

Built, Not Unbuilt: The Asymmetric Response of Property Rights to Political Change

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Abstract

Democratization improves the protection of private property. Whether autocratization undoes it has not been tested, although the mechanism usually invoked — that regime type determines economic institutions because democrats and autocrats have different interests — implies that it should. I estimate the transmission from political to economic institutions using a symmetric definition of political change: the first year in which a country's three-year change in electoral democracy exceeds a threshold in either direction, dated at the start of the change window, with coup-linked events excluded. Applying a matched design to 73 improvements and 41 deteriorations across 138 countries between 1950 and 2024, the political shock is close to symmetric, + 0.459 and – 0.409 within-country standard deviations.

Transmission is not. Rule of law, impartial public administration, high-court independence, transparent laws, physical integrity and corruption all respond symmetrically; none rejects the equal-and-opposite restriction. Deteriorating countries begin with more room to fall than improving countries have to rise. It survives eleven alternative specifications and a placebo that randomizes which country in a matched block is treated. Property protection is built when a political opening builds the administrative machinery that enforces rules and is not dismantled when that machinery decays.

JEL Classification Codes: D72, O43, P48, K11

1. Introduction

A large literature holds that political institutions determine economic ones. Clague et al., (1996) established that property and contract rights are better protected in democracies. Krieger (2022) supplies a model in which democratic governments have a stronger interest in secure property because their support base is broader and shows that measured property protection improves after a country completes a transition from autocracy to democracy. The mechanism is a claim about the incentives attached to regime type, and it carries an implication that has not been tested: if democracy improves property rights because democrats want them, then losing democracy should erode them, because the rulers who replace democrats do not. Whether it does is an open question with immediate stakes. The question also bears directly on the institutions-and-growth literature: if property protection is a stock of accumulated institutional capital rather than a contemporaneous reflection of regime type, political openings may build institutions whose effects persist after political conditions deteriorate. The Varieties of Democracy (V-Dem) project counts more countries autocratizing than democratizing in every year since 2019 (Coppedge et al., 2026). If the standard mechanism is right, the world is in the middle of a sustained decline in the security of private property, with consequences for investment and growth that follow directly from the institutions-and-growth literature. If it is wrong, the democratic recession may be doing less damage to property protection than that literature predicts, and the reason would tell us something about the accumulated nature of property-rights institutions.

Testing the implication requires a shock definition that is symmetric by construction, so that the two directions are comparable. I define a political shock as the first year in which a country's three-year

change in the V-Dem electoral democracy index exceeds a threshold in either direction. Each event is dated at the start of its three-year change window. Events within five years of a coup attempt are dropped, so the estimates are not driven by the irregular seizures of power studied elsewhere. Each treated country is matched to up to three countries in the same political region with the closest pre-shock level of electoral democracy and no shock of their own, and the comparison is made within event blocks.

The six comparison institutions are central to the argument. If deterioration events were a different species of political episode rather than the negative counterpart of improvements, there would be no reason for the institutions surrounding property rights to show mirror-image responses. They do, and that is what licenses reading the property-rights result as being about direction and not about two incomparable kinds of event. Rule of law moves + 0.439 and - 0.503, impartial public administration + 0.367 and - 0.502, high-court independence + 0.369 and - 0.434, physical integrity + 0.404 and - 0.248, and corruption - 0.163 and + 0.340. In each case the two effects sum to approximately zero and the hypothesis of symmetric transmission cannot be rejected. Property rights behave differently: +0.286 when politics improves and + 0.081 when it deteriorates, with the sum at + 0.367 and a z-statistic of 3.74. Of the nine institutional outcomes examined, property rights is the only one for which the symmetry restriction is rejected.

The first objection to a null based on 41 events is power, and it should be answered immediately. The question is not whether the deterioration coefficient is significantly negative; with 41 events it need not be. The question is whether it is distinguishable from the negative effect that the improvement estimate implies under symmetry, and it is. Three checks establish that the deterioration null is a finding and not simply absence of evidence. It is not a power problem: the confidence interval on the deterioration estimate excludes the mirror of the improvement effect, so symmetry is rejected, and Section 5 gives the figures. It is not a floor effect: deteriorating countries begin with more room to fall than improving countries have to rise, and the effect attenuates at high baselines in both directions, which points to compression in the index more than to a bound on the deterioration side. And it is not an artefact of block composition: randomly reassigning which country in each matched block is treated produces a placebo distribution centered on + 0.001, and no draw in two hundred reached the observed improvement estimate.

The asymmetry survives eleven alternative specifications spanning four thresholds, three event windows, three change horizons, one to five matched controls, and the retention of coup-linked events, with the symmetry test rejected in everyone. Pre-treatment paths are flat in both directions and for both the political shock and property rights, with no pre-period t-statistic exceeding 1.58. What explains it is harder to establish than the fact itself, and I am careful about the distinction. The natural account is that property protection is embodied in registries, titling systems, cadastral records, commercial codes and the courts that enforce them, that political openings build this machinery, and that the machinery outlasts the political conditions that produced it. Consistent with this, the improvement effect is concentrated where legal and administrative capacity actually improved, and sequential g-estimation

attributes 56 per cent of it to a capacity channel with a capacity coefficient that is strongly significant in both directions. But that decomposition conditions on a post-treatment quantity and is causal only under an assumption about unmeasured confounding of the capacity-property relationship that I cannot defend in a country panel. I report it as suggestive.

The contribution is therefore the asymmetry, not the mechanism. The improvement half of the result reproduces Krieger (2022) with a different design; the deterioration half is new, and it contradicts the mechanism in the paper that established the improvement half. Property protection is therefore not determined by current regime conditions alone: its response to political change depends on the direction from which the change arrives.

2. Related Literature

The claim that political institutions shape economic ones is foundational. North and Weingast (1989) read the English constitutional settlement as the source of credible commitment to private property. Acemoglu et al., (2001) treat colonial political institutions as the mechanism determining property rights and, through them, income. Clague et al., (1996) document that property and contract rights are more secure in democracies and that the gap widens with democratic durability, which anticipates the history-dependence I find but does not test it against a shock.

