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# The Social Compression Hypothesis

*A Pre-Registered Test of Coordination Demand, Institutional Capacity, and Instability in Historical and Modern Societies, with a Formal Framework and Agent-Based Experiments*

Simon-Pierre Boucher

Département des sciences administratives
Université du Québec en Outaouais

simon-pierre.boucher@uqo.ca

Gatineau – Pavillon Alexandre-Taché
283, boulevard Alexandre-Taché
Gatineau, Québec, Canada J9A 1L8

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**Abstract**

Institutions, standards, records and administrative hierarchies can be read as devices that lower the communication cost of coordinating many people: a form of compression. This paper states that reading formally, derives what it does and does not imply, and tests its empirical corollary—that a mismatch between a society's coordination demand (population, territory, settlement structure) and its coordination capacity (administration, standardized rules and money, communication infrastructure, record-keeping) predicts subsequent instability—under a frozen pre-registration. We build demand, capacity and mismatch indices from the Seshat Polaris-2026 databank (626 polities; 127 CrisisDB crisis records with pre-crisis predictors), the Ethnographic Atlas (1,291 societies) and a V-Dem–Maddison country-year panel (14,637 country-years, 2,692 instability events, 1820–2025), with a transparent block index and a latent-variable alternative, uncertainty propagation and out-of-sample validation. Capacity and scale are positively associated in all three datasets ([EMPIRICAL]). The mismatch index is positively associated with crisis severity in Seshat (0.20 log-odds per SD, stable across regions and eras) and with instability onsets in modern states (0.40 per SD), but by the pre-registered criterion it fails: it does not beat the scale-only baseline out of sample in Seshat, nor the capacity-only baseline in the modern panel; a placebo index performs comparably; the modern association is as strong with past as with future events; and an Oster adjustment reverses its sign. No threshold is found in Seshat and only a negligible nonlinearity in the modern panel. Five theorems make the compression reading precise (coding gain, break-even scale, stale-code penalty, hierarchical routing, backlog dynamics), and factorial agent-based experiments (6,048 runs, five imposed institutional regimes) confirm them in silico while locating the regimes where institutions hurt: unpaid set-up costs, saturated hierarchies, and weakly structured task environments ([SIMULATED]). We conclude that the compression framework is coherent and simulable, that scale and capacity each matter, but that their mismatch, as measurable in existing historical data, carries no identifiable predictive content of its own. The universal theory is not supported; the narrower results are stated with their scope.

**Keywords:** institutions, coordination, state capacity, social complexity, Seshat, pre-registration, agent-based simulation, information theory

**JEL Classification:** D02, D83, H11, N40, P48, C63

## 1. Introduction

Large societies coordinate. Taxes are assessed and collected, grain is stored and moved, disputes are settled, armies are raised, and the same unit of length is understood in two towns that have never exchanged a messenger. Each of these acts requires that people who do not know each other act on a shared description of the situation. The description has to be communicated, verified and remembered, and the cost of doing so grows with the number of people, the distances between them and the variety of things that must be agreed upon. The claim examined in this paper is that institutions—standardized rules, money, measurement systems, written records, administrative hierarchies, postal systems—are best understood as devices that lower that cost per coordinated task, in the ordinary sense in which a dictionary or a code lowers the cost of saying things: they compress. We call this reading the *Social Compression Hypothesis* (SCH). Its empirical corollary, which gives it teeth, is that a society whose coordination *demand* outruns its coordination *capacity* should be more likely to experience institutional breakdown.

The idea has a long ancestry, which Section 2 documents. Scalar stress (Johnson, 1982), administrative hierarchies as information-processing structures (WRIGHT and JOHNSON, 1975), the declining marginal returns to complexity (Tainter, 1988), organizational codes (Arrow, 1974; Cremer et al., 2007) and the informational foundations of state capacity (Lee and Zhang, 2017; Brambor et al., 2019) all contain pieces of it. The closest precursor is Shin et al. (2020), who found in the Seshat databank a second axis of variation opposing scale to information-processing characteristics and wrote, explicitly as a conjecture left for future research, that a "mismatch among aspects of social development should be considered as a generic cause of instability". To our knowledge, no one has operationalized a demand–capacity mismatch, linked it to an independently coded instability outcome, and tested it with out-of-sample validation. That is what this paper does, and the answer it reaches is mostly negative.

We proceed in the order a skeptical reader would want. First, we make the compression reading precise. Five theorems (Section 3) state when a shared code lowers expected communication per coordinated task (Shannon's source-coding bound), when the fixed cost of an institution is repaid (a break-even condition in scale and task intensity), what a code optimized for yesterday's tasks costs today (the Kullback–Leibler divergence), what hierarchical routing buys and what it concentrates at the apex, and how a backlog behaves when demand exceeds capacity (Lindley's recursion). None of these is new mathematics; their value is that they say exactly what a "mismatch" is in the model—the utilization $\rho = D/K$—and what would have to be true of historical proxies for an index such as $P = z(\log D) - z(K)$ to track it. We state that requirement as a conjecture, not a result.

Second, we pre-registered the empirical tests before computing any association between predictors and outcomes (the frozen document and its hash are in the replication package). The

pre-registration fixes the datasets, the unit of analysis, disjoint indicator sets for demand $D$ and capacity $K$, the outcomes, the horizon, the models, the baselines that $P$ must beat out of sample, the multiple-testing correction, a power calculation, the robustness set and the decision rule for abandoning the universal framing. We then built the indices from three real sources with full provenance: the Seshat Polaris-2026 release (626 polities; 56,274 facts, of which only 2,503 are dated; a CrisisDB table of crisis consequences for 150 polities) (Turchin et al., 2015; Hoyer et al., 2023), the D-PLACE Ethnographic Atlas v3.3 (Kirby et al., 2016; Murdock, 1967), and a V-Dem v16 country-year panel (Coppedge et al., 2020) merged with Maddison 2023 population (Bolt and van Zanden, 2024). Measurement is done twice, with a transparent block index and a two-parameter latent-variable model, and uncertainty in ranges and dates is carried through.

Third, we report what the pre-registered tests found (Sections 6 and 7). Capacity and scale are positively associated in every dataset, as the framework and much prior work predict, although the direction is not identified. The mismatch index $P$ is positively associated with how badly a recorded crisis unfolds in Seshat (about 0.20 log-odds per standard deviation, stable when any macro-region or era is held out) and with the probability of a civil war, coup or irregular regime end in the following five years in modern states (about 0.40 per standard deviation). But the pre-registration asked a harder question than whether $\beta_P$ is positive: it asked whether $P$ beats the obvious baselines out of sample. It does not. In Seshat, a scale-only model does as well, a placebo index that replaces capacity with production infrastructure does as well, and $P$ loses to scale alone when regions are held out. In the modern panel, a capacity-only model does as well or better in every forward-chaining and leave-one-region-out comparison, past instability is associated with $P$ as strongly as future instability, and an Oster-type adjustment for unobservables reverses the sign. We find no threshold in Seshat and a nonlinearity of negligible size in the modern panel. Polity duration and discontinuous succession are unrelated to $P$. Under the decision rule we wrote down in advance, the universal framing is not supported: scale and capacity each carry signal, their difference carries none that we can identify.

Fourth, we report the constructive part. Factorial agent-based experiments with five *imposed* institutional regimes (Section 8; 6,048 main runs, 584 ablation runs, all labelled [SIMULATED]) measure messages, bits, success rates, latency, overhead, welfare and inequality directly. Shared codes and records lower the operating cost per successful task in essentially every cell and raise success by eliminating decoding errors between heterogeneous agents; the stale-code penalty after a distribution shock equals the divergence to within the rounding of Theorem 3; hierarchies fail abruptly when relayed load exceeds administrative capacity, and federated routing keeps most of the benefit at half the capacity. The same experiments locate where institutions hurt: a learning cost of 200 bits per agent is repaid only with a large and concentrated task space and a long horizon, saturated centralized hierarchies do worse than no institution at all, and a code can be longer than

raw description when the task distribution is nearly uniform. These are the cases the framework predicts, and they are the cases historians describe.

The contribution, then, is threefold. We give the compression reading of institutions a formal statement that distinguishes what is proved from what is conjectured. We provide the first pre-registered, out-of-sample test of the demand–capacity mismatch conjecture on real historical and modern data, with a negative result that we think is more useful than another positive one would have been, because it shows that the mismatch, as it can be measured today, is a residual of two collinear quantities. And we provide a simulation testbed in which the mechanisms that the historical record cannot reveal—message counts, code lengths, queue saturation—can be measured. Section 9 states the scope conditions that survive, Section 10 the limitations, and Section 11 what a credible next test would require.

## 2. Related work and contribution

Every reference in this section was checked against Crossref by DOI or bibliographic query on 1 November 2026 (`research/prior_art.csv`, `research/bib_overrides.csv`); the abstracts and key results of the eighteen works closest to our hypothesis were read from primary or repository sources in a separate verification pass (`docs/novelty_search_report.md`). Works that we know only through reviews or citing articles are flagged in footnotes and are cited for their general thesis only.

### 2.1. Scalar stress and information-processing hierarchies

The oldest form of our idea is Johnson's *scalar stress*:[1] as the number of units that must be coordinated grows, the communication and decision load grows faster than linearly, and groups respond either by fission or by adding a hierarchical level, with a modal span of about six units per level (Johnson, 1982). Alberti (2014) estimate a related fission threshold on Hutterite colonies. WRIGHT and JOHNSON (1975) define the state as a specialized administrative hierarchy with at least three decision-making levels and reject population growth and exchange as single sufficient causes of its emergence in Susiana; Johnson (1978) extends the information-processing reading of decision-making organizations. Carneiro (1967) documents the log-log regularity between population and the number of organizational traits, NAROLL (1956) proposes an early index of social development, Feinman (2010) reviews the size–complexity relation, and Flannery (1972) frames institutional evolution as a change in information-processing architecture. None of this strand builds a demand/capacity *ratio*, and none tests it against instability.

[1] Known to us through library records and through Alberti (2014); the chapter itself was not read.

### 2.2. Declining returns and collapse

Tainter (1988) argues that complexity is a problem-solving investment with declining marginal returns, so that collapse (simplification) eventually becomes rational and the society loses its capacity to absorb shocks.[2] Middleton (2017), Butzer (2012) and Cline (2014) review the collapse literature and warn against mono-causal accounts. Structural-demographic theory (Turchin, 2004; Turchin and Nefedov, 2009; Turchin, 2023) models secular cycles driven by population pressure, elite overproduction and fiscal distress; Goldstone (1991) is its historical origin. Sng (2014) is the closest economic precedent: in late-imperial China the cost of transmitting information over distance creates a principal–agent problem whose severity rises with territorial size, so that fiscal capacity decays during the prosperous eighteenth century—a demand (territory) versus capacity (monitoring) argument, tested within a single polity with prefecture-level data. Using the Seshat CrisisDB, Hoyer et al. (2024) show that crisis consequences are heterogeneous, largely uncorrelated with one another, and not well predicted by territory, religion or administrative complexity taken one at a time ("no typical societal crisis").

### 2.3. Seshat-based quantitative history

The Seshat Global History Databank (Turchin et al., 2015; François et al., 2016) underlies a growing quantitative literature. Turchin et al. (2017) found that nine complexity characteristics (population, territory, capital population, hierarchical levels, government, infrastructure, writing, texts, money) load on a single principal component explaining about three quarters of the variance across 414 polity-centuries. Shin et al. (2020) decomposed the remaining variance and identified a second axis opposing *scale* characteristics to *information-processing* characteristics, defined a "scale threshold" and an "information threshold" along the first component, and wrote—explicitly as a conjecture left for future research—that "mismatch among aspects of social development should be considered as a generic cause of instability". Miranda and Freeman (2020) clustered Seshat polities into two super-types with an unstable transitional cluster in which polities dwell only a few centuries. Turchin et al. (2022) compared more than 100,000 predictor combinations for the *growth* of complexity and found agricultural productivity interacting with military technology best supported; they did not model instability. Related Seshat-based work includes Currie et al. (2010) on the rise and fall of political complexity in Austronesia, Sheehan et al. (2018) on the coevolution of intensive agriculture and hierarchy, Kohler et al. (2017); Kohler and Smith, eds (2018) on long-run inequality, Peregrine (2017, 2018) on resilience to disasters, and Hoyer et al. (2023) on the long-run factors that shape responses to changing climate. Whitehouse et al. (2019) was retracted (Whitehouse et al., 2021) after Beheim et al. (2021) showed that recoding missing moralizing-god data as "absent"

[2] Content known from reviews and secondary sources; the book was not re-read for this paper.

drove the result; the Seshat team's "retake" (Whitehouse et al., 2022) reports qualitatively similar conclusions with corrected coding, and Slingerland et al. (2020) record the historians' objections. The methodological lesson—never treat unknown as absent, never forward-fill, report results on extant data—is written into our pre-registration.

### 2.4. State capacity and its informational foundations

Mann (1984, 1986) distinguish despotic from *infrastructural* power; Soifer (2008) and Hanson and Sigman (2021) discuss its measurement, the latter with a Bayesian latent model over 21 indicators for 1960–2015. Scott (1998) describes *legibility*—standardized names, cadastres, censuses, weights and measures—as the state's way of making society readable, and SCOTT (2017) traces it to the earliest states. Lee and Zhang (2017) operationalize legibility through age-heaping in censuses and show that it predicts tax contributions; Brambor et al. (2019) build an "information capacity" index (census, statistical agency, yearbooks, civil and population registers) for 85 polities since 1789; D'Arcy and Nistotskaya (2016) use cadastral records. Besley and Persson (2009, 2011), Dincecco (2017), Johnson and Koyama (2017), Mayshar et al. (2022), Borcan et al. (2017) and Acemoglu et al. (2001) study the origins and consequences of fiscal and legal capacity; Gennaioli and Voth (2015) link capacity to military competition, as do Tilly (1992), McNeill (1982) and HOFFMAN (2015). STASAVAGE (2010) shows that geographic scale limited representative assemblies in Europe—a direct instance of distance-driven coordination cost—and Stasavage (2020); AHMED and STASAVAGE (2020) and BLAYDES and CHANEY (2013) trace the long-run divergence of political institutions; Ko et al. (2018), Fernández-Villaverde et al. (2023), SCHEIDEL (2019) and Dincecco and Wang (2018) compare China and Europe. Classic comparative treatments of empires and bureaucracy are Weber (1978), Eisenstadt (1963), Trigger (2003), Yoffee (2005), Blanton and Fargher (2008), Fukuyama (2011) and Graeber and Wengrow (2021). None of these operationalize a *ratio* of coordination demand to capacity, nor test it against instability across pre-modern polities.

### 2.5. Transaction costs, codes and communication in organizations

Coase (1937), Williamson (1985) and North (1990, 1991) treat institutions as devices that economize on transaction costs; Hayek (1945) on dispersed knowledge, Simon (1955) on bounded rationality and Simon (1962) on near-decomposable hierarchies are the classic information-processing views. Arrow (1974) introduced organizational *codes*;[3] Cremer et al. (2007) model the firm's language as a code of bounded size in which precise words go to frequent events—literally lossy compression that trades scope against precision. Marschak and Radner (1972), Radner (1993), Bolton and Dewatripont (1994), Van Zandt (1999) and Garicano (2000) model hierarchies as information-

[3]Verified only through secondary citations.

processing networks with explicit communication costs; Dessein (2002) and Dewatripont and Tirole (2005) study authority and modes of communication; Crawford and Sobel (1982), HAGENBACH and KOESSLER (2009), Galeotti et al. (2013) and Calvó-Armengol et al. (2015) analyze strategic communication on networks. Mechanism-design results bound the *message-space size* needed to implement allocations (Mount and Reiter, 1974; Segal, 2007; Nisan and Segal, 2006), and communication complexity (Yao, 1979; Kushilevitz, 1997) gives the computer-science counterpart. The business-history literature documents the same economizing in practice: Chandler (1977), Yates (1989), Beniger (1986) and, earlier, Innis (1950). These results are the formal basis for Section 3, but they concern firms, markets or abstract protocols, not polities.

### 2.6. Coordination, common knowledge and conventions

Lewis (1969) and Schelling (1960) define conventions and focal points; Young (1993), Kandori et al. (1993) and Ellison (1993) show how conventions are selected and how local interaction speeds convergence; Morris (2000) on contagion and Skyrms (2010) on signalling complete the picture. Chwe (2000, 2001) argues that coordination requires *common knowledge* and that rituals and public communication structures exist to manufacture it cheaply; Rubinstein (1989) shows that almost-common knowledge is not enough. Morris and Shin (2002) and Angeletos and Pavan (2007) quantify the social value of public signals, and DEWAN and MYATT (2008) model leaders as coordination devices. Ostrom (1990, 2010) and Olson (1965) are the collective-action anchors; Boyd and Richerson (1985, 1988), Henrich (2004), Richerson et al. (2014) and Turchin (2016) supply the cultural-evolution mechanisms for norm maintenance in large groups, Gavrilets et al. (2008) and Turchin and Gavrilets (2009) the evolutionary dynamics of hierarchy, and Stanish (2017) the role of ritual in stateless societies; Greif (1993, 2006) and Milgrom et al. (1990) are the historical cases of reputation- and record-based enforcement.

### 2.7. Scaling and information theory

Bettencourt et al. (2007); Bettencourt (2013), Ortman et al. (2014) and Lobo et al. (2019) show superlinear scaling of socioeconomic outputs with settlement population, ancient and modern; Dunbar (1993) and Hamilton et al. (2007) document scaling constraints in small-scale societies. Scaling theory implies that interaction demand grows faster than population, but it assumes that infrastructure and institutions co-scale; it predicts benefits, not instability. Dedeo (2017) is the closest explicit statement that transitions in political order are driven by information-processing innovations and that coarse-graining (lossy compression) is the central mechanism; Wolpert et al. (2017) surveys state-space compression, and Marzen and DeDeo (2017) give a rate-distortion model with a high-fidelity regime (cost linear in environmental complexity) and a low-fidelity regime.

Flack (2012, 2017), Daniels et al. (2017) and Krakauer et al. (2020) develop coarse-graining and collective computation in animal societies; Hidalgo (2015) popularizes the information-growth view of economies. Shannon (1948) and Cover and Thomas (2005) are cited only for the definitions used in the simulations, where messages and distributions are actually defined. To our knowledge no published work uses the name "Social Compression Hypothesis" (search of 1 November 2026), but the idea has the clear ancestors listed above, which this paper credits.

