# Cohort Effects of Democratization on Diffuse Regime Support and Emancipative Values

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June 3, 2026

**Abstract**

Does coming of age under democracy durably shape political attitudes? We test the formative-window hypothesis using birth-cohort variation in democratic exposure across first-transition countries in the World Values Survey and Latinobarómetro, identifying effects from cross-country differences in transition timing. On regime support we find no evidence that formative exposure raises support, an informative null whose equivalence bounds rule out effects of substantively meaningful size within this design. On emancipative values—the Inglehart-Welzel tradition's signature formative outcome—an apparent cohort gradient is not identified as formative: it appears among never-exposed cohorts, shows no discontinuity at transition, and persists where exposure is fixed at its maximum and formation cannot operate. It is observationally equivalent to a secular cohort trend correlated with transition timing. A country-specific cohort-trend control absorbs most of the gradient while leaving the regime null intact. The formative-window account of emancipative change is not recoverable from cross-country observational variation.

# 1 Introduction

A citizen who came of age under democracy: does she support the democratic regime more strongly than one who came of age under authoritarian rule? The intuition is straightforward, but the comparative-attitudes literature has not converged on an answer. Three influential research programs have offered evidence pointing in different directions, and the resulting impression of disagreement has lingered for two decades.

The Inglehart–Welzel modernization tradition holds that exposure to stable, materially secure conditions during a critical formative window — canonically ages 16 to 25 — imprints emancipative values that persist into adulthood (**???**). The empirical referent is robust: across decades of the World Values Survey, cohorts formed under democracy report systematically higher tolerance toward minorities, greater support for women's rights, and broader endorsement of personal autonomy than cohorts formed under authoritarianism. Extending this logic to attitudes toward the regime itself — diffuse support for the political system, satisfaction with democracy, confidence in its representative institutions — yields the natural prediction that the same formative window should also shape support for the regime.

The post-communist transitions literature returned an opposing verdict. **???**, analyzing diffuse support for democracy in Central and Eastern Europe, argue that it is shaped predominantly by contemporary evaluations of government and economic performance rather than by formative exposure. **?** apply an age-period-cohort analysis to eighteen consolidated European democracies and document a nuanced version of the same pattern: broad endorsement of democracy as a system of government holds steady across cohorts, while country-specific evidence indicates erosion in the liberal components of that support among younger cohorts.

A third strand, identified with **?** at the country level and **?** at the individual level, finds evidence consistent with a different formation mechanism. Comparing East and West Germans after reunification, Fuchs-Schündeln and Schündeln document that *cumulative* exposure to democracy across the life course modulates political preferences, including support for the democratic regime. The relevant treatment, under this view, is not the formative window but the stock of years lived under democratic institutions.

These three positions do not obviously reconcile, and the disagreement has persisted in part because each program tested a different combination of attitude class and exposure regime on different data. We bring the formative-window prediction common to them to a single cross-country research design and ask, for each attitude class, whether the cohort effect it implies is recoverable from that variation.

**This paper.** We estimate cohort effects of democratization on individual attitudes using a formative-window specification (years aged 16–25 spent under democracy) with country-by-survey-wave fixed effects and birth-year fixed effects, clustered by country with wild cluster bootstrap inference. The country-by-wave effects absorb every shock common to a country in a given survey wave. Identification comes from the differential overlap between birth cohorts' age-16-to-25 windows and country-specific transition timing: within a country-wave cell, cohorts differ in formative exposure, and the birth-year fixed effects then compare those cohort differences across countries that transitioned at different dates. We restrict throughout to completed formative windows (respondents aged 26 and over), for whom exposure is well-defined rather than projected. Two attitude classes are kept distinct: *diffuse support for the political regime* (the Churchill question, satisfaction with democracy, and an index of confidence in representative institutions) and *personal emancipative values* (tolerance toward homosexuality, the most canonical Inglehart-Welzel item).

The design is applied to three samples: Latinobarómetro across the fourteen first-transition Latin American countries surveyed between 1995 and 2023, the World Values Survey restricted to those same fourteen countries, and the WVS global sample of fifty countries with V-Dem-identified first transitions to democracy between 1981 and the present (approximately 142,000 respondents on the canonical regime-support item under the completed-window restriction). Transition dates are derived from the V-Dem Core Dataset v15 (**?**).

