freedoms, giving unfair advantages to state-owned (and in practice government-controlled) media over their private counterparts, and ensured that the electoral authority favored the ruling party *vis-à-vis* the opposition (Conaghan 2008; see also Levitsky and Way 2010, ch. 4, de la Torre 2013 and Sánchez-Sibony 2017:131-4).

End: Regime continued in power as of December 31, 2015.

Egypt (2012-2013)

Start: 11/22/2012 Elected president Mohamed Morsi issued a temporary constitutional declaration that granted him unlimited powers. Although he later restricted some of these, he maintained that his original declaration would stand.

End: 07/03/2013 President Mohamed Morsi removed in a military coup.

Egypt (2013-)

Start: 07/03/2013 President Mohamed Morsi removed in a military coup.

End: Regime continued in power as of December 31, 2015.

Ivory Coast (2000-2011)

End: 04/11/2011 President Gbagbo, who had refused to concede electoral defeat in the 2010 election, finally lost control of the capital and was taken into custody.

Libya (1969-2011)

End: 08/23/2011 Qaddafi and his sons fled the Libya capital as insurgents approached.

Madagascar (2009-2013)

End: 12/20/2013 Second round of the free and fair elections in which the incumbent president did not participate.

Myanmar (2010-)

Start: 11/07//2010 Competitive but flawed election in which the military-sponsored party won by a landslide and obtained a supermajority of seats. The new regime is coded as indirect military rather than EAR because the 2008 constitution establishes that the army will retain control of the home, defense and border-affairs ministers, plus the armed forces and the civil service, regardless of electoral outcomes. The constitution also bars Aung San Suu Kyi, the main opposition leader, from becoming president because her former husband and children are foreigners, and allows the army to legally seize power in "emergency" situations. Furthermore, the army is guaranteed control of 25% of parliamentary seats, enough to block a constitutional reform. Thus, despite the NLD's landslide victory in the 2015 general election, the military retained control of several crucial policy areas ("A charter for thugocracy," *The Economist*, 6 September 2007; "Myanmar's awful choice," *The Economist*, 23 April 2008; "Still the generals' election," *The Economist*, 31 October 2015; "A new era," *The Economist*, 14 November 2015; "A strange new world," *The Economist*, 6 February 2016; "Why Myanmar's path to democracy will be bumpy," *The Economist*, 3 April 2016; Steinberg 2011; Than 2011, 2013; Hlaing 2012).

End: Regime continued in power as of December 31, 2015.

Nicaragua (2008-)

Start: 11/09//2008 The flawed municipal elections indicate the point at which the Ortega government crossed the line from democracy to dictatorship. Since the previous months, the government had increasingly indicated that it would not hold a free and fair election: it banned two opposition parties (some of which may have captured votes from the ruling FSLN) on flimsy grounds; harassed independent journalists; and refused to allow international observers. On election day, the head of the Supreme Electoral Council, the body in charge of organizing the elections, accompanied Daniel Ortega to the voting booth, and the government did nothing to dispel allegations of fraud in several strategic races, including the capital Managua ("Tearing up the rules," *The Economist*, 14 August 2008; "How to steal an election," *The Economist*, 13 November 2008; and "The new Somoza," *The Economist*, 19 February 2009, for the allegations that the municipal elections were fraudulent; and Anderson and Dodd 2009 for the claims about growing authoritarianization at the national-level).

End: Regime continued in power as of December 31, 2015.

Niger (2009-2010)

Start: 06/26/2009 Date in which president Mamadou Tandja assumed special powers. The constitution barred Tandja to run for a third term after his second mandate expired in December. Unable to muster enough votes in parliament to remove term limits, he proposed a new constitution that would be approved directly by voters in a referendum. After the Constitutional Court declared the proposal unconstitutional, Tandja (who had already, and constitutionally, dissolved the National Assembly) announced that he was assuming emergency powers and dissolved the Constitutional Court a few days later. On 4 August 2009 the referendum passed by a huge margin, temporarily extending the president's term by three years. A few months later, the president's party won a two-thirds majority of seats in the National Assembly on the face of an opposition boycott (Muller et al. 2011).

End: 02/18/2010 The military ousted president Tandja in a coup.

Thailand (2014-)

Start: 05/22/2014 The military ousted the incumbent prime minister in a coup.

End: Regime continued in power as of December 31, 2015.

Yemen (1978-2015)

End: 01/22/2015 President Abd Rabbuh Mansur Hadi resigned as rebels took over the presidential palace. Subsequently, civil war ensued.

B Regimes included in the sample

Table A1 presents a list of the 258 authoritarian regimes included in the analysis. Regimes for which there is no data on economic growth (e.g., East Germany) are not included in the analysis and thus do not appear in the table. For each regime, the table indicates:

(1) Regime name/ID.

(2) The year the regime was originally established. Note that regimes appear in the data the year after they were established.

(3) The year the regime broke down, if applicable. Since data on economic growth is only available until 2011, regimes that broke down after that date are coded as having survived.

(4) Whether the regime was coded as closed authoritarian and/or EAR during at least a fraction of its lifetime (during the country-years included in the sample).

(5) A brief description of how the regime ended, if applicable. Note that when a regime ended due to a popular uprising or a military coup, the coup or the uprising may have been a direct consequence of an election.

Table A1: Authoritarian regimes included in the analysis

regime ID	begin year	end year	closed auth.	EAR	how ended	regime ID	begin year	end year	closed auth.	EAR	how ended
Afghanistan 29-73	1929	1973	1	0	military coup	Burkina Faso 87-14	1987	2014	1	1	popular uprising
Afghanistan 73-78	1973	1978	1	0	military coup	Burundi 62-66	1962	1966	1	0	military coup
Afghanistan 78-92	1978	1992	1	0	insurgency/revolution	Burundi 66-87	1966	1987	1	0	military coup
Afghanistan 96-01	1996	2001	1	0	foreign invasion	Burundi 87-93	1987	1993	1	0	electoral defeat
Albania 44-91	1944	1991	1	0	popular uprising	Burundi 96-03	1996	2003	1	0	rule change (election)
Algeria 62-92	1962	1992	1	0	military coup	Cambodia 53-70	1953	1970	1	1	military coup
Algeria 92-NA	1992		1	1	N/A	Cambodia 70-75	1970	1975	1	1	insurgency/revolution
Angola 75-NA	1975		1	1	N/A	Cambodia 75-79	1975	1979	1	0	foreign invasion
Argentina 51-55	1951	1955	0	1	military coup	Cambodia 79-NA	1979		1	1	N/A
Argentina 55-58	1955	1958	1	0	military coup	Cameroon 60-83	1960	1983	1	1	rule change (continuity)
Argentina 66-73	1966	1973	1	0	rule change (election)	Cameroon 83-NA	1983		1	1	N/A
Argentina 76-83	1976	1983	1	0	rule change (election)	Cen. Af. Rep. 60-65	1960	1965	1	0	military coup
Armenia 94-98	1994	1998	0	1	popular uprising	Cen. Af. Rep. 65-79	1966	1979	1	0	foreign invasion
Armenia 98-NA	1998		0	1	N/A	Cen. Af. Rep. 79-81	1979	1981	1	0	military coup
Azerbaijan 93-NA	1993		0	1	N/A	Cen. Af. Rep. 81-93	1981	1993	1	0	electoral defeat
Bangladesh 71-75	1971	1975	0	1	military coup	Cen. Af. Rep. 03-13	2003	2013	1	1	insurgency/revolution
Bangladesh 75-82	1975	1982	1	1	military coup	Chad 60-75	1960	1975	1	0	military coup
Bangladesh 82-90	1982	1990	1	1	popular uprising	Chad 75-79	1975	1979	1	0	insurgency/revolution
Bangladesh 07-08	2007	2008	1	0	rule change (election)	Chad 82-90	1982	1990	1	0	insurgency/revolution
Belarus 91-94	1991	1994	1	0	electoral defeat	Chad 90-NA	1990		1	1	N/A
Belarus 94-NA	1994		1	1	N/A	Chile 73-89	1973	1989	1	0	rule change (election)
Benin 60-63	1960	1963	0	1	popular uprising	China 49-NA	1949		1	0	N/A
Benin 63-65	1963	1965	1	0	military coup	Colombia 49-53	1949	1953	0	1	military coup
Benin 65-67	1965	1967	1	0	military coup	Colombia 53-58	1953	1958	1	0	rule change (election)
Benin 67-69	1967	1969	1	0	military coup	Congo 60-63	1960	1963	1	0	popular uprising
Benin 69-70	1969	1970	1	0	rule change (election)	Congo 63-68	1963	1968	1	0	military coup
Benin 72-90	1972	1990	1	0	popular uprising	Congo 68-91	1968	1991	1	0	popular uprising
Bolivia 09-NA	1929		0	1	N/A	Congo 97-NA	1997		1	1	N/A
Bolivia 51-52	1951	1952	1	0	insurgency/revolution	Costa Rica 48-49	1948	1949	1	0	rule change (no election)
Bolivia 52-64	1952	1964	0	1	military coup	Cuba 52-59	1952	1959	1	1	insurgency/revolution
Bolivia 64-69	1964	1969	1	1	military coup	Cuba 59-NA	1959		1	0	N/A
Bolivia 69-71	1969	1971	1	0	military coup	Czechoslovakia 48-89	1948	1989	1	0	popular uprising
Bolivia 71-79	1971	1979	1	0	rule change (election)	DR Congo 60-97	1960	1997	1	0	insurgency/revolution
Bolivia 80-82	1980	1982	1	0	popular uprising	DR Congo 97-NA	1997		1	1	N/A
Botswana 66-NA	1966		0	1	N/A	Dom. Republic 30-62	1930	1962	1	1	military coup
Brazil 64-85	1964	1985	1	1	electoral defeat	Dom. Republic 63-65	1963	1965	1	0	insurgency/revolution
Bulgaria 44-90	1944	1990	1	0	rule change (no election)	Dom. Republic 66-78	1966	1978	0	1	electoral defeat
Burkina Faso 60-66	1960	1966	1	0	popular uprising	Ecuador 63-66	1963	1966	1	0	popular uprising
Burkina Faso 66-80	1966	1980	1	1	military coup	Ecuador 70-72	1970	1972	1	0	military coup
Burkina Faso 80-82	1980	1982	1	0	military coup	Ecuador 72-79	1972	1979	1	0	rule change (election)
Burkina Faso 82-87	1982	1987	1	0	military coup	Ecuador 07-NA	1973		0	1	N/A

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regime ID	begin year	end year	closed auth.	EAR		how ended
Egypt 22-52	1922	1952	1	0	military coup
Egypt 52-11	1952	2010	1	1	popular uprising
El Salvador 31-48	1931	1948	1	0	military coup
El Salvador 48-82	1948	1982	1	1	foreign invasion
Ethiopia 1889-1974	1889	1974	1	0	popular uprising
Ethiopia 74-91	1974	1991	1	0	insurgency/revolution
Gabon 60-NA	1960		1	1	N/A
Gambia 65-94	1965	1994	0	1	military coup
Gambia 94-NA	1994		1	1	N/A
Georgia 92-03	1992	2003	0	1	popular uprising
Ghana 60-66	1960	1966	1	1	military coup
Ghana 66-69	1966	1969	1	0	rule change (election)
Ghana 72-79	1972	1979	1	0	rule change (election)
Ghana 81-00	1981	2000	1	1	electoral defeat
Greece 67-74	1967	1974	1	0	rule change (election)
Guatemala 54-58	1954	1958	1	0	electoral defeat
Guatemala 63-66	1963	1966	1	0	electoral defeat
Guatemala 70-85	1970	1985	1	0	rule change (election)
Guinea 58-84	1958	1984	1	0	military coup
Guinea 84-08	1984	2008	1	1	military coup
Guinea 08-10	2008	2010	1	0	rule change (election)
Guinea-Bissau 74-80	1974	1980	1	0	military coup
Guinea-Bissau 80-99	1980	1999	1	1	insurgency/revolution
Guinea-Bissau 02-03	2002	2003	0	1	military coup
Haiti 50-56	1950	1956	1	0	popular uprising
Haiti 57-86	1957	1986	1	1	popular uprising
Haiti 86-88	1986	1988	1	0	military coup
Haiti 88-90	1988	1990	1	0	rule change (election)
Haiti 91-94	1991	1994	1	0	foreign invasion
Haiti 99-04	1999	2004	0	1	insurgency/revolution
Honduras 33-56	1933	1956	1	1	military coup
Honduras 63-71	1963	1971	1	1	rule change (election)
Honduras 72-81	1972	1981	1	0	rule change (election)
Hungary 47-90	1947	1990	1	0	electoral defeat
Indonesia 49-66	1949	1966	1	0	military coup
Indonesia 66-99	1966	1999	1	0	electoral defeat
Iran 25-79	1925	1979	1	0	popular uprising
Iraq 32-58	1932	1958	1	0	military coup
Iraq 58-63	1958	1963	1	0	military coup
Iraq 63-68	1963	1968	1	0	military coup
Iraq 68-79	1968	1979	1	0	rule change (continuity)
Iraq 79-03	1979	2003	1	0	foreign invasion
Iraq 10-NA	2009		0	1	N/A
Ivory Coast 60-99	1960	1999	1	1	military coup
Ivory Coast 99-00	1999	2000	1	0	popular uprising
Ivory Coast 00-11	2000	2010	0	1	insurgency/revolution
Jordan 46-NA	1946		1	0	N/A
Kazakhstan 91-NA	1991		1	1	N/A
Kenya 63-02	1963	2002	1	1	electoral defeat
Kuwait 61-NA	1961		1	0	N/A
Kyrgyzstan 91-05	1991	2005	1	1	popular uprising
Kyrgyzstan 05-10	2005	2010	0	1	popular uprising
Laos 59-60	1959	1960	1	0	military coup
Laos 60-62	1960	1962	1	0	foreign invasion
Laos 75-NA	1975		1	0	N/A
Lesotho 70-86	1970	1986	1	1	military coup
Lesotho 86-93	1986	1993	1	0	rule change (election)
Liberia 44-80	1944	1980	1	1	military coup
Liberia 80-90	1980	1990	1	1	insurgency/revolution
Liberia 97-03	1997	2003	0	1	insurgency/revolution
Libya 51-69	1951	1969	1	0	military coup
Libya 69-11	1969	2011	1	0	insurgency/revolution
Madagascar 60-72	1960	1972	1	1	popular uprising
Madagascar 72-75	1972	1975	1	0	rule change (continuity)
Madagascar 75-91	1975	1993	1	0	rule change (no election)
Madagascar 09-13	2009	2013	1	0	rule change (election)
Malawi 64-94	1964	1994	1	0	electoral defeat
Malaysia 57-NA	1957		0	1	N/A
Mali 60-68	1960	1968	1	0	military coup
Mali 68-91	1968	1991	1	0	popular uprising
Mauritania 60-78	1960	1978	1	0	military coup
Mauritania 78-05	1978	2005	1	1	military coup
Mauritania 05-07	2005	2007	1	0	rule change (election)
Mauritania 08-NA	2008		1	0	N/A
Mexico 15-00	1915	2000	0	1	electoral defeat
Mongolia 21-93	1921	1993	1	1	electoral defeat
Morocco 56-NA	1956		1	0	N/A
Mozambique 75-NA	1975		1	1	N/A
Myanmar 58-60	1958	1960	0	1	rule change (election)
Myanmar 62-88	1962	1988	1	0	popular uprising
Myanmar 88-10	1988	2010	1	0	rule change (no election)
Namibia 90-NA	1990		0	1	N/A
Nepal 51-91	1951	1991	1	0	electoral defeat
Nepal 02-06	2002	2006	1	0	popular uprising
Nicaragua 36-79	1936	1979	1	1	insurgency/revolution
Nicaragua 08-NA	1944		0	1	N/A
Nicaragua 79-90	1979	1990	1	1	electoral defeat
Niger 60-74	1960	1974	1	0	military coup
Niger 09-10	1962	1992	0	1	military coup
Niger 74-91	1974	1991	1	0	popular uprising

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regime ID	begin year	end year	closed auth.	EAR	how ended	regime ID	begin year	end year	closed auth.	EAR	how ended
Niger 96-99	1996	1999	0	1	rule change (election)	Sudan 58-64	1958	1964	1	0	popular uprising
Nigeria 66-79	1966	1979	1	0	rule change (election)	Sudan 69-85	1969	1985	1	0	popular uprising
Nigeria 83-93	1983	1993	1	0	rule change (continuity)	Sudan 85-86	1985	1986	1	0	rule change (election)
Nigeria 93-99	1993	1999	1	0	rule change (election)	Sudan 89-NA	1989		1	1	N/A
North Korea 48-NA	1948		1	0	N/A	Swaziland 68-NA	1968		1	0	N/A
Oman 71-NA	1971		1	0	N/A	Syria 51-54	1951	1954	1	0	military coup
Pakistan 58-71	1958	1971	1	0	popular uprising	Syria 57-58	1957	1958	0	1	state disappearance
Pakistan 75-77	1975	1977	0	1	military coup	Syria 63-NA	1963		1	0	N/A
Pakistan 77-88	1977	1988	1	0	rule change (election)	Taiwan 49-00	1949	2000	1	1	electoral defeat
Pakistan 99-08	1999	2008	1	0	rule change (no election)	Tajikistan 91-NA	1991		1	1	N/A
Panama 49-51	1949	1951	1	0	military coup	Tanzania 64-NA	1964		1	1	N/A
Panama 53-55	1953	1955	0	1	rule change (no election)	Thailand 47-57	1947	1957	1	0	military coup
Panama 68-82	1968	1982	1	0	military coup	Thailand 57-73	1957	1973	1	1	popular uprising
Panama 82-89	1982	1989	1	0	foreign invasion	Thailand 76-88	1976	1988	1	1	electoral defeat
Paraguay 39-48	1939	1948	1	0	military coup	Thailand 91-92	1991	1992	1	0	popular uprising
Paraguay 48-54	1948	1954	1	0	military coup	Thailand 06-07	2006	2007	1	0	electoral defeat
Paraguay 54-93	1954	1993	1	1	rule change (no election)	Togo 60-63	1960	1963	1	0	military coup
Peru 48-56	1948	1956	1	0	electoral defeat	Togo 63-NA	1963		1	1	N/A
Peru 62-63	1962	1963	1	0	rule change (election)	Tunisia 56-11	1956	2010	1	1	popular uprising
Peru 68-80	1968	1980	1	0	rule change (election)	Turkey 23-50	1923	1950	0	1	electoral defeat
Peru 92-00	1992	2000	0	1	rule change (no election)	Turkey 57-60	1957	1960	0	1	military coup
Philippines 72-86	1972	1986	1	1	popular uprising	Turkey 60-61	1960	1961	1	0	rule change (election)
Poland 44-89	1944	1989	1	0	electoral defeat	Turkey 80-83	1980	1983	1	0	electoral defeat
Portugal 26-74	1926	1974	1	0	military coup	Turkmenistan 91-NA	1991		1	0	N/A
Romania 45-89	1945	1989	1	0	popular uprising	UAE 71-NA	1971		1	0	N/A
Russia 93-NA	1993		0	1	N/A	Uganda 66-71	1966	1971	1	1	military coup
Rwanda 62-73	1962	1973	1	1	military coup	Uganda 71-79	1971	1979	1	0	foreign invasion
Rwanda 73-94	1973	1994	1	0	insurgency/revolution	Uganda 80-85	1980	1985	0	1	military coup
Rwanda 94-NA	1994		1	1	N/A	Uganda 86-NA	1986		1	1	N/A
Saudi Arabia 27-NA	1927		1	0	N/A	Uruguay 73-84	1973	1984	1	0	rule change (election)
Senegal 60-00	1960	2000	1	1	electoral defeat	Uzbekistan 91-NA	1991		1	0	N/A
Serbia 91-00	1991	2000	0	1	popular uprising	Venezuela 48-58	1948	1958	1	0	popular uprising
Sierra Leone 67-68	1967	1968	1	0	military coup	Venezuela 05-NA	2005		0	1	N/A
Sierra Leone 68-92	1968	1992	1	1	military coup	Vietnam 54-NA	1954		1	0	N/A
Sierra Leone 92-96	1992	1996	1	0	rule change (election)	Yemen 18-62	1918	1962	1	0	military coup
Sierra Leone 97-98	1997	1998	1	0	foreign invasion	Yemen 62-67	1962	1967	1	0	military coup
Singapore 65-NA	1965		0	1	N/A	Yemen 67-74	1967	1974	1	0	military coup
Somalia 69-91	1969	1991	1	0	insurgency/revolution	Yemen 74-78	1974	1978	1	0	rule change (continuity)
Korea, South 48-60	1948	1960	0	1	popular uprising	Yemen 78-15	1978	2015	1	1	insurgency/revolution
Korea, South 61-87	1961	1987	1	1	popular uprising	Yugoslavia 45-90	1945	1990	1	0	state disappearance
Soviet Union 17-91	1917	1991	1	0	state disappearance	Zambia 67-91	1967	1991	1	1	electoral defeat
Spain 39-76	1939	1976	1	0	rule change (no election)	Zambia 96-11	1996	2010	0	1	electoral defeat
Sri Lanka 78-94	1978	1994	0	1	electoral defeat	Zimbabwe 80-NA	1980		0	1	N/A

C Breakdowns in election years

Table A2: Regimes that broke down in years with competitive executive elections

Election included in analysis			**Election not included in analysis**		
regime ID	end date	type	regime ID	end date	type
Albania 44-91	1991-06-01	closed	Argentina 55-58	1958-02-23	closed
Belarus 91-94	1994-07-10	closed	Argentina 66-73	1973-03-11	closed
Bulgaria 44-90	1990-08-01	closed	Argentina 76-83	1983-10-30	closed
Burundi 87-93	1993-06-01	closed	Armenia 94-98	1998-02-04	EAR
Central African Republic 79-81	1981-09-01	closed	Bangladesh 07-08	2008-12-29	closed
Central African Republic 81-93	1993-09-19	closed	Benin 69-70	1970-05-07	closed
Chile 73-89	1989-12-14	closed	Bolivia 71-79	1979-07-01	closed
Dominican Republic 66-78	1978-05-16	EAR	Colombia 53-58	1958-05-10	closed
Ghana 81-00	2000-12-28	EAR	Dominican Republic 30-62	1962-01-16	closed
Guatemala 63-66	1966-05-10	closed	Ecuador 72-79	1979-04-29	closed
Honduras 33-56	1956-10-21	closed	El Salvador 48-82	1982-03-28	closed
Hungary 47-90	1990-04-08	closed	Ghana 66-69	1969-08-29	closed
Indonesia 66-99	1999-06-07	closed	Ghana 72-79	1979-07-09	closed
Ivory Coast 99-00	2000-10-26	closed	Greece 67-74	1974-07-23	closed
Kenya 63-02	2002-12-27	EAR	Guatemala 54-58	1958-02-02	closed
Korea, South 48-60	1960-04-27	EAR	Guatemala 70-85	1985-12-08	closed
Laos 59-60	1960-08-09	closed	Guinea-Bissau 80-99	1999-05-07	EAR
Malawi 64-94	1994-05-17	closed	Haiti 86-88	1988-09-17	closed
Mexico 15-00	2000-07-02	EAR	Haiti 88-90	1990-03-10	closed
Mongolia 21-93	1993-06-06	EAR	Honduras 63-71	1971-03-28	EAR
Nicaragua 79-90	1990-02-25	EAR	Honduras 72-81	1981-11-29	closed
Pakistan 75-77	1977-07-05	EAR	Korea, South 61-87	1987-06-29	EAR
Peru 48-56	1956-06-17	closed	Kyrgyzstan 05-10	2010-04-08	EAR
Peru 92-00	2000-11-21	EAR	Kyrgyzstan 91-05	2005-03-24	EAR
Philippines 72-86	1986-02-25	EAR	Lesotho 86-93	1993-03-27	closed
Senegal 60-00	2000-03-19	EAR	Mauritania 05-07	2007-03-25	closed
Serbia 91-00	2000-10-05	EAR	Myanmar 58-60	1960-02-06	EAR
Sri Lanka 78-94	1994-11-09	EAR	Myanmar 88-10	2010-11-07	closed
Taiwan 49-00	2000-03-18	EAR	Nepal 51-91	1991-05-12	closed
Thailand 76-88	1988-07-24	EAR	Niger 96-99	1999-11-24	EAR
Thailand 91-92	1992-05-20	closed	Nigeria 66-79	1979-08-11	closed
Turkey 23-50	1950-05-14	EAR	Nigeria 93-99	1999-02-27	closed
Zambia 67-91	1991-10-31	closed	Pakistan 77-88	1988-11-16	closed
Zambia 96-11	2011-09-20	EAR	Paraguay 54-93	1993-05-09	EAR
			Peru 62-63	1963-06-09	closed
			Peru 68-80	1980-05-18	closed
			Sierra Leone 92-96	1996-03-15	closed
			Sudan 85-86	1986-04-12	closed
			Syria 51-54	1954-02-25	closed
			Thailand 06-07	2007-12-23	closed
			Thailand 47-57	1957-09-16	closed
			Tunisia 56-11	2011-02-27	EAR
			Turkey 60-61	1961-10-15	closed
			Turkey 80-83	1983-11-06	closed
			Uruguay 73-84	1984-11-25	closed
			Venezuela 48-58	1958-01-23	closed

D EARs and closed regimes

Conceptualization. As explained in the text, the argument is based upon a basic distinction between two kinds of authoritarian regimes. Electoral authoritarian regimes or EARs are those in which both the executive and the legislature are chosen in formally competitive elections with universal (male) suffrage. In other words, in such regimes political institutions are formally democratic, though in practice election quality may fall (well) below minimally acceptable democratic standards, for example if the ruling party monopolizes access to the media, harasses opposition leaders and supporters with impunity, or resorts to widespread fraud. Thus, my classification is very similar to that of Levitsky and Way (2010), though unlike these authors I do *not* distinguish between "competitive" authoritarian regimes – in which elections are meaningful even if manipulated – and "hegemonic" ones, in which elections are little more than a façade. The rationale for this decision is twofold. Since both competitive and hegemonic regimes employ similar *formal* institutions, the distinction between the two must be made on the basis of other criteria, such as vote shares. For example, a regime may qualify at hegemonic if the ruling party obtained more than 70% of the vote in the last election, and competitive otherwise; yet such thresholds are not theory-driven. Furthermore, observed vote shares reflect *equilibrium outcomes* rather than fundamental institutional differences: a regime may receive 70% of the vote not because it will not accept a lower vote share, but rather because insiders are waiting for a suitable opportunity to defect. The case of the Mexican PRI – Miguel De la Madrid obtained 71% of the vote in 1982, but Carlos Salinas barely (and suspiciously) crossed the 50% mark after a massive defection six years later – illustrates this point clearly, as do regimes that become less competitive over time as it becomes evident that the incumbent cannot be effectively challenged at the polls – with Belarus under Lukashenka as a prime example.

In addition to EARs, there are three other types of authoritarian regimes (see Table A3). *Competitive oligarchies* are similar to EARs except for the fact that the franchise is severely restricted, as in South Africa under *apartheid* (Levitsky and Way 2010, ch. 1). These regimes were quite common

Table A3: Classifying authoritarian regime types

criterion	democracy	closed authoritarian	competitive oligarchy	indirect regime	electoral authoritarian
Elected executive and legislature	✓	✗/✓	✓	✓	✓
All elections formally competitive	✓	✗	✓	✓	✓
Elections free and fair	✓	N/A	✓	✗/✓	✗
Universal (male) sufrage	✓	✗/✓	✗	✓	✓
Elected authorities effectively govern	✓	N/A	✓	✗	✓

during the late XIXth and the early XXth century, but have almost vanished after 1946 (Miller 2015). As noted in fn. 21, I exclude them from the sample both due to their rarity and because the issue with such regimes is not that electoral alternation is precluded, but rather that a large subset of the population has no say in the electoral process. In contrast, *indirect regimes* are those in which there are formal democratic institutions – and elections may even qualify as minimally democratic – but effective political power resides in some unelected body and/or official, such as the military (as in Guatemala in the early 1980s), an individual dictator (Panama under Noriega) or a religious council (Iran since the Revolution). Thus, while elections in such regimes may be contested and informative, electoral alternation is not enough to dislodge the actual rulers from power, as seen in Iran between 1997 and 2005 (Brownlee 2007, ch. 5). In contrast, in EARs the actual ruler of the country is the elected executive, and thus electoral turnover means dislodging him from office.

Finally, I classify as *closed authoritarian* all those regimes that do not qualify as democratic, electoral authoritarian, competitive oligarchies, or indirect (see Table A3). Such regimes are a diverse bunch: they include autocracies in which there are no national elections at all, either because the executive is a monarch (Saudi Arabia), because the military acquired power in a coup and never institutionalized it (Argentina 1976-83, Uruguay 1973-84), or because the ruling party selects its

leaders in a different way (China); Communist regimes that hold single-party elections (Cuba, the Soviet Union); and autocracies that hold competitive elections for the legislature only (Jordan, Morocco).[1] Despite their differences, all such regimes share a crucial characteristic: they do not allow the opposition to access executive power by winning an election, *even in principle*. Thus, the claims made in this paper about elections' potential to foster coordination do not make sense in closed autocracies.

Of course, competitive elections are just one dimension around which autocracies may be classified. Other possibilities include coding the degree of military intervention in politics (Svolik 2012), or whether the regime is military, personalist, party-based, monarchic, or some combination of these (Geddes, Wright and Frantz 2014). The reason why I distinguish between closed regimes and EARs is theoretical: since the argument is about the impact of elections on authoritarian survival, the relevant criteria is whether elections are held, and if so, what characteristics they have. Alternative classifications are concerned with other criteria: although monarchies and military regimes generally qualify as closed, single-party and personalist regimes may be either closed or EARs.

Coding. $EAR_{r,t}$ is a dummy that takes the value of 1 if regime r qualified as electoral authoritarian at the beginning of year t, and 0 otherwise. To code this variable, I proceeded in the following way:

(1) I relied on GWF's coding scheme to identify all indirect regimes and competitive oligarchies, which I excluded from the sample (though the results in Table A29 show that coding indirect regimes as EARs and competitive oligarchies as closed does not change the results).

(2) Since GWF provide no data on authoritarian institutions, I turned to the Democracy and Dictatorship (DD) dataset (Cheibub, Gandhi and Vreeland 2010), which provides information on whether (a) the executive was elected (`exselec` variable); (b) the legislature was elected (`legselec`) and (c) open (`closed`), (d) multiple parties were legally allowed (`dejure`) and (e) did exist in

[1]Regimes that hold competitive elections for the executive but not for the legislature are theoretically possible but extremely rare in practice. In any case, they almost always correspond to short transitional periods in which the executive has been already elected but the legislature has not.

practice (`defacto2`). I extended the data until 2015 (the dataset ends in 2008) and took into account the fact that DD reports a country's status on December 31 of a given year, while I am interested in what happened on January 1st.

(3) After crossing GWF's data with DD, I cross-checked it with NELDA to confirm that the executive and the legislature had been elected in minimally competitive elections, as defined by Hyde and Marinov (2012).

To check the robustness of the results, I constructed two additional variables. *EAR (LIED)*$_{r,t}$ is similarly defined but constructed using data from the Lexical Index of Electoral Democracy (LIED; see Skaaning, Gerring and Bartusevičius 2015). This variable takes the value of 1 if and only if

(1) There is an elected legislature (`legislative_elections` = 1) `AND`

(2) There is an elected executive (`executive_elections` = 1) `AND`

(3) Opposition is formally allowed (`multiparty_legislative_elections` = 1) `AND`

(4) There is universal male or female suffrage (`male_suffrage` = 1 or `female_suffrage` = 1).[2]

Alternatively, *EAR (V-Dem)*$_{r,t}$ is constructed using data from the Varieties of Democracy (V-Dem) project (Coppedge et al. 2017). I proceeded in two steps. First, I coded a regime as having a legislature elected in formally competitive elections if:

(1) There is male or universal suffrage (`v2elsuffrage` $\geq$ 50
$\vee$ [`v2elgvsuflvl_imp` = 6 $\vee$ `v2elgvsuflvl_imp` = 8]) `AND`

(2) The legislature is elected (`v2xlg_elecreg` = 1)

(3) In formally multiparty elections (`v2elmulpar_ord_leg_imp` $\geq$ 2).

[2] Again, I took into account the fact that LIED, like DD, reports a country's status on December 31 of a given year.

