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Political Institutions Without Property-Rights Deterioration: Evidence from Coup Attempts, 1950–2024

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October 2026



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Abstract

A large literature treats political and economic institutions as jointly determined, implying that a shock that removes constraints on the executive should weaken both. I test this transmission hypothesis using data for 95 countries from 1950 to 2024. Using complementary causal quasi-experimental designs, I find that coups sharply deteriorate political, legal, and administrative institutions and increase corruption. Against these large responses, however, property rights change little. This is not evidence of inertness: the same property-rights index rises by 0.45 standard deviations following a democratic transition. Coups also move property rights within countries, increasing absolute deviations from pre-coup levels without a common direction. The evidence therefore points to asymmetric institutional adjustment: political constraints can collapse sharply while broad measured property-rights protection remains comparatively persistent.

Keywords: coup d'etat, political institutions, property rights, institutional change, corruption

JEL Codes: D72, O43, P48, N40

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1. Introduction

That political and economic institutions move together is one of the most consequential propositions in modern political economy. North and Weingast (1989) argue that constraints on the English Crown created a credible commitment that protected private property. Acemoglu et al. (2001) place political institutions at the center of the determination of property rights and, through them, economic development. In addition, Acemoglu and Robinson (2006) formalize a political mechanism in which economic institutions are central to the distributional conflict over political power. In these accounts, political and economic institutions are closely connected because both ultimately depend on limits to executive discretion. Acemoglu and Johnson (2005) sharpen this distinction by separating property-rights institutions, which protect citizens against expropriation by governments and elites, from contracting institutions governing private agreements, and find the former to be first-order for growth and investment. Glaeser et al. (2004) challenge the interpretation of institutional measures: standard indices, they argue, record outcomes and cannot distinguish a dictator who chooses to respect property from a democrat who is constrained to do so. Both literatures rely heavily on historically determined cross-sectional variation. Neither can hold a country fixed, remove its political constraints, and observe what happens to property rights.

This paper tests these competing views empirically using data from coup attempts around the world. Between 1950 and 2024 there were 493 coup attempts, roughly half of which succeeded. I define a coup as an illegal and overt effort by military or other state elites to remove the sitting executive. Using a staggered differences-in-differences approach as the main method, I compare countries experiencing a successful coup with countries that do not. Because the central question is whether a coup causes a level change in property-rights protection, I make the no-coup counterfactual primary and use the success-versus-failure design as a complementary exercise.¹ Successful coups reduce electoral democracy by 0.57 within-country standard deviations over the first decade after the coup, physical integrity by 0.56, freedom of expression by 0.45, academic freedom by 0.46, liberal democracy by 0.48, and high-court independence by 0.42. The effects extend beyond political rights to law enforcement, the rule of law, access to justice, impartial

¹ The familiar successful-versus-failed comparison (Jones and Olken, 2005), conditions on the occurrence of an attempt, but failed coups can themselves affect institutions. Please, also see the section on methodology, below.

public administration, corruption, and other institutional outcomes. The treatment therefore produces a substantial institutional rupture.

Property-rights protection, by contrast, does not respond in the same way. Every primary specification employed yields an economically small and statistically insignificant effect. The important point is not simply that the coefficient fails to reach significance, but that the estimated effects are small relative to the institutional changes that occur elsewhere. The implication is not that economic institutions are insulated from politics. Contracting and legal institutions deteriorate sharply, as does administrative impartiality, while corruption increases. Nor is it that coup regimes are generally restrained. They repress, subvert legal constraints, weaken the bureaucracy, and extract more through corruption. What they do not do, according to my findings, is to produce a broad deterioration in citizen property-rights protection. A useful benchmark addresses the possibility that coups are simply too small a political shock to reveal an economic-institution response. Democratic transitions are roughly 2.9 times as large as coups on the political dimension, and scaling the property-rights response to transitions by that relative magnitude implies a coup effect of approximately -0.158 standard deviations. This makes a purely treatment-size explanation less plausible, while making no claim that democratization and coups should move institutions proportionally. The evidence instead reveals an asymmetry. Political improvement raises measured property-rights protection, whereas political collapse does not produce a comparable decline. I refer to this pattern as a ratchet.

The findings also speak to the channel through which political institutions may affect income. Acemoglu et al. (2019) establish that democracy causes growth. If that effect operated primarily through property-rights security, a shock that degrades democratic institutions as sharply as a coup should also register in measured property protection. It does not.² Two interpretations are consistent with the evidence. Institutions may be separable objects, or property-rights measures may capture dimensions of institutional quality that respond asymmetrically to changes in political constraints. Either interpretation cautions against inferring the response of one institutional dimension from a political shock by looking only at another. The contribution is therefore narrower than a claim that political and economic institutions are unrelated. The evidence remains consistent

² This suggests that other channels, such as public goods, human capital, or legal and administrative capacity, may account for part of the growth effect. Identifying these channels would require additional research beyond the scope of this paper.

with long-run co-movement generated by common fundamentals, and it does not show that investors, firms, or particular groups of owners are insulated from expropriation. What it challenges is the stronger claim that the two dimensions must move together when political constraints are suddenly removed: institutions may be bundled in levels without being bundled in their responses.

The institutions-and-growth tradition running from North and Weingast (1989) through Acemoglu, Johnson, and Robinson (2001) to Acemoglu and Robinson (2006) treats constraints on the executive as a foundation for secure property rights and, ultimately, investment and growth. Acemoglu and Johnson (2005) distinguish property-rights institutions from contracting institutions and find the former to be first-order. Glaeser et al. (2004), in contrast, argue that institutional indices may primarily record what governments actually do, concluding that human capital and policy can drive growth while political institutions improve afterward. That disagreement yields a sharp within-country prediction that has not been tested directly: if property-rights indices largely capture realized government behavior, removing political constraints should not necessarily produce a corresponding deterioration in the index. The economic consequences of coups were first studied through cross-country growth regressions, in which coup frequency predicted lower investment (Barro 1991) and political instability predicted slower growth (Alesina et al., 1996). More recently, Bennett et al. (2021), using the coup data of Bjørnskov and Rode (2020), find that successful coups increase political corruption and reduce judicial constraints, with the effect driven by military coups.³ Bjørnskov and Pfaff (2021) show that effects on physical integrity vary by coup type; Bjørnskov et al. (2022) trace changes in press freedom around coups; and Derpanopoulos et al. (2016) find that coups in autocracies more often produce new dictatorships than democracies. Two recent contributions are especially close methodologically. Grier et al. (2025) use doubly robust difference-in-differences to show that real GDP per capita falls by 10 to 12 percent in the five years after a successful coup, attributing part of the effect to a weakening of the rule of law. Bjørnskov and Voigt (2026) show that 42 percent of successful coup-makers initiate a new constitution within three years and that post-coup constitutions are unusually difficult to amend. Across this literature, outcomes are political, judicial, constitutional, or macroeconomic. Property rights have not been the outcome variable, and where property rights

³ Their appendix extends the analysis to transparent laws with predictable enforcement and to rigorous and impartial public administration, finding deterioration in both.

and coups appear together, the relationship typically runs in the opposite direction, with Tusalem (2010) treating secure property rights as a deterrent to coup risk.

