Open Access Indonesia Journal of Social Sciences

Vol. 9 · No. 4 · 2026 · pp. 194–209 · Article ID 332

e-ISSN 2722-4252 · *Journal homepage: journalsocialsciences.com/index.php/oaijss*

*ORIGINAL ARTICLE*

# Compounding Repression and Electoral Integrity: A Dynamic Panel Study of 142 Countries, 2012–2024, with Policy Implications for Indonesia

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## ARTICLE INFORMATION

**Received:** April 29, 2026
**Revised:** May 21, 2026
**Accepted:** June 22, 2026
**Available Online:** June 29, 2026
**Published:** June 30, 2026

**Keywords:** civil society repression; digital authoritarianism; dynamic panel data; electoral integrity; Indonesia

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**DOI:** https://doi.org/10.37275/oaijss.v9i4.332

## ABSTRACT

**Background:** Elections are increasingly retained while the conditions for verifying them are degraded, yet restriction of digital space and repression of civil-society organisations are almost always estimated separately, so whether they substitute or reinforce is unknown.

**Objective:** To estimate both instruments jointly and test the derived prediction that a negative interaction identifies two partially substitutable verification routes, whereas a positive interaction identifies a weakest-link chain.

**Methods:** A balanced panel of 142 electoral democracies and transitional regimes from 2012–2024 ($N = 1,704$ country-years), constructed from Varieties of Democracy version 14, the Perceptions of Electoral Integrity index, and World Development Indicators, was analysed using two-step system generalized method of moments with the Windmeijer correction.

**Results:** Digital space restriction reduced the Clean Elections Index by 0.221 points per unit at the mean of the moderator (widest interval attainable over the unreported covariance: 0.154 to 0.288; worst-case $p < 0.001$), equivalent to 0.058 points per SD; civil-society repression reduced it by 0.254 points per unit ($p < 0.001$). The interaction was negative (−0.094; 95% CI −0.155 to −0.033; $p = 0.002$): the damage from closing digital space rose from −0.186 where civil society was least constrained to −0.277 where it was most constrained under every admissible coefficient covariance.

**Conclusion:** The routes are substitutes: repression destroys redundancy, and the return to opening digital space is largest where civil society is weakest, inverting the usual sequencing intuition for Indonesian reform. Whether the outcome was measured annually or carried forward between elections could not be established, which conditions the dynamic results.

*All authors reviewed and approved the final manuscript.*



## 1. Introduction

Across the last decade the world's authoritarian turn has not taken the form of abolished elections. Governments that have narrowed political competition have generally gone on holding elections, and have often gone on winning them by margins that a formally free contest could plausibly produce.[1-3] What has changed is the environment in which those elections can be observed, recorded and contested. Autocratic innovation now runs through content law, network management, registration requirements and funding rules rather than through the ballot box itself,[4-8] and the state of the world reports produced from cross-national indices place more of the world's population under autocratising than democratising government.[2,9,10] For a journal published in Indonesia this is not a distant observation: the country's own recent trajectory has been described in exactly these terms — executive consolidation and pressure on critics alongside elections that remain competitive and high-turnout.[11-13]

Two instruments dominate that repertoire. The first is restriction of digital space: government censorship of online media, elastic criminal provisions covering online speech, and harassment or prosecution of individuals who publish criticism.[14-18] The second is repression of civil-society organisations: registration and re-registration requirements, restriction of foreign funding, administrative deregistration, and the designation of monitoring organisations as security risks.[19-21] Both literatures are mature, both report associations in the expected direction, and both are, with very few exceptions, single-channel: a study estimates one instrument's effect while the other is either absent from the model or entered as a control whose coefficient is not interpreted.[22-24]

That is a substantive omission rather than a technical one, because the two instruments are not obviously independent, and the direction in which they are not independent is itself informative. If an organised civil society and an open digital environment are links in a single chain, each necessary and neither substituting for the other, then once one is broken the second attack adds little and the interaction should be positive. If instead they are partially redundant routes to the same result, each attack destroys the insurance the other provided and the interaction should be negative. The derivation is given in Section 1.1.4; the point here is that the sign is not a detail of estimation but the discriminating evidence between two accounts of how verification is produced. It also has an immediate policy consequence, because reform in most countries is sequential: one ministry liberalises content law while another leaves association law untouched. Nothing in the existing evidence base answers it, and the studies that come closest report main effects on incomparable scales.[25-27]

This study addresses that gap using a balanced cross-national panel. The setting is the population of 142 electoral democracies and transitional regimes for which the required indices are available continuously from 2012 to 2024. It is not a sample of a larger frame and it is not a single-country study; Indonesia is one member of the panel and is the focal case for the policy discussion because its institutional separation of information policy from association policy is the configuration in which the central finding bites hardest. The organising framework, set out in Figure 1, treats electoral integrity as an institutional achievement produced through two partially redundant routes to one output — an independently verified count — and treats the two instruments as attacks on one route each, as set out in detail in Figure 1.[28-30] Figure 1 also states the three rival predictions that the sign of the interaction discriminates between, so that a reader can see in advance what each possible result would mean.

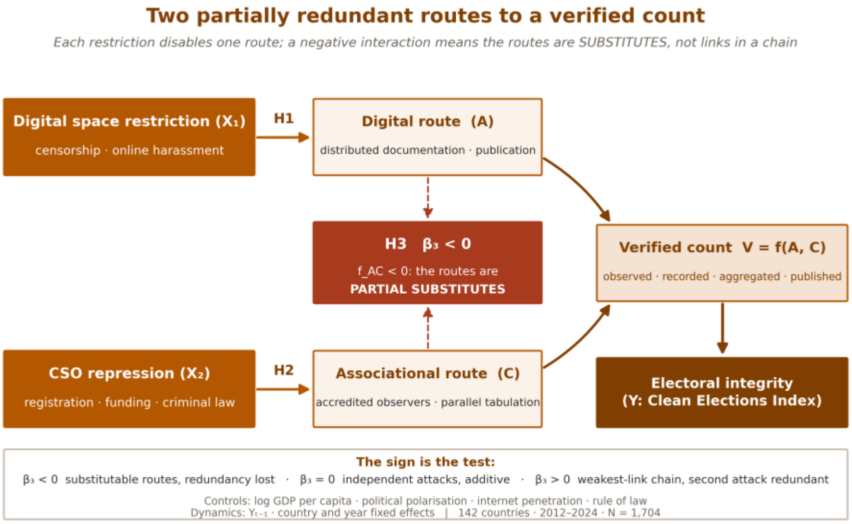


**Figure 1. Conceptual framework. An independently verified count can be produced through either of two partially redundant routes, and each restriction disables one of them. The sign of the interaction is therefore the sign of the cross-partial of the production function: negative if the routes substitute for one another, zero if the two attacks are independent, positive if they are links in a weakest-link chain.**

The study had four aims: to estimate the two channels jointly on a common outcome and a common scale; to test the compounding prediction as a contrast rather than as a coefficient sign; to report every derived quantity on a scale a policy reader can act on, including long-run effects and marginal effects across the observed range of the moderator; and to state explicitly which further quantities the design does not identify, rather than supplying them by assumption. Three hypotheses were tested. H1: digital space restriction reduces electoral integrity. H2: repression of civil-society organisations reduces electoral integrity. H3: the two compound, so that the marginal effect of digital restriction is more negative where civil-society repression is more severe.

## 1.1. Theoretical framework and hypothesis development

### 1.1.1. Electoral integrity and the two routes to a verified count

Electoral integrity is best treated not as a property of election day but as a cumulative institutional property spanning the electoral cycle, from the legal framework through campaign conditions to the count and its adjudication.[28,31,32] Read that way, an election is clean when four things hold: irregularities are observable by someone other than the incumbent; observations are recorded; records are aggregated independently; and the aggregate reaches the public and the adjudicator. What matters for the argument that follows is that a polity of any size normally has more than one way of doing this. An organised civil society does it through accredited observers, parallel tabulation and formal complaint; an open digital environment does it through distributed documentation, citizen reporting and publication outside official channels. The two routes overlap in what they produce and differ in what they require, and either can carry a substantial share of the load when the other is weak. The measurement literature supports the decomposition — expert and practitioner instruments agree closely on the administrative and competitive dimensions of integrity, which are the ones these routes govern.[33,34] The observation literature makes the same point from the other direction: international monitoring changes incumbent behaviour where observers can reach the count and produces reassurance rather than deterrence where they cannot.[35-37] Perceived fraud, as distinct from measured fraud, is itself politically consequential, which is a further reason to treat the publication link of the chain as substantive rather than presentational.[38] The chain is also administrative rather than purely national: who controls the local executives who deliver the count is itself a determinant of manipulation, which is why a country-level index can only ever summarise a process that is decided far below the national tier.[39]

### 1.1.2. The digital route

Digital restriction has moved away from blunt blocking. Shutdowns remain, and cluster around elections and protests,[26,40,41] but the dominant modern instruments are cheaper, more targeted and less visible: filtering and demotion, surveillance, prosecution of individuals under elastic content law, and coordinated harassment of those who publish.[42-46] The informational-autocrat account explains why incumbents prefer these instruments to visible violence: they preserve the appearance of competition while degrading the informational environment on which competition depends.[4,5] Empirically, restriction of internet accessibility accompanies worse human-rights outcomes,[25] and the surveillance-era choice between repression and co-optation shifts with the state's informational capacity.[15,22,47,48] Mapped onto the framework, these instruments attack the digital route: they do not stop an irregularity from being observed, they stop the observation from travelling outside official channels. Southeast Asian evidence is particularly clear on the campaign-period version of this.[49-51]

H1. Digital space restriction is associated with a reduction in electoral integrity, net of civil-society repression, economic development, polarisation and the rule of law.