The closest paper is Krieger (2022), who models the greater interest of democratic governments in good economic institutions and shows, using the same V-Dem private property index employed here, that economic institutional quality improves after a full transition from autocracy to democracy. Rode and Gwartney (2012) show that democratization is followed by economic liberalization, and Méon and Sekkat (2022) that most of the change in economic institutions arrives within a few years of a political transition. Giuliano et al., (2013) find that democratization is followed by economic reform more generally. All of this literature runs in one direction: it studies transitions to democracy.

The literature on movement in the other direction is large and almost entirely about political outcomes. Lührmann and Lindberg (2019) characterize a third wave of autocratization; the Episodes of Regime Transformation project has generated a substantial body of work on which countries backslide, how fast, and by what means. Boese and coauthors examine the survival of democracies under strain. What this work does not ask is what happens to economic institutions while political ones deteriorate. The literature reviewed here does not, to my knowledge, subject the democratization result to a deliberately symmetric reversal test, and that is the gap this paper addresses.

The paper also speaks to work on the persistence of institutions. Acemoglu and Robinson (2008) show that elites who lose de jure political power can compensate with de facto influence, so that changes in formal institutions need not change outcomes. That mechanism predicts attenuation in both directions and not to asymmetry. Closer is the idea that institutional quality is a stock, which appears informally in much of this literature but is rarely given a direction-dependent test.

Finally, a recent literature identifies the consequences of irregular political change. Grier et al., (2025) show that real income per capita falls by 10 to 12 per cent in the five years after a successful coup, attributing the effect partly to a weakening of the rule of law. Bennett et al., (2021) find that successful coups raise corruption and reduce judicial constraints. I exclude coup-linked events from the main sample precisely so that the estimates here speak to ordinary political change.

3. Data and Empirical Strategy

Institutional measures come from the Varieties of Democracy dataset, version 16 (Coppedge et al. 2026), covering all sovereign states annually. The political variable is the electoral democracy index. The economic institution of interest is the private property rights index, together with its disaggregated components for men and for women. For comparison I use rule of law, impartial and rigorous public administration, high-court independence, transparent laws with predictable enforcement, physical integrity of the person, and the political corruption index. Coup dates are from Powell and Thyne (2011).

A political shock is the first year in which a country's three-year change in electoral democracy exceeds $\theta = 0.08$ in absolute value. Since that change accumulates over the three years ending at the measurement date, the shock begins three years earlier, and each event is dated at the start of its window. This matters: dating events at the end of the window places the political movement inside the pre-treatment period and produces pre-treatment t-statistics above seven for the political variable itself. Events within five years of any coup attempt are excluded, as are countries whose window is incomplete.

Two features of this construction deserve emphasis. The event date is the first year of the political movement, not the year in which the movement becomes visible in a three-year difference; the index at t is therefore pre-shock, and $rel = -1$ is the last fully pre-shock year. And the date is identified using information that only becomes available later, so $t = 0$ marks when the political change began, not when an observer could have known it was beginning. This is a statement about how events are located in time, not about what treated, and control countries knew, and it does not bear on the comparison between them. For each event, up to three control countries are drawn from the same V-Dem political region, matched on the level of electoral democracy in the year before the shock, and required to have experienced no shock of their own and no coup attempt within the window. The estimating equation indexes event blocks by b , countries by i and relative years by $k = t - t_{\text{shock}} \in \{-5, \dots, 5\}$, and sets $D_i = 1$ for the country that experienced the shock:

$$Y_{itk} = \beta \cdot (D_i \times 1[k \geq 0]) + \alpha_{\text{block}} + \gamma_{\text{rel}} + \varepsilon_{itk}, \quad (1)$$

where α_{block} is a block-by-country fixed effect and γ_{rel} a block-by-relative-year fixed effect. The block-by-country effects absorb any fixed difference between a treated country and its own controls; the block-by-relative-year effects absorb whatever moves a block in common at a given distance from its event. The design therefore estimates the differential change in treated countries relative to their own matched controls within event blocks, under the assumption that, absent the political shock, a treated country

would have followed the trajectory traced by its controls. Matching does not by itself deliver that assumption; it makes the comparison more plausible by removing fixed differences in region and in the pre-shock level of electoral democracy, and Section 4 reports the pre-treatment paths that bear on it. Standard errors are clustered by country.

To test symmetry, I estimate both directions in a single regression, interacting treatment with an indicator for the direction of the shock:

$$Y_{it} = \beta_u \cdot (D_i \times Post \times Up) + \beta_d \cdot (D_i \times Post \times Down) + \alpha_i + \gamma_t + \varepsilon_{it}. \quad (2)$$

Symmetric transmission means an equal response per unit of political change in either direction. Writing $\theta_d = \beta_d / \beta_u$ for the response per unit in direction d, the null is $\theta_u = \theta_d$. When the two political shocks are equal in magnitude this reduces to $\beta_u + \beta_d = 0$, and I report that restriction because it is transparent, and its standard error follows from the covariance matrix of the two coefficients. The two shocks here are not exactly equal, though the difference between them is not statistically distinguishable, so Section 5 also reports the ratio test in its exact form with standard errors from a block bootstrap. Reporting is in within-country standard deviations of each outcome over the 1950–2024 panel, so that magnitudes are comparable across indices on different scales. The sample contains 73 improvements and 41 deteriorations across 138 countries and 4,851 country-year observations.

4. Main Results

Table 1 reports the two effects and the test of their sum. Panel A confirms that the treatment is symmetric by construction: electoral democracy moves + 0.459 on improvement and – 0.409 on deterioration, and the sum of + 0.050 is indistinguishable from zero. Whatever differences appear in the other panels are therefore about transmission and not about the size of the shock.