This analysis extends the literature in two ways. First, it treats property-rights protection as a direct outcome and places it alongside political, legal, administrative, and corruption measures generated by the same shock. The null is therefore evaluated against outcomes that demonstrably respond to coups. Second, it treats a null result as an empirical claim that requires more than an insignificant coefficient. The design must identify a level effect, the same treatment must generate strong responses in related institutional measures, and the result must survive specification choices that could otherwise produce a false null. I reproduce the corruption and judicial-constraint findings of Bennett et al. (2021) using their specification on my data, providing a check on both the data construction and the broader empirical pattern. The contribution is the contrast: the same shock, in the same panel, and under the same broad empirical framework produces large changes in political, legal, administrative, and corruption outcomes but a precisely estimated near-zero effect on property rights.

The remainder of the paper proceeds as follows. Section 2 describes the coup data, the institutional measures, and the empirical designs used to identify the consequences of successful coups. Section 3 presents the main results. It first establishes that successful coups produce large and persistent deteriorations in political constraints, legal and administrative institutions, and corruption, and then shows that these changes are accompanied by little or no deterioration in broad measures of citizen property-rights protection. Section 4 tests the robustness of this asymmetry across alternative specifications, samples, and measures, strengthening the case that the property-rights result is not driven by a particular empirical design or index. Section 5 places the property-rights finding in the context of the broader institutional consequences of coups and benchmarks its magnitude against the effects on political and administrative institutions. Section 6 examines pre-treatment dynamics and measurement concerns, asking whether the absence of a property-rights response can plausibly be attributed to limited variation or other features of the index. Section 7 interprets the evidence as an asymmetric adjustment of political and economic institutions and discusses what the findings imply for the relationship between political constraints and property rights. Section 8 concludes. Appendix A reports a complementary success-versus-failure design that provides an additional test of the main findings.

2. Data and Methods

Coup attempts come from Powell and Thyne (2011) recording 493 attempts in 97 countries between 1950 and 2024 with exact dates and a binary success indicator; 246 succeeded and 247 failed. An attempt is an illegal and overt effort by the military or other state elites to unseat the sitting executive, coded successful if the perpetrators hold power at least seven days. Institutional outcomes come from V-Dem version 16 (Coppedge et al. 2026). I use six political measures, four contracting and legal measures, four measures of bureaucratic quality and state capacity, seven corruption measures and three property rights measures. V-Dem is merged to the coup data on Correlates of War country codes. I drop attempts in states without a continuous series across the event window, which removes the Republic of Vietnam and the two pre-unification Yemens. Requiring a balanced five-year window either side leaves 469 attempts in 95 countries, 231 successful and 238 failed. I also examine the World Bank Worldwide Governance Indicators (WGI) and the Transparency International Corruption Perceptions Index as external checks. Both have much shorter time coverage than V-Dem: the WGI series begin in 1996, leaving 106 attempts and 594 observations, while the CPI leaves only 25 attempts. In the stacked design the shorter samples are too small to provide precise estimates, but in the matched design the four WGI governance measures move significantly in the same direction as the V-Dem institutional outcomes. Table 1 reports summary statistics for the estimation sample.

Table 1

	Mean	SD between	SD within	Min	Max
<i>Panel A. Property rights</i>					
Property rights index	0.496	0.250	0.159	0.00	0.97
Property rights, men	0.385	1.076	0.796	-4.38	2.69
Property rights, women	0.280	1.137	0.768	-3.82	3.29
<i>Panel B. Political institutions</i>					
Electoral democracy	0.252	0.162	0.161	0.01	0.92
Liberal democracy	0.155	0.127	0.136	0.01	0.90
Physical integrity	0.372	0.234	0.186	0.01	0.99
Freedom of expression	0.408	0.246	0.212	0.01	0.99
High-court independence	-0.339	1.035	0.730	-3.52	3.49
Academic freedom	0.408	0.226	0.179	0.01	0.98
Coup attempts	469				

	Mean	SD between	SD within	Min	Max
successful	231				
failed	238				
Countries	95				

Notes: Means and between-country standard deviations are computed in the year before the attempt across the 469 attempts in the estimation sample. Within-country standard deviations are computed on the full 1950–2024 V-Dem panel after removing country means. Indices are scaled 0–1 except the disaggregated property rights measures and high-court independence, which are scaled 0–4.

I estimate three designs that differ in their counterfactuals. Because the central claim is a null, the counterfactual is especially important. A null is informative only if the design can credibly detect an effect of economically meaningful size. My primary design is a staggered difference-in-differences estimator in which treatment is a country’s first successful coup and the comparison group consists of countries that have not yet experienced a successful coup. Let G_i denote the first year in which country i experiences a successful coup, with $G_i = \infty$ for countries that never do, and let Y_{it} denote the institutional outcome. The building block is the group-time average treatment effect for cohort g in year t ,

$$ATT(g, t) = E[Y_{it} - Y_{i, G_i-1} \mid G_i = g] - E[Y_{it} - Y_{i, G_i-1} \mid G_i > \max(t, g)] \quad (1)$$

The change in the outcome between the year before treatment and year t among countries first treated in year g , minus the same change among countries not yet treated by either date. The second expectation defines the not-yet-treated comparison group; substituting $G_i = \infty$ gives the never-treated alternative reported in the robustness battery. The base period is universal, so every $ATT(g, t)$ is normalized to $g - 1$ rather than to the period immediately preceding t .

The group-time effects are aggregated into event time. Writing $e = t - g$ for the number of years since treatment,

$$\theta(e) = \sum^G w^G \cdot ATT(g, g + e), \quad \text{with } w^G = P(G = g \mid G + e \leq T) \quad (2)$$

so that each cohort is weighted by its share among cohorts observed at horizon e . The estimate reported in Table 2 is the average of these effects over the first decade after treatment,

$$\theta = (1/11) \cdot \sum_{e=0}^{10} \theta(e) \quad (3)$$

The effects $\theta(e)$ for $e < -1$ are pre-treatment placebos and provide the test of parallel trends reported in Table 3 and column (5) of Table 4. Inference uses the multiplier bootstrap with 999

replications, clustered at the country level. This design identifies a level effect: the post-coup institutional path of a country that experiences a successful coup is compared with the path of otherwise comparable countries that have not yet experienced one. The identifying assumption is parallel trends between treated and not-yet-treated countries, which I assess directly using pre-treatment group-time effects. The main cost is sample size: focusing on the first successful coup leaves 77 treated countries and discards information from subsequent and failed attempts.