The paper's more striking result is what it does to the canonical case. The emancipative cohort gradient that the Inglehart-Welzel tradition treats as its cleanest formative referent — the result one would expect to confirm most easily — is not recoverable as a formative effect from cross-country variation. The regime-support side, by contrast, yields a clean and informative null, one that confirms a prediction the post-communist literature had already advanced. We present the identified null first, as the cleaner result, and then show why the emancipative gradient is not identified. The formative-window specification returns a null on regime support independently in all three samples: in Latinobarómetro the estimated effects on support for democracy, on satisfaction with democracy, and on the institutional-confidence index are each indistinguishable from zero, and the same null appears in the World Values Survey, both in its Latin American subset and in the global sample of fifty first-transition countries, on the corresponding democratic-system and Churchill items.

This null is informative rather than merely inconclusive. In the two well-powered samples the smallest effect the design could reliably have detected lies below one percent of an outcome standard deviation per year of exposure, and an equivalence test places every regime-support estimate inside a band narrower than the per-year magnitude the comparative literature treats as substantive — the cumulative-exposure scale reported by **?**, who study a different mechanism, used only as a reference for what counts as a recoverable effect, on our own per-year standard-deviation conversion rather than as the effect the null rules out. The null further survives a country-specific cohort-trend control in both well-powered samples.

The same specification returns an apparent positive gradient on tolerance toward homosexuality. This gradient, however, is not identified as a formative effect, and three features of its shape show why. It is present among cohorts whose formative window closed entirely before any democratic transition, who experienced no formative democracy at all; it shows no discontinuity at the transition, rising smoothly across cohorts regardless of exposure;

and it persists undiminished among the cohorts for whom formative exposure is fixed at its ten-year maximum, where a formative mechanism can no longer move it. The gradient is, in short, observationally equivalent to a secular cohort trend correlated with transition timing, which a cross-country design cannot separate from formation. Tellingly, the same cohort-trend control that absorbs most of the apparent values gradient leaves the regime null intact in both well-powered samples — the identification asymmetry on which our reading turns.

A cumulative-exposure specification does not recover the Fuchs-Schündeln pattern in our cross-country observational variation: the cumulative coefficient is directionally positive to null across samples, and the post-communist bloc's marginal positive signal on the Churchill item does not survive leave-one-out or a continuous gradient test under wild cluster bootstrap. We report this in robustness rather than as a finding, and do not read our cross-country observational results as refuting the cumulative mechanism that Fuchs-Schündeln and Schündeln identify via the German natural experiment.

**Contribution.** The contribution is twofold. First, an informative null: we find no evidence that formative-window exposure to democracy builds support for the political regime, in three independent samples, with power to detect effects of the magnitude the literature treats as substantive, and robust to country-specific cohort trends. Second, a non-identification result: the canonical Inglehart-Welzel emancipative cohort effect is not recoverable from cross-country observational variation, being observationally equivalent to a secular cohort trend correlated with transition timing. The two are bound by a single diagnostic — the country-specific cohort-trend control that absorbs most of the emancipative gradient leaves the regime null intact. That the null survives this control is evidence it reflects an informative null within this design rather than an insensitive estimator. That the same control absorbs most of the emancipative gradient is consistent with, though not proof of, a secular rather than formative source. The two outcomes correspond to **??**'s distinction between diffuse regime support and personal values, but the paper's spine is identification — what cohort variation can and cannot recover from cross-country data — rather than a theory of how attitude classes differ in their formation.

The remainder of the paper is organized as follows. Section 2 develops the theoretical framework and locates the two attitude classes in the existing literature. Section 3 describes the Latinobarómetro and WVS samples and the construction of exposure variables from V-Dem transition dates. Section 4 sets out the formative and cumulative specifications, the cross-country age-period-cohort identification assumption, and the inference procedure used for the small-cluster setting. Section 5 reports the regime-support null with its minimum-detectable-effect and equivalence analysis, then the emancipative gradient and the event-study and secular-decomposition evidence that it is not identified as formative. Section 7 discusses what cross-country variation can and cannot recover and notes limitations. Section 8 concludes.