### 2.8. Instability forecasting and methods

Goldstone et al. (2009) forecast instability onsets for 1955–2003 with regime type as the dominant predictor; FEARON and LAITIN (2003) and Hegre (2001) are the civil-war baselines, and CEDERMAN (2003) models the size distribution of wars. For binary time-series–cross-section data we follow Beck et al. (1998) and Carter and Signorino (2010); for event history, Cox (1972) and Box-Steffensmeier and Jones (2004); for logistic calibration, Hosmer et al. (2013); for clustered and spatial inference, Colin Cameron and Miller (2015) and Conley (1999); for sensitivity to unobservables, Oster (2017) and Cinelli and Hazlett (2019); for Galton's problem, Mace et al. (1994) and Dow (2007); for latent measurement, Bollen (1989), Samejima (1969), Treier and Jackman (2008) and Pemstein et al. (2018); for agent-based modelling, Epstein and Axtell (1996), Axtell et al. (2002), Macy and Willer (2002), Watts and Strogatz (1998) and Jackson (2008); and for pre-registration and the statistical crisis, Nosek et al. (2018) and Gelman and Loken (2014). Data sources are documented by Seshat: Global History Databank (2026), Kirby et al. (2016) and Murdock (1967), Bolt and van Zanden (2024), Coppedge et al. (2020) and Coppedge et al. (2026), and Hammarström et al. (2026).

### 2.9. Novelty matrix

Table 1 summarizes which elements of the Social Compression Hypothesis already exist in the closest works. The ranking by overlap is: Shin et al. (2020) (a verbal mismatch–instability conjecture, declared untested); Johnson (1982) (scalar stress); Sng (2014) (distance-driven information cost, one polity); Tainter (1988); the codes-as-compression micro-foundation of Arrow (1974), Cremer et al. (2007) and Dedeo (2017); Miranda and Freeman (2020); the CrisisDB results of Hoyer et al. (2024); and the settlement-scaling literature as a foil.

### 2.10. Precise contributions and non-claims

1. *Empirical.* The first pre-registered test of the scale/information mismatch conjecture of Shin et al. (2020) against an independently coded instability outcome (the Seshat CrisisDB), with a transparent and a latent measurement model, propagated uncertainty, and out-of-sample

**Table 1.** Novelty matrix: elements of the Social Compression Hypothesis in the closest prior work

| Work | Demand/scale | Capacity | Mismatch | Instability |
|---|---|---|---|---|
| Johnson (1982) | group size | hierarchy levels | implicit | fission (ethnogr.) |
| WRIGHT and JOHNSON (1975) | settlement hier. | admin. levels | – | – |
| Tainter (1988) | – | complexity | – | collapse (cases) |
| Sng (2014) | territory, distance | monitoring | implicit | fiscal decay |
| Turchin et al. (2017) | pop., territory | govt., writing | – | – |
| Shin et al. (2020) | PC2 scale side | PC2 info. side | verbal | conjectured |
| Miranda and Freeman (2020) | cluster features | cluster features | – | dwell time |
| Turchin et al. (2022) | complexity (outcome) | – | – | – |
| Hoyer et al. (2023, 2024) | territory (covariate) | admin. (covariate) | – | CrisisDB |
| Lee and Zhang (2017); Brambor et al. (2019) | – | legibility, info. capacity | – | – |
| Hanson and Sigman (2021) | – | latent capacity | – | – |
| Goldstone et al. (2009) | pop. threshold | regime type | – | onsets |
| Cremer et al. (2007); Arrow (1974) | org. scope | code size | trade-off | – |
| Chwe (2000, 2001) | network | common knowledge | – | – |
| Bettencourt (2013); Ortman et al. (2014) | settlement pop. | infrastructure | – | – |
| Dedeo (2017); Marzen and DeDeo (2017) | – | coarse-graining | – | – |
| This paper | log pop., territory, largest settlement, hierarchy | 4 disjoint blocks (mean, IRT, PC) | $P = z(\log D) - z(K)$ | CrisisDB severity; haz |

validation, replicated in form on a modern country-year panel. The result is negative in the pre-registered sense: the mismatch index $P$ is associated with crisis severity and with modern instability onsets, but it does not predict either outcome beyond its own components (scale in the historical panel, capacity in the modern panel), fails the out-of-sample rule against those baselines, and performs no better than a placebo index in the historical panel. What survives is that scale predicts crisis severity among crisis cases and that capacity predicts fewer onsets in modern states—neither of which is new, and neither of which is the Social Compression Hypothesis.

2. *Theoretical.* Formal propositions stating when a shared code or record system lowers the expected communication cost per coordinated task as a function of scale, when fixed institutional overhead makes it worse, by how much a stale code raises cost after a shock, and when hierarchies saturate; each is stated as a theorem with proof or as a conjecture, never as an empirical fact.
3. *Simulated.* A factorial agent-based comparison of five imposed institutional regimes with directly measured messages, bits, success rates and overhead, including an explicit search for regimes in which institutions reduce efficiency or add fragility (set-up costs that are never repaid, saturated hierarchies, weakly concentrated plan spaces).
4. *Not claimed.* No causal identification in either panel; no literal Shannon compression of historical institutions; no universal theory. The name "Social Compression Hypothesis" has no prior academic use that we could find, but the idea has clear ancestors (Table 1), which this paper credits, and the data do not support the universal framing under which the project was mandated.

## 3. A formal framework: institutions as codes

This section states the theory behind the Social Compression Hypothesis in a form that separates what is *proved* from what is *conjectured*. Definitions are conventions; theorems are statements proved below or standard results whose proofs are reproduced; conjectures are statements we believe worth testing, each with its falsifier. Nothing in this section is an empirical finding. The results concern an idealized communication system; Section 3.8 states what is not claimed and which pre-registered test, if any, can speak to each statement.

### 3.1. Primitives and definitions

**Definition 1** (agents, tasks, plans)**.** A society has $N$ agents. In each period, coordination tasks arrive at rate $\lambda$ per agent. A task requires that $k \geq 2$ participants settle on a common element of a finite *plan set* $S$, $|S| = s$, the required plan being drawn independently across tasks from a distribution $p$ on $S$ with Shannon entropy $H(p) = -\sum_{x \in S} p(x) \log_2 p(x) \leq \log_2 s$ (Shannon, 1948; Cover and Thomas, 2005). Communication is counted in *messages* (one transmission between two agents) and

in *bits* (the length of what is transmitted). Verification and record-keeping use a second channel whose cost is counted separately as *administrative overhead* (Definition 6).

**Definition 2** (coordination demand)**.** The coordination demand of a period is the task volume that must be settled: $D = N\lambda k$ participant-slots per period. In the worst case in which every pair of agents must be reconciled, $D$ is bounded by the number of pairs, $N(N-1)/2$, i.e. $D = \Theta(N^2)$; under bounded $\lambda$ and $k$, $D = \Theta(N)$. Either way $D$ is increasing in $N$.

**Definition 3** (coordination capacity)**.** Fix a tolerated error rate $\varepsilon \in (0,1)$. The coordination capacity $K$ of an institutional system is the maximal number of tasks it can settle per period such that the fraction of tasks settled on a wrong or inconsistent plan is at most $\varepsilon$. $K$ depends on the available communication budget (bits per period), the code in use (Section 3.2), the routing structure (Section 3.5) and the overhead (Section 3.3).

**Definition 4** (utilization)**.** $\rho = D/K$. When $D$ and $K$ are measured in the same units (tasks per period), $\log D - \log K = \log \rho$ exactly.

**Limitation (not a theorem).** The pre-registered empirical index $P = z(\log D) - z(K)$ uses standardized proxies (population, territory, settlement hierarchy for $D$; shares of administrative, legal/monetary, communication and record-keeping indicators for $K$). It is a monotone proxy for $\log \rho$ only under Conjecture 1; it is not a measurement of $\rho$, and the Seshat and V-Dem indicators are not Shannon quantities.

### 3.2. Coding gain of a shared code

**Definition 5** (code)**.** A *shared code* is an injective map $c : S \to \{0,1\}^*$ known to all participants, with lengths $\ell(x) = |c(x)|$. It is *prefix-free* if no codeword is a prefix of another. Institutions in the sense of this paper (standardized names, units, legal forms, accounting categories, calendars) are shared codes in exactly this sense (Arrow, 1974; Cremer et al., 2007).

**Assumption 1.** Settling a task means that the initiating participant identifies the required plan $x \in S$ to the other $k-1$ participants without ambiguity; messages to different participants are counted separately in messages but identically in bits per recipient, so the result is stated per recipient.

**Theorem 1** (coding gain)**.** *(i) Without a shared code, identifying an arbitrary element of $S$ without error requires at least $\lceil \log_2 s \rceil$ bits per recipient in the worst case.*

*(ii) With a shared prefix-free code that is optimal for $p$, the expected length per recipient $L_p^* = \min_{c \text{ prefix-free}} \sum_x p(x)\ell(x)$ satisfies*

$$H(p) \le L_p^* < H(p) + 1. \tag{1}$$

*(iii) Hence the expected saving per recipient, $G(p) = \lceil \log_2 s \rceil - L_p^*$, obeys $G(p) > \lceil \log_2 s \rceil - H(p) - 1 \geq 0$ whenever $\lceil \log_2 s \rceil \geq H(p) + 1$, and $G(p)$ is strictly positive as soon as $p$ is sufficiently concentrated ($H(p) < \lceil \log_2 s \rceil - 1$).*

*Proof.* (i) Without a shared code there is no prior agreement on which binary string denotes which plan, so the only error-free strategy is a fixed-length enumeration of $S$ agreed by the trivial convention of listing; any scheme that distinguishes $s$ alternatives with binary strings of a fixed length $m$ needs $2^m \geq s$, i.e. $m \geq \lceil \log_2 s \rceil$. (The point is not that $\log_2 s$ is a lower bound for *all* conceivable codes; it is the cost of the *codeless* baseline, in which the message must be self-describing.)

(ii) *Lower bound.* For any prefix-free code the Kraft inequality holds: $\sum_x 2^{-\ell(x)} \leq 1$ (Cover and Thomas, 2005, Thm. 5.2.1). Put $q(x) = 2^{-\ell(x)}/Z$ with $Z = \sum_x 2^{-\ell(x)} \leq 1$. Then

$$\sum_x p(x)\ell(x) - H(p) = \sum_x p(x) \log_2 \frac{p(x)}{2^{-\ell(x)}} = \sum_x p(x) \log_2 \frac{p(x)}{q(x)} - \log_2 Z = D_{KL}(p\|q) - \log_2 Z \geq 0,$$

because $D_{KL} \geq 0$ (Gibbs' inequality) and $-\log_2 Z \geq 0$. *Upper bound.* Choose $\ell(x) = \lceil -\log_2 p(x) \rceil$. Then $\sum_x 2^{-\ell(x)} \leq \sum_x p(x) = 1$, so by the converse Kraft theorem (Cover and Thomas, 2005, Thm. 5.2.1) a prefix-free code with these lengths exists, and $\sum_x p(x)\ell(x) < \sum_x p(x)(-\log_2 p(x) + 1) = H(p) + 1$. The optimal code does at least as well.

(iii) Subtract (ii) from (i). □

**Remark.** Theorem 1 is the source-coding theorem of Shannon (1948) applied to the plan set. Its content for the paper is only this: *a shared code converts a worst-case identification cost into an average-case cost that is lower exactly to the extent that tasks are predictable.* The "compression" in the name of the hypothesis refers to this step and to nothing else. The same economy appears in the mechanism-design bounds on message-space size (Mount and Reiter, 1974; Segal, 2007; Nisan and Segal, 2006) and in communication complexity (Yao, 1979; Kushilevitz, 1997); the rate–distortion view of Marzen and DeDeo (2017) and the coarse-graining view of Dedeo (2017) are the closest conceptual neighbours.

### 3.3. Break-even scale of an institution

**Definition 6** (institutional cost structure)**.** Adopting a code costs a fixed set-up $F \geq 0$ (designing and promulgating the standard), a learning cost $\ell \geq 0$ per agent (each of the $N$ agents must learn the code), and an administrative overhead $a \geq 0$ bits per task (verification, record-keeping, enforcement). It saves $G > 0$ bits per task (Theorem 1, per recipient; multiply by $k-1$ if desired; the argument is unchanged).

**Theorem 2** (break-even)**.** *Over a horizon with $T$ tasks, the institution lowers total communication cost if and only if*

$$T\,(G-a) > F + N\ell. \tag{2}$$

*With $T = N\lambda\tau$ over $\tau$ periods, this is equivalent to*

$$\lambda\tau > \frac{F/N+\ell}{G-a} \quad \textit{when } G > a, \tag{3}$$

*and the inequality can never hold when $a \geq G$.*

*Proof.* Total cost without the institution is $C_0 = T\lceil \log_2 s \rceil$. With it, $C_1 = F + N\ell + T(L_p^* + a)$. Then $C_1 < C_0$ if and only if $F + N\ell < T(\lceil \log_2 s \rceil - L_p^* - a) = T(G-a)$. If $a \geq G$ the right-hand side is $\leq 0 < F + N\ell$ unless $F = \ell = 0$ and $a = G$, in which case the costs are equal, so strict improvement is impossible. If $G > a$, substitute $T = N\lambda\tau$ and divide both sides by $N(G-a) > 0$. □

**Corollary 1** (scale advantage)**.** *The threshold $(F/N+\ell)/(G-a)$ is decreasing in $N$: holding $\lambda, \tau$ fixed, a larger society crosses the break-even point sooner, because the set-up cost is spread over more agents. It is* not *decreasing in $N$ through $\ell$: per-agent learning costs do not benefit from scale.*

This is the formal version of the observation that institutions "pay off at scale" (Johnson, 1982; Garicano, 2000; Bolton and Dewatripont, 1994), and it also makes precise when they do not: when overhead $a$ is close to the coding gain $G$, or when horizons $\tau$ are short.

### 3.4. Mismatched code penalty: institutional rigidity

**Theorem 3** (cost of a stale code)**.** *Let the code be optimal for $p$ in the sense of using lengths $\ell_p(x) = \lceil -\log_2 p(x) \rceil$, and let the true task distribution be $q$ with $q \ll p$ (every plan with positive $q$-probability has positive $p$-probability). Then the expected length under $q$ satisfies*

$$H(q) + D_{KL}(q\|p) \leq \mathbb{E}_q[\ell_p(X)] < H(q) + D_{KL}(q\|p) + 1. \tag{4}$$

*The excess cost of using the $p$-code when the world is $q$, relative to the $q$-optimal code, lies in $(D_{KL}(q\|p) - 1,\ D_{KL}(q\|p) + 1)$ bits per task.*

*Proof.* Since $-\log_2 p(x) \leq \ell_p(x) < -\log_2 p(x) + 1$,

$$\mathbb{E}_q[\ell_p(X)] \geq \sum_x q(x)\big(-\log_2 p(x)\big) = \sum_x q(x)\log_2 \frac{q(x)}{p(x)} + \sum_x q(x)\big(-\log_2 q(x)\big) = D_{KL}(q\|p) + H(q),$$

and the same computation with the $+1$ gives the strict upper bound. The $q$-optimal code has expected length in $[H(q), H(q)+1)$ by Theorem 1(ii), which yields the stated interval for the excess. This is the "wrong code" result of Cover and Thomas (2005, Thm. 5.4.3). □

**Corollary 2** (fragility)**.** *A shock that moves the task distribution from $p$ to $q$ raises the per-task communication cost of an existing institution by about $D_{KL}(q\|p)$ bits. Combined with Theorem 2, the institution remains worthwhile after the shock only if $G(p) - D_{KL}(q\|p) > a$ (up to the $\pm 1$ rounding).*

An institution can therefore *lower* efficiency after a shock, and the more specialized the code (the smaller $H(p)$ relative to $\log_2 s$), the larger the divergence a given shock can produce. This is the precise sense in which standardization trades efficiency for rigidity; it echoes the declining marginal returns of Tainter (1988) but gives the mechanism a unit (bits per task) and a magnitude (a divergence).

### 3.5. Hierarchical routing bound and root load

**Definition 7** (hierarchy)**.** A hierarchy is a rooted tree whose $N$ leaves are the agents and whose internal nodes are administrative units; every internal node has at most $b \geq 2$ children; the depth (leaf-to-root distance) is $d = \lceil \log_b N \rceil$ for a complete $b$-ary tree. A task between two leaves is *settled by relay* when each participant sends a message up to their lowest common ancestor (LCA) and the LCA sends the settlement down.

**Theorem 4** (routing and root load)**.** *(i)* Message bound. *Any pairwise task can be settled with at most $2d$ messages (up-path plus down-path through the LCA), versus up to $N-1$ messages for a flat broadcast in which the initiator must reach every other agent. For $M$ pairwise tasks the total is at most $2M\lceil \log_b N \rceil$.*

*(ii)* Root load. *In a complete $b$-ary tree with $N = b^d$ leaves, if the two participants of a task are drawn uniformly at random among leaves (independently, with replacement), the probability that their LCA is the root is $1 - 1/b$. Hence the expected number of tasks that pass through the root is $M(1-1/b)$.*

*(iii)* Federated or modular coordination. *If a fraction $\theta \in [0,1]$ of tasks is* local *(both participants belong to the same first-level module, i.e. child subtree of the root, by construction of the task process) and the remaining $1-\theta$ are drawn uniformly as in (ii), the expected root load is $M(1-\theta)(1-1/b)$.*

*Proof.* (i) In a tree there is a unique simple path between two leaves; it passes through their LCA and has length at most $2d$ since each leaf is at depth $\leq d$ and the LCA is at depth $\geq 0$. Sending one message per edge along the two leaf-to-LCA paths settles the task in $\leq 2d$ messages. A flat broadcast from one initiator to the other $N-1$ agents uses $N-1$ messages. Summing over $M$ tasks gives (i).

(ii) The root has $b$ children, each the root of a subtree with $b^{d-1}$ leaves. The first participant lies in some child subtree $j$; the second participant, drawn uniformly and independently, lies in the same

subtree $j$ with probability $b^{d-1}/b^d = 1/b$. The LCA is the root exactly when the two participants lie in different child subtrees, which has probability $1 - 1/b$. Expectation of a sum of $M$ indicators gives $M(1-1/b)$.

(iii) Local tasks never reach the root (their LCA lies inside a module); the non-local fraction behaves as in (ii). By linearity of expectation the load is $M(1-\theta)(1-1/b)$. □

**Remark.** Theorem 4 formalizes the trade-off between centralized administration and federated coordination (Radner, 1993; Bolton and Dewatripont, 1994): a hierarchy cuts the *message* cost of coordination from linear to logarithmic in $N$, but concentrates a constant fraction $1-1/b$ of all non-local tasks on the apex, whose processing rate then bounds $K$ (Definition 3). Modularity reduces apex load by the locality fraction $\theta$ at the price of *not* reconciling across modules. These are the five regimes compared in the pre-registered simulations (no institution, shared norms, standardized records, centralized administration, federated), and the simulations measure messages and bits directly rather than assuming the bounds.