EAR (V-Dem)$_{r,t}$ takes the value of 1 if the legislature had been elected in formally competitive elections `AND`

(1) The regime is parliamentary, meaning that

   (a) The head of government (HOG) was directly appointed by the legislature (`v2ex_legconhog` = 1) `AND`

   (b) The HOG is different from the head of state (HOS) (`v2exhoshog` = 0) `AND`

   (c) The HOG has more powers than the HOS (`v2ex_hosw` < 0.5)

   `OR`

(2) The regime is presidential, and the executive was directly elected in formally multiparty elections:

   (a) The HOG and the HOS are the same (`vdem_hos` = 1) `AND`

   (b) The HOS is directly elected (`v2ex_elechos` = 1) `AND`

   (c) Elections are formally multiparty (`v2elmulpar_ord_ex_imp` ≥ 3).[3]

[3] As before, I updated the data to reflect that V-Dem report values for December 31 of a given year.

Table A4: Agreement between alternative EAR classifications

	main			**LIED**			**V-Dem**		
(a) *Main sample*	closed	EAR	NA's	closed	EAR	NA's	closed	EAR	NA's
closed	1.00	0.00	0.00	0.94	0.06	0.00	0.90	0.09	0.01
EAR	0.00	1.00	0.00	0.09	0.91	0.00	0.24	0.76	0.00
(b) *LIED sample*									
closed	0.96	0.04	0.00	1.00	0.00	0.00	0.91	0.08	0.01
EAR	0.13	0.87	0.00	0.00	1.00	0.00	0.26	0.74	0.00
(c) *V-Dem sample*									
closed	0.90	0.10	0.00	0.89	0.11	0.00	1.00	0.00	0.00
EAR	0.22	0.78	0.00	0.20	0.80	0.00	0.00	1.00	0.00

The values indicate the proportion of observations coded as closed (or EAR) in one sample that are also coded as closed (or EAR) in another.

Table A4 reports the proportion of country-years classified as closed or EARs according to one dataset that are coded as closed or EARs by the others.[4] The agreement between the three measures is high, especially with respect to closed regimes. Indeed, Tables A30 and A31 show that replacing $EAR_{r,t}$ with either *EAR (LIED)*$_{r,t}$ or *EAR (V-Dem)*$_{r,t}$ does not change the gist of the results.

[4]There are also some missing values, mostly because V-Dem has no data for the United Arab Emirates.

E Descriptive statistics

Table A5: Descriptive statistics

	pooled sample		within-regime			
	mean	std. dev.	mean	std. dev.	min.	max.
breakdown$_t$	0.05	0.22	0.2	0.29	0	1
end (insider)$_t$	0.02	0.13	0.08	0.11	0	1
end (outsider)$_t$	0.02	0.14	0.06	0.1	0	1
end (voluntary)$_t$	0.01	0.1	0.05	0.06	0	1
other autocracy$_{t+1}$	0.02	0.15	0.1	0.14	0	1
democracy$_{t+1}$	0.02	0.15	0.09	0.13	0	1
protest$_t$	0.19	0.39	0.23	0.28	0	1
violent protest$_t$	0.05	0.22	0.07	0.08	0	1
coup attempt$_t$	0.06	0.23	0.16	0.24	0	1
successful coup$_t$	0.03	0.17	0.11	0.17	0	1
unsuccessful coup$_t$	0.03	0.17	0.06	0.13	0	1
growth$_{t-1}$ (Maddison)	0.02	0.06	0.01	0.05	-0.61	0.59
growth$_{t-1}$ (PWT v9.0)	0.02	0.08	0.01	0.05	-0.67	1.98
growth$_{t-1}$ (WDI)	0.02	0.06	0.02	0.05	-0.65	0.92
recession$_{t-1}$ (Maddison)	0.09	0.29	0.11	0.21	0	1
EAR$_t$	0.27	0.44	0.28	0.12	0	1
EAR (LIED)$_t$	0.28	0.45	0.3	0.14	0	1
EAR (V-Dem)$_t$	0.26	0.44	0.33	0.18	0	1
executive (competitive)$_t$	0.07	0.25	0.08	0.16	0	1
executive (non-competitive)$_t$	0.04	0.18	0.03	0.08	0	1
legislative (competitive)$_t$	0.04	0.2	0.04	0.1	0	1
executive (competitive) (this year)$_t$	0.06	0.23	0.07	0.15	0	1
executive (competitive) (other year)$_t$	0.22	0.41	0.23	0.17	0	1
executive (non-competitive) (this year)$_t$	0.03	0.18	0.03	0.07	0	1
executive (non-competitive) (other year)$_t$	0.17	0.38	0.15	0.15	0	1
legislative only (competitive) (this year)$_t$	0.04	0.19	0.04	0.09	0	1
legislative only (competitive) (other year)$_t$	0.21	0.41	0.04	0.09	0	1
GDP per capita$_{t-1}$ (th. 1990 Int. GK$)	2.87	3.36	2.26	0.5	0.2	30.93
oil and gas per capita$_{t-1}$ (th. 2000 dollars)	0.77	3.55	0.35	0.27	0	60.64
prop. democratic neighbors$_t$	0.22	0.22	0.27	0.06	0	1
party-based regime	0.48	0.5	0.29	0	0	1
poor country	0.69	0.46	0.72	0	0	1

The unit of observation is the country-year. The (Maddison) sample covers the 1948-2011 period. Observations: 4, 061; regimes: 258; countries: 115.

F Robustness checks

Main results

(1) *Full results.* Table A6 presents the full set of coefficients (except for the fixed effects and the duration polynomials) for the models reported in Table 1. In turn, Table A7 reports similar results but discriminating between (a) regimes that do not hold elections at all; (b) regimes that hold single-party (executive) elections only; and (c) EARs.

(2) *Alternative outcomes.* Tables A8 through A17 and the accompanying figures present the full set of coefficients (except for the fixed effects and the duration polynomials) for the models reported in Table 2. The values reported in Table 2 correspond to the marginal effects of $(growth_{t-1}|election_t)$ for the models reported in columns 4 and 7.

(3) *Electoral manipulation.* The models in Table A18 restrict the sample to *competitive* elections for an *executive* office to examine whether economic performance affects electoral manipulation. Manipulation is measured with four alternative indicators: from NELDA, (i) a dummy that takes the value of 1 if the government harassed the opposition (`nelda15`); and from V-Dem, continuous measures of the extent to which (ii) the election can be considered free and fair (`v2elfrfair`); (iii) there were voting irregularities (`v2elirreg`); or (iv) the government intimidated the opposition (`v2elintim`). Note that the sign of the last two variables was reversed so higher values indicate *more* irregularities and *higher* levels of intimidation. Panel (a) focuses on $growth_{t-1}$ exclusively, while panel (b) analyses whether the effect of economic conditions is different in breakdown years.

Alternative specifications

(1) *Additional controls.* Tables A19 and A21 to A23 and the accompanying figures replicate the results of Table A6, but controlling for *GDP per capita*$_{t-1}$ (log), *oil and gas per capita*$_{t-1}$ (log), *proportion of democratic neighbors*$_t$ and all three variables simultaneously, respectively. Table A20 and Figure A13 replicate the results in Table A6 but restricting the sample to observations for which data on *oil and gas per capita*$_{t-1}$ is available.

(2) *Alternative breakdown dates.* Table A24 and Figure A17 replicate the results of Table 1, but employing a more restrictive definition of regime breakdown. Specifically, a breakdown is coded when a regime first begins a move towards liberalization or democratization (e.g., by calling a

constituent assembly), even if that happens years in advance of the actual devolution of power or if the incumbent actually runs in the election. For example, in Indonesia the fall of Suharto did not immediately dislodge the ruling party from power, and indeed Golkar participated in the legislative election of June 1999; thus, GWF code the transition as occurring in that date (Geddes, Wright and Frantz 2014:codebook, p. 69). Using a more restrictive of breakdown, however, the regime's demise is coded as occurring on the day of Suharto's resignation (see Schuler, Gueorguiev and Cantú 2017).

(3) *Survival models (cloglog).* Table A25 reports the results of a series of generalized linear models with a cloglog link, which are equivalent to Cox survival model with discrete time (Box-Steffensmeier and Jones 2004; Carter and Signorino 2010). Survival models cannot accommodate regime fixed effects, so I control for *GDP per capita*$_{t-1}$ (log) as well as dummies for regime types (*party-based*, *military*, *personal* or *monarchy*) as defined by Geddes, Wright and Frantz (2014), region and year fixed effects, and a duration polynomial of order 3. Figure A18 reports the corresponding marginal effects for the linear term.

(4) *Survival models (probit).* The models in Table A26 are identical save for the fact that they employ a probit rather than a cloglog link. The models in Table A27 also add unit means for all time-varying variables (including the interaction terms; see Wooldridge 2010, ch. 15.8). Figures A19 and A20 report the corresponding marginal effects.

(5) *Country fixed effects.* Table A28 replicates the results of Table A6 but employing country instead of regime fixed effects.

Alternative measures of regime type

(1) *Indirect regimes and competitive oligarchies.* Table A29 replicates the results of Table 1 but adding indirect authoritarian regimes (coded as EARs) and competitive oligarchies (coded as closed authoritarian). See fn. 21 and Appendix D for a description of these regime types.

(2) *LIED coding.* Table A30 replicates the results of Table 1 but using the LIED data (Skaaning, Gerring and Bartusevičius 2015) to code EARs and closed regimes (see Appendix D for details).

(3) *V-Dem coding.* Table A31 replicates the results of Table 1 but using the V-Dem data (Coppedge et al. 2017) to code EARs and closed regimes (see Appendix D for details).

Alternative economic indicators

(1) *Recession dummy.* Table A32 and Figure A25 replicate the specifications of Table A6 but replacing $growth_{t-1}$ with a $recession_{t-1}$ dummy that takes the value of 1 if $growth_{t-1} < -0.05$, and 0 otherwise. Note that $recession_{t-1}$ should have a positive rather than a negative effect on $breakdown_t$.

(2) *PWT data.* Table A33 and Figure A26 replicate the specifications of Table A6 but measuring $growth_{t-1}$ using the Penn World Tables (PWT) v9.0[5] rather than Maddison data. These sources do not overlap perfectly, so the analysis covers 229 regimes in 106 countries between 1952 and 2015.

(3) *WDI data.* Table A34 and Figure A27 replicate the specifications of Table A6 but measuring $growth_{t-1}$ using the World Bank's World Development Indicators (WDI)[6] rather than Maddison data. These sources do not overlap perfectly, so the analysis covers 202 regimes in 104 countries between 1962 and 2015.

Split samples

(1) *Party-based regimes.* Tables A35 and A36 and the accompanying figures replicate the specifications of Table A6 but restricting the sample to regimes that Geddes, Wright and Frantz (2014) code as party-based (including party-personal, party-military and party-military-personal regimes) or not, respectively.

(2) *Regimes in poor vs. rich countries.* Tables A37 and A38 and the accompanying figures replicate the specifications of Table A6 but restricting the sample to *countries* located below or above the median value of *GDP per capita* in the year they first enter the sample (or the first year data is available), respectively.

[5] http://www.rug.nl/ggdc/productivity/pwt/.

[6] http://data.worldbank.org/data-catalog/world-development-indicators.

Table A6: Elections, economic performance and authoritarian breakdown, 1948-2011: Full results

		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.15		-0.12	-0.12	-0.13		-0.11	-0.12	-0.16	-0.17	-0.14	-0.11
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
$election_t$		0.07	0.09	0.10	0.10	0.10	0.12	0.12	-0.01	0.02	0.01	0.04
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.74	-0.31	0.03		-1.15	-0.93	0.41	-0.27	-0.06	-0.41
			(0.36)	(0.76)	(0.83)		(0.43)	(0.45)	(0.58)	(0.37)	(0.19)	(0.27)
$growth_{t-1} \times EAR_t$				-0.01	0.05				-0.01		-0.11	
				(0.17)	(0.17)				(0.17)		(0.18)	
$growth_{t-1} \times election_t$				-0.66	-0.90				-0.22		0.38	
$\times EAR_t$				(0.90)	(0.97)				(0.59)		(0.35)	
EAR_t				0.01	0.02				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.01	-0.02				-0.03		-0.04	
				(0.06)	(0.06)				(0.05)		(0.03)	
$election\ (other\ year)_t$						-0.00	-0.01	-0.00		-0.01		0.00
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.08	0.12		0.18		0.08
$\times\ election\ (other\ year)_t$							(0.14)	(0.15)		(0.15)		(0.14)
$election$												0.14
$(executive + legislative)_t$												(0.04)
$growth_{t-1} \times election$												-1.22
$(executive + legislative)_t$												(0.51)
$GDP\ per\ capita_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
$oil\ and\ gas$					-0.00			-0.00				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
$proportion\ of$					0.14			0.15				
$democratic\ neighbors_t$					(0.06)			(0.06)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

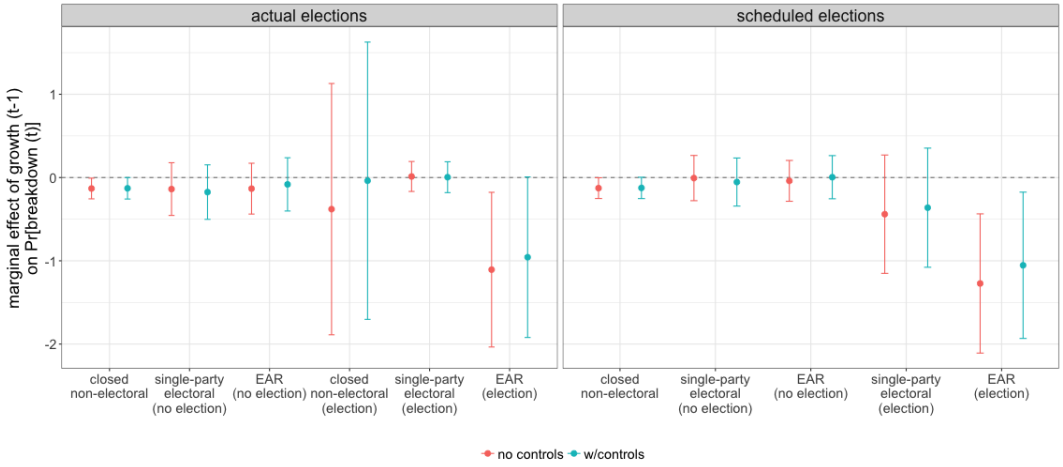
OLS regression estimates. The dependent variable is $breakdown_t$. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

Table A7: Including competitive and non-competitive elections simultaneously

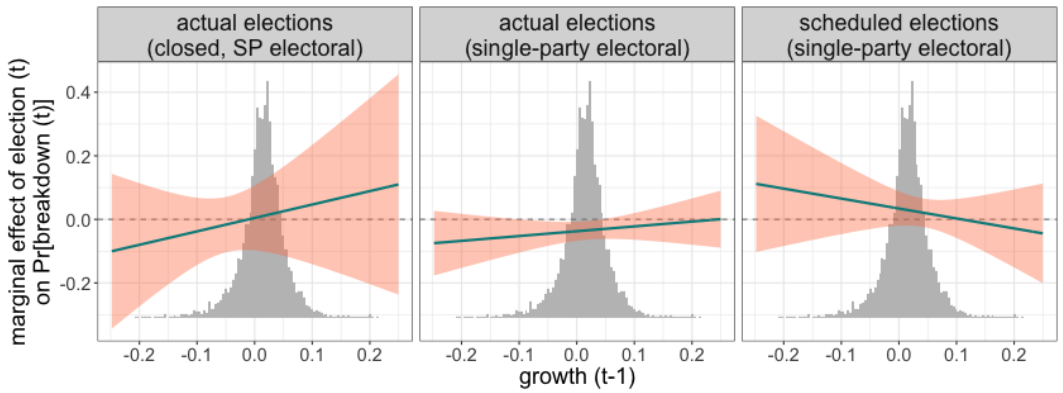
		actual elections				scheduled elections		
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election
(a) *Closed, no elections*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$growth_{t-1}$	-0.15		-0.13	-0.13	-0.13		-0.13	-0.12
	(0.06)		(0.06)	(0.06)	(0.07)		(0.06)	(0.07)
(b) *EARs: Elections are formally competitive*								
$election_t$		0.07	0.09	0.10	0.10	0.11	0.13	0.13
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.73	-0.25	0.09		-1.14	-0.93
			(0.36)	(0.76)	(0.84)		(0.43)	(0.45)
$growth_{t-1} \times EAR_t$				-0.00	0.05			
				(0.17)	(0.18)			
$growth_{t-1} \times election_t$				-0.72	-0.97			
$\times EAR_t$				(0.90)	(0.98)			
EAR_t				0.03	0.03			
				(0.02)	(0.02)			
$election_t \times EAR_t$				-0.01	-0.02			
				(0.06)	(0.06)			
election (other year)$_t$						0.01	0.00	0.00
						(0.02)	(0.02)	(0.02)
$growth_{t-1}$							0.09	0.13
$\times$ *election (other year)*$_t$							(0.14)	(0.15)
(c) *Closed regimes with single-party elections*								
$election_t$		-0.02	-0.03	0.00	-0.01	0.03	0.03	0.02
		(0.02)	(0.02)	(0.05)	(0.05)	(0.02)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			0.18	0.42	0.43		-0.31	-0.24
			(0.14)	(0.58)	(0.60)		(0.37)	(0.37)
$growth_{t-1} \times EAR_t$				-0.01	-0.05			
				(0.17)	(0.18)			
$growth_{t-1} \times election_t$				-0.27	-0.25			
$\times EAR_t$				(0.59)	(0.62)			
EAR_t				0.04	0.03			
				(0.02)	(0.02)			
$election_t \times EAR_t$				-0.04	-0.03			
				(0.05)	(0.06)			
election (other year)$_t$						0.01	0.01	0.01
						(0.02)	(0.02)	(0.02)
$growth_{t-1}$							0.12	0.07
$\times$ *election (other year)*$_t$							(0.15)	(0.16)
(d) *Control variables*								
GDP per capita$_{t-1}$ (log)					-0.00			-0.00
					(0.02)			(0.02)
oil and gas					-0.00			-0.00
per capita$_{t-1}$ (log)					(0.01)			(0.01)
proportion of					0.15			0.15
democratic neighbors$_t$					(0.06)			(0.06)
observations	4061	4061	4061	4061	3823	4061	4061	3823
regimes	258	258	258	258	244	258	258	244
countries	115	115	115	115	114	115	115	114

OLS regression estimates. The dependent variable is $breakdown_t$. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$



(b) $election_t \mid growth_{t-1}$ (single-party electoral)



(c) $election_t \mid growth_{t-1}$ (EARs)

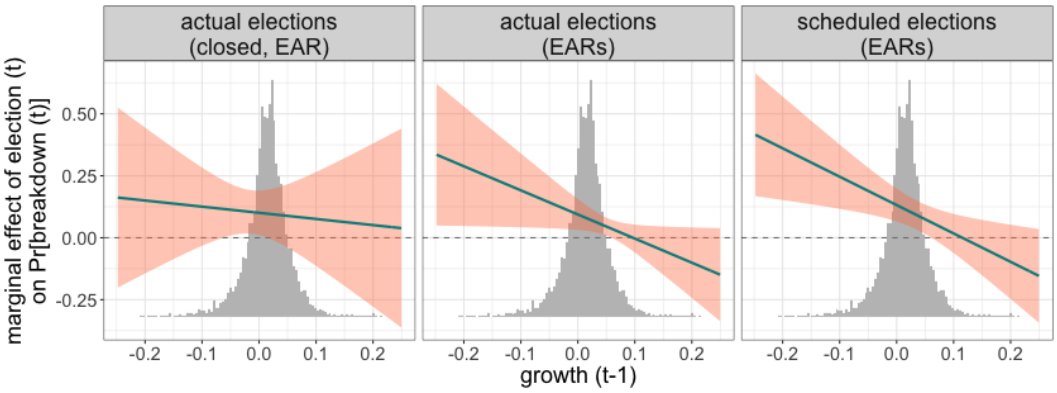


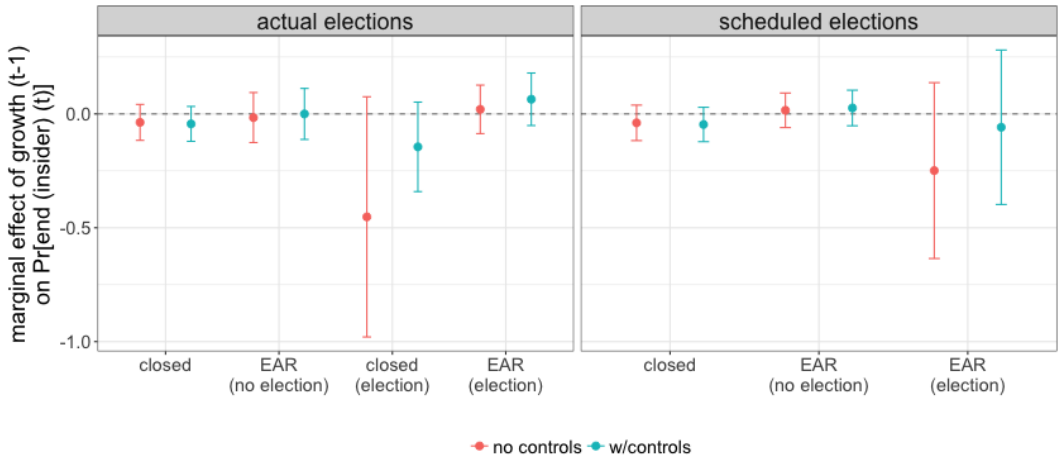
Figure A1: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A7. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4b and 7b; and the results reported in panel (c) correspond to models 4a and 7a, respectively.

Table A8: Alternative outcomes (1): Insider-driven end

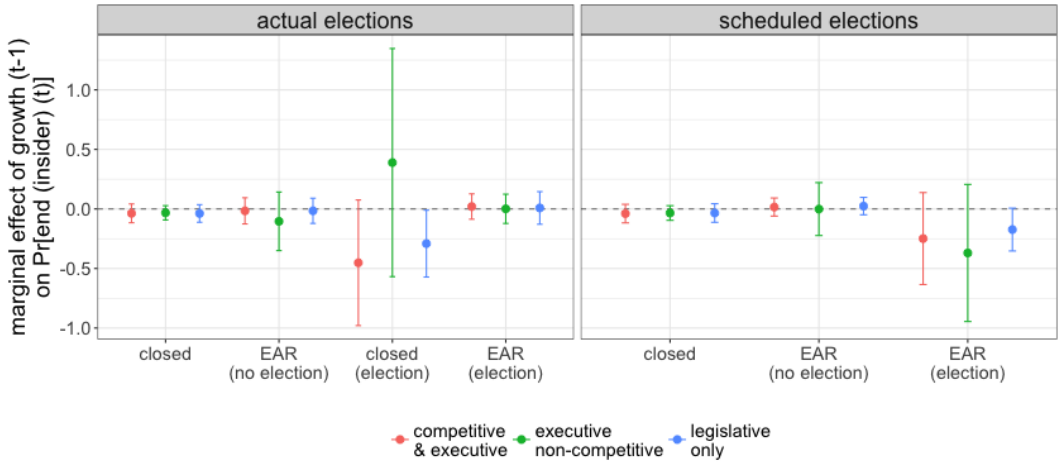
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.04		-0.03	-0.04	-0.04		-0.04	-0.05	-0.03	-0.03	-0.04	-0.03
	(0.03)		(0.03)	(0.04)	(0.04)		(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
$election_t$		-0.00	-0.00	-0.01	-0.01	-0.00	0.00	0.00	0.02	0.02	0.00	-0.00
		(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.02)	(0.02)	(0.01)
$growth_{t-1} \times election_t$			-0.12	-0.41	-0.10		-0.21	-0.01	0.42	-0.34	-0.25	-0.14
			(0.12)	(0.28)	(0.11)		(0.20)	(0.18)	(0.49)	(0.30)	(0.14)	(0.09)
$growth_{t-1} \times EAR_t$				0.02	0.04				-0.07		0.02	
				(0.07)	(0.07)				(0.13)		(0.06)	
$growth_{t-1} \times election_t$				0.45	0.17				-0.32		0.28	
$\times EAR_t$				(0.28)	(0.13)				(0.50)		(0.16)	
EAR_t				-0.00	0.00				0.02		-0.00	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				0.00	0.00				-0.05		-0.01	
				(0.02)	(0.01)				(0.04)		(0.02)	
election (other year)$_t$						-0.01	-0.01	-0.00		0.01		-0.01
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							0.06	0.07		0.03		0.06
$\times$ *election (other year)*$_t$							(0.05)	(0.05)		(0.12)		(0.05)
election												0.01
(executive + legislative)$_t$												(0.02)
$growth_{t-1} \times$ *election*												-0.29
(executive + legislative)$_t$												(0.27)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.01)			(0.01)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.00)			(0.00)				
proportion of					-0.01			-0.01				
democratic neighbors$_t$					(0.02)			(0.02)				
observations	4052	4052	4052	4052	3814	4052	4052	3814	4052	4052	4052	4052
regimes	257	257	257	257	243	257	257	243	257	257	257	257
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *end (insider)*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

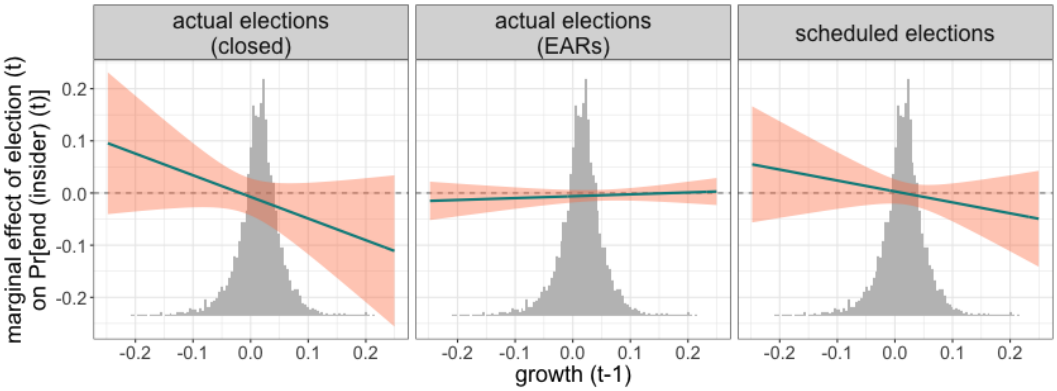


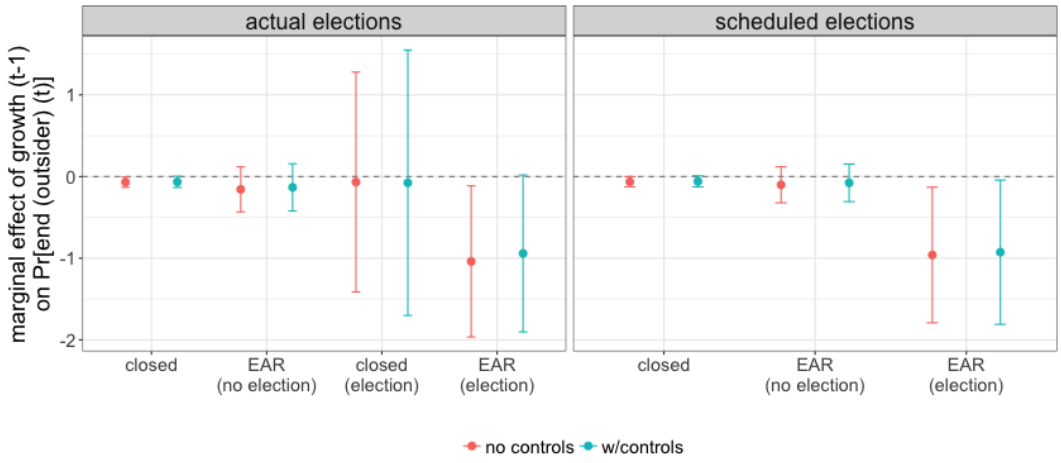
Figure A2: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of *end (insider)*$_t$, based on the results reported in Table A8. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A9: Alternative outcomes (2): Outsider-driven end

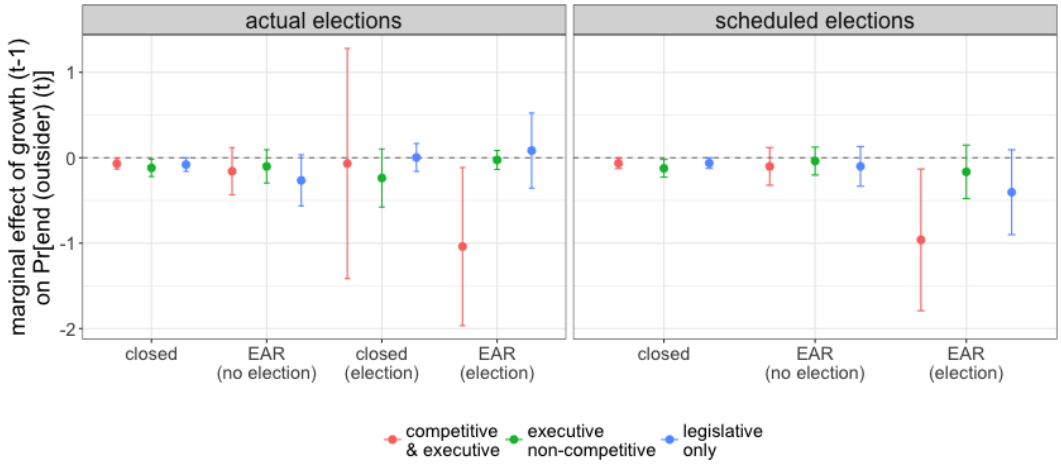
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.11		-0.09	-0.07	-0.07		-0.06	-0.06	-0.12	-0.12	-0.08	-0.06
	(0.05)		(0.04)	(0.03)	(0.04)		(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)
$election_t$		0.07	0.08	0.09	0.09	0.11	0.12	0.12	-0.04	0.01	-0.00	0.05
		(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
$growth_{t-1} \times election_t$			-0.57	0.00	-0.01		-0.90	-0.87	-0.12	-0.04	0.08	-0.34
			(0.35)	(0.68)	(0.82)		(0.42)	(0.45)	(0.18)	(0.17)	(0.08)	(0.25)
$growth_{t-1} \times EAR_t$				-0.09	-0.07				0.02		-0.18	
				(0.14)	(0.15)				(0.11)		(0.16)	
$growth_{t-1} \times election_t$				-0.88	-0.80				0.19		0.27	
$\times EAR_t$				(0.84)	(0.97)				(0.19)		(0.28)	
EAR_t				0.02	0.03				-0.00		0.03	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				0.00	-0.01				0.03		-0.02	
				(0.05)	(0.05)				(0.02)		(0.02)	
$election\ (other\ year)_t$						0.02	0.02	0.02		-0.01		0.02
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							-0.04	-0.02		0.09		-0.04
$\times\ election\ (other\ year)_t$							(0.12)	(0.12)		(0.10)		(0.12)
$election$												0.13
$(executive + legislative)_t$												(0.03)
$growth_{t-1} \times election$												-0.87
$(executive + legislative)_t$												(0.51)
$GDP\ per\ capita_{t-1}$ (log)					0.00			0.00				
					(0.01)			(0.01)				
$oil\ and\ gas$					-0.00			-0.00				
$per\ capita_{t-1}$ (log)					(0.00)			(0.00)				
$proportion\ of$					0.08			0.08				
$democratic\ neighbors_t$					(0.05)			(0.05)				
observations	4052	4052	4052	4052	3814	4052	4052	3814	4052	4052	4052	4052
regimes	257	257	257	257	243	257	257	243	257	257	257	257
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using $end\ (outsider)_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