# 2 Theoretical framework: two axes for an apparent puzzle

The apparent disagreement among Inglehart-Welzel, Mishler-Rose, and Fuchs-Schündeln is in part a disagreement about different objects. Two distinctions organize them. The first, due to Easton, separates attitudes toward the political regime from personal-dispositional values. The second separates two mechanisms of formation: a formative window with sharply weighted impressionable years versus cumulative exposure throughout the life course. We do not use these distinctions to settle the traditions into a reconciling matrix. We use them to pose a single question — under a common cross-country research design, is the formative-window cohort effect each tradition implies *recoverable* for each attitude class? — and the answer differs by class in a way the paper traces to identification, not to a theory of differential formation.

## 2.1 Axis 1: Easton's diffuse–specific distinction

**??** introduced the now-canonical distinction between two forms of political support. *Diffuse support* refers to generalized adherence to the regime and to its constitutive principles. The proposition that "democracy may have its problems, but it is better than any other form of government" is the textbook diffuse-support item: it asks for an evaluation of the regime as such, not of any particular government. *Specific support* refers to evaluation of concrete performance, of authorities in office, and of policies enacted. Satisfaction with how democracy functions in one's country, confidence in the current government, and approval of particular policies sit on the specific end of the continuum.

The items that we measure in Latinobarómetro and WVS sit at various points along this continuum, but they sit clearly on the regime side of a second, prior distinction. Support for democracy as a system (the Churchill-type Latinobarómetro item; WVS E117), confidence in representative institutions such as the Congress or political parties, and satisfaction with democracy are all measurements of attitudes *toward the political system*, even if they vary in how diffuse or specific they are. They contrast with attitudes that are about the individual's own dispositions and values, not about the political system: tolerance toward homosexuality, justification of divorce or abortion, attitudes about gender roles. These latter items measure what **?** call emancipative values.

The distinction matters for what follows because the two classes of attitudes can plausibly follow different formation regimes. Aggregating them into a single index of "democratic values" — a common move in the older comparative literature — obscures this possibility.

## 2.2 Axis 2: Formative window versus cumulative exposure

Two distinct mechanisms by which exposure to the political regime might shape individual attitudes have been formalized in the literature.

The *formative-window* mechanism descends from **?**'s argument that political consciousness is formed in late adolescence and early adulthood, and from **?**'s empirical operationalization that locates the critical window at ages 16 to 25. Under this hypothesis, exposure to democracy during these years leaves a disproportionately durable imprint relative to exposure of equal duration during later adulthood. Empirically, the formative hypothesis motivates an exposure measure that counts only the years of the 16–25 window spent under democracy. In a panel with country, birth-year, and survey-wave fixed effects, this exposure measure is identified off the interaction of cohort with country-specific transition timing, and is not collinear with current age within a given birth-year-by-survey-wave cell.

The *cumulative-exposure* mechanism is articulated at the country level by **?** under the label "democratic capital," and at the individual level by **?**. Under this hypothesis, what matters is the stock of years lived under democratic institutions, regardless of the age at which those years accrue. Transmission operates through repeated exposure to democratic norms, practices, and institutions across the life course, not through a privileged formative period. The cumulative hypothesis motivates an exposure measure defined as total years of life under democracy at the survey date. In a panel with country and wave fixed effects and a flexible polynomial control for current age, this exposure is identified partly through the cohort margin — older respondents have accumulated more years of democracy if the transition is far in the past — and partly through the country margin, holding age fixed across countries with different transition years.

The two hypotheses generate distinct empirical predictions and require distinct estimators. They are not nested. A formative effect would imply discontinuities at the cohort boundary defined by the transition year, with weight concentrated on cohorts whose 16–25 window overlapped the transition. A cumulative effect would not exhibit such discontinuities; it would, instead, scale roughly with years of exposure.

## 2.3 Predictions by attitude class

We take each tradition's formative-window prediction in turn and ask whether the cohort effect it implies is recoverable from cross-country variation.

**Personal emancipative values: predicted formative imprint, not identified.** The Inglehart-Welzel program predicts that exposure to democracy during the formative window imprints emancipative values that persist into adulthood — tolerance toward minorities, support for gender equality, endorsement of personal autonomy. This is the tradition's strongest empirical referent, documented across decades of World Values Survey data (**???**).