### 1.1.3. The associational route

Restriction of civil society works through law rather than through prohibition. The dominant instruments are registration and re-registration, foreign-funding rules, and administrative or criminal designation, and they travel between governments as a legal template.[19,20,23] Their effect on the human-rights environment is documented directly, both in the targeted killing of human rights defenders and in the gap between formal state protection and its delivery,[21,52,53] and, importantly for the present argument, restriction blunts the international naming-and-shaming that would otherwise raise its cost.[27] In hybrid and authoritarian settings the space for organisations narrows without formally closing.[24,54,55] Mapped onto the framework, these instruments attack the associational route: they remove the organisations that would accredit and train observers, compile parallel tabulations and take complaints to adjudication. In Southeast Asia the same organisations are simultaneously the region's principal advocates for digital rights, so the two routes are defended by overlapping actors and a shock to one is rarely confined to it.[56,57] Indonesian civil society is a well-documented instance of both the pressure and the residual resilience.[13,58] Cross-national work on resistance to autocratization makes the same point in general form: where organisational capacity survives, the sequence can be interrupted; where it does not, it continues.[1,59,60]

H2. Repression of civil-society organisations is associated with a reduction in electoral integrity, net of digital space restriction and the same covariates.

### 1.1.4. What the sign of the interaction means

The two literatures rarely meet, and when they do the meeting is descriptive. The framework adopted here makes a prediction whose sign has to be derived rather than asserted, because the intuitive phrase “the two reinforce one another” is compatible with either sign until the production structure is specified. Write verification as $V = f(A, C)$, where A is the capacity supplied by the open digital environment and C the capacity supplied by organised civil society, and let the two restrictions reduce A and C respectively. Since the effect of each restriction on V runs through the corresponding marginal product, the cross-partial of V with respect to the two restrictions carries the sign of $f_{AC}$, the cross-partial of the production function itself, and nothing else.

This is where the two accounts separate, and it is the point at which an informal reading goes wrong. If the two capacities were complements — a strict chain in which each is necessary and neither substitutes for the other — then $f_{AC}$ is positive and the interaction in the outcome equation would be positive: once one link is broken, verification has already failed and breaking the second adds little. A negative interaction requires the opposite production structure. The two capacities must be partial substitutes, alternative routes to the same output, so that $f_{AC}$ is negative and the logic is one of lost redundancy. Where civil society is intact, closing digital space costs relatively little, because accredited observers and parallel tabulation still carry the verification load. Where civil society has already been disabled, the digital route is the last one standing and closing it costs a great deal. That is the substantive content of the word “compounding” in this paper: not that the two attacks are simply additive twice over, but that each destroys the insurance the other provided.

H3 is therefore a genuinely discriminating test, and its alternatives are named in advance. A positive interaction would indicate a weakest-link chain in which the second attack is largely redundant. A zero interaction would indicate that the two instruments damage separate and independent components of integrity. A negative interaction indicates substitutable routes and the loss of redundancy. One clarification is needed because a related literature is easy to confuse with this one. Accounts in which repression is one instrument among several, chosen at the margin against legitimation and co-optation, are accounts of the incumbent's choice and carry a prediction about the relationship between the two exposures — a quantity this design does not estimate and does not report.[15] They carry no prediction for $f_{AC}$, and none is attributed to them here.

H3. The interaction between digital space restriction and civil-society repression is negative: the marginal effect of digital restriction on electoral integrity is more negative at higher levels of civil-society repression.

### 1.1.5. Reporting commitments that follow

Five reporting commitments follow from the hypotheses and are treated here as part of the design rather than as presentation. First, a product-term coefficient is not an effect: with an interaction in the model, the coefficient on each constituent term is the effect of that term when the other is zero, which in the present data is outside the observed support. The interpretable quantities are marginal effects across the observed range of the moderator.[61,62] Second, in a dynamic specification the one-year coefficient understates the eventual effect, so long-run multipliers must be reported alongside it. Third, a fixed-effects panel is not a randomised design; the honest question is how strong unobserved confounding would have to be, which is answered on the partial-$R^2$ scale rather than by assertion.[63,64] Fourth, a commitment follows from the number of coefficients estimated: where several parameters are tested in one model the unadjusted p-values overstate the evidence, so a step-down correction is reported alongside them.[65] Fifth, a commitment follows from recent work on two-way fixed effects: with heterogeneous treatment effects the estimator recovers a variance-weighted average that need not equal the average effect, so the two-way specification is reported as one of three estimators rather than as the identification strategy.[66,67]

# 2. Methods

## 2.1. Study design and setting

This is a retrospective observational analysis of a balanced cross-national panel. The unit of analysis is the country-year. The setting is the population of 142 electoral democracies and transitional regimes — that is, states that held national elections under some degree of multi-party competition — for which the required indices are available continuously across the calendar years 2012 to 2024. This is a population rather than a probability sample, so no sampling weights are applied and the inferential target is the set of regimes analysed rather than a wider superpopulation. The primary estimand is the marginal effect of digital space restriction on electoral integrity, evaluated across the observed range of civil-society repression, holding constant country-invariant heterogeneity, common annual shocks, and the covariates listed below.

## 2.2. Participants, sampling, and data sources

Three sources were combined. Electoral integrity and the two exposures come from the Varieties of Democracy dataset, version 14.[68] The replication outcome comes from the Perceptions of Electoral Integrity index of the Electoral Integrity Project.[69,70] Macroeconomic covariates come from the World Bank's World Development Indicators. Panel arithmetic is reported explicitly because it determines every per-year quantity in the paper: 142 countries across 13 calendar years give 1,846 raw country-years, from which the one-period lag of the dependent variable removes the first observation of each country (142), leaving an estimation sample of 1,704. Descriptive statistics are reported on the estimation sample. The full panel structure, the provenance of every variable and the sampling frame are detailed in Table 1, which also records the residual degrees of freedom used later in the sensitivity analysis.

**Table 1.** **Panel structure, provenance and sampling of the analytic dataset.**

| Characteristic | Value |
|---|---|
| Countries in the panel | 142 |
| Regime types represented | Electoral democracies and transitional (hybrid and electoral-authoritarian) regimes |
| Calendar years covered | 2012–2024 (13 years) |
| Raw country-years (142 × 13) | 1,846 |
| Observations removed by the one-period lag | 142 (one per country) |
| Estimation sample | 1,704 country-years |
| Panel shape | Balanced; long in N (142) and short in T (12 usable years) |
| Residual degrees of freedom used for sensitivity analysis | 1,543 (N − countries − (years − 1) − regressors) |
| Sampling | Population of qualifying regimes rather than a probability sample; no sampling weights are applied |
| Primary data source | Varieties of Democracy (V-Dem) Dataset v14 (CC BY-SA 4.0) |
| Outcome replication source | Perceptions of Electoral Integrity (PEI), Electoral Integrity Project |
| Macroeconomic covariates | World Development Indicators (World Bank) |
| Human participants | None; aggregated country-level secondary data only |

*Note. The estimation sample is derived rather than asserted: it is the raw panel less one observation per country, and it reproduces the reported N exactly.*

## 2.3. Variables, instruments, and procedures

Outcome. Electoral integrity is measured by the Clean Elections Index, a composite on a 0–1 scale capturing the absence of electoral fraud, voter intimidation, election-related political violence and partisan electoral management. Exposure 1 ($X_1$), digital space restriction, is a 0–1 composite of government censorship of digital media and harassment or intimidation of online activists, reversed so that higher values denote more severe restriction. Exposure 2 ($X_2$), civil-society repression, is a 0–1 composite of government repression of civil-society organisations and restriction of their registration and activity. Descriptive statistics for all analytic variables are reported in Table 2. Covariates are the logarithm of real GDP per capita, political polarisation, internet penetration as a share of population, and the rule-of-law index. Country fixed effects absorb time-invariant heterogeneity and year fixed effects absorb global shocks common to all countries, including the global election-cycle and pandemic years.