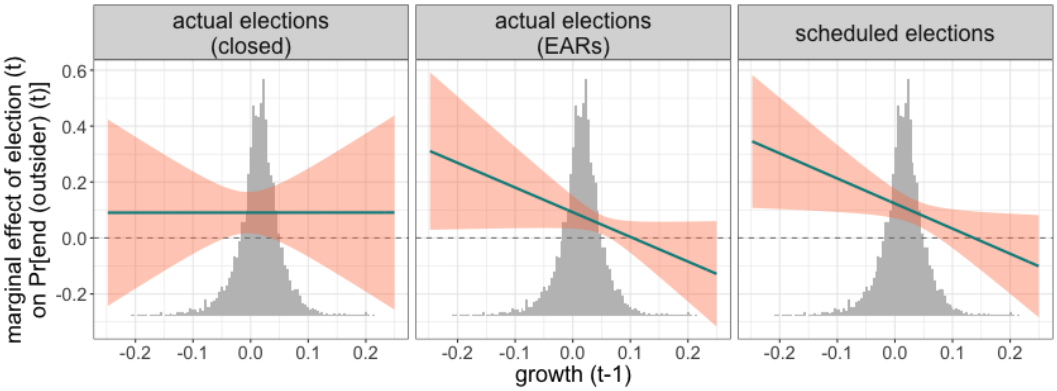


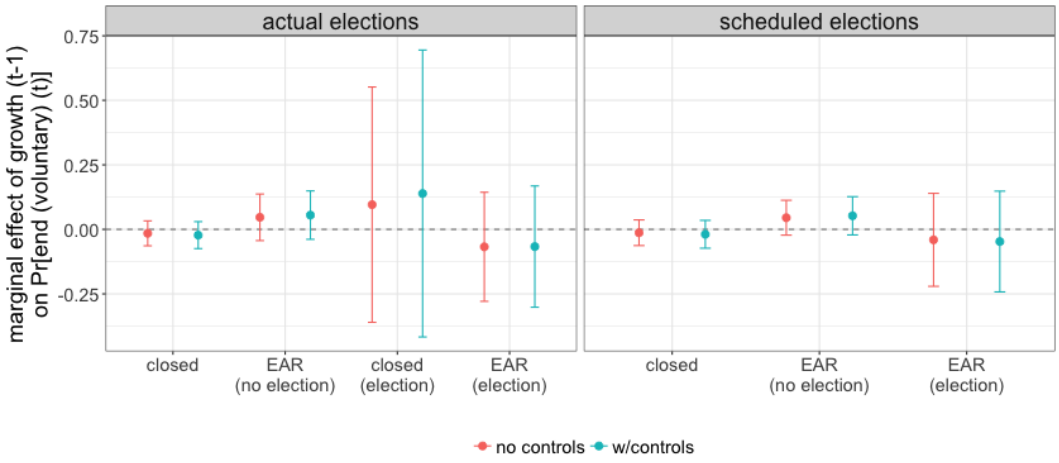
Figure A3: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $end\ (outsider)_t$, based on the results reported in Table A9. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A10: Alternative outcomes (3): Voluntary end

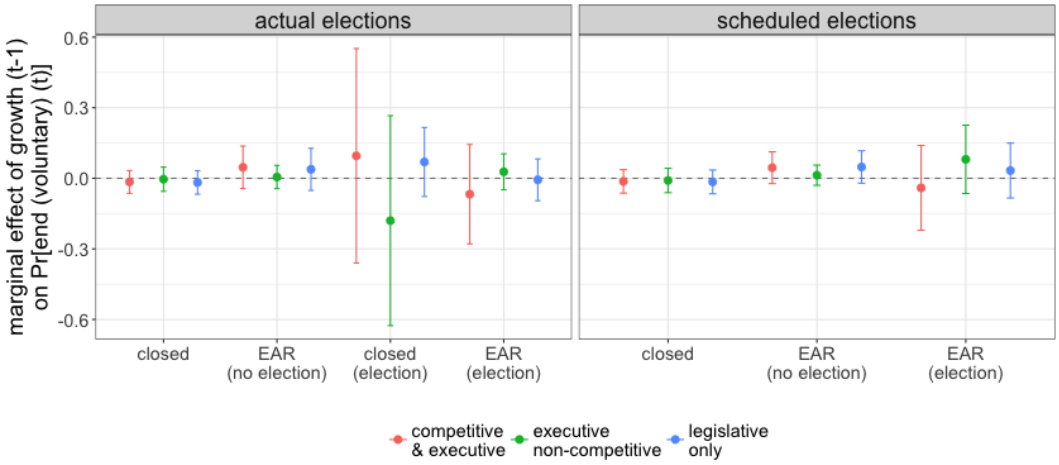
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.00		-0.00	-0.02	-0.02		-0.01	-0.02	-0.00	-0.01	-0.02	-0.02
	(0.02)		(0.02)	(0.02)	(0.03)		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$election_t$		0.01	0.01	0.02	0.02	0.00	0.00	0.00	0.02	-0.01	0.01	-0.00
		(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
$growth_{t-1} \times election_t$			-0.05	0.11	0.16		-0.03	-0.03	-0.18	0.09	0.09	0.05
			(0.09)	(0.23)	(0.28)		(0.09)	(0.10)	(0.23)	(0.08)	(0.08)	(0.06)
$growth_{t-1} \times EAR_t$				0.06	0.08				0.01		0.06	
				(0.05)	(0.05)				(0.04)		(0.05)	
$growth_{t-1} \times election_t$				-0.23	-0.28				0.20		-0.13	
$\times EAR_t$				(0.26)	(0.31)				(0.24)		(0.10)	
EAR_t				-0.00	-0.01				-0.00		-0.00	
				(0.00)	(0.01)				(0.00)		(0.00)	
$election_t \times EAR_t$				-0.01	-0.01				-0.02		-0.01	
				(0.02)	(0.02)				(0.03)		(0.01)	
election (other year)$_t$						-0.01	-0.01	-0.02		-0.01		-0.01
						(0.01)	(0.01)	(0.01)		(0.00)		(0.01)
$growth_{t-1}$							0.06	0.07		0.02		0.06
$\times$ *election (other year)*$_t$							(0.04)	(0.04)		(0.03)		(0.04)
election												0.01
(executive + legislative)$_t$												(0.02)
$growth_{t-1} \times$ *election*												-0.04
(executive + legislative)$_t$												(0.12)
GDP per capita$_{t-1}$ (log)					0.00			0.00				
					(0.01)			(0.01)				
oil and gas					0.00			0.00				
per capita$_{t-1}$ (log)					(0.00)			(0.00)				
proportion of					0.08			0.08				
democratic neighbors$_t$					(0.04)			(0.04)				
observations	4052	4052	4052	4052	3814	4052	4052	3814	4052	4052	4052	4052
regimes	257	257	257	257	243	257	257	243	257	257	257	257
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *end (voluntary)*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

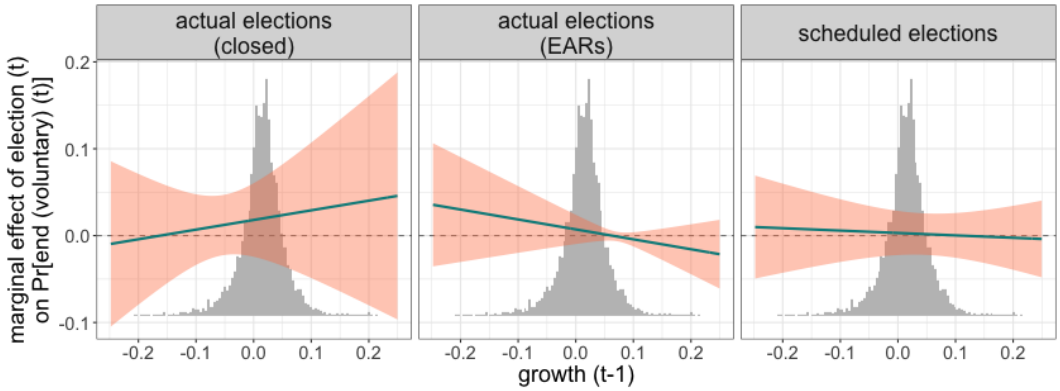


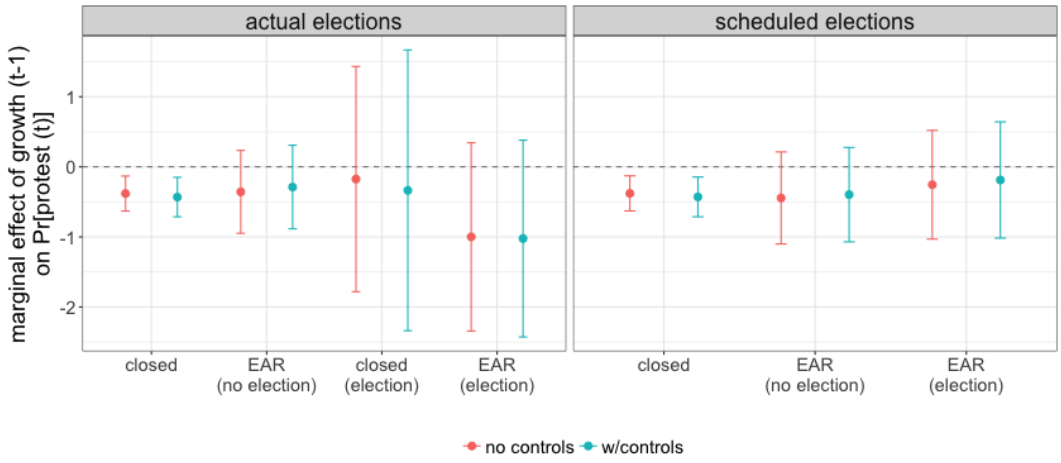
Figure A4: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $end\ (voluntary)_t$, based on the results reported in Table A10. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A11: Alternative outcomes (4): Protest(s)

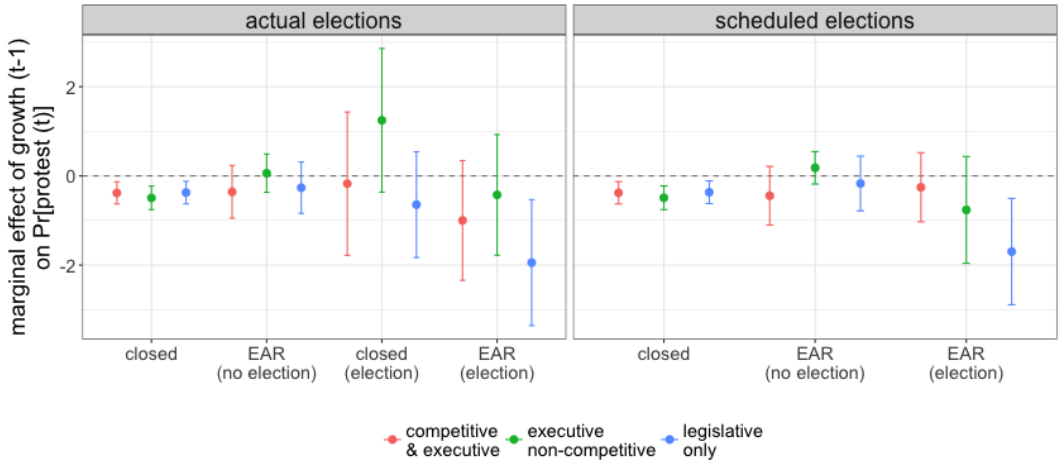
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.39		-0.38	-0.38	-0.43		-0.38	-0.43	-0.49	-0.49	-0.37	-0.37
	(0.12)		(0.12)	(0.13)	(0.14)		(0.13)	(0.14)	(0.13)	(0.14)	(0.13)	(0.13)
$election_t$		0.07	0.07	0.05	0.04	0.05	0.05	0.04	0.07	0.04	0.03	0.02
		(0.03)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.07)	(0.03)	(0.05)	(0.05)
$growth_{t-1} \times election_t$			-0.18	0.21	0.10		0.12	0.24	1.74	-0.27	-0.27	-1.33
			(0.53)	(0.83)	(1.03)		(0.41)	(0.44)	(0.82)	(0.62)	(0.62)	(0.63)
$growth_{t-1} \times EAR_t$				0.02	0.14				0.55		0.11	
				(0.33)	(0.33)				(0.24)		(0.32)	
$growth_{t-1} \times election_t$				-0.85	-0.83				-2.22		-1.41	
$\times EAR_t$				(1.07)	(1.25)				(1.04)		(0.91)	
EAR_t				-0.01	-0.01				0.02		-0.00	
				(0.03)	(0.03)				(0.02)		(0.03)	
$election_t \times EAR_t$				0.06	0.06				-0.08		-0.03	
				(0.07)	(0.07)				(0.09)		(0.08)	
election (other year)$_t$						-0.03	-0.03	-0.03		0.01		-0.01
						(0.03)	(0.03)	(0.04)		(0.02)		(0.03)
$growth_{t-1}$							-0.07	0.03		0.67		0.20
$\times$ *election (other year)*$_t$							(0.36)	(0.37)		(0.22)		(0.34)
election												0.03
(executive + legislative)$_t$												(0.05)
$growth_{t-1} \times$ *election*												-0.30
(executive + legislative)$_t$												(0.44)
GDP per capita$_{t-1}$ (log)					0.00			0.00				
					(0.04)			(0.04)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.29			0.30				
democratic neighbors$_t$					(0.10)			(0.10)				
observations	3538	3538	3538	3538	3309	3538	3538	3309	3538	3538	3538	3538
regimes	233	233	233	233	219	233	233	219	233	233	233	233
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using $protest_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

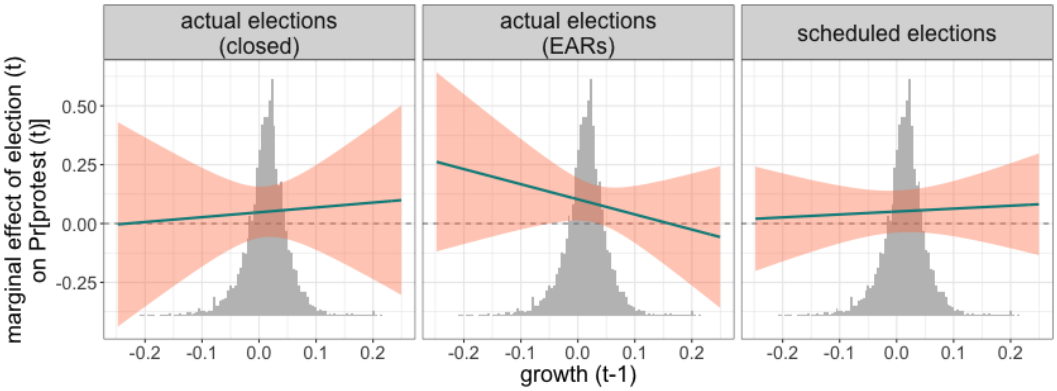


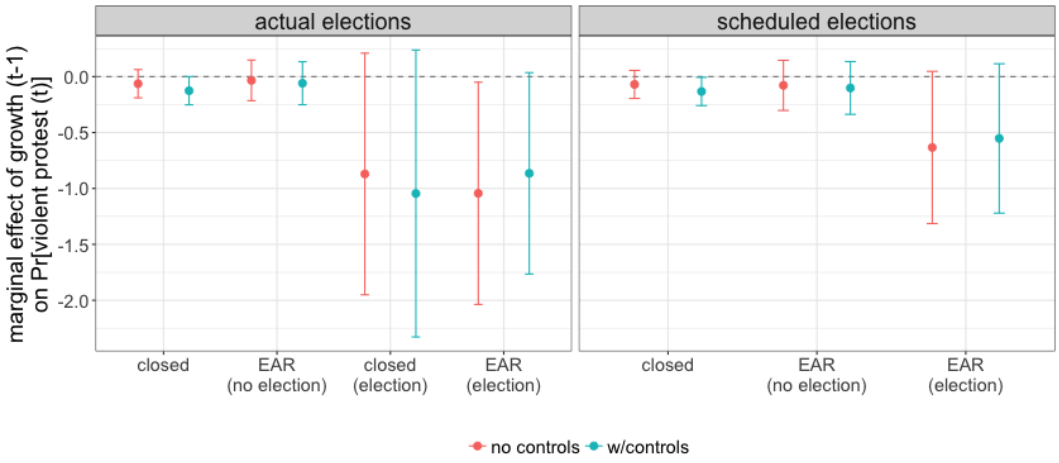
Figure A5: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $protest_t$, based on the results reported in Table A11. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A12: Alternative outcomes (5): Violent protest(s)

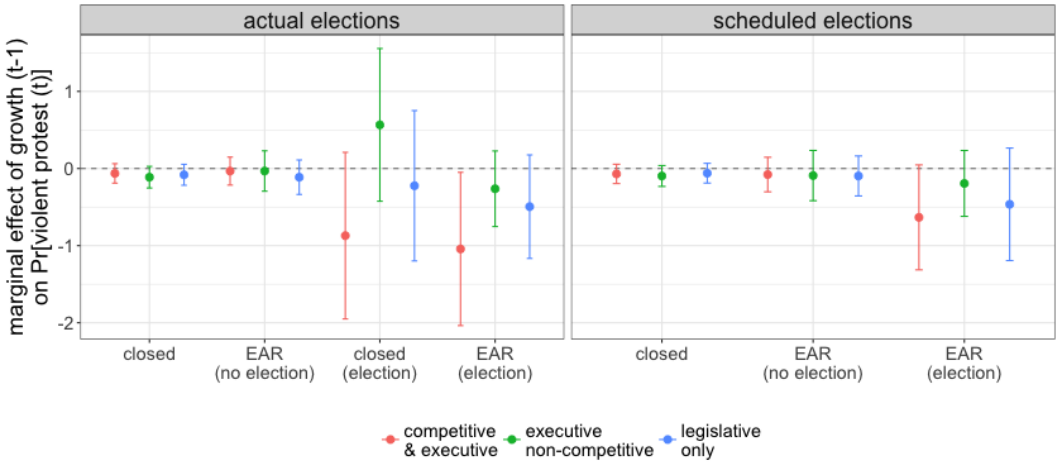
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.10		-0.06	-0.06	-0.13		-0.07	-0.13	-0.11	-0.10	-0.08	-0.06
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
$election_t$		0.05	0.06	0.04	0.05	0.03	0.04	0.03	-0.00	0.02	-0.01	-0.02
		(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.86	-0.81	-0.92		-0.56	-0.42	0.68	-0.10	-0.14	-0.40
			(0.34)	(0.54)	(0.64)		(0.35)	(0.34)	(0.53)	(0.24)	(0.51)	(0.38)
$growth_{t-1} \times EAR_t$				0.03	0.07				0.08		-0.03	
				(0.11)	(0.11)				(0.15)		(0.13)	
$growth_{t-1} \times election_t$				-0.20	0.11				-0.91		-0.24	
$\times EAR_t$				(0.79)	(0.83)				(0.61)		(0.64)	
EAR_t				-0.01	-0.01				0.01		-0.00	
				(0.02)	(0.02)				(0.02)		(0.02)	
$election_t \times EAR_t$				0.03	0.02				-0.00		-0.04	
				(0.04)	(0.05)				(0.04)		(0.04)	
election (other year)$_t$						-0.01	-0.01	-0.01		0.01		0.00
						(0.02)	(0.02)	(0.02)		(0.02)		(0.02)
$growth_{t-1}$							-0.01	0.03		0.01		-0.04
$\times$ *election (other year)*$_t$							(0.13)	(0.13)		(0.18)		(0.14)
election												0.04
(executive + legislative)$_t$												(0.03)
$growth_{t-1} \times$ *election*												-0.57
(executive + legislative)$_t$												(0.39)
GDP per capita$_{t-1}$ (log)					0.03			0.03				
					(0.03)			(0.03)				
oil and gas					-0.01			-0.01				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.03			0.03				
democratic neighbors$_t$					(0.08)			(0.08)				
observations	3538	3538	3538	3538	3309	3538	3538	3309	3538	3538	3538	3538
regimes	233	233	233	233	219	233	233	219	233	233	233	233
countries	115	115	115	115	0	115	115	0	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *violent protest*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

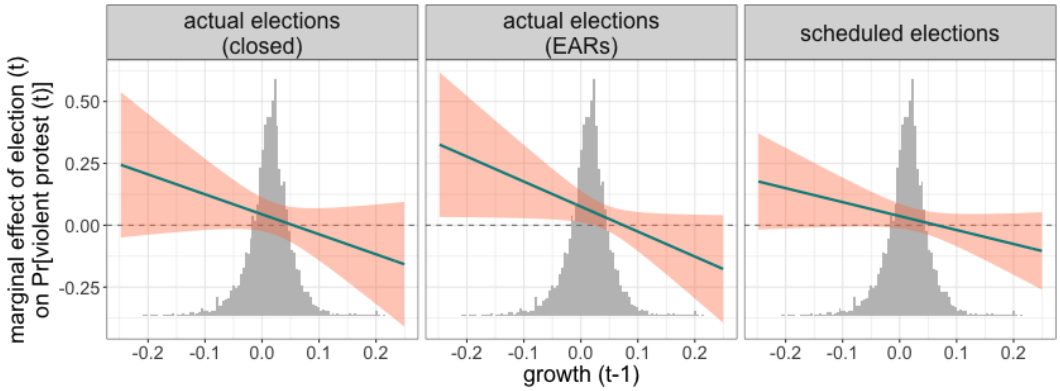


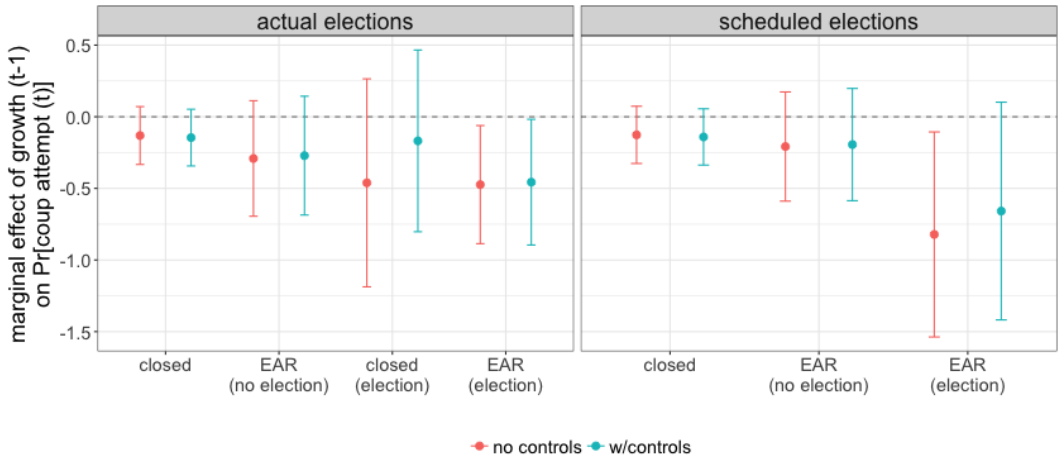
Figure A6: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of *violent protest*$_t$, based on the results reported in Table A12. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A13: Alternative outcomes (6): Coup attempt

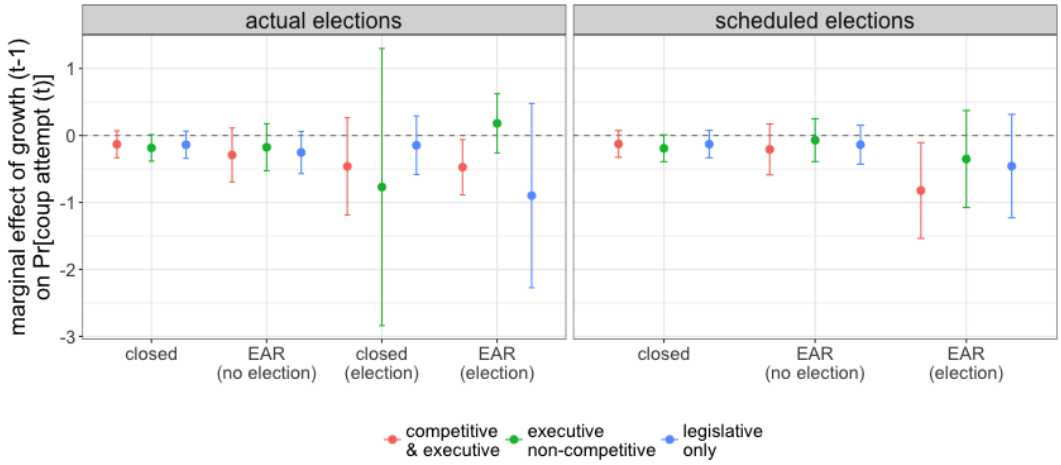
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.18		-0.17	-0.13	-0.15		-0.13	-0.14	-0.19	-0.19	-0.14	-0.13
	(0.09)		(0.09)	(0.10)	(0.10)		(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
$election_t$		0.01	0.01	-0.02	-0.02	0.00	0.02	0.02	-0.00	0.02	0.02	-0.01
		(0.01)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.03)	(0.04)	(0.03)
$growth_{t-1} \times election_t$			-0.19	-0.33	-0.02		-0.70	-0.52	-0.58	-0.16	-0.01	-0.33
			(0.20)	(0.39)	(0.34)		(0.38)	(0.40)	(1.03)	(0.38)	(0.25)	(0.41)
$growth_{t-1} \times EAR_t$				-0.16	-0.13				0.01		-0.12	
				(0.23)	(0.23)				(0.20)		(0.19)	
$growth_{t-1} \times election_t$				0.15	-0.16				0.94		-0.64	
$\times EAR_t$				(0.46)	(0.43)				(1.09)		(0.68)	
EAR_t				-0.01	-0.00				0.01		-0.01	
				(0.02)	(0.02)				(0.02)		(0.02)	
$election_t \times EAR_t$				0.05	0.05				-0.04		-0.01	
				(0.04)	(0.04)				(0.05)		(0.04)	
election (other year)$_t$						-0.02	-0.02	-0.01		0.00		-0.02
						(0.02)	(0.02)	(0.02)		(0.02)		(0.02)
$growth_{t-1}$							-0.08	-0.05		0.12		-0.01
$\times$ *election (other year)*$_t$							(0.22)	(0.22)		(0.19)		(0.18)
election												0.03
(executive + legislative)$_t$												(0.03)
$growth_{t-1} \times$ *election*												-0.94
(executive + legislative)$_t$												(0.48)
GDP per capita$_{t-1}$ (log)					-0.01			-0.01				
					(0.01)			(0.01)				
oil and gas					-0.01			-0.01				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.09			0.09				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	4037	4037	4037	4037	3817	4037	4037	3817	4037	4037	4037	4037
regimes	255	255	255	255	244	255	255	244	255	255	255	255
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *coup attempt*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

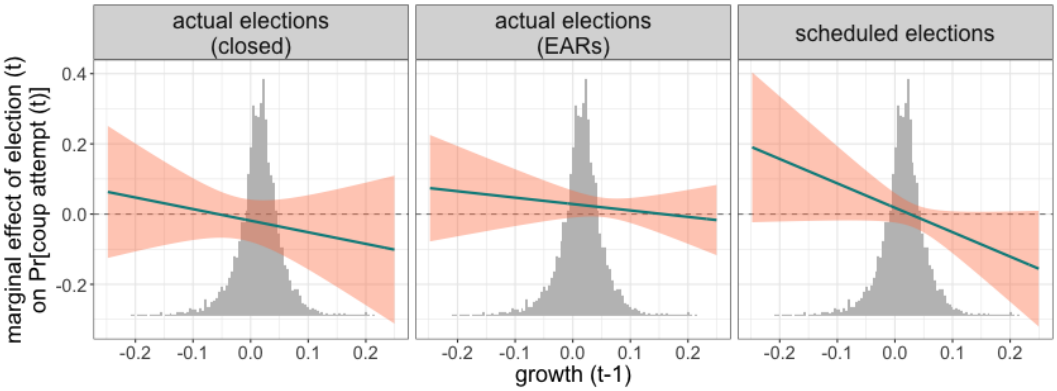


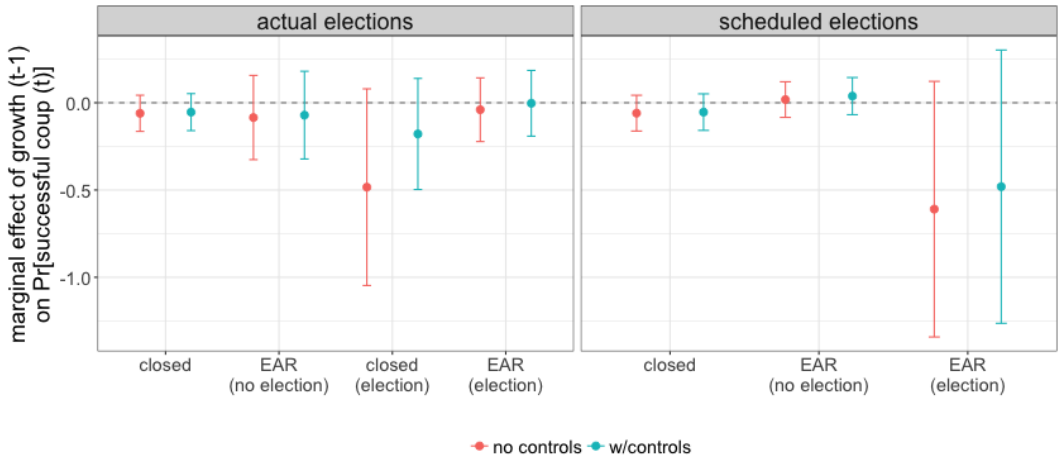
Figure A7: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $coup\ attempt_t$, based on the results reported in Table A13. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A14: Alternative outcomes (7): Successful coup

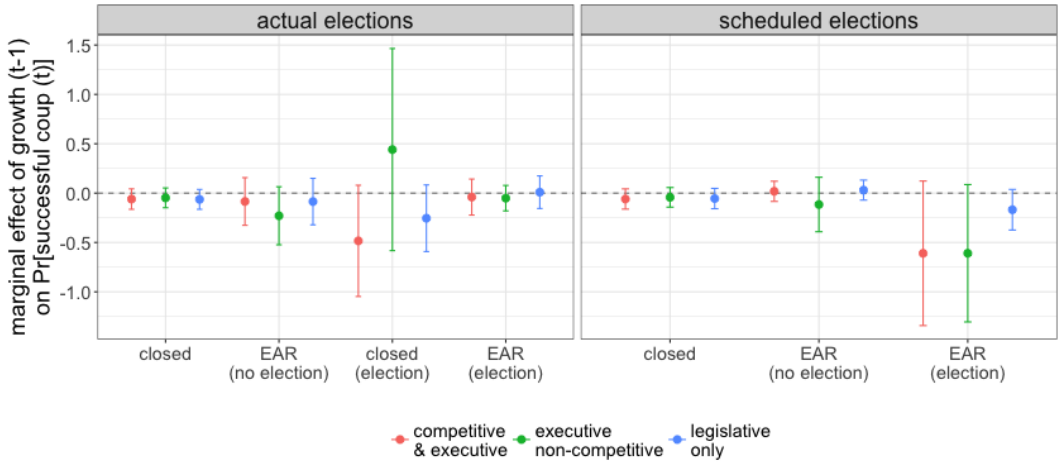
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.07		-0.07	-0.06	-0.05		-0.06	-0.05	-0.05	-0.04	-0.06	-0.05
	(0.05)		(0.05)	(0.05)	(0.05)		(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
$election_t$		0.00	0.00	-0.01	-0.01	-0.00	0.01	0.01	-0.01	0.02	0.00	-0.01
		(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
$growth_{t-1} \times election_t$			-0.11	-0.42	-0.13		-0.55	-0.43	0.49	-0.57	-0.19	-0.11
			(0.13)	(0.30)	(0.17)		(0.38)	(0.40)	(0.52)	(0.36)	(0.17)	(0.11)
$growth_{t-1} \times EAR_t$				-0.02	-0.02				-0.18		-0.02	
				(0.13)	(0.14)				(0.16)		(0.13)	
$growth_{t-1} \times election_t$				0.47	0.19				-0.31		0.29	
$\times EAR_t$				(0.32)	(0.22)				(0.53)		(0.21)	
EAR_t				-0.00	0.00				0.01		-0.00	
				(0.01)	(0.01)				(0.02)		(0.01)	
$election_t \times EAR_t$				0.01	0.02				-0.03		-0.01	
				(0.03)	(0.02)				(0.04)		(0.02)	
election (other year)$_t$						-0.01	-0.01	-0.01		0.00		-0.02
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							0.08	0.09		-0.07		0.09
$\times$ *election (other year)*$_t$							(0.07)	(0.08)		(0.15)		(0.07)
election												0.02
(executive + legislative)$_t$												(0.02)
$growth_{t-1} \times$ *election*												-0.77
(executive + legislative)$_t$												(0.48)
GDP per capita$_{t-1}$ (log)					-0.01			-0.01				
					(0.01)			(0.01)				
oil and gas					-0.01			-0.01				
per capita$_{t-1}$ (log)					(0.00)			(0.00)				
proportion of					0.05			0.05				
democratic neighbors$_t$					(0.04)			(0.04)				
observations	4037	4037	4037	4037	3817	4037	4037	3817	4037	4037	4037	4037
regimes	255	255	255	255	244	255	255	244	255	255	255	255
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *successful coup*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