The Inglehart-Welzel mechanism and a secular cohort trend make opposite shape predictions across birth cohorts. A formative effect concentrates where exposure is changing: a discontinuity at the cohort boundary defined by the transition, a flat tolerance profile among cohorts whose 16–25 window closed before the transition (for whom formative exposure is zero, so there is nothing to move them) and among cohorts whose window falls entirely after it (for whom exposure is constant at the ten-year cap, so a treatment-driven effect can no longer move them), and a rise only in the straddling cohorts between, where exposure is actually changing. A secular cohort trend implies, instead, a smooth monotone rise across all cohorts, including the never-exposed and fully-exposed regions where formative exposure does not vary. The two are distinguishable by shape, which Section 5.3 exploits.

**System and regime support: an informative null.** The prediction here is contested. A naive extension of the Mannheim-Inglehart intuition would expect a formative effect on system support as well: those who came of age under democracy should adhere more strongly to the democratic regime. The contrary prediction, articulated by Mishler and Rose for the post-communist transitions and replicated in the European consolidated-democracies literature, is that diffuse support for the system is governed primarily by contemporary evaluations of performance and is not crystallized in the formative years (**?????**).

**Cumulative exposure.** ? introduce democratic capital as a stock that accumulates with each additional year of life under democracy, modulating regime persistence at the country level. ? take this argument to the individual level in their East–West German comparison, identifying a cumulative-exposure effect on political preferences including support for the democratic regime. The mechanism is distinct from the formative-window mechanism above: it does not require a critical window, operating instead through repeated exposure to democratic norms across the life course, and it predicts an effect that scales with the stock of years lived under democracy rather than a discontinuity at any cohort boundary.

**Contemporary determination: framing, not estimation.** This cell is the one our design does not estimate directly but that frames the interpretation of the other three. The Mishler-Rose position holds that the dominant predictor of diffuse support is contemporary evaluation of regime performance: economic conditions, perceived corruption, government effectiveness. ? extends this to consolidated democracies: critical citizens update their diffuse support in light of observed performance rather than biographical exposure. The economic-voting tradition descended from ? provides the empirical infrastructure for this position.

We do not estimate the contemporary-performance margin here. Our inclusion of country-by-wave and birth-year fixed effects in the formative specification, and of country and wave fixed effects with flexible age controls in the cumulative specification, absorbs contemporary aggregate shocks at the country-by-wave level. Any contemporary effect on diffuse support operates within those cells and is not the object of our identification. Its role in the argument is framing: the formative null on regime support, together with the unrecovered cumulative signal, leaves room for contemporary determination to be the dominant driver of system support, which is the Mishler-Rose position. Our evidence is consistent with that claim; it does not prove it.

# 3 Data

## 3.1 Latinobarómetro 1995–2023

Our first data source is the Latinobarómetro survey, an annual face-to-face cross-section of public opinion conducted by Corporación Latinobarómetro in Latin America since 1995. We use all twenty-three publicly released waves through 2023. Latinobarómetro did not field a survey in 1999, 2012, 2014, 2019, or 2022, so no observations exist for those calendar years; the 2020 wave was conducted online because of the COVID-19 pandemic and we treat it as comparable to the prior face-to-face waves on the variables used here, while noting the methodological caveat.

The analytical sample restricts to respondents in the fourteen Latin American countries listed in Table 1, that is, the countries with V-Dem-identified first democratic transitions between 1982 and 2000. After stacking all twenty-three waves and applying an age filter (16–99, the population from which Latinobarómetro samples) and an ISO-numeric country-code mapping, the working sample comprises approximately 335,000 respondent-wave observations. Per-country observation counts for the twenty-one waves from 1997 onward (314,454 observations, with the 1995 and 1996 waves omitted from the breakdown) are reported in Appendix Table 8.

Harmonization across the twenty-three waves was substantial. The country-code and current-age variables change names across waves, the 2018 file uses dotted variable names that Stata cannot reference directly, and the crime-victimization item used in our sensitivity analysis underwent a scale change between 2009 and 2010. We resolve each through a documented procedure — a shape-validation rule that identifies the raw-age variable by its distribution rather than its name, a Python preprocessing step that renames the dotted variables, and a collapse of the post-2009 crime scale back to the original binary to avoid a measurement-induced break. Appendix A sets out the procedures in full, and the complete wave-by-wave variable mapping covering 23 waves and 10 outcome concepts is reported in the accompanying Online Appendix.