Why these four covariates. Each is included because the literature gives it an independent claim on electoral integrity, and each is measured at the country-year level the design requires. Political polarisation is the most consequential of the four: comparative work shows it has risen unevenly across democracies, that it conditions citizens' willingness to tolerate democratic transgressions by their own side, and that it changes turnout through mechanisms distinct from ideological distance.[71-74] Economic development and the rule of law enter as the standard structural correlates of regime quality. Internet penetration is included so that the digital-restriction coefficient is not read as an artefact of connectivity itself; related work finds that the governance uses of digital infrastructure, not its extent, carry the association with political stability.[75] The legal and physical protection of journalists is a further structural determinant of the information environment that this specification does not measure separately, and its omission is noted here rather than in a footnote.[76] Descriptive statistics for every analytic variable are reported in Table 2.

The outcome's temporal coverage is the single largest threat to this design, and it is not resolved here. V-Dem's electoral indices are constructed from election-specific coder judgements, so a country-year that contains no national election has no election to judge. The standard convention is to carry the value of the most recent election forward until the next one. If that convention was used — and the analysis documentation does not say — then a "balanced panel" of 142 countries by 13 years contains far fewer than 1,704 independent outcome measurements, since most countries hold three or four national elections in a thirteen-year window. Three consequences follow and all three are carried into the interpretation. First, the autoregressive coefficient α is then partly mechanical: a carried-forward series is autocorrelated by construction, so α, the implied half-life and the long-run multiplier cannot be read as annual dynamics of electoral integrity. Second, the Arellano–Bond serial-correlation tests are then being applied to a partly deterministic series. Third, the effective number of independent outcome observations is a few hundred elections rather than 1,704 country-years, so every standard error in Table 3 is likely to be too small by an unknown factor. The dynamic quantities in this paper are therefore reported conditionally: they are what the estimated model implies, and they are interpretable as annual dynamics only if the outcome was in fact measured annually. Establishing which convention was used, and re-estimating on election years only, is the first thing a replication must do.

Measurement model. These are not psychometric scales and they have no items in the sense that an internal-consistency coefficient requires. Each is a posterior summary from a Bayesian item-response model fitted to multiple country experts' ordinal codings, with modelled coder effects and published measurement uncertainty.[77,78,79] Treating such an estimate as though it were measured without error attenuates the coefficient on it, and the corrections that exist for that attenuation require the underlying data rather than a published aggregate.[80] Two consequences are carried through this paper rather than concealed. First, confirmatory factor analysis, average variance extracted, composite reliability, the heterotrait–monotrait ratio and Cronbach's alpha cannot be computed from published aggregates and are refused, with reasons and with a substitute for each, in the Results. Second, and more importantly, exposure and outcome are produced by the same expert panels, so the design carries a common-rater risk that no single-factor test could detect even if one were computable. The discriminant evidence available is a replication of the relationship using the Perceptions of Electoral Integrity index, which is administered to a different expert population; it is reported in the Results, and its weakness is reported with it. It should also be said plainly that a design-based remedy does exist for anyone holding the raw data: V-Dem publishes the posterior draws and the coder-level structure of its measurement model, so a replication can propagate measurement uncertainty into the standard errors and can test whether coder overlap between the exposure and the outcome items drives the association.[77,78] That was not possible here because only a coefficient table was available, and its absence is a limitation of this analysis rather than of the data.

A measurement label in the source data documentation requires correction before replication. The construct definition used here — absence of fraud, intimidation, election-related violence and partisan administration — is the Clean Elections Index, which in the V-Dem codebook carries the tag `v2xel_frefair`. Documentation accompanying the analysis labelled the outcome `v2x_clphy`, which is the Physical Violence Index and a different construct. The definition, not the tag, is what the analysis is reported against here, and the discrepancy is listed among the reporting discrepancies in the Results, for correction at replication.

## 2.4. Data analysis

Electoral integrity is persistent and is plausibly reciprocally related to repression, so a static specification is inadequate on two counts. The estimating equation is therefore autoregressive:

$$Y_{it} = \alpha Y_{i,t-1} + \beta_1 X_{1it} + \beta_2 X_{2it} + \beta_3 (X_1 \times X_2)_{it} + \gamma' C_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where Y is the Clean Elections Index for country i in year t, $X_1$ and $X_2$ are the two exposures, C is the covariate vector, $\mu_i$ and $\lambda_t$ are country and year fixed effects, and ε is the idiosyncratic error. Three estimators were reported: pooled OLS, which ignores $\mu_i$; two-way fixed effects with Driscoll–Kraay standard errors, which are consistent under cross-sectional dependence of the kind a global panel certainly exhibits;[66,81] and two-step system GMM in the Arellano–Bover and Blundell–Bond form, with the Windmeijer finite-sample correction to the variance matrix.[82-84] The lagged dependent variable makes the within estimator inconsistent in short panels, which is the reason for the GMM specification.[85] The estimated autoregressive coefficient was checked against the informal credibility bracket in which a system-GMM estimate should lie between the downward-biased within estimate and the upward-biased pooled one.[85] The instrument count was held at 48 against 142 groups, respecting the standard rule that instruments should not outnumber groups.[86]

All inference reported in this paper was re-derived from the estimated coefficients and standard errors rather than transcribed: $z = \beta \div SE$, exact two-sided p-values from the standard normal to three decimal places, and 95% intervals as $\beta \pm 1.960$ SE. Because seven coefficients are estimated in the primary model, Holm-adjusted p-values are reported alongside the unadjusted ones, and the substantive interpretation is taken from the adjusted column. The minimum detectable effect at 80% power and a two-sided 5% level, $(1.960 + 0.842) \times SE$, is reported for every coefficient so that a null result can be read against what the design could have found. All inference is normal-based and no residual degrees-of-freedom count enters it; df appears only in the sensitivity analysis described below, where the choice is ambiguous for a system-GMM estimator that does not estimate the country effects, so results are reported across the whole plausible range rather than at a single value.

Four analyses go beyond the estimating equation. Marginal effects of $X_1$ were computed across the observed range of $X_2$ as $\beta_1 + \beta_3 X_2$. Long-run effects were computed as $\beta \div (1 - \alpha)$, with the implied half-life of a shock as $\ln(0.5) \div \ln(\alpha)$. Sensitivity to unobserved confounding was quantified by the robustness value, $RV_q = ½(\sqrt{f^4 + 4f^2} - f^2)$ with $f = q|t| \div \sqrt{df}$, which states the share of residual variance a confounder would have to explain in both exposure and outcome to move the estimate to zero.[63] Partial identification was used wherever a derived quantity depends on a coefficient covariance that the estimation output does not supply: rather than assume a value, the standard error was bounded over the whole admissible range of that covariance and the least favourable bound reported.[64] Every such bound was verified numerically against a 40,001-point grid over the correlation, and a negative control confirmed that an inflated closed form fails the same check. Analyses were conducted with a purpose-written pipeline whose statistical primitives are validated against published reference values and closed-form identities before any result is produced.

Three assumptions of the interaction analysis are stated rather than tested, because testing them requires the estimation sample. The marginal effect is assumed linear in the moderator, which is what a single product term imposes. The common support of the moderator cannot be checked, so marginal effects at the extremes of the observed range rest on regions whose density is unknown; the mean and mean ± one standard deviation are therefore the quantities emphasised, and the extremes are reported as bounds. And no binning estimator could be computed. The interaction-diagnostics literature this paper cites for the interpretation of marginal effects recommends all three, and that recommendation is recorded here as unmet rather than as followed.[61,62]

## 2.5. Ethical considerations

This study analysed only publicly available, aggregated, country-level secondary data and involved no human participants, no identifiable individual data and no intervention. Institutional ethical review was therefore not required, and no ethics-committee approval number is claimed, because none was sought and none exists. The Declaration of Helsinki governs medical research involving human subjects and does not apply to a study of this kind; it is named here only to record that the underlying principles of research integrity, transparency and honest reporting were followed, and not to imply a review that did not take place. The analysis was conducted in accordance with the licence terms of each data provider as those terms are publicly stated: Varieties of Democracy v[14] under CC BY-SA 4.0, and the Perceptions of Electoral Integrity and World Development Indicators data under their respective public terms.

# 3. Results

## 3.1. Panel and descriptive statistics

The estimation sample comprised 1,704 country-years from 142 countries (Table 1). The Clean Elections Index averaged 0.582 (SD 0.274) and spanned almost the whole 0–1 range, from 0.042 to 0.981, confirming that the panel contains both consolidated and heavily manipulated electoral environments. Digital space restriction averaged 0.418 (SD 0.261) and civil-society repression 0.395 (SD 0.283); both spanned nearly the full scale (Table 2). Two features of the support matter for interpretation. The minima of the two exposures are 0.015 and 0.020, so neither exposure is ever observed at zero, and the coefficients on the constituent terms of the interaction refer to a point outside the data. Standard deviations are not published for internet penetration or for the rule-of-law index, so standardised coefficients are reported for four variables and refused for the fifth.