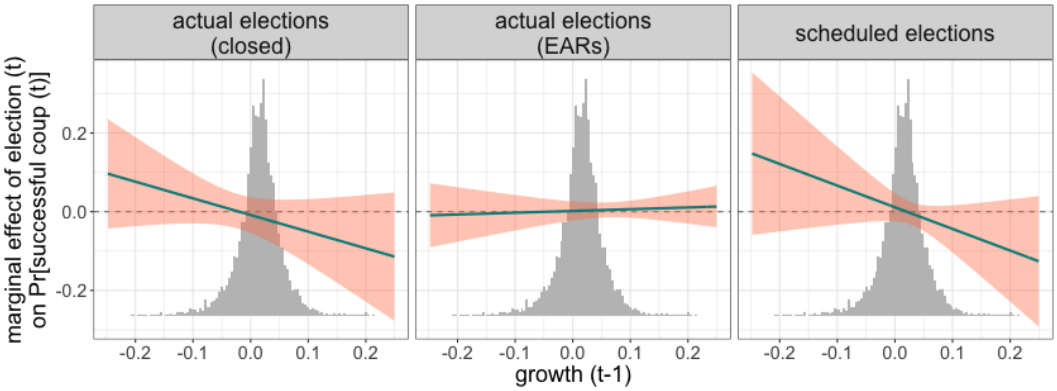


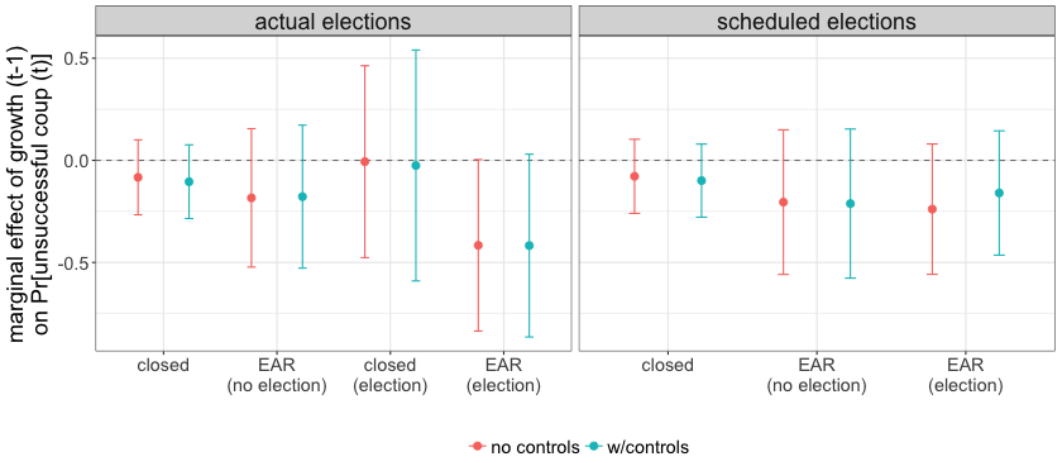
Figure A8: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of *successful coup*$_t$, based on the results reported in Table A14. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A15: Alternative outcomes (8): Unsuccessful coup

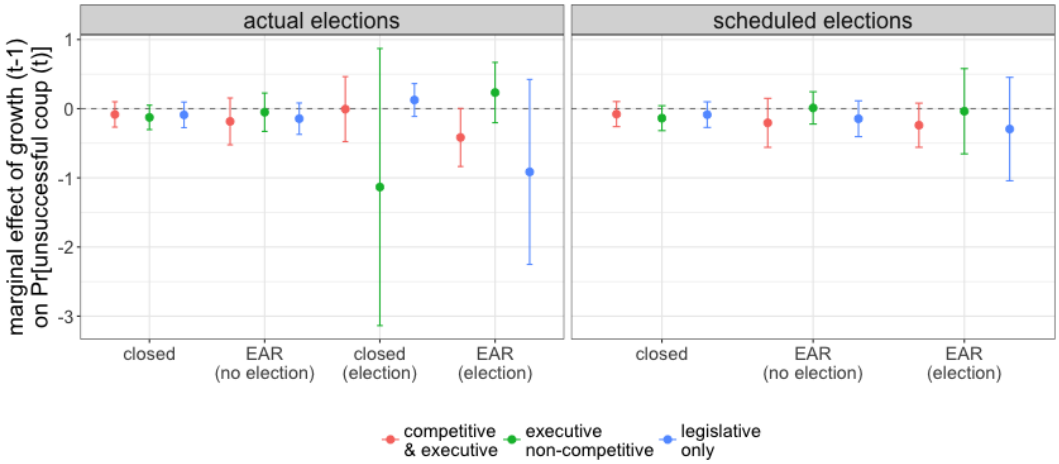
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.11		-0.11	-0.08	-0.10		-0.08	-0.10	-0.13	-0.14	-0.09	-0.09
	(0.08)		(0.08)	(0.09)	(0.09)		(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
$election_t$		0.01	0.01	-0.01	-0.02	0.00	0.00	0.01	-0.00	0.01	0.01	0.00
		(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.04)	(0.02)	(0.03)	(0.02)
$growth_{t-1} \times election_t$			-0.10	0.08	0.08		-0.16	-0.06	-1.01	0.10	0.21	-0.21
			(0.16)	(0.26)	(0.30)		(0.19)	(0.19)	(1.00)	(0.33)	(0.16)	(0.40)
$growth_{t-1} \times EAR_t$				-0.10	-0.07				0.07		-0.06	
				(0.20)	(0.20)				(0.17)		(0.15)	
$growth_{t-1} \times election_t$				-0.31	-0.32				1.29		-0.98	
$\times EAR_t$				(0.34)	(0.38)				(1.05)		(0.63)	
EAR_t				-0.02	-0.01				0.00		-0.01	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				0.03	0.04				-0.00		0.01	
				(0.03)	(0.03)				(0.04)		(0.04)	
election (other year)$_t$						-0.01	-0.01	-0.00		-0.01		-0.01
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							-0.13	-0.11		0.15		-0.06
$\times$ *election (other year)*$_t$							(0.20)	(0.21)		(0.15)		(0.16)
election												0.01
(executive + legislative)$_t$												(0.02)
$growth_{t-1} \times$ *election*												-0.18
(executive + legislative)$_t$												(0.23)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.01)			(0.01)				
oil and gas					-0.01			-0.01				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.04			0.04				
democratic neighbors$_t$					(0.04)			(0.04)				
observations	4037	4037	4037	4037	3817	4037	4037	3817	4037	4037	4037	4037
regimes	255	255	255	255	244	255	255	244	255	255	255	255
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *unsuccessful coup*$_t$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

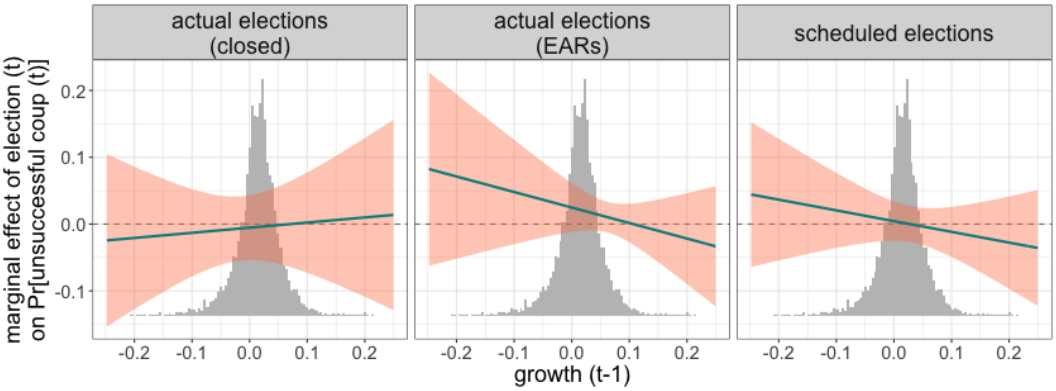


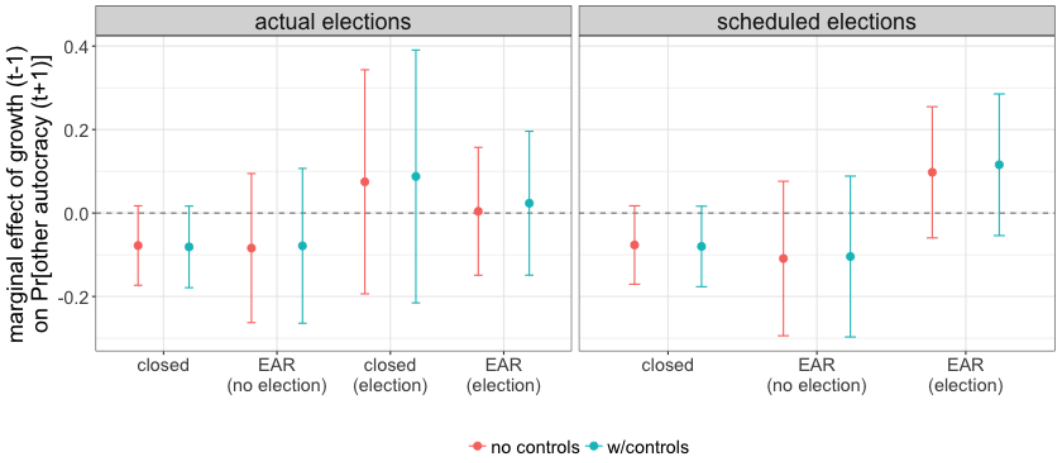
Figure A9: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of *unsuccessful coup*$_t$, based on the results reported in Table A15. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A16: Alternative outcomes (9): Another autocracy at $t+1$

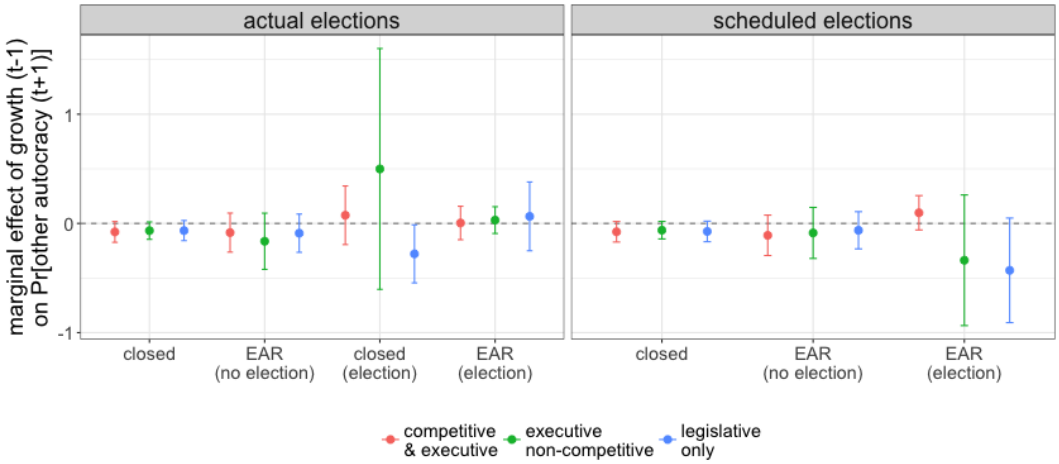
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.07		-0.08	-0.08	-0.08		-0.08	-0.08	-0.07	-0.06	-0.07	-0.07
	(0.04)		(0.04)	(0.05)	(0.05)		(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
$election_t$		-0.01	-0.01	-0.01	-0.01	-0.00	-0.01	-0.01	0.03	0.02	-0.01	0.00
		(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.05)	(0.02)	(0.02)	(0.02)
$growth_{t-1} \times election_t$			0.11	0.15	0.17		0.17	0.20	0.56	-0.28	-0.21	-0.36
			(0.08)	(0.15)	(0.16)		(0.09)	(0.10)	(0.56)	(0.31)	(0.14)	(0.25)
$growth_{t-1} \times EAR_t$				-0.01	0.00				-0.10		-0.02	
				(0.10)	(0.11)				(0.13)		(0.10)	
$growth_{t-1} \times election_t$				-0.06	-0.07				-0.37		0.37	
$\times EAR_t$				(0.18)	(0.19)				(0.57)		(0.23)	
EAR_t				-0.01	-0.00				0.01		-0.00	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				0.00	0.00				-0.06		-0.00	
				(0.02)	(0.02)				(0.05)		(0.02)	
election (other year)$_t$						-0.01	-0.00	-0.00		0.00		-0.01
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							-0.03	-0.02		-0.03		0.01
$\times$ *election (other year)*$_t$							(0.11)	(0.11)		(0.12)		(0.10)
election												-0.00
(executive + legislative)$_t$												(0.01)
$growth_{t-1} \times$ *election*												0.24
(executive + legislative)$_t$												(0.11)
GDP per capita$_{t-1}$ (log)					-0.01			-0.01				
					(0.01)			(0.01)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.00)			(0.00)				
proportion of					0.01			0.01				
democratic neighbors$_t$					(0.03)			(0.03)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using *other autocracy*$_{t+1}$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

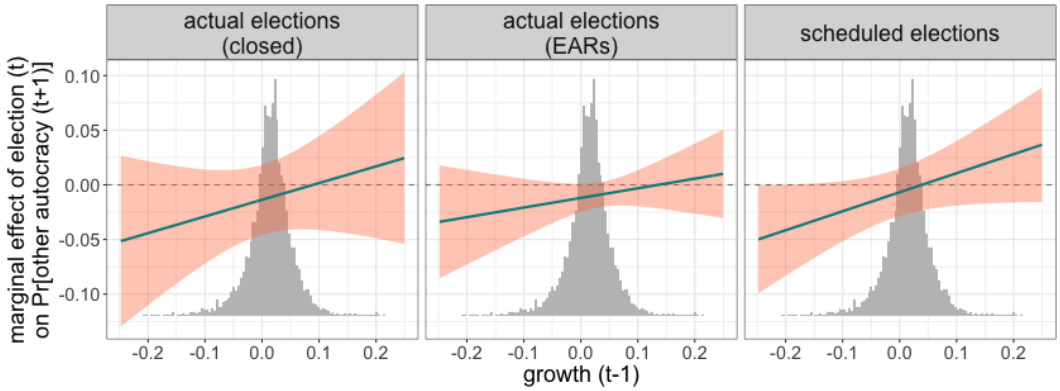


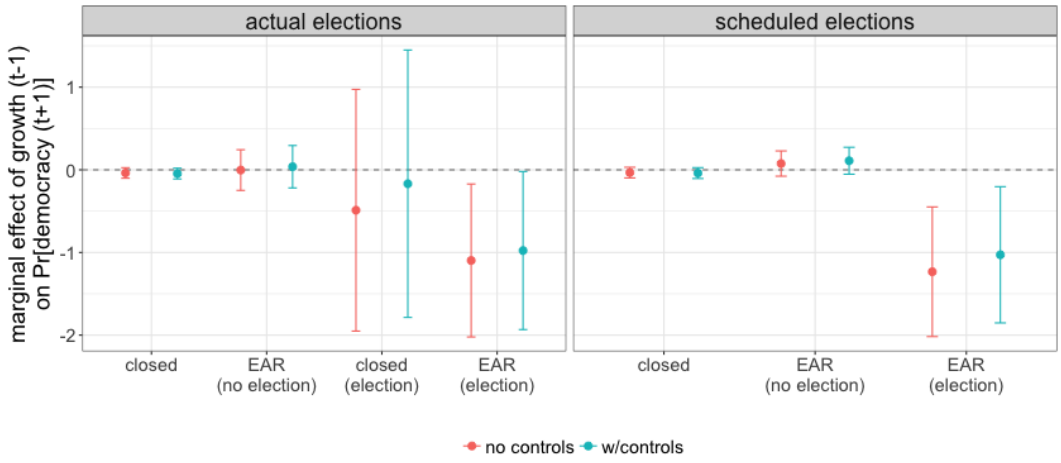
Figure A10: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of *other autocracy*$_{t+1}$, based on the results reported in Table A16. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A17: Alternative outcomes (10): Democracy (or provisional government) at $t+1$

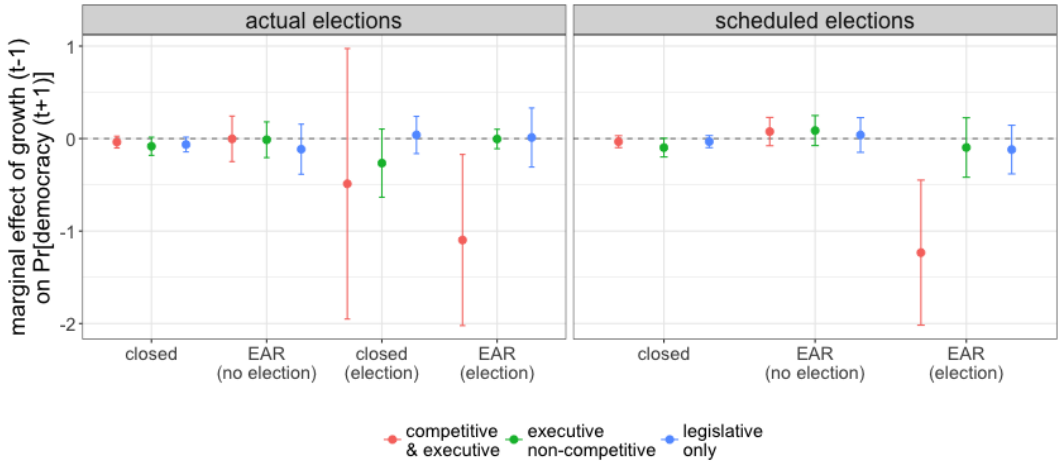
		actual elections				**scheduled elections**			**non-competitive**		**legislative**	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.07		-0.03	-0.04	-0.05		-0.03	-0.04	-0.08	-0.10	-0.06	-0.03
	(0.04)		(0.04)	(0.03)	(0.03)		(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)
$election_t$		0.09	0.10	0.12	0.12	0.10	0.13	0.12	-0.03	-0.00	0.02	0.04
		(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
$growth_{t-1} \times election_t$			-0.87	-0.45	-0.12		-1.20	-0.99	-0.18	0.00	0.10	-0.09
			(0.35)	(0.74)	(0.82)		(0.40)	(0.42)	(0.19)	(0.17)	(0.10)	(0.14)
$growth_{t-1} \times EAR_t$				0.03	0.08				0.07		-0.05	
				(0.13)	(0.13)				(0.11)		(0.14)	
$growth_{t-1} \times election_t$				-0.64	-0.89				0.19		0.02	
$\times EAR_t$				(0.89)	(0.96)				(0.21)		(0.23)	
EAR_t				0.01	0.02				0.01		0.01	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				-0.01	-0.02				0.02		-0.04	
				(0.05)	(0.05)				(0.02)		(0.02)	
election (other year)$_t$						0.01	-0.00	0.00		-0.01		0.01
						(0.01)	(0.01)	(0.01)		(0.01)		(0.01)
$growth_{t-1}$							0.11	0.15		0.18		0.07
$\times$ *election (other year)*$_t$							(0.08)	(0.09)		(0.10)		(0.10)
election												0.14
(executive + legislative)$_t$												(0.03)
$growth_{t-1} \times$ *election*												-1.29
(executive + legislative)$_t$												(0.46)
GDP per capita$_{t-1}$ (log)					0.01			0.01				
					(0.01)			(0.01)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.00)			(0.00)				
proportion of					0.09			0.10				
democratic neighbors$_t$					(0.04)			(0.04)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. Specifications replicate those reported in Table 1, but using $democracy_{t+1}$ as the outcome variable. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

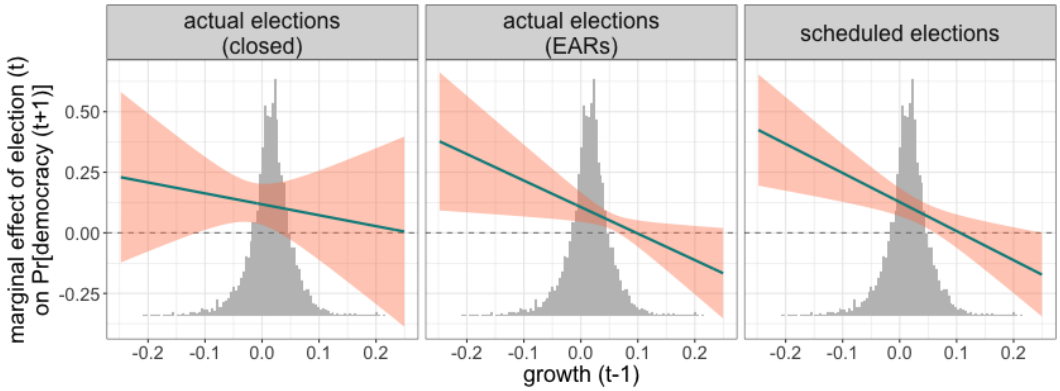


Figure A11: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $democracy_{t+1}$, based on the results reported in Table A17. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A18: Alternative Outcomes (11): Electoral manipulation

	harassment (NELDA)			**free and fair (V-Dem)**			**irregularities (V-Dem)**			**intimidation (V-Dem)**		
(a) growth_{t-1} *only*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-1.29	-0.84	0.58	2.08	0.89	0.22	-3.11	-0.73	0.27	-0.53	0.64	-0.40
	(1.52)	(1.61)	(0.84)	(1.87)	(1.61)	(0.72)	(2.07)	(1.15)	(0.73)	(1.56)	(1.53)	(0.71)
GDP per capita$_{t-1}$ (log)		-0.17			0.29			-0.46			-0.28	
		(0.17)			(0.12)			(0.17)			(0.13)	
oil and gas per capita$_{t-1}$ (log)		-0.04			-0.08			0.10			0.03	
		(0.04)			(0.04)			(0.05)			(0.03)	
proportion of democratic neighbors$_t$		-0.04			0.59			-1.19			-0.67	
		(0.78)			(0.74)			(0.72)			(0.66)	
party-based regime		0.11			-0.05			-0.08			0.28	
		(0.28)			(0.24)			(0.21)			(0.24)	
military regime		0.28			-0.37			0.43			0.17	
		(0.31)			(0.29)			(0.26)			(0.26)	
personalist regime		0.48			-0.71			0.79			0.71	
		(0.29)			(0.25)			(0.22)			(0.26)	
Cold War$_t$		-0.10			-0.29			-0.20			-0.09	
		(0.33)			(0.26)			(0.26)			(0.27)	
(Intercept)	-0.32	1.19		-0.76	-2.86		0.97	4.79		0.88	2.97	
	(0.11)	(1.46)		(0.13)	(1.08)		(0.13)	(1.39)		(0.11)	(1.06)	
(b) *Conditioning on* breakdown_t												
$growth_{t-1}$	-1.69	-1.77	0.55	3.50	2.58	0.35	-4.38	-2.08	0.28	-1.50	-0.72	-0.41
	(1.69)	(1.90)	(0.89)	(1.95)	(1.61)	(0.70)	(2.25)	(1.17)	(0.76)	(1.65)	(1.57)	(0.70)
$breakdown_t$	-0.27	-0.45	-0.04	1.07	1.04	0.77	-0.93	-0.95	-0.34	-0.82	-0.82	-0.46
	(0.25)	(0.29)	(0.13)	(0.22)	(0.22)	(0.27)	(0.21)	(0.19)	(0.21)	(0.19)	(0.18)	(0.24)
$growth_{t-1} \times breakdown_t$	1.34	3.80	0.25	-3.42	-4.66	4.08	3.21	2.85	-3.33	1.67	3.82	-3.88
	(4.23)	(4.78)	(2.51)	(3.68)	(3.73)	(3.01)	(3.42)	(3.24)	(2.75)	(2.94)	(3.59)	(3.74)
GDP per capita$_{t-1}$ (log)		-0.18			0.31			-0.47			-0.29	
		(0.18)			(0.13)			(0.17)			(0.13)	
oil and gas per capita$_{t-1}$ (log)		-0.04			-0.07			0.09			0.02	
		(0.04)			(0.04)			(0.05)			(0.03)	
proportion of democratic neighbors$_t$		-0.04			0.57			-1.16			-0.65	
		(0.77)			(0.61)			(0.62)			(0.55)	
party-based regime		0.13			-0.05			-0.10			0.28	
		(0.29)			(0.19)			(0.20)			(0.22)	
military regime		0.32			-0.45			0.49			0.23	
		(0.31)			(0.24)			(0.24)			(0.24)	
personalist regime		0.47			-0.61			0.67			0.62	
		(0.30)			(0.21)			(0.20)			(0.26)	
Cold War$_t$		-0.13			-0.17			-0.31			-0.18	
		(0.33)			(0.22)			(0.23)			(0.24)	
(Intercept)	-0.28	1.38		-0.93	-3.33		1.11	5.19		1.01	3.35	
	(0.12)	(1.51)		(0.13)	(1.10)		(0.14)	(1.40)		(0.12)	(1.04)	
specification	probit	probit	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
region FEs	no	yes	no	no	yes	no	no	yes	no	no	yes	no
regime FEs	no	no	yes	no	no	yes	no	no	yes	no	no	yes
observations	268	254	268	267	253	267	267	253	267	267	253	267
regimes	105	105	105	104	104	104	104	104	104	104	104	104
countries	80	80	80	79	79	79	79	79	79	79	79	79

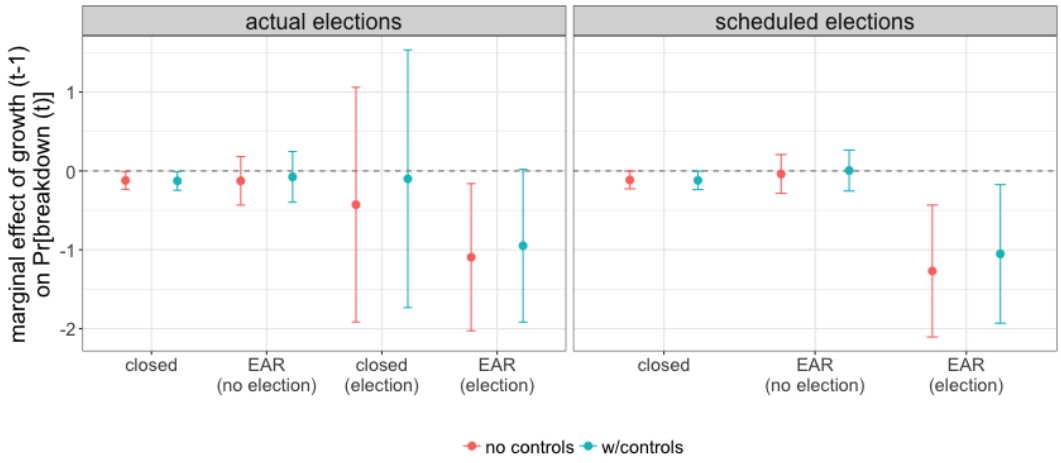
The dependent variables are alternative measures of electoral manipulation: (i) whether there is evidence that the government harassed the opposition (from NELDA); (ii) whether elections qualify as "free and fair;" (iii) the extent to which there is evidence of *intentional* irregularities by either government or opposition parties; and (iv) whether the government intimidated the opposition (from V-Dem). The sample is restricted to *competitive* elections for the *executive* office in autocracies. Robust standard errors clustered by regime in parentheses.

Table A19: Adding controls (1): *GDP per capita*$_{t-1}$ (log)

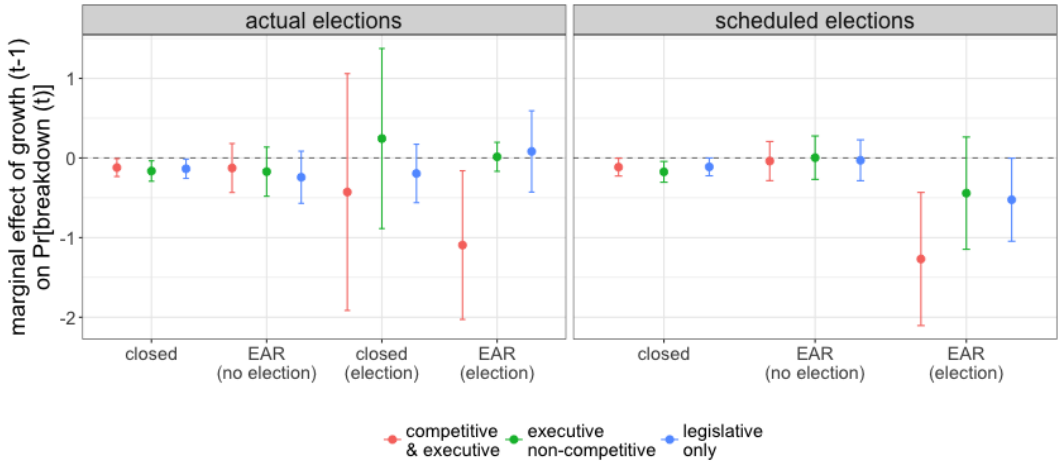
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
growth$_{t-1}$	-0.16		-0.12	-0.12	-0.13		-0.11	-0.12	-0.16	-0.17	-0.14	-0.11
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
election$_t$		0.07	0.09	0.10	0.10	0.10	0.12	0.12	-0.01	0.02	0.01	0.04
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
growth$_{t-1}$ × *election*$_t$			-0.74	-0.31	0.03		-1.15	-0.93	0.41	-0.27	-0.06	-0.41
			(0.36)	(0.76)	(0.83)		(0.43)	(0.45)	(0.58)	(0.37)	(0.19)	(0.27)
growth$_{t-1}$ × *EAR*$_t$				-0.01	0.05				-0.01		-0.11	
				(0.17)	(0.17)				(0.17)		(0.18)	
growth$_{t-1}$ × *election*$_t$				-0.66	-0.90				-0.22		0.38	
× *EAR*$_t$				(0.90)	(0.97)				(0.59)		(0.35)	
EAR$_t$				0.01	0.02				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
election$_t$ × *EAR*$_t$				-0.01	-0.02				-0.03		-0.04	
				(0.06)	(0.06)				(0.05)		(0.03)	
election (other year)$_t$						-0.00	-0.01	-0.00		-0.01		0.00
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
growth$_{t-1}$							0.08	0.12		0.18		0.08
× *election (other year)*$_t$							(0.14)	(0.15)		(0.15)		(0.14)
election												0.14
(executive + legislative)$_t$												(0.04)
growth$_{t-1}$ × *election*												-1.22
(executive + legislative)$_t$												(0.51)
GDP per capita$_{t-1}$ (log)	0.00	-0.00	0.00	0.00	-0.00	-0.00	0.00	-0.00	0.00	0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.14			0.15				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but controlling for *GDP per capita*$_{t-1}$ (log). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

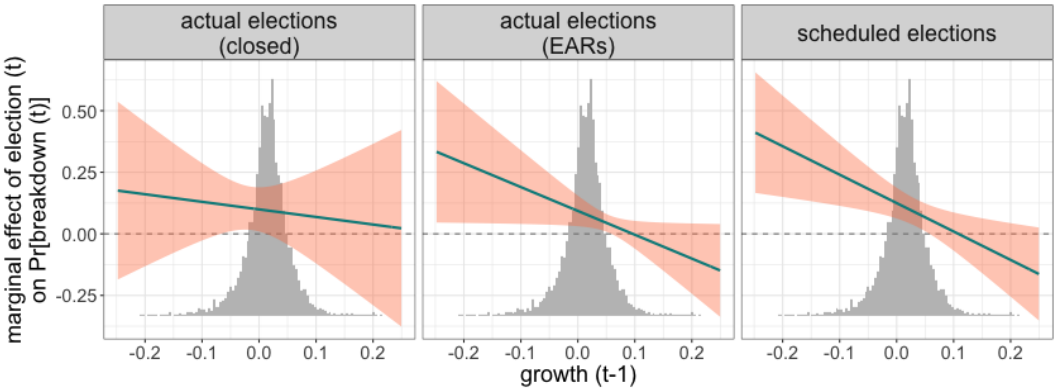


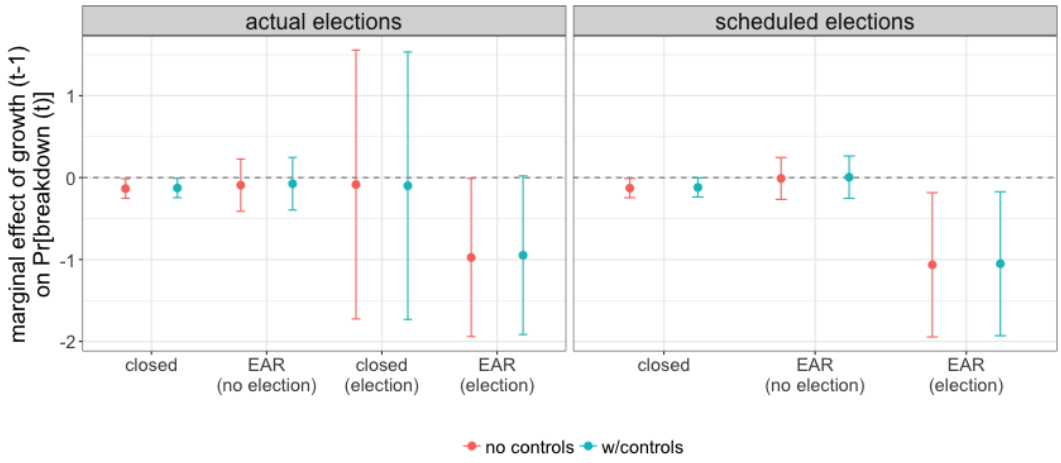
Figure A12: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A19. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A20: Adding controls (2): *oil and gas per capita*$_{t-1}$ sample