Table 1
Improvement and Deterioration

Dependent variable	(1)	(2)	(3)	(4)	(5)
	Improvement	Deterioration	Sum	s.e. of sum	z
<i>Panel A. The political shock</i>					
Electoral democracy	+ 0.459***	−0.409***	+ 0.050	(0.119)	0.42
	(0.061)	(0.101)			
<i>Panel B. Legal and administrative institutions</i>					
Rule of law	+ 0.439***	−0.503***	−0.064	(0.137)	−0.47
	(0.091)	(0.111)			
Impartial public administration	+ 0.367***	−0.502***	−0.135	(0.170)	−0.79
	(0.112)	(0.125)			
High-court independence	+ 0.369***	−0.434***	−0.065	(0.162)	−0.40
	(0.083)	(0.139)			
Transparent laws	+ 0.454***	−0.309**	+ 0.146	(0.146)	1.00
	(0.096)	(0.120)			
Physical integrity	+ 0.404***	−0.248***	+ 0.156	(0.120)	1.30
	(0.077)	(0.095)			
Political corruption	−0.163*	+ 0.340***	+ 0.176	(0.136)	1.30
	(0.095)	(0.110)			
<i>Panel C. Property rights</i>					
Property rights index	+ 0.286***	+ 0.081	+ 0.367	(0.098)	3.74***
	(0.072)	(0.064)			
Notes: Each panel reports the coefficients on treatment interacted with the direction of the political shock, estimated jointly, with standard errors clustered by country in parentheses beneath. Column (3) is the sum of the two effects, which is zero under symmetric transmission because the political shock is symmetric in magnitude; column (4) is its standard error, computed from the full covariance matrix, and column (5) the associated z-statistic. Estimates are in within-country standard deviations of each outcome over the 1950–2024 panel. *** p < 0.01, ** p < 0.05, * p < 0.10.					

Dependent variable	(1)	(2)	(3)	(4)	(5)
	Improvement	Deterioration	Sum	s.e. of sum	z
Property rights, men	+ 0.318*** (0.094)	+ 0.074 (0.068)	+ 0.391	(0.115)	3.40***
Property rights, women	+ 0.224** (0.091)	+ 0.040 (0.061)	+ 0.264	(0.112)	2.35**
Events	73	41			
Countries	138				
Observations	4,851				
Notes: Each panel reports the coefficients on treatment interacted with the direction of the political shock, estimated jointly, with standard errors clustered by country in parentheses beneath. Column (3) is the sum of the two effects, which is zero under symmetric transmission because the political shock is symmetric in magnitude; column (4) is its standard error, computed from the full covariance matrix, and column (5) the associated z-statistic. Estimates are in within-country standard deviations of each outcome over the 1950–2024 panel. *** p < 0.01, ** p < 0.05, * p < 0.10.					

Figure 1 visualizes the same direction-specific estimates with their corresponding 95 percent confidence intervals.

Figure 1. The two direction-specific effects for every outcome, with 95 per cent confidence intervals. Hollow markers are the six comparison institutions, solid markers the three property-rights measures. For five of the six comparison institutions the two intervals sit on opposite sides of zero; the exception is political corruption, whose improvement interval just includes zero while its deterioration interval does not. For all three property-rights measures both point estimates are positive.

Panel B contains the comparison institutions. Rule of law rises 0.439 standard deviations when politics improves and falls 0.503 when it deteriorates. Impartial public administration moves + 0.367 and – 0.502, high-court independence + 0.369 and – 0.434, transparent laws + 0.454 and – 0.309, physical integrity + 0.404 and – 0.248, and corruption improves by 0.163 and worsens by 0.340. All six are significant in both directions. For all six, the sum is small relative to its standard error and the symmetry restriction cannot be rejected, with z-statistics between – 0.79 and + 1.30. Legal, administrative and repressive institutions follow politics in whichever direction politics moves. Panel C contains property rights. The improvement effect is + 0.286 with a t-statistic of 4.0, close to the magnitudes in Panel B and consistent with Krieger (2022). The deterioration effect is + 0.081 with a t-statistic of 1.3 – wrong-signed for a symmetric response and not distinguishable from zero. The sum is + 0.367 with a standard error of 0.098, rejecting symmetry at z = 3.74. The disaggregated measures agree: +0.318 and + 0.074 for men, + 0.224 and + 0.040 for women, with symmetry rejected at 3.40 and 2.35. Of the nine institutional

outcomes in the table — six comparison institutions and the three property-rights measures — property rights is the only category for which the restriction fails.

Table 2 shows the same result dynamically and provides the test of the identifying assumption. The pre-treatment paths are flat in all four columns: the mean pre-treatment effect is + 0.012 for democracy on improvement, + 0.020 on deterioration, – 0.048 for property rights on improvement and – 0.075 on deterioration, and the largest pre-treatment t-statistic anywhere in the table is 1.58. After the shock, democracy separates sharply in both directions and property rights separates only upward. Figure 2 plots the corresponding event-study paths.

Table 2
Event-Study Estimates and Pre-Treatment Tests

Relative year	(1)	(2)	(3)	(4)
	Democracy	Property rights	Democracy	Property rights
	<i>improvement</i>	<i>improvement</i>	<i>deterioration</i>	<i>deterioration</i>
-5	+ 0.025 (0.049)	-0.050 (0.040)	+ 0.025 (0.083)	-0.136 (0.105)
-4	+ 0.005 (0.042)	-0.054 (0.040)	+ 0.006 (0.081)	-0.110 (0.076)
-3	+ 0.002 (0.036)	-0.046 (0.030)	+ 0.037 (0.060)	-0.035 (0.045)
-2	+ 0.015 (0.030)	-0.041 (0.026)	+ 0.012 (0.038)	-0.020 (0.018)
-1	0 (omitted)	0 (omitted)	0 (omitted)	0 (omitted)
-0	-0.020 (0.027)	-0.001 (0.017)	+ 0.127* (0.067)	+ 0.056 (0.041)
+ 1	-0.019 (0.037)	+ 0.014 (0.041)	+ 0.042 (0.063)	+ 0.053 (0.046)
+ 2	+ 0.049 (0.047)	+ 0.079 (0.060)	-0.121 (0.076)	+ 0.041 (0.054)
+ 3	+ 0.741*** (0.066)	+ 0.449*** (0.105)	-0.704*** (0.090)	+ 0.000 (0.070)
+ 4	+ 0.987*** (0.096)	+ 0.503*** (0.121)	-0.856*** (0.116)	-0.028 (0.077)
+ 5	+ 1.072*** (0.115)	+ 0.445*** (0.144)	-0.842*** (0.122)	+ 0.003 (0.091)

Notes: Event-study coefficients on treatment interacted with indicators for each relative year, with relative year - 1 omitted, estimated separately for improvements and deteriorations. Block-by-country and block-by-relative-year fixed effects throughout; standard errors clustered by country in parentheses. Estimates in within-country standard deviations. The final two rows summarize the pre-treatment coefficients, which test the parallel-trends assumption. *** p < 0.01, ** p < 0.05, * p < 0.10.

