

Authoritarian Legislatures and Economic Growth

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Abstract

The accepted wisdom in the literature is that, by providing credible constraints on incumbents, authoritarian legislatures promote investment and hence growth. However, studies of individual legislatures show that these bodies have limited policy-making capacity and their members enjoy relatively little autonomy from the government, casting doubts on their capacity to actually constrain incumbents' behavior. Moreover, the relationship between legislatures and economic performance is generally based on cross-sectional variation, thus ignoring the possibility that regimes that always had a legislature may be systematically different from those that did not. Drawing on a substantially larger sample than previous studies, employing three alternative measures of authoritarian legislatures and accounting for the possibility of heterogeneous effects, I find no evidence that authoritarian legislatures are associated with higher growth rates or increased access to private credit, either cross-sectionally or over time.

Keywords: authoritarian regimes – authoritarian legislatures – economic performance

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Do authoritarian legislatures improve economic performance? The conventional wisdom among political scientists is that they do (Gandhi 2008a,b; Gehlbach and Keefer 2012; Jensen, Malesky and Weymouth 2014), even if the effect is restricted to non-personalist regimes (Wright 2008a; Wilson and Wright 2015). By inducing cooperation from society and/or limiting the executive's capacity to act unilaterally, the argument goes, authoritarian legislatures establish credible constraints on the government's behavior, diminishing the risk of expropriation (Jensen, Malesky and Weymouth 2014; Wilson and Wright 2015) and hence promoting investment (Wright 2008b; Gehlbach and Keefer 2011, 2012) and ultimately growth (Gandhi 2008a,b; Wright 2008b).

Recently, however, these claims have been subject to increased skepticism. On the empirical side, case studies of individual legislatures in Brazil (Desposato 2001), Egypt (Blaydes 2010; Loidolt and Mecham 2016), Vietnam (Malesky and Schuler 2010, 2011; Malesky, Schuler and Tran 2012; Schuler forthcoming) or China (Truex 2014) provide little evidence that such bodies actually constrain the executive's behavior, even though only one of those regimes (Egypt 1952-2011) qualifies as "personalist" (but also military and party-based; see Geddes, Wright and Frantz 2014). On the methodological side, Pepinsky (2014) notes that authoritarian institutions are endogenous, and thus the outcomes that researchers attribute to them may actually be the product of the conditions under which the institution arose. To put it differently, autocracies with legislatures may be systematically different from those that lack them, and it may be the underlying differences between these regimes, rather than the legislatures themselves, that explain their variation in economic performance. While there is no easy way to account for such between-regime differences, at the very least scholars could employ fixed-effects to focus on within-regime variation in legislative institutions (Schuler and Malesky 2014); yet most existing studies are either cross-sectional in nature (Gehlbach and Keefer 2012; Jensen, Malesky and Weymouth 2014) or use panel data but without accounting for unit heterogeneity (Gandhi 2008a,b; Wright 2008b; see Wilson and Wright 2015 for an exception).

In this paper I seek to fill this gap by providing the first systematic within-regime analysis of the economic consequences of authoritarian legislatures. In doing so, I extend the existing literature

in three ways. First, by looking at more than 200 autocracies between 1946 and 2010, my sample size is 39 to 122% larger than that of existing studies.¹ This makes it more rather than less likely to find significant results. Second, to account for the possibility that only some kinds of legislatures may matter, and then only in some kinds of autocracies, I report results for three different measures of authoritarian legislatures (with up to six categories each), plus their interactions with four measures of regime type. Finally, to determine whether the results should be attributed to between- or within-regime variation (or both), I report both cross-sectional estimates – regressing a regime’s average performance on the kind of legislature it had at the beginning of its second year – and fixed-effects ones, thus accounting for regime characteristics that remained fixed over time – including the country where it was established, the type of regime, and its founding episode.

Either way, I find no evidence that authoritarian legislatures contribute to economic performance. In both the cross-sectional and the panel data analyses, the estimates are close to zero in absolute terms, and far from statistically significant at conventional levels. Accounting for differences between legislatures and/or regime type does not change these findings. Given the large number of coefficients reported, it is inevitable that some of them are significant at conventional levels, but there is no clear pattern in the results. Thus, while we cannot discard the possibility that authoritarian legislatures may serve to coordinate the behavior of regime elites, (Svolik 2012; Boix and Svolik 2013; Bonvecchi and Simison 2017), identify local notables (Blaydes 2010; Malesky and Schuler 2011; Reuter et al. 2016), distribute rents (Blaydes 2010; Truex 2014) or co-opt potential opponents (Reuter and Robertson 2015), these results suggest that the claim that authoritarian legislatures are associated with increased economic performance should be revised.

¹As detailed in Tables A3 to A8, I employ a sample of up to 189 observations for the cross-sectional analysis and 4,018 for the panel estimates. In contrast, the cross-sectional studies of Gehlbach and Keefer (2012) and Jensen, Malesky and Weymouth (2014) have no more than 101 or 85 observations (see Tables 2 and 1, respectively). Gandhi reports estimates based on $N = 2,069$ to $2,443$ (see 2008a, Table 5.2 and 2008b, Table IV, respectively), while Wright (2008b) has up to 2,342 observations (see Table 6) and Wilson and Wright (2015, Table S-2) report 2,886.

Theoretical framework

Existing literature. Many autocracies have political institutions – including parties, (competitive) elections and legislatures – that resemble democratic ones (Gandhi [2008b](#); Gandhi and Lust-Okar [2009](#); Magaloni and Kricheli [2010](#); Morse [2012](#); Svolik [2012](#); Brancati [2014](#); Ginsburg and Simpser [2014](#); Schuler and Malesky [2014](#)). This has naturally raised the question of why autocrats may want to introduce such institutions (or keep them in place). Regarding legislatures, the existing literature has provided three alternative (but not necessarily incompatible) arguments.

First, legislatures may serve to co-opt political leaders and other elites, thus ensuring their co-operation with – or at least their reluctant acquiescence to – the regime. For example, offering some legislative representation to opposition groups may serve to drive a wedge between the moderate and the radical opposition (Lust-Okar [2004, 2006](#); Gandhi [2008b](#); Reuter and Robertson [2015](#); Rivera [2017](#)). Alternatively, by permitting some limited competition at the subnational level, the government can identify local notables whose support is crucial to the regime's survival (Blaydes [2010](#); Malesky and Schuler [2011](#); Reuter et al. [2016](#)).

Second, legislatures may serve as fora where powerful insiders meet regularly to exchange information and monitor the government's behavior. This makes it harder for the executive to betray his own allies: even if the legislature regularly approves the government's proposals – not always the case, even in autocracies (Desposato [2001](#); Bonvecchi and Simison [2017](#) and Saiegh [2011](#), ch. 5) –, the fact that all relevant measures must be announced beforehand means that the government's behavior becomes common knowledge, preventing it from launching a surreptitious attack against its own supporters (Gehlbach and Keefer [2011, 2012](#); Svolik [2012](#); Boix and Svolik [2013](#)).

Lastly, having a legislature may make it harder for the government to expropriate the wealth of its citizens or foreign investors. If a legislature exists, important regime insiders are likely to seat in it, which allows them to amend bills to protect their interests (Gandhi [2008b](#); Jensen, Malesky and Weymouth [2014](#)), block measures they do not like (Bonvecchi and Simison [2017](#)) or coordinate

against a government that threatens their interests (Gehlbach and Keefer 2011). The executive may retain the option of closing the legislature and ruling by decree, but doing so signals that he is not to be trusted, thus discouraging investment.

To be sure, there are some nuances to these arguments, but they can be accommodated more or less easily. For example, Gehlbach and Keefer (2012) note that since only domestic investors are represented in the legislature, their effect should be limited to domestic, but exclude foreign, investment (Jensen et al's 2014 claim that authoritarian legislatures matter mostly for corporate governance rules implicitly makes a similar prediction). Wright (2008b) and Wilson and Wright (2015) note that personalist autocracies *de facto* concentrate all political power in a single individual (see also Bonvecchi and Simison 2017); thus, legislatures in such regimes should have little constraining capacity, and be unable to promote economic performance.