### 3.2 World Values Survey: LATAM subset and global sample

The second data source is the World Values Survey Time Series 1981–2022, version 5.0 (**?**). We use the WVS in two modes. The first is a Latin American subset restricted to the same fourteen countries as Latinobarómetro, for cross-source replication of the formative null and the emancipative cohort gradient within a directly comparable sample. The second is a global subset comprising the fifty countries with V-Dem-identified first transitions to democracy in the post-1981 V-Dem coverage window, used as our broader cross-national test of the formative specification.

The global subset is constructed by intersecting the WVS sample with the V-Dem criterion described in Section 3.3: a country is included if its V-Dem binary democracy indicator transitions from zero to one for the first time in the post-1981 series. This excludes countries that were already democratic at the start of the V-Dem coverage (and therefore have no observable first transition) and countries that have not transitioned in the series. The resulting fifty countries span six broad regions: Latin America (twelve — the eleven Latinobarómetro LATAM-14 countries that the WVS Time Series covers, plus Peru as the twelfth, included because its first observable zero-to-one transition in the post-1981 V-Dem series occurs in 2001, even though Section 3.1 excludes Peru from the Latinobarómetro LATAM-14 sample as a re-democratization outside the 1982–2000 first-transition window), the post-Soviet space (five) and Eastern Europe (fourteen) which we combine as the nineteen-cluster post-communist bloc in the robustness analyses of Section 6.2, sub-Saharan Africa (seven), East and Southeast Asia (eight, including Korea, Taiwan, Indonesia, the Philippines, and others), and the Middle East and North Africa (four). Honduras, Panama, and Paraguay are absent from the WVS Time Series fielding window and so do not enter the global subset despite being in the Latinobarómetro LATAM-14 panel. Per-country observation counts and transition years for the global subset are reported in Appendix Table 9.

Variable identification in the WVS is straightforward. The country code is `S003` (ISO numeric, same scheme as Latinobarómetro), the survey year is `S020`, and the respondent's current age is `X003`. The year of birth `X002` is reported by respondents in some but not all waves; when missing or coded as non-response, we impute it as S020 − X003.

Our primary WVS outcomes for the regime-support arm are two items. The first is E117, "Having a democratic political system" (1 = very good, 2 = fairly good, 3 = fairly bad, 4 = very bad), the most-used regime-evaluation item in the comparative literature; we binarize as E117_support = 1 if E117 $\in \{1, 2\}$ and zero if E117 $\in \{3, 4\}$. The second is E123, the WVS analog of the Latinobarómetro Churchill question ("Democracy may have its problems but it is better than any other form of government"; 4-point scale), binarized identically. For the emancipative-values arm we use F118, "Justifiable: Homosexuality", on a 1–10 justifiability scale; we use the continuous version as the primary outcome and a binarized version tol_homo_hi = 1 if F118 $\geq 6$ as a robustness specification.

WVS coverage of our two subsets is uneven. On the LATAM-14 subset, we have approximately 35,000 completed-window observations on E117 across eleven country-clusters and approximately 7,000 on E123 across only six country-clusters; the E123 LATAM-14 estimate is reported only as an internal cross-check because six clusters is below the threshold at which cluster-robust inference — including wild cluster bootstrap — remains informative. On the global fifty-country subset, the canonical E117 regime item is observed for approximately 142,000 completed-window respondents across all fifty clusters — the sample for the cross-national formative test — and the emancipative item F118 for approximately 154,000. The Churchill item E123 is fielded in only thirty-four of the fifty global countries (approximately 48,000 completed-window respondents), so its cluster count is thirty-four rather than fifty.

The WVS LATAM-14 sample is one to two orders of magnitude smaller than the Latinobarómetro sample on the same fourteen countries, and within Latin America our identification draws more power from Latinobarómetro. The role of the WVS LATAM-14 subset is independent cross-source replication on canonical comparative-attitudes items. The role of the WVS global subset is the broader test of the formative null and the emancipative cohort gradient on a sample whose country composition extends beyond Latin America.

### 3.3 V-Dem and transition-year construction

We use the V-Dem Core Dataset, version 15 (**?**), to date democratic transitions. Following the convention used in the prior literature, we collapse V-Dem's ordinal regime classification into a binary indicator that takes value one for electoral democracy and liberal democracy regime types and zero otherwise. A democratic transition for country $c$ in year $y$ is defined as the first year-on-year transition from zero to one in this binary series after 1980. Our analytical sample restricts to countries whose first transition under this definition falls between 1982 and 2000, yielding the fourteen Latin American countries listed in Table 1.