Relative year	(1)	(2)	(3)	(4)
	Democracy	Property rights	Democracy	Property rights
	<i>improvement</i>	<i>improvement</i>	<i>deterioration</i>	<i>deterioration</i>
Mean, pre-treatment	+ 0.012	-0.048	+ 0.020	-0.075
Largest pre-treatment t	0.51	1.58	0.62	1.45
Notes: Event-study coefficients on treatment interacted with indicators for each relative year, with relative year - 1 omitted, estimated separately for improvements and deteriorations. Block-by-country and block-by-relative-year fixed effects throughout; standard errors clustered by country in parentheses. Estimates in within-country standard deviations. The final two rows summarize the pre-treatment coefficients, which test the parallel-trends assumption. *** p < 0.01, ** p < 0.05, * p < 0.10.				

Figure 2. Event-study estimates with 95 per cent confidence intervals, by outcome and direction of the political shock. Relative year - 1 is the omitted base period. Given that each event is dated at the start of its three-year change window, the political shock accumulates over years 0 to 3; the figure makes that construction visible. Electoral democracy separates in both panels and property rights only in the left one.

5. Is the Asymmetry an Artefact?

A null on one side of a comparison invites three objections: that the design lacked the power to detect the effect, that the scale left no room for it, and that the estimate reflects something about how blocks were assembled. Table 3 addresses each.

Table 3
Power, Scale, and Placebo Tests for Property Rights

<i>Panel A. Room to move</i>	Improvement	Deterioration
Pre-shock property rights, treated	0.404	0.670
Distance to the scale floor (SD)	2.53	4.20
Distance to the scale ceiling (SD)	3.55	1.88
<i>Panel B. Power</i>		
Estimate	+ 0.286	+ 0.081
95 per cent interval		[− 0.046, + 0.207]
Minimum detectable effect		0.181
Mirror of the improvement effect		−0.286 outside
<i>Panel C. Placebo</i>		
Observed estimate	+ 0.286	+ 0.081
Placebo mean	+ 0.001	−0.003
Placebo standard deviation	0.064	0.064
Share of draws at least as large	0.000	0.200
Notes: Panel A reports the pre-shock level of the property rights index among treated countries and its distance from the bounds of the observed scale, in within-country standard deviations; the index runs from 0.001 to 0.968 in this panel. Panel B reports the deterioration estimate with its 95 per cent interval, the effect detectable with 80 per cent power at the 5 per cent level, and the mirror of the improvement effect that symmetric transmission implies. Panel C randomly reassigns which country within each matched block is designated treated, holding the block and the event date fixed, and re-estimates 200 times.		

Table 3 collects the evidence. The interval on the deterioration estimate is [− 0.046, + 0.207] and the minimum detectable effect is 0.181 standard deviations, so the design could have detected a symmetric response and did not. Nor is the result driven by the scale, though this needs more than the simple comparison of distances. The index runs from 0.001 to 0.968 in this panel; deteriorating countries begin at 0.670, which is 4.20 within-country standard deviations above the floor, while improving countries begin at 0.404, which is 3.55 below the ceiling. The group whose property rights do not fall has more room to fall than the group whose rights rise has room to rise, so the simplest floor story predicts the opposite asymmetry. But distance from the observed bounds is not decisive, because the mapping from the index to the underlying construct need not be linear.

Table 4 therefore estimates the effect separately by the pre-shock level of property rights. The deterioration effect does become more negative at higher baselines: +0.212 in the bottom third, + 0.137 in the middle and – 0.103 in the top, with a continuous interaction of – 0.635 and a t-statistic of – 2.21. Read alone, that is what a floor artefact would produce. It is not the right reading, because the improvement side shows a stronger gradient in the same direction: +0.609, + 0.203 and + 0.047 across the terciles, with an interaction of – 0.771 and a t-statistic of – 3.67. Both directions attenuate as the baseline rises, which is the signature of compression near the top of the index, not property rights falling where there is room to fall. What the two gradients jointly imply is that the asymmetry is a feature of the lower and middle parts of the distribution, and that at high baselines the two directions converge because neither moves much.

Table 4
Property-Rights Effects by Pre-Shock Baseline

Pre-shock tercile	(1)	(2)	(3)	(4)
	Baseline level	Effect	t	Events
<i>Panel A. Improvement</i>				
Bottom third	0.085	+ 0.609***	4.81	25
Middle third	0.387	+ 0.203**	2.22	24
Top third	0.753	+ 0.047	0.43	24
Interaction with baseline level		–0.771***	–3.67	
<i>Panel B. Deterioration</i>				
Bottom third	0.418	+ 0.212	1.32	14
Middle third	0.715	+ 0.137	1.64	13
Top third	0.878	–0.103	–1.38	14
Interaction with baseline level		–0.635**	–2.21	
Notes: The matched specification estimated separately within terciles of the treated country's property rights index in the year before the shock, and with a continuous interaction between treatment and the demeaned baseline level. Column (1) reports the mean baseline within each tercile and column (4) the number of treated events. Effects are in within-country standard deviations with standard errors clustered by country. *** p < 0.01, ** p < 0.05, * p < 0.10.				