Limitations. More seriously, these arguments are vulnerable to three kinds of criticism. First, a growing body of research shows that legislatures in developing countries play a limited role in policy-making (Cox and Morgenstern 2001; Machado, Scartascini and Tommasi 2011; Palanza, Scartascini and Tommasi 2016). If this is the norm in undeniably democratic countries, one should not expect much from authoritarian regimes where political institutions are even less powerful. Using data from V-Dem (Coppedge et al. 2017) on latent measures of legislative professionalization – as based on country experts' responses to questions such as whether the legislature is likely to question or investigate executive officials in practice, opposition parties exercise oversight over the executive branch, or the legislature controls its own funds, among others –, Figure 1 shows that legislatures in advanced democracies are by far the most professionalized, followed by those in developing democracies and the ones in electoral autocracies – defined as those in which the chief executive is elected in formally competitive elections –, with legislatures in closed autocracies in a comfortable fourth place. Admittedly, these measures are based on experts' subjective codings, and thus this comparison should be taken with a grain of salt. Still, the differences shown in Figure 1 are consistent with other pieces of evidence. For example, while Saiegh (2011, ch. 5) shows that authoritarian executives

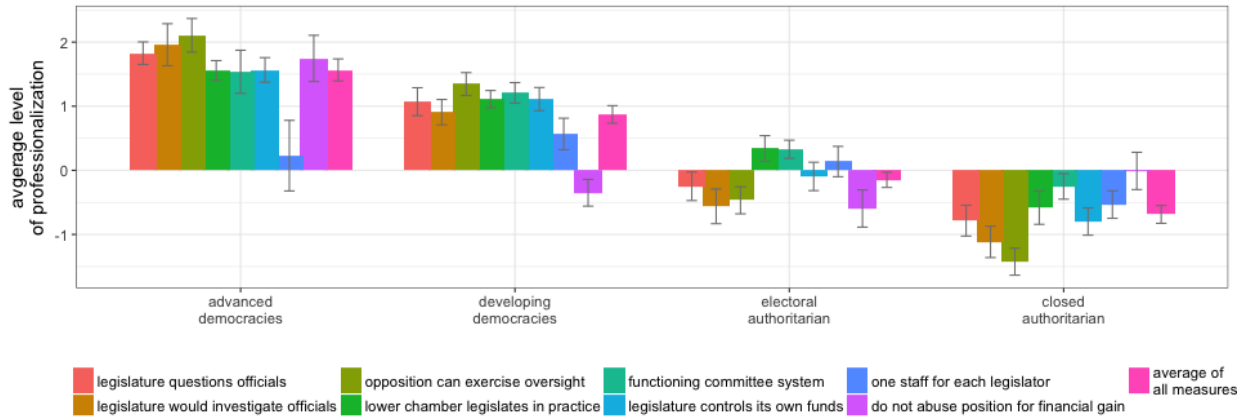


Figure 1: Average levels of legislative professionalization in different kinds of countries. The unit of observation is the country-year. 95% CIs based on robust standard errors clustered by regime. Measures of legislative professionalization are latent estimates calculated by V-Dem (Coppedge et al. 2017) on the basis of country experts' responses, and re-scaled to have mean = 0 and SD \approx 1. See Table A2 for the full set of estimates.

sometimes see their bills defeated, their average rate of success is higher than in democracies. Similarly, case studies of Egypt (Blaydes 2010), China (Truex 2014) and Russia (Reuter and Robertson 2015) show that in these countries, legislative positions are more valuable as a means for personal enrichment or a source of patronage than for the political power they confer. Some authoritarian legislatures allow(ed) some limited questioning of government policies (Desposato 2001; Malesky and Schuler 2010; Malesky, Schuler and Tran 2012; Schuler forthcoming; Loidolt and Mecham 2016), but this is restricted to minority groups that cannot block the government's core proposals.

The strongest case for the claim that authoritarian legislatures can constrain incumbent behavior comes from Medieval and early Modern Europe. Stasavage (2010) shows that between 1250 and 1750, geographic distance explained variation on legislatures' *de facto* capacity to place constraints on monarchs. North and Weingast's (1989) interpretation of the Glorious Revolution highlights how a victorious Parliament ushered in an era of unprecedented financial development by establishing new institutions to place real constraints on the Crown. Yet these authors focus on legislatures that represented the demands of already powerful social groups, such as noblemen and wealthy merchants (or merchant towns). This is especially visible in the British case, where Parliament had

to overthrow the Crown twice before being able to place effective constraints on it. This brings us to Pepinsky's (2014) criticism of the literature on authoritarian institutions, namely that such institutions are endogenous to the circumstances in which they originated, and that it may be these circumstances, rather than the institutions themselves, that make a difference. This does not necessarily mean that institutions themselves do not matter; as Gehlbach and Keefer (2011) and Boix and Svolik (2013) note, powerful authoritarian elites may need a legislature to solve their collective action problem. Yet if that is the case, then authoritarian legislatures should only matter when elites had some independent power beforehand – implying that authoritarian legislatures should only matter in non-personalist regimes (Wright 2008b; Wilson and Wright 2015; Bonvecchi and Simison 2017).²

This reasoning, however, runs against the evidence from case studies of individual legislatures, which show that many of them have little independent power – and not only in personalist regimes. For example, although the Brazilian military continued to hold elections and kept the legislature open for almost 20 years, it sacked around 200 legislators with little short-term consequences. Thus, while legislators sometimes defeated the government's proposals in the floor, they were always mindful of the potential consequences; it was only when the military became increasingly unpopular that challenging the government became easier (Desposato 2001). Similarly, although some Vietnamese legislators submit questions to government ministers, these tend to be restricted to local issues (Malesky and Schuler 2010) or to non-sensitive topics about which the government is willing to allow criticism (Schuler forthcoming), and in any case, a large majority of legislators speak little but always vote with the government (Malesky and Schuler 2010; Malesky, Schuler and Tran 2012; Schuler forthcoming). Both the Egyptian (Blaydes 2010) and the Chinese (Truex 2014) legislatures appear(ed) to serve mostly to distribute rents to government supporters, though in the first case the Muslim Brotherhood's representatives took advantage of their position to ask embarrassing questions and force the government to defend unpopular positions (Loidolt and Mecham 2016).

²Levitsky and Way (2013) make an analogous claim with respect to authoritarian parties: only those emerging from successful revolutions or independence wars should be exceptionally durable.

Lastly, the large- N literature on authoritarian legislatures is either cross-sectional (Gehlbach and Keefer 2012; Jensen, Malesky and Weymouth 2014), or fails to account for country- or regime-specific effects (Gandhi and Przeworski 2007; Gandhi 2008a,b; Wright 2008b; Svulik 2012; Boix and Svulik 2013; Rivera 2017), making the findings vulnerable to the endogeneity concerns raised by Pepinsky (2014).³ As Schuler and Malesky (2014) point out, what we need is a before-and-after comparison; to the best of my knowledge, only Wilson and Wright (2015) have followed this approach, but they look at expropriation rather than economic performance. Tellingly, Gandhi compares before-and-after growth rates in dictatorships that changed institutions and finds that the effects are small in magnitude, not always in the expected direction, and statistically insignificant anyway (2008a:17).

Implications. Taken together, the previous considerations suggest that (a) cross-sectional (“between”) and fixed-effects (“within”) models may produce different results; and (b) the nature of such differences may be informative about the economic impact (or lack thereof) authoritarian legislatures. By “between” estimates I refer to an analysis of whether regimes that have a legislature at the beginning of their lifetime experience better economic performance over the long run (see Gehlbach and Keefer 2012). If that were the case, and results also showed that regimes that introduce a legislature during their lifetime experience better performance afterwards, the claim that authoritarian legislatures do make a difference for economic performance would be substantially strengthened. Finding a positive effect for between estimates but a null one for within ones would bolster Pepinsky’s (2014) claim that authoritarian institutions do not matter *per se*, but rather that autocracies with institutions are fundamentally different, in unobserved (unobservable?) ways, from those that lack them. Lastly, failing to find any effect for either cross-sectional or within estimates would imply that authoritarian legislatures are inconsequential for economic growth.⁴ In either case,

³The Heckman selection model employed by Gandhi (2008a) is not a satisfactory solution because it does not account for time dependence: legislatures are not created anew each year, but tend to persist once they are in place.

⁴Finding a within effect only would be harder to interpret, though in such case it could be argued that either (a) some regimes experienced events (such as an insurgency or a civil war) that both lowered their economic performance and

one may search for heterogeneous effects by distinguishing between different kinds of autocracies (monarchy, military, party-based or personal) and/or legislatures (e.g., single- versus multi-party).

Of course, the observational nature of the data means that any result may be interpreted with caution. Nonetheless, some interpretations are more plausible than others. The literature reviewed above suggest that any positive relationship between legislatures and economic performance is vulnerable to omitted variable bias, as regimes that already managed to constrain the executive may enjoy better performance even if no legislature exists. An analogous reasoning applies to the claim that, say, political turmoil both worsens economic performance and forces the closure of the legislature. Similarly, within estimated may be problematic insofar as regimes that have more to gain by introducing a legislature are the ones more likely to establish one (Gandhi 2008a). On the other hand, a null result could only mask a positive effect of authoritarian legislatures on economic performance if regimes that expect to do worse economically have incentives to introduce a legislature. While this possibility cannot be dismissed out of hand, such a story is less convincing than the previous ones: why would better-performing autocracies be more unwilling to introduce legislatures if these provided real benefits? The point is that there is an asymmetry here: finding no systematic association between authoritarian legislatures and economic performance is much more supportive of a “no effect” story than finding a positive relationship is of a “positive (causal) effect” story.

Data and methods

I examine these claims on a sample of 268 authoritarian regimes between 1948 and 2011.⁵ The list of autocracies is from Geddes, Wright and Frantz (2014; henceforth GWF).⁶ The main advantage of this dataset is that it groups authoritarian country-years by authoritarian *regime* rather than country, where a regime is defined as the “set of formal and/or informal rules for choosing leaders and

forced them to close the legislature; or (b) only regimes that expected to do especially well economically had enough reasons to establish a legislature when none existed.

⁵Online Appendix B lists all regimes included in the analysis.