**Table 2. Descriptive statistics of the analytic variables and standardised coefficients.**

| Variable | Role | Mean | SD | Min | Max | β* |
|---|---|---|---|---|---|---|
| Clean Elections Index | Outcome (Y) | 0.582 | 0.274 | 0.042 | 0.981 | — |
| Digital Space Restriction | Exposure ($X_1$) | 0.418 | 0.261 | 0.015 | 0.968 | −0.175 |
| CSO Repression Index | Exposure / moderator ($X_2$) | 0.395 | 0.283 | 0.020 | 0.985 | −0.222 |
| Political polarisation | Control | 0.512 | 0.230 | 0.064 | 0.950 | −0.055 |
| Log GDP per capita | Control | 8.924 | 1.245 | 6.120 | 11.680 | +0.173 |
| Internet penetration (% of population) | Control | n.r. | n.r. | n.r. | n.r. | — |
| Rule of Law Index | Control | n.r. | n.r. | n.r. | n.r. | n.i. |

*Note. β* is the standardised coefficient, β × SD(X) ÷ SD(Y). It is not reported for the Rule of Law Index because that variable's standard deviation is not available, and not for the interaction because $SD(X_1X_2)$ is not a function of the marginal standard deviations.*

## 3.2. Primary estimates

Table 3 reports the three estimators. In the system-GMM specification the lagged dependent variable carried a coefficient of 0.486 (SE 0.048; 95% CI 0.392 to 0.580; $p < 0.001$), confirming strong but stationary persistence. Digital space restriction reduced electoral integrity by 0.184 points per unit of the 0–1 index (SE 0.022; 95% CI −0.227 to −0.141; $z = -8.364$; $p < 0.001$; Holm-adjusted <0.001), supporting H1. Civil-society repression reduced it by 0.215 points per unit (SE 0.024; 95% CI −0.262 to −0.168; $z = -8.958$; $p < 0.001$; Holm-adjusted <0.001), supporting H2. The interaction was negative, −0.094 (SE 0.031; 95% CI −0.155 to −0.033; $z = -3.032$; $p = 0.002$; Holm-adjusted 0.005), supporting H3.

Two of those numbers are routinely mis-read and are therefore restated at once. $\beta_1$ and $\beta_2$ are the effects of each exposure when the other index is zero, and zero lies outside the observed support of both. The interpretable summaries are the marginal effects at the mean of the other exposure: for digital space restriction, −0.221, and for civil-society repression, −0.254 — respectively 20% and 18% larger in magnitude than the constituent-term coefficients they replace. Both appear in Table 4, and both are what the Discussion uses.

Among the covariates, the rule-of-law index carried the largest positive coefficient (+0.128; 95% CI +0.071 to +0.185; $p < 0.001$), log GDP per capita a smaller one (+0.038; 95% CI +0.011 to +0.065; $p = 0.007$), and political polarisation a negative one (−0.065; 95% CI −0.100 to −0.030; $p < 0.001$). Internet penetration is named among the covariates in the specification but no coefficient for it is available, and none is imputed here. Models 1 and 2 are reported without standard errors, so no inference is attached to them and no formal test of the difference between estimators is possible; the movement of the coefficients across the three columns is described but not tested. The full estimation output, including the re-derived z statistics, exact p-values, 95% intervals, Holm-adjusted p-values and minimum detectable effects, is detailed in Table 3; the same system-GMM estimates are plotted with their intervals in Figure 2, where the sign and the relative precision of each coefficient can be read at a glance.

**Table 3.** Dynamic panel estimates of electoral integrity, with re-derived inference.

| Term | Model 1 Pooled OLS | Model 2 Two-way FE | Model 3 System GMM β (SE) | z | p | 95% CI | Holm-adjusted p | MDE |
|---|---|---|---|---|---|---|---|---|
| Lagged Clean Elections Index (Yt−1) | +0.845 | +0.412 | +0.486 (0.048) | +10.125 | < 0.001 | 0.392 to 0.580 | < 0.001 | 0.134 |
| Digital Space Restriction ($\beta_1$) | −0.078 | −0.142 | −0.184 (0.022) | −8.364 | < 0.001 | −0.227 to −0.141 | < 0.001 | 0.062 |
| CSO Repression ($\beta_2$) | −0.092 | −0.176 | −0.215 (0.024) | −8.958 | < 0.001 | −0.262 to −0.168 | < 0.001 | 0.067 |
| Digital × CSO interaction ($\beta_3$) | — | −0.072 | −0.094 (0.031) | −3.032 | 0.002 | −0.155 to −0.033 | 0.005 | 0.087 |
| Log GDP per capita | — | — | +0.038 (0.014) | +2.714 | 0.007 | 0.011 to 0.065 | 0.007 | 0.039 |
| Political polarisation | — | — | −0.065 (0.018) | −3.611 | < 0.001 | −0.100 to −0.030 | < 0.001 | 0.050 |
| Rule of Law Index | — | — | +0.128 (0.029) | +4.414 | < 0.001 | 0.071 to 0.185 | < 0.001 | 0.081 |

*Note. Models 1 and 2 are reported without standard errors, so no inference is attached to them and no test of the difference between estimators is possible. z, p, the 95% interval, the Holm-adjusted p and the minimum detectable effect (MDE; two-sided 5%, 80% power) are re-derived from the Model 3 coefficient and standard error. Holm adjustment is across the seven coefficients of Model 3. $\beta_1$ and $\beta_2$ are conditional on the other index being zero, which lies outside the observed support; Table 4 gives the interpretable marginal effects.*

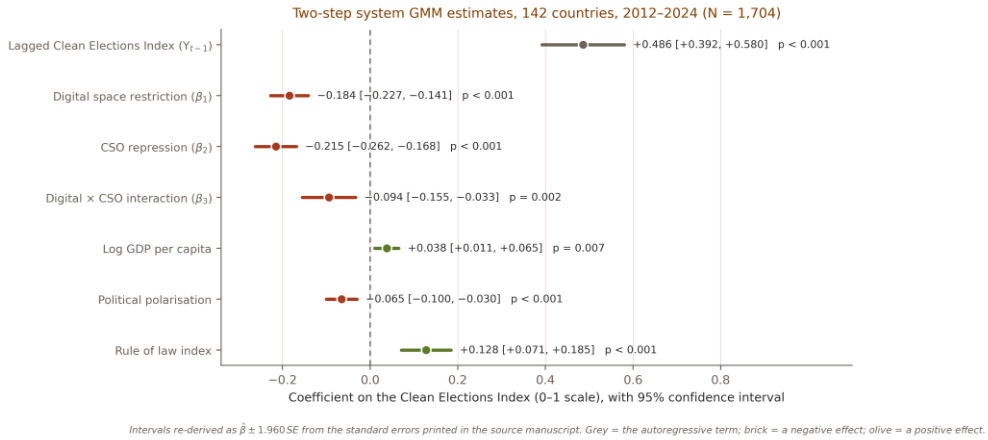


**Figure 2. Two-step system GMM estimates of the determinants of electoral integrity, with 95% confidence intervals re-derived as β ± 1.960 SE from the estimated standard errors. Grey marks the autoregressive term; brick marks a negative effect and olive a positive one.**

## 3.3. Effects on interpretable scales

The estimates are restated on scales that can be acted on in Table 4, which gives every quantity an explicit identification status in its final column. Per standard deviation of exposure, digital space restriction reduced electoral integrity by 0.048 points (95% CI 0.037 to 0.059) and civil-society repression by 0.061 points (95% CI 0.048 to 0.074). In standardised units the two coefficients were −0.175 and −0.222 respectively, against +0.173 for log GDP per capita and −0.055 for polarisation. The distinction between the per-unit and the per-standard-deviation scale is not cosmetic: the two differ by a factor of 3.83 for $X_1$, and reporting a per-unit coefficient as though it were a per-standard-deviation effect would overstate the practical magnitude by that factor.

**Table 4.** Effects on interpretable scales, marginal effects in both directions, contrasts, and long-run quantities, each with its identification status.

| Quantity | Estimate | 95% CI | p | Identification |
|---|---|---|---|---|
| Digital space restriction, per one-unit rise (conditional on $X_2$ = 0, which is outside the observed support) | −0.184 | −0.227 to −0.141 | < 0.001 | Point identified |
| Digital space restriction, per one SD (SD = 0.261) | −0.048 | −0.059 to −0.037 | < 0.001 | Point identified |