The second primary design recovers the attempt-level sample. For each of the 469 attempts, I select up to three countries in the same V-Dem political region whose electoral-democracy score in the year before the attempt is closest to that of the treated country, requiring that the control experience no coup attempt within the event window. I then estimate Treated $\times$ Post with event-by-country and event-by-relative-year fixed effects. This produces 469 event blocks and 131 control countries; 99 percent of attempts are matched to three controls and the remainder to two. Like the staggered design, this is a no-coup counterfactual and therefore identifies a level effect. Because matching is based on political conditions rather than property rights, the property-rights estimate is not mechanically constrained toward zero. The two primary designs address the same question with different counterfactuals and samples, so their agreement provides an important cross-check. Indexing event blocks by $b = 1, \dots, 469$, countries by i , and relative years by $k = t - t^b \in \{-5, \dots, 5\}$, and letting $D_i = 1$ for the country that experienced the attempt and $D_i = 0$ for its matched controls, the pooled specification is

$$Y_{i^b k} = \beta \cdot (D_i \times I[k \geq 0]) + \alpha_i^b + \gamma_k^b + \varepsilon_{i^b k}, \quad (4)$$

where α_i^b is a block-by-country fixed effect and γ_k^b a block-by-relative-year fixed effect. The block-by-country effects absorb any time-invariant difference between a treated country and its controls within a block, and the block-by-relative-year effects absorb whatever moves a block in common at a given distance from the attempt. The coefficient β is therefore identified from the movement of the treated country relative to its own matched controls around its own attempt date, and is the estimate reported in columns (3) and (4) of Table 2. The event-study version replaces the single post indicator with a full set of relative-year interactions,

$$Y_{i^b k} = \sum_{k \neq -1} \beta_k \cdot (D_i \times I[k]) + \alpha_i^b + \gamma_k^b + \varepsilon_{i^b k}, \quad (5)$$

with $k = -1$ omitted as the base period; these β_k appear in columns (3) and (4) of Table 3. The same construction applied to $|Y_{i^b k} - Y_{i^b k-1}|$ yields the absolute-deviation estimates discussed in

Section 7, which are positive when a coup moves an institution without a consistent direction. Standard errors are clustered by country throughout.

The third design is the one most familiar from this literature (Jones and Olken, 2005), and we report it in the Appendix while cautioning against reading a null from it.⁴ Its attraction is that it conditions on an attempt having occurred, so selection into coup risk is absorbed and the comparison is between countries that were all close to losing their executive by force. Its defect is that the control group is treated. Failed coups are consequential in their own right; Easton and Siverson (2018) document purges of the officer corps by surviving incumbents, and Thyne and Powell (2016) find that some failed attempts are followed by liberalization. Whatever a coup attempt does to an institution through channels common to success and failure is differenced away. The design therefore estimates the incremental effect of success, not the effect of a coup, and it is biased toward zero for any outcome that responds to attempts as such.⁵

3. Main Results

Table 2 presents the primary estimates. Columns (1) and (2) report the staggered design, while columns (3) and (4) report the matched attempt-level design. Keeping the counterfactual visible is important because both primary designs compare countries that experienced a successful coup with countries that did not experience a coup at all.

⁴ For each attempt we assemble eleven years centered on the attempt, indexed by $k = t - t_0$, and estimate $Y_{ek} = \sum_k \beta_k \cdot \text{Success}_e \cdot 1[\text{rel} = k] + \alpha_e + \gamma_k + \varepsilon_{ek}$, with α_e an attempt fixed effect, γ_k a relative-year fixed effect and $k = -1$ omitted; in pooled form the interaction set is replaced by a single $\text{Success} \times \text{Post}$ term.

⁵ The concern is not hypothetical, and corruption illustrates it. Corruption rises after successful coups: the political corruption index increases by 0.190 standard deviations against matched never-attempt controls and by 0.266 in the staggered design. Our stacked specification, which averages the effect over the six years beginning with the coup, returns 0.0067 with a standard error of 0.0095. It does not show that the success-versus-failure contrast is incapable of detecting the effect (Bennett, et al., 2021). What it shows is that our particular stacked implementation, averaging over a six-year window with attempt fixed effects and no lagged dependent variable, is not well suited to an outcome that moves immediately and then persists. Since the property rights null is the paper's central claim, we rest it on designs that recover level effects and report the stacked estimates in Appendix A, where they answer a different question: conditional on an attempt, does its success matter?

Table 2

Dependent variable	(1) Staggered CS (2021)	(2) SD units	(3) Matched never-attempt	(4) SD units
<i>Panel A. Property rights</i>				
Property rights index	0.0038 (0.0131)	+0.024	0.0022 (0.0077)	+0.014
Property rights, men	-0.0093 (0.0771)	-0.012	-0.0011 (0.0379)	-0.001
Property rights, women	-0.0120 (0.0687)	-0.016	0.0062 (0.0344)	+0.008
<i>Panel B. Political institutions</i>				
Electoral democracy	-0.0914*** (0.0182)	-0.570	-0.0769*** (0.0105)	-0.479
Liberal democracy	-0.0646*** (0.0150)	-0.476	-0.0537*** (0.0085)	-0.396
Physical integrity	-0.1030*** (0.0272)	-0.553	-0.0764*** (0.0149)	-0.410
Freedom of expression	-0.0959*** (0.0278)	-0.452	-0.0679*** (0.0144)	-0.320
High-court independence	-0.3009*** (0.1075)	-0.412	-0.1729*** (0.0481)	-0.237
Academic freedom	-0.0818*** (0.0236)	-0.456	-0.0512*** (0.0135)	-0.285
Treated units	77 countries		469 attempts	
Countries	180		131	
Observations	12,120		10,131	
Comparison group	not-yet-treated		never-attempt	

Notes: Columns (1) and (2) report Callaway and Sant’Anna (2021) group-time average treatment effects aggregated over event years 0 to 10, with treatment the country’s first successful coup and not-yet-treated units as the comparison group. Columns (3) and (4) report the coefficient on Treated $\times$ Post from the matched design, up to three never-attempt control countries per attempt, with event-by-country and event-by-relative-year fixed effects. Columns (2) and (4) express the adjacent estimate in within-country standard deviations of the 1950–2024 panel. Standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In the staggered design, successful coups reduce electoral democracy by 0.0914 index points over the first decade, equivalent to 0.57 within-country standard deviations, with a bootstrap standard error of 0.0182. Physical integrity falls by 0.1030, freedom of expression by 0.0959, academic freedom by 0.0818, liberal democracy by 0.0646, and high-court independence by 0.3009; in

standardized terms, these effects range from 0.41 to 0.57 standard deviations. The matched design delivers the same qualitative pattern at the attempt level: electoral democracy falls by 0.0769, or 0.48 standard deviations, and every political measure is significant at the one-percent level.

Property rights do not respond in either primary design. The staggered estimate for the aggregate index is 0.0038, with a bootstrap standard error of 0.0131, equivalent to 0.024 standard deviations and a 95 percent interval of $[-0.138, +0.185]$. The matched estimate is 0.0022 (0.0077), or 0.014 standard deviations, with an interval of $[-0.081, +0.108]$. The disaggregated measures tell the same story: the staggered estimates are -0.0093 and -0.0120 , while the matched estimates are -0.0011 and 0.0062 . None is statistically distinguishable from zero. The substantive content of the null lies in its magnitude, not in statistical insignificance alone. Failure to reject zero would provide weak evidence if the confidence interval were wide enough to accommodate effects comparable to those observed for political institutions. It is not. In the matched design, the 95 percent interval rules out a negative property-rights effect larger than about 0.08 within-country standard deviations, whereas the political effects range from 0.24 to 0.48 standard deviations. In the staggered design the interval is wider, but it still rules out an effect comparable to the political deterioration, which ranges from 0.42 to 0.57 standard deviations. I therefore interpret the estimates as evidence against a large transmission from political collapse to broad measured property-rights protection, not as proof of an exactly zero effect.