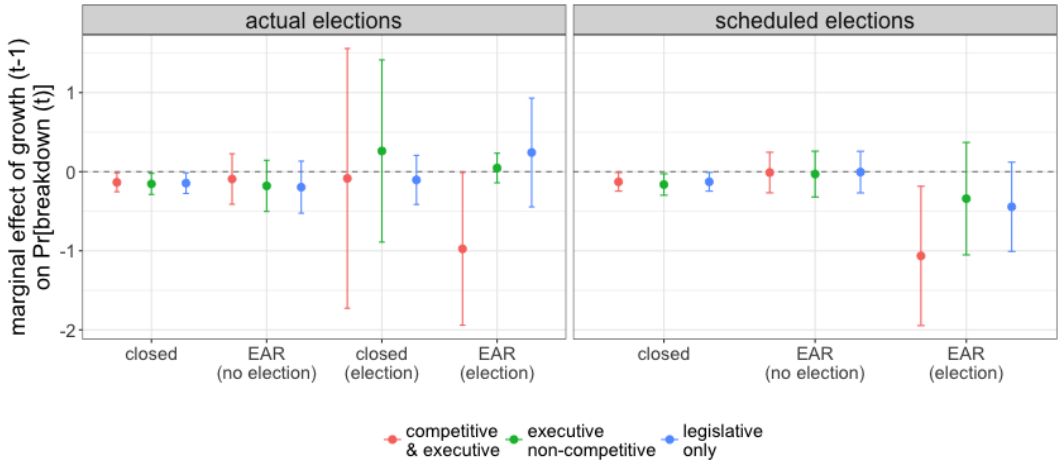
		actual elections				**scheduled elections**			**non-competitive**		**legislative**	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
growth$_{t-1}$	-0.15		-0.12	-0.13	-0.13		-0.13	-0.12	-0.15	-0.16	-0.14	-0.13
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
election$_t$		0.07	0.08	0.10	0.10	0.11	0.12	0.12	-0.01	0.01	-0.00	0.03
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
growth$_{t-1}$ × *election*$_t$			-0.59	0.05	0.03		-0.94	-0.93	0.42	-0.18	0.04	-0.32
			(0.37)	(0.83)	(0.83)		(0.45)	(0.45)	(0.59)	(0.37)	(0.16)	(0.29)
growth$_{t-1}$ × *EAR*$_t$				0.04	0.05				-0.03		-0.05	
				(0.17)	(0.17)				(0.17)		(0.18)	
growth$_{t-1}$ × *election*$_t$				-0.93	-0.90				-0.19		0.40	
× *EAR*$_t$				(0.98)	(0.97)				(0.61)		(0.42)	
EAR$_t$				0.02	0.02				0.01		0.02	
				(0.02)	(0.02)				(0.01)		(0.02)	
election$_t$ × *EAR*$_t$				-0.02	-0.02				-0.03		-0.03	
				(0.06)	(0.06)				(0.05)		(0.03)	
election (other year)$_t$						0.01	0.00	-0.00		-0.01		0.01
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
growth$_{t-1}$							0.12	0.12		0.13		0.12
× *election (other year)*$_t$							(0.14)	(0.15)		(0.16)		(0.15)
election												0.14
(executive + legislative)$_t$												(0.04)
growth$_{t-1}$ × *election*												-0.93
(executive + legislative)$_t$												(0.54)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.14			0.15				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823
regimes	244	244	244	244	244	244	244	244	244	244	244	244
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but restricting the sample to observations for which there is data for *oil and gas per capita*$_{t-1}$. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

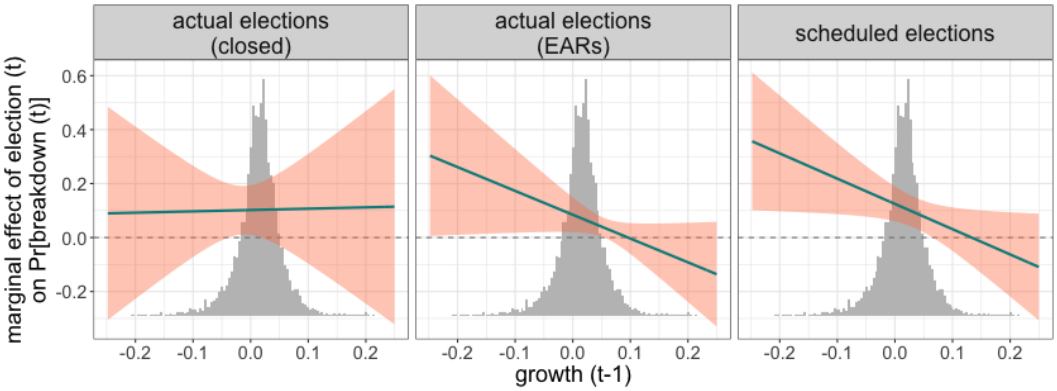


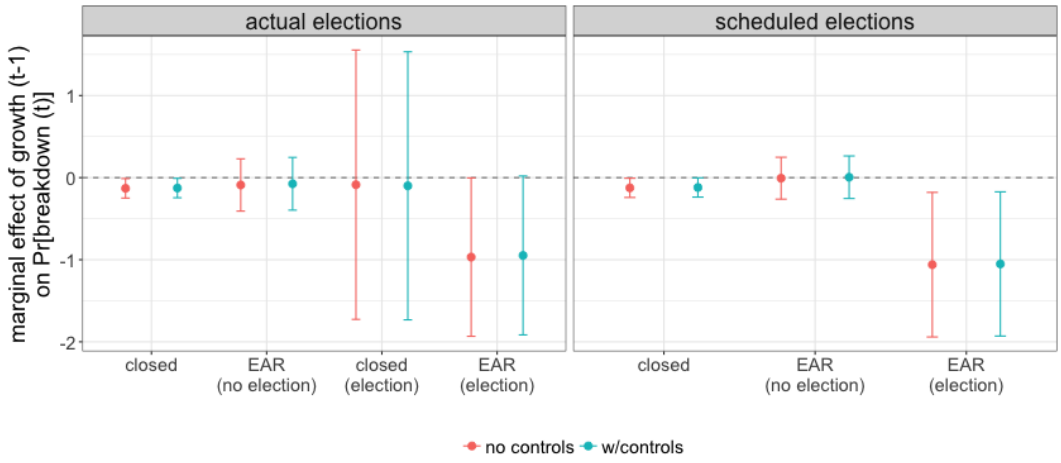
Figure A13: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A20. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A21: Adding controls (3): *oil and gas per capita*$_{t-1}$ (log)

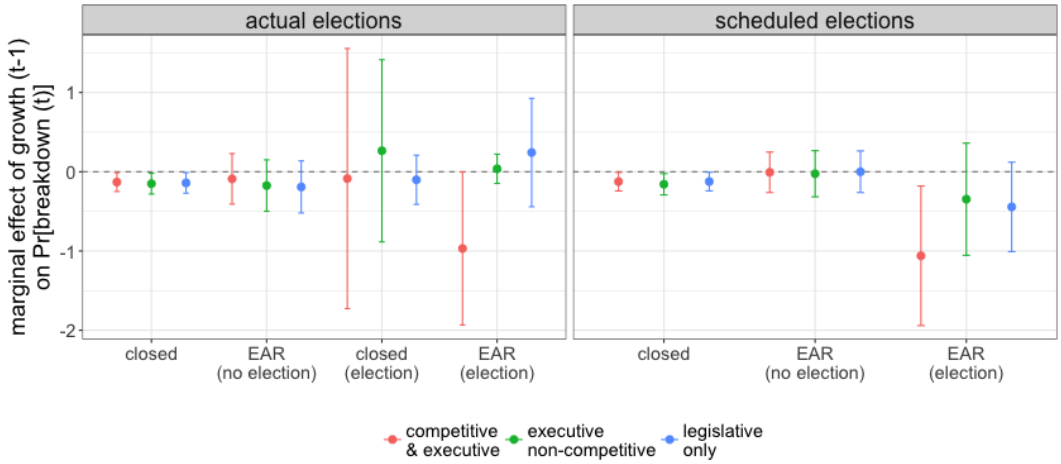
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
growth$_{t-1}$	-0.14		-0.12	-0.13	-0.13		-0.12	-0.12	-0.15	-0.16	-0.14	-0.12
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
election$_t$		0.07	0.08	0.10	0.10	0.11	0.13	0.12	-0.01	0.01	-0.00	0.04
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
growth$_{t-1}$ × *election*$_t$			-0.59	0.04	0.03		-0.94	-0.93	0.41	-0.19	0.04	-0.32
			(0.37)	(0.83)	(0.83)		(0.45)	(0.45)	(0.59)	(0.37)	(0.16)	(0.29)
growth$_{t-1}$ × *EAR*$_t$				0.04	0.05				-0.02		-0.05	
				(0.17)	(0.17)				(0.17)		(0.18)	
growth$_{t-1}$ × *election*$_t$				-0.92	-0.90				-0.20		0.39	
× *EAR*$_t$				(0.98)	(0.97)				(0.61)		(0.41)	
EAR$_t$				0.02	0.02				0.01		0.02	
				(0.02)	(0.02)				(0.01)		(0.02)	
election$_t$ × *EAR*$_t$				-0.02	-0.02				-0.03		-0.03	
				(0.06)	(0.06)				(0.05)		(0.03)	
election (other year)$_t$						0.01	0.00	-0.00		-0.01		0.01
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
growth$_{t-1}$							0.12	0.12		0.13		0.12
× *election (other year)*$_t$							(0.14)	(0.15)		(0.16)		(0.15)
election												0.14
(executive + legislative)$_t$												(0.04)
growth$_{t-1}$ × *election*												-0.93
(executive + legislative)$_t$												(0.54)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
oil and gas	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
per capita$_{t-1}$ (log)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
proportion of					0.14			0.15				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823
regimes	244	244	244	244	244	244	244	244	244	244	244	244
countries	114	114	114	114	114	114	114	114	114	114	114	114

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but controlling for *oil and gas per capita*$_{t-1}$ (log). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

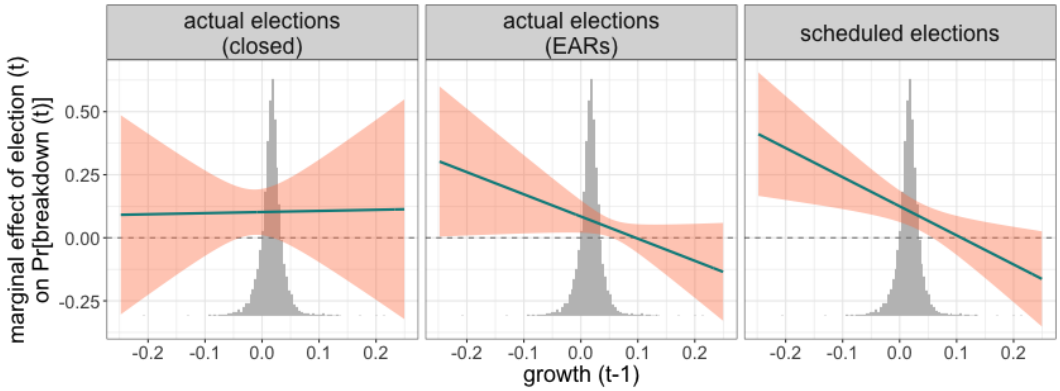


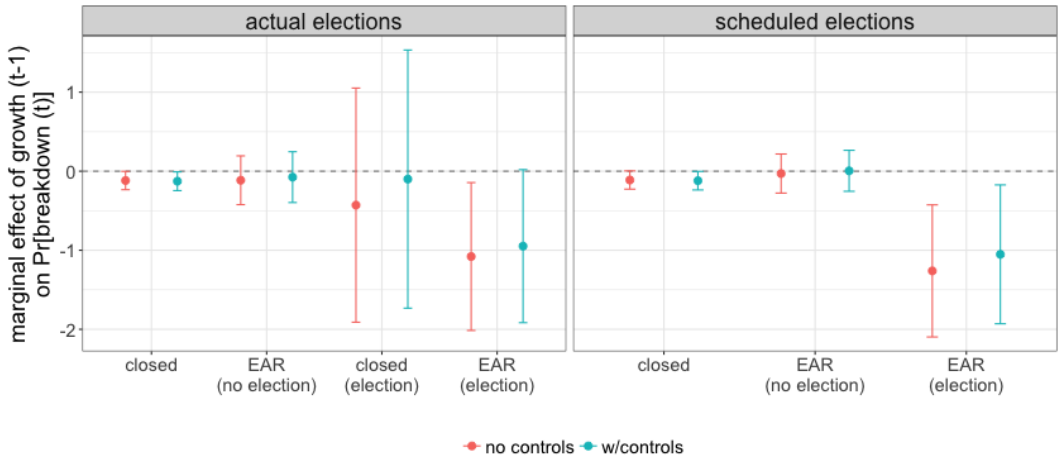
Figure A14: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A21. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A22: Adding controls (4): *proportion of democratic neighbors*$_t$

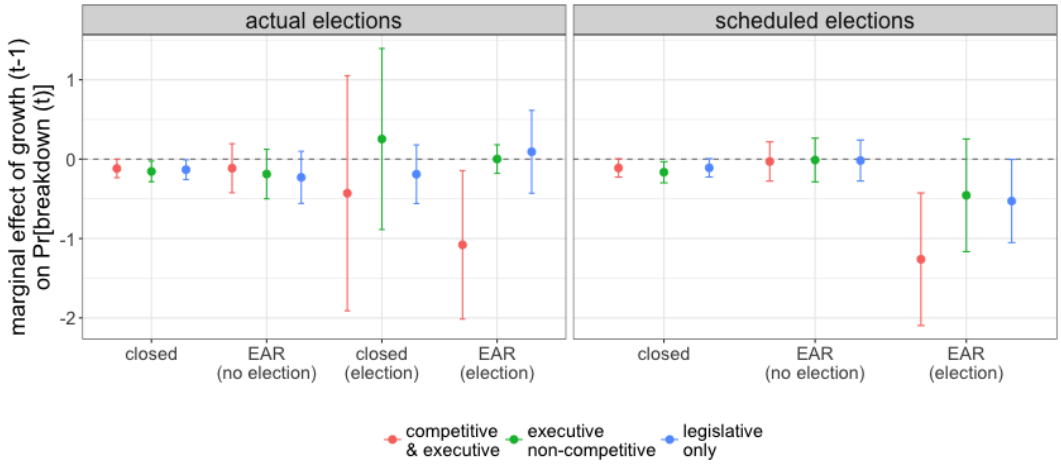
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.15		-0.12	-0.12	-0.13		-0.11	-0.12	-0.15	-0.16	-0.13	-0.11
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
$election_t$		0.07	0.09	0.10	0.10	0.10	0.12	0.12	-0.01	0.02	0.01	0.04
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.74	-0.31	0.03		-1.15	-0.93	0.41	-0.29	-0.06	-0.42
			(0.36)	(0.75)	(0.83)		(0.43)	(0.45)	(0.58)	(0.37)	(0.19)	(0.27)
$growth_{t-1} \times EAR_t$				0.00	0.05				-0.03		-0.10	
				(0.17)	(0.17)				(0.17)		(0.18)	
$growth_{t-1} \times election_t$				-0.65	-0.90				-0.22		0.38	
$\times EAR_t$				(0.90)	(0.97)				(0.60)		(0.36)	
EAR_t				0.01	0.02				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.00	-0.02				-0.03		-0.04	
				(0.06)	(0.06)				(0.05)		(0.03)	
election (other year)$_t$						-0.00	-0.01	-0.00		-0.01		0.00
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.08	0.12		0.15		0.09
$\times$ *election (other year)*$_t$							(0.14)	(0.15)		(0.15)		(0.14)
election												0.14
(executive + legislative)$_t$												(0.04)
$growth_{t-1} \times$ *election*												-1.21
(executive + legislative)$_t$												(0.51)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of	0.12	0.12	0.12	0.11	0.14	0.12	0.12	0.15	0.13	0.12	0.12	0.12
democratic neighbors$_t$	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but controlling for *proportion of democratic neighbors*$_t$. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

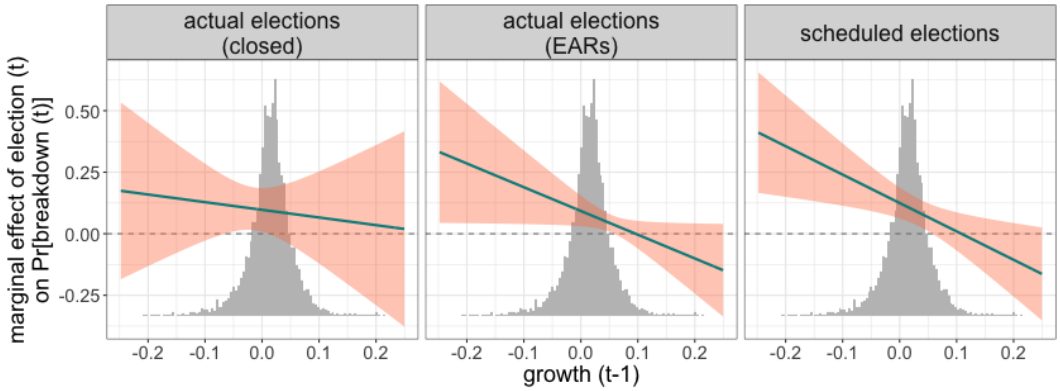


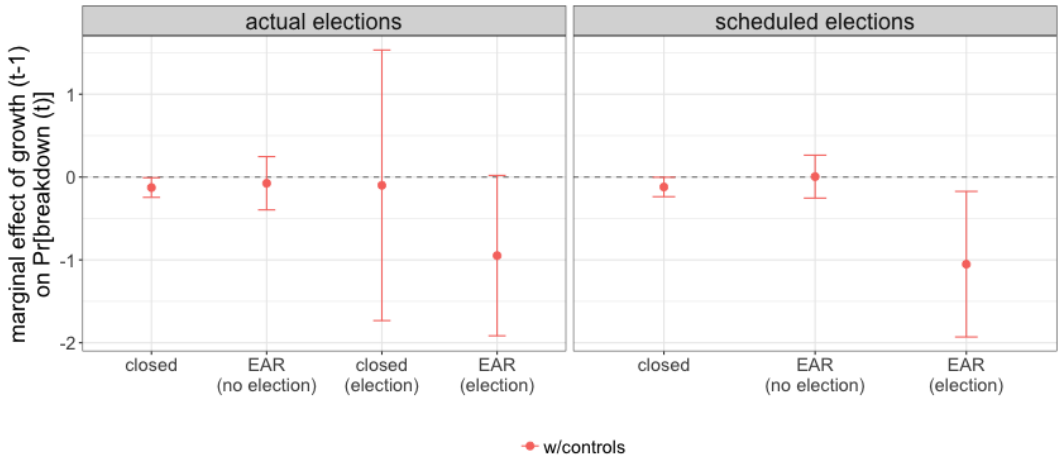
Figure A15: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A22. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A23: Adding controls (5): *GDP per capita*$_{t-1}$ (log), *oil and gas per capita*$_{t-1}$ (log) and *proportion of democratic neighbors*$_t$

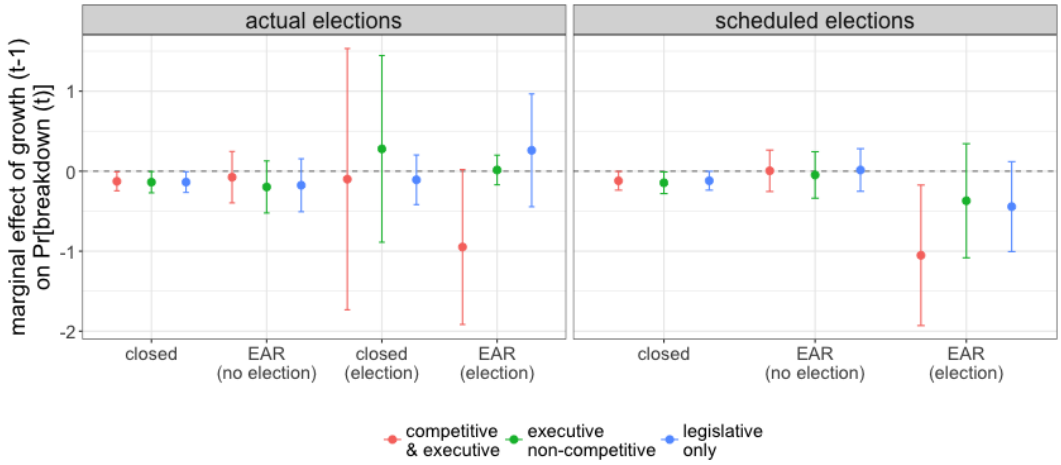
		actual elections			**scheduled elections**		**non-competitive**		**legislative**	
	growth only	election only	growth × election	growth × el. × EAR	election only	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
growth$_{t-1}$	-0.14		-0.11	-0.13		-0.12	-0.14	-0.15	-0.14	-0.12
	(0.06)		(0.06)	(0.06)		(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
election$_t$		0.07	0.08	0.10	0.10	0.12	-0.02	0.01	-0.00	0.03
		(0.02)	(0.02)	(0.05)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
growth$_{t-1}$ × *election*$_t$			-0.59	0.03		-0.93	0.42	-0.22	0.03	-0.32
			(0.37)	(0.83)		(0.45)	(0.60)	(0.37)	(0.16)	(0.29)
growth$_{t-1}$ × *EAR*$_t$				0.05			-0.06		-0.04	
				(0.17)			(0.17)		(0.18)	
growth$_{t-1}$ × *election*$_t$				-0.90			-0.20		0.41	
× *EAR*$_t$				(0.97)			(0.62)		(0.42)	
EAR$_t$				0.02			0.02		0.02	
				(0.02)			(0.01)		(0.02)	
election$_t$ × *EAR*$_t$				-0.02			-0.02		-0.03	
				(0.06)			(0.06)		(0.03)	
election (other year)$_t$					0.00	-0.00		-0.01		0.00
					(0.02)	(0.02)		(0.01)		(0.02)
growth$_{t-1}$						0.12		0.10		0.13
× *election (other year)*$_t$						(0.15)		(0.16)		(0.15)
election										0.14
(executive + legislative)$_t$										(0.04)
growth$_{t-1}$ × *election*										-0.92
(executive + legislative)$_t$										(0.54)
GDP per capita$_{t-1}$ (log)	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
oil and gas	-0.00	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
per capita$_{t-1}$ (log)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
proportion of	0.15	0.15	0.15	0.14	0.15	0.15	0.16	0.15	0.15	0.15
democratic neighbors$_t$	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
observations	3823	3823	3823	3823	3823	3823	3823	3823	3823	3823
regimes	244	244	244	244	244	244	244	244	244	244
countries	115	115	115	114	115	114	115	115	115	115

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but including all controls simultaneously. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

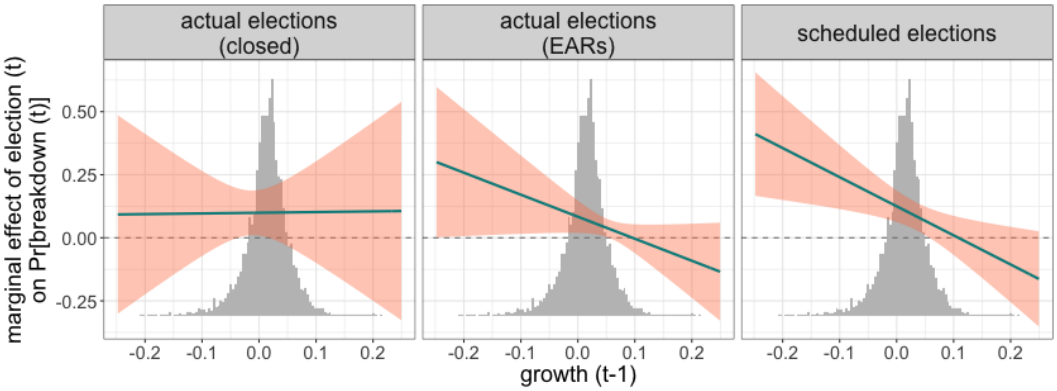


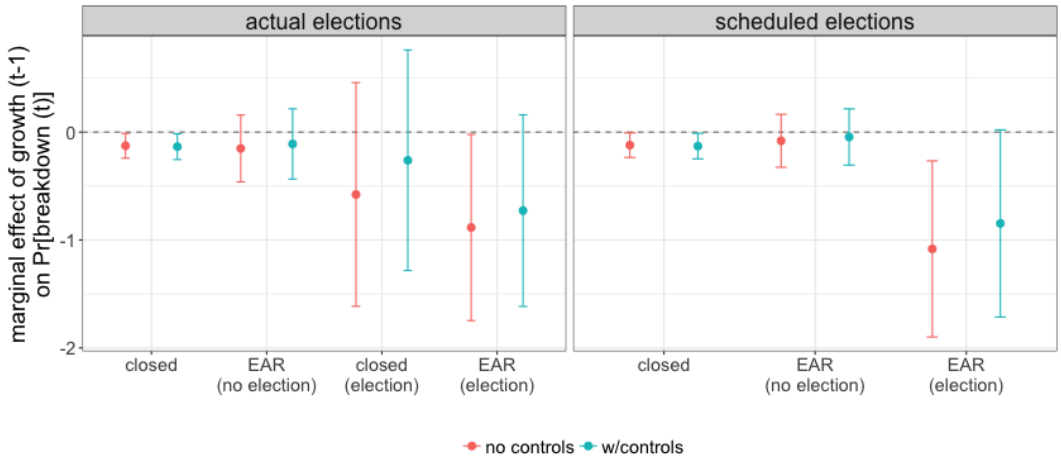
Figure A16: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A23. Panel (a) is based on model 4 (for actual elections) or 6 (for scheduled elections); panel (b), on models 4, 7 and 9 (actual) or 6, 8 and 10 (scheduled); while results in panel (c) correspond to models 4 and 6, respectively.

Table A24: Alternative breakdown dates

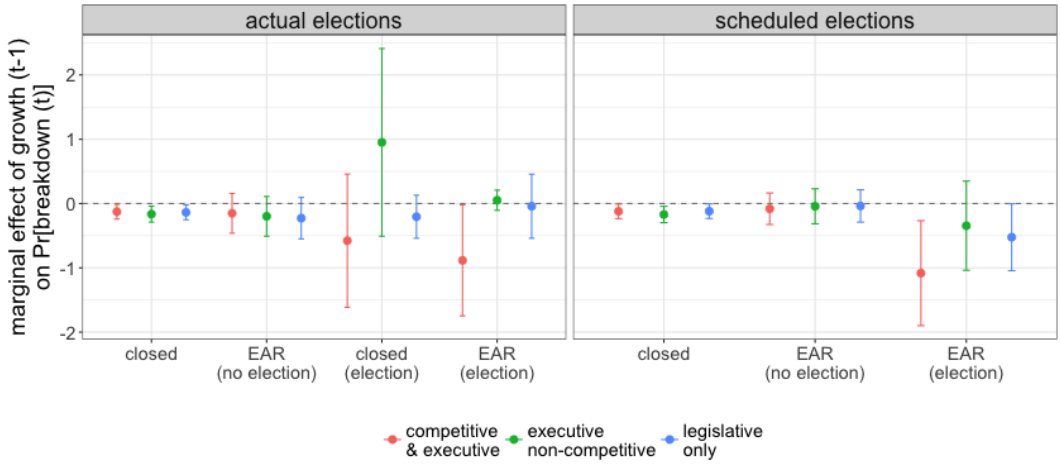
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.16		-0.13	-0.13	-0.13		-0.12	-0.13	-0.16	-0.17	-0.14	-0.12
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
$election_t$		0.05	0.06	0.04	0.04	0.06	0.08	0.08	0.07	0.03	-0.01	0.01
		(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)	(0.06)	(0.03)	(0.02)	(0.02)
$growth_{t-1} \times election_t$			-0.54	-0.45	-0.13		-0.96	-0.72	1.12	-0.17	-0.07	-0.40
			(0.31)	(0.53)	(0.52)		(0.42)	(0.44)	(0.75)	(0.36)	(0.17)	(0.27)
$growth_{t-1} \times EAR_t$				-0.02	0.03				-0.03		-0.09	
				(0.17)	(0.18)				(0.17)		(0.17)	
$growth_{t-1} \times election_t$				-0.28	-0.49				-0.87		0.25	
$\times EAR_t$				(0.70)	(0.70)				(0.75)		(0.34)	
EAR_t				0.00	0.01				0.03		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				0.04	0.03				-0.11		0.00	
				(0.05)	(0.05)				(0.06)		(0.03)	
$election\ (other\ year)_t$						-0.01	-0.02	-0.01		-0.00		-0.02
						(0.01)	(0.01)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.04	0.08		0.13		0.08
$\times\ election\ (other\ year)_t$							(0.14)	(0.15)		(0.15)		(0.14)
election												0.10
$(executive + legislative)_t$												(0.03)
$growth_{t-1} \times$ *election*												-1.11
$(executive + legislative)_t$												(0.50)
$GDP\ per\ capita_{t-1}$ (log)					0.01			0.01				
					(0.02)			(0.02)				
oil and gas					-0.01			-0.01				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.11			0.11				
$democratic\ neighbors_t$					(0.06)			(0.06)				
observations	4021	4021	4021	4021	3783	4021	4021	3783	4021	4021	4021	4021
regimes	256	256	256	256	242	256	256	242	256	256	256	256
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but coding $breakdown_t$ in a more restrictive way as explained in the text. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