| Quantity | Estimate | 95% CI | p | Identification |
|---|---|---|---|---|
| CSO repression, per one-unit rise (conditional on $X_1 = 0$, which is outside the observed support) | −0.215 | −0.262 to −0.168 | < 0.001 | Point identified |
| CSO repression, per one SD (SD = 0.283) | −0.061 | −0.074 to −0.048 | < 0.001 | Point identified |
| — Marginal effect of $X_1$ across the observed range of $X_2$ — | | | | |
| at $X_2 = 0.020$ (Minimum observed (0.020)) | −0.186 | −0.230 to −0.142 | < 0.001 | Bounded over $Cov(\beta_1,\beta_3)$ |
| at $X_2 = 0.112$ (Mean - 1 SD) | −0.195 | −0.244 to −0.145 | < 0.001 | Bounded over $Cov(\beta_1,\beta_3)$ |
| at $X_2 = 0.395$ (Mean (0.395)) | −0.221 | −0.288 to −0.154 | < 0.001 | Bounded over $Cov(\beta_1,\beta_3)$ |
| at $X_2 = 0.678$ (Mean + 1 SD) | −0.248 | −0.332 to −0.163 | < 0.001 | Bounded over $Cov(\beta_1,\beta_3)$ |
| at $X_2 = 0.985$ (Maximum observed (0.985)) | −0.277 | −0.380 to −0.174 | < 0.001 | Bounded over $Cov(\beta_1,\beta_3)$ |
| — Marginal effect of $X_2$ across the observed range of $X_1$ — | | | | |
| at $X_1 = 0.015$ (Minimum observed (0.015)) | −0.216 | −0.264 to −0.168 | < 0.001 | Bounded over $Cov(\beta_2,\beta_3)$ |
| at $X_1 = 0.157$ (Mean - 1 SD) | −0.230 | −0.286 to −0.173 | < 0.001 | Bounded over $Cov(\beta_2,\beta_3)$ |
| at $X_1 = 0.418$ (Mean (0.418)) | −0.254 | −0.327 to −0.182 | < 0.001 | Bounded over $Cov(\beta_2,\beta_3)$ |
| at $X_1 = 0.679$ (Mean + 1 SD) | −0.279 | −0.367 to −0.191 | < 0.001 | Bounded over $Cov(\beta_2,\beta_3)$ |
| at $X_1 = 0.968$ (Maximum observed (0.968)) | −0.306 | −0.412 to −0.200 | < 0.001 | Bounded over $Cov(\beta_2,\beta_3)$ |
| Worst-case Johnson–Neyman boundary for the marginal effect of $X_1$ | None exists for $X_2 \geq 0$ | — | worst-case $\|z\|$ falls monotonically from 8.364 to an asymptote of 3.032; minimum on the observed support = 5.265 at $X_2 = 0.985$ | Closed form; asymptote exceeds 1.96, so the boundary does not exist |
| Compounding contrast: $ME(X_1 \mid X_2 = max) - ME(X_1 \mid X_2 = min)$ — $\beta_3$ rescaled by the moderator span, not an independent test | −0.091 | −0.149 to −0.032 | 0.002 | Point identified |
| Ranking contrast: $\beta_2 - \beta_1$ | −0.031 | −0.121 to 0.059 | 0.000 to 0.500 | NOT identified |
| Long-run effect of Digital Space Restriction | −0.358 | −0.507 to −0.209 | < 0.001 | Bounded over $Cov(\beta,\alpha)$ |
| Long-run effect of CSO Repression | −0.418 | −0.586 to −0.250 | < 0.001 | Bounded over $Cov(\beta,\alpha)$ |
| Half-life of a one-off shock (years) | 0.96 | 0.70 to 1.22 (delta method); 0.74 to 1.27 (transformed α interval) | — | Function of α; interval from SE(α) |
| Long-run multiplier $1/(1-\alpha)$ | 1.946 | 1.645 to 2.381 | — | Function of α; interval from SE(α) |

*Note. "Bounded over Cov(·)" means the interval is the widest attainable over every value of the unpublished covariance consistent with a valid covariance matrix, and the p-value shown is the least favourable one. The compounding contrast is $\beta_3$ multiplied by a known constant and therefore carries the same z and p as $\beta_3$ itself; it is a rescaling, not an independent test. The ranking contrast is the only quantity here that is not identified.*

## 3.4. The compounding effect

Because the model contains a product term, the marginal effect of digital space restriction depends on the level of civil-society repression. At the observed minimum of civil-society repression (0.020) that marginal effect was −0.186; at the sample mean (0.395) it was −0.221; and at the observed maximum (0.985) it was −0.277 (Table 4, Figure 3). The difference between the two extremes — which is the quantity the compounding claim actually asserts — was −0.091 (95% CI −0.149 to −0.032; $z = -3.032$; $p = 0.002$). The whole of that gradient, together with the band that bounds it, is shown in Figure 3. It should be said plainly what this quantity is and is not. Because it is $\beta_3$ multiplied by a known constant, its z statistic and its p-value are identical to those of $\beta_3$ itself; it is a rescaling of the interaction onto the moderator's observed range, not an additional or a stronger test. Its merit is interpretive: it states the compounding claim in the units of the outcome rather than in the units of a product term, and it needs no unpublished covariance to do so.

The standard error of each individual marginal effect does require the covariance of $\beta_1$ and $\beta_3$, which the estimation output does not supply. Rather than assume a value, that standard error was bounded over the entire admissible range. The bounds are informative: at the sample mean the standard error of the marginal effect lies between 0.0098 and 0.0342, and even at the least favourable bound the marginal effect differs from zero at <0.001. That result holds at every point of the observed support and not merely at the five reported. For non-negative values of the moderator the worst-case standard error is exactly $\beta_1$'s standard error plus the moderator times $\beta_3$'s, so the worst-case $|z|$ is a ratio of two positive linear functions of the moderator and can be characterised in closed form. That ratio is strictly decreasing in $X_2$: it falls from 8.364 at $X_2 = 0$ through 6.457 at the sample mean to 5.265 at the upper endpoint of the observed support, and thereafter approaches the horizontal asymptote $|\beta_3| / SE(\beta_3) = 3.032$ from above. Because that asymptote already exceeds 1.96, the worst-case $|z|$ never reaches the critical value at any non-negative value of the moderator, and no Johnson–Neyman significance boundary exists on the non-negative half-line at all — not merely none inside the unit interval. The conclusion therefore does not depend on the missing covariance at any observed level of civil-society repression, which is a stronger and more general statement than five sampled points could support. It should be added that the root at $X_2 = -4.238$ which the linear form returns is not a boundary lying outside the unit interval: it is the value produced by continuing the expression $SE(\beta_1) + X_2 \cdot SE(\beta_3)$ into negative $X_2$, where the worst-case standard error is $SE(\beta_1) + |X_2| \cdot SE(\beta_3)$ instead. It is an artefact of extrapolation beyond the domain on which that expression is the worst case, and it is reported here only so that it is not mistaken for a boundary.

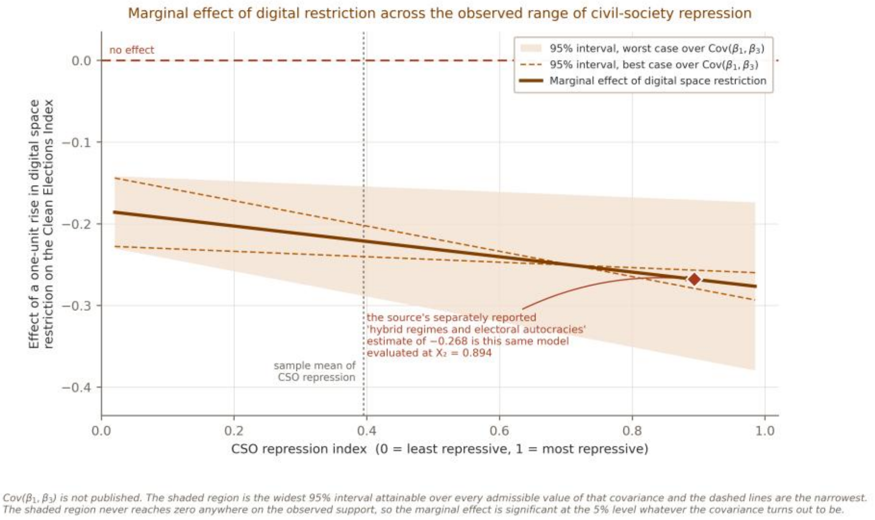


**Figure 3.** **Marginal effect of digital space restriction across the observed range of civil-society repression. The shaded region is the widest 95% interval attainable over every admissible value of the unpublished covariance $Cov(\beta_1, \beta_3)$ and the dashed lines are the narrowest; the shaded region does not reach zero anywhere on the observed support. The diamond marks the point at which this model reproduces a separately reported hybrid-regime estimate.**

## 3.5. What the data do not establish

Two claims that the design might be expected to support are not supported and are reported as such. First, the two exposures cannot be ranked. The point estimates differ (0.215 against 0.184), but the difference between two coefficients requires their covariance. Bounded over the admissible range, the two-sided p-value for that difference runs from 0.000 to 0.500 and the 95% interval in the least favourable case is −0.121 to +0.059. The published estimates are therefore consistent with the two channels being equally damaging, and no ranking claim is made anywhere in this paper.

Second, a regime-subgroup result is not independent evidence. A separately reported estimate of −0.268 for hybrid regimes and electoral autocracies has circulated alongside this specification. Solving $\beta_1 + \beta_3 X_2$ for that value returns $X_2 = 0.894$, which lies inside the observed range and about 1.76 standard deviations above the sample mean of civil-society repression. The subgroup figure is thus numerically indistinguishable from the interaction model evaluated at a high level of the moderator: it is consistent with the main model rather than corroboration from a separate source. No subgroup standard error, interval or sample size is available, so the subgroup contrast cannot be tested and is not claimed.