### 3.6. Backlog dynamics under mismatch

**Definition 8** (backlog)**.** Let $D_t$ be the number of tasks arriving in period $t$ and $K_t$ the number the system can settle in period $t$. The backlog of unsettled tasks follows the Lindley recursion

$$B_{t+1} = \max\{0,\ B_t + D_t - K_t\}, \qquad B_0 \geq 0. \tag{5}$$

**Theorem 5** (backlog dynamics)**.** *(i)* Deterministic case. *If $D_t = D$ and $K_t = K$ for all $t$, then: if $D \leq K$, $B_t \leq \max\{B_0 - t(K-D), 0\}$, so the backlog reaches 0 after at most $\lceil B_0/(K-D)\rceil$ periods when $D < K$ and stays constant at $B_0$ when $D = K$; if $D > K$, $B_t = B_0 + t(D-K) \to \infty$ linearly.*

*(ii)* Stochastic case. *If $(D_t - K_t)_{t\geq 0}$ is i.i.d. with finite mean, then when $\mathbb{E}[D_t] < \mathbb{E}[K_t]$ the backlog has a proper stationary distribution to which $B_t$ converges from any $B_0$, and when $\mathbb{E}[D_t] > \mathbb{E}[K_t]$, $B_t \to \infty$ almost surely.*

*(iii)* Delay under a Markovian approximation. *If arrivals are Poisson with rate $\lambda_D$ and settlements are exponential with rate $\mu$ (an M/M/1 queue), with $\rho = \lambda_D/\mu < 1$, the expected sojourn time is $W = 1/(\mu - \lambda_D) = (1/\mu)/(1-\rho)$, which is increasing and convex in $\rho$ on $[0,1)$ and diverges as $\rho \uparrow 1$.*

*Proof.* (i) If $D \leq K$, then $B_{t+1} = \max\{0, B_t - (K-D)\}$; by induction $B_t \leq \max\{0, B_0 - t(K-D)\}$, with equality as long as the argument is nonnegative, which gives the hitting time when $D < K$ and constancy when $D = K$. If $D > K$, then $B_t + D - K > 0$ for all $t$ (since $B_t \geq 0$), so the maximum is never binding and $B_{t+1} = B_t + (D-K)$, hence $B_t = B_0 + t(D-K)$.

(ii) The recursion is the single-server waiting-time recursion of Lindley (1952), and the stability dichotomy is the theorem of Loynes (1962); the proof is not reproduced here and the result is cited.

(iii) The M/M/1 stationary number in system is geometric with parameter $\rho$, so the mean is $\rho/(1-\rho)$ and Little's law gives $W = \rho/((1-\rho)\lambda_D) = 1/(\mu(1-\rho)) = 1/(\mu-\lambda_D)$. The function $f(\rho) = (1-\rho)^{-1}$ has $f' = (1-\rho)^{-2} > 0$ and $f'' = 2(1-\rho)^{-3} > 0$ on $[0,1)$, and $f(\rho) \to \infty$ as $\rho \uparrow 1$. □

**Scope of Theorem 5.** This is a *mechanism*, not evidence. Polities are not single-server queues: capacity is endogenous (institutions are built in response to backlog), arrivals are correlated across periods and agents, and "unsettled tasks" are not observed in the historical record. The theorem is used only to motivate two qualitative predictions, that costs rise smoothly and convexly in utilization and that sustained $\rho > 1$ is qualitatively different from $\rho < 1$, and both are stated as conjectures below.

### 3.7. Conjectures

The following statements are unproved. Each is given with its reason and its falsifier.

**Conjecture 1** (proxy monotonicity)**.** *The pre-registered Seshat and V-Dem proxies are monotone in the model's primitives: the scale variables (population, territory, largest settlement, settlement hierarchy) increase with $D$ of Definition 2, and the four indicator blocks (administration, standardized rules and money, communication infrastructure, record-keeping) increase with $K$ of Definition 3, so that $P = z(\log D) - z(K)$ is monotone in $\log \rho$ up to measurement error.*

*Reason:* population and territory raise the number and the dispersion of participants; written records, legal codes, money and measurement systems are shared codes (Theorem 1); roads, couriers and postal systems raise bits per period; administrative levels implement the routing of Theorem 4. *Falsifier:* the proxies for $D$ and $K$ are so collinear (as Seshat's first principal component suggests; Shin et al., 2020) that $P$ retains almost no variance, or alternative defensible $K$ weightings reverse the sign of $P$ for a substantial share of polities; both are reported in the measurement section.

**Conjecture 2** (sustained overload raises breakdown probability)**.** *Periods with $\rho > 1$ sustained longer than the time needed to build capacity raise the probability of institutional breakdown.*

*Mechanism family (not proved):* unsettled tasks accumulate (Theorem 5), so participants cannot form the common knowledge required for coordinated action (Chwe, 2000) and local defection spreads by contagion through the interaction network (Morris, 2000); institutions whose codes are stale after a shock lose their cost advantage (Theorem 3) and are abandoned. *Falsifier:* the pre-registered H2 tests find $\beta_P$ not positive, or $P$ does not beat the population-only and capacity-only baselines out of sample.

**Conjecture 3** (smooth, not step)**.** *The relation between P and instability is convex and smooth away from* $\rho \approx 1$ *and steepens near it, rather than a discrete threshold.*

*Reason:* Theorem 5(iii) gives a smooth, convex delay in $\rho$; discontinuities would require an additional tipping mechanism (e.g. a critical mass in a contagion model) that the framework does not supply. *Falsifier:* the pre-registered H4 comparison finds that a hinge or step model beats the smooth model out of sample in at least 80% of folds, which would favour a tipping mechanism; conversely, if neither nonlinear form beats the linear model, the "steepening" clause of the conjecture is unsupported.

### 3.8. What is not claimed, and which test speaks to each statement

- **No Shannon measurement of history.** Theorems 1 and 3 are about codes for a plan set with a known distribution. Seshat indicators (presence of writing, coins, courts, roads) are not message lengths, and no entropy or divergence is computed from historical data anywhere in this paper. Shannon quantities appear only in the simulations, where the plan distribution $p$ and the messages are defined by the experimenter and can be measured.
- **No causal claim.** The theorems describe an idealized communication system; the empirical tests are associational and were pre-registered as such; the identification discussion is in Section 5.5.
- **Endogeneity of capacity.** The framework takes $K$ as given within a period; historically, $K$ is built in response to $D$ (Johnson, 1982; Shin et al., 2020). This is why H1 is pre-registered as an association without a direction.

## 4. Data and historical measurement

Every source was downloaded by a script that records the release, URL, retrieval time, licence, size and SHA-256 hash of the raw file (Table 3); raw files are preserved unmodified and never redistributed. All counts in this section are produced by the parsers and written to machine-readable diagnostics from which the tables are generated. Missing values are never recoded as absent, no fact is forward-filled across polities or periods, and no outcome is imputed—the lesson of the moralizing-gods episode (Beheim et al., 2021; Whitehouse et al., 2022).

**Table 2.** Which evidence can speak to each statement of the framework.

| Statement | What can speak to it | Pre-registered test |
|---|---|---|
| Theorem 1 (coding gain) | Simulated messages and bits with vs. without a shared code | H3 (regimes b, c vs. a) |
| Theorem 2 (break-even) | Simulated total cost across $N$, $\lambda$, horizon, overhead $a$ | H3 factorial cells; search for cells where institutions lose |
| Theorem 3 (stale code) | Simulated shocks to the task distribution | H3 adverse regimes (shock treatment) |
| Theorem 4 (routing, root load) | Simulated messages and apex load under centralized vs. federated regimes | H3 (regimes d, e) |
| Theorem 5 (backlog) | Not testable with the historical data (backlog unobserved); illustrated in simulation only | none (stated as mechanism) |
| Conjecture 1 | Measurement diagnostics: collinearity of $D$ and $K$ proxies, sensitivity of $P$ to weights | Measurement section |
| Conjecture 2 | Lagged $P$ and subsequent instability vs. baselines, out of sample | H2 (Seshat Y1–Y3; V-Dem Y4–Y6), H5 |
| Conjecture 3 | Linear vs. hinge vs. spline in $P$, out of sample | H4 |

**Table 3.** Sources, releases and provenance. Hashes are the first twelve hexadecimal digits of the SHA-256 of the raw file as downloaded on 2026-11-01; the full registry is in the replication package.

| Dataset | Release | Licence | Bytes | SHA-256 |
|---|---|---|---|---|
| Seshat Global History Databank — Polaris-2026 snapshot | Polaris-2026 (repository commit 55ca5fbc2b, 2026 | MIT (repository LICENSE, Copyright (c) 2 | 2,899,093 | `cf60c9f76eed` |
| Seshat Global History Databank — Equinox-2020 snapshot (spre | Equinox-2020, spreadsheet dated 05.2023 (reposit | MIT (repository LICENSE); original Equin | 1,720,038 | `7964d74c3ae3` |
| Seshat Databank: Equinox Release with Culturally and Institu | Zenodo 8120128 (2023-07-06) | CC-BY-4.0 | 70,197 | `4eb6c1a72de9` |
| Maddison Project Database 2023 | MPD 2023 (Bolt & van Zanden 2024) | Free for academic use with citation (GGD | 4,903,804 | `ecc5916ca127` |
| Maddison Project Database 2023 (Stata file) | MPD 2023 | Free for academic use with citation | 10,892,389 | `2bc62ccbd795` |
| D-PLACE dataset derived from Murdock et al. 1999 'Ethnograph | v3.3 (2025-12-19) | CC BY-NC 4.0 | 7,039,773 | `9ec031e01101` |
| V-Dem Country-Year Dataset v16 (vdemdata R package copy) | v16 (package commit f4dd26922e, 2026-03-17 'v16 | V-Dem data: free for non-commercial use | 33,878,255 | `39b412d39a06` |
| V-Dem codebook (R object) | v16 | GPL-3 (package) | 148,633 | `2840d879cb90` |
| Glottolog CLDF languages table (language family for Galton c | glottolog-cldf v5.3 (2026-03-02) | CC BY 4.0 | 2,477,017 | `1a50a393bc81` |

### 4.1. Three panels, kept separate

**Historical polities (Seshat Polaris-2026).** The Seshat Global History Databank codes historical polities on hundreds of variables from the published historiography (Turchin et al., 2015; François et al., 2016). We use the Polaris-2026 snapshot, the current official release, which contains 626 polities in 42 regions (35 natural geographic areas) spanning −13,600 to 2020 CE, and 56,274 coded facts in the social-complexity, warfare, religion and general sheets. Each fact carries a value or a value range (`value_from`, `value_to`), an optional date range, and flags for disputed and uncertain codes; presence variables are coded present (P), absent (A), inferred present (IP), inferred absent (IA) or unknown (SU). Two structural features of the release shape the design. First, only 2,503 facts carry a date; the other 93.7% describe the whole polity period. A polity-year panel would therefore be an interpolation that the data do not support, and the historical unit of analysis is the polity, with region and era effects. Second, the release contains the CrisisDB "crisis consequences" table (Hoyer et al., 2023, 2024): for 166 crisis episodes in 150 polities, historians coded whether each of 21 consequences occurred, among them decline, collapse, civil war, uprising, fragmentation, loss of the capital, conquest, assassination and deposition. This is the independently coded instability outcome. It is also a crisis-selected sample: a polity without a CrisisDB record is one for which no crisis was coded, not one that had no crisis, so the outcome measures how badly a recorded crisis unfolded rather than whether one occurred. Table 4 summarizes coverage; Figure 1 shows the temporal distribution of polities and crises and the pattern of coded and missing indicators.

**Ethnographic societies (D-PLACE Ethnographic Atlas v3.3).** The Ethnographic Atlas (Murdock, 1967), as curated in D-PLACE (Kirby et al., 2016), describes 1,291 societies at an ethnographic focal year (between −2000 and 1965, most in the nineteenth and twentieth centuries) on a fixed set of categorical variables. We use mean size of local communities (EA031, eight ordinal categories converted to geometric midpoints of the category ranges printed in the codebook), population size (EA202, continuous), and jurisdictional hierarchy beyond the local community (EA033, from acephalous to four levels), with subsistence, agricultural intensity and class differentiation as covariates. Galton's problem—societies are related by descent and diffusion—is handled with Glottolog 5.3 language families (173 families among the 1,291 societies), regions, and spatial heteroskedasticity-and-autocorrelation-consistent standard errors (Conley, 1999; Mace et al., 1994; Dow, 2007). The Atlas has no instability outcome and is used only for the capacity–scale association.

**Modern states (V-Dem v16 and Maddison 2023).** The Varieties of Democracy country-year dataset (Coppedge et al., 2020; Pemstein et al., 2018) provides expert-coded institutional variables for 202 countries from 1789 to 2025; the Maddison Project Database 2023 (Bolt and van Zanden,

2024) provides population and GDP per capita, which we merge by ISO3 code (163 countries matched; the 39 unmatched V-Dem entities are mostly historical polities and micro-states and drop out of the population-based index). Outcomes are the first year of a civil war (Correlates-of-War coding carried in V-Dem, 1816–2006), a coup (1789–2008), and the last year of a V-Dem regime whose coded end type is a military or non-military coup, a self-coup, an assassination, a loss in civil or inter-state war, a foreign intervention or a popular uprising (1789–2025); natural deaths and guided or negotiated transitions are not events. We never map modern countries onto historical polities: the two panels answer the same hypotheses separately.

**Table 4.** Coverage of the three panels after parsing (counts produced by the build script).

| Quantity | Value | Detail |
|---|---|---|
| Seshat Polaris-2026 polities | 626 | 42 regions / 10 macro-regions |
| Seshat facts (dated) | 56,274 (2,503) | unparsed 132, inferred 8,094 |
| Polities with population / territory / any D | 299 / 355 / 561 | |
| Polities with $\geq$ 50% of K indicators | 376 | |
| CrisisDB records (polities) | 166 (150) | Y1 coded 166; Y1b coded 158, positive 122 |
| Y3 succession coded (discontinuous) | 207 (37) | polities censored at 1900: 30 |
| D-PLACE EA societies | 1291 | EA031 611, EA202 953, EA033 1155; 173 language families |
| Modern country-years (countries) | 28,092 (202) | 1789–2025; Maddison population 15,821 |
| Modern events | civil-war onsets 176 / 13,608 | coups 599 / 13,813; irregular regime ends 1041 / 27,846 |

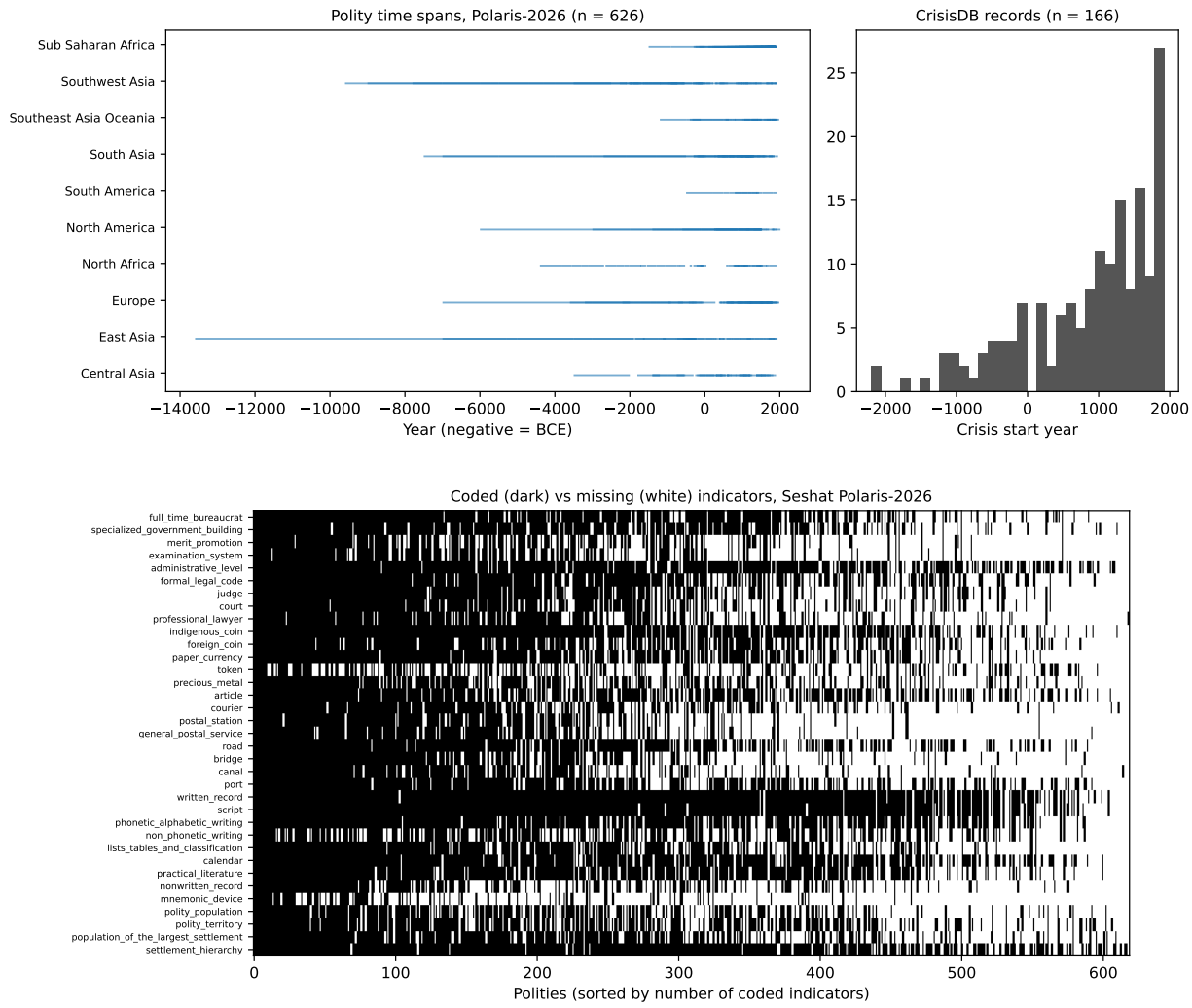



**Figure 1.** Seshat Polaris-2026. Top: polity time spans by macro-region (left) and CrisisDB crisis start years (right). Bottom: coded (dark) versus missing (white) pre-registered indicators, polities sorted by number of coded indicators. Missing is never treated as absent.