⁶I extended these authors’ data until 2015 and made a few changes in their coding; see Online Appendix A for details.

policies” (Geddes, Wright and Frantz 2014:codebook).⁷ In practice, this means that a change of regime is coded whenever an authoritarian executive and his ruling coalition are replaced by a different set of rulers, even if the latter are also authoritarian. Thus, cases such as the displacement of the Iranian Shah or the Cuban Revolution are coded as instances of regime transition even though neither country became democratic. This is important for my purposes because if the displacement of an authoritarian ruler by another is accompanied by the establishment (or closure) of a legislature, attributing any subsequent change in performance to the legislature would be problematic. In contrast, other widely used measures of regime type – like 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) – simply code the democratic status of a given country-year, even when there has been a substantial change in the nature of the ruling coalition. GWF also account 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 aspires to endure.

GWF provide no measure of authoritarian institutions, so I relied on V-Dem (Coppedge et al. 2017) to create three different measures of authoritarian legislatures:

- (1) To begin with, I simply employ a dummy that codes whether a country had a *legislature* at the beginning of year t .
- (2) To account for the possibility that the effect of legislatures may depend on the representation of opposition parties (Gandhi 2008a,b; Gehlbach and Keefer 2012), in some specifications I distinguish between three categories: countries with *no legislature* at the beginning of year t ; countries with a *single-party legislature* (which also includes non-partisan legislatures); and countries with a *multi-party legislature*.

⁷Other authors using this data for similar reasons include Hollyer, Rosendorff and Vreeland (2015); Wright, Frantz and Geddes (2015); Knutsen, Nygård and Wig (2017); Schuler, Gueorguiev and Cantú (2017) and Lucardi (forthcoming).

- (3) For a more nuanced analysis, I created a factor with six mutually exclusive categories: *no legislature*; *unelected legislature*; *single-party legislature*, meaning that the legislature, but not the executive, was elected in single-party elections; *single-party legislature + executive*, if both the legislature and the executive were elected in single-party elections; *multi-party legislature*, if the legislature, but not the executive, was elected in (formally) multi-party elections; and *multi-party legislature + executive*, if both were. This allows for the possibility that the effect of authoritarian legislatures may depend on the nature of the executive office, which has much more power and is hence much more relevant for a regime's survival (Lucardi [forthcoming](#)).⁸

I look at two outcomes. $growth\ rate\ (\%)_{c,t}$ indicates country c 's change in per capita income during year t . To maximize sample size, I employ data from the Maddison Project,⁹ To examine the claim that authoritarian legislatures mostly affect domestic investment, I also look at $credit/GDP\ (\%)_{c,t}$, the amount of *domestic* credit provided to the private sector, which is taken from the World Development Indicators (WDI).¹⁰ Depending on the specification, I sometimes control for three variables that may affect both a regime's propensity to have a legislature and its economic performance: regime type (monarchy, military, personal or partisan, from GWF);¹¹ the country's level of development, proxied by its $GDP\ per\ capita_{c,t-1}$ (from Maddison); and the availability of natural resources (*oil and gas per capita* _{$c,t-1$} , from Ross and Mahdavi [2015](#)).

⁸I ignore other combinations (such as the executive being elected in multiparty elections while the legislature is not) because, although they are theoretically possible, they are extremely rare in practice.

⁹<http://www.ggd.net/maddison/maddison-project/home.htm>, 2013 version.

¹⁰<http://data.worldbank.org/data-catalog/world-development-indicators>.

¹¹I exclude all regimes that GWF classify as *oligarchies* because there are only a handful of cases (South Africa 1910-1994; Bolivia 1946-1951; and Pakistan 1947-1958), and thus results are very sensitive to minor differences, especially when including interaction terms.

Table 1: Descriptive statistics

	averaged sample (regimes: 199; countries: 114)					country-year sample (regimes: 202; countries: 116)				
	N	mean	std. dev.	min.	max.	N	mean	std. dev.	min.	max.
<i>growth rate (%) (Maddison)</i>	189	1.77	2.92	-7.37	16.70	4018	1.94	6.43	-61.49	59.01
<i>credit/GDP (%) (WDI)</i>	152	18.82	15.24	0.21	94.24	2875	23.05	22.55	0.00	158.50
<i>legislature (0/1)</i>	199	0.83	0.38	0.00	1.00	4532	0.87	0.34	0.00	1.00
<i>single-party legislature</i>	199	0.31	0.46	0.00	1.00	4532	0.45	0.50	0.00	1.00
<i>multi-party legislature</i>	199	0.52	0.50	0.00	1.00	4532	0.42	0.49	0.00	1.00
<i>no legislature</i>	199	0.17	0.38	0.00	1.00	4532	0.13	0.34	0.00	1.00
<i>unelected legislature</i>	199	0.13	0.33	0.00	1.00	4532	0.13	0.33	0.00	1.00
<i>single-party legislature only</i>	199	0.05	0.21	0.00	1.00	4532	0.12	0.33	0.00	1.00
<i>single-party legislature + executive</i>	199	0.14	0.34	0.00	1.00	4532	0.20	0.40	0.00	1.00
<i>multi-party legislature only</i>	199	0.16	0.37	0.00	1.00	4532	0.09	0.29	0.00	1.00
<i>multi-party legislature + executive</i>	199	0.36	0.48	0.00	1.00	4532	0.32	0.47	0.00	1.00
<i>monarchy</i>	199	0.08	0.26	0.00	1.00	4532	0.12	0.33	0.00	1.00
<i>party-based</i>	199	0.37	0.48	0.00	1.00	4532	0.51	0.50	0.00	1.00
<i>military</i>	199	0.27	0.45	0.00	1.00	4532	0.18	0.39	0.00	1.00
<i>personal</i>	199	0.54	0.50	0.00	1.00	4532	0.45	0.50	0.00	1.00
<i>GDP per capita (lagged) (th. 1990 Int. GK\$)</i>	168	2.02	2.54	0.27	26.11	4036	2.74	3.02	0.20	30.93
<i>oil and gas per capita (lagged) (th. 2000 dollars)</i>	157	0.34	1.96	0.00	22.37	4130	0.62	2.67	0.00	54.90
<i>population (millions)</i>	138	15.81	25.74	0.34	148.34	3797	34.15	131.96	0.34	1371.22
<i>area (th. km²)</i>	137	785.29	1695.28	0.68	17098.24	3763	879.83	1868.36	0.68	17098.25

Table 2: Legislatures in authoritarian regimes, 1946-2015

	pooled sample	within avg.	first year	last year	% interlude 100%	90%
<i>no legislature</i>	0.14	0.22	0.17	0.16	0.05	0.08
<i>single-party legislature</i>	0.43	0.34	0.30	0.30	0.07	0.12
<i>multiparty legislature</i>	0.43	0.44	0.51	0.54	0.18	0.21
<i>any legislature</i>	0.86	0.78	0.82	0.84	0.51	0.61
<i>unelected legislature</i>	0.12	0.10	0.12	0.06	0.02	0.02
<i>single-party legislature</i>	0.12	0.08	0.04	0.07	0.00	0.00
<i>single-party legislature + executive</i>	0.19	0.16	0.13	0.16	0.02	0.04
<i>multiparty legislature</i>	0.10	0.08	0.16	0.09	0.00	0.00
<i>multiparty legislature + executive</i>	0.33	0.37	0.36	0.46	0.14	0.19

Regimes: 202; country-years: 4,532. “Interlude” reports the proportion of regimes that had a given type of legislature 100% or 90% of the time, respectively.

Table 1 presents the descriptive statistics, split into two samples. In the averaged sample, the unit of observation is the *authoritarian regime*: the outcome variables are average annual values over a regime’s lifetime (for all years for which there is data), while the legislature indicators and the control variables correspond to the first year the regime appears in the sample.¹² Autocracies that lasted for less than five years were excluded in order to make the results less sensible to particularly short-lived regimes. I will use this sample to determine the long-run effects of having (some kind of) legislature *at the beginning*. The country-year sample, on the other hand, will be used to determine whether that autocracies that introduced a legislature (or closed one) saw an improvement (slump) in their economic performance.

As Table 2 show, while there is substantial variation in the extent to which a given regime maintains the same legislative institutions over time, a nontrivial fraction do not introduce changes. Especially for the first four rows, the “first year” and “last year” columns take similar values, meaning that the overall proportion of autocracies that had a given kind of legislature at the beginning and the end did not change substantially. What is more, the “100%” and “90%” columns indicate that 5% and 51% of regimes, either never had a legislature or always had one, while 8% and 61% did not

¹²That is, regimes that were established before 1946 are coded depending on the institutions they had when they first appeared in the data, which may not be the ones they had at their founding.

or did have one for at least 90% of country-years. The last five rows of the table shows that making a more nuanced distinction according to the kind of legislature in place introduces more within-regime variability, though as Figure 2 shows, many of these are short-lived, and changes of the kind of legislature within a country are often associated with *regime* changes.

Results

Cross-sectional analysis. I estimate OLS models of the form

$$\bar{y}_r = \alpha + \sum \beta^i \cdot legislature_r^i + \theta \cdot \mathbf{C}_r + \varepsilon_r,$$

where \bar{y}_r is the average value of the outcome variable during regime r 's lifetime; $legislature_r^i$, either a dummy or a factor with multiple levels, indicates what kind of legislature regime r had at the beginning of its second year in office;¹³ \mathbf{C}_r is a vector of controls; and ε_r is the error term. I cluster the standard errors by country to account for the fact that some countries have lived under multiple regimes. Very weak autocracies may fare worse economically and be less likely to have a legislature, so I exclude all regimes that survived for less than five years from the sample. To account for heterogeneous effects, in some specifications I interact $legislature_r^i$ with a regime type dummy taken from GWF: monarchy (the baseline), party, military, or personal.¹⁴

The combination of (a) three specifications – no controls; region fixed effects;¹⁵ and region fixed effects plus *population* (log), *area* (km^2) (log), *GDP per capita* (log) and *oil and gas per capita* (log) –; and (b) three different measures of authoritarian legislatures – with up to six different levels –, interacted with (c) four measures of authoritarian regime types, produces a total of 120 estimates

¹³Following GWF, I measure a regime's institutions as they stood on January 1st of a given year; thus, if a regime was originally established in year t , I report the corresponding value of $legislature_r^i$ for year $t + 1$.