This is a scope condition as Table 5 states. Dropping the top tercile strengthens the result: the improvement effect rises to 0.372, the deterioration effect to 0.175, and the z-statistic on their sum to 4.12. Dropping the top quartile gives 0.340 and 0.167, with z of 3.89. Within the top tercile alone, improvement gives 0.035 and deterioration – 0.018, and the restriction cannot be rejected, at z of 0.12. The asymmetry operates where property protection is not already high and disappears where the index leaves little room to move in either direction.

Table 5
Property-Rights Asymmetry by Baseline Sample

Sample	(1)	(2)	(3)	(4)	(5)
	Improvement	Deterioration	z sum	Events, +	Events, -
Full sample	+ 0.286***	+ 0.081	3.74***	73	41
Excluding the top tercile	+ 0.372***	+ 0.175	4.12***	55	21
Excluding the top quartile	+ 0.340***	+ 0.167	3.89***	63	22
Top tercile only	+ 0.035	-0.018	0.12	18	20
Notes: The joint specification of Table 1 for the property rights index, estimated on samples defined by the treated country's pre-shock level of that index; the top tercile begins at 0.704. Estimates in within-country standard deviations with standard errors clustered by country. *** p < 0.01, ** p < 0.05, * p < 0.10.					

Finally, the placebo. Holding each matched block and its event date fixed and randomly reassigning which of its countries is treated gives a distribution centered on + 0.001 with a standard deviation of 0.064. The observed improvement estimates of + 0.286 was not reached by any of two hundred draws. The deterioration estimates of + 0.081 sits inside its placebo distribution, with 20 per cent of draws at least as large in absolute value. This does not prove the deterioration effect is zero; a permutation test cannot. What it establishes is that the estimate is not distinguishable from what random reassignment within the same blocks produces, which is the relevant contrast with the improvement estimate.

Table 6 varies the specification. Two features of the event counts are worth explaining. The counts move with the window even though the window does not enter the definition of a shock: an event is retained only if the country is observed across the full span and its controls are free of their own shock over the same span, so a seven-year window drops events near the ends of the sample and a three-year window admits more. And lowering the threshold from 0.08 to 0.05 leaves the improvement count at 73, which looks like a coincidence and is one; the underlying samples differ. Threshold crossings rise from 147 to 161 and the coup-free subset from 112 to 129, but the additional events are dated earlier, so more of them fail the complete-window requirement.

Table 6
Robustness to Alternative Shock and Sample Definitions

Specification	(1)	(2)	(3)	(4)	(5)
	Improvement	Deterioration	z sum	Events, +	Events, -
Baseline	+ 0.286***	+ 0.081	3.74***	73	41
Threshold 0.05	+ 0.138**	+ 0.045	2.05**	73	54
Threshold 0.10	+ 0.208**	-0.020	1.66*	65	34
Threshold 0.15	+ 0.322***	-0.055	2.18**	58	25
Window $\pm$ 7 years	+ 0.338***	+ 0.150*	3.44***	66	35
Window $\pm$ 3 years	+ 0.144***	+ 0.064	2.93***	86	49
One-year change window	+ 0.453***	-0.010	2.83***	69	23
Five-year change window	+ 0.155***	+ 0.127*	3.26***	70	52
One matched control	+ 0.209**	+ 0.086	2.64***	73	41
Five matched controls	+ 0.291***	+ 0.080	3.64***	73	41
Coup-linked events retained	+ 0.309***	+ 0.054	4.31***	99	90

Notes: Each row re-estimates the joint specification of Table 1 for the property rights index under a different definition of the sample or the shock, reporting the two direction-specific effects in within-country standard deviations, the z-statistic on their sum, and the number of events in each direction. The baseline is a three-year change threshold of 0.08 with a five-year window, three matched controls, and coup-linked events excluded. Event counts move with the window because an event enters only if the country is observed over the full 2W + 1 years and its controls are free of their own shock over the same span; widening the window to seven years therefore drops events near the ends of the sample and narrowing it to three admits more. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Of the 73 events at each threshold, 60 countries appear in both, 19 of those at a different event year, and 13 appear at only one threshold. The symmetry test is rejected in every specification, with z between 1.66 and 4.31, and the improvement effect is significant throughout while the deterioration effect never is. A second refinement concerns the symmetry restriction itself. The test in Table 1 imposes $\beta_{\text{U}} + \beta_{\text{D}} = 0$, which is exact only when the two political shocks are equal in magnitude. They are close but not identical, at + 0.459 and - 0.409, and although their difference is not statistically distinguishable — the two sum to + 0.050 with a z-statistic of 0.42 — the restriction is approximate.

Table 7 reports the test in its exact form, estimating the response per unit of political change in each direction and testing their equality, with standard errors from a bootstrap that resamples event blocks.

Table 7
Exact Symmetry Test: Response per Unit of Political Change

Dependent variable	(1)	(2)	(3)	(4)	(5)
	θ , improvement	θ , deterioration	Difference	s.e.	z
Property rights index	+ 0.624	-0.198	+ 0.822	(0.300)	2.74***
Rule of law	+ 0.958	+ 1.232	-0.274	(0.482)	-0.57
Impartial public administration	+ 0.800	+ 1.229	-0.429	(0.574)	-0.75
Notes: θ is the response of the outcome per unit of political change in that direction, computed as the ratio of the outcome effect to the effect on electoral democracy in the same direction and sample. The null of symmetric transmission is θ improvement = θ deterioration. Standard errors from 999 bootstrap replications resampling event blocks with replacement. Estimates in within-country standard deviations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.					

The ratios are signed, and the sign convention matters for reading them. A symmetric response requires the two signed ratios to be equal, because both numerator and denominator change sign under a deterioration; a positive θ in either direction means the outcome moves with politics, and a negative θ means it moves against. Property rights transmit at + 0.624 per unit of political improvement and - 0.198 per unit of deterioration — the negative sign indicating that property rights edged up slightly as democracy fell, not that they fell. The difference is 0.822 with a bootstrap standard error of 0.300 and a z-statistic of 2.74. Rule of law transmits at 0.958 and 1.232, impartial public administration at 0.800 and 1.229; neither difference approaches significance, at - 0.57 and - 0.75. The exact test is less precise than the approximate one, as ratios always are, and the z-statistic on property rights falls from 3.74 to 2.74. The conclusion is unchanged.