Table 3 shows the same separation dynamically in both primary designs and provides the clearest single visual test in the paper. Electoral democracy follows its pre-treatment path, breaks sharply in the coup year to -0.0672 in the staggered design and -0.0599 in the matched design and remains below that path throughout the following decade.

Table 3

	(1)	(2)	(3)	(4)
Relative year	Property rights <i>staggered</i>	Elect. democracy <i>staggered</i>	Property rights <i>matched</i>	Elect. democracy <i>matched</i>
-5	-0.0215* (0.0115)	-0.0021 (0.0100)	0.0027 (0.0064)	0.0151** (0.0072)
-4	-0.0185* (0.0103)	0.0011 (0.0066)	0.0002 (0.0058)	0.0130** (0.0064)
-3	-0.0113 (0.0073)	0.0037 (0.0043)	0.0002 (0.0047)	0.0113** (0.0046)
-2	-0.0043 (0.0042)	0.0083*** (0.0023)	0.0009 (0.0030)	0.0081*** (0.0029)
-1	0 (omitted)	0 (omitted)	0 (omitted)	0 (omitted)
0	0.0136* (0.0070)	-0.0670*** (0.0116)	-0.0003 (0.0033)	-0.0599*** (0.0084)
+1	0.0202** (0.0102)	-0.1121*** (0.0152)	0.0051 (0.0048)	-0.0876*** (0.0107)
+2	0.0200* (0.0112)	-0.1038*** (0.0165)	0.0032 (0.0056)	-0.0728*** (0.0111)
+3	0.0166 (0.0115)	-0.0950*** (0.0181)	0.0024 (0.0065)	-0.0632*** (0.0114)
+4	0.0111 (0.0120)	-0.0941*** (0.0188)	0.0048 (0.0078)	-0.0620*** (0.0130)
+5	-0.0010 (0.0143)	-0.0898*** (0.0188)	0.0028 (0.0090)	-0.0591*** (0.0139)
Observations	12,120	12,120	10,131	10,131
Comparison group	not-yet-treated	not-yet-treated	never-attempt	never-attempt

Notes: Columns (1) and (2) report Callaway and Sant'Anna (2021) group-time average treatment effects by event year, in native V-Dem units, from the staggered design; treatment is the country's first successful coup, and the comparison group is not-yet-treated units, with a universal base period and 999 multiplier-bootstrap replications. Columns (3) and (4) report event-study coefficients on $Treated \times 1[rel = k]$ from the matched design, with event-by-country and event-by-relative-year fixed effects and standard errors clustered by country. Relative year -1 is the omitted base period. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Property rights show no comparable break: post-treatment event coefficients on the aggregate index never exceed 0.0203 index points in the staggered design or 0.0051 in the matched design, against a within-country standard deviation of 0.159. The pre-treatment coefficients also provide the direct test of the identifying assumption. In the staggered design, the electoral-democracy

effects at $e = -6$ through -2 are 0.000, -0.012 , $+0.007$, $+0.023$, and $+0.052$ within-country standard deviations, averaging -0.002 , and none is individually large. In the matched design, the four pre-coup coefficients are positive and statistically significant, but the drift runs in the opposite direction from the post-coup decline.

Given that the matched pre-trend in electoral democracy is positive while the estimated treatment effect is negative, that drift biases the political estimate toward zero; if anything, the underlying political effect may be larger than reported. The corresponding property-rights coefficients over the same window are $+0.0027$, $+0.0002$, $+0.0002$, and $+0.0009$, none remotely significant. The pre-trend therefore appears specific to the political series rather than to property rights. After the coup, electoral democracy breaks sharply and remains depressed, while property rights remain essentially flat in both designs.

The staggered design nonetheless warrants one qualification. From $e = -6$ to -2 , property rights drift downward before a country's first successful coup: the average pre-treatment effect is -0.094 within-country standard deviations for the aggregate index and -0.139 for the male sub-index. Only the male sub-index approaches conventional significance, but the pattern means that the staggered property-rights null is not identified against a perfectly flat pre-period. The matched design does not exhibit this drift, and the stacked design in Appendix A has comparatively flat property-rights pre-trends. The convergence of the two primary no-coup estimates is therefore important, as the near-zero post-coup response does not depend on the staggered design alone.

4. Robustness

Given that the central conclusion is a null, the robustness analysis focuses on magnitude. I ask whether the property-rights estimate remains small when the comparison group, normalization, anticipation window, covariate set, sample, treatment definition, and trend specification are changed, and whether the political estimate remains large under the same perturbations. An underpowered specification could produce insignificance mechanically; the relevant question is whether it continues to produce a small property-rights coefficient alongside a large political effect. Table 4 reports sixteen alternatives to the baseline.

Table 4

<i>Specification</i>	(1) <i>Property ATT</i>	(2) <i>Standard Error</i>	(3) <i>Democracy ATT</i>	(4) <i>Standard Error</i>	(5) <i>Pre-period max t </i>
Baseline (not-yet-treated)	+0.024	(0.082)	-0.570***	(0.113)	1.87
<i>Panel A. Estimation choices</i>					
Never-treated comparison	+0.046	(0.081)	-0.560***	(0.113)	2.01
Varying base period	+0.024	(0.082)	-0.570***	(0.113)	1.73
Anticipation, 1 year	+0.061	(0.096)	-0.643***	(0.122)	1.62
Anticipation, 2 years	+0.092	(0.100)	-0.599***	(0.123)	1.36
Conditional on log GDP p.c.	+0.114	(0.091)	-0.464***	(0.119)	2.57
<i>Panel B. Sample</i>					
Drop 1950s cohorts	+0.038	(0.089)	-0.553***	(0.135)	1.89
Drop largest cohort (1966)	+0.023	(0.089)	-0.568***	(0.122)	1.82
Drop E. Europe / C. Asia	+0.032	(0.083)	-0.581***	(0.115)	1.87
Drop Latin America	+0.023	(0.096)	-0.475***	(0.109)	2.13
Drop N. Africa / Middle East	-0.006	(0.087)	-0.614***	(0.131)	1.49
Drop sub-Saharan Africa	+0.028	(0.110)	-0.544***	(0.176)	1.00
Drop W. Europe / N. America	-0.022	(0.082)	-0.652***	(0.101)	1.61
Drop Asia / Pacific	+0.090	(0.091)	-0.530***	(0.129)	1.98
<i>Panel C. Alternative designs</i>					
Treatment = first attempt	-0.003	(0.078)	-0.452***	(0.092)	2.37
Two-way fixed effects	+0.131	(0.144)	-0.284**	(0.142)	—
Randomized treatment years	+0.027	[0.78]	-0.000	[0.00]	—

Notes: Callaway and Sant'Anna (2021) event-time aggregates over years 0 to 10, in within-country standard deviations, standard errors in parentheses. Column (5) reports the largest absolute t-statistic among the pre-period estimates for the property rights specification. Panel C row three reports the mean estimate across 200 draws in which treatment years are randomly reassigned among treated countries, with the share of draws whose absolute estimate exceeds the observed one in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A varies the estimation choices. Replacing not-yet-treated units with never-treated units, using varying rather than universal base periods, allowing one or two years of anticipation, and conditioning on log GDP per capita leave the property-rights estimate between +0.024 and +0.114 standard deviations and the democracy estimate between -0.464 and -0.643. The largest property-rights estimate, +0.114, comes from the covariate-adjusted specification; it remains insignificant ($t = 1.25$) and is only about one quarter the size of the corresponding democracy estimate of -0.464. The pre-treatment evidence warrants some caution. Under the universal base period, the largest pre-period t-statistic for electoral democracy is 3.57, whereas under varying base periods it falls

to 1.37 with essentially unchanged post-treatment estimates. The pre-trend evidence is therefore supportive but not conclusive. Property-rights pre-trends are more stable in the matched and stacked designs, and the agreement of the two primary no-coup designs provides an important cross-check.