¹⁴Mixed-type regimes (e.g., military-personal) are coded as military *and* personal, and so on.

¹⁵I 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).

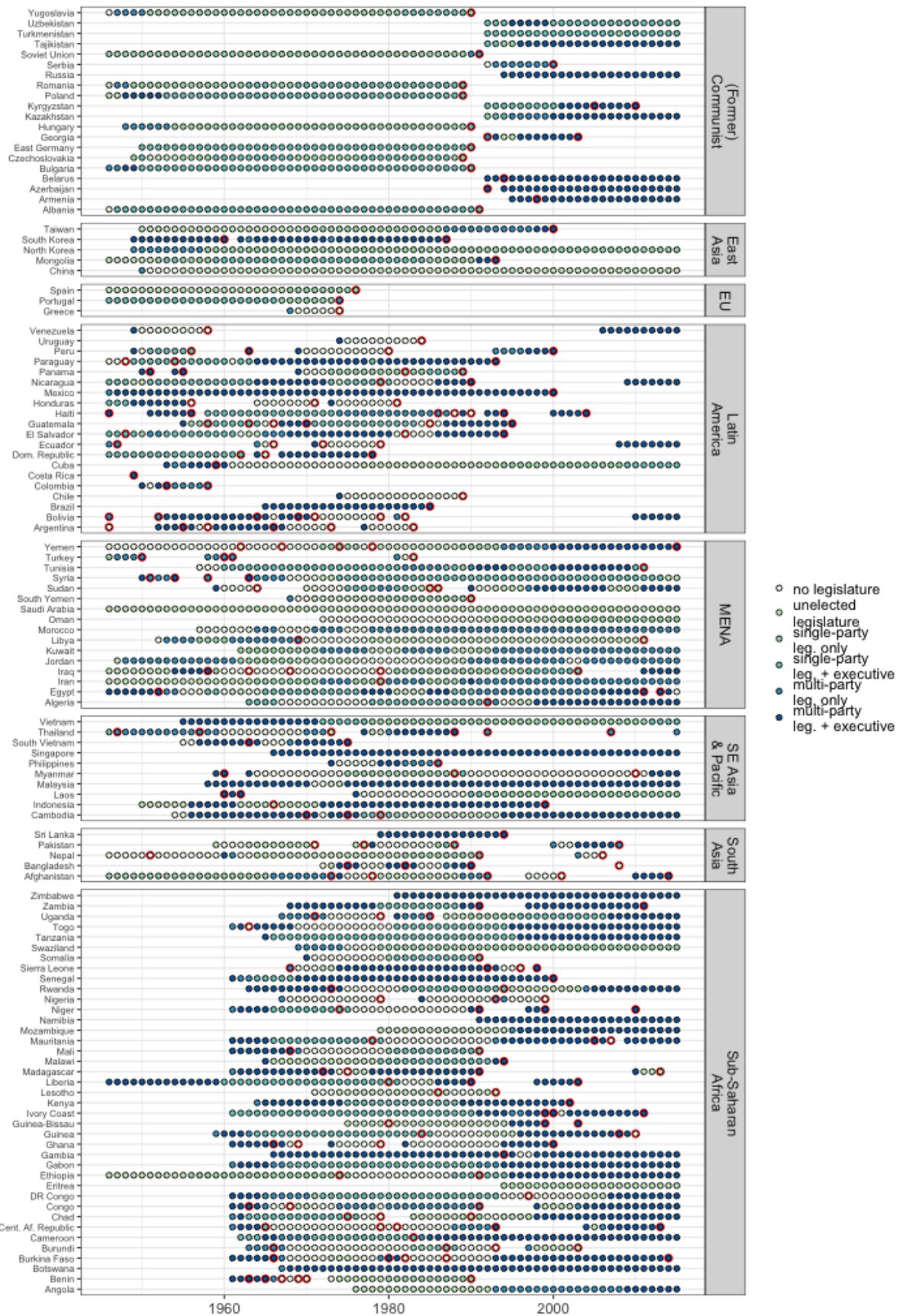


Figure 2: Country-years with authoritarian legislatures, 1946-2015. Red border indicate regime breakdowns.

for every outcome variable. To simplify the interpretation of the results, Figure 3 reports the point estimates and 95% confidence intervals for the estimates of interest, while the full set of results are relegated to Tables A3 to A5 in the Online Appendix.

While the large number of coefficients naturally introduces some variability in the estimates, the overall trend is clear: there is no evidence that authoritarian legislatures lead to improved economic performance. Consider the first column of Figure 3, which looks at the average (annual) growth rate during a regime's lifetime. The top panel shows that the estimates of the effect of having a legislature at the beginning of a regime's second year in office are not only statistically significant, but they are very close to zero in absolute terms, and remain relatively unchanged when introducing controls. Conditioning for regime type does not change the results; if anything, legislatures improve the economic performance of military and *personalist* regimes somewhat, but the estimates are far from being statistically significant at conventional levels.

The next panel examines whether the null effect for legislatures may be masking differences between single- and multi-party legislatures. This does not seem to be the case: for both single-party and multi-party regimes, the estimate is very close to zero in absolute terms, and far from statistically significant. Including interactions by regime type introduces some variability in the results, but no clear pattern emerges. Out of 24 estimates, only one – the interaction between a *multi-party legislature* and a *monarchy* in the specification with controls – is statistically significant at the 0.05 percent level; all other coefficients are much smaller in absolute terms and far from statistically significant. At most, there is weak evidence that military regimes perform better when there is a single-party legislature; the results for both party-based and personalist regimes are unremarkable.

Lastly, the panel at the bottom of Figure 3 shows that (a) distinguishing between unelected and single-party legislatures; and (b) accounting for the executive's mode of selection does not alter the results either. Again, just looking at the kind of legislature in place produces unremarkable results, while including interaction terms does introduce some variability in the results but without showing any clear pattern. The most remarkable result is that military regimes grow at (substantially) higher