Table 1: Latinobarómetro LATAM-14 sample: V-Dem-identified first democratic transition years.

| ISO3 | Country | First-transition year |
|---|---|---|
| DOM | Dominican Republic | 1982 |
| ARG | Argentina | 1984 |
| URY | Uruguay | 1985 |
| BOL | Bolivia | 1986 |
| BRA | Brazil | 1987 |
| CHL | Chile | 1990 |
| NIC | Nicaragua | 1990 |
| COL | Colombia | 1991 |
| PAN | Panama | 1991 |
| PRY | Paraguay | 1993 |
| HND | Honduras | 1994 |
| MEX | Mexico | 1996 |
| SLV | El Salvador | 1999 |
| GTM | Guatemala | 2000 |

*Notes.* First year-on-year transition from non-democracy to democracy in the V-Dem v15 binary regime series after 1980, as constructed in Section 3.3. Sample window spans 1982 (Dominican Republic) to 2000 (Guatemala). Costa Rica, Ecuador, and Venezuela are excluded because V-Dem codes them as democratic at the start of the V-Dem series; Peru is excluded because the first observable transition in our window is its 2001 re-democratization rather than a first transition.

Three Latin American countries that the V-Dem series codes as democratic in 1981 (Costa Rica, Ecuador, Venezuela) are excluded from the main analysis on the basis that their first transitions are either historical (Costa Rica 1949, Ecuador 1979) or contested with respect to subsequent backsliding (Venezuela; Chavez-era and post-2014 trajectory). Peru is excluded from the main 14-country sample because V-Dem's first observable transition in our window is its 2001 re-democratization following the Fujimori autogolpe, which is a re-democratization rather than a first transition. We report robustness specifications that include each of these excluded countries with appropriately flagged caveats.

### 3.4 Exposure variables

Two exposure variables are constructed at the respondent level.

The *formative-window* exposure for respondent $i$ in country $c$ with transition year $T_c$ is defined as

$$E_{ic}^{\text{form}} = \sum_{a=16}^{25} \mathbf{1}\{\text{birth}_i + a \geq T_c\},$$

the count of years in the respondent's age-16-to-25 window that fall on or after the democratic transition, bounded between 0 and 10. It measures the number of years of the respondent's 16–25 life window spent under democracy.

The *cumulative* exposure is defined as

$$E_{ic}^{\text{cum}} = \max(0,\, y_i - \max(\text{birth}_i, T_c)),$$

where $y_i$ is the year of the survey. It measures the total years of life under democracy at the moment of the survey, capped at the respondent's age. By construction, $E_{ic}^{\text{cum}} \geq E_{ic}^{\text{form}}$ for any respondent whose formative window included democratic years, and the two coincide only for cohorts whose formative window opened on or after the transition year and who are surveyed at age 25.

Formative exposure partitions the sample into three regions that the shape tests of Section 5.3 exploit, and whose coverage determines how much those tests can identify. A respondent's region is set jointly by birth cohort and country transition year: cohorts whose 16–25 window closed before their country's transition have zero formative exposure, cohorts whose window opened on or after it are exposed for the full ten-year cap, and cohorts whose window straddles the transition take intermediate values. Because countries transitioned at different dates, the same birth year falls in different regions across countries, so the regions are not birth-cohort bins. Table 2 reports the partition for the WVS global completed-window sample. The fully-exposed region — the one on which the constant-treatment plateau slope of Section 5.3 is estimated, where exposure is pinned at the cap and any remaining cohort gradient cannot be a treatment effect — carries 21,280 respondents in 37 country-clusters spanning birth cohorts 1966–1999, so that slope is identified off substantial within-region cohort variation rather than a residual sliver.