### 4.2. Coding rules and uncertainty

Presence codes are mapped to 1 (P, IP), 0 (A, IA) and 0.5 (disputed codes of the form A∼P), with inferred codes flagged so that a sensitivity run can drop them; unknown (SU), blank and unparseable values are missing (132 of 56,274 facts were unparseable and are logged). Numeric ranges are summarized by their geometric midpoint for population, territory and largest-settlement population and by the arithmetic midpoint for level counts, and the bounds are kept: the paper re-estimates its main model at the lower and upper bounds. Dates are kept as intervals; a crisis is dated by the start of its coded interval; nothing is interpolated. When several facts exist for a polity and a variable, the polity value is the mean over facts (the mean log for logged variables); for each

crisis record, "pre-crisis" aggregates use dated facts ending before the crisis start together with the undated whole-polity facts, which, given the date coverage, means that temporal precedence is weak and is acknowledged as such.

### 4.3. Demand, capacity, and the mismatch index

The pre-registration fixed disjoint indicator sets before any outcome was examined. Coordination demand $D$ is scale: the first principal component of the pairwise-complete correlation matrix of log population, log territory, log largest-settlement population and settlement-hierarchy levels, scored as the loading-weighted mean of whatever is available for a polity (at least two of the four), with log population alone as the secondary measure. The component explains 82% of the variance with nearly equal loadings and is defined for 442 polities. Coordination capacity $K$ is the unweighted mean of four block scores, each the share of coded indicators present and each requiring at least half of its indicators coded, with at least three blocks present: administration (full-time bureaucrats, specialized government buildings, merit promotion, examination system, rank-scaled administrative levels), standardized rules and money (legal code, judges, courts, professional lawyers, indigenous and foreign coins, paper currency, tokens, precious metals, articles), communication infrastructure (couriers, postal stations, general postal service, roads, bridges, canals, ports) and record-keeping (written records, script, phonetic and non-phonetic writing, lists and tables, calendar, practical literature, non-written records, mnemonic devices). $K$ is defined for 344 polities. Military, religious and production-infrastructure indicators are excluded from $K$ by design and serve as rival-explanation covariates. The mismatch index is $P = z(\log D) - z(K)$, standardized over the polity sample; because $K$ is a bounded share that can be zero, it is standardized rather than logged, a pre-registered deviation from the candidate formula in the mandate. In the modern panel $D$ is log population and $K$ the mean of six standardized V-Dem items—rigorous and impartial public administration, meritocratic appointment, transparent and predictably enforced laws, fiscal capacity, territorial control and election-management capacity—none of which is a democracy index; in the Atlas the capacity-side measure is the number of jurisdictional levels.

**Table 5.** Measurement model: construction and diagnostics. Values from the measurement diagnostics written by the index-building script; no outcome variable was read by that script.

| Index | Construction and diagnostics |
|---|---|
| D: PC1 of log population, log territory, log largest settlement, settlement hierarchy | variance share 0.821; loadings log_pop 0.26, log_terr 0.24, log_cap 0.26, settl_hier 0.24; $n = 442$ |
| K: mean of four blocks (admin, rules/money, communication, records) | inter-block corr. 0.75–0.79; PC1 share 0.824; $n = 344$ |
| K (2PL IRT, 30 items, MAP) | corr. with block mean 0.963; posterior SD median 0.34; $n = 579$ |
| $\mathrm{corr}(z(D), z(K))$ | 0.838 (IRT: 0.800) |
| P = $z(D) - z(K)$ | variance share of $z(D) + z(K)$ retained 0.166; corr. with D 0.39, with K -0.18, with complexity PC1 -0.04; $n = 317$ polities, 127 crisis records |
| IRT invariance (Old World vs Americas/Oceania fits) | difficulty correlation 0.88 over 30 items |
| Modern K (6 V-Dem items) | PC1 share 0.605; $\mathrm{corr}(D, K)$ 0.062; $\mathrm{corr}(K, \text{polyarchy})$ 0.754; $n = 15,697$ |

Two alternatives to the block mean were pre-registered and are reported side by side: the first principal component of the four blocks, which correlates 0.999 with the mean, and a two-parameter logistic item-response model estimated by marginal maximum likelihood with mild priors on the thirty binary items, which handles item missingness through the likelihood rather than by imputation, is defined for 579 polities, and correlates 0.96 with the block mean. The latent score carries a median posterior standard deviation of 0.34 on a unit-variance scale, which is the honest measure of how uncertain an individual polity's capacity is. Item parameters refitted separately on the Old World and on the Americas and Oceania have difficulties correlated 0.88, so the ordering of institutions by rarity is similar across the two worlds, although the New-World sample (94 polities) is too small for a strict invariance test and the leave-region-out analyses are the operational safeguard. The block structure is stable: inter-block correlations are 0.75–0.79 overall, Cronbach's $\alpha$ across the four blocks is between 0.83 and 0.97 in every macro-region, and dropping any block leaves $K$ correlated at least 0.98 with the full index.

The central measurement fact of the paper is collinearity (Figure 2, Table 5). Scale and capacity correlate 0.84 across polities, as the single-dimension result of Turchin et al. (2017) implies, so the mismatch index retains only 17% of the variance of $z(D) + z(K)$; it correlates 0.39 with $D$, $-0.18$ with $K$ and $-0.04$ with a Turchin-style first principal component of 57 social-complexity indicators.

*P* is a residual, nearly orthogonal to overall complexity. That is what would make it informative if it carried signal, and what makes it fragile: measurement error in either index leaks into it, and any association with *P* can be a disguised association with one of its components. The pre-registered baselines (scale only, capacity only, complexity only, scale and capacity entered separately) exist to detect exactly this.

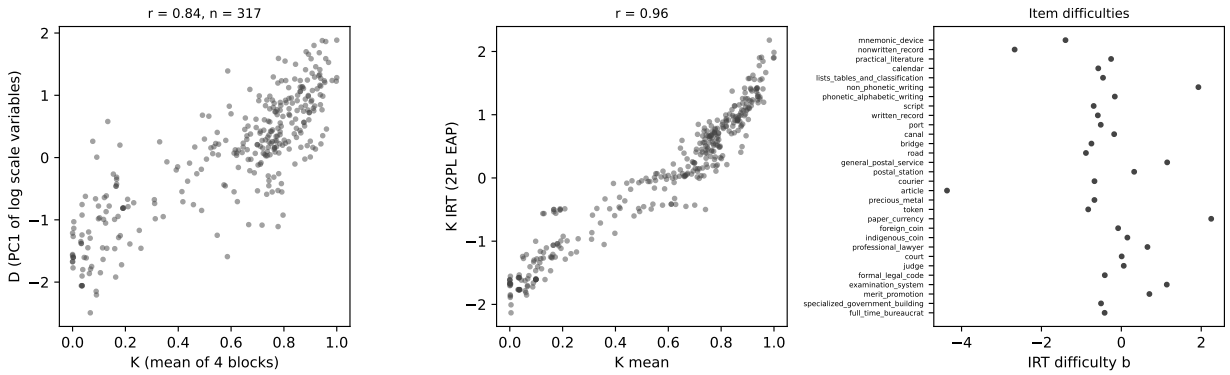



**Figure 2.** Measurement. Left: capacity index against the scale component across Seshat polities. Middle: block-mean against latent (IRT) capacity. Right: item difficulties from the two-parameter model (items ordered by block).

### 4.4. A-priori power

Before estimation, we drew random outcomes with the actual distribution of pre-crisis *P* among the 127 crisis records with a defined index and computed power at the 5% level with region-clustered standard errors (Table 6). For the binary "severe outcome" indicator (base rate 0.77) the minimum detectable effect at 80% power is about 0.6 log-odds per standard deviation, above the pre-registered threshold of 0.5, so that outcome was declared underpowered before any result was seen and is reported as secondary. For the fractional severity share (base rate 0.29 over fourteen items) the minimum detectable effect is about 0.15, and that outcome is primary.

**Table 6.** A-priori power for the Seshat crisis-severity test (random outcomes, actual $P$ distribution, $n = 127$, 400 replications per effect size).

| Outcome | $\beta$ per SD of $P$ | Power | $n$ |
|---|---|---|---|
| Y1b binary | 0.20 | 0.19 | 127 |
| Y1b binary | 0.30 | 0.31 | 127 |
| Y1b binary | 0.40 | 0.47 | 127 |
| Y1b binary | 0.50 | 0.62 | 127 |
| Y1b binary | 0.60 | 0.79 | 127 |
| Y1b binary | 0.80 | 0.94 | 127 |
| Y1b binary | 1.00 | 0.99 | 127 |
| Y1 fractional | 0.10 | 0.49 | 127 |
| Y1 fractional | 0.15 | 0.81 | 127 |
| Y1 fractional | 0.20 | 0.96 | 127 |
| Y1 fractional | 0.30 | 1.00 | 127 |
| Y1 fractional | 0.40 | 1.00 | 127 |
| Y1 fractional | 0.50 | 1.00 | 127 |

# 5. Econometric methods and identification

## 5.1. Pre-registration

The analysis plan was frozen before any predictor–outcome association was computed; its SHA-256 hash is recorded in the decision log and every deviation is a numbered amendment. The plan fixes five hypotheses with falsifiers (H1: capacity and scale are positively associated conditional on era, region and rival covariates, with no direction claimed; H2: lagged mismatch predicts subsequent instability *beyond* scale-only, capacity-only, complexity-only and persistence baselines, judged out of sample; H3: in simulation, shared protocols and institutions lower communication cost per successfully coordinated task; H4: a threshold form outperforms a smooth form out of sample; H5: estimates keep their sign in every held-out macro-region and era, otherwise the domain is narrowed), the samples and exclusions, the disjoint indicator sets of Section 4.3, three primary tests with a Holm correction (H1 in Seshat, H2 on Seshat crisis severity, H2 on the modern composite outcome), a robustness set, and the rule for abandoning the universal framing: if H2 fails in both panels, the paper reports the hypothesis as not supported and keeps only what survived.

### 5.2. Historical polities

For H1 we regress the scale component on the capacity index with macro-region and era fixed effects (ten macro-regions grouping the 42 Seshat regions; six era blocks on the polity midpoint), with and without the rival covariates (warfare-technology index, production-infrastructure index, military professionalization), with standard errors clustered by Seshat region, and we repeat the exercise with a region random intercept, with the latent capacity score, with the four blocks entered separately, and without quasi-polities.

For H2 the primary outcome is crisis severity $Y_1$, the share of the fourteen instability consequences coded present among those coded at all for a CrisisDB record (unknown codes are missing, never zero). The model is a fractional logit,

$$\mathbb{E}[Y_{1,c}] = \Lambda\big(\alpha + \beta\, P_{i(c)} + \gamma' X_{i(c)} + \mu_{r(c)} + \tau_{e(c)}\big)\,, \tag{6}$$

where $P_{i(c)}$ is the pre-crisis mismatch of the polity of crisis $c$, $X$ contains the warfare-technology and production-infrastructure indices and an indicator that an epidemic accompanied the crisis, and $\mu$ and $\tau$ are macro-region and era effects; standard errors are clustered by Seshat region (alternatively by natural geographic area and by polity). The binary secondary outcome $Y_{1b}$—any of collapse, fragmentation, century-long conflict, conquest or civil war coded present, defined only when all five are coded—is fitted by logit; several region–era cells are all-severe, so era effects are dropped for that outcome and heteroskedasticity-robust errors are used. Baselines are fitted on the same observations: fixed effects only, scale only, capacity only, a Turchin-style complexity component, a persistence indicator (an earlier CrisisDB crisis in the same natural geographic area), and scale and capacity entered separately. A placebo index replaces $K$ with the production-infrastructure share that was excluded from $K$ by design. Out-of-sample comparison uses leave-one-macro-region-out folds (region effects removed) and ten repetitions of five-fold cross-validation; the pre-registered rule is that $P$ "beats" a baseline if its mean out-of-sample log-likelihood is higher in at least 80% of folds. For H4, a hinge model whose knot is chosen by profile likelihood on the training fold only and a natural cubic spline with three degrees of freedom are compared with the linear form on the same folds. For H5, $\beta$ is re-estimated with each macro-region and each era held out, and, where the held-out block has at least twelve crisis records, within the block.

Two secondary historical outcomes use all polities. Polity duration (end year minus start year) is modelled by a Cox proportional-hazards regression stratified by macro-region, with the polity's end as the event and right-censoring for polities that reach 1900, robust errors clustered by region and a Schoenfeld test (Cox, 1972; Box-Steffensmeier and Jones, 2004); discontinuous succession—the successor polity's relationship to its predecessor coded as anything other than continuity—is modelled by logit. Both are reported with the caveat that Seshat polity boundaries are periodization

decisions.

### 5.3. Ethnographic societies

Log community size is regressed on jurisdictional levels with subsistence, agricultural intensity, class differentiation and region controls; standard errors are Conley spatial HAC with 500 and 1,000 km cut-offs (Conley, 1999) and, alternatively, clustered by Glottolog language family. The reverse specification, an ordered logit of jurisdictional levels on log community size, is reported with family-clustered errors.

### 5.4. Modern states

The modern outcome $Y_{456}$ equals one if a civil-war onset, a coup or an irregular regime end occurs in $(t, t+5]$ and is missing if no year in the window is coded. The primary model is a pooled logit with region and year fixed effects and country-clustered standard errors,

$$\Pr(Y_{456,i,t+1..t+5} = 1) = \Lambda\left(\alpha + \beta P_{it} + \gamma' X_{it} + \mu_{r(i)} + \tau_t\right), \tag{7}$$

with $X$ containing log GDP per capita, the V-Dem electoral-democracy index (because the capacity items correlate 0.75 with it) and a cubic in years since the last event (Carter and Signorino, 2010; Beck et al., 1998); a conditional logit with country and decade effects and a linear probability model with country and year effects are secondary (Colin Cameron and Miller, 2015). Baselines are population only, capacity only, development only, persistence (an event in the previous five years), and population and capacity entered separately; the placebo index replaces $K$ with the democracy index. Two negative controls are pre-registered: events in $[t-5, t-1]$ regressed on $P_t$ with the same controls should not load more strongly than future events, and the placebo should not perform as well as $P$. Out-of-sample validation is by forward chaining (train to 1945, test 1946–1975; train to 1975, test 1976–2005; train to 2005, test 2006 onward; year effects replaced by decade effects so that test years have a fitted level) and by leave-one-region-out on the six V-Dem politico-geographic regions, with log-likelihood, area under the ROC curve, Brier score and calibration slope (Goldstone et al., 2009). H4 and H5 proceed as in the historical panel with country-blocked folds and with the six regions and four periods (pre-1900, 1900–1945, 1946–1989, 1990 onward) as blocks. The robustness set varies the horizon (1, 3, 10 years), the outcome components, the capacity aggregation, the standardization window, the inclusion of land area, the sample period and the control set, and reports Oster's bias-adjusted coefficient and the Cinelli–Hazlett robustness value on the linear-probability version of the primary model (Oster, 2017; Cinelli and Hazlett, 2019).

### 5.5. What the designs identify

Nothing in this paper is causal, and the text does not use causal language for H1 or H2. The estimands are conditional associations with pre-specified direction, baselines, out-of-sample validation and falsification tests. The reasons are stated in advance in the identification note of the replication package and summarized here. Scale creates the demand for administration and administration enables scale, so H1 is symmetric by construction. The historical outcome is crisis-selected, most Seshat facts are undated so that "pre-crisis" capacity is mostly whole-polity capacity, the institutional codes and the crisis codes are produced by the same project from overlapping historiography, and the mismatch index is a residual of two indices that correlate 0.84. In the modern panel the capacity items are expert-coded with knowledge of later events and compete with regime type as an explanation, which the pre-registered placebo, negative control and sensitivity analysis are designed to expose rather than to remove. A credible design would move demand or capacity for reasons unrelated to local instability; no such variation is coded at the polity level in Seshat, and the candidate instruments in the modern panel (printing, railways, telegraphy) do not satisfy an exclusion restriction for instability. The paper therefore claims association, prediction and robustness, and declines a causal claim.

## 6. Results: historical polities and ethnographic societies

All estimates in this section are [EMPIRICAL] and associational. Coefficients are per standard deviation of the standardized regressor unless noted; stars denote $p < 0.10$ ($^{*}$), 0.05 ($^{**}$), 0.01 ($^{***}$) with the clustering stated in each table.

### 6.1. H1: capacity and scale

Capacity and scale move together in every dataset (Tables 7 and 8). In Seshat, a one-unit increase in the capacity share (from no indicator present to all present) is associated with 2.36 units of the scale component in the primary specification with region and era effects (SE 0.17, $n = 317$), which is 0.75 standard deviations of $D$ per standard deviation of $K$; with the warfare, production-infrastructure and military covariates the coefficient falls to 1.21 (SE 0.43, $n = 191$), still clearly positive, and it is 2.18 (SE 0.13) with a region random intercept instead of fixed effects. With the latent capacity score the association is 0.37 scale units per IRT standard deviation (SE 0.09). Entering the four blocks separately, communication infrastructure carries most of the association (1.09, SE 0.35) while administration, rules and money, and records are individually indistinguishable from zero given the others—which, with inter-block correlations near 0.8, is a statement about collinearity rather than about mechanisms. In the Ethnographic Atlas, each additional level of jurisdictional

hierarchy beyond the local community is associated with 0.70 log units of mean community size ($n = 579$; Conley standard errors 0.09 and 0.10 at 500 and 1,000 km; 0.12 clustered by language family) and with 0.59 log units of population size ($n = 799$); the ordered-logit specification in the other direction gives 0.49 log-odds of one more level per log unit of community size (SE 0.12). In the modern panel the within-country association of log population with capacity is 0.15 (SE 0.06) and the fifty-year-difference association 0.13 (SE 0.05), while the pooled cross-sectional coefficient is positive but imprecise (0.19, SE 0.24). The sign is stable across datasets, specifications and the two worlds; the first pre-registered primary test is passed. Nothing here identifies a direction.