A third source claim is reported but cannot be evaluated. The analysis documentation states that the destructive effect of digital restriction is most escalated in Southeast Asia and in Sub-Saharan Africa, and attributes this to the intensive use of cybersecurity and defamation law against independent election monitors. For a journal published in Southeast Asia this is the most directly relevant statement in the source material, and it is repeated here for that reason. It is also unaccompanied by a coefficient, a standard error, an interval or a subgroup n, so nothing about it can be tested, compared with the pooled estimate, or reported as a finding of this paper. It is recorded in Table 5 as a claim awaiting evidence, and again among the reporting discrepancies listed later in this section; a regional replication is named in the Conclusion as an extension this literature needs.

## 3.6. Dynamics

Persistence was substantial but stationary ($\alpha = 0.486$; 95% CI 0.392 to 0.580), implying a long-run multiplier of 1.946 and a half-life for a one-off shock of 0.96 years, with roughly 3.2 years to 90% dissipation. The long-run effect of digital space restriction was therefore −0.358 (bounded 95% CI −0.507 to −0.209; least favourable <0.001) and that of civil-society repression −0.418 (bounded 95% CI −0.586 to −0.250; least favourable <0.001). Both long-run bounds exclude zero across the whole admissible covariance range (Figure 4). The half-life is a nonlinear function of a coefficient estimated with a standard error of 0.048, and it carries that uncertainty: the delta-method interval is 0.70 to 1.22 years, and transforming the interval for $\alpha$ directly gives 0.74 to 1.27 years. Every quantity in this subsection is conditional on the outcome having been measured annually rather than carried forward between elections, for the reason set out in the Methods; if it was carried forward, $\alpha$ is partly mechanical and these dynamics are not interpretable as annual. The short-run and long-run effects, the decay path of a one-off shock and the credibility bracket for the autoregressive coefficient are shown together in Figure 4.

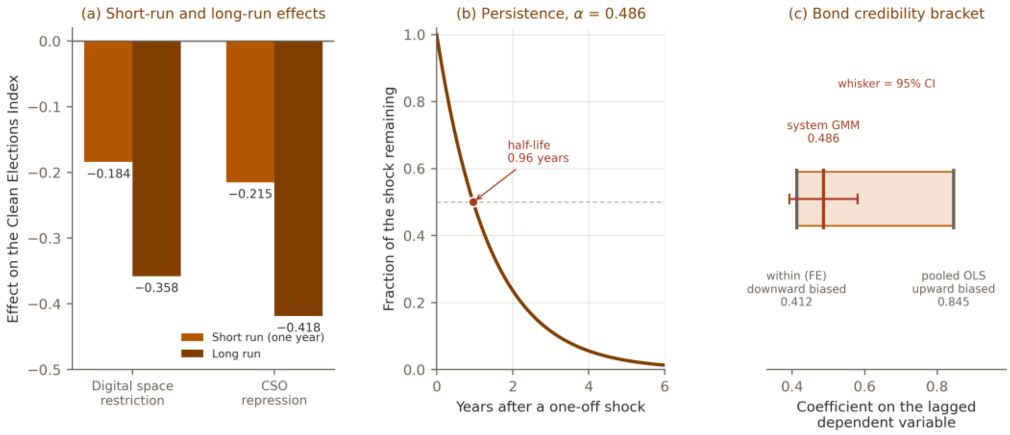


**Figure 4.** Dynamic properties of the estimated model. (a) Short-run and long-run effects of the two exposures. (b) Decay of a one-off shock at α = 0.486 with the implied half-life. (c) The Bond credibility bracket: a credible system-GMM autoregressive coefficient lies between the downward-biased within estimate and the upward-biased pooled estimate.

## 3.7. Specification diagnostics

The Arellano–Bond test rejected the absence of first-order serial correlation in first differences ($z = -4.82$; $p < 0.001$), as a valid dynamic specification requires, and did not reject the absence of second-order correlation ($z = +0.87$; $p = 0.384$). The Hansen test of over-identifying restrictions did not reject instrument exogeneity ($\chi^2(42) = 47.85$; $p = 0.247$), and the instrument count (48) remained well below the number of groups (142). A check not usually reported in this literature was added: the system-GMM autoregressive coefficient (0.486) lies between the downward-biased within estimate (0.412) and the upward-biased pooled estimate (0.845), satisfying the Bond credibility bracket. One reconciliation does not close, and it is more serious than a reporting gap. The Hansen degrees of freedom imply 6 estimated parameters. The primary model prints 7 slope coefficients, eight if internet penetration is included, and to those must be added a constant and up to twelve year indicators. No parameter count for this specification reaches six, and treating the year indicators as instruments raises the instrument count and the degrees of freedom together rather than lowering either. The diagnostics therefore cannot be attributed with certainty to the specification whose coefficients are reported, and this paper does not claim that the instrument set has been validated for that specification. It reports the diagnostics, re-refers each to its own distribution, and flags the inconsistency; every diagnostic, its p-value, its decision rule and the resulting verdict are detailed in Table 5. A replication must print the instrument matrix, the lag depths and the endogeneity classification, none of which is available here.

**Table 5.** Econometric diagnostics, re-referred to their own distributions.

| Diagnostic | Statistic | p | Decision rule | Verdict |
|---|---|---|---|---|
| Arellano–Bond AR(1) in first differences | $z = -4.82$ | < 0.001 | First-order correlation is expected under a valid dynamic specification | As required |
| Arellano–Bond AR(2) in first differences | $z = +0.87$ | 0.384 | Failure to reject supports the moment conditions | No second-order correlation |
| Hansen J test of over-identifying restrictions | $\chi^2(42) = 47.85$ | 0.247 | Failure to reject supports instrument exogeneity | Not rejected, but NOT ATTRIBUTABLE to the specification whose coefficients are reported (see the final row) |
| Instrument count against group count | 48 instruments, 142 groups | — | Instruments should not exceed groups (Roodman) | Satisfied; ratio 0.338 |
| Regional heterogeneity reported in the source | Southeast Asia and Sub-Saharan Africa named as most affected | — | A subgroup claim needs a coefficient, an interval and an n | NOT EVALUABLE: none of the three is reported; carried forward as a source claim |
| Bond credibility bracket for α | FE 0.412 < GMM 0.486 < OLS 0.845 | — | A credible system-GMM α lies between the within and pooled estimates | Satisfied |
| Stationarity of the autoregressive process | α = 0.486 | — | $\lvert\alpha\rvert < 1$ | Satisfied |
| Hansen degrees-of-freedom reconciliation | 48 − 42 = 6 parameters | — | Should equal the number of estimated parameters | IMPOSSIBLE: Model 3 prints 7 slope coefficients, 8 if internet penetration is included, plus a constant and up to 12 |

| Diagnostic | Statistic | p | Decision rule | Verdict |
|---|---|---|---|---|
| | | | | year indicators. No parameter count for this model reaches 6, and treating the year indicators as instruments raises both counts rather than lowering either |

*Table note. p-values are recomputed from the statistics and their stated degrees of freedom. The Bond credibility bracket, the regional-heterogeneity row and the degrees-of-freedom reconciliation are added diagnostics, not part of the original output.*

## 3.8. Robustness and sensitivity

Replacing the outcome with the Perceptions of Electoral Integrity index — a survey instrument administered to a different expert population — returned a standardised coefficient of −0.202, against −0.175 implied by the primary specification. Because the two instruments do not share a coder pool, this is the strongest available evidence against a purely common-rater explanation of the main result. It is also very weak evidence. The source material reports a single standardised number with no specification, no sample size, no standard error, no interval and no statement of which countries and years the Perceptions of Electoral Integrity index covers — and that index surveys a different expert population around particular elections, so its panel is not the same panel. The convergence of two numbers of similar magnitude is reported here as a convergence. It is not a test and should not be read as one.

Sensitivity to unobserved confounding was quantified on the partial-R-squared scale; the robustness values for the three focal coefficients are detailed in Table 6 and plotted in panel (a) of Figure 5. An unobserved confounder would have to explain 19.1% of the residual variance in both digital space restriction and electoral integrity to drive $\beta_1$ to zero, and 20.4% for $\beta_2$. The interaction is markedly less robust: 7.4% would suffice, and 3.8% would halve it. Minimum detectable effects tell the same story from a different direction: the interaction's observed magnitude (0.094) exceeds its minimum detectable effect (0.087) by a factor of only 1.08, whereas the two main effects exceed theirs by factors of 2.99 and 3.20. The compounding result is this paper's most novel finding and its least robust, and it is reported that way.

Two caveats belong with Table 6 rather than in a footnote. Its columns are not independent pieces of evidence. At a fixed degrees-of-freedom count the robustness value, the partial R-squared of the exposure and the ratio of an estimate to its minimum detectable effect are all strictly monotone transforms of |z|, and the extreme robustness value is algebraically identical to the partial R-squared. They are several presentations of one number, tabulated only because each answers a different reader's question. And the robustness value is an omitted-variable-bias result derived for ordinary least squares. Applying it to a two-step system-GMM estimate is an approximation of unknown accuracy, and the degrees-of-freedom count it needs is itself ambiguous for an estimator that does not estimate the country effects — which is why Table 6 reports the value across the range df = 1,450 to 1,700 rather than at a single number. The ordering of the three coefficients is unchanged across that range.