Table 2: Formative-exposure regions in the WVS global completed-window sample

| Region | $N$ | Clusters | Birth cohorts | Mean $E^{\text{form}}$ | Mean tolerance |
|---|---|---|---|---|---|
| Never-exposed ($E = 0$) | 108,933 | 50 | 1891–1992 | 0.0 | 2.57 |
| Straddling ($0 < E < 10$) | 24,095 | 45 | 1957–1995 | 4.7 | 3.31 |
| Fully-exposed ($E = 10$) | 21,280 | 37 | 1966–1999 | 10.0 | 4.00 |
| Total | 154,308 | 50 | 1891–1999 | — | — |

Note: Completed-window sample (respondents aged 26 and over with non-missing tolerance, transition year, and birth year). Region is defined by formative-window exposure $E^{\text{form}}$: zero, strictly between zero and the ten-year cap, or at the cap. Mean tolerance is the F118 "justifiable: homosexuality" item on its one-to-ten scale. Cluster counts are countries; the never-exposed region draws on all 50 first-transition countries, the fully-exposed region only on the earlier transitioners whose post-transition cohorts have reached age 26.

### 3.5 Outcome variables

We construct five primary outcomes from Latinobarómetro and two from the WVS. We do *not* aggregate across these into "democratic values" or analogous composites: the theoretical argument of Section 2 is that diffuse system support and personal emancipative values plausibly follow different formation regimes, and aggregating them would obscure precisely the distinction the design is built to test. We report item-by-item estimation throughout.

One aggregation is the exception, and it requires explicit justification. We do construct an institutional-confidence index as the mean of five binarized items measuring confidence in representative institutions (Congress, government, police, armed forces, political parties). This aggregation is *within* a single construct, not across constructs: the five items differ only in which representative institution is named, while the underlying attitude being elicited — confidence in representative institutions — is shared across all five. The empirical coherence of the battery is high (Cronbach's $\alpha$ between 0.66 and 0.76 across waves; see below), supporting the claim that the items operationalize a single latent attitude. The aggregations we *decline* to perform — pooling support for democracy with satisfaction with democracy, or with tolerance toward homosexuality — would be aggregations across constructs that our theoretical framework treats as conceptually distinct, and would foreclose the within-paper tests of the diffuse-versus-personal distinction.

**Support for democracy as a regime (`churchill_support`).** The Latinobarómetro Churchill question asks respondents to choose the statement they most agree with: (1) democracy is preferable to any other form of government; (2) in some circumstances an authoritarian government may be preferable; (3) for people like me it makes no difference whether the regime is democratic or not. The variable code rotates across waves (from `p20` in 1995 to `P10STGBS` in 2020 and 2023; full mapping in the Online Appendix) but the question wording is stable across the time series. We code churchill_support = 1 if the response is option (1), zero if (2) or (3). Non-substantive responses ("don't know", "no answer") are treated as missing.

**Satisfaction with democracy (`satisf_high`).** The Latinobarómetro item "In general, would you say you are very satisfied, somewhat satisfied, not very satisfied, or not at all satisfied with the functioning of democracy in (country)?". We code satisf_high = 1 if the response is "very" or "somewhat" satisfied, zero otherwise. Available in all twenty-three waves with stable wording.

**Institutional confidence index (`trust_index`).** A within-respondent mean of five binarized confidence items, one per representative institution: the Congress, the national government, the police, the armed forces, and the political parties. Each constituent item uses the standard 4-point trust scale (much, some, little, none); we binarize each to 1 if the respondent reports "much" or "some" trust, zero otherwise. The within-wave Cronbach's $\alpha$ for the five-item battery ranges from 0.664 to 0.760 across waves, with a mean of approximately 0.71. In waves 1997–2001 the "government" item is not asked; the index uses the four available items for those waves and the four-item $\alpha$ ranges from 0.699 to 0.743. All within-wave $\alpha$ values exceed 0.6, the conventional threshold for treating a battery as a unidimensional construct, and we accordingly use the item mean as the primary measure. We also report robustness results on each constituent item separately in Appendix C to confirm the index is not driven by a single component.

**Tolerance toward homosexuality (`tol_homo_c`).** The Latinobarómetro analog of the canonical WVS Inglehart-Welzel emancipative-values item: "How justifiable is the following: homosexuality?" on a 1–10 scale, where 1 is "never justifiable" and 10 is "always justifiable". We use the continuous version as the primary outcome and a binarized version tol_homo_hi $= 1$ if response $\geq 6$ as a robustness specification.