**Table 7.** H1 in Seshat: scale regressed on capacity. OLS, standard errors clustered by Seshat region (42 clusters); region random intercept in the last row. $K$ is a share in $[0, 1]$; the last column rescales the coefficient per standard deviation of $K$.

| Specification | Term | Coef. | SE | $n$ | $R^2$ |
|---|---|---|---|---|---|
| H1-S1 primary: D_pc1 ∼ K_mean + FE | K_mean | 2.356*** | 0.173 | 317 | 0.73 |
| H1-S2: D_pc1 ∼ K_mean + X + FE | K_mean | 1.214*** | 0.432 | 191 | 0.83 |
| H1-S3: log_pop ∼ K_mean + X + FE | K_mean | 3.482* | 1.934 | 146 | 0.80 |
| H1-S4: log_terr ∼ K_mean + X + FE | K_mean | 2.103 | 2.376 | 158 | 0.60 |
| H1-S5: D_pc1 ∼ K_irt + X + FE | K_irt | 0.366*** | 0.094 | 208 | 0.81 |
| H1-S6: D_pc1 ∼ K blocks + X + FE | K_admin | 0.148 | 0.335 | 164 | 0.86 |
| H1-S6: D_pc1 ∼ K blocks + X + FE | K_rules_money | -0.290 | 0.294 | 164 | 0.86 |
| H1-S6: D_pc1 ∼ K blocks + X + FE | K_comm_infra | 1.085*** | 0.350 | 164 | 0.86 |
| H1-S6: D_pc1 ∼ K blocks + X + FE | K_records | 0.220 | 0.253 | 164 | 0.86 |
| H1-S7: D_pc1 ∼ K_mean + X + FE, no quasi-polities | K_mean | 1.360** | 0.530 | 155 | 0.79 |
| H1-S8: D_pc1 ∼ K_mean + era, region random intercept | K_mean | 2.180*** | 0.126 | 317 | – |

**Table 8.** H1 in the Ethnographic Atlas (top) and in the modern panel (bottom).

| Specification | Term | Coef. | SE | $n$ | $R^2$ |
|---|---|---|---|---|---|
| H1-E1 OLS log community size ∼ hierarchy + X + region, Conley 500 km | hier | 0.702*** | 0.089 | 579 | 0.75 |
| H1-E1 OLS log community size ∼ hierarchy + X + region, Conley 1000 km | hier | 0.702*** | 0.103 | 579 | 0.75 |
| H1-E2 same, SE clustered by language family | hier | 0.702*** | 0.123 | 579 | 0.75 |
| H1-E3 log population (EA202) ∼ hierarchy + X + region, family-clustered | hier | 0.587*** | 0.107 | 799 | 0.67 |
| H1-E4 ordered logit hierarchy ∼ log community size + X + region (family-clustered) | log_community_size | 0.489*** | 0.116 | 579 | – |
| H1-M1 within-country: log pop ∼ K + log gdppc + polyarchy + year FE | K_modern_dm | 0.148** | 0.062 | 14647 | 0.65 |
| H1-M2 pooled: log pop ∼ K + log gdppc + polyarchy + year FE + region FE | K_modern | 0.188 | 0.240 | 14647 | 0.23 |
| H1-M3 50-year differences: Δlog pop ∼ ΔK + Δlog gdppc + Δpolyarchy + year FE | dK_modern | 0.135** | 0.053 | 6836 | 0.17 |

### 6.2. H2: crisis severity

Table 9 and Figure 3 report the pre-registered primary test. In the primary specification, a one-standard-deviation higher pre-crisis mismatch is associated with 0.20 higher log-odds that a given adverse consequence is recorded for the crisis (SE 0.06, $n = 127$ crisis records in 117 polities and 31 Seshat regions with a defined pre-crisis index); with the rival covariates the estimate is 0.20 (SE 0.09, $n = 87$), and it is unchanged when the overall complexity component is added. The coefficient is positive and precise, survives the Holm correction over the three primary tests, and is stable under every pre-registered perturbation of the measurement model (Table 10): latent capacity (0.15, SE 0.09), population only (0.27, SE 0.11), lower and upper range bounds, inferred codes dropped, within-era and within-region standardization, ranks, quasi-polities dropped (0.15, SE 0.11), crises starting at least 25 years after the polity's start (0.25, SE 0.09), records with at least twelve coded items, no era effects, no region effects, no effects at all, and clustering by natural geographic area or by polity. Held-out macro-regions and eras (Table 12, Figure 5) give pooled coefficients between 0.18 and 0.33, every one with a confidence interval excluding zero except the block that drops the first millennium BCE (0.20, SE 0.17), and the within-block estimates are positive wherever they can be computed, if imprecise. By the standards of a conventional regression table, the mismatch hypothesis would be reported as confirmed.

**Table 9.** H2 in Seshat: crisis severity $Y_1$ (fractional logit; share of fourteen instability consequences coded present) with macro-region and era effects; standard errors clustered by Seshat region. The binary outcome $Y_{1b}$ uses region effects only (era cells are all-severe) and robust errors.

| Specification | Term | Coef. | SE | $n$ |
|---|---|---|---|---|
| A1 primary: Y1 ∼ P + FE | P_main_z | 0.196*** | 0.056 | 127 |
| A2: Y1 ∼ P + X + FE | P_main_z | 0.204** | 0.085 | 87 |
| A3: Y1 ∼ P + D + K + X + FE (P given components) | P_main_z | 0.204** | 0.083 | 87 |
| B1 baseline D only | D_pc1_z | 0.257* | 0.138 | 94 |
| B2 baseline K only | K_mean_z | -0.034 | 0.195 | 93 |
| B3 baseline complexity PC1 | complexity_pc1_z | -0.010 | 0.240 | 100 |
| B4 baseline persistence | prior_crisis_nga | 0.345 | 0.242 | 100 |
| B5 D and K separately | D_pc1_z | 0.341** | 0.164 | 87 |
| B5 D and K separately | K_mean_z | -0.213 | 0.193 | 87 |
| Placebo: P built with production infrastructure instead of K | P_placebo | 0.257* | 0.138 | 94 |
| A1b: Y1b ∼ P + region FE (logit, HC1; era FE dropped: separation) | P_main_z | -0.178 | 0.203 | 123 |
| A2b: Y1b ∼ P + X + region FE (logit, HC1) | P_main_z | 0.184 | 0.324 | 86 |
| A3b: Y1b ∼ P (logit, HC1, no FE) | P_main_z | -0.068 | 0.180 | 123 |
| Exploratory: reform consequences ∼ P + X + FE | P_main_z | -0.015 | 0.181 | 86 |

The pre-registration asked a different question, and the rest of Table 9 answers it. Scale alone is associated with severity at 0.26 per standard deviation (SE 0.14); capacity alone at −0.03 (SE 0.20); the Turchin-style complexity component at −0.01 (SE 0.24); the persistence indicator at 0.35 (SE 0.24). When scale and capacity are entered separately, scale carries 0.34 (SE 0.16) and capacity −0.21 (SE 0.19): capacity has the sign the hypothesis predicts, but it is not distinguishable from zero. The placebo index, which replaces capacity by the share of production-infrastructure indicators that were excluded from $K$ precisely because they are not coordination capacity, is associated with severity at 0.26 (SE 0.14)—the same as scale alone, and within sampling error of $P$. Out of sample (Table 11), the model with $P$ has a higher log-likelihood than the fixed-effects-only, capacity-only and complexity-only models in 79–85% of the fifty cross-validation folds, but beats the scale-only model in 75%, the persistence model in 69% and the scale-plus-capacity model in 75%, below the 80% rule; with macro-regions held out it loses to scale alone in seven of ten folds and to scale plus capacity in eight. For the binary outcome, which was declared underpowered in advance, the coefficient is negative and imprecise (−0.18, SE 0.20 with region effects; −0.07, SE 0.18 without), and no model discriminates severe from non-severe crises out of sample (areas under the curve 0.62–0.64 for every model, including the baselines). Reform consequences are unrelated to $P$. The conclusion we are obliged to draw is that what the mismatch index predicts about crisis

severity is what polity scale predicts, and that nothing in the data distinguishes a capacity shortfall from a large population.

**Table 10.** Pre-registered robustness set for the Seshat primary coefficient (fractional logit of $Y_1$ on the alternative index, rival covariates, macro-region and era effects; region-clustered standard errors unless stated).

| Variant | Term | Coef. | SE | $n$ |
|---|---|---|---|---|
| R: K = IRT latent | _P | 0.146* | 0.088 | 94 |
| R: D = log population only | _P | 0.265** | 0.105 | 70 |
| R: K = PC1 of blocks | _P | 0.206** | 0.084 | 87 |
| R: quasi-polities dropped | P_main_z | 0.153 | 0.107 | 83 |
| R: crises starting $\geq$25 years after polity start | P_main_z | 0.250*** | 0.091 | 76 |
| R: $\geq$12 of 14 items coded | P_main_z | 0.184** | 0.091 | 84 |
| R: no era FE | P_main_z | 0.252** | 0.105 | 87 |
| R: no region FE | P_main_z | 0.232*** | 0.084 | 87 |
| R: no FE | P_main_z | 0.240** | 0.097 | 87 |
| R: SE clustered by NGA | P_main_z | 0.204** | 0.085 | 87 |
| R: SE clustered by polity | P_main_z | 0.204** | 0.096 | 87 |

**Table 11.** Out-of-sample comparison in Seshat. Top: share of folds in which the model with $P$ (rival covariates included) has a higher out-of-sample log-likelihood than the baseline, and the mean difference per observation; the pre-registered rule requires 0.80. Bottom: out-of-sample metrics for the binary outcome $Y_{1b}$ (ten repetitions of five-fold cross-validation).

| Baseline | Scheme | Folds | Win share | Mean Δ log-lik. |
|---|---|---|---|---|
| B0 FE only | loro | 10 | 0.80 | +0.0104 |
| B1 D only | loro | 10 | 0.30 | -0.0027 |
| B2 K only | loro | 10 | 0.70 | +0.0104 |
| B3 complexity | loro | 10 | 0.80 | +0.0109 |
| B4 persistence | loro | 10 | 0.70 | +0.0030 |
| B5 D+K | loro | 10 | 0.20 | -0.0028 |
| B0 FE only | kfold | 48 | 0.79 | +0.0036 |
| B1 D only | kfold | 48 | 0.75 | +0.0018 |
| B2 K only | kfold | 48 | 0.81 | +0.0050 |
| B3 complexity | kfold | 48 | 0.85 | +0.0051 |
| B4 persistence | kfold | 48 | 0.69 | +0.0019 |
| B5 D+K | kfold | 48 | 0.75 | +0.0018 |

| Model ($Y_{1b}$) | OOS log-lik. | AUC | Brier |
|---|---|---|---|
| A2 P | -1.350 | 0.633 | 0.258 |
| B0 FE only | -1.278 | 0.641 | 0.249 |
| B1 D only | -1.339 | 0.634 | 0.257 |
| B2 K only | -1.318 | 0.630 | 0.259 |
| B3 complexity | -1.322 | 0.629 | 0.261 |
| B4 persistence | -1.341 | 0.624 | 0.263 |
| B5 D+K | -1.396 | 0.621 | 0.268 |

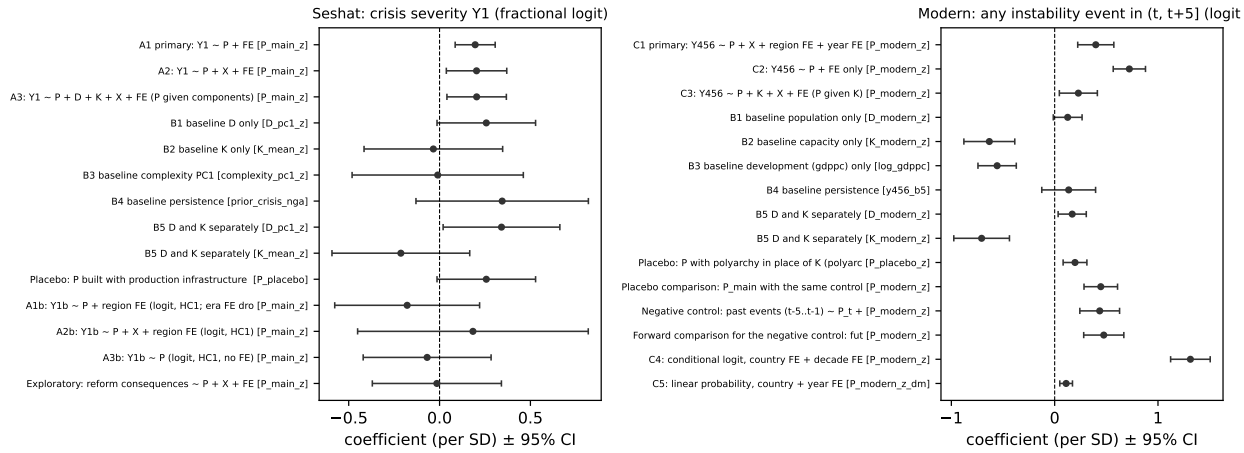



**Figure 3.** Coefficients (per standard deviation) with 95% confidence intervals for the pre-registered specifications, baselines, placebos and negative controls. Left: Seshat crisis severity. Right: modern instability events.

**Table 12.** H5 in Seshat: $\beta_P$ with one macro-region or era held out (pooled column), the number of crisis records in the held-out block, and the coefficient estimated within the held-out block when it has at least twelve records.

| Held-out block | Pooled $\beta_P$ | SE | $n$ | Held-out $n$ | Within-block (SE) |
|---|---|---|---|---|---|
| leave-region-out: Central_Asia | 0.225*** | 0.086 | 84 | 13 | – |
| leave-region-out: East_Asia | 0.229** | 0.107 | 64 | 30 | 0.189 (0.180) |
| leave-region-out: Europe | 0.327*** | 0.124 | 72 | 39 | 0.196 (0.161) |
| leave-region-out: North_Africa | 0.230*** | 0.082 | 78 | 12 | – |
| leave-region-out: North_America | 0.210** | 0.090 | 85 | 13 | – |
| leave-region-out: South_America | 0.261*** | 0.089 | 84 | 6 | – |
| leave-region-out: South_Asia | 0.225*** | 0.084 | 82 | 5 | – |
| leave-region-out: Southeast_Asia_Oceania | 0.214** | 0.093 | 81 | 13 | – |
| leave-region-out: Southwest_Asia | 0.235*** | 0.090 | 70 | 29 | 0.437 (0.234) |
| leave-region-out: Sub_Saharan_Africa | 0.177*** | 0.061 | 83 | 6 | – |
| leave-era-out: 1-1000 | 0.319** | 0.137 | 66 | 40 | 0.127 (0.177) |
| leave-era-out: 1000BCE-0 | 0.204 | 0.174 | 69 | 28 | 0.340 (0.081) |
| leave-era-out: 1001-1500 | 0.263*** | 0.100 | 65 | 43 | 0.205 (0.091) |
| leave-era-out: 1501-1800 | 0.224*** | 0.073 | 73 | 31 | 0.300 (0.524) |
| leave-era-out: post-1800 | 0.224** | 0.108 | 81 | 16 | 0.279 (0.157) |
| leave-era-out: pre-1000BCE | 0.242** | 0.107 | 81 | 8 | – |

### 6.3. H4: no threshold

A hinge in $P$ with its knot chosen on the training fold, and a three-degree-of-freedom spline, improve on the linear model out of sample in only 10% and 8% of the fifty folds (Table 19); the in-sample

hinge places its knot at 0.92 standard deviations with a negative post-knot slope, the opposite of a tipping point. The smooth form is retained for Seshat.

### 6.4. Polity duration and succession

The mismatch index is unrelated to the hazard of a polity's end (Table 13): hazard ratios of 1.03 (SE 0.06) and 1.00 (SE 0.07, Schoenfeld test $p = 0.98$) with 302 and 201 events. Capacity, entered with scale, is associated with a *higher* hazard (ratio 1.75, SE 0.20 on the log scale), which we read as periodization rather than fragility: well-documented complex polities are divided into more, shorter Seshat polities than poorly documented simple ones. Discontinuous succession, coded for 207 polities with 37 discontinuities, is unrelated to $P$ (0.02 and 0.24 log-odds, SE 0.39 and 0.33).

**Table 13.** Secondary historical outcomes. Cox proportional hazards for polity duration, stratified by macro-region with region-clustered robust errors (coefficients on the log-hazard scale, hazard ratio in the last column), and logit for discontinuous succession with region and era effects.

| Specification | Term | Coef. | SE | $n$ | HR | Events |
|---|---|---|---|---|---|---|
| Y2-1 Cox: P + strata(macro-region) | P_z | 0.031 | 0.060 | 317 | 1.03 | 302 |
| Y2-2 Cox: P + X + midpoint + strata | P_z | 0.000 | 0.068 | 208 | 1.00 | 201 |
| Y2-3 Cox: D + K + X + midpoint + strata | D_pc1_z | 0.128 | 0.133 | 208 | 1.14 | 201 |
| Y2-3 Cox: D + K + X + midpoint + strata | K_mean_z | 0.559*** | 0.200 | 208 | 1.75 | 201 |
| Y2-4 Cox: complexity PC1 + X + strata | complexity_pc1_z | 0.399 | 0.291 | 208 | 1.49 | 201 |
| Y3-1 logit: discontinuous succession ~ P + FE | P_z | 0.018 | 0.393 | 145 | – | 24 |
| Y3-2 logit: + X | P_z | 0.239 | 0.325 | 117 | – | 18 |

## 7. Results: modern states, 1820–2025

The modern panel is the form of the test that the mandate's event-risk model has in mind: a country-year panel with lagged predictors, fixed effects, forward-chaining validation and held-out regions. All estimates are [EMPIRICAL] and associational.

### 7.1. H2: instability events in the next five years

In the primary specification (Table 14), a one-standard-deviation higher mismatch is associated with 0.40 higher log-odds of at least one civil-war onset, coup or irregular regime end in the following five years (SE 0.09; 14,637 country-years, 2,692 event windows), conditional on development, electoral democracy, time since the last event, region and year. The coefficient is 0.72 without covariates, 1.31 in a conditional logit with country and decade effects (which uses only countries that experience at least one event window), and 0.11 in probability units in a linear model with country

and year effects. It is positive for every horizon (0.33 at one year, 0.39 at three, 0.45 at ten), largest for civil-war onsets (1.01, SE 0.20), small and imprecise for coups (0.20, SE 0.14) and intermediate for irregular regime ends (0.25, SE 0.09); it does not depend on the capacity aggregation, on standardizing within years, on adding land area to demand, on dropping the nineteenth century, on removing the democracy control or on adding the persistence control (Table 15). Every held-out region and period gives a pooled coefficient between 0.29 and 0.47, and every within-block estimate is positive (Table 17, Figure 5). The second H2 primary test also passes the conventional bar.