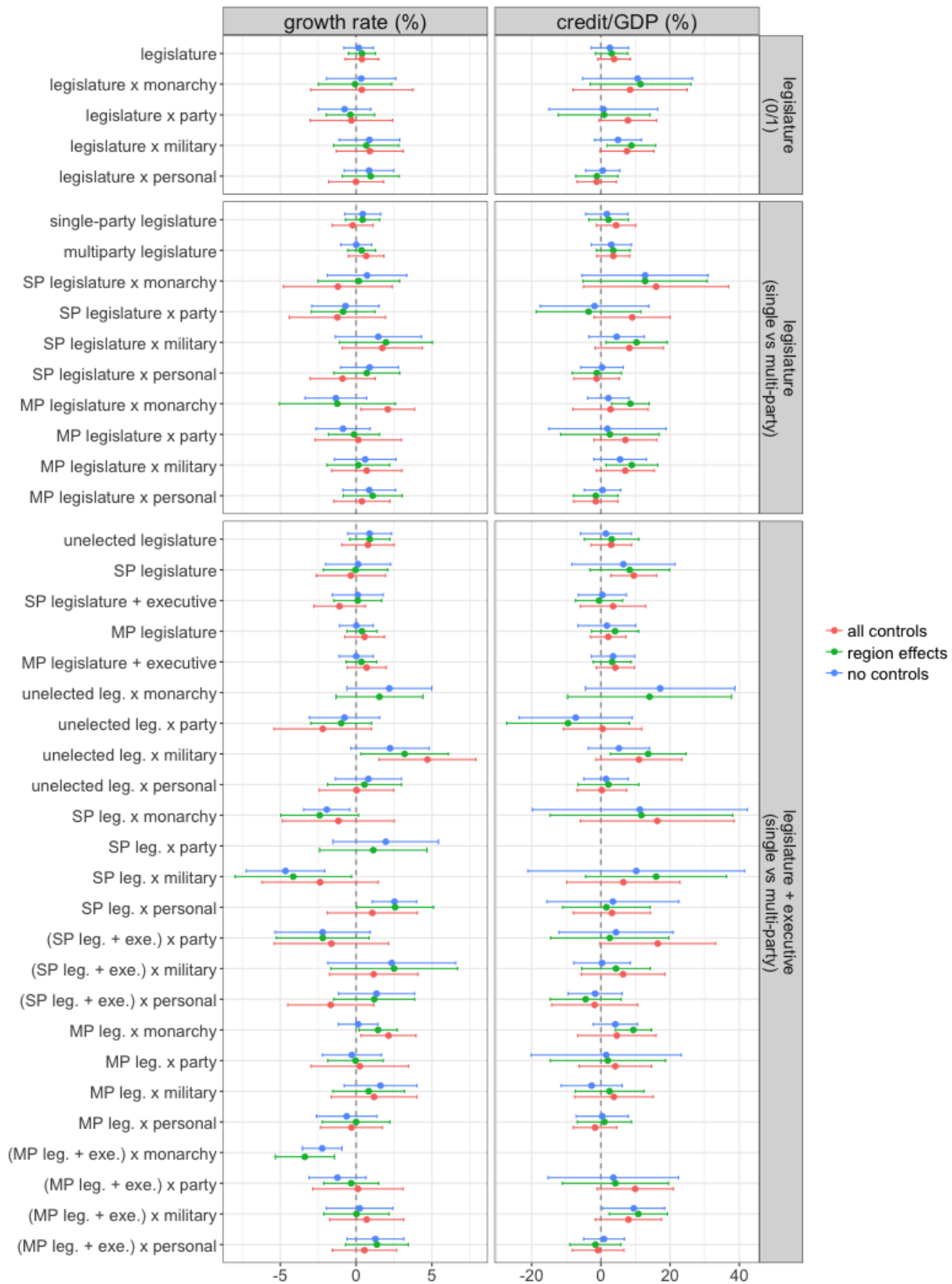


Figure 3: Between-regime results. Point estimates and 95% confidence intervals of the effect of authoritarian legislatures on economic performance. Exceptionally large estimates and wide confidence intervals are not shown in their entirety so smaller estimates are easier to appreciate. See Tables A3 to A5 for the full set of estimates.

rates when they had an unelected legislature at the beginning, but fare substantially worse when the legislature was elected in single-party elections and the executive was unelected. Contrary to expectations, personalist regimes grow at higher average rates (an extra 2.5 pp. per year) when the legislature was elected in single-party elections whereas the executive was not, but this result is sensitive to the inclusion of controls. There is little evidence that the performance of party-based regimes depends on the presence of a legislature, and contrary to Gehlbach and Keefer's (2012) findings, the results for multi-party legislatures are especially weak.

Even if legislatures do not improve economic growth, the guarantees they provide against expropriation by regime (Gehlbach and Keefer 2011, 2012; Wilson and Wright 2015) or private (Jensen, Malesky and Weymouth 2014) actors may nonetheless encourage private investment. Thus, the left column of Figure 3 looks at their effect on *credit/GDP (%)*. The estimates become substantially larger in magnitude, mainly because the outcome takes much higher values on average and displays much more variability (see Table 1). The overall gist of the results does not change, however: there is little evidence that authoritarian legislatures increase private investment in the long run, even for a defined subset of regime types: the effect for personalist regimes is close to zero in absolute terms, but the same applies to party-based regimes; military regimes do perform somewhat better (between +5 and +10 pp.) when both the legislature and the executive are elected in formally multiparty elections, but the strength of this result is very sensitive to the inclusion of controls. In sum, and even before accounting for the fact that autocracies with legislatures may be systematically different from those that lack them, there is little evidence that having a legislature is associated with subsequent economic performance, even for a subset of autocracies.

Within estimates. In this case, I ran OLS specifications of the form

$$y_{r,t} = \sum \beta^i \cdot legislature_{r,t}^i + \theta \cdot C_{r,t-1} + \mu_r + \delta_t + \varepsilon_{r,t},$$

where $y_{r,t}$ is the growth rate (%) or provision of private credit as % of GDP for regime r in year t ; $legislature_{r,t}^i$ is a (set of) dummies indicating the kind of legislature in place in regime r at the beginning of year t ; $C_{r,t-1}$ is a vector of time-varying controls; μ_r and δ_t are time and year fixed-effects respectively, and $\varepsilon_{r,t}$ is the error term. Standard errors are clustered by regime. Again, regimes that survived for less than five years are excluded from the sample.

Figure 4 reports the results graphically, while Tables A6 to A8 in the Online Appendix provide the full set of estimates. The fact that some regimes switched between legislatures for only a handful of years (see Figure 2) results in somewhat unstable estimates, especially when including interaction terms, but the gist of the results is the same: there is little evidence that authoritarian legislatures do make a difference for economic performance.

Consider the results for *growth rate (%)* first. As in Figure 3, just employing the (type of) legislature as a predictor results in substantively small and statistically insignificant estimates. Moreover, and in line with the previous results, there is little difference between the pooled models and the fixed-effects specifications, implying that the null findings are not the product of restricting the analysis to within-regime variation. The interaction terms introduce some heterogeneity in the results, but again the overall direction is toward null findings. The top panel of Figure 4 shows little variation by regime type, as the results remain very small in absolute terms and far from statistically significant at conventional levels. Contrary to expectations, the estimates are generally positive for *personalist* regimes, though never significant. The next panel shows that these results are driven by personalist regimes with multi-party legislatures, but even though the magnitude of the effect is not trivial (between 0.85 and 1.40 additional percentage points per year; see Table A7), both their magnitude and reliability is sensitive to the inclusion of controls. For other kinds of regimes, the results are both smaller in absolute size and less reliably estimated; in contrast with the previous findings, military regimes seem to do worse when there is a multiparty legislature, but the estimate is not entirely precise. The bottom panel shows that introducing even more legislature \times regime combinations does not change the overall results. Party-based regimes appear to grow somewhat faster

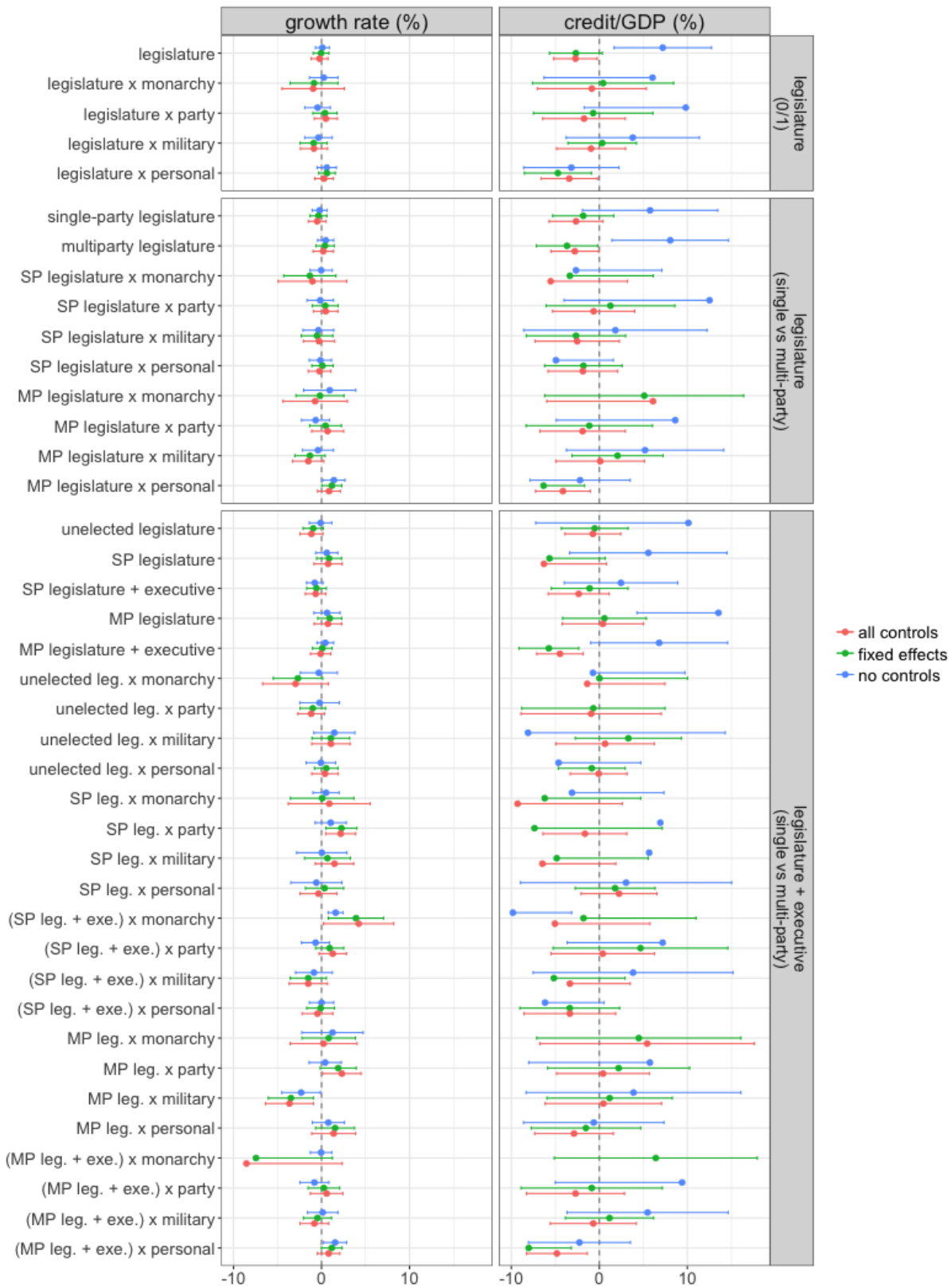


Figure 4: Within-regime results. Point estimates and 95% confidence intervals of the effect of authoritarian legislatures on economic performance. Exceptionally large estimates and wide confidence intervals are not shown in their entirety so smaller estimates are easier to appreciate. See Tables A6 to A8 for the full set of estimates.

when there is a single- or a multi-party legislature (but not an executive), but multi-party legislatures in personalist regimes have a similar effect, and in any case, these findings are not statistically significant. The only substantial difference is for monarchies, where the sign and the magnitude of the results vary greatly depending on the kind of legislature in place. Given the relative rarity of such regimes (see Table 1), these results should be taken with a grain of salt.