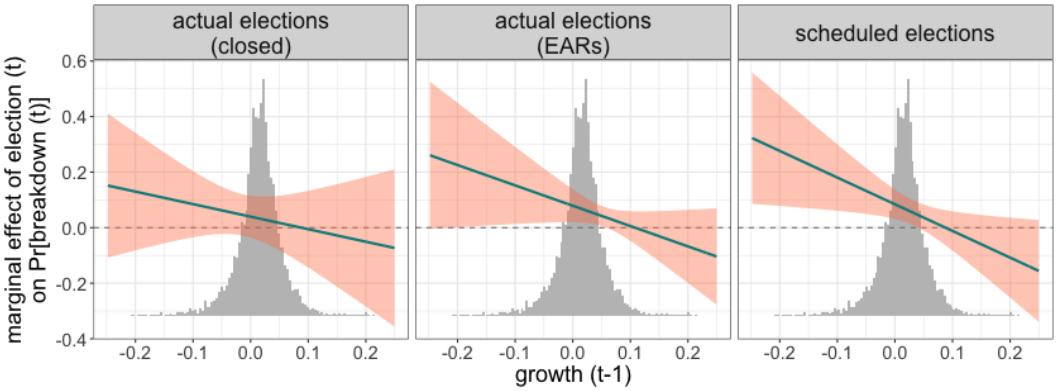


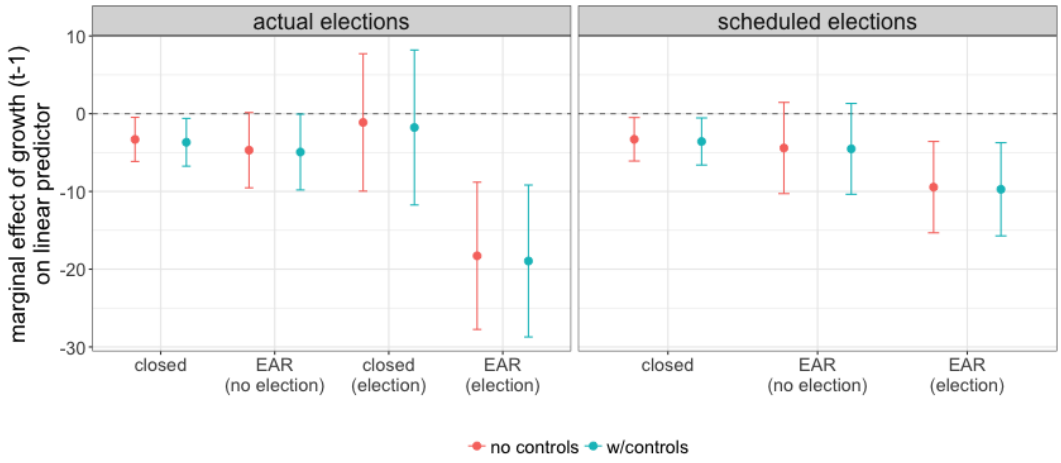
Figure A17: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A24. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A25: Survival models (1): Cloglog link

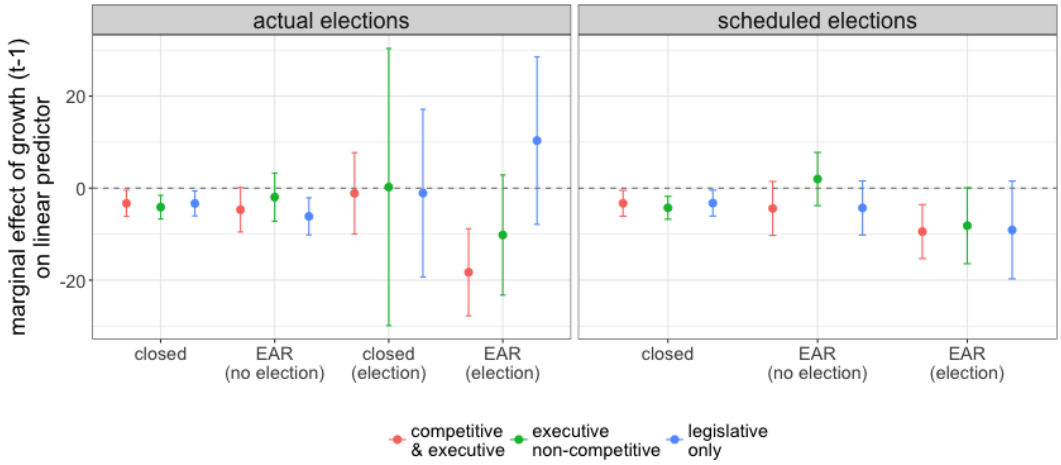
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-3.83		-3.67	-3.31	-3.68		-3.29	-3.57	-4.13	-4.28	-3.33	-3.25
	(1.31)		(1.42)	(1.53)	(1.67)		(1.49)	(1.64)	(1.42)	(1.37)	(1.49)	(1.51)
$election_t$		1.13	1.20	1.35	1.37	1.40	1.50	1.43	0.06	0.57	0.01	0.32
		(0.20)	(0.21)	(0.29)	(0.30)	(0.24)	(0.24)	(0.26)	(0.72)	(0.36)	(0.62)	(0.41)
$growth_{t-1} \times election_t$			-4.48	2.18	1.90		-6.15	-6.15	4.36	-3.89	2.24	-5.83
			(3.58)	(4.95)	(6.11)		(3.03)	(3.15)	(10.80)	(3.71)	(4.66)	(4.69)
$growth_{t-1} \times EAR_t$				-1.38	-1.25				2.18		-2.80	
				(3.18)	(3.24)				(3.05)		(2.62)	
$growth_{t-1} \times election_t$				-15.77	-15.92				-12.60		14.23	
$\times EAR_t$				(7.79)	(8.66)				(12.16)		(6.79)	
EAR_t				0.17	0.16				0.60		0.32	
				(0.24)	(0.25)				(0.19)		(0.21)	
$election_t \times EAR_t$				-0.15	-0.26				-1.32		-1.27	
				(0.41)	(0.43)				(1.08)		(0.89)	
$election\ (other\ year)_t$						-0.09	-0.08	-0.10		0.09		-0.07
						(0.26)	(0.26)	(0.29)		(0.22)		(0.26)
$growth_{t-1}$							-1.12	-0.95		6.26		-1.05
$\times\ election\ (other\ year)_t$							(3.44)	(3.49)		(3.38)		(3.41)
$election$												1.55
$(executive + legislative)_t$												(0.25)
$growth_{t-1} \times election$												-5.67
$(executive + legislative)_t$												(3.10)
$GDP\ per\ capita_{t-1}$ (log)	-0.18	-0.23	-0.18	-0.19	-0.10	-0.22	-0.17	-0.07	-0.17	-0.19	-0.19	-0.16
	(0.13)	(0.13)	(0.13)	(0.13)	(0.16)	(0.13)	(0.13)	(0.16)	(0.13)	(0.13)	(0.13)	(0.13)
$oil\ and\ gas$					-0.06			-0.06				
$per\ capita_{t-1}$ (log)					(0.04)			(0.04)				
$proportion\ of$					2.36			2.51				
$democratic\ neighbors_t$					(0.67)			(0.70)				
AIC	1564	1548	1538	1539	1424	1536	1525	1412	1565	1564	1569	1536
BIC	2062	2046	2049	2075	1967	2040	2049	1943	2101	2088	2105	2072
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061

GLS regression estimates with a cloglog link. The dependent variable is $breakdown_t$. All specifications include dummies for regime type as well as region and year fixed effects and a duration polynomial of order 3; estimates not reported to save space. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

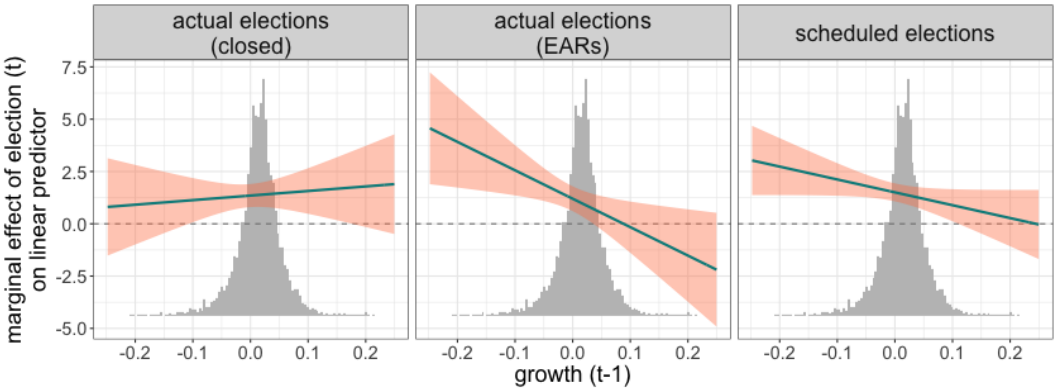


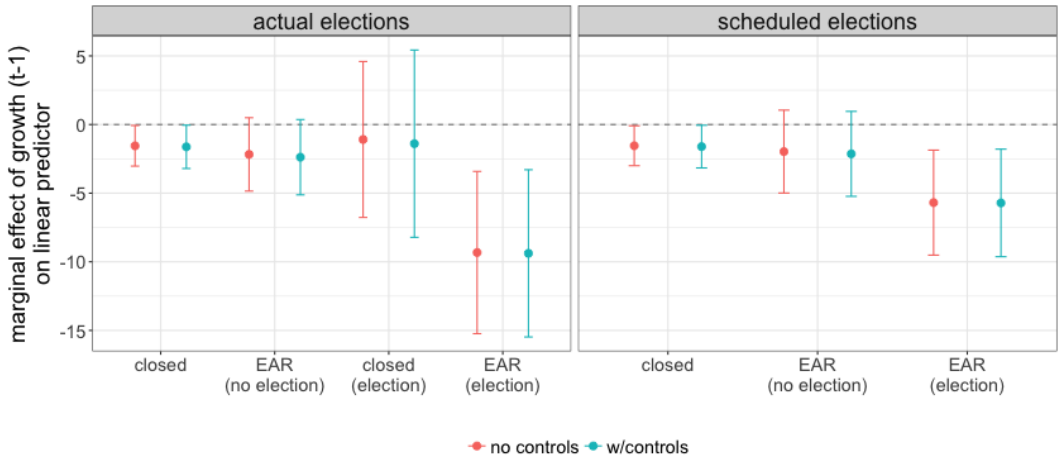
Figure A18: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the linear predictor of $breakdown_t$, based on the results reported in Table A25. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A26: Survival models (2): Probit link

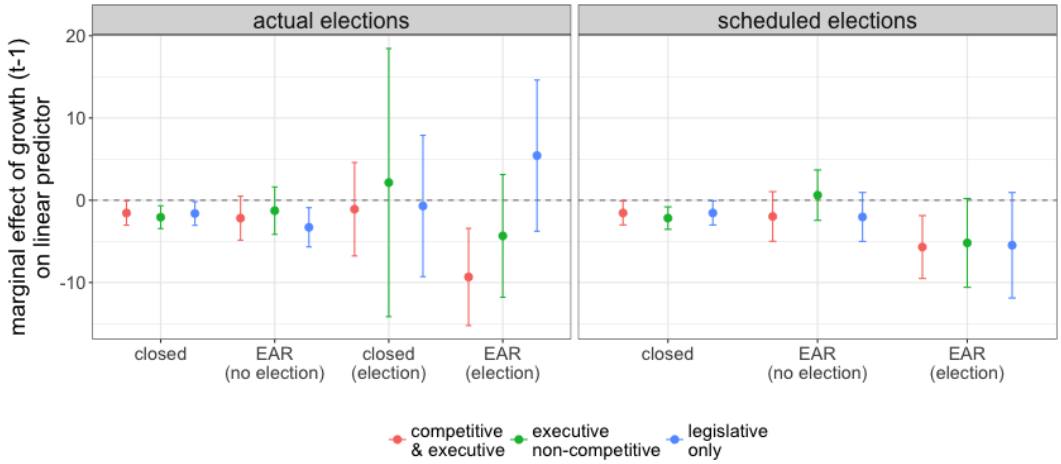
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-1.90		-1.66	-1.55	-1.63		-1.55	-1.61	-2.06	-2.17	-1.61	-1.55
	(0.74)		(0.78)	(0.86)	(0.94)		(0.84)	(0.92)	(0.82)	(0.81)	(0.85)	(0.85)
$election_t$		0.64	0.70	0.78	0.77	0.80	0.87	0.82	0.02	0.31	0.00	0.20
		(0.12)	(0.12)	(0.18)	(0.19)	(0.14)	(0.14)	(0.15)	(0.39)	(0.20)	(0.31)	(0.21)
$growth_{t-1} \times election_t$			-3.65	0.46	0.23		-4.14	-4.10	4.21	-3.01	0.92	-3.92
			(2.16)	(3.12)	(4.25)		(1.92)	(2.00)	(6.18)	(2.52)	(2.19)	(2.75)
$growth_{t-1} \times EAR_t$				-0.63	-0.76				0.79		-1.68	
				(1.72)	(1.80)				(1.74)		(1.53)	
$growth_{t-1} \times election_t$				-7.61	-7.23				-7.27		7.80	
$\times EAR_t$				(4.75)	(5.58)				(6.78)		(3.42)	
EAR_t				0.10	0.08				0.30		0.18	
				(0.12)	(0.12)				(0.10)		(0.11)	
$election_t \times EAR_t$				-0.09	-0.13				-0.56		-0.59	
				(0.25)	(0.25)				(0.54)		(0.43)	
election (other year)$_t$						-0.04	-0.03	-0.05		0.05		-0.01
						(0.12)	(0.12)	(0.14)		(0.11)		(0.13)
$growth_{t-1}$							-0.42	-0.53		2.79		-0.48
$\times$ *election (other year)*$_t$							(1.80)	(1.89)		(1.97)		(1.78)
election												0.90
(executive + legislative)$_t$												(0.16)
$growth_{t-1} \times$ *election*												-3.89
(executive + legislative)$_t$												(2.02)
GDP per capita$_{t-1}$ (log)	-0.10	-0.14	-0.12	-0.11	-0.07	-0.14	-0.11	-0.06	-0.10	-0.11	-0.11	-0.10
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)
oil and gas					-0.03			-0.03				
per capita$_{t-1}$ (log)					(0.02)			(0.02)				
proportion of					1.25			1.34				
democratic neighbors$_t$					(0.37)			(0.38)				
AIC	1563	1545	1536	1539	1425	1533	1524	1412	1564	1563	1568	1534
BIC	2061	2043	2047	2075	1969	2037	2048	1943	2100	2087	2104	2071
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061

GLS regression estimates with a probit link. The dependent variable is *breakdown*$_t$. All specifications include dummies for regime type as well as region and year fixed effects and a duration polynomial of order 3; estimates not reported to save space. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

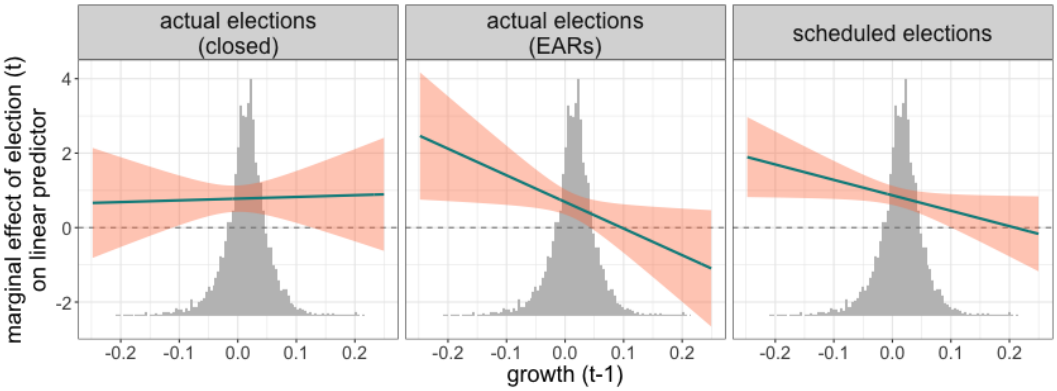


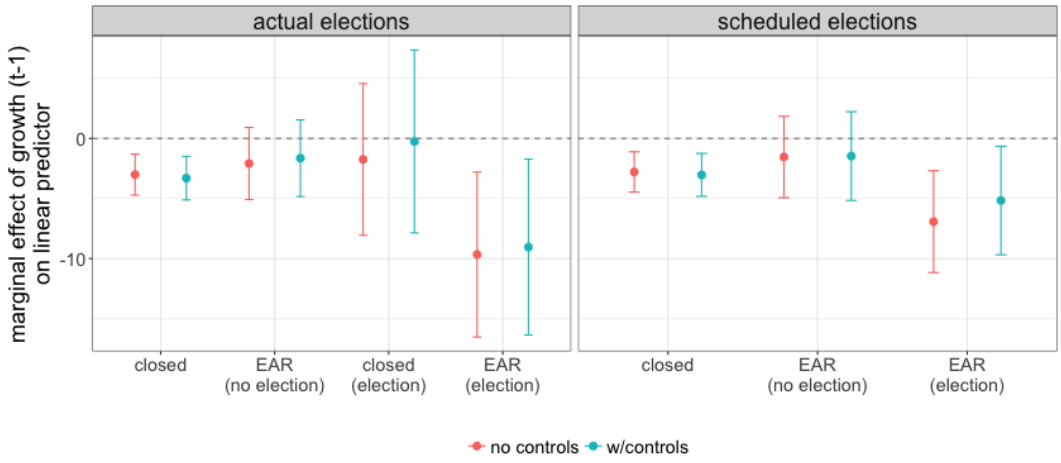
Figure A19: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the linear predictor of $breakdown_t$, based on the results reported in Table A26. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A27: Survival models (3): Probit link (+ unit means)

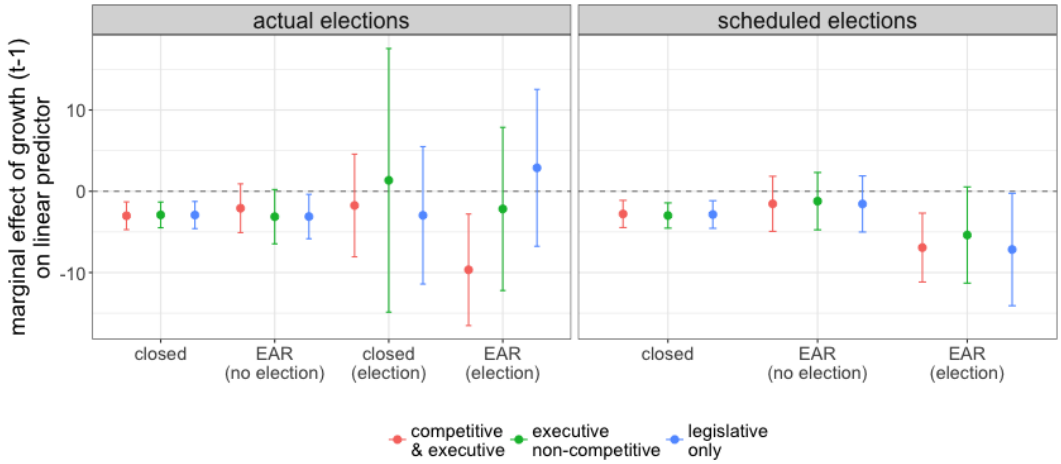
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-2.81		-2.64	-3.01	-3.30		-2.79	-3.03	-2.90	-2.98	-2.92	-2.86
	(0.79)		(0.83)	(0.89)	(0.97)		(0.85)	(0.92)	(0.83)	(0.83)	(0.90)	(0.87)
$election_t$		0.69	0.72	0.76	0.78	1.09	1.14	1.08	-0.06	0.21	0.23	0.62
		(0.13)	(0.13)	(0.21)	(0.22)	(0.18)	(0.18)	(0.20)	(0.49)	(0.24)	(0.35)	(0.25)
$growth_{t-1} \times election_t$			-2.55	1.27	3.05		-4.14	-2.13	4.25	-2.40	-0.03	-4.30
			(2.54)	(3.47)	(4.26)		(2.23)	(2.41)	(6.00)	(2.83)	(2.05)	(3.23)
$growth_{t-1} \times EAR_t$				0.92	1.66				-0.22		-0.18	
				(1.89)	(2.09)				(2.05)		(1.77)	
$growth_{t-1} \times election_t$				-8.84	-10.45				-3.30		6.03	
$\times EAR_t$				(5.35)	(5.93)				(6.23)		(3.63)	
EAR_t				0.44	0.49				0.32		0.52	
				(0.18)	(0.20)				(0.20)		(0.18)	
$election_t \times EAR_t$				0.08	-0.04				-0.59		-0.58	
				(0.27)	(0.28)				(0.65)		(0.44)	
$election\ (other\ year)_t$						0.31	0.26	0.28		-0.06		0.33
						(0.17)	(0.18)	(0.20)		(0.17)		(0.18)
$growth_{t-1}$							1.25	1.57		1.77		1.30
$\times\ election\ (other\ year)_t$							(2.06)	(2.43)		(2.30)		(2.10)
$election$												1.20
$(executive + legislative)_t$												(0.20)
$growth_{t-1} \times election$												-3.79
$(executive + legislative)_t$												(2.44)
$GDP\ per\ capita_{t-1}$ (log)	1.36	1.20	1.35	1.37	1.13	1.19	1.37	1.10	1.36	1.35	1.40	1.35
	(0.25)	(0.23)	(0.25)	(0.26)	(0.31)	(0.23)	(0.26)	(0.30)	(0.25)	(0.25)	(0.26)	(0.26)
$oil\ and\ gas$					0.17			0.18				
$per\ capita_{t-1}$ (log)					(0.06)			(0.06)				
$proportion\ of$					1.75			1.75				
$democratic\ neighbors_t$					(0.54)			(0.53)				
AIC	1491	1482	1469	1456	1343	1462	1444	1336	1500	1497	1484	1452
BIC	2002	1993	2006	2043	1949	1986	2006	1917	2087	2059	2071	2039
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061

GLS regression estimates with a probit link. The dependent variable is $breakdown_t$. Specifications include unit-specific means for all time-varying predictors (including every interaction term) as well as dummies for regime type, region and year fixed effects and a duration polynomial of order 3; estimates not reported to save space. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

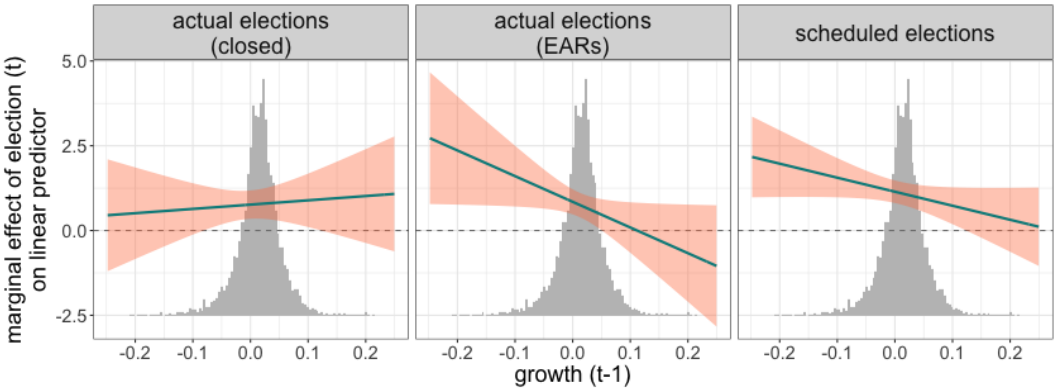


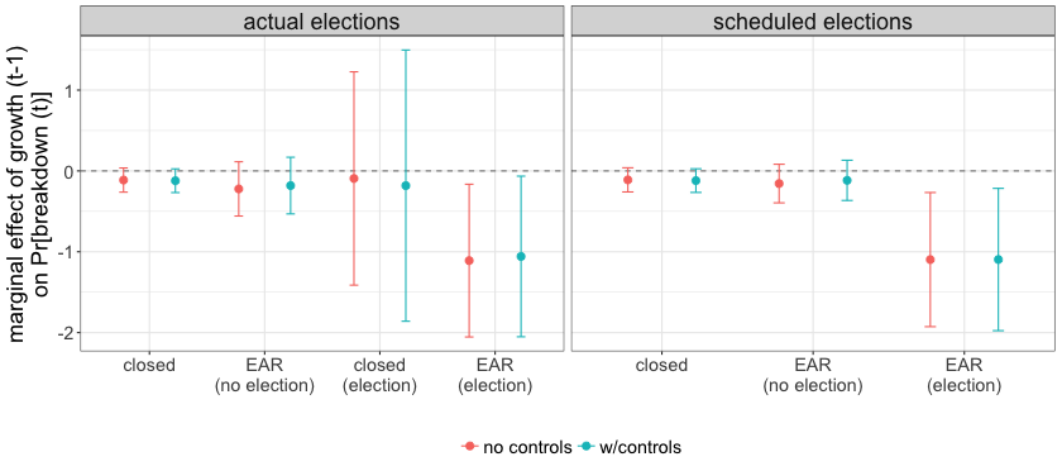
Figure A20: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the linear predictor of $breakdown_t$, based on the results reported in Table A27. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A28: Country fixed effects

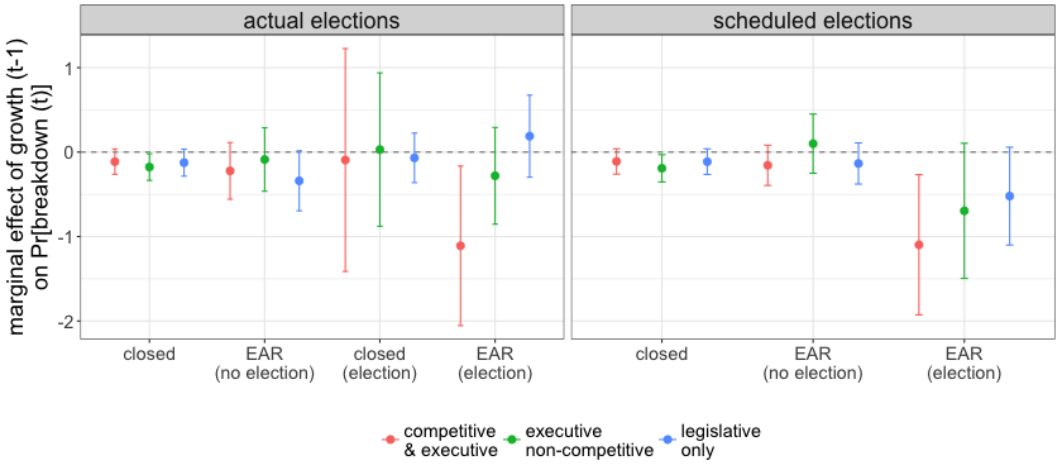
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.16		-0.13	-0.11	-0.12		-0.11	-0.12	-0.18	-0.19	-0.12	-0.11
	(0.07)		(0.07)	(0.08)	(0.07)		(0.08)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
$election_t$		0.08	0.09	0.13	0.11	0.09	0.11	0.11	-0.02	0.03	-0.01	0.02
		(0.02)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.69	0.02	-0.06		-0.99	-0.98	0.21	-0.50	0.06	-0.41
			(0.37)	(0.67)	(0.85)		(0.42)	(0.45)	(0.46)	(0.40)	(0.17)	(0.30)
$growth_{t-1} \times EAR_t$				-0.11	-0.06				0.09		-0.22	
				(0.19)	(0.20)				(0.20)		(0.20)	
$growth_{t-1} \times election_t$				-0.91	-0.82				-0.40		0.47	
$\times EAR_t$				(0.81)	(0.98)				(0.58)		(0.34)	
EAR_t				-0.00	-0.00				0.02		-0.00	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.04	-0.03				-0.01		-0.03	
				(0.05)	(0.05)				(0.05)		(0.03)	
$election\ (other\ year)_t$						-0.02	-0.02	-0.02		-0.01		-0.02
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							-0.05	0.00		0.29		-0.02
$\times\ election\ (other\ year)_t$							(0.15)	(0.15)		(0.20)		(0.15)
$election$												0.12
$(executive + legislative)_t$												(0.03)
$growth_{t-1} \times election$												-0.99
$(executive + legislative)_t$												(0.50)
$GDP\ per\ capita_{t-1}$ (log)					0.00			0.00				
					(0.02)			(0.02)				
$oil\ and\ gas$					-0.00			-0.00				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
$proportion\ of$					0.19			0.20				
$democratic\ neighbors_t$					(0.06)			(0.06)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but employing country instead of regime fixed effects. All specifications include country and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by country in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

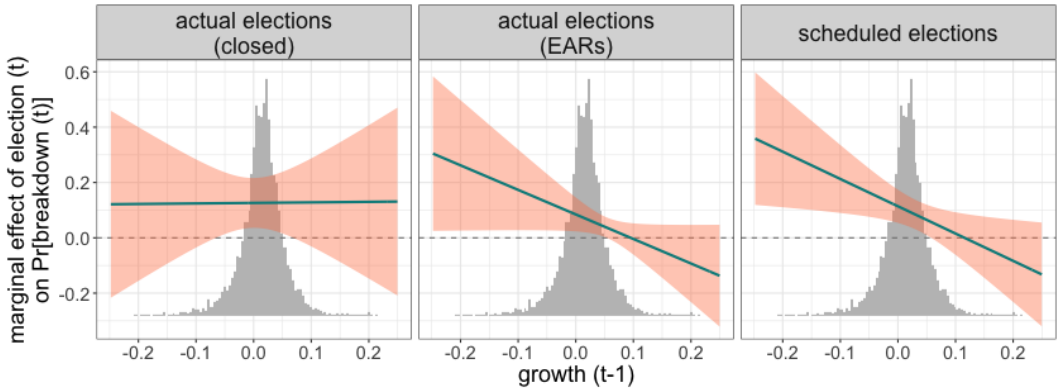


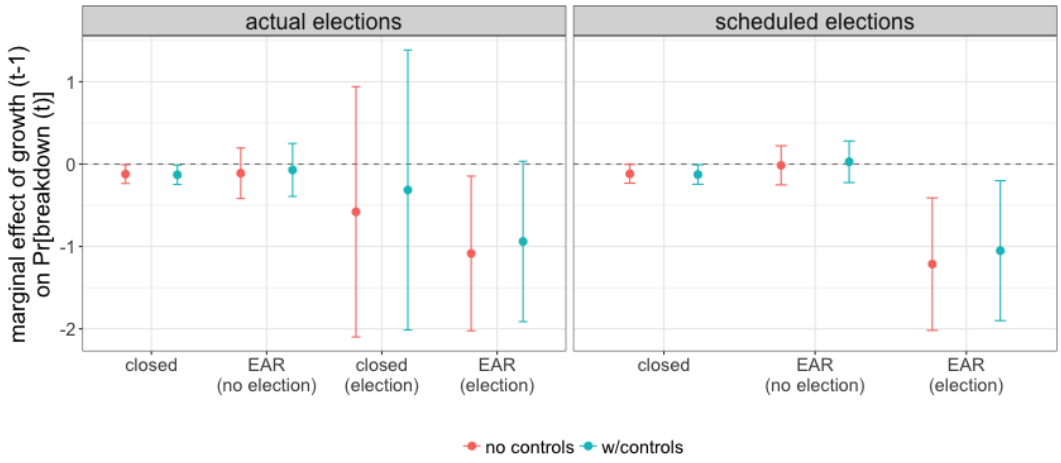
Figure A21: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A28. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A29: Alternative measures of regime type (1): Adding indirect regimes and oligarchies

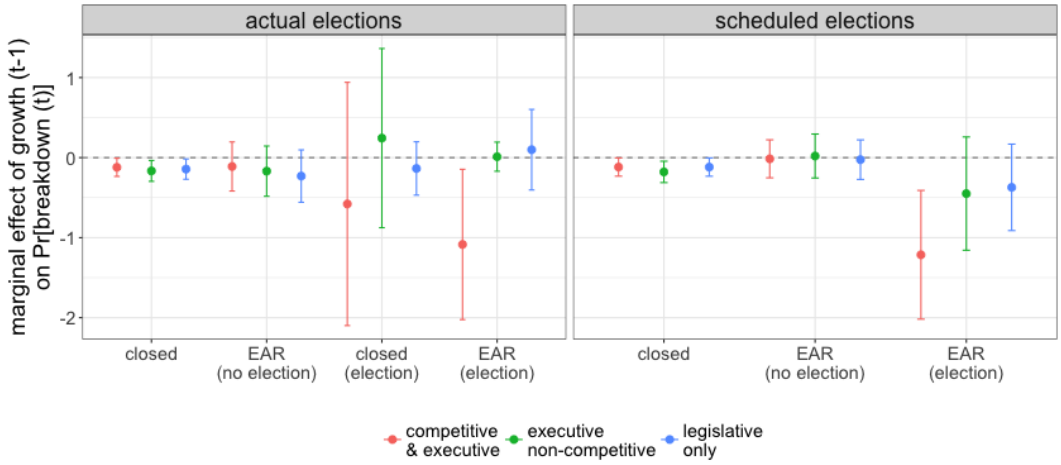
		actual elections				**scheduled elections**			**non-competitive**		**legislative**	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.16		-0.12	-0.12	-0.13		-0.12	-0.13	-0.17	-0.18	-0.14	-0.12
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
$election_t$		0.07	0.09	0.10	0.10	0.09	0.11	0.11	-0.01	0.02	-0.01	0.02
		(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.77	-0.46	-0.18		-1.10	-0.92	0.41	-0.27	0.01	-0.25
			(0.37)	(0.77)	(0.86)		(0.41)	(0.43)	(0.57)	(0.37)	(0.17)	(0.28)
$growth_{t-1} \times EAR_t$				0.01	0.06				-0.00		-0.09	
				(0.17)	(0.17)				(0.17)		(0.18)	
$growth_{t-1} \times election_t$				-0.52	-0.68				-0.23		0.32	
$\times EAR_t$				(0.91)	(1.00)				(0.59)		(0.35)	
EAR_t				0.01	0.01				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.00	-0.01				-0.03		-0.02	
				(0.05)	(0.05)				(0.05)		(0.03)	
election (other year)$_t$						-0.02	-0.02	-0.02		-0.01		-0.02
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.10	0.15		0.20		0.09
$\times$ *election (other year)*$_t$							(0.13)	(0.14)		(0.15)		(0.14)
election												0.13
(executive + legislative)$_t$												(0.04)
$growth_{t-1} \times$ *election*												-1.19
(executive + legislative)$_t$												(0.50)
GDP per capita$_{t-1}$ (log)					0.00			-0.00				
					(0.02)			(0.02)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.15			0.16				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	4225	4225	4225	4225	3957	4225	4225	3957	4225	4225	4225	4225
regimes	269	269	269	269	254	269	269	254	269	269	269	269
countries	116	116	116	116	115	116	116	115	116	116	116	116