6. What Might Explain It

The fact is that property protection responds to political improvement and not to political deterioration, while every neighboring institution responds to both. The explanation is harder to establish, and this section is deliberately more tentative than the last two. The account I find most plausible is that property protection is a stock. Secure property depends on registries, titling systems, cadastral records, commercial codes and courts that can enforce them. These are costly to build and, once built, costly to remove; a government that wants to weaken the constraints on itself has cheaper instruments than dismantling the land registry, and little reason to reach for that one. Political openings supply the administrative investment that builds this machinery. Political closings degrade the quality of administration — Panel B of Table 1 shows impartial public administration falling half a standard deviation — without removing the infrastructure that administration had already produced.

Table 8 offers evidence using sequential g-estimation in the manner of Acharya et al., (2016). A capacity index is formed from impartial public administration, transparent laws and rule of law. The first stage

recovers the association between capacity and property rights within event blocks; the second subtracts the capacity component from the outcome and re-estimates the treatment effect, yielding a controlled direct effect.

Table 8
Sequential G-Estimation: Capacity Channel

Total effect on property rights	Improvement	Deterioration
	+ 0.286	+ 0.081
Controlled direct effect	+ 0.126*	+ 0.177***
Component running through capacity	+ 0.161	−0.096
Share running through capacity	56 per cent	—
Capacity coefficient γ	+ 0.108***	+ 0.062***
Notes: Sequential g-estimation following Acharya et al., (2016). The capacity index is the mean of standardized impartial public administration, transparent laws and rule of law. The controlled direct effect is the treatment effect on the outcome net of the capacity component; the difference from the total effect is the part running through capacity. Estimates in within-country standard deviations, standard errors clustered by country. The decomposition is causal only under the assumption of no unmeasured confounding of the capacity-property relationship, which this design cannot support; see the discussion in the text. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.		

On the improvement side the decomposition is coherent: 56 per cent of the total effect runs through capacity, the controlled direct effect falls to + 0.126, and the capacity coefficient is + 0.108 with a t-statistic of 5.03. On the deterioration side it is not interpretable — the controlled direct effect of + 0.177 exceeds a total effect of + 0.081 — which is what one expects when the total effect being decomposed is itself zero. This does not identify the mechanism, and the reason is worth stating plainly: capacity is measured after the shock, so conditioning on it selects on a post-treatment outcome, and the decomposition is causal only under an assumption of no unmeasured confounding of the capacity-property relationship that a country panel cannot support. Splitting deteriorations by the incoming regime's expected horizon or by accumulated democratic experience gives interactions of + 0.115 and − 0.112, with t-statistics of 0.76 and 0.83; with 41 events split in two, neither test has power to separate a stock account from Olson's stationary bandit, under which a ruler expecting to remain in power wants secure property and there is nothing to erode. Establishing the channel would require an instrument for administrative capacity that this data does not contain.

A third account can be set aside on the evidence. Acemoglu and Robinson (2008) show that elites can offset losses of de jure political power with de facto influence; that mechanism predicts attenuation in both directions, not the one-sided pattern observed here. One further result bears on the timing. Extending the window to fifteen years, the deterioration estimate is − 0.049 over years zero to four, + 0.052 over years five to nine and − 0.016 over years ten to fifteen, never significant and with no trend. I find no evidence of delayed deterioration over the fifteen-year horizon the data allow. With 41

deterioration events the long-horizon estimates are imprecise, so this is weak evidence against slow decay.

7. Discussion

The result has a direct implication for how the institutions-and-growth literature should read the current period. On the standard account, in which regime type determines economic institutions through the interests attached to it, the autocratization wave of the past decade should be producing a broad decline in the security of property. It is not. The measured protection of private property is not responding to political deterioration at all, while the legal and administrative institutions around it are responding fully.

This does not imply that autocratization is economically harmless. The institutions that do deteriorate including rule of law, impartial administration and judicial independence are the ones Grier et al., (2025) identify as the channel through which coups reduce national income. What the result implies is narrower and more specific: the damage does not run through a general collapse in the security of property, and models that treat property protection as the summary statistic for economic institutions will misstate both the mechanism and its timing. It also bears on how property-rights measures should be interpreted in empirical work. If protection is a stock built by past political conditions, then a country's contemporaneous property-rights score carries information about its political history that its contemporaneous political score does not. Cross-sectional regressions that use the two together are not measuring two facets of one underlying constraint; they are measuring a present condition and an accumulated one.

Three limitations bound the claims. The V-Dem property items ask whether men and women can acquire, hold, inherit and sell private property; this is a population-level measure of civil protection rather than the investor-facing expropriation risk that part of the growth literature has in mind. The deterioration sample contains 41 events, which is enough to reject symmetry but not enough to support subgroup analysis, as the failed mechanism tests demonstrate. And the horizon is fifteen years at most; whether property protection eventually erodes under sustained autocracy is a question this design cannot answer.

8. Summary and Conclusions

Using a symmetric definition of political change applied to 73 improvements and 41 deteriorations across 138 countries between 1950 and 2024, and a matched design that compares each treated country to its own regional controls, I find that transmission from political to economic institutions is direction-dependent for property rights and for property rights alone. Rule of law, impartial administration, judicial independence, transparent laws, physical integrity and corruption all move symmetrically. Property protection rises 0.286 standard deviations when politics improves and does not fall when politics deteriorates, with symmetry rejected at $z = 3.74$, and the result surviving eleven specifications, a placebo, and tests for power and for scale effects.

The improvement half of this reproduces what is already known. The deterioration half is new, and it contradicts the mechanism that the improvement half is usually taken to support. If democracy improved property rights because democratic rulers wanted them, autocratic rulers should reverse it, and they do not. The pattern is consistent with the interpretation that political openings build institutional machinery whose effects outlast the conditions that produced it, in other words, property protection is built, and not unbuilt, but the evidence here does not distinguish that reading from the alternatives set out in this research. Establishing that channel causally requires variation in administrative capacity that is independent of regime change, which this data does not contain and which is the natural next research step.