Panel B varies the sample. Dropping the 1950s cohorts, dropping the largest single cohort, and dropping each of the six V-Dem political regions in turn leaves the democracy estimate between -0.475 and -0.652 , significant at the one-percent level in every case. The property-rights estimate ranges from -0.022 to $+0.090$ and is insignificant throughout. No region drives either result, so the property-rights null is not attributable to a particular part of the world, nor is the political effect.

Panel C changes the treatment definition and estimator. Defining treatment as a country's first coup attempt of any kind yields -0.003 for property rights and -0.452 for democracy, indicating that the contrast does not depend on conditioning treatment on success. A conventional two-way fixed-effects specification, which the staggered-adoption literature cautions against when treatment effects are heterogeneous, yields $+0.131$ for property rights and -0.284 for democracy. Even under this estimator, which I do not regard as the preferred specification, the qualitative contrast survives.

The placebo provides a useful design diagnostic. Randomly reassigning treatment years among treated countries and re-estimating the model 200 times produces placebo distributions centered near zero for both outcomes. The observed democracy effect lies far in the tail of its placebo distribution, whereas the property-rights estimate lies close to the center. The pattern is consistent with a genuine treatment effect on political institutions and with the property-rights estimate being difficult to distinguish from noise under randomly timed treatment.

A further check applies the specification used by the closest prior study, Bennett et al. (2021). They estimate a country-year dynamic panel with a twice-lagged dependent variable, one-year coup dummies for success and failure, country and year fixed effects, country-specific linear trends, and country-clustered standard errors. Applying that specification to my data and using Powell and Thyne (2011) coup dates and V-Dem version 16 rather than their Bjørnskov and Rode (2020) dates and version 7.1, reproduces their findings closely. Successful coups raise political corruption by 0.0202 index points, compared with their reported 0.013, and reduce high-court independence by 0.1564, compared with 0.114; both are significant at one percent. Failed coups

are insignificant for both outcomes, as in their paper. Their appendix results also replicated: transparent laws fall by 0.2967 and impartial public administration by 0.2414, both significant at one percent.⁶ Under the same specification, the property-rights index is 0.0064 (0.0045), the male sub-index is 0.0164 (0.0219), and the female sub-index is 0.0411 (0.0226), with only the female estimate significant at ten percent and positive. The property-rights null therefore survives the specification used in the study whose political and corruption findings I reproduce, providing strong evidence that the result is not an artefact of my preferred specification.

Two additional checks address trends. Removing a linear trend fitted separately for each country using only pre-treatment observations strengthens the political result, moving electoral democracy to -0.696 standard deviations, while the property-rights estimate remains insignificant at -0.072 , with an interval of $[-0.277, +0.134]$. Fitting the trend out of sample is important: a trend fitted over the full series could absorb a persistent level shift caused by the coup. The detrended evidence is nevertheless less decisive for the property-rights null than the matched design. The interval for the male sub-index, $[-0.393, +0.116]$, is wide enough to admit an effect comparable to the smaller political effects. This is the one respect in which the staggered evidence is weaker than the matched evidence, and it is why I do not rest the sharp version of the null on the staggered design alone. Finally, the political effect attenuates over longer horizons. Aggregating over the first decade gives -0.570 within-country standard deviations for electoral democracy; aggregating over all post-treatment years, which for the earliest cohorts extends for seven decades, gives -0.294 , while the never-treated comparison gives -0.286 over the same horizon. Coups therefore have large effects that persist but gradually attenuate over time.

5. What Coups Do Change

A null becomes informative when the same treatment demonstrably changes related outcomes. Table 5 therefore places property rights alongside political institutions, contracting and legal institutions, bureaucratic quality, corruption, and World Bank governance indicators, all estimated using the matched design. The exercise is not intended to catalogue every V-Dem variable. Instead,

⁶ I cannot reproduce their decomposition by coup type because Powell and Thyne (2011) record the date and outcome of an attempt but not whether its leaders were military or civilian; their finding that the corruption effect is driven by military coups therefore remains untested here.

it establishes the institutional benchmark against which the property-rights coefficient should be interpreted.

Table 5

Dependent variable	(1) Matched	(2) s.e.	(3) SD units	(4) Stacked
<i>Panel A. Political institutions</i>				
Electoral democracy	-0.0769***	(0.0105)	-0.479	-0.488
Liberal democracy	-0.0537***	(0.0085)	-0.396	-0.438
Physical integrity	-0.0764***	(0.0149)	-0.410	-0.424
Freedom of expression	-0.0679***	(0.0144)	-0.320	-0.429
High-court independence	-0.1729***	(0.0481)	-0.237	-0.347
Academic freedom	-0.0512***	(0.0135)	-0.285	-0.395
<i>Panel B. Contracting and legal institutions</i>				
Transparent laws	-0.3101***	(0.0628)	-0.412	-0.435
Rule of law	-0.0484***	(0.0102)	-0.386	-0.370
Equality before the law	-0.0570***	(0.0116)	-0.324	-0.308
Access to justice, men	-0.1947***	(0.0519)	-0.251	-0.215
<i>Panel C. Bureaucracy and state capacity</i>				
Impartial public administration	-0.2912***	(0.0566)	-0.384	-0.285
Neo patrimonial rule	+0.0489***	(0.0092)	+0.385	+0.414
State authority over territory	-0.6239	(0.4048)	-0.108	+0.005
Clientelism	+0.0116	(0.0105)	+0.097	+0.013
<i>Panel D. Corruption</i>				
Political corruption index	+0.0247***	(0.0091)	+0.190	+0.051
Public sector corruption	+0.0084	(0.0113)	+0.060	+0.034
Executive corruption	+0.0269**	(0.0118)	+0.181	+0.118
Executive bribery	+0.1287**	(0.0526)	+0.180	+0.089
Executive embezzlement	+0.1038*	(0.0571)	+0.150	+0.096
Public sector bribery	+0.0380	(0.0510)	+0.058	-0.010
Public sector theft	+0.0305	(0.0538)	+0.047	+0.012
<i>Panel E. World Bank indicators, 1996–</i>				
Govt effectiveness (WGI)	-0.1311***	(0.0463)	-0.528	-0.059
Regulatory quality (WGI)	-0.1321***	(0.0427)	-0.560	-0.410
Rule of law (WGI)	-0.1882***	(0.0451)	-0.929	-0.339
Control of corruption (WGI)	-0.0952**	(0.0483)	-0.427	-0.001
<i>Panel F. Property rights</i>				
Property rights index	+0.0022	(0.0077)	+0.014	+0.011