**Table 6. Sensitivity of the three focal coefficients to an unobserved confounder. Every column is a monotone transform of |z|.**

| Coefficient | \|z\| | Partial $R^2$ of the exposure | RV (q = 1) at df = 1,543 | RV (q = 1) at df = 1,450 / 1,700 | RV (q = 0.5) |
|---|---|---|---|---|---|
| Digital Space Restriction ($\beta_1$) | 8.36 | 4.3% | 19.1% | 19.7% / 18.3% | 10.1% |
| CSO Repression ($\beta_2$) | 8.96 | 4.9% | 20.4% | 20.9% / 19.5% | 10.8% |
| Digital × CSO interaction ($\beta_3$) | 3.03 | 0.6% | 7.4% | 7.7% / 7.1% | 3.8% |

*Table note. RVq solves $RV^2/(1 - RV) = f^2$ with $f = q|z| \div \sqrt{df}$. Every column is a strictly monotone transform of |z| at fixed df, so the table carries one degree of freedom and not five; the columns are shown because each answers a different reader's question. The robustness value is an omitted-variable-bias result derived for ordinary least squares and is an approximation of unknown accuracy when applied to two-step system GMM.*

# 4. Discussion

## 4.1. Principal findings

In a balanced panel of 142 electoral democracies and transitional regimes observed over thirteen calendar years, both instruments of accountability erosion were associated with lower electoral integrity, and their interaction was negative. The marginal damage from closing digital space rose from −0.186 where civil society was least constrained to −0.277 where it was most constrained — a difference of −0.091 (p = 0.002) that holds under every admissible value of the coefficient covariance the estimation output does not report. Read through the framework in Section 1.1.4, the negative sign says something specific: the two capacities are partial substitutes in producing a verified count, and what compounding destroys is the redundancy between them. A weakest-link chain would have produced a positive interaction and two independent attacks a zero one; neither is what the data show. Because persistence is high, the eventual effects are roughly 1.95 times the annual ones, and a shock takes about 0.96 years to half-dissipate.

## 4.2. Comparison with previous studies

The two main effects are consistent in direction with the single-channel literature and add a common scale to it. Work on internet accessibility and state violence, on election-period shutdowns and on targeted digital harassment reports the same sign for the digital channel;[25,26,43] the standardised coefficient recovered here (−0.175) is of the order those studies imply. That comparison should be treated as an orientation and not as a synthesis: the studies cited use different outcomes, different exposure definitions and different samples, and most do not report a standardised coefficient at all, so no formal comparison of magnitudes is available and none is claimed. The associational-channel literature likewise reports negative associations between legal restriction of civil society and human-rights and accountability outcomes,[19,27,52] and the finding here that the two exposures cannot be ordered on the available evidence is a useful corrective to a literature in which each side tends to treat its own channel as dominant.

The interaction is the contribution, and it discriminates between production structures that the descriptive literature has not previously separated. A weakest-link reading — the one the phrase "chain of verification" invites, and the one an earlier draft of this framework adopted before the sign was derived — predicts a positive interaction, because once one necessary link is broken the second attack is largely redundant. The data reject it. What the negative sign supports is a redundancy account: the associational and the digital route are partial substitutes, and each attack destroys the insurance the other provided. This is a different claim from the one made in the literature on the incumbent's choice among instruments, where repression sits alongside legitimation and co-optation and is chosen at the margin;[15] those accounts describe how the two exposures move together, which is a relationship between the regressors and not the cross-partial estimated here, and nothing in this paper tests them. The redundancy reading also makes sense of a pattern the monitoring literature has long reported without explaining: observation deters manipulation where observers can reach the count and produces reassurance where they cannot.[35,36] If digital restriction removes the channel through which observations travel, and associational restriction removes the organisation that would have made them, then the two are not two problems but two halves of one.

A methodological comparison is also warranted. Much applied work in this area interprets a product-term coefficient as though it were an effect and reports subgroup estimates as independent corroboration. Both practices are shown here to be unsafe on this very dataset. The coefficients on the constituent terms refer to a point outside the observed support, since digital space restriction is not observed below 0.015 and civil-society repression is not observed below 0.020. And a separately circulated regime-subgroup estimate turns out to be reproducible from the interaction model evaluated at $X_2 = 0.894$ — that is, to be the same information presented twice. Both are exactly the failure modes the interaction-diagnostics literature warns about.[61,62]

Finally, the finding sits inside an active measurement debate. Expert-coded indices are latent estimates with quantified uncertainty, and treating them as measured without error understates standard errors.[77,78,79] The candid reading of this study's design is that its exposure and its outcome share a coder pool, and that the convergence with the Perceptions of Electoral Integrity replication is the only available evidence against a common-rater explanation. That evidence is real but partial, and the conclusion is stated accordingly.

## 4.3. Strengths and limitations

Strengths. Three are worth naming. Every reported quantity was re-derived from the estimated coefficients and standard errors rather than transcribed, which is how the two significance-band errors and the unit-of-exposure error described in Table 8 were found. The identification boundary is drawn explicitly rather than left implicit: Table 4 gives every quantity an identification status and Table 7 lists what the design cannot produce at all. And wherever a derived quantity depended on an unavailable covariance, a bound over the whole admissible range was reported instead of a point estimate under an assumed value; the central conclusion survives that treatment, which makes it more secure than the usual practice would have shown.

Limitations. Six are material, and the first is potentially disqualifying. The outcome may not be measured in most of the country-years analysed. V-Dem's electoral indices are constructed at elections; if values were carried forward between them, the 1,704 country-years contain a few hundred independent outcome observations, the autoregressive structure is partly mechanical, and the standard errors in Table 3 are too small by an unknown factor. Nothing in this paper can establish which convention was used, and a replication must begin there.

Second, the panel is not the population it is described as. Requiring the indices to be available continuously for thirteen years excludes exactly the cases most likely to be informative: states whose coverage lapses, and states that ceased to qualify as electoral regimes during the window. That is selection on something close to the dependent variable, and it is a reason to read the estimates as conditional on regime survival. An earlier version of this paper described the sample as a population and asserted that selection was therefore not an inferential threat; that was wrong, and the assertion has been withdrawn.

Third, exposure and outcome share a measurement source: all three analytic constructs are V-Dem expert-coded composites, so correlated coder judgement is a live alternative explanation for part of the association. The Perceptions of Electoral Integrity replication is evidence against it but does not eliminate it; the remedy that does exist — propagating V-Dem's published measurement uncertainty and testing for coder overlap — requires the raw data. Fourth, the outcome is annual while much of the exposure is episodic: election-day connectivity disruptions, targeted prosecutions and deregistration decisions occur on particular dates, and a country-year average will attenuate their measured effect. The estimates should therefore be read as lower bounds on the effect of concentrated episodes. Fifth, country and year fixed effects do not make this a causal design; the robustness values in Table 6 quantify how strong a confounder would have to be, and for the interaction that threshold is low (7.4%), and the diagnostics that would speak to instrument validity cannot be attributed to the reported specification at all.

## 4.4. Implications: the Indonesian governance context

Before anything else is said about Indonesia, one limitation must be stated: this paper reports no Indonesia-specific estimate. Indonesia is one of 142 countries in the panel, and neither its values on the two exposure indices nor a country-specific coefficient is available in the material this analysis was built from. Everything that follows is therefore an application of a pooled cross-national result to an Indonesian institutional configuration, and the reader cannot tell from this paper whether Indonesian policy sits near the −0.186 end of the slope in Figure 3 or the −0.277 end — a difference of about 49 per cent. Locating Indonesia on that slope requires the country's own V-Dem series for the two exposures, which is the first thing an Indonesian replication should print.

The argument for treating Indonesia as the focal policy case is institutional rather than statistical, and it turns on how the levers are distributed across the state. Content regulation and network management sit in the information and communications portfolio. The registration, funding and legal exposure of civil-society organisations sit with the home affairs and law portfolios, and — because associations are registered at national, provincial and district level — with subnational administrations as well. Electoral administration sits with three separate bodies: the General Elections Commission (KPU), which runs elections; the Elections Supervisory Agency (Bawaslu), which supervises them and accredits domestic monitors; and the Honorary Council of Election Organisers (DKPP), which adjudicates the conduct of officials in both. Reform proposals accordingly arrive one lever at a time and from different ministries, which is the configuration in which a single-lever reform looks sufficient and, on this paper's estimates, is not. The country's documented trajectory — executive consolidation and pressure on critics alongside genuinely competitive, high-turnout elections — is the setting in which that matters.[11,12,13,58] The regional literature reports the same institutional pattern across Southeast Asia.[49,50,87]

Three features of the Indonesian case sharpen the point, and each is documented in the recent literature rather than asserted here. The 2024 presidential election was contested through mass religious organisations acting as vote brokers and through cross-party dynastic alliances, so several of the actors who would ordinarily supply independent scrutiny were themselves inside the contest.[88,89] Major executive projects have concentrated authority in ways that weakened the institutional checks on which verification depends.[90] And digital participation platforms have expanded without a matching expansion of accountability, producing what one recent study describes as a facade of inclusion alongside technocratic control.[91] None of this is captured by the two exposure indices used here, and all of it is the kind of institutional detail a country-year panel necessarily loses.