Declarations

Author Contribution

AC did everything.

Data Availability

Data are fully available in the public domain, where I collected them from. My research has received no funding and I have no conflict of interests with any party.

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Footnotes

1. Dating it at the end places the political movement inside the pre-treatment period and manufactures a pre-trend.
2. In Chong (2026) I study the role of coups on property rights.
3. Two designs that would have avoided the assumption failed. Using V-Dem's coded judicial reforms as an independent capacity treatment, isolated from regime change and coups, leaves 28 independence-improving and 12 independence-reducing events and property-rights nulls in both directions. That design also fails its own pre-treatment test: the largest pre-period t-statistic reaches 2.20, against a maximum of 1.58 anywhere in the main specification. I therefore draw no inference from it in either direction and report it only to record that the route was attempted.

Unsectioned Paragraphs

JEL Classification Codes: D72, O43, P48, K11

Notes: Each panel reports the coefficients on treatment interacted with the direction of the political shock, estimated jointly, with standard errors clustered by country in parentheses beneath. Column (3) is the sum of the two effects, which is zero under symmetric transmission because the political shock is

symmetric in magnitude; column (4) is its standard error, computed from the full covariance matrix, and column (5) the associated z-statistic. Estimates are in within-country standard deviations of each outcome over the 1950–2024 panel. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: Event-study coefficients on treatment interacted with indicators for each relative year, with relative year – 1 omitted, estimated separately for improvements and deteriorations. Block-by-country and block-by-relative-year fixed effects throughout; standard errors clustered by country in parentheses. Estimates in within-country standard deviations. The final two rows summarize the pre-treatment coefficients, which test the parallel-trends assumption. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: Panel A reports the pre-shock level of the property rights index among treated countries and its distance from the bounds of the observed scale, in within-country standard deviations; the index runs from 0.001 to 0.968 in this panel. Panel B reports the deterioration estimate with its 95 per cent interval, the effect detectable with 80 per cent power at the 5 per cent level, and the mirror of the improvement effect that symmetric transmission implies. Panel C randomly reassigns which country within each matched block is designated treated, holding the block and the event date fixed, and re-estimates 200 times.

Notes: The matched specification estimated separately within terciles of the treated country's property rights index in the year before the shock, and with a continuous interaction between treatment and the demeaned baseline level. Column (1) reports the mean baseline within each tercile and column (4) the number of treated events. Effects are in within-country standard deviations with standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: The joint specification of Table 1 for the property rights index, estimated on samples defined by the treated country's pre-shock level of that index; the top tercile begins at 0.704. Estimates in within-country standard deviations with standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: Each row re-estimates the joint specification of Table 1 for the property rights index under a different definition of the sample or the shock, reporting the two direction-specific effects in within-country standard deviations, the z-statistic on their sum, and the number of events in each direction. The baseline is a three-year change threshold of 0.08 with a five-year window, three matched controls, and coup-linked events excluded. Event counts move with the window because an event enters only if the country is observed over the full $2W + 1$ years and its controls are free of their own shock over the same span; widening the window to seven years therefore drops events near the ends of the sample and narrowing it to three admits more. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: θ is the response of the outcome per unit of political change in that direction, computed as the ratio of the outcome effect to the effect on electoral democracy in the same direction and sample. The null of symmetric transmission is $\theta_{\text{improvement}} = \theta_{\text{deterioration}}$. Standard errors from 999 bootstrap replications resampling event blocks with replacement. Estimates in within-country standard deviations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Notes: Sequential g-estimation following Acharya et al., (2016). The capacity index is the mean of standardized impartial public administration, transparent laws and rule of law. The controlled direct effect is the treatment effect on the outcome net of the capacity component; the difference from the total effect is the part running through capacity. Estimates in within-country standard deviations, standard errors clustered by country. The decomposition is causal only under the assumption of no unmeasured confounding of the capacity-property relationship, which this design cannot support; see the discussion in the text. *** p < 0.01, ** p < 0.05, * p < 0.10.