Property rights, men	-0.0011	(0.0379)	-0.001	+0.014
Property rights, women	+0.0062	(0.0344)	+0.008	-0.007

Notes: Column (1) reports the coefficient on Treated $\times$ Post from the matched design of Section 2, successful attempts against up to three never-attempt control countries each, with event-by-country and event-by-relative-year fixed effects; standard errors clustered by country in column (2). Column (3) expresses the estimate in within-country standard deviations. Column (4) reports the corresponding success-versus-failure estimate in standard deviations, from the stacked design described in Section 2; Appendix Table A1 gives the full stacked specification grid for the political and property-rights measures, and the remaining stacked estimates are in the replication files. Sign conventions differ between panels and are as follows. In Panels A to C and F a negative coefficient denotes deterioration. In Panel D the V-Dem corruption indices are coded higher-is-more-corrupt, so a positive coefficient denotes deterioration; the four component measures, natively coded higher-is-less-corrupt, are negated to match. In Panel E the World Bank indicators are coded higher-is-better-governance, so a negative coefficient denotes deterioration. Panel E begins in 1996 and rests on a much smaller sample. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Four groups of outcomes move sharply. Contracting and legal institutions deteriorate: transparent and predictably enforced laws fall by 0.412 standard deviations, the rule of law by 0.386, equality before the law by 0.324, and access to justice by 0.251, all significant at one percent. This is especially informative because Acemoglu and Johnson (2005) distinguish contracting institutions from property-rights institutions. Two of these outcomes are already documented in the literature: Bennett et al. (2021) report reductions in transparent laws and impartial public administration, and my estimates agree with theirs in sign and significance. The contribution here is to place those outcomes alongside property rights in the same panel and under the same designs. The coup therefore provides a within-country unbundling result: one institutional category deteriorates sharply while the broad property-rights measure does not.

Bureaucratic quality also deteriorates. Impartial and rigorous public administration declines by 0.384 standard deviations, while neo-patrimonial rule rises by 0.385. State authority over territory and clientelism do not move. Panel E provides an external check over the shorter post-1996 period: the World Bank governance indicators show declines of 0.528 standard deviations in government effectiveness, 0.560 in regulatory quality, 0.929 in the rule of law, and 0.427 in control of corruption. Since these estimates rely on a much smaller sample, I treat them as corroborating evidence rather than as primary results. Corruption also rises. In the matched design, successful coups increase the political corruption index by 0.190 standard deviations, executive corruption by 0.181, executive bribery by 0.180, and executive embezzlement by 0.150. The remaining three corruption measures are positive but statistically insignificant, as reported in Table 5.

The three remaining component measures—public-sector corruption, public-sector bribery, and public-sector theft—are positive but insignificant, at 0.060, 0.058, and 0.047, respectively. The staggered estimates also point in the same direction, although fewer reach conventional significance. These results are consistent with Bennett et al. (2021), whose specification I reproduce directly in Section 4. Failed attempts show uniformly positive but mostly insignificant corruption estimates, roughly half the magnitude of the successful-coup estimates. This helps explain the small success-failure differential: the design used to identify the property-rights null is not well suited to detecting the corruption contrast, so I do not draw conclusions from that differential.

Nineteen measures in Table 5 respond significantly to a successful coup in the matched design at the five-percent level, and a twentieth, executive embezzlement, is significant at ten percent. Their magnitudes range from 0.15 to 0.93 standard deviations, whereas the three property-rights measures are +0.014, −0.001, and +0.008. Property rights are not unique in failing to move: state authority over territory (−0.108) and clientelism (+0.097) are also statistically unchanged. What distinguishes property rights is their stability in the institutional category most often invoked as the economic counterpart to political constraints. The relevant comparison is therefore not that property rights are the only institutional dimension unaffected by coups, but that they remain broadly stable while political constraints, legal predictability, administrative impartiality, and several forms of corruption deteriorate.

6. Are Property Rights Simply More Difficult to Measure?

A null result raises two distinct measurement concerns. The first is whether V-Dem coders update the property-rights index only when formal statutes change, in which case the measure might fail to capture a deterioration in de facto protection. The second is conceptual: the index may capture realized protection, in which case it could remain unchanged after constraints are removed even if it is measured perfectly. The first concern would make the estimate uninformative. The second would make it informative about a different object than the one intended.

Three pieces of evidence, reported in Table 6, speak to the first concern. First, the variation available for identification is comparable across indices: the property-rights index has a within-country standard deviation of 0.159 versus 0.161 for electoral democracy, and the standard deviation of annual first differences is 0.035 versus 0.041. The index is therefore not constant.

Second, the frequency and size of changes differ across measures—electoral democracy changes in 64 percent of country-years, usually in small increments, while property rights change in 13.4 percent, usually in larger steps—but step-like coding does not prevent a response. High-court independence changes in 14.7 percent of country-years, yet falls by 0.237 standard deviations with a t-statistic of 3.60. Third, and most directly, the property-rights index responds to a different political shock: it rises by 0.452 standard deviations following a democratic transition, as shown in Section 7.

Table 6

Indicator	SD within	SD(Δ)	% years changed	Coefficient
Liberal democracy	0.136	0.033	65.6	−0.0537***
Electoral democracy	0.161	0.041	64.2	−0.0769***
Freedom of expression	0.212	0.052	46.7	−0.0679***
Academic freedom	0.179	0.045	33.8	−0.0512***
Physical integrity	0.186	0.054	28.0	−0.0764***
Freedom from torture	0.939	0.285	19.3	−0.3570***
High-court independence	0.730	0.258	14.7	−0.1729***
Property rights index	0.159	0.035	13.4	0.0022
Property rights, women	0.768	0.180	10.6	0.0062
Property rights, men	0.796	0.186	9.4	−0.0011

Notes: SD within removes country means, 1950–2024. SD(Δ) is the standard deviation of annual first differences. Per cent years changed is the share of country-years in which the index differs from the previous year. The final column reports the matched-design coefficient in native units, the same estimate as column (1) of Table 5. Rows are ordered by frequency of annual change; the comparison between high-court independence and the property rights index, adjacent in granularity and opposite in response, is the answer to the measurement objection. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A related concern is attenuation. If the property-rights index contains more measurement error than the political indices, its coefficient would be biased toward zero more severely. V-Dem publishes a measurement-model standard error for each index, allowing a direct assessment. Comparing within-country signal variance with mean measurement-error variance over 1950–2024 gives a reliability of 0.84 for the property-rights index, implying an attenuation factor of 1.19. The corresponding reliabilities are 0.94 for electoral democracy, 0.90 for physical integrity, 0.69 for transparent laws, 0.68 for impartial public administration, and 0.64 for high-court independence. Property rights are therefore measured more reliably than every legal and administrative measure that responds to coups. Correcting for the estimated attenuation increases the matched property-rights effect only from 0.014 to 0.017 standard deviations. Measurement

error therefore cannot plausibly account for the null; if anything, the greater noise in several political measures makes their significant responses harder to detect.