The rightmost column of Figure 4 shows that the inclusion of regime fixed effects do make a difference for explaining the availability of private credit in autocracies: the pooled estimates are usually positive, large in magnitude, and statistically significant at conventional levels. In line with Gehlbach and Keefer (2012), these results are especially strong – of about 15 percentage points, if not more – for autocracies with a multiparty legislature. However, the inclusion of regime fixed effects drives these estimates towards zero and even makes them negative, suggesting that the pooled estimates were capturing (unobserved) differences between autocracies rather than variation within regimes over time. The interaction terms indicate that personalist regimes have a negative effect on investment in personalist regimes. The bottom two panels suggest that this effect is driven by multi-party legislatures, especially when the executive is also elected in multi-party contests. While this finding is consistent with the results reported by Wright (2008b) and Wilson and Wright (2015), the lack of findings about the effect of authoritarian legislatures overall makes it unclear whether this represents a genuine effect – in which case, it is unclear why the estimates are negative –, a statistical fluke, or the fact that personalist regimes are especially likely to introduce multiparty elections in contexts that discourage the provision of credit. The fact that several African countries – many of which are coded as “personalist” by GWF – adopted multiparty elections following severe economic shocks in the late 1980s or early 1990s (Bratton and van de Walle 1997; Burke and Leigh 2010; Brückner and Ciccone 2011; Aidt and Leon 2016) gives credence to this latter interpretation.

Discussion and Conclusion

The increased awareness that democratic-looking institutions like parties, elections, and legislatures are relatively common in autocracies has naturally increased political scientists' interest in where such institutions come from, and what they do (Gandhi and Lust-Okar 2009; Magaloni and Kricheli 2010; Morse 2012; Svolik 2012; Brancati 2014; Schuler and Malesky 2014). Yet in order to answer these questions, we must first get the facts right. Political scientists first explained authoritarian elections as information-generating devices (Gandhi 2008b,a; Gandhi and Lust-Okar 2009; Cox 2009; Magaloni and Kricheli 2010; Little 2015), a view that was contradicted by several case studies (Tucker 2007; Kuntz and Thompson 2009; Levitsky and Way 2010) that showed how risky such devices could be. The result was a second generation of theoretical studies that endogeneized autocrats' decision to concede electoral defeat (Fearon 2011; Little, Tucker and LaGatta 2015), which are in line with recent – and more sophisticated – empirical studies (see Knutsen, Nygård and Wig 2017; Schuler, Gueorguiev and Cantú 2017; Lucardi forthcoming). Similarly, the claim that party institutionalization is associated with regime survival (Geddes 2006; Brownlee 2007; Svolik 2012) has been contested with the argument that it is the conditions under which parties are formed, rather than their levels of institutionalization, that make a difference (Levitsky and Way 2012, 2013; Meng 2017a). More sophisticated measures of party institutionalization in autocracies do indeed lead credence to this interpretation (Meng 2017b; Lucardi 2017).

This paper attempts to do the same with respect to authoritarian legislatures. The results presented here showed that by either including many more observations than recent studies *or* accounting for regime-specific effects, the relationship between authoritarian legislatures and economic performance disappears. These results hold across multiple combinations of legislature and regime types, implying that the lack of a relationship is not driven by an extremely narrow definition of what counts as an authoritarian legislature. This is especially important given that several scholars see the relationship between authoritarian legislatures and economic performance as rela-

tively robust (Jensen, Malesky and Weymouth 2014; Wilson and Wright 2015). In line with the case study literature on individual authoritarian legislatures (Desposato 2001; Blaydes 2010; Loidolt and Mecham 2016; Malesky and Schuler 2010, 2011; Malesky, Schuler and Tran 2012; Schuler [forthcoming](#); Truex 2014; Reuter and Robertson 2015), this paper suggest that this is not the case: whatever authoritarian legislatures do – and they must be doing something if they are so common –, constraining incumbents to promote investment and hence economic growth does not seem to be the main story.

But if so, what do these legislatures do? A full discussion of this question is outside the scope of this paper, but two things are worth noting. One is that another major explanation of authoritarian legislatures is that they contribute to regime survival (Svolik 2012; Boix and Svolik 2013). While this is a plausible story, finding support for it can be complicated by several reasons, from endogeneity concerns (Pepinsky 2014) to the fact that authoritarian legislatures are very common – Table 1 shows that 87% of authoritarian country-years featured one –, while breakdowns are fairly rare. Moreover, while the qualitative and the quantitative evidence linking competitive elections to breakdown is substantial (Tucker 2007; Kuntz and Thompson 2009; Levitsky and Way 2010; Knutsen, Nygård and Wig 2017; Schuler, Gueorguiev and Cantú 2017; Lucardi [forthcoming](#)), there is little *direct* evidence linking authoritarian legislatures to regime survival. Rather, what evidence we have points in the direction of authoritarian legislatures serving as a fora for co-opting (potential) opposition leaders and distributing rents among regime supporters, plus providing some (limited) information about the performance of state officials. While these can certainly contribute to a regime's survival, the relative rarity of authoritarian breakdowns, coupled with the fact that legislatures are not the forum where “high politics” is decided, suggest that the effect of legislatures on survival will be weak at best. Rather than looking at momentous events such as alternation in office, we should follow the lead of the case study literature and examine more modest outcomes, such as legislature's (or legislative elections') capacity to co-opt potential opponents or distribute rents.

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Online Appendix for “Authoritarian Legislatures”

- 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.
- Section [C](#) reports the full results for the figures included in the text.

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

- (1) *Legislative professionalization*. Table A2 presents the estimates and standard errors for the numbers reported in Figure 1.
- (2) *Between estimates*. Tables A3 to A5 present the coefficients and standard errors for the estimates reported in Figure 3. Table A3 presents the results for the models with no interactions, while Tables A4 and Tables A5 presents the results for the interactions with the first two and the third measures of authoritarian legislatures, respectively.
- (3) *Within estimates*. Tables A6 to A8 present the coefficients and standard errors for the estimates reported in Figure 4. Table A6 presents the results for the models with no interactions, while Tables A7 and Tables A8 presents the results for the interactions with the first two and the third measures of authoritarian legislatures, respectively.

Table A2: Regime type and legislative professionalization, 1946-2015

	advanced democ.	other democ.	electoral auth.	closed auth.	N
	(1)	(2)	(3)	(4)	(5)
<i>legislature questions officials in practice</i>	1.83 (0.09)	1.07 (0.11)	-0.25 (0.11)	-0.78 (0.12)	7513
<i>legislature would investigate officials in practice</i>	1.96 (0.17)	0.91 (0.10)	-0.56 (0.14)	-1.12 (0.13)	7512
<i>opposition parties can exercise oversight</i>	2.11 (0.13)	1.35 (0.09)	-0.47 (0.11)	-1.42 (0.11)	7513
<i>the lower chamber legislates in practice</i>	1.56 (0.08)	1.11 (0.07)	0.34 (0.10)	-0.58 (0.13)	7519
<i>lower chamber has a functioning committee system</i>	1.54 (0.17)	1.21 (0.08)	0.33 (0.07)	-0.25 (0.10)	7516
<i>legislature controls its own funds in practice</i>	1.57 (0.10)	1.11 (0.09)	-0.10 (0.11)	-0.80 (0.11)	7517
<i>one staff member for each lower house member</i>	0.23 (0.28)	0.57 (0.13)	0.14 (0.12)	-0.53 (0.11)	7516
<i>legislators do not abuse position for financial gain</i>	1.75 (0.18)	-0.35 (0.11)	-0.60 (0.15)	-0.01 (0.15)	7512
<i>average of all measures</i>	1.57 (0.09)	0.87 (0.07)	-0.15 (0.06)	-0.69 (0.07)	7508

Estimates report mean values of legislative professionalization based on the latent variable estimates reported by Coppedge et al. (2017). The unit of observation is the country-year. Robust standard errors clustered by regime in parentheses. (i) *Advanced democracies*: country-years coded as “democratic” by Geddes, Wright and Frantz (2014) that correspond to *Western Europe and British Settler colonies* (Miller forthcoming). (ii) *Other democracies*: all other democratic country-years. (iii) *Electoral authoritarian*: country-years coded as autocratic by Geddes, Wright and Frantz (2014) where both the executive and the legislature are elected in competitive multiparty elections according to Coppedge et al. (2017). (iv) *Closed authoritarian*: all other authoritarian country-years.