Figures

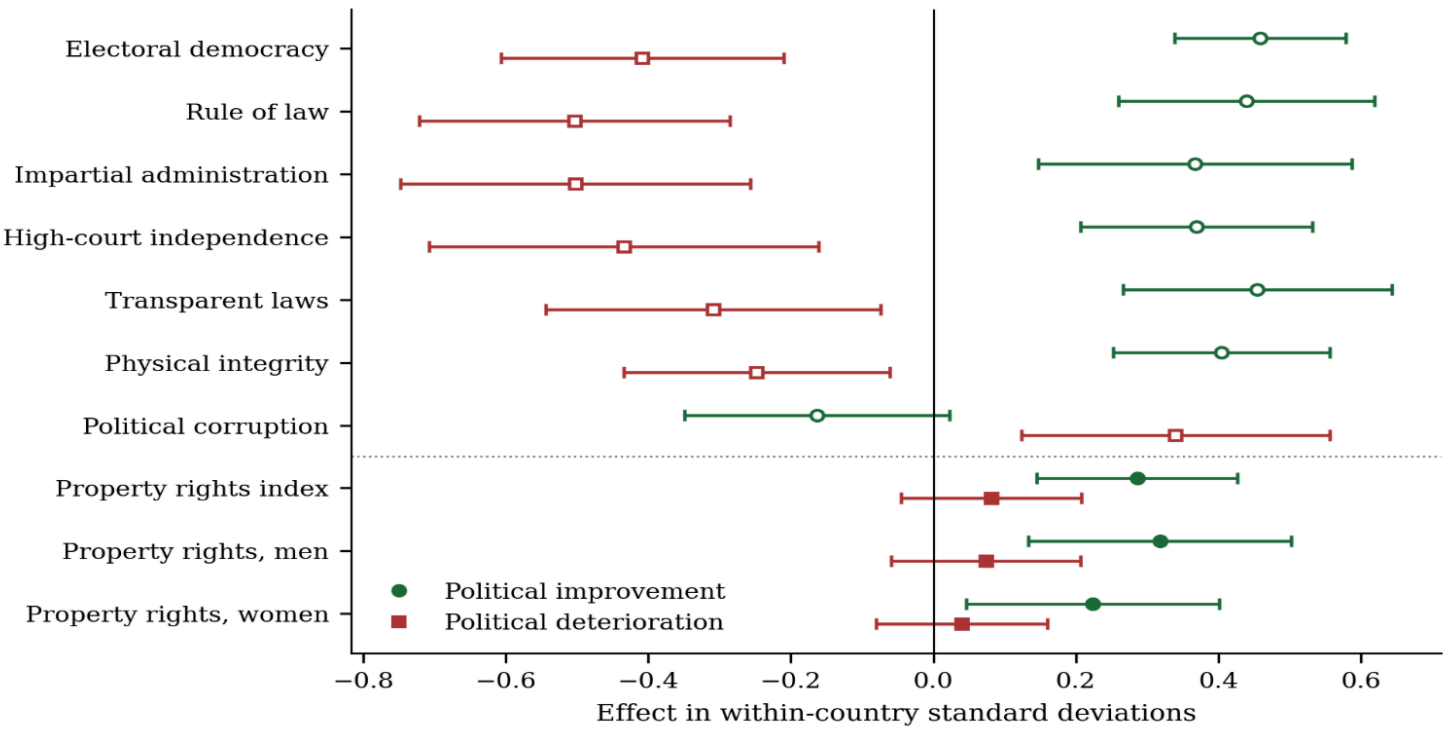


Figure 1

Improvement and Deterioration by Variable

The two direction-specific effects for every outcome, with 95 per cent confidence intervals. Hollow markers are the six comparison institutions, solid markers the three property-rights measures. For five of the six comparison institutions the two intervals sit on opposite sides of zero; the exception is political corruption, whose improvement interval just includes zero while its deterioration interval does not. For all three property-rights measures both point estimates are positive.

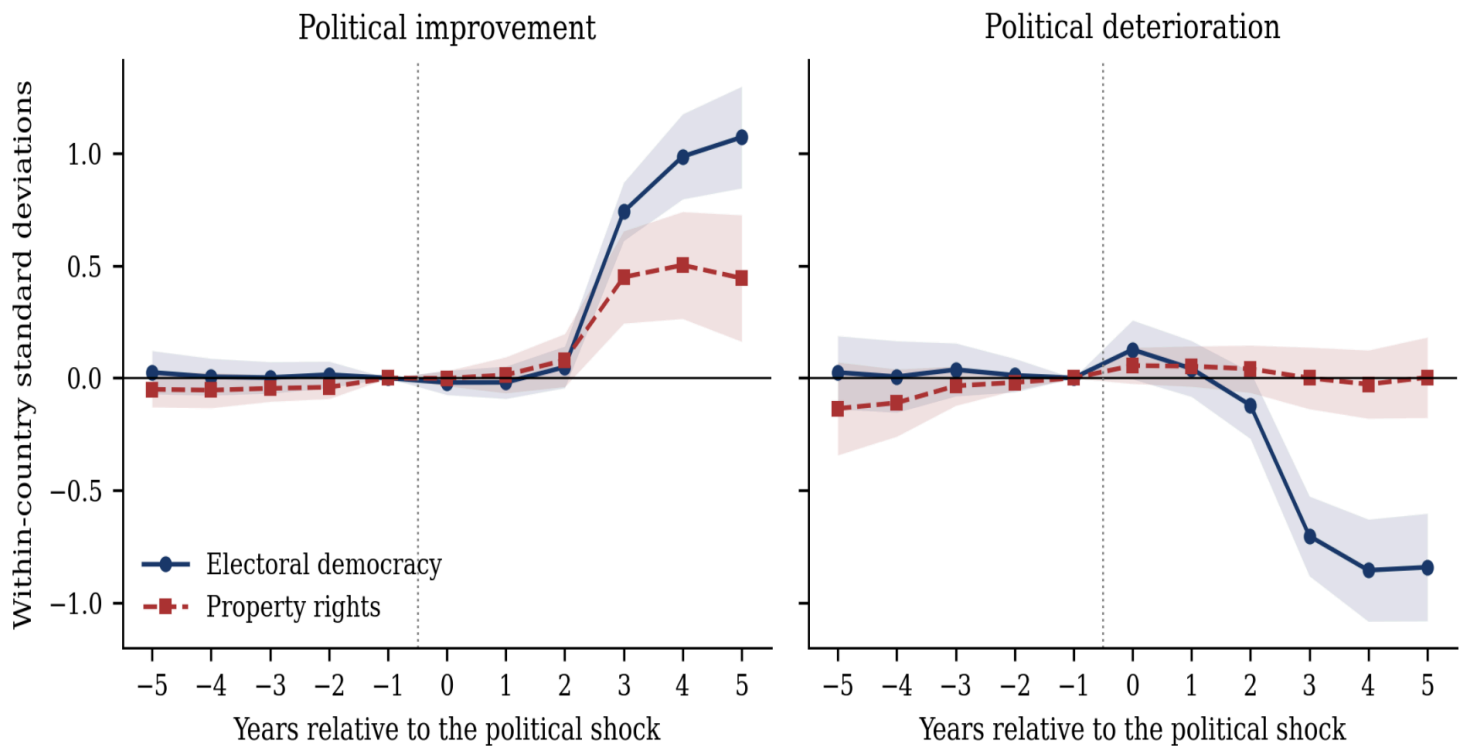


Figure 2

Event Studies

Event-study estimates with 95 per cent confidence intervals, by outcome and direction of the political shock. Relative year -1 is the omitted base period. Given that each event is dated at the start of its three-year change window, the political shock accumulates over years 0 to 3; the figure makes that construction visible. Electoral democracy separates in both panels and property rights only in the left one.