A third concern is that property rights in coup-prone countries may already be near the floor of the scale, leaving little room to decline. The distribution does not support this as a general explanation: the index ranges from 0.001 to 0.968 in the panel, the pre-coup mean among treated countries is 0.496, the tenth percentile is 0.153, only 5 percent of attempts occur within one tenth of the panel floor, and the average treated country lies 3.1 within-country standard deviations above it. The possibility of a ceiling effect, however, deserves attention. Splitting attempts into terciles by pre-coup property-rights protection yields estimates of +0.079 ($t = 1.03$) in the bottom tercile, +0.034 ($t = 0.37$) in the middle, and -0.091 ($t = -1.98$) in the top tercile. Where property rights begin at high levels, there is some evidence of a decline.

I regard this as a bounded qualification for two reasons. The decline appears in only one of three subgroups and would not survive a correction for multiple testing. More importantly, the same countries and specification show a fall in electoral democracy of 0.686 standard deviations. Thus, even in the subgroup with the greatest scope for property rights to decline, the political response is seven and a half times larger; the corresponding ratios are 4.2 in the bottom tercile and 13.4 in the middle. The appropriate conclusion is not that property rights never move after a coup, but that their average response is an order of magnitude smaller than the response of political institutions, including where the scale leaves substantial room for further deterioration. The conceptual measurement objection is not fully resolved by these tests, and I do not claim that it is. This is the issue raised by Glaeser et al. (2004), to which I return below.

7. Interpreting the Institutional Asymmetry

Why would a regime that has just seized power by force, closed newspapers, subordinated courts, weakened administrative impartiality, and increased corruption leave broad property-rights protection largely unchanged? The candidate explanations make different predictions, and the data allow three of them to be examined. The first is that the index is simply unable to move. Column (3) of Table 7 rejects that explanation. Property rights rise by 0.452 standard deviations after democratic transitions on the aggregate index, by 0.502 for men, and by 0.418 for women, all significant at one percent. The same measure that changes little after a coup responds strongly to

political liberalization. This establishes that the index contains information about institutional change; it does not imply that coups and democratization should produce symmetric responses.

Table 7

Dependent variable	(1) Coup, signed	(2) Coup, deviation 	(3) Democratic transition	(4) Ratio (3)/(1)
<i>Panel A. Property rights</i>				
Property rights index	+0.014 (0.048)	+0.110*** (0.033)	+0.452*** (0.121)	—
Property rights, men	-0.001 (0.048)	+0.130*** (0.033)	+0.502*** (0.155)	—
Property rights, women	+0.008 (0.045)	+0.093*** (0.035)	+0.418*** (0.120)	—
<i>Panel B. Other institutions</i>				
Electoral democracy	-0.479*** (0.065)	+0.373*** (0.050)	+1.374*** (0.161)	2.87
Transparent laws	-0.412*** (0.083)	+0.546*** (0.065)	+1.104*** (0.205)	2.68
Rule of law	-0.386*** (0.081)	+0.539*** (0.058)	+1.230*** (0.259)	3.19
Impartial public admin.	-0.384*** (0.075)	+0.489*** (0.069)	+0.983*** (0.225)	2.56
Events	469	469	39	
Comparison group	never-attempt	never-attempt	matched non-transitions	

Notes: Column (1) repeats the matched-design coefficient on Treated $\times$ Post from column (1) of Table 2, expressed in within-country standard deviations. Column (2) reports the same matched specification with the dependent variable replaced by the absolute deviation of the outcome from its value in the year before the attempt, which is positive if a coup moves an institution without a consistent direction even when the signed effect is zero. Column (3) reports the effect of a country's first democratic transition, coded from the Boix, Miller and Rosato regime classification, estimated against up to three matched non-transitioning control countries per event with event-by-country and event-by-relative-year fixed effects; transitions occurring within five years of a coup attempt are excluded, leaving 39 events, of which 33 are matched to three controls, five to one and one to none. Column (4) is the ratio of the transition effect to the coup effect. Standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A second exercise asks whether treatment intensity alone can explain the null. Democratic transitions move electoral democracy by 1.374 standard deviations, compared with 0.479 for coups, making transitions roughly 2.9 times as large on the political dimension. A proportional scaling therefore implies a coup property-rights effect of -0.158 standard deviations. This

benchmark lies outside the matched-design confidence interval but close to the boundary of the staggered interval. I therefore treat the calculation as supplementary evidence against a pure treatment-size explanation, not as a test that democratization and coups must have symmetric institutional effects.

The evidence instead points to a genuine asymmetry. Political improvement raises measured property-rights protection, whereas political collapse does not lower it by a comparable amount. One possible explanation is institutional persistence. Property-rights regimes are embedded in land registries, titling systems, cadastral records, commercial codes, and settled expectations, so changing them may require more administrative and political capacity than a new regime possesses in the short run. Another possibility is strategic restraint, as rulers may have incentives to preserve broad property protection while selectively targeting particular assets or groups. These mechanisms are plausible, but the reduced-form design cannot distinguish among them. I therefore treat the ratchet as a descriptive pattern that requires explanation, not as an identified mechanism.

The second hypothesis is that the aggregate null masks offsetting movements. Column (2) provides evidence consistent with this possibility. Against never-attempt controls, the signed effect on the property-rights index is +0.014 standard deviations, while the absolute deviation from the pre-coup level rises by 0.110, with a t-statistic of 3.37. The male and female sub-indices show absolute deviations of +0.130 and +0.093, both significant at one percent, despite signed effects of -0.001 and $+0.008$. Coups therefore move property rights within individual countries even though the movements have no common direction. One interpretation is redistribution: a regime could expropriate a deposed elite while extending title to another constituency, producing offsetting changes in a population-level index. Land reforms following several coups in the sample provide examples consistent with this possibility. Panel C of Table 5 is also consistent with the idea: state authority over territory and clientelism, which need not change when property titles are redistributed, are likewise unmoved. The evidence has an important limitation. Absolute deviations increase whenever variance increases, including through measurement noise, and the data cannot distinguish genuine redistribution from coder disagreement during unstable periods. The churn result therefore establishes movement without a common direction, not the mechanism behind that movement.

The third hypothesis concerns the scope of the measure. V-Dem's property-rights items ask whether men and women enjoy the right to acquire, hold, inherit, and sell private property. This is a population-level measure of property protection, not a direct measure of investor-facing expropriation risk. A successful coup could selectively expropriate politically connected owners, nationalize particular firms, or alter treatment of foreign investors without producing a large movement in the broad V-Dem index. The result is therefore about broad citizen property-rights protection, not about the absence of selective or sovereign expropriation.

Subgroup estimates reinforce this interpretation. In the matched design, the property-rights effect is -0.038 for attempts in the 1950s and 1960s, $+0.144$ for the 1970s and 1980s, and -0.130 from 1990 onward. By region, the estimates are $+0.197$ in Latin America, -0.008 in North Africa and the Middle East, -0.083 in sub-Saharan Africa, and -0.119 in Asia and the Pacific. Electoral democracy falls significantly in every subsample, with t -statistics between 2.51 and 8.34, so the absence of a property-rights response cannot be attributed to a general lack of power. The property-rights estimates, by contrast, differ in sign across regions. The Latin American estimate is significantly positive at $t = 3.09$, while the Asian estimate is marginally negative at $t = -1.79$. I do not interpret the Latin American estimate as evidence that coups improve property protection because it is one of nine subgroup tests and would not survive a multiplicity correction.