**Table 14.** H2 in the modern panel: any instability event in $(t, t+5]$. Logit with region and year effects and country-clustered standard errors unless stated; $X$ = log GDP per capita, electoral democracy, cubic in years since the last event. "Events" counts country-years whose five-year window contains an event.

| Specification | Term | Coef. | SE | $n$ | Events |
|---|---|---|---|---|---|
| C1 primary: Y456 ∼ P + X + region FE + year FE | P_modern_z | 0.398*** | 0.089 | 14637 | 2692.00 |
| C2: Y456 ∼ P + FE only | P_modern_z | 0.723*** | 0.080 | 15687 | 2981.00 |
| C3: Y456 ∼ P + K + X + FE (P given K) | P_modern_z | 0.229** | 0.094 | 14637 | 2692.00 |
| B1 baseline population only | D_modern_z | 0.126* | 0.071 | 14754 | 2710.00 |
| B2 baseline capacity only | K_modern_z | -0.632*** | 0.126 | 15675 | 2957.00 |
| B3 baseline development (gdppc) only | log_gdppc | -0.557*** | 0.095 | 15862 | 2980.00 |
| B4 baseline persistence | y456_b5 | 0.136 | 0.133 | 15806 | 2968.00 |
| B5 D and K separately | D_modern_z | 0.170** | 0.070 | 14637 | 2692.00 |
| B5 D and K separately | K_modern_z | -0.707*** | 0.137 | 14637 | – |
| Placebo: P with polyarchy in place of K (polyarchy not separately controlled) | P_placebo_z | 0.197*** | 0.059 | 14754 | 2710.00 |
| Placebo comparison: P_main with the same controls as the placebo | P_modern_z | 0.446*** | 0.083 | 14676 | 2706.00 |
| Negative control: past events (t-5..t-1) ∼ P_t + gdppc + polyarchy + FE (no duration terms) | P_modern_z | 0.436*** | 0.098 | 14608 | 2757.00 |
| Forward comparison for the negative control: future events ∼ P_t + gdppc + polyarchy + FE | P_modern_z | 0.475*** | 0.099 | 14637 | 2692.00 |
| C4: conditional logit, country FE + decade FE | P_modern_z | 1.314*** | 0.098 | 14637 | 2692.00 |
| C5: linear probability, country + year FE | P_modern_z_dm | 0.111*** | 0.031 | 14637 | 2692.00 |

The baselines, placebo and negative control tell the rest (Table 14, Figure 3). Population alone is weakly associated with instability (0.13, SE 0.07); capacity alone is strongly and negatively associated ($-0.63$, SE 0.13), as is development ($-0.56$ per log unit of GDP per capita). With population and capacity entered separately, population carries 0.17 (SE 0.07) and capacity $-0.71$ (SE 0.14); conditional on capacity, the mismatch adds 0.23 (SE 0.09). The signal in $P$ is, here, overwhelmingly the capacity component with the sign reversed. The placebo that replaces capacity with electoral democracy is associated at 0.20 (SE 0.06) against 0.45 (SE 0.08) for $P$ with the same controls, so the capacity items do carry information beyond regime type, which is reassuring for the

measurement model but not for the hypothesis. The negative control is not reassuring: past events, in the five years before $t$, are associated with $P_t$ at 0.44 (SE 0.10), against 0.48 (SE 0.10) for future events with the same controls. A predictor that is as strongly related to what has already happened as to what will happen is a marker of chronically unstable countries, not a leading indicator, and the pre-registration said in advance that a negative control of this kind would be read that way.

**Table 15.** Pre-registered robustness set for the modern primary coefficient (same specification as the primary row of Table 14 unless stated).

| Variant | Term | Coef. | SE | $n$ | Events |
|---|---|---|---|---|---|
| R: horizon h=1 | P_modern_z | 0.326*** | 0.091 | 14635 | 902.00 |
| R: horizon h=3 | P_modern_z | 0.386*** | 0.087 | 14636 | 1911.00 |
| R: horizon h=10 | P_modern_z | 0.453*** | 0.101 | 14639 | 4143.00 |
| R: component y4_f5 | P_modern_z | 1.011*** | 0.197 | 10279 | 566.00 |
| R: component y5_f5 | P_modern_z | 0.196 | 0.137 | 10853 | 1460.00 |
| R: component y6_f5 | P_modern_z | 0.245*** | 0.094 | 14636 | 2068.00 |
| R: K = PC1 | P_modern_pc1_z | 0.397*** | 0.090 | 14637 | 2692.00 |
| R: standardized within year | P_modern_within_year_z | 0.409*** | 0.091 | 14637 | 2692.00 |
| R: D = (log pop + log area)/2 | P_modern_area_z | 0.352*** | 0.098 | 11735 | 2359.00 |
| R: years $\geq$ 1900 | P_modern_z | 0.394*** | 0.092 | 13083 | 2475.00 |
| R: polyarchy not controlled | P_modern_z | 0.446*** | 0.083 | 14676 | 2706.00 |
| R: + persistence control | P_modern_z | 0.390*** | 0.089 | 14598 | 2682.00 |

Out of sample (Table 16, Figure 4) the verdict is the same as in Seshat with the roles of the components exchanged. Under forward chaining, the model with $P$ has a slightly higher area under the curve than the fixed-effects model (0.773 against 0.768) and than population alone, but a lower one than capacity alone (0.777) and than population and capacity together (0.779), and it never beats the capacity-only model on log-likelihood or discrimination in any of the three temporal splits; with regions held out it beats capacity alone in one of six folds on log-likelihood and three of six on discrimination. All models are well calibrated under forward chaining (slopes 0.95–0.99) and all are badly calibrated when a whole region is held out (slopes about 0.43), which is itself a finding: the level of instability risk is regional, and no index built from scale and capacity transports it. Under the pre-registered 80% rule, $P$ fails against the capacity-only and the scale-plus-capacity baselines in both schemes, and the second H2 primary test is therefore not passed.

**Table 16.** Out-of-sample validation in the modern panel. Top: share of folds in which the model with *P* beats the baseline on the metric; the rule requires 0.80. Bottom: mean metrics by scheme and model (forward chaining: three temporal splits; LORO: six held-out regions).

| Baseline | Scheme | Metric | Folds | Win share | Mean Δ |
|---|---|---|---|---|---|
| B0 FE+X | forward | oos_loglik | 2 | 0.50 | -0.0002 |
| B0 FE+X | forward | auc | 2 | 1.00 | +0.0047 |
| B1 D | forward | oos_loglik | 2 | 0.50 | +0.0002 |
| B1 D | forward | auc | 2 | 1.00 | +0.0044 |
| B2 K | forward | oos_loglik | 2 | 0.00 | -0.0029 |
| B2 K | forward | auc | 2 | 0.00 | -0.0041 |
| B4 persistence | forward | oos_loglik | 2 | 1.00 | +0.0016 |
| B4 persistence | forward | auc | 2 | 1.00 | +0.0096 |
| B5 D+K | forward | oos_loglik | 2 | 0.00 | -0.0019 |
| B5 D+K | forward | auc | 2 | 0.00 | -0.0059 |
| B0 FE+X | loro | oos_loglik | 6 | 0.83 | -0.0004 |
| B0 FE+X | loro | auc | 6 | 0.83 | +0.0077 |
| B1 D | loro | oos_loglik | 6 | 0.83 | +0.0007 |
| B1 D | loro | auc | 6 | 1.00 | +0.0074 |
| B2 K | loro | oos_loglik | 6 | 0.17 | -0.0093 |
| B2 K | loro | auc | 6 | 0.50 | +0.0018 |
| B4 persistence | loro | oos_loglik | 6 | 0.83 | -0.0008 |
| B4 persistence | loro | auc | 6 | 0.67 | +0.0075 |
| B5 D+K | loro | oos_loglik | 6 | 0.17 | -0.0084 |
| B5 D+K | loro | auc | 6 | 0.50 | -0.0012 |

| Scheme | Model | OOS log-lik. | AUC | Brier | Calibration slope |
|---|---|---|---|---|---|
| forward | B0 FE+X | -0.334 | 0.768 | 0.102 | 0.97 |
| forward | B1 D | -0.334 | 0.769 | 0.103 | 0.96 |
| forward | B2 K | -0.331 | 0.777 | 0.101 | 0.99 |
| forward | B4 persistence | -0.336 | 0.763 | 0.103 | 0.95 |
| forward | B5 D+K | -0.332 | 0.779 | 0.102 | 0.97 |
| forward | C1 P | -0.334 | 0.773 | 0.103 | 0.95 |
| loro | B0 FE+X | -0.518 | 0.674 | 0.142 | 0.44 |
| loro | B1 D | -0.519 | 0.674 | 0.142 | 0.44 |
| loro | B2 K | -0.509 | 0.680 | 0.139 | 0.43 |
| loro | B4 persistence | -0.517 | 0.674 | 0.141 | 0.44 |
| loro | B5 D+K | -0.510 | 0.683 | 0.140 | 0.43 |
| loro | C1 P | -0.518 | 0.681 | 0.142 | 0.44 |

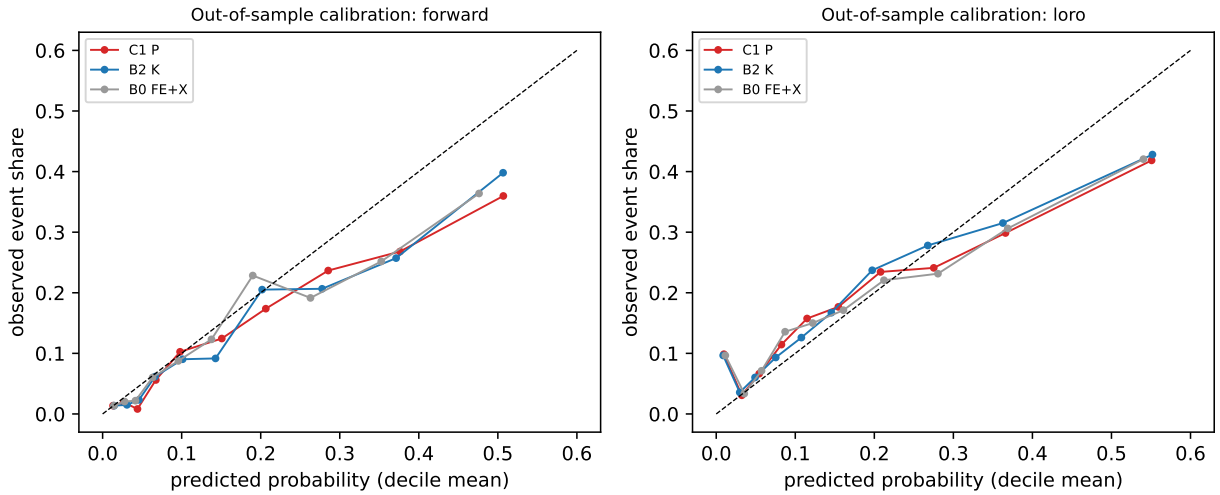



**Figure 4.** Out-of-sample calibration in the modern panel (decile means of predicted probability against observed event shares) for the model with $P$, the capacity-only baseline and the fixed-effects baseline, under forward chaining (left) and leave-one-region-out (right).

**Table 17.** H5 in the modern panel: $\beta_P$ with one region or period held out (pooled), and estimated within the held-out block.

| Held-out block | Pooled $\beta_P$ | SE | $n$ | Within $n$ | Within-block (SE) |
|---|---|---|---|---|---|
| leave-region-out: 1.0 | 0.293*** | 0.094 | 13252 | 1385 | 1.067 (0.432) |
| leave-region-out: 2.0 | 0.328*** | 0.099 | 12017 | 2620 | 0.542 (0.292) |
| leave-region-out: 3.0 | 0.303*** | 0.097 | 13175 | 1462 | 0.449 (0.350) |
| leave-region-out: 4.0 | 0.312*** | 0.117 | 11363 | 3274 | 0.391 (0.167) |
| leave-region-out: 5.0 | 0.294*** | 0.097 | 10717 | 3920 | 0.781 (0.285) |
| leave-region-out: 6.0 | 0.470*** | 0.093 | 12661 | 1976 | 0.234 (0.206) |
| leave-period-out: 1900-1945 | 0.415*** | 0.099 | 12616 | 2021 | 0.298 (0.187) |
| leave-period-out: 1946-1989 | 0.361*** | 0.113 | 8927 | 5710 | 0.435 (0.122) |
| leave-period-out: 1990+ | 0.417*** | 0.106 | 9285 | 5352 | 0.348 (0.161) |
| leave-period-out: pre-1900 | 0.394*** | 0.092 | 13083 | 1554 | 0.665 (0.277) |

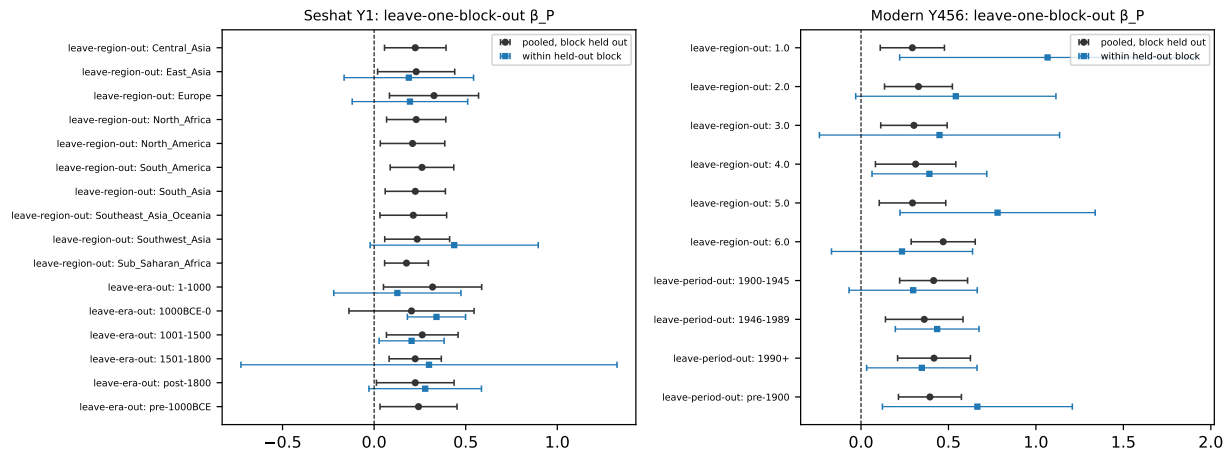



**Figure 5.** H5: the mismatch coefficient with each macro-region or era (Seshat, left) and each region or period (modern panel, right) held out, and the coefficient estimated inside the held-out block where possible.

### 7.2. Sensitivity to unobservables

Table 18 applies Oster's and Cinelli–Hazlett's diagnostics to the linear-probability version of the primary model. Adding the controls moves the coefficient from 0.082 to 0.038 while raising $R^2$ from 0.127 to 0.161; the bias-adjusted coefficient at $R_{\max} = 1.3R^2$ and $\delta = 1$ is $-0.024$, on the other side of zero. The partial $R^2$ of $P$ is 0.006 and the robustness value is 0.072: an unobserved confounder that explained seven percent of the residual variance of both the mismatch and the outcome would suffice to remove the association. Under the conventions of the sensitivity literature this is a fragile coefficient.

**Table 18.** Sensitivity of the modern primary coefficient to unobservables (linear probability model with the primary controls).

| Quantity | Value |
|---|---|
| LPM $\beta_P$ with controls | 0.0383 |
| LPM $\beta_P$ without controls | 0.0819 |
| $R^2$ with / without controls | 0.161 / 0.127 |
| Oster bias-adjusted $\beta_P$ ($R_{\max} = 1.3R^2$, $\delta = 1$) | -0.0238 |
| Partial $R^2$ of $P$ | 0.0056 |
| Cinelli–Hazlett robustness value $RV_{q=1}$ | 0.072 |

### 7.3. H4: a nonlinearity of negligible size

With country-blocked folds, the hinge improves on the linear form in 12 of 15 folds and the spline in 13 (Table 19), which meets the pre-registered 80% rule by one fold. The improvement is 0.001 nats per observation; the in-sample curve (Figure 6) is a mild convexity around the median rather

than a step, and the hinge knot is at $-0.08$ standard deviations, i.e. at the centre of the distribution. We report this as a marginal nonlinearity consistent with the smooth convexity of Theorem 5 and not as a threshold; the pre-registration's falsifier for H4 was a hinge or step model that *outperforms* the smooth one, and a spline is a smooth model.

**Table 19.** H4: linear against hinge (knot chosen on the training fold) and natural cubic spline, out of sample.

| Panel | Folds | Hinge wins | Spline wins | Log-lik. linear | hinge | spline |
|---|---|---|---|---|---|---|
| Seshat Y1 (10×5-fold) | 48 | 0.10 | 0.08 | -0.6036 | -0.6092 | -0.6058 |
| Modern Y456 (3×5 country-blocked folds) | 15 | 0.80 | 0.87 | -0.4096 | -0.4087 | -0.4064 |

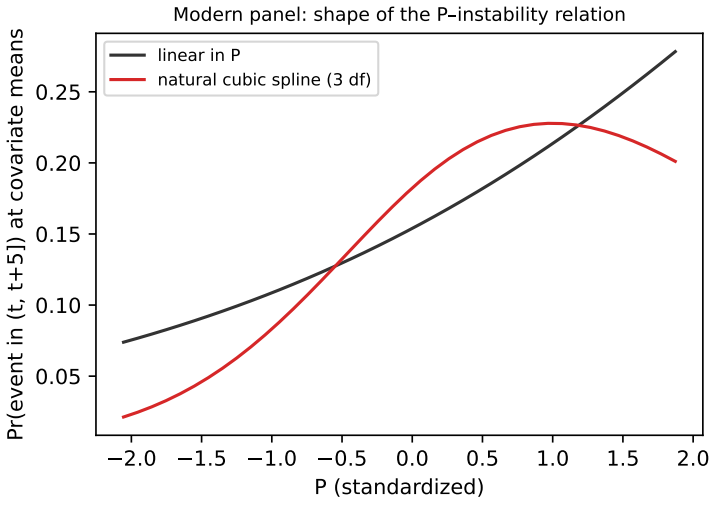



**Figure 6.** Shape of the relation between $P$ and the five-year event probability in the modern panel, at covariate means, linear and spline fits.

## 8. Agent-based experiments

Everything in this section is [SIMULATED]. The simulation exists to measure what the historical record cannot: messages, bits, queues, overhead and failures under institutional regimes that are *imposed* by the experimenter. Nothing emerges in these experiments, and nothing in them is evidence about history; they test the theorems of Section 3 and search for the conditions under which institutions reduce efficiency or add fragility, as the pre-registration requires.