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but adding indirect regimes (coded as EARs) and oligarchies (coded as closed regimes) to the sample (see fn. 21 and Appendix D). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

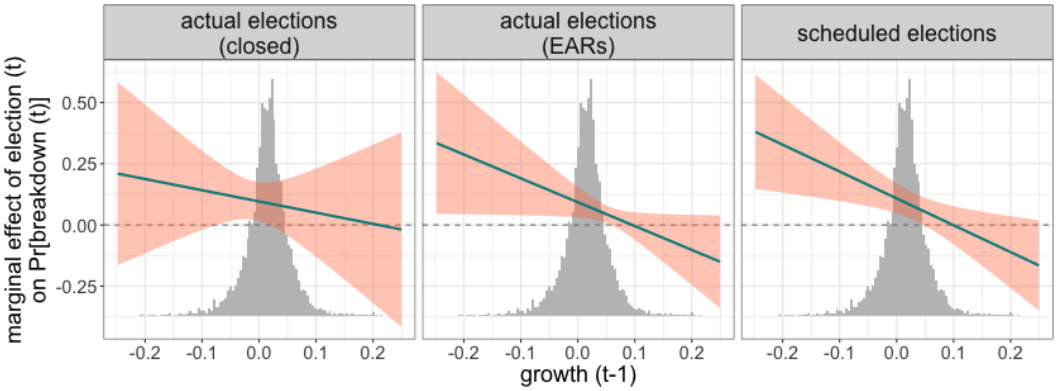


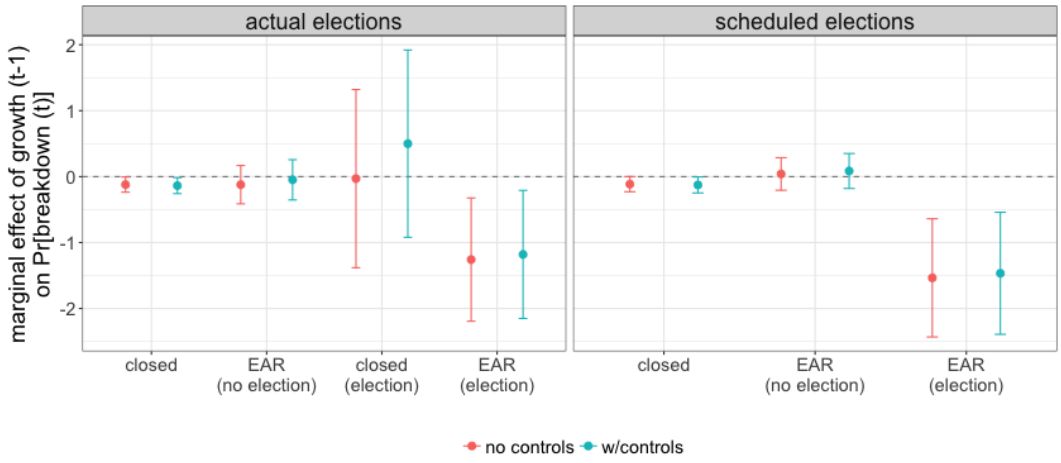
Figure A22: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A29. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A30: Alternative measures of regime type (2): LIED data

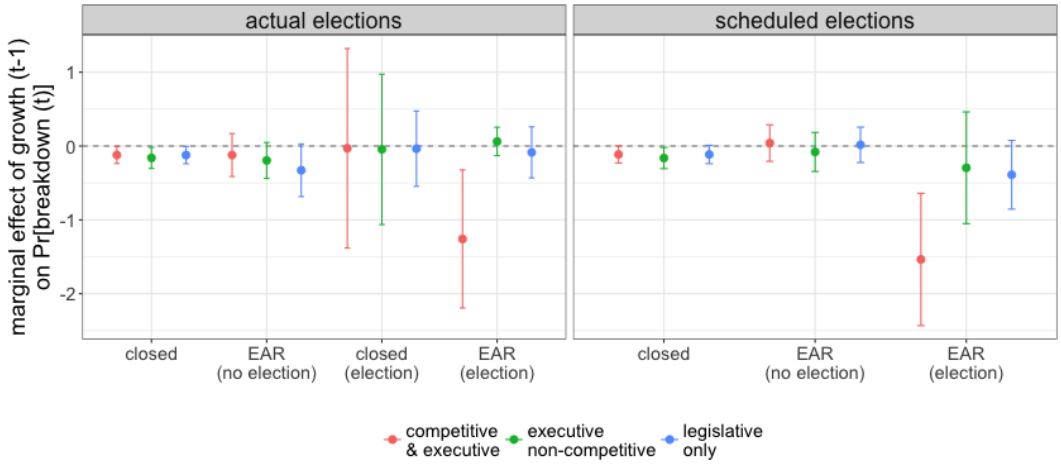
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.16		-0.12	-0.12	-0.14		-0.11	-0.13	-0.16	-0.16	-0.12	-0.11
	(0.06)		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
$election_t$		0.07	0.09	0.08	0.07	0.10	0.12	0.12	-0.02	0.02	-0.01	-0.02
		(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)
$growth_{t-1} \times election_t$			-0.77	0.09	0.64		-1.42	-1.34	0.12	-0.13	0.09	-0.28
			(0.37)	(0.70)	(0.73)		(0.46)	(0.48)	(0.52)	(0.39)	(0.26)	(0.24)
$growth_{t-1} \times EAR_t$				-0.00	0.09				-0.04		-0.21	
				(0.16)	(0.16)				(0.14)		(0.19)	
$growth_{t-1} \times election_t$				-1.23	-1.77				0.14		0.16	
$\times EAR_t$				(0.85)	(0.89)				(0.55)		(0.35)	
EAR_t				-0.00	-0.01				0.00		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				0.03	0.03				-0.01		-0.03	
				(0.05)	(0.05)				(0.05)		(0.03)	
election (other year)$_t$						-0.01	-0.03	-0.02		-0.01		-0.02
						(0.01)	(0.02)	(0.02)		(0.02)		(0.01)
$growth_{t-1}$							0.15	0.21		0.08		0.13
$\times$ *election (other year)*$_t$							(0.14)	(0.15)		(0.15)		(0.14)
election												0.13
(executive + legislative)$_t$												(0.03)
$growth_{t-1} \times$ *election*												-1.39
(executive + legislative)$_t$												(0.46)
GDP per capita$_{t-1}$ (log)					-0.00			0.00				
					(0.02)			(0.02)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.15			0.12				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	4220	4220	4220	4220	3952	4074	4074	3826	4220	3647	4220	3907
regimes	269	269	269	269	254	265	265	251	269	264	269	262
countries	116	116	116	116	115	116	116	115	116	116	116	116

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but using the LIED data (Skaaning, Gerring and Bartusevičius 2015) to code EARs and closed regimes (see Appendix D for details). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

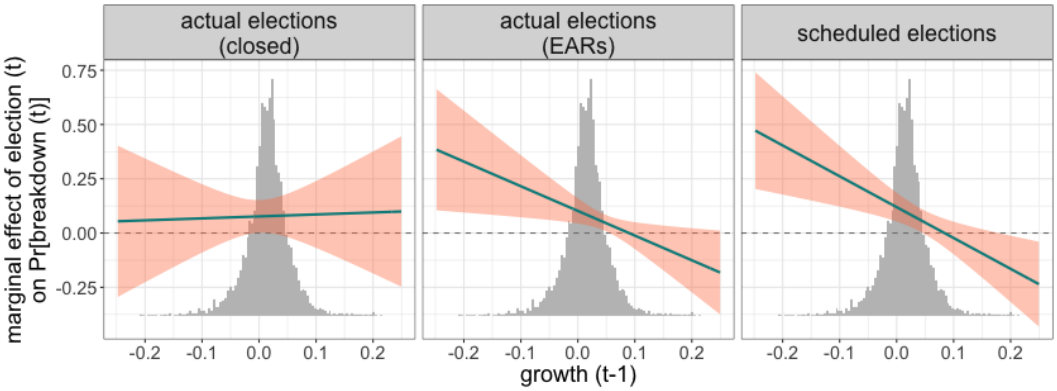


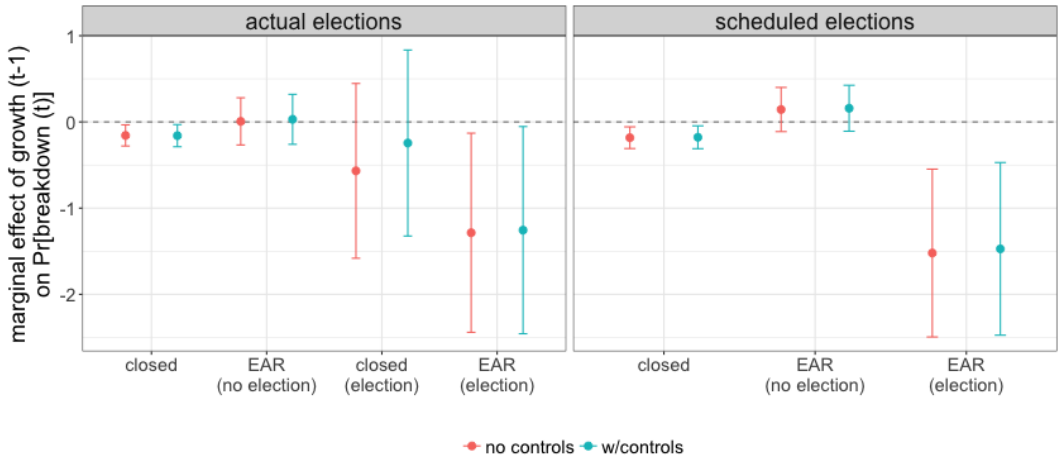
Figure A23: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A30. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A31: Alternative measures of regime type (3): V-Dem data

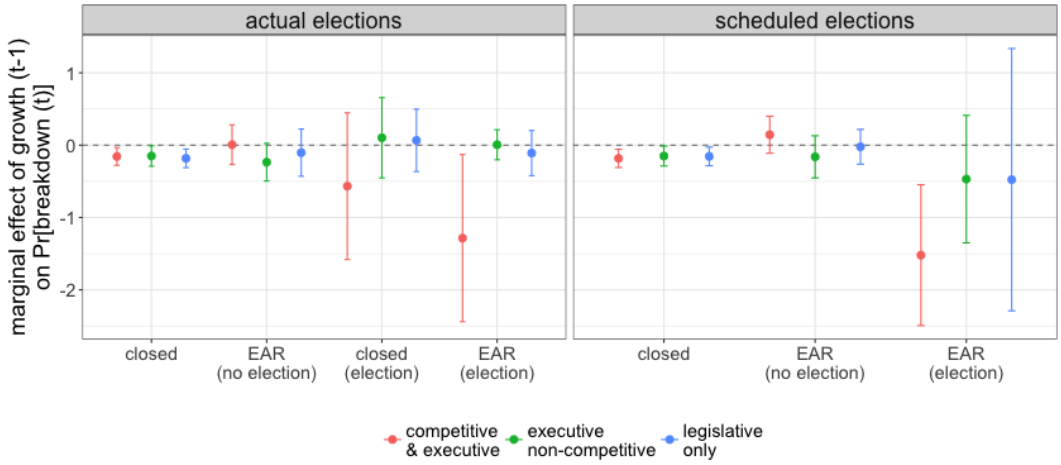
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.16		-0.12	-0.16	-0.16		-0.18	-0.18	-0.15	-0.15	-0.18	-0.16
	(0.06)		(0.06)	(0.06)	(0.07)		(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
$election_t$		0.07	0.09	0.08	0.07	0.10	0.11	0.13	-0.03	0.04	-0.01	0.11
		(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.06)
$growth_{t-1} \times election_t$			-0.77	-0.41	-0.09		-1.34	-1.29	0.25	-0.32	0.25	-0.32
			(0.37)	(0.52)	(0.55)		(0.50)	(0.51)	(0.29)	(0.45)	(0.22)	(0.92)
$growth_{t-1} \times EAR_t$				0.16	0.19				-0.09		0.08	
				(0.15)	(0.16)				(0.15)		(0.18)	
$growth_{t-1} \times election_t$				-0.88	-1.20				-0.01		-0.25	
$\times EAR_t$				(0.79)	(0.83)				(0.33)		(0.31)	
EAR_t				-0.00	-0.01				-0.01		-0.00	
				(0.01)	(0.01)				(0.01)		(0.01)	
$election_t \times EAR_t$				0.04	0.04				0.00		-0.01	
				(0.05)	(0.05)				(0.03)		(0.03)	
$election\ (other\ year)_t$						-0.00	-0.04	-0.01		-0.02		-0.01
						(0.02)	(0.01)	(0.02)		(0.01)		(0.01)
$growth_{t-1}$							0.33	0.34		-0.01		0.13
$\times\ election\ (other\ year)_t$							(0.14)	(0.15)		(0.16)		(0.14)
$election$												0.14
$(executive + legislative)_t$												(0.04)
$growth_{t-1} \times election$												-1.51
$(executive + legislative)_t$												(0.50)
$GDP\ per\ capita_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
$oil\ and\ gas$					-0.00			-0.01				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
$proportion\ of$					0.16			0.13				
$democratic\ neighbors_t$					(0.06)			(0.06)				
observations	4187	4187	4187	4187	3919	3932	3932	3675	4187	3860	4187	3581
regimes	268	268	268	268	253	255	255	242	268	266	268	245
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but using the V-Dem data (Coppedge et al. 2017) to code EARs and closed regimes (see Appendix D for details). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

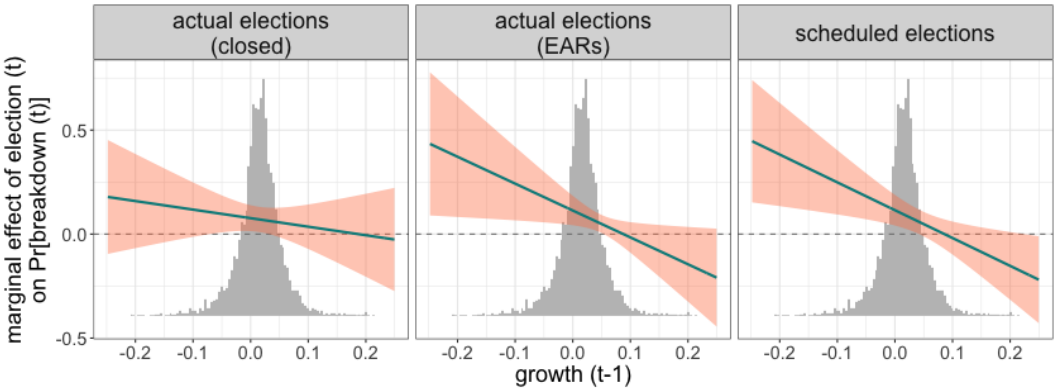


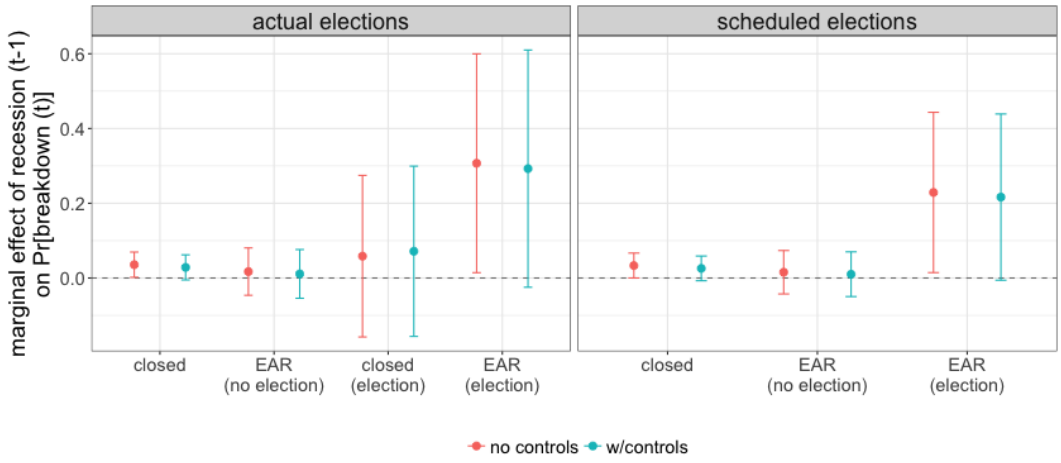
Figure A24: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A31. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A32: Alternative measures of economic performance (1): Recession dummy

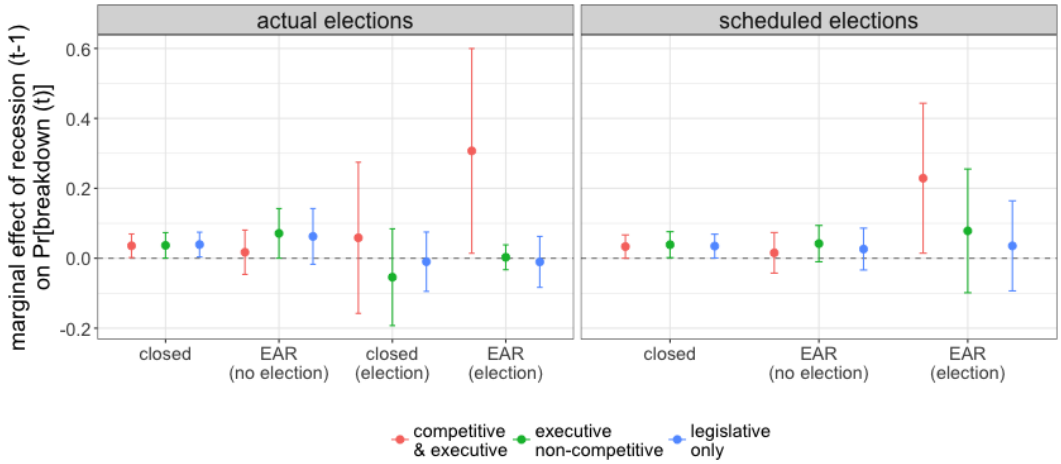
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$recession_{t-1}$	0.04		0.03	0.04	0.03		0.03	0.03	0.04	0.04	0.04	0.03
	(0.02)		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$election_t$		0.07	0.06	0.10	0.09	0.10	0.08	0.09	0.01	0.01	0.01	0.03
		(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.06)	(0.02)	(0.03)	(0.03)
$recession_{t-1} \times election_t$			0.15	0.02	0.04		0.20	0.19	-0.09	0.04	-0.05	0.00
			(0.09)	(0.11)	(0.12)		(0.11)	(0.11)	(0.07)	(0.09)	(0.04)	(0.07)
$recession_{t-1} \times EAR_t$				-0.02	-0.02				0.03		0.02	
				(0.04)	(0.04)				(0.04)		(0.04)	
$recession_{t-1} \times election_t$				0.27	0.24				0.02		-0.02	
$\times EAR_t$				(0.19)	(0.20)				(0.08)		(0.07)	
EAR_t				0.01	0.02				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.01)	
$election_t \times EAR_t$				-0.05	-0.05				-0.04		-0.03	
				(0.05)	(0.05)				(0.06)		(0.04)	
$election\ (other\ year)_t$						-0.00	-0.00	0.00		-0.01		0.01
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$recession_{t-1}$							-0.02	-0.02		0.00		-0.01
$\times\ election\ (other\ year)_t$							(0.03)	(0.03)		(0.03)		(0.03)
election												0.10
(executive + legislative)$_t$												(0.03)
$recession_{t-1} \times$ *election*												0.22
(executive + legislative)$_t$												(0.13)
GDP per capita$_{t-1}$ (log)					-0.00			-0.00				
					(0.02)			(0.02)				
oil and gas					-0.01			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.15			0.15				
democratic neighbors$_t$					(0.06)			(0.06)				
observations	4061	4061	4061	4061	3823	4061	4061	3823	4061	4061	4061	4061
regimes	258	258	258	258	244	258	258	244	258	258	258	258
countries	115	115	115	115	114	115	115	114	115	115	115	115

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but replacing $growth_{t-1}$ with a $recession_{t-1}$ dummy that takes the value of 1 if $growth_{t-1} < 0.05$, and 0 otherwise. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $recession_{t-1} \mid election_t$ (main results)



(b) $recession_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid recession_{t-1}$ (main results)

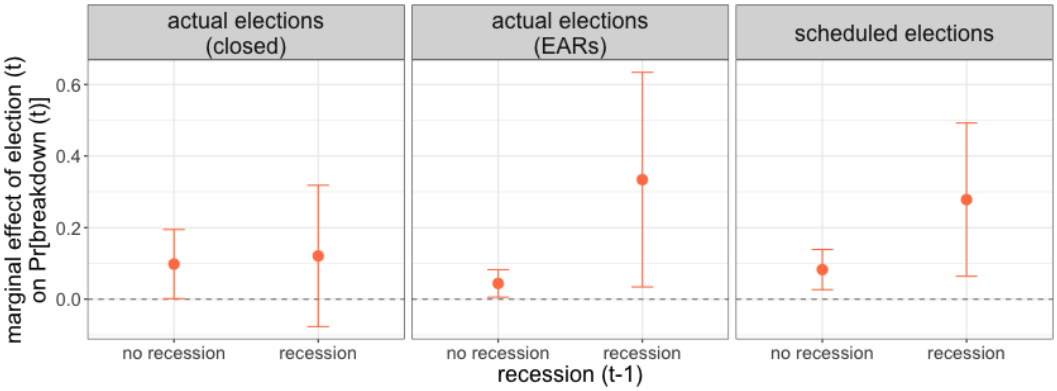


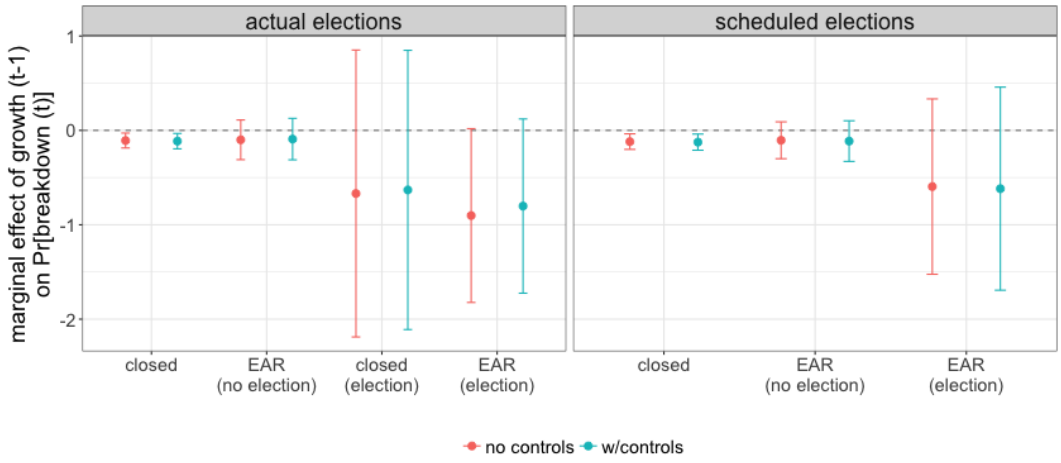
Figure A25: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A32. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A33: Alternative measures of economic performance (2): PWT data (1952-2015)

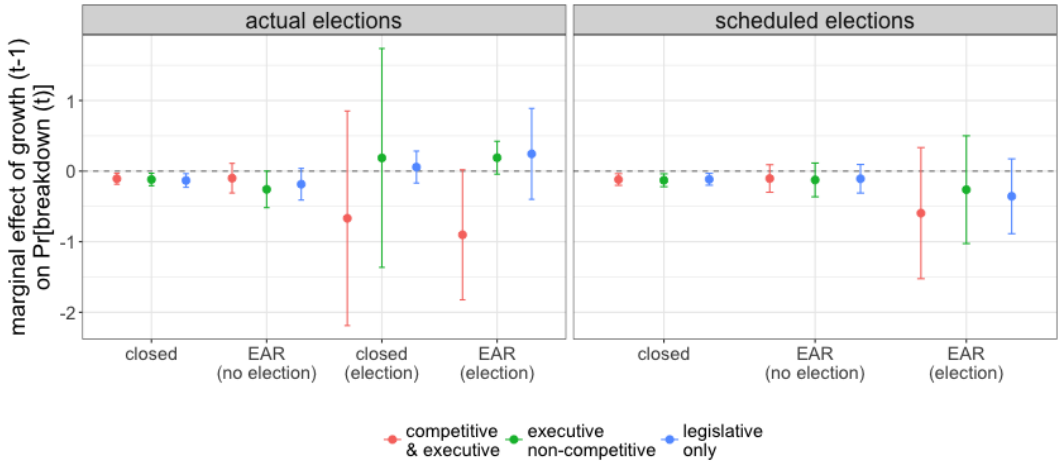
		actual elections				**scheduled elections**			**non-competitive**		**legislative**	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.13		-0.11	-0.11	-0.11		-0.12	-0.12	-0.12	-0.13	-0.13	-0.12
	(0.04)		(0.04)	(0.04)	(0.04)		(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)
$election_t$		0.06	0.08	0.10	0.09	0.09	0.10	0.11	0.01	0.01	0.01	0.04
		(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.07)	(0.03)	(0.03)	(0.03)
$growth_{t-1} \times election_t$			-0.77	-0.56	-0.52		-0.48	-0.49	0.31	-0.13	0.19	-0.24
			(0.37)	(0.77)	(0.75)		(0.47)	(0.55)	(0.79)	(0.39)	(0.12)	(0.28)
$growth_{t-1} \times EAR_t$				0.01	0.02				-0.14		-0.05	
				(0.11)	(0.12)				(0.14)		(0.12)	
$growth_{t-1} \times election_t$				-0.24	-0.19				0.14		0.24	
$\times EAR_t$				(0.91)	(0.90)				(0.80)		(0.37)	
EAR_t				0.01	0.01				0.02		0.01	
				(0.02)	(0.02)				(0.02)		(0.02)	
$election_t \times EAR_t$				-0.03	-0.02				-0.06		-0.04	
				(0.05)	(0.05)				(0.08)		(0.03)	
election (other year)$_t$						0.01	0.00	-0.00		-0.01		0.01
						(0.02)	(0.02)	(0.02)		(0.02)		(0.02)
$growth_{t-1}$							0.01	0.01		0.00		0.01
$\times$ *election (other year)*$_t$							(0.11)	(0.12)		(0.13)		(0.11)
election												0.12
(executive + legislative)$_t$												(0.04)
$growth_{t-1} \times$ *election*												-0.45
(executive + legislative)$_t$												(0.64)
GDP per capita$_{t-1}$ (log)					-0.02			-0.02				
					(0.02)			(0.02)				
oil and gas					-0.00			-0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.15			0.16				
democratic neighbors$_t$					(0.06)			(0.07)				
observations	3586	3586	3586	3586	3494	3586	3586	3248	3586	3586	3586	3586
regimes	229	229	229	229	223	229	229	219	229	229	229	229
countries	106	106	106	106	106	106	106	106	106	106	106	106

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but measuring $growth_{t-1}$ with PWT v9.0 data. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

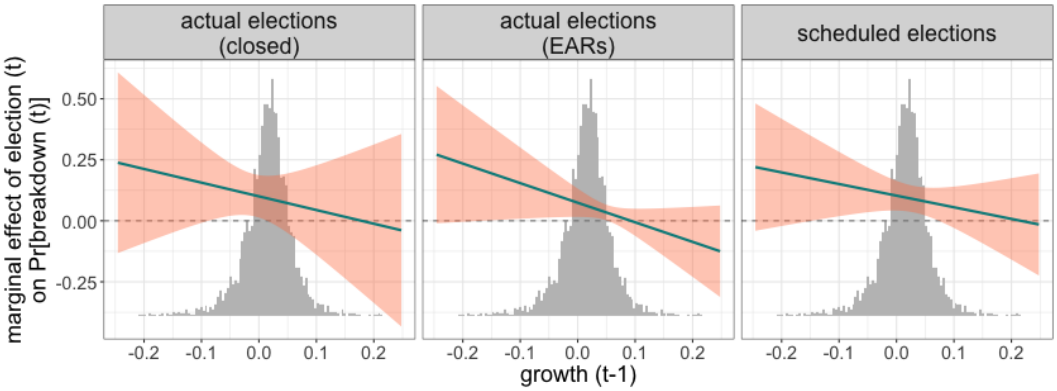


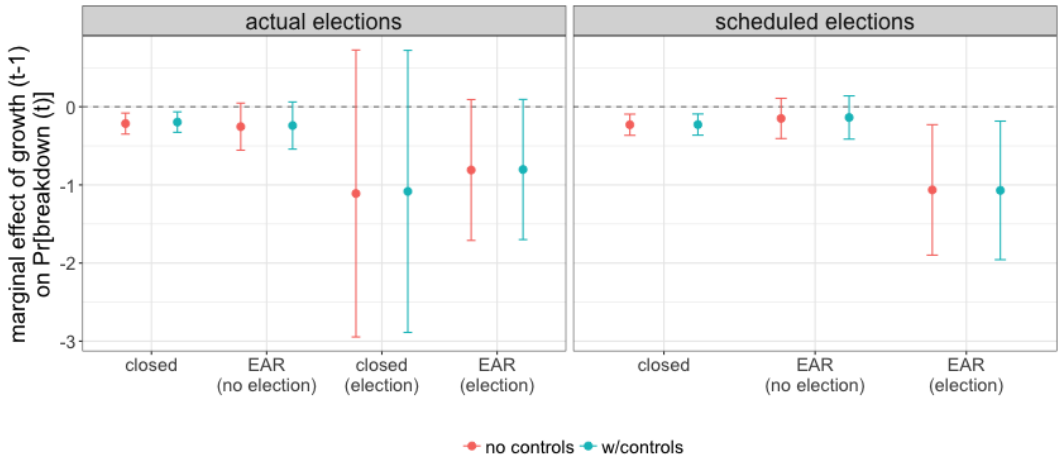
Figure A26: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A33. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A34: Alternative measures of economic performance (3): WDI data (1962-2015)