Table A3: Authoritarian legislatures and economic performance: Between estimates (1)

	avg. growth rate (%)			avg. credit/GDP (%)		
(a) Legislature (0/1)	(1)	(2)	(3)	(4)	(5)	(6)
<i>legislature</i>	0.169 (0.493)	0.388 (0.450)	0.379 (0.557)	2.586 (2.724)	3.105 (2.362)	3.826 (2.370)
(b) Legislature (single vs. multi-party)						
<i>SP legislature</i>	0.436 (0.603)	0.423 (0.573)	-0.229 (0.682)	1.704 (3.089)	2.235 (2.895)	4.391 (2.878)
<i>MP legislature</i>	0.012 (0.516)	0.364 (0.464)	0.666 (0.591)	3.007 (2.934)	3.562 (2.466)	3.580 (2.410)
(c) Legislature (all types)						
<i>unelected legislature</i>	0.891 (0.731)	0.901 (0.671)	0.781 (0.872)	1.438 (3.720)	3.108 (3.973)	2.984 (2.954)
<i>SP legislature</i>	0.134 (1.087)	-0.036 (1.077)	-0.343 (1.155)	6.497 (7.549)	8.317 (5.827)	9.515 (3.339)
<i>SP legislature + executive</i>	0.108 (0.852)	0.109 (0.799)	-1.096 (0.856)	0.428 (3.511)	-0.521 (3.439)	3.516 (4.768)
<i>MP legislature</i>	0.019 (0.564)	0.389 (0.501)	0.563 (0.664)	1.726 (4.230)	4.102 (3.447)	2.142 (2.579)
<i>MP legislature + executive</i>	0.009 (0.570)	0.350 (0.511)	0.695 (0.647)	3.515 (3.174)	3.240 (2.775)	4.182 (2.800)
region FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	189	189	130	152	152	122

OLS regression estimates. Robust standard errors clustered by country in parentheses. Models 3 and 6 control for *population* (log), *area* (*sq. km*) (log), *GDP per capita* (log) and *oil and gas per capita* (log), plus a set of dummies for regime type.

Table A4: Authoritarian legislatures and economic performance: Between estimates (2)

	avg. growth rate (%)			avg. credit/GDP (%)		
(a) Legislature (0/1) + interactions	(1)	(2)	(3)	(4)	(5)	(6)
<i>legislature</i> × <i>monarchy</i>	0.335 (1.156)	-0.073 (1.226)	0.374 (1.701)	10.597 (8.040)	11.455 (7.370)	8.441 (8.319)
<i>legislature</i> × <i>party</i>	-0.757 (0.878)	-0.376 (0.809)	-0.309 (1.374)	0.731 (7.948)	0.926 (6.713)	7.794 (4.205)
<i>legislature</i> × <i>military</i>	0.882 (1.013)	0.671 (1.092)	0.904 (1.114)	4.933 (3.418)	8.800 (3.548)	7.514 (3.938)
<i>legislature</i> × <i>personal</i>	0.858 (0.832)	0.960 (0.951)	-0.004 (0.914)	0.541 (2.504)	-1.149 (3.078)	-1.189 (2.862)
(b) Legislature (single vs. multi-party) + interactions						
<i>SP legislature</i> × <i>monarchy</i>	0.724 (1.330)	0.175 (1.364)	-1.205 (1.811)	12.763 (9.226)	12.768 (9.092)	15.947 (10.596)
<i>SP legislature</i> × <i>party</i>	-0.703 (1.122)	-0.861 (1.065)	-1.234 (1.604)	-1.836 (7.934)	-3.558 (7.656)	9.033 (5.542)
<i>SP legislature</i> × <i>military</i>	1.466 (1.439)	1.966 (1.554)	1.729 (1.334)	4.559 (4.043)	10.296 (4.478)	8.233 (4.977)
<i>SP legislature</i> × <i>personal</i>	0.889 (0.968)	0.704 (1.103)	-0.888 (1.089)	0.341 (3.108)	-1.184 (3.601)	-1.241 (3.319)
<i>MP legislature</i> × <i>monarchy</i>	-1.339 (1.032)	-1.238 (1.942)	2.090 (0.897)	2.148 (3.023)	8.529 (2.738)	2.781 (5.498)
<i>MP legislature</i> × <i>party</i>	-0.866 (0.903)	-0.141 (0.853)	0.145 (1.440)	1.917 (8.571)	2.610 (7.207)	7.080 (4.590)
<i>MP legislature</i> × <i>military</i>	0.599 (1.032)	0.145 (1.049)	0.699 (1.171)	5.550 (3.846)	8.925 (3.787)	7.046 (4.235)
<i>MP legislature</i> × <i>personal</i>	0.869 (0.880)	1.095 (0.986)	0.383 (0.936)	0.488 (2.642)	-1.470 (3.260)	-1.460 (3.232)
region FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	189	189	130	152	152	122

OLS regression estimates. Robust standard errors clustered by country in parentheses. Models 3 and 6 control for *population* (log), *area* (*sq. km*) (log), *GDP per capita* (log) and *oil and gas per capita* (log), plus a set of dummies for regime type.

Table A5: Authoritarian legislatures and economic performance: Between estimates (3)

Legislature (all types) + interactions	avg. growth rate (%)			avg. credit/GDP (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>unelected legislature</i>	2.191	1.541		17.126	14.057	
× <i>monarchy</i>	(1.421)	(1.453)		(10.934)	(12.000)	
<i>unelected legislature</i>	-0.763	-0.980	-2.195	-7.274	-9.505	0.517
× <i>party</i>	(1.175)	(1.017)	(1.623)	(8.276)	(8.973)	(5.714)
<i>unelected legislature</i>	2.241	3.202	4.706	5.221	13.674	10.990
× <i>military</i>	(1.310)	(1.460)	(1.612)	(4.483)	(5.535)	(6.274)
<i>unelected legislature</i>	0.810	0.556	0.030	1.480	2.186	0.284
× <i>personal</i>	(1.106)	(1.235)	(1.241)	(3.251)	(4.445)	(3.615)
<i>SP legislature</i>	-1.935	-2.400	-1.170	11.264	11.703	16.280
× <i>monarchy</i>	(0.773)	(1.299)	(1.865)	(15.718)	(13.346)	(11.224)
<i>SP legislature</i>	1.955	1.134				
× <i>party</i>	(1.764)	(1.798)				
<i>SP legislature</i>	-4.659	-4.144	-2.375	10.219	15.957	6.491
× <i>military</i>	(1.317)	(1.945)	(1.942)	(15.829)	(10.300)	(8.260)
<i>SP legislature</i>	2.529	2.570	1.069	3.465	1.594	3.198
× <i>personal</i>	(0.743)	(1.286)	(1.500)	(9.645)	(6.399)	(5.626)
<i>SP legislature + executive</i>	-2.210	-2.198	-1.628	4.352	2.556	16.413
× <i>party</i>	(1.586)	(1.551)	(1.906)	(8.346)	(8.637)	(8.458)
<i>SP legislature + executive</i>	2.354	2.508	1.157	0.332	4.357	6.401
× <i>military</i>	(2.135)	(2.115)	(1.477)	(4.135)	(5.021)	(6.102)
<i>SP legislature + executive</i>	1.350	1.198	-1.671	-1.661	-4.395	-1.814
× <i>personal</i>	(1.272)	(1.354)	(1.434)	(3.946)	(5.189)	(6.263)
<i>MP legislature</i>	0.136	1.454	2.139	4.166	9.392	4.594
× <i>monarchy</i>	(0.664)	(0.633)	(0.919)	(3.238)	(2.644)	(5.715)
<i>MP legislature</i>	-0.281	-0.039	0.244	1.537	2.017	4.152
× <i>party</i>	(0.983)	(0.926)	(1.627)	(10.980)	(8.428)	(5.288)
<i>MP legislature</i>	1.613	0.830	1.184	-2.674	2.494	3.771
× <i>military</i>	(1.216)	(1.196)	(1.431)	(4.459)	(5.013)	(5.717)
<i>MP legislature</i>	-0.625	0.002	-0.305	0.407	1.003	-1.687
× <i>personal</i>	(1.010)	(1.133)	(1.031)	(3.820)	(3.959)	(3.166)
<i>MP legislature + executive</i>	-2.238	-3.380				
× <i>monarchy</i>	(0.664)	(0.988)				
<i>MP legislature + executive</i>	-1.228	-0.320	0.122	3.590	4.184	9.909
× <i>party</i>	(0.952)	(0.915)	(1.506)	(9.535)	(7.746)	(5.556)
<i>MP legislature + executive</i>	0.233	0.024	0.694	9.490	10.820	7.974
× <i>military</i>	(1.115)	(1.092)	(1.236)	(4.519)	(4.251)	(4.814)
<i>MP legislature + executive</i>	1.276	1.371	0.552	0.880	-1.586	-0.811
× <i>personal</i>	(0.951)	(1.048)	(1.068)	(2.970)	(3.737)	(3.758)
region FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	189	189	130	152	152	122

Table A6: Authoritarian legislatures and economic performance: Within estimates (1)

	growth rate (%)			credit/GDP (%)		
(a) Legislature (0/1)	(1)	(2)	(3)	(4)	(5)	(6)
<i>legislature</i>	0.106 (0.405)	-0.055 (0.455)	-0.227 (0.488)	7.205 (2.827)	-2.670 (1.533)	-2.713 (1.260)
(b) Legislature (single vs. multi-party)						
<i>SP legislature</i>	-0.213 (0.435)	-0.351 (0.488)	-0.496 (0.514)	5.764 (3.919)	-1.834 (1.772)	-2.651 (1.562)
<i>MP legislature</i>	0.473 (0.466)	0.398 (0.532)	0.197 (0.594)	8.057 (3.380)	-3.674 (1.778)	-2.789 (1.382)
(c) Legislature (all types)						
<i>unelected legislature</i>	-0.118 (0.655)	-0.947 (0.588)	-1.146 (0.669)	10.115 (8.849)	-0.529 (1.935)	-0.737 (1.614)
<i>SP legislature</i>	0.590 (0.643)	0.853 (0.723)	0.743 (0.818)	5.564 (4.558)	-5.658 (3.222)	-6.296 (3.624)
<i>SP legislature + executive</i>	-0.770 (0.476)	-0.574 (0.568)	-0.676 (0.595)	2.457 (3.290)	-1.102 (2.219)	-2.347 (1.766)
<i>MP legislature</i>	0.622 (0.760)	0.930 (0.691)	0.718 (0.795)	13.535 (4.724)	0.581 (2.426)	0.397 (2.356)
<i>MP legislature + executive</i>	0.426 (0.484)	0.086 (0.565)	-0.104 (0.588)	6.795 (3.968)	-5.741 (1.733)	-4.472 (1.345)
regime FE	no	yes	yes	no	yes	yes
year FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	4018	4018	3750	2875	2875	2603
regimes	192	192	184	155	155	150
years	65	65	65	56	56	51

OLS regression estimates. Robust standard errors clustered by regime in parentheses. Models 3 and 6 control for $GDP\ per\ capita_{t-1}$ (log) and $oil\ and\ gas\ per\ capita_{t-1}$ (log).