### 8.1. Design

$N$ agents are placed at random in the unit square and linked on a lattice-like nearest-neighbour graph, a small-world rewiring of it, or a random graph of the same degree. Each period, tasks arrive at rate $\lambda = 0.5$ per producing agent; a task is initiated by one agent and requires $k = 3$ participants—the initiator and two network neighbours—to hold the same plan, drawn from a set of $s = 64$ plans with a Zipf distribution of exponent 0 (uniform, entropy 6 bits) or 1 (entropy 4.86 bits). The initiator must communicate the plan. Each agent can process a bounded number of messages per period (2 or 8); excess messages queue first-in-first-out, and a task whose last message is served more than three periods after arrival fails. Agents differ in a continuous "dialect" that is partly spatial; without a shared code, a message is misdecoded with probability proportional to the dialect distance (0.25 per unit), and misdecoded tasks fail. Five regimes are compared. *None*: raw messages of $\lceil \log_2 s \rceil$ bits per recipient, dialect errors, one acknowledgement round. *Norms*: a shared Shannon code optimal for the plan distribution, lower decoding error (0.02), a one-time learning cost of 200 bits per agent, and, after a shift in the plan distribution, a stale code until the institution re-learns it ten periods later. *Records*: norms plus a written record per task (four bits of administrative overhead) against which recipients verify, eliminating decoding errors and the acknowledgement round. *Central*: records plus an eight-ary administrative tree through whose root every task is relayed; administrators have their own bandwidth and are withdrawn from production. *Federated*: the same tree, but a task is relayed only up to the lowest common ancestor of its participants, so that local tasks never reach the root. Administrative bandwidth is set as a ratio (0.5 or 2) of the expected relayed load. Shocks at period 20 of 40 either shift the plan distribution (a cyclic permutation chosen to reach a target Kullback–Leibler divergence; the achieved divergence, 0.52 bits, is recorded) or remove half of the links. The main design crosses $N \in \{50, 200, 800\}$, three networks, two bandwidths, two entropies, three shock conditions and five regimes with eight seeds (6,048 runs); the ablations (584 runs) vary verification, adaptation speed, horizon, learning cost, heterogeneity, administrative capacity, branching and the plan space. Measured outcomes are task success, messages and bits per task, bits per successful task with and without set-up, latency, failure shares by cause, administrative overhead share, welfare (task value minus a cost per bit), the Gini coefficient of benefits among producers, and root load. Entropies, code lengths and divergences are computed because the plan distribution and the code are defined by the experimenter.

### 8.2. H3: institutions lower the cost of coordination—with exceptions

Table 20 pairs each institutional run with the no-institution run of the same cell and seed. Shared norms lower the operating bits per successful task in 99.7% of pairs and raise success in 99.3%, by eliminating most decoding failures between heterogeneous agents; records do so in 71% and

100% of pairs, and raise welfare in every pair. Once set-up costs are included, however, norms are never cheaper than nothing within the forty-period horizon: the coding gain of about one bit per recipient on a six-bit plan space does not repay 200 bits of learning per agent, exactly as Theorem 2 says it should not, and the ablation confirms that the code pays for itself only with a 256-plan space, a Zipf exponent of 1.5 and a 160-period horizon (Table 24, Figure 8). Centralized administration with capacity twice the expected load is indistinguishable from records in success and bits, adds messages (relay), and lowers welfare in half the cells because administrators are withdrawn from production; with capacity at half the expected load it is worse than no institution in 99% of pairs, success falls by 0.44 on average, and welfare by 28 units per agent (Table 21). Federated routing at the same insufficient capacity keeps success higher than no institution in 84% of pairs, because most tasks never reach the root: the root-load bound of Theorem 4 is what separates the two hierarchies (Table 23, Figure 8). Figure 7 shows the scaling with $N$: messages per task rise with the depth of the tree for the hierarchies and are constant for the flat regimes, bits per success are flat in $N$ for all regimes, and inequality of benefits is slightly higher under the hierarchies.

**Table 20.** [SIMULATED] Paired comparison of each institutional regime with the no-institution run of the same cell and seed (main design, 864 pairs per row). Shares of pairs in which the regime has lower bits per successful task (operating; including set-up), higher success and higher welfare, and mean differences.

| Regime | Cap. ratio | Pairs | Lower bits | Lower incl. set-up | Higher success | Higher welfare | Δ bits/success | Δ success | Δ welfare |
|---|---|---|---|---|---|---|---|---|---|
| norms | 2.0 | 864 | 1.00 | 0.00 | 0.99 | 0.90 | -5.5 | +0.032 | +1.5 |
| records | 2.0 | 864 | 0.71 | 0.50 | 1.00 | 1.00 | -43.6 | +0.434 | +25.7 |
| central | 0.5 | 864 | 0.00 | 0.00 | 0.01 | 0.00 | +145.9 | -0.442 | -28.0 |
| central | 2.0 | 864 | 0.72 | 0.50 | 1.00 | 0.50 | -43.1 | +0.414 | +11.6 |
| federated | 0.5 | 864 | 0.61 | 0.44 | 0.83 | 0.47 | -28.6 | +0.272 | +4.7 |
| federated | 2.0 | 864 | 0.72 | 0.50 | 1.00 | 0.50 | -43.1 | +0.414 | +11.6 |

**Table 21.** [SIMULATED] Fragility search: share of the 108 design cells (averaged over seeds) in which the regime has lower welfare, higher bits per success including set-up, or lower success than no institution.

| Regime | Cap. ratio | Cells | Welfare loss | Bits loss | Success loss |
|---|---|---|---|---|---|
| central | 0.5 | 108 | 1.00 | 1.00 | 1.00 |
| central | 2.0 | 108 | 0.50 | 0.50 | 0.00 |
| federated | 0.5 | 108 | 0.56 | 0.56 | 0.22 |
| federated | 2.0 | 108 | 0.50 | 0.50 | 0.00 |
| norms | 2.0 | 108 | 0.06 | 1.00 | 0.00 |
| records | 2.0 | 108 | 0.00 | 0.50 | 0.00 |

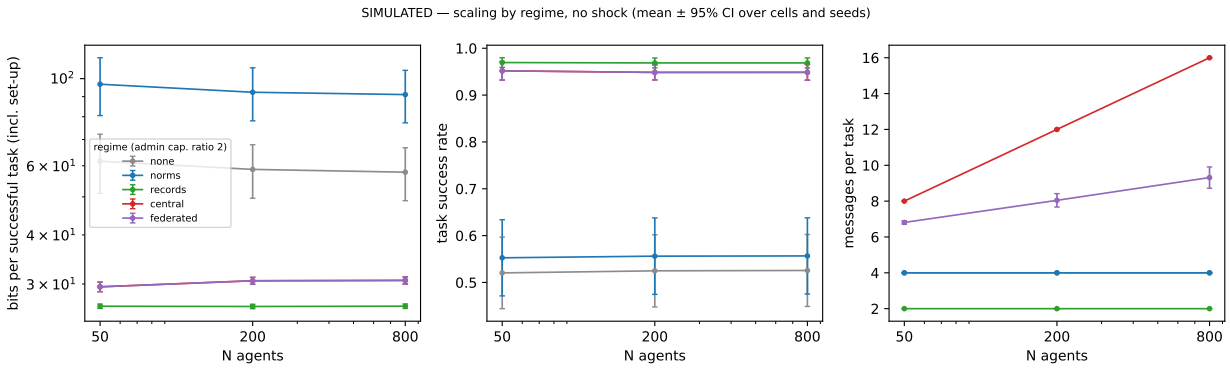



**Figure 7.** [SIMULATED] Scaling by regime without shocks (administrative capacity ratio 2): bits per successful task including set-up (log scale), success rate and messages per task against $N$; means and 95% confidence intervals over cells and seeds.

### 8.3. Theorems in silico

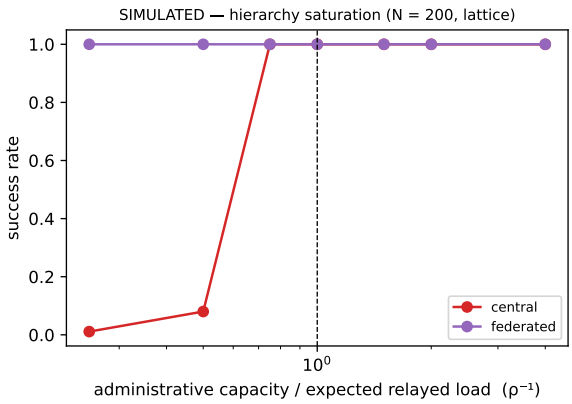
The coding-gain sweep reproduces Theorem 1 cell by cell: the expected code length lies in $[H(p), H(p)+1)$ for every plan space and exponent, and the code is shorter than raw description whenever $H(p) \leq \log_2 s - 1$; in the one cell where the distribution is nearly uniform (exponent 0.5, entropy 5.78 bits) the Shannon code is *longer* than the six raw bits (6.24), which is the exception the theorem allows and a reminder that standardization is not free when there is little regularity to exploit. After a distribution shock, a non-adapting code costs 1.90 extra bits per recipient against a divergence of 1.98 bits, within the one-bit rounding of Theorem 3; a code that re-learns after ten periods pays half of that, and an instantly adapting code almost nothing (Table 24). Because decoding success does not depend on which plan was drawn, distribution shocks raise cost without lowering success in this model (Table 22); link removal, by contrast, concentrates load on surviving neighbours and lowers success by about 0.08 under the hierarchies and records and by almost nothing under flat regimes (difference-in-differences against the unshocked run of the same cell). The capacity sweep traces the saturation of Theorem 5: central success collapses as the capacity ratio falls below one, while federated routing degrades gracefully (Figure 8).

**Table 22.** [SIMULATED] Shock responses by regime: difference-in-differences of success and of bits per task (post minus pre, shocked minus unshocked run of the same cell and seed); standard error of the mean in parentheses; achieved divergence for distribution shocks.

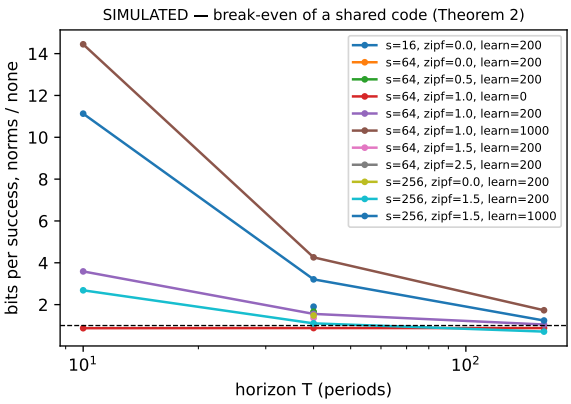
| Shock | Regime | Cap. ratio | Δ success (SE) | Δ bits/task | $D_{KL}(q\|p)$ |
|---|---|---|---|---|---|
| distribution | central | 0.5 | +0.000 (0.000) | +0.523 | 0.515 |
| distribution | central | 2.0 | +0.000 (0.000) | +0.523 | 0.515 |
| distribution | federated | 0.5 | +0.000 (0.000) | +0.523 | 0.515 |
| distribution | federated | 2.0 | +0.000 (0.000) | +0.523 | 0.515 |
| distribution | none | 2.0 | +0.000 (0.000) | +0.000 | 0.515 |
| distribution | norms | 2.0 | +0.000 (0.000) | +0.515 | 0.515 |
| distribution | records | 2.0 | +0.000 (0.000) | +0.524 | 0.515 |
| network | central | 0.5 | +0.000 (0.000) | +0.012 | 0.000 |
| network | central | 2.0 | -0.076 (0.006) | +0.012 | 0.000 |
| network | federated | 0.5 | -0.080 (0.012) | +0.012 | 0.000 |
| network | federated | 2.0 | -0.076 (0.006) | +0.012 | 0.000 |
| network | none | 2.0 | -0.004 (0.001) | +0.000 | 0.000 |
| network | norms | 2.0 | -0.004 (0.001) | +0.001 | 0.000 |
| network | records | 2.0 | -0.070 (0.005) | -0.003 | 0.000 |

**Table 23.** [SIMULATED] Hierarchy saturation ($N = 200$, lattice): success, latency failures, mean latency and root load against the administrative capacity ratio.

| Regime | Cap. ratio | Success | Latency failures | Latency | Root load |
|---|---|---|---|---|---|
| central | 0.25 | 0.011 | 0.989 | 1.50 | 4261 |
| central | 0.50 | 0.080 | 0.920 | 2.57 | 2311 |
| central | 0.75 | 1.000 | 0.000 | 1.99 | 418 |
| central | 1.00 | 1.000 | 0.000 | 1.01 | 302 |
| central | 1.50 | 1.000 | 0.000 | 1.00 | 302 |
| central | 2.00 | 1.000 | 0.000 | 1.00 | 302 |
| central | 4.00 | 1.000 | 0.000 | 1.00 | 302 |
| federated | 0.25 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 0.50 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 0.75 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 1.00 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 1.50 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 2.00 | 1.000 | 0.000 | 1.00 | 42 |
| federated | 4.00 | 1.000 | 0.000 | 1.00 | 42 |




**(a)** Saturation of hierarchies.




**(b)** Break-even of a shared code.

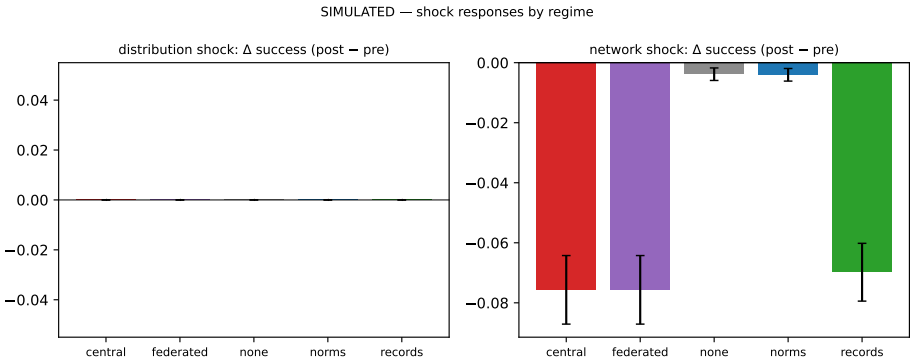



**Figure 8.** [SIMULATED] Phase diagram of success against administrative capacity (left), ratio of bits per success with and without a shared code against the horizon (right), and difference-in-differences shock responses (bottom).

**Table 24.** [SIMULATED] Ablations ($N = 200$, lattice, eight seeds) and theorem checks.

| Ablation | Result |
|---|---|
| Reference (N=200, lattice, no shock): success / bits per success / welfare per agent | central: 1.000 / 28.1 / 44.4; federated: 1.000 / 28.1 / 44.4; none: 0.918 / 15.3 / 55.0; norms: 0.960 / 23.7 / 57.3; records: 1.000 / 24.8 / 59.3 |
| Records without verification | success 0.960, bits per success 25.8, welfare 57.2 |
| Norms, extra bits per task after a distribution shock by adaptation delay | delay 0: +0.03; delay 10: +1.92; delay 1000: +3.79 (KL = 1.98 bits) |
| Break-even (norms cheaper than none incl. set-up) | s=16,zipf=0.0,T=40,learn=200.0: no; s=64,zipf=0.0,T=40,learn=200.0: no; s=64,zipf=0.5,T=40,learn=200.0: no; s=64,zipf=1.0,T=10,learn=0.0: yes; s=64,zipf=1.0,T=10,learn=200.0: no; s=64,zipf=1.0,T=10,learn=1000.0: no; s=64,zipf=1.0,T=40,learn=0.0: yes; s=64,zipf=1.0,T=40,learn=200.0: no; s=64,zipf=1.0,T=40,learn=1000.0: no; s=64,zipf=1.0,T=160,learn=0.0: yes; s=64,zipf=1.0,T=160,learn=200.0: no; s=64,zipf=1.0,T=160,learn=1000.0: no; s=64,zipf=1.5,T=40,learn=200.0: no; s=64,zipf=2.5,T=40,learn=200.0: no; s=256,zipf=0.0,T=40,learn=200.0: no; s=256,zipf=1.5,T=10,learn=200.0: no; s=256,zipf=1.5,T=10,learn=1000.0: no; s=256,zipf=1.5,T=40,learn=200.0: no; s=256,zipf=1.5,T=40,learn=1000.0: no; s=256,zipf=1.5,T=160,learn=200.0: yes; s=256,zipf=1.5,T=160,learn=1000.0: no |
| Heterogeneity $\varepsilon$: success none vs norms | eps=0.05,none: 0.984; eps=0.05,norms: 0.960; eps=0.25,none: 0.918; eps=0.25,norms: 0.960; eps=0.6,none: 0.808; eps=0.6,norms: 0.960 |
| Federated branching $b$: success / messages per task / root load | b=4: 1.00 / 8.86 / 48; b=8: 1.00 / 6.61 / 42; b=16: 1.00 / 6.27 / 208 |
| Coding gain (norms): $H(p)$, $L_{code}$, $L_{raw}$ | s=16,zipf=0.0: 4.00, 4.00, 4; s=64,zipf=0.0: 6.00, 6.00, 6; s=64,zipf=0.5: 5.78, 6.24, 6; s=64,zipf=1.0: 4.86, 5.44, 6; s=64,zipf=1.5: 3.44, 4.00, 6; s=64,zipf=2.5: 1.45, 1.96, 6; s=256,zipf=0.0: 8.00, 8.00, 8; s=256,zipf=1.5: 3.92, 4.44, 8 |
| Theorem checks | T1 ($H \le L < H+1$): True; code shorter than raw whenever $H \le \log_2 s - 1$: True; T3 extra bits per recipient 1.90 vs KL 1.98 |

### 8.4. What the experiments say about the hypothesis

H3 is supported in the operating sense and refuted in the total-cost sense within short horizons, which is the content of Theorem 2; the experiments also show that the benefit of institutions in this model comes at least as much from eliminating misunderstanding between heterogeneous agents as from shortening messages, since the coding gain on a six-bit plan space is small while the decoding

gain is large (with heterogeneity at 0.6, success is 0.81 without a code and 0.96 with one; with heterogeneity at 0.05 the ranking reverses, 0.98 against 0.96, because the code's own error rate is then the larger one). The fragility search found what the framework predicts: institutions hurt when their fixed cost is not amortized, when a centralized hierarchy is asked to relay more than it can process, and when there is too little regularity for a code to compress. None of these cases is exotic. All of them are described in the historical literature on over-extended administrations and rigid bureaucracies, and none of them is measurable with the indicators available in Seshat.

## 9. Synthesis: theory, data, and rival explanations

### 9.1. What is observable and what is not

Table 25 sets the framework's mechanisms against the two kinds of evidence. The coding gain, the stale-code penalty and the routing bounds are measured in the simulation and are not observable in Seshat, which records whether writing, coins or courts existed and never how long a message was or how many tasks waited. The break-even condition is reflected in the historical record only through the association of capacity with scale (H1), which is consistent with institutions being built where they pay but equally consistent with the reverse. Backlog dynamics are observable nowhere in history. The one mechanism that the historical tests address, sustained overload leading to breakdown (Conjecture 2), is addressed only through the proxy $P$, and the proxy's claim to track utilization (Conjecture 1) is what the collinearity and placebo results undermine.