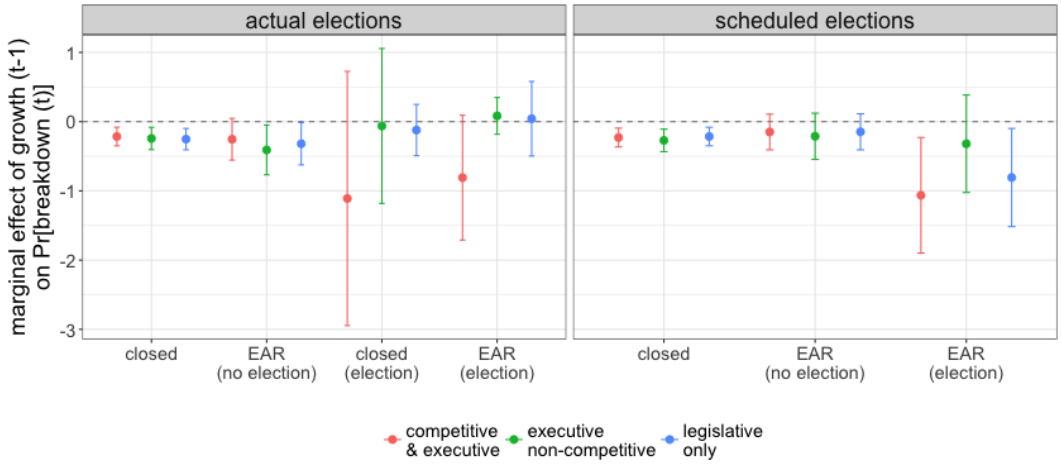
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.26		-0.23	-0.21	-0.20		-0.23	-0.23	-0.24	-0.27	-0.25	-0.21
	(0.07)		(0.07)	(0.07)	(0.07)		(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.07)
$election_t$		0.05	0.07	0.08	0.07	0.08	0.10	0.11	-0.01	0.01	-0.01	0.03
		(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.06)	(0.03)	(0.02)	(0.03)
$growth_{t-1} \times election_t$			-0.70	-0.89	-0.89		-0.83	-0.84	0.18	-0.05	0.13	-0.59
			(0.39)	(0.93)	(0.91)		(0.42)	(0.45)	(0.58)	(0.37)	(0.19)	(0.36)
$growth_{t-1} \times EAR_t$				-0.04	-0.04				-0.17		-0.07	
				(0.16)	(0.16)				(0.20)		(0.17)	
$growth_{t-1} \times election_t$				0.34	0.32				0.31		0.23	
$\times EAR_t$				(1.04)	(1.03)				(0.60)		(0.36)	
EAR_t				0.01	0.00				0.02		0.01	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.01	-0.01				-0.04		-0.02	
				(0.05)	(0.05)				(0.06)		(0.03)	
$election\ (other\ year)_t$						0.00	-0.01	-0.01		-0.01		-0.00
						(0.02)	(0.02)	(0.02)		(0.02)		(0.02)
$growth_{t-1}$							0.08	0.09		0.06		0.07
$\times\ election\ (other\ year)_t$							(0.14)	(0.15)		(0.19)		(0.14)
$election$												0.12
$(executive + legislative)_t$												(0.04)
$growth_{t-1} \times election$												-0.86
$(executive + legislative)_t$												(0.52)
$GDP\ per\ capita_{t-1}$ (log)					-0.03			-0.02				
					(0.02)			(0.02)				
$oil\ and\ gas$					0.00			0.01				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
$proportion\ of$					0.13			0.13				
$democratic\ neighbors_t$					(0.07)			(0.07)				
observations	3048	3048	3048	3048	3048	3048	3048	2777	3048	3048	3048	3048
regimes	202	202	202	202	202	202	202	196	202	202	202	202
countries	104	104	104	104	104	104	104	104	104	104	104	104

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but measuring $growth_{t-1}$ with WDI data. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

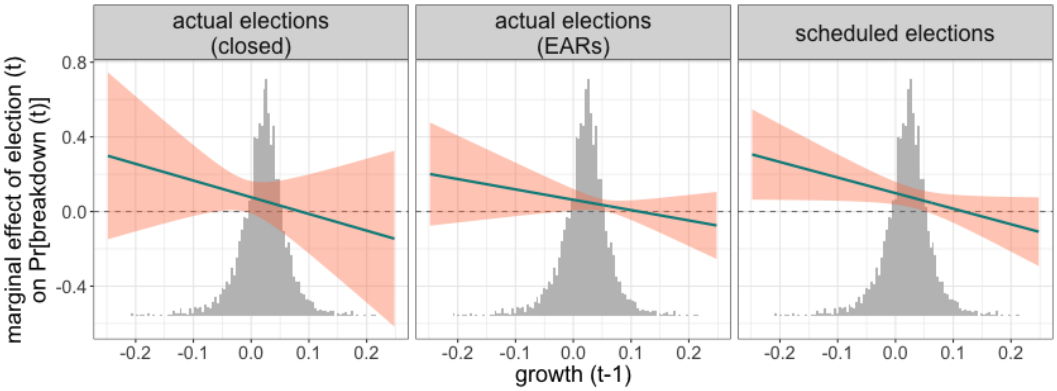


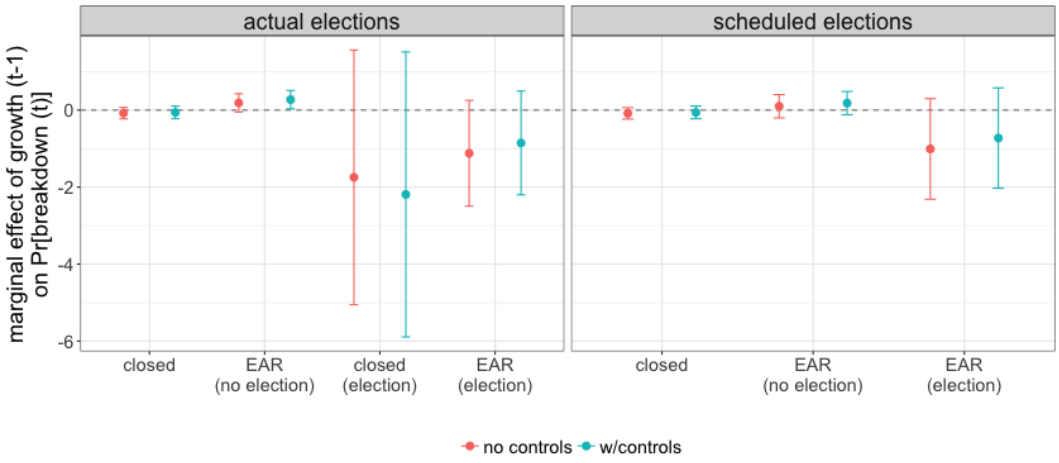
Figure A27: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A34. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A35: Party-based regimes

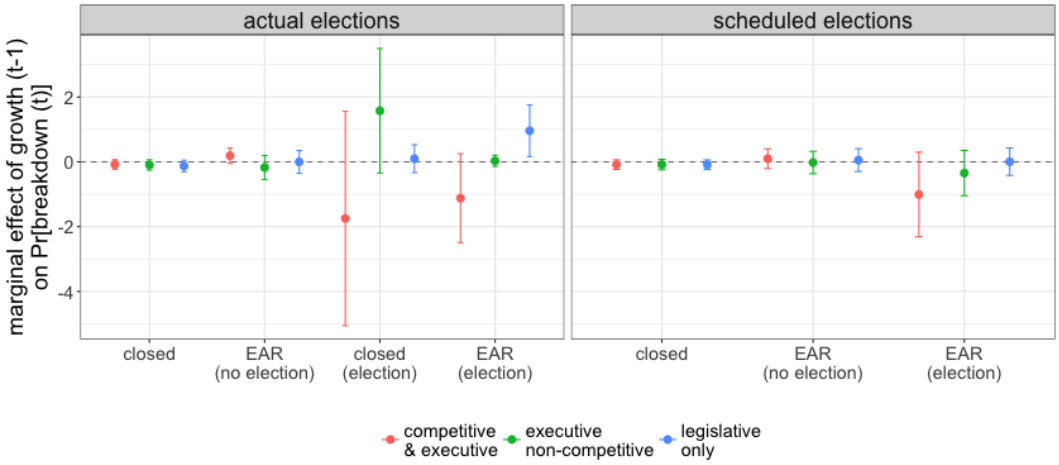
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.09		-0.03	-0.08	-0.06		-0.09	-0.06	-0.10	-0.08	-0.13	-0.09
	(0.08)		(0.06)	(0.08)	(0.08)		(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.08)
$election_t$		0.10	0.13	0.17	0.16	0.11	0.13	0.12	0.06	0.01	-0.01	0.01
		(0.03)	(0.04)	(0.08)	(0.08)	(0.04)	(0.05)	(0.05)	(0.08)	(0.03)	(0.05)	(0.04)
$growth_{t-1} \times election_t$			-1.36	-1.67	-2.13		-0.92	-0.67	1.67	-0.26	0.23	0.09
			(0.65)	(1.69)	(1.88)		(0.67)	(0.67)	(0.98)	(0.36)	(0.22)	(0.23)
$growth_{t-1} \times EAR_t$				0.27	0.33				-0.08		0.13	
				(0.14)	(0.15)				(0.19)		(0.20)	
$growth_{t-1} \times election_t$				0.36	1.01				-1.47		0.73	
$\times EAR_t$				(1.83)	(2.01)				(1.00)		(0.51)	
EAR_t				0.00	0.00				0.01		0.00	
				(0.02)	(0.02)				(0.01)		(0.02)	
$election_t \times EAR_t$				-0.06	-0.05				-0.08		-0.02	
				(0.09)	(0.09)				(0.08)		(0.05)	
election (other year)$_t$						-0.01	-0.02	-0.01		-0.02		0.00
						(0.02)	(0.02)	(0.02)		(0.01)		(0.02)
$growth_{t-1}$							0.18	0.24		0.06		0.14
$\times$ *election (other year)*$_t$							(0.17)	(0.17)		(0.18)		(0.19)
election												0.13
(executive + legislative)$_t$												(0.05)
$growth_{t-1} \times$ *election*												-0.86
(executive + legislative)$_t$												(0.71)
GDP per capita$_{t-1}$ (log)					-0.03			-0.03				
					(0.02)			(0.02)				
oil and gas					-0.01			-0.01				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.09			0.09				
democratic neighbors$_t$					(0.08)			(0.09)				
observations	1966	1966	1966	1966	1866	1966	1966	1866	1966	1966	1966	1966
regimes	75	75	75	75	73	75	75	73	75	75	75	75
countries	65	65	65	65	65	65	65	65	65	65	65	65

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but restricting the sample to regimes that GWF classify as party-based (including party-personal, party-military and party-military-personal). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

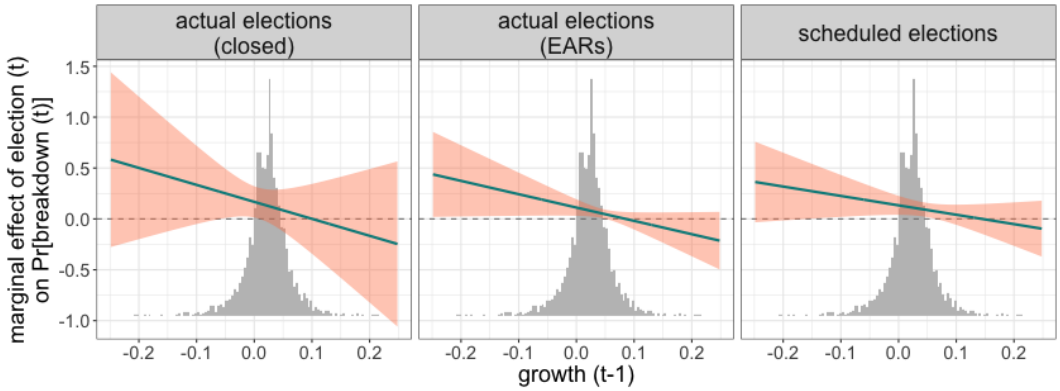


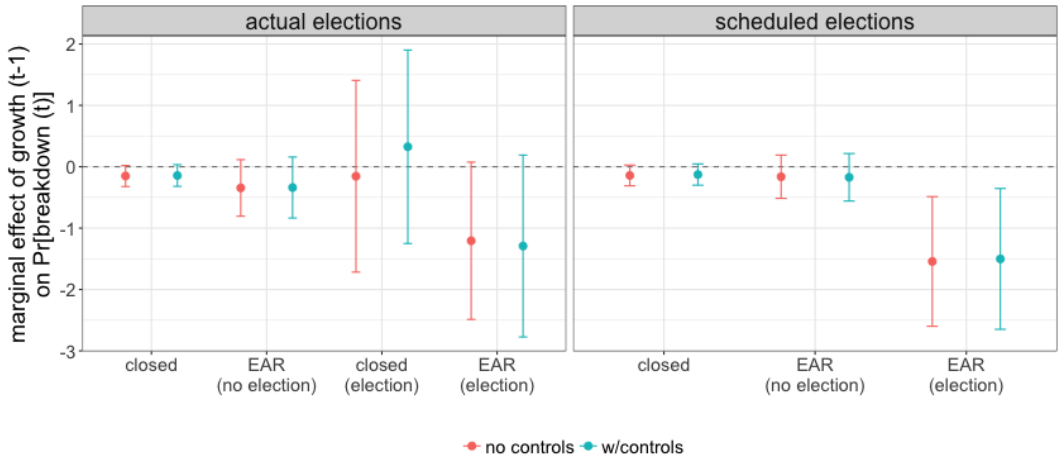
Figure A28: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A35. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A36: Regimes that are not party-based

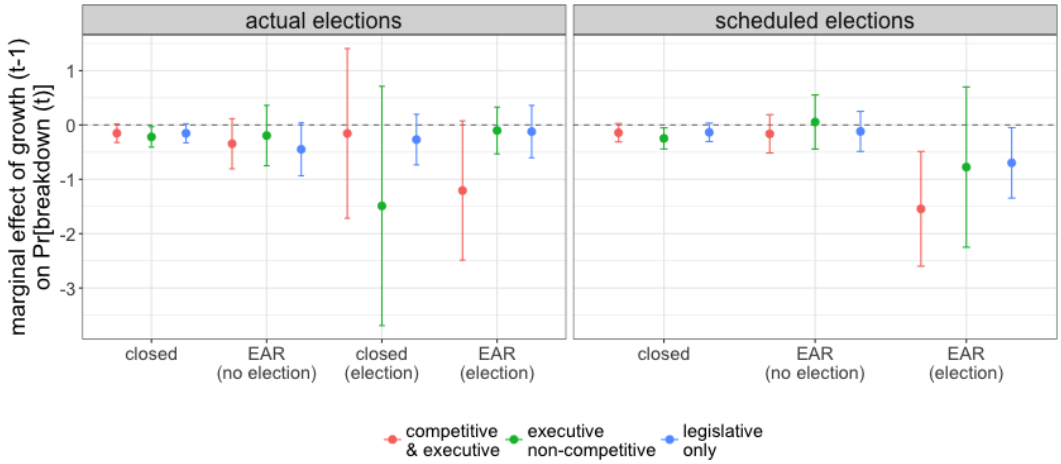
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.22		-0.20	-0.15	-0.14		-0.14	-0.13	-0.22	-0.25	-0.15	-0.14
	(0.09)		(0.09)	(0.09)	(0.09)		(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)
$election_t$		0.05	0.05	0.06	0.06	0.07	0.10	0.08	-0.05	0.03	0.02	0.04
		(0.03)	(0.03)	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)	(0.09)	(0.06)	(0.03)	(0.04)
$growth_{t-1} \times election_t$			-0.48	-0.00	0.47		-1.40	-1.37	-1.27	-0.53	-0.12	-0.56
			(0.39)	(0.80)	(0.80)		(0.54)	(0.59)	(1.12)	(0.76)	(0.24)	(0.33)
$growth_{t-1} \times EAR_t$				-0.19	-0.20				0.02		-0.29	
				(0.25)	(0.27)				(0.30)		(0.26)	
$growth_{t-1} \times election_t$				-0.86	-1.42				1.36		0.44	
$\times EAR_t$				(1.06)	(1.12)				(1.20)		(0.41)	
EAR_t				0.00	-0.02				0.05		0.00	
				(0.03)	(0.03)				(0.03)		(0.02)	
$election_t \times EAR_t$				0.01	0.00				-0.02		-0.06	
				(0.07)	(0.08)				(0.09)		(0.05)	
election (other year)$_t$						-0.00	-0.01	-0.03		0.02		-0.02
						(0.03)	(0.03)	(0.03)		(0.03)		(0.03)
$growth_{t-1}$							-0.02	-0.04		0.30		0.02
$\times$ *election (other year)*$_t$							(0.20)	(0.22)		(0.28)		(0.21)
election												0.15
(executive + legislative)$_t$												(0.06)
$growth_{t-1} \times$ *election*												-1.56
(executive + legislative)$_t$												(0.68)
GDP per capita$_{t-1}$ (log)					0.03			0.03				
					(0.03)			(0.03)				
oil and gas					0.00			0.00				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.39			0.40				
democratic neighbors$_t$					(0.12)			(0.12)				
observations	2095	2095	2095	2095	1957	2095	2095	1957	2095	2095	2095	2095
regimes	183	183	183	183	171	183	183	171	183	183	183	183
countries	84	84	84	84	80	84	84	80	84	84	84	84

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but restricting the sample to regimes that GWF classify as not party-based. All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

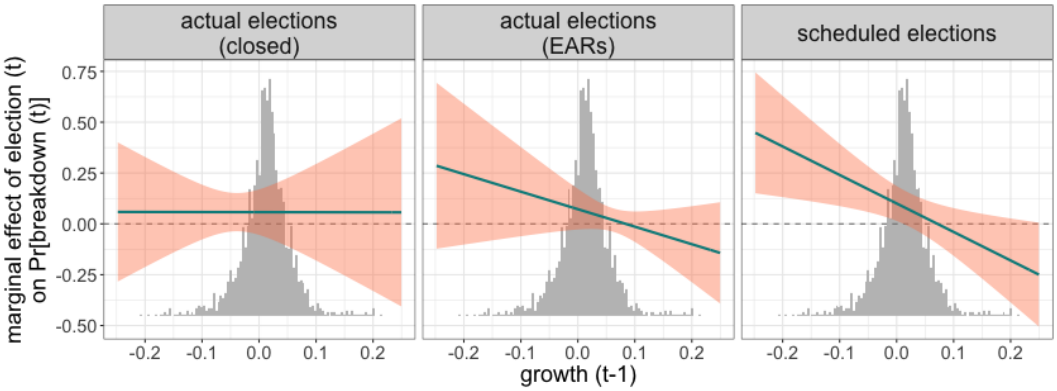


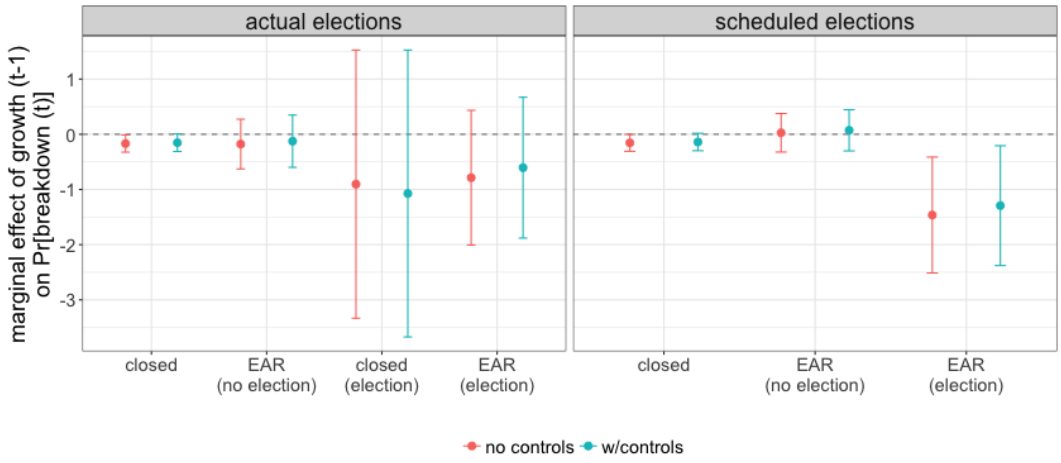
Figure A29: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A36. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A37: Poor countries

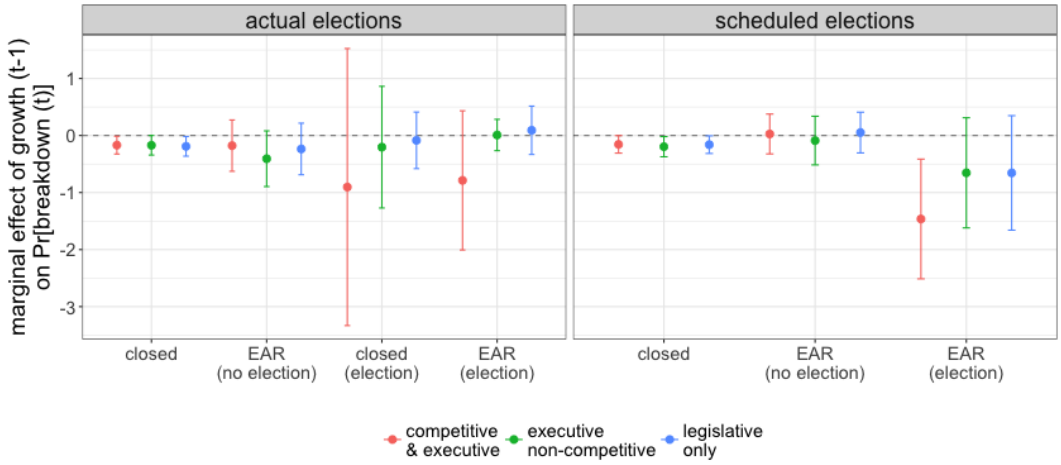
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.20		-0.17	-0.17	-0.15		-0.16	-0.14	-0.17	-0.20	-0.19	-0.16
	(0.08)		(0.08)	(0.08)	(0.08)		(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.08)
$election_t$		0.07	0.08	0.05	0.04	0.08	0.10	0.10	-0.04	0.03	-0.02	0.04
		(0.02)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)
$growth_{t-1} \times election_t$			-0.54	-0.74	-0.92		-1.31	-1.15	-0.03	-0.46	0.11	-0.50
			(0.55)	(1.23)	(1.32)		(0.54)	(0.55)	(0.55)	(0.50)	(0.26)	(0.51)
$growth_{t-1} \times EAR_t$				-0.01	0.03				-0.24		-0.05	
				(0.24)	(0.26)				(0.26)		(0.25)	
$growth_{t-1} \times election_t$				0.13	0.44				0.45		0.22	
$\times EAR_t$				(1.38)	(1.47)				(0.61)		(0.41)	
EAR_t				-0.01	-0.00				0.02		0.00	
				(0.02)	(0.02)				(0.02)		(0.02)	
$election_t \times EAR_t$				0.05	0.05				0.01		-0.04	
				(0.06)	(0.06)				(0.05)		(0.04)	
$election\ (other\ year)_t$						-0.02	-0.02	-0.02		-0.01		-0.01
						(0.02)	(0.02)	(0.02)		(0.02)		(0.02)
$growth_{t-1}$							0.18	0.21		0.11		0.21
$\times\ election\ (other\ year)_t$							(0.20)	(0.21)		(0.23)		(0.20)
$election$												0.11
$(executive + legislative)_t$												(0.04)
$growth_{t-1} \times election$												-1.42
$(executive + legislative)_t$												(0.58)
$GDP\ per\ capita_{t-1}$ (log)					-0.01			-0.01				
					(0.02)			(0.02)				
$oil\ and\ gas$					-0.00			-0.00				
$per\ capita_{t-1}$ (log)					(0.01)			(0.01)				
$proportion\ of$					0.11			0.12				
$democratic\ neighbors_t$					(0.09)			(0.09)				
observations	2786	2786	2786	2786	2627	2786	2786	2627	2786	2786	2786	2786
regimes	187	187	187	187	181	187	187	181	187	187	187	187
countries	72	72	72	72	72	72	72	72	72	72	72	72

OLS regression estimates. The dependent variable is $breakdown_t$. Specifications replicate those reported in Table 1, but restricting the sample to countries located below the median value of *GDP per capita* in the year they first enter the sample (or the first year data is available). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

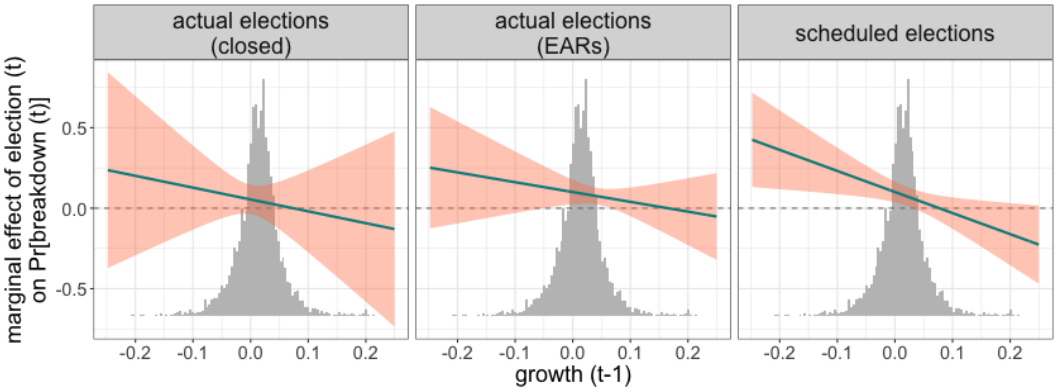


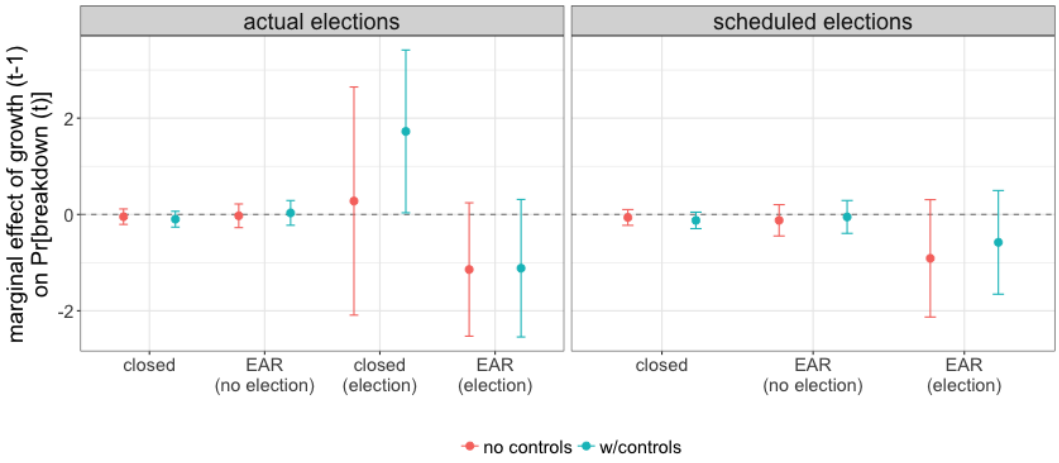
Figure A30: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A37. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

Table A38: Rich countries

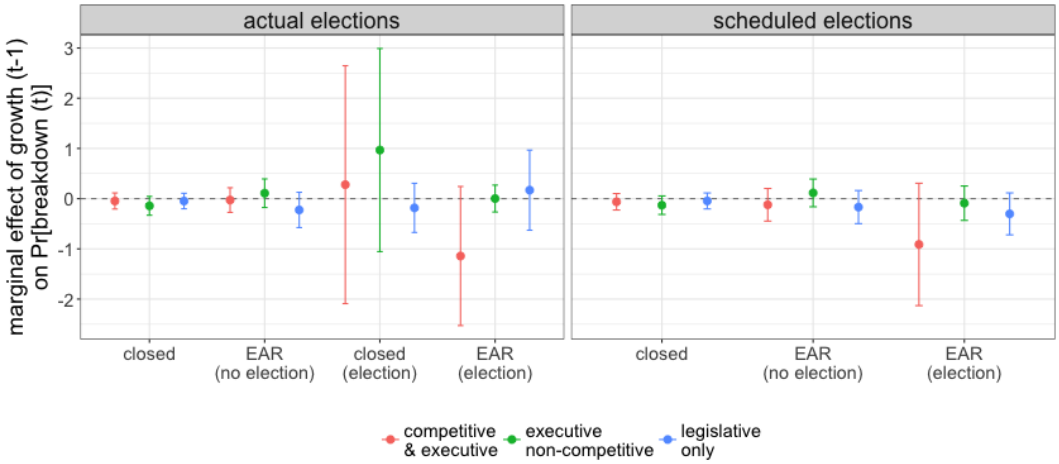
		actual elections				scheduled elections			non-competitive		legislative	
	growth only	election only	growth × election	growth × el. × EAR	growth × el. × EAR	election only	growth × election	growth × election	actual	sched.	actual	sched.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$growth_{t-1}$	-0.08		-0.04	-0.05	-0.10		-0.06	-0.12	-0.14	-0.13	-0.05	-0.05
	(0.08)		(0.07)	(0.08)	(0.08)		(0.08)	(0.09)	(0.10)	(0.09)	(0.08)	(0.08)
$election_t$		0.06	0.09	0.23	0.31	0.15	0.18	0.19	0.12	-0.02	0.03	0.05
		(0.04)	(0.04)	(0.12)	(0.12)	(0.08)	(0.09)	(0.09)	(0.13)	(0.02)	(0.05)	(0.06)
$growth_{t-1} \times election_t$			-0.84	0.32	1.82		-0.85	-0.46	1.11	0.04	-0.14	-0.26
			(0.44)	(1.23)	(0.86)		(0.64)	(0.55)	(1.04)	(0.20)	(0.25)	(0.21)
$growth_{t-1} \times EAR_t$				0.02	0.13				0.25		-0.18	
				(0.14)	(0.14)				(0.17)		(0.19)	
$growth_{t-1} \times election_t$				-1.44	-2.97				-1.22		0.53	
$\times EAR_t$				(1.41)	(1.14)				(1.02)		(0.49)	
EAR_t				0.03	0.04				0.04		0.01	
				(0.03)	(0.04)				(0.02)		(0.02)	
$election_t \times EAR_t$				-0.16	-0.25				-0.17		-0.03	
				(0.13)	(0.13)				(0.14)		(0.06)	
election (other year)$_t$						0.05	0.04	0.07		-0.00		0.04
						(0.07)	(0.06)	(0.07)		(0.02)		(0.06)
$growth_{t-1}$							-0.06	0.07		0.24		-0.12
$\times$ *election (other year)*$_t$							(0.18)	(0.18)		(0.17)		(0.18)
election												0.24
(executive + legislative)$_t$												(0.10)
$growth_{t-1} \times$ *election*												-0.87
(executive + legislative)$_t$												(1.00)
GDP per capita$_{t-1}$ (log)					0.03			0.04				
					(0.03)			(0.03)				
oil and gas					-0.02			-0.02				
per capita$_{t-1}$ (log)					(0.01)			(0.01)				
proportion of					0.19			0.19				
democratic neighbors$_t$					(0.09)			(0.09)				
observations	1275	1275	1275	1275	1196	1275	1275	1196	1275	1275	1275	1275
regimes	71	71	71	71	63	71	71	63	71	71	71	71
countries	43	43	43	43	42	43	43	42	43	43	43	43

OLS regression estimates. The dependent variable is *breakdown*$_t$. Specifications replicate those reported in Table 1, but restricting the sample to countries located above the median value of *GDP per capita* in the year they first enter the sample (or the first year data is available). All specifications include regime and year fixed effects, as well as a duration polynomial of order 3. Robust standard errors clustered by regime in parentheses.

(a) $growth_{t-1} \mid election_t$ (main results)



(b) $growth_{t-1} \mid election_t$ (non-competitive and legislative)



(c) $election_t \mid growth_{t-1}$ (main results)

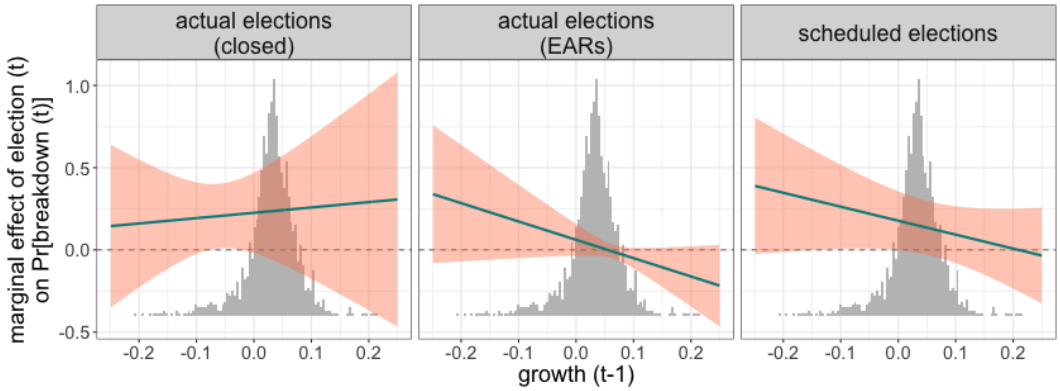


Figure A31: Point estimates and 95% confidence intervals of the marginal effects of $growth_{t-1}$ or $election_t$ on the probability of $breakdown_t$, based on the results reported in Table A38. Panel (a) is based on models 4 and 5 (for actual elections) or 7 and 8 (for scheduled elections); panel (b), on models 4, 9 and 11 (actual) or 7, 10 and 12 (scheduled); while results in panel (c) correspond to models 4 and 7, respectively.

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