Table A7: Authoritarian legislatures and economic performance: Within estimates (2)

	growth rate (%)			credit/GDP (%)		
(a) Legislature (0/1) + interactions	(1)	(2)	(3)	(4)	(5)	(6)
<i>legislature</i> × <i>monarchy</i>	0.250 (0.826)	-0.850 (1.385)	-0.955 (1.806)	6.044 (6.290)	0.422 (4.091)	-0.854 (3.159)
<i>legislature</i> × <i>party</i>	-0.454 (0.740)	0.387 (0.696)	0.487 (0.672)	9.822 (5.893)	-0.699 (3.459)	-1.739 (2.393)
<i>legislature</i> × <i>military</i>	-0.351 (0.795)	-0.898 (0.786)	-0.859 (0.785)	3.796 (3.862)	0.323 (1.984)	-0.941 (1.998)
<i>legislature</i> × <i>personal</i>	0.590 (0.558)	0.611 (0.484)	0.286 (0.537)	-3.184 (2.761)	-4.712 (1.941)	-3.421 (1.633)
(b) Legislature (single vs. multi-party) + interactions						
<i>SP legislature</i> × <i>monarchy</i>	-0.039 (0.643)	-1.328 (1.514)	-1.035 (1.989)	-2.659 (4.983)	-3.349 (4.831)	-5.535 (4.450)
<i>SP legislature</i> × <i>party</i>	-0.150 (0.770)	0.412 (0.753)	0.470 (0.710)	12.524 (8.435)	1.256 (3.732)	-0.647 (2.380)
<i>SP legislature</i> × <i>military</i>	-0.360 (0.894)	-0.508 (0.916)	-0.286 (0.908)	1.822 (5.315)	-2.661 (2.877)	-2.521 (2.445)
<i>SP legislature</i> × <i>personal</i>	-0.132 (0.646)	0.109 (0.617)	-0.223 (0.656)	-4.933 (3.326)	-1.812 (2.243)	-1.876 (2.018)
<i>MP legislature</i> × <i>monarchy</i>	0.929 (1.510)	-0.181 (1.391)	-0.728 (1.867)	20.274 (8.012)	5.097 (5.766)	6.095 (6.156)
<i>MP legislature</i> × <i>party</i>	-0.675 (0.809)	0.453 (0.919)	0.691 (0.928)	8.616 (6.885)	-1.147 (3.661)	-1.902 (2.475)
<i>MP legislature</i> × <i>military</i>	-0.407 (0.903)	-1.313 (0.876)	-1.508 (0.903)	5.195 (4.559)	2.067 (2.639)	0.096 (2.569)
<i>MP legislature</i> × <i>personal</i>	1.399 (0.646)	1.169 (0.577)	0.848 (0.657)	-2.191 (2.903)	-6.341 (2.377)	-4.139 (1.586)
regime FE	no	yes	yes	no	yes	yes
year FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	4018	4018	3750	2875	2875	2603
regimes	192	192	184	155	155	150
years	65	65	65	56	56	51

OLS regression estimates. Robust standard errors clustered by regime in parentheses. Models 3 and 6 control for $GDP\ per\ capita_{t-1}$ (log) and $oil\ and\ gas\ per\ capita_{t-1}$ (log).

Table A8: Authoritarian legislatures and economic performance: Within estimates (3)

Legislature (all types) + interactions	growth rate (%)			credit/GDP (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>unelected legislature</i>	-0.310	-2.680	-2.956	-0.727	0.013	-1.377
× <i>monarchy</i>	(1.073)	(1.428)	(1.907)	(5.333)	(5.107)	(4.500)
<i>unelected legislature</i>	-0.226	-0.994	-1.172	30.020	-0.681	-0.934
× <i>party</i>	(1.144)	(0.737)	(0.774)	(20.026)	(4.154)	(4.064)
<i>unelected legislature</i>	1.449	1.052	1.064	-8.107	3.292	0.650
× <i>military</i>	(1.189)	(1.091)	(1.111)	(11.431)	(3.074)	(2.855)
<i>unelected legislature</i>	-0.072	0.534	0.397	-4.636	-0.864	-0.079
× <i>personal</i>	(0.858)	(0.681)	(0.770)	(4.763)	(1.939)	(1.648)
<i>SP legislature</i>	0.506	0.076	0.877	-3.111	-6.209	-9.290
× <i>monarchy</i>	(0.757)	(1.841)	(2.374)	(5.326)	(5.567)	(6.071)
<i>SP legislature</i>	1.043	2.254	2.175	6.927	-7.373	-1.640
× <i>party</i>	(0.902)	(0.895)	(0.871)	(13.053)	(7.404)	(2.432)
<i>SP legislature</i>	0.033	0.673	1.470	5.659	-4.846	-6.468
× <i>military</i>	(1.456)	(1.328)	(1.114)	(13.021)	(5.301)	(4.250)
<i>SP legislature</i>	-0.585	0.340	-0.354	3.035	1.787	2.235
× <i>personal</i>	(1.478)	(1.106)	(1.066)	(6.116)	(2.315)	(2.198)
<i>SP legislature + executive</i>	1.601	3.911	4.215	-9.827	-1.802	-5.048
× <i>monarchy</i>	(0.435)	(1.603)	(2.037)	(3.404)	(6.525)	(5.507)
<i>SP legislature + executive</i>	-0.683	0.917	1.272	7.187	4.670	0.384
× <i>party</i>	(0.814)	(0.812)	(0.796)	(5.528)	(5.065)	(2.997)
<i>SP legislature + executive</i>	-0.859	-1.508	-1.505	3.832	-5.173	-3.343
× <i>military</i>	(1.064)	(1.045)	(1.109)	(5.794)	(4.126)	(3.492)
<i>SP legislature + executive</i>	-0.000	-0.105	-0.460	-6.173	-3.364	-3.357
× <i>personal</i>	(0.706)	(0.803)	(0.889)	(3.426)	(2.888)	(2.658)
<i>MP legislature</i>	1.251	0.815	0.230	21.386	4.481	5.433
× <i>monarchy</i>	(1.777)	(1.548)	(1.937)	(7.835)	(5.917)	(6.217)
<i>MP legislature</i>	0.413	1.893	2.305	5.742	2.189	0.419
× <i>party</i>	(0.934)	(1.043)	(1.114)	(7.020)	(4.113)	(2.691)
<i>MP legislature</i>	-2.320	-3.483	-3.645	3.882	1.167	0.455
× <i>military</i>	(1.119)	(1.309)	(1.393)	(6.224)	(3.632)	(3.372)
<i>MP legislature</i>	0.779	1.537	1.378	-0.639	-1.523	-2.880
× <i>personal</i>	(0.931)	(1.128)	(1.273)	(4.069)	(3.175)	(2.279)
<i>MP legislature + executive</i>	-0.052	-7.450	-8.532	-22.135	6.412	
× <i>monarchy</i>	(0.624)	(4.428)	(5.550)	(3.404)	(5.881)	
<i>MP legislature + executive</i>	-0.811	0.267	0.576	9.400	-0.870	-2.710
× <i>party</i>	(0.840)	(0.915)	(0.944)	(7.348)	(4.094)	(2.839)
<i>MP legislature + executive</i>	0.136	-0.463	-0.815	5.482	1.153	-0.685
× <i>military</i>	(0.885)	(0.813)	(0.832)	(4.671)	(2.556)	(2.494)
<i>MP legislature + executive</i>	1.541	1.159	0.806	-2.260	-8.024	-4.825
× <i>personal</i>	(0.670)	(0.598)	(0.649)	(2.956)	(2.466)	(1.760)
regime FE	no	yes	yes	no	yes	yes
year FE	no	yes	yes	no	yes	yes
controls	no	no	all	no	no	all
observations	4018	4018	3750	2875	2875	2603
regimes	192	192	184	155	155	150
years	65	65	65	56	56	51

OLS regression estimates. Robust standard errors clustered by regime in parentheses. Models 3 and 6 control for $GDP\ per\ capita_{t-1}$ (log) and *oil and gas per capita*_{*t*-1} (log).

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