**Table 25.** Mechanisms of the framework against the two kinds of evidence.

| Mechanism | Historical record ([EMPIRICAL]) | Simulation ([SIMULATED]) |
|---|---|---|
| Coding gain (Thm. 1) | Not observable: presence of codes, not message lengths | Reproduced in every cell; one cell where the code is longer than raw |
| Break-even (Thm. 2) | Indirect: capacity rises with scale (H1) | Code repaid only with large concentrated plan space and long horizon |
| Stale code (Thm. 3) | Not observable | Extra bits 1.90 vs divergence 1.98 |
| Routing and root load (Thm. 4) | Partially: administrative levels coded, loads not | Central collapses below capacity ratio 1; federated degrades gracefully |
| Backlog (Thm. 5) | Not observable; $P$ is a proxy under C1 | By construction |
| Overload $\rightarrow$ breakdown (C2) | Tested through $P$: positive but not beyond components | Not modelled |
| Smooth vs threshold (C3) | No threshold in Seshat; negligible nonlinearity in modern panel | Convex latency in utilization |

### 9.2. The model against simpler rivals

The pre-registration named the rivals and the results rank them. In Seshat, a scale-only model explains crisis severity as well as the mismatch model, in and out of sample; the placebo built from production infrastructure does as well; capacity alone does nothing. The simplest reading is Johnson's: larger polities face more that can go wrong when a crisis strikes, and whether their administrations were "adequate" is not something the presence codes can tell us. In the modern panel, a capacity-only model explains instability onsets as well as or better than the mismatch model; so does development; population adds little. The simplest reading is the state-capacity literature's (Besley and Persson, 2009; Hanson and Sigman, 2021; Goldstone et al., 2009): states with impartial administrations, predictable law and fiscal and territorial reach experience fewer civil wars, coups and irregular transitions, and the mismatch index inherits that association with its sign reversed. In neither panel does the *difference* between scale and capacity carry information that its components do not.

The rival mechanisms named in the mandate fare as follows. Military competition: the warfare-technology index is included as a covariate in every specification and does not displace the scale association; it is not itself associated with severity once scale is in the model. Environmental shocks: an epidemic accompanying the crisis is a covariate; the climate-mediated pathway of

Hoyer et al. (2023) is not testable with Polaris alone. Geography: absorbed by region effects, and the leave-one-region-out results show no region driving the estimates. Resource base: the production-infrastructure index is a covariate and, used as a placebo, mimics capacity. Inequality and elite conflict: not measurable at the polity level in Polaris and only through regime-type proxies in V-Dem; the structural-demographic mechanism (Turchin, 2004; Turchin and Nefedov, 2009) remains an open rival that this paper cannot distinguish from scale. Cultural diffusion and selection: addressed by region and family controls for inference and by the crisis-selection caveat, not for identification. Coercion: the military professionalization index is a covariate in H1 and was not associated with severity.

### 9.3. Consistency with simulation-derived predictions

The simulation predicts that institutions raise success and lower operating cost per successful task, which agrees with the positive association of capacity with scale and, in the modern panel, with the negative association of capacity with instability; agreement of this kind is weak evidence because the data cannot distinguish capacity enabling scale from scale demanding capacity. The simulation also predicts a sharp utilization effect—hierarchies fail when relayed load exceeds administrative capacity—whose historical analogue would be a threshold in the mismatch index. The historical tests find no threshold in Seshat and a negligible nonlinearity in the modern panel. Either there is no utilization threshold in history, or the proxy does not measure utilization; the collinearity of the proxy's components makes the second reading at least as likely, and the paper cannot tell them apart. Disagreement is more informative than agreement here: it marks the precise place where better data would be decisive.

### 9.4. Scope conditions and failed predictions

Three statements survive, each with its scope. First, capacity and scale are positively associated across 317 historical polities, 579 ethnographic societies and 14,647 modern country-years, with the sign stable within regions and eras and over fifty-year differences; the direction is not identified. Second, among the 127 historical crises with a defined pre-crisis index, a larger polity's crisis is recorded with more adverse consequences, at about 0.26–0.34 log-odds per standard deviation of scale; this is a statement about scale, not about mismatch. Third, modern states with higher administrative, legal, fiscal and territorial capacity experience fewer instability onsets in the following five years, at about $-0.63$ log-odds per standard deviation; this replicates a known result and is as strongly related to past as to future instability. Four predictions failed. The mismatch index does not outperform its components out of sample in either panel. The placebo index performs comparably in Seshat. The negative control in the modern panel is as strong as the forward association. There is no

threshold. By the rule written before the data were examined, the universal theory of coordination, institutions and civilizational complexity is not supported by the historical and modern evidence available in 2026, and the paper is written as the test that did not confirm it.

## 10. Limitations

The limitations were written down during the analysis and before this section; the full list is in the replication package. The ones that bear on how the results should be read are these.

**Data.** The CrisisDB outcome is crisis-selected (150 of 626 polities), so the Seshat test concerns the severity of recorded crises, not their occurrence. Only 6% of Polaris facts are dated, so pre-crisis capacity is in practice whole-polity capacity and temporal precedence is weak; a dated-only test was not estimable. Seshat capacity items record the existence of institutions, not their reach, quality or cost, and the demand side is scale only because the databank has no usable indicator of division of labour or dispersion. Institutional codes and crisis codes come from the same project and overlapping historiography, which permits circularity that the inferred-code and late-crisis sensitivities can only partly address. Seshat polity boundaries are periodization decisions, so polity duration is not an instability outcome. In the modern panel, the six capacity items correlate 0.75 with electoral democracy and are rated by experts who know what came later; population begins in 1820 for most countries and there is no dispersion measure; 39 V-Dem entities without Maddison population drop out. The Ethnographic Atlas is a cross-section with coarse ordinal community sizes and no outcome.

**Design.** Nothing is identified causally. The negative control shows that the modern mismatch is associated with past instability as strongly as with future instability; the Oster adjustment crosses zero and the robustness value is 0.07. The Seshat binary outcome was underpowered in advance. There are 42 regions and ten macro-regions, so cluster-robust inference is anti-conservative and alternative clusterings are reported. The verdict on H2 rests on a pre-registered out-of-sample rule against baselines, which is the right test of "beyond conventional baselines" but is also a demanding one at $n = 127$; we report the fold-level results so that readers who prefer a different rule can apply it. The Equinox-2020 replication listed in the pre-registration was not executed and is recorded as a blocked task in the audit.

**Theory.** The theorems describe an idealized communication system in which capacity is exogenous; historically it is built in response to demand. The conjectures that link the framework to the proxies are exactly what the measurement results call into question: with scale and capacity

correlated 0.84, the proxy $P$ retains 17% of their joint variance, and a residual of that kind cannot be assumed to track utilization.

**Simulation.** Regimes are imposed; the model says nothing about emergence. Bit and message accounting is stylized, and only within-simulation comparisons are meaningful. Decoding success does not depend on the plan drawn, so distribution shocks affect cost but not success by construction. The saturation of hierarchies follows from a free parameter swept across its range; it is confirmed, not discovered. Shannon rather than Huffman codes are used so that the stale-code penalty is comparable to the theorem.

**Process.** The study was executed autonomously by a language-model agent under a human mandate in a single session: the pre-registration, code, analyses and text were produced by the agent, with the mandate's prohibitions on fabrication, imputation of missing data as zero and unapproved spending enforced by the execution log and the replication package. The principal investigator has not yet reviewed the code or the text at the time of writing; three adversarial reviews commissioned from independent reviewer personas and the responses to them are included in the package. Reference verification relied on Crossref metadata and primary-source abstracts; the chapter contents of Johnson (1982) and the published tables of Sng (2014) were not read.

## 11. Conclusion

We set out to state a universal theory of human coordination in a form that could be tested, and to test it. The statement survived: institutions as shared codes, records and routing structures that lower the communication cost per coordinated task are a coherent object, with five provable properties and a simulable cost structure, and the simulations show both the gains and the three ways institutions can hurt—unpaid set-up costs, saturated hierarchies, and task environments with too little regularity to compress. The test did not confirm the theory's empirical corollary. In the Seshat databank and in a two-century country-year panel, the mismatch between coordination demand and coordination capacity is associated with instability in the predicted direction, robustly across regions and eras, and yet it adds nothing identifiable to what scale alone (in history) or capacity alone (in modern states) already explain; a placebo does as well, past instability loads on the index as strongly as future instability, and the coefficient does not survive a standard adjustment for unobservables. Under the rule we committed to before looking, the universal framing is not supported, and we have written the narrower paper instead.

The result should be read for what it says about measurement as much as about the hypothesis. Scale and capacity are so tightly coupled across the historical record that their difference is a

residual carrying one sixth of their joint variance; a mismatch index built from presence codes cannot track utilization in the sense the theory requires, and the data that would—message volumes, administrative workloads, backlogs, the reach rather than the existence of institutions—are not in any databank we know of. The one historical case in which such quantities have been reconstructed, Sng's late-imperial China, found the effect of distance on fiscal reach that the framework predicts. A credible next test would need either more cases of that kind, or a source of variation in demand or capacity unrelated to local instability, or a historical databank that codes institutional reach and administrative load rather than presence. Until then, the honest summary is that capacity matters, scale matters, and that whether their mismatch matters is a question the available evidence cannot answer.

Three things are offered for reuse: a pre-registration and measurement model for Seshat that treats missing as missing and carries ranges, dates and latent-score uncertainty through to inference; a validated bibliography and novelty matrix for the strands that converge on this hypothesis; and a factorial agent-based testbed in which the mechanisms are measurable. The replication package reproduces every number in this paper from the raw downloads with one command.

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# Appendices

## A. Pre-registration summary

The analysis plan (`research/preregistration.md`, SHA-256 `ecda8804e17f9317...`) was frozen on 2026-11-01 before any association between a predictor and an outcome was computed. It fixes: the five hypotheses and their falsifiers; the units (polity, society, country-year) and the reasons for them (93.7% of Polaris facts undated; CrisisDB crisis-selected); the disjoint indicator sets for $D$ and $K$ listed in Section 4.3; the outcomes $Y_1$ (severity share), $Y_{1b}$, $Y_2$ (duration), $Y_3$ (succession), $Y_4$–$Y_6$ (modern events) and the composite; the five-year horizon; the models of Section 5; the baselines and the 80% fold rule; the Holm correction over three primary tests; the a-priori power computation and its decision rule; the robustness set; and the rule for narrowing the claim. Seven decisions and no post-hoc amendments are recorded in `research/decisions.md`; the single deviation from the mandate's candidate formula ($K$ standardized rather than logged, because it is a bounded share that can be zero) was recorded before analysis. The use of decade rather than year effects in the forward-chaining folds, so that test years have a fitted level, is noted in the code and in Section 5.4.

## B. Data dictionary (abridged)

**Seshat facts** (56,274 rows): sheet, section, region, polity, variable, value (presence codes P/IP $\to$ 1, A/IA $\to$ 0, disputed $\to$ 0.5, SU/blank $\to$ missing; numeric midpoints with bounds), date interval, flags for dated, inferred, disputed, unparsed, uncertain. **Polity aggregates**: mean, min, max, mean log, bound extremes, counts of facts, dated facts, inferred and disputed facts, per polity and variable; a pre-crisis version per CrisisDB record. **Polity metadata**: region, macro-region (ten groups fixed in `configs/regions.yaml`), era block (six), start and end years, midpoint, duration, censoring at 1900, centralization, relationships to preceding and succeeding entities, matched successor and its relationship, discontinuity indicator. **CrisisDB**: record, polity, start and end years, the 21 consequence codes, the number of instability items coded, the number present, the severity share, the binary severe indicator (defined when the five severe items are all coded), the reform share and the epidemic flag. **Ethnographic Atlas**: society, coordinates, Glottocode and Glottolog family, region, focal year, EA030–EA034, EA042, EA066–EA068, EA070, EA072–EA073, EA090, EA005, EA028, EA113, community-size midpoint, population, jurisdictional levels. **Modern panel**:

V-Dem identifiers and year, six capacity items, electoral democracy, regime identifiers and end types, civil war, coups, population and GDP per capita from Maddison (ISO3 merge; 39 unmatched V-Dem entities listed in the crosswalk file), derived event indicators and windows. The full dictionary with counts is `data/metadata/data_dictionary.md`.

## C. Additional measurement detail

Item parameters of the two-parameter model are listed in `results/empirical/measurement_irt_item_parameters.csv`. Five record-keeping items (written record, script, lists and tables, calendar, practical literature) reach the discrimination ceiling because they are coded almost jointly in Seshat; the latent score is therefore dominated by the writing cluster, which is why the transparent block mean is the primary index. Figure 9 compares item difficulties estimated on the Old World and on the Americas and Oceania. Figure 10 shows the mismatch index over time by macro-region and the raw relation between pre-crisis mismatch and crisis severity.

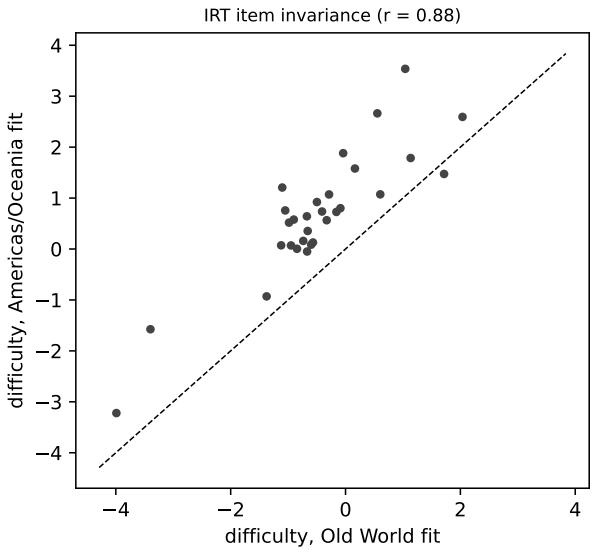



**Figure 9.** Item difficulties from two-parameter models fitted separately on Old-World polities and on the Americas and Oceania.

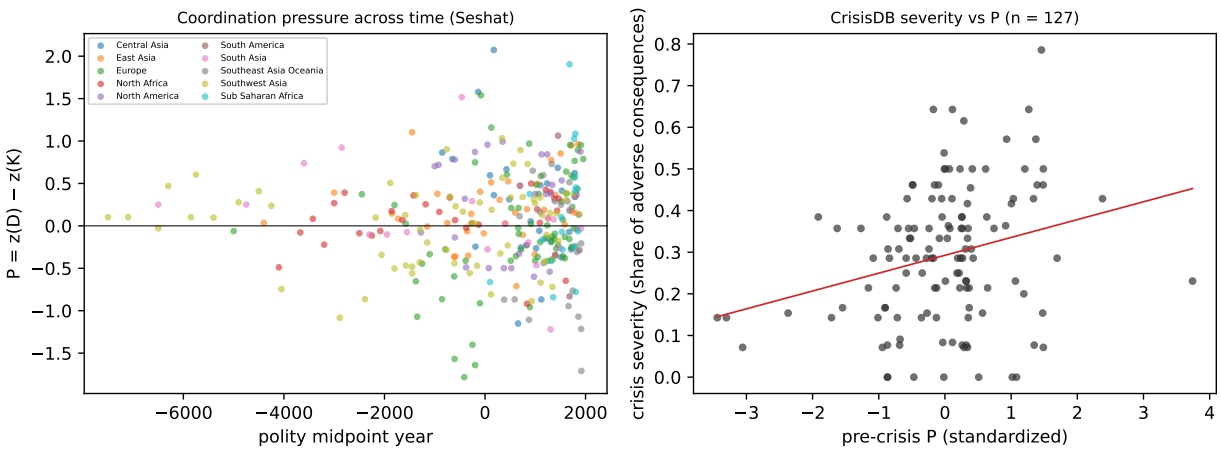



**Figure 10.** Left: mismatch index $P$ against polity midpoint year, by macro-region. Right: crisis severity against pre-crisis $P$ for the 127 CrisisDB records with a defined index, with the least-squares line.

## D. Simulation scaling table

**Table 26.** [SIMULATED] Means by regime and $N$ over all main-design cells without shocks (administrative capacity ratio 2).

| Regime | $N$ | Success | Msgs/task | Bits/task | Bits/success (set-up) | Welfare/agent | Gini |
|---|---|---|---|---|---|---|---|
| central | 50 | 0.939 | 8.00 | 15.5 | 30.1 | 45.0 | 0.145 |
| central | 200 | 0.936 | 12.00 | 15.5 | 31.1 | 41.3 | 0.157 |
| central | 800 | 0.937 | 16.00 | 15.5 | 31.2 | 41.2 | 0.158 |
| federated | 50 | 0.939 | 6.81 | 15.5 | 30.1 | 45.0 | 0.145 |
| federated | 200 | 0.936 | 8.06 | 15.5 | 31.1 | 41.3 | 0.157 |
| federated | 800 | 0.937 | 9.34 | 15.5 | 31.2 | 41.2 | 0.158 |
| none | 50 | 0.520 | 4.00 | 14.0 | 62.4 | 30.7 | 0.224 |
| none | 200 | 0.524 | 4.00 | 14.0 | 59.1 | 31.0 | 0.224 |
| none | 800 | 0.525 | 4.00 | 14.0 | 58.1 | 30.9 | 0.222 |
| norms | 50 | 0.552 | 4.00 | 13.5 | 98.4 | 32.3 | 0.222 |
| norms | 200 | 0.555 | 4.00 | 13.5 | 93.1 | 32.5 | 0.220 |
| norms | 800 | 0.556 | 4.00 | 13.5 | 92.0 | 32.3 | 0.219 |
| records | 50 | 0.957 | 2.00 | 15.5 | 26.8 | 56.5 | 0.133 |
| records | 200 | 0.957 | 2.00 | 15.5 | 26.8 | 56.6 | 0.134 |
| records | 800 | 0.958 | 2.00 | 15.5 | 26.8 | 56.5 | 0.134 |

## E. Replication guide

The package is a Python 3.12 project with pinned dependencies. From a clean clone:

```
uv venv -p 3.12 .venv && uv pip install -p .venv/bin/python -e .
make fetch     # 9 raw sources, SHA-256 verified
make data      # three panels + diagnostics
make measure   # D, K, P, IRT, invariance, power
make econ      # H1, H2, H4, H5, robustness
make sim       # 6,048 + 584 runs and analysis
make figures   # all tables and figures
make paper     # latexmk -> paper/main.pdf
make test      # unit and integration tests
```

Seeds are fixed (20261101 for the estimation code, 0–7 for the simulations); raw files are not redistributed because of the D-PLACE and V-Dem licences, but the registry allows byte-level verification of a fresh download. The execution log (results/logs/execution_log.md) records every command and every correction made during the study, and the final audit (results/logs/final_research_audit.md) lists completed, failed and blocked tasks.