What these subgroup results do show is that the aggregate null averages over countries and regions that can move in different directions, consistent with the absolute-deviation evidence. Finally, the Glaeser et al. (2004) interpretation remains relevant: property-rights indices may record governments' revealed conduct. The democratization result cuts against the strongest version of this objection, because an index that measured only fixed conduct would not respond so sharply to a change in political constraints. A weaker version remains possible, however. The index may capture conduct that improves when constraints are added but persists when they are removed. On that interpretation, the measured object is real but asymmetric, and the assumption that an economic institutional index should mechanically reflect a political shock fails in one direction.

A final limitation concerns the control group in the success-versus-failure design. Failed coups are not untreated: Easton and Siverson (2018) show that surviving incumbents may purge the officer corps, while Thyne and Powell (2016) find that some failed attempts are followed by liberalization. If failed coups also leave property rights unchanged, the differenced null is uninformative about

levels. This is precisely why the level designs are primary. The matched estimates in column (2) of Table 2 and the staggered estimates in column (1) compare coup countries with countries that experienced no coup at all, and both yield the same near-zero property-rights response.

8. Conclusion

Using coup attempts across 95 countries over three quarters of a century, and identifying their effects in both a staggered difference-in-differences design keyed to each country's first successful coup and a matched design pairing each of 469 attempts with never-attempt controls, I find that successful coups substantially degrade political institutions, contracting and legal institutions, and bureaucratic impartiality, while increasing corruption. The effects are generally between one quarter and one half of a standard deviation. Measured property-rights protection does not respond comparably. The estimate is close to zero across specifications, and its confidence interval excludes effects of the magnitude observed for the institutional outcomes that respond significantly.

The result is an asymmetry. The same property-rights index rises sharply after a democratic transition, showing that it is neither unmeasurable nor immovable. Coups also move property rights within individual countries, but without a common direction. What coups do not produce is the broad deterioration in measured property-rights protection that a strong version of the institutional-bundle hypothesis would predict.

This result also bears on the question of how coups damage economies. Grier et al. (2025) find that real GDP per capita falls by 10 to 12 percent in the five years after a successful coup and attribute part of that effect to a weakening of the rule of law. My estimates identify a substantial institutional deterioration in the legal and administrative machinery that enforces rules: the rule of law falls by 0.386 standard deviations, transparent and predictably enforced laws by 0.412, and impartial public administration by 0.384, while measured property-rights protection does not move comparably. I do not estimate income effects and therefore cannot identify the mechanism behind the growth decline. The evidence does, how

ever, suggest that legal and administrative institutions may be an important part of the transmission from coups to economic performance. Several limitations bound the interpretation. First, the V-Dem measure captures broad citizen property rights rather than investor-facing expropriation risk, so the paper cannot rule out selective or foreign-asset expropriation. Second, the reduced form

does not identify why property-rights protection is comparatively persistent; institutional persistence, strategic restraint, coalition structure, external constraints, and measurement remain possible explanations. Third, the staggered property-rights series exhibits some pre-treatment drift, although the matched design and the success-versus-failure event study provide complementary evidence. The paper therefore makes a deliberately narrow claim: a successful coup can dismantle political constraints without producing a commensurate change in broad measured property-rights protection.

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Appendix. The Success-versus-Failure Design

This appendix reports the stacked design described in Section 2, which compares attempts that succeeded with attempts that failed. I report it because it answers a distinct question and because its pre-trends are informative. For the reasons discussed in Section 2, however, I do not use it as the basis for the property-rights null. The design conditions on the occurrence of a coup attempt and asks whether its success matters, absorbing selection into coup risk through attempt fixed effects.

For political institutions the answer is that success matters a great deal. Electoral democracy falls by 0.0784 index points relative to failed attempts, 0.49 standard deviations, and the estimate is stable across the restriction to isolated attempts, the exclusion of the 1950s and the removal of event-specific trends. Since failed coups are themselves consequential, this is a lower bound on the level effect, and it is reassuring that it is close to what the level designs deliver: the political consequences of a coup are concentrated in the coups that succeed. For property rights the stacked estimates are again zero, at 0.0018 (0.0076) on the aggregate index, or 0.011 within-country standard deviations. Its property-rights pre-trends are flat, which is a point in its favor given the mild downward drift in the staggered design, though that drift is itself significant only for the male sub-index. But I do not treat them as evidence for the null in their own right, for the reason set out in Section 2: on an outcome that responds to attempts irrespective of their success, this design returns zero by construction. Corruption is the demonstration. I therefore read the stacked property-rights result as consistent with the level designs.

Table A1

Dependent variable	(1) All attempts	(2) Isolated	(3) 1960+	(4) Detrended
<i>Panel A. Property rights</i>				
Property rights index	0.0018 (0.0076)	0.0054 (0.0206)	-0.0002 (0.0080)	0.0000 (0.0126)
Property rights, men	0.0115 (0.0454)	0.0395 (0.1127)	0.0030 (0.0488)	-0.0344 (0.0769)
Property rights, women	-0.0054 (0.0393)	0.0221 (0.1128)	-0.0030 (0.0410)	0.0197 (0.0616)
<i>Panel B. Political institutions</i>				
Electoral democracy	-0.0784*** (0.0147)	-0.0700*** (0.0233)	-0.0789*** (0.0137)	-0.0667*** (0.0202)
Liberal democracy	-0.0594*** (0.0123)	-0.0449** (0.0186)	-0.0578*** (0.0115)	-0.0443*** (0.0154)
Physical integrity	-0.0790*** (0.0182)	-0.0401 (0.0310)	-0.0655*** (0.0181)	-0.0660** (0.0282)
Freedom of expression	-0.0909*** (0.0217)	-0.0406 (0.0343)	-0.0846*** (0.0215)	-0.0631* (0.0331)
High-court independence	-0.2530*** (0.0793)	-0.0109 (0.1231)	-0.1884** (0.0829)	-0.1709 (0.1086)
Academic freedom	-0.0708*** (0.0194)	-0.0333 (0.0297)	-0.0619*** (0.0179)	-0.0314 (0.0201)
Coup attempts	469	108	409	440
Countries	95	68	92	92
Observations	5,159	1,188	4,499	9,240
Attempt FE	Yes	Yes	Yes	Yes
Relative-year FE	Yes	Yes	Yes	Yes

Notes: Coefficient on Success $\times$ Post from the stacked design with attempt and relative-year fixed effects, native V-Dem units, standard errors clustered by country in parentheses. Column (2) restricts to attempts with no other attempt in the same country within five years. Column (3) excludes the 1950s. Column (4) removes an event-specific linear trend fitted on relative years -10 to -1 . *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The stacked design does contribute one piece of evidence the others cannot. Because it holds fixed the fact of an attempt, it rules out a specific alternative: that the property-rights null reflects something common to countries where coups are attempted and not the coups themselves. Whatever that common factor would be, it is differenced out here, and the null survives.