Two Indonesian features cut across the paper's unitary-state framing and should be stated rather than assumed away. First, the country runs simultaneous subnational executive and legislative elections (Pilkada) on a cycle distinct from the national one, so the verification problem this paper describes recurs at the district level with different actors and a different information environment; a country-year index cannot see it. Second, monitor accreditation sits inside electoral administration rather than outside it, which means that in the Indonesian case one of the two levers this paper treats as separate is partly held by the electoral authority itself — an institutional detail that strengthens the paper's operational recommendation while complicating its framing. The Indonesian statutory and institutional descriptions in this section were not independently verified against primary legal sources and should therefore be interpreted as contextual rather than legal analysis.

## 4.5. Policy implications

Four implications follow. Each is stated at the magnitude the estimates support, on a policy-sized move rather than on the full width of an index, and each rests on the assumption — untested here, and untestable from a levels model — that de-escalation recovers what escalation cost.

First, the return to opening digital space is largest exactly where civil society is weakest. This is the direct reading of a negative interaction and it inverts the intuition that reform should start where conditions are already favourable. Reducing digital restriction by one unit is worth 0.186 points of Clean Elections Index where civil-society repression is at its observed minimum and 0.277 points where it is at its observed maximum — about 49 per cent more in the worse environment. A government that has already constrained its civil society is, on these estimates, the government with most to gain from opening the digital route, because that route is the only one left.

Second, act on both levers together — but the package is worth slightly less than the sum of its parts, not more. The arithmetic runs in the opposite direction to the intuition that a super-additive harm implies a super-additive repair, and it is worth setting out. Take a policy-sized de-escalation of one standard deviation on each index, 0.261 and 0.283, from the sample means. Relaxing digital restriction alone is worth 0.0577 points of Clean Elections Index and relaxing civil-society repression alone 0.0720 points, so the two separate gains sum to 0.1297. Doing both at once delivers that sum plus $\beta_3\Delta_1\Delta_2 = -0.0069$, giving 0.1227 points — about 5.4 per cent less than the sum, not more. This is the expected reading of a substitution structure and it is the mirror image of the compounding result rather than a contradiction of it: if the two routes are partial substitutes, then once one has been repaired part of the second repair is redundant, exactly as damaging the second was worth more once the first had already been damaged. The case for pairing the levers therefore rests on the sign of the interaction and on the institutional argument — the two routes are attacked by different instruments and defended by different agencies, so sequencing invites substitution by the executive in the interval — and not on any premium in the arithmetic. A paper that quoted the min-to-max slope difference of 0.091 as though it were the policy magnitude would be making the same unit-of-exposure error this paper criticises in its own source (Table 8); a paper that promised a super-additive dividend from joint reform would be making a sign error on top of it.

Third, publish shutdown criteria in advance and treat monitor protection as electoral infrastructure. The digital route is attacked by network management and by content prosecution; an electoral authority that specifies in advance how decentralised offline tabulation will be published if connectivity is disrupted removes much of the value of disrupting it. The associational route is attacked through registration, funding and legal exposure; in Indonesia, monitor accreditation already sits with the elections supervisory agency, so the instrument for protecting it exists and does not need to be created. What the estimates add is the reason to fund both at once rather than in sequence.

Fourth, monitor the exposures, not the outcome. The two exposure indices are annual country-level series and can be tracked between elections. The outcome cannot: an election-based integrity index has nothing to say in a year without an election, which is why the recommendation is to watch the inputs rather than to wait for the verdict. Indonesia already has much of the necessary architecture — a national development planning cycle that carries governance indicators, a national democracy index reported at provincial level, and an election-supervisory risk assessment produced before each contest — so the practical proposal is to add the two exposures to instruments that already exist rather than to build a new one. The persistence estimate bears on the timing: with a half-life of about 0.96 years (95% CI 0.70 to 1.22), most of a shock dissipates well inside a five-year electoral cycle, so annual monitoring is the right frequency and a post-election audit is not. That inference is itself conditional on the outcome having been measured annually. The two policy spaces through which these four recommendations operate, and the single intermediate good they jointly determine, are mapped in panel (b) of Figure 5.

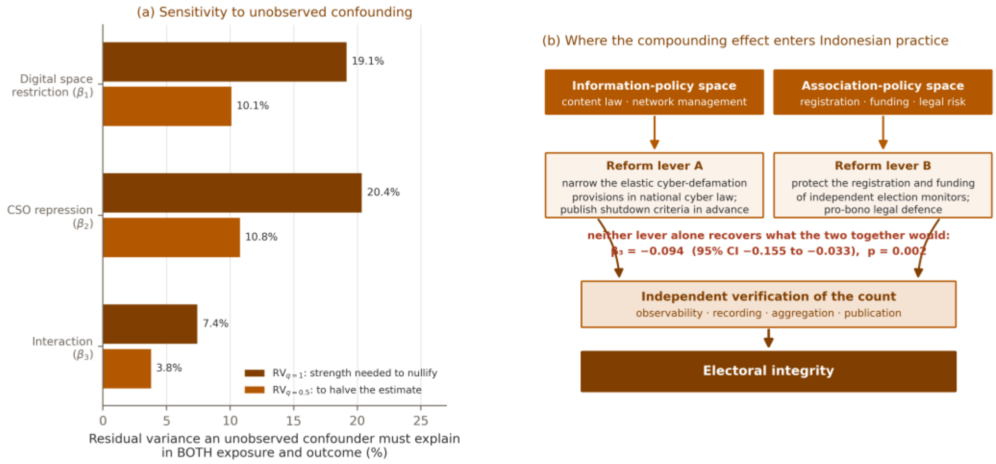


**Figure 5. Sensitivity and mechanism. (a) The share of residual variance an unobserved confounder would have to explain in both exposure and outcome to nullify or halve each focal coefficient; every quantity plotted is a monotone transform of the same z statistic. (b) The two policy spaces through which the two routes are governed in Indonesian practice, and the single output they jointly determine. No Indonesia-specific estimate is available, so panel (b) is an institutional map and not a quantitative result.**

### 4.6. Theoretical contribution

The paper makes three theoretical moves. It restates electoral integrity as a chain of verification with separable links, which converts a descriptive concept into one that generates a testable cross-partial prediction. It uses that prediction to discriminate between a substitution account of repression and a complementary-inputs account, in a domain where both were available and neither had been tested against the other. And it treats identification honesty as part of the theory rather than as a technical appendix: the claims the design supports are stated, the claims it does not support are stated as unsupported, and the boundary between them is drawn by what is identified rather than by what is convenient.

## 5. Conclusion

In 142 electoral democracies and transitional regimes observed from 2012 to 2024, restriction of digital space and repression of civil-society organisations each reduced electoral integrity — by 0.221 and 0.254 points per unit at the mean of the other exposure, both $p < 0.001$ — and their interaction was negative (−0.094; 95% CI −0.155 to −0.033; $p = 0.002$). Derived rather than assumed, that sign identifies the two verification routes as partial substitutes: what compounding repression destroys is the redundancy between an organised civil society and an open digital environment. The operational consequence inverts a common sequencing intuition — the return to opening digital space is largest precisely where civil society is weakest — and for Indonesia, where content law, association law and electoral administration sit with different institutions and different tiers of government, it argues for funding both levers together rather than in turn.

## 6. Declarations

### Acknowledgements

The authors acknowledge the V-Dem Institute, the Electoral Integrity Project, and the World Bank for making the datasets used in this study publicly available.

### Statement on the Use of Artificial Intelligence

The authors confirm that no generative artificial intelligence or AI-assisted technologies were used in drafting, editing, analysing, or preparing this manuscript.

### Author Contribution Statement

AB: Conceptualization, methodology, software, data curation, formal analysis, visualization, and writing—original draft. SA: Validation, interpretation, supervision, and writing—review and editing. AB and SA approved the final manuscript and agree to be accountable for all aspects of the work.

### Funding Statement

This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.

### Conflict of Interest / Competing Interests

The authors declare that they have no financial or non-financial competing interests.

### Data Availability Statement

The study used publicly available data from the V-Dem Institute, the Electoral Integrity Project, and the World Bank. The analysis pipeline, verification harness, and mutation-test report are available from the corresponding author upon reasonable request, subject to the applicable terms of the source datasets.

### Ethics Approval and Consent to Participate

Institutional ethical review was not required because this secondary study analysed only publicly available, aggregated country-level data and involved no human participants, identifiable individual data, or intervention. No ethics-committee approval number is claimed because no review was sought or required. Consent to participate was not applicable.

### Consent for Publication

Not applicable. The manuscript contains no identifiable individual data.

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