The wave coverage of this item warrants note. F118 is asked in only four Latinobarómetro waves — 2002, 2004, 2008, and 2009 — concentrated in a single seven-year window, whereas the system-support outcomes draw on all twenty-three Latinobarómetro waves between 1995 and 2023. This asymmetry does not threaten the system-support null, whose informativeness rests on the minimum detectable effect and the equivalence bounds (Section 5.2) rather than on the emancipative gradient. It does bear on the emancipative analysis of Section 5.3: the four-wave F118 sample provides roughly 41,000 valid completed-window responses across the fourteen-country panel, comparable to the WVS Latin American subsample, so the formative analysis of the emancipative item rests on materially thinner cohort coverage than the system-support nulls.

**WVS outcomes.** E117_support $= 1$ if E117 $\in \{1, 2\}$ (very or fairly good evaluation of the democratic political system as such), zero if $\in \{3, 4\}$. E123_agree $= 1$ if E123 $\in \{1, 2\}$ (agreement with the Churchill statement). The latter is reported only as a reference because of its limited cluster count (Section 3.2).

**Secondary outcomes for sensitivity analysis.** We also report sensitivity results on four additional Latinobarómetro items whose underlying literatures motivated them as cohort-design candidates: interpersonal trust (the Putnam-Uslaner generalized social-trust item, "Confianza Interpersonal"), agreement with the market as the unique vehicle of development ("La economía de mercado es el único sistema"), agreement with traditional gender roles ("Es mejor la mujer en casa y el hombre en el trabajo"), and a measure of personal economic optimism ("Situación económica futura familiar"). These outcomes are reported in Section 5 as sensitivity reference points and serve to locate our primary findings within a broader pattern. They do not enter the headline tables.

## 3.6 Descriptive statistics

Table 3 reports summary statistics for the analysis variables in each sample, on the completed-window estimation universe (respondents aged 26 and over). The outcome counts vary within a sample because the regime, satisfaction, trust, and emancipative items are fielded in different waves; the F118 tolerance item in Latinobarómetro, in particular, appears in only four waves, which is why its count is an order of magnitude below the regime items in the same panel. Formative exposure averages between two and four years across samples, with the full zero-to-ten range represented in each; cumulative exposure, available in the World Values Survey panels, averages eleven to fifteen years.

Table 3: Descriptive statistics for the analysis variables, by sample (completed-window universe, respondents aged 26 and over).

| Variable | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| *Panel A. Latinobarómetro LATAM-14* ($N$ = 253,401 respondent-waves) | | | | | |
| Churchill agreement (binary) | 231,565 | 0.602 | 0.489 | 0 | 1 |
| Satisfaction with democracy (binary) | 240,737 | 0.362 | 0.481 | 0 | 1 |
| Trust-index composite (0–1) | 251,831 | 0.336 | 0.325 | 0 | 1 |
| Tolerance, homosexuality (1–10) | 40,781 | 3.900 | 3.125 | 1 | 10 |
| Formative exposure (years) | 253,401 | 3.666 | 4.398 | 0 | 10 |
| Age (years) | 253,401 | 45.65 | 14.38 | 26 | 96 |
| *Panel B. WVS global, 50 first-transition countries* ($N$ = 168,402 respondent-waves) | | | | | |
| E117, democratic system (binary) | 141,993 | 0.880 | 0.324 | 0 | 1 |
| E123, Churchill agreement (binary) | 47,736 | 0.852 | 0.355 | 0 | 1 |
| Tolerance, homosexuality (1–10) | 154,308 | 2.882 | 2.746 | 1 | 10 |
| Abortion justifiability (1–10) | 157,214 | 3.148 | 2.743 | 1 | 10 |
| Divorce justifiability (1–10) | 156,561 | 4.514 | 3.079 | 1 | 10 |
| Formative exposure (years) | 168,402 | 2.066 | 3.672 | 0 | 10 |
| Cumulative exposure (years) | 168,402 | 11.258 | 9.703 | 0 | 37 |
| Age (years) | 168,402 | 45.42 | 13.96 | 26 | 99 |
| *Panel C. WVS Latin American 14-country subset* ($N$ = 43,082 respondent-waves) | | | | | |
| E117, democratic system (binary) | 34,647 | 0.875 | 0.331 | 0 | 1 |
| E123, Churchill agreement (binary) | 7,019 | 0.858 | 0.349 | 0 | 1 |
| Tolerance, homosexuality (1–10) | 40,928 | 3.668 | 3.142 | 1 | 10 |
| Abortion justifiability (1–10) | 42,053 | 2.618 | 2.618 | 1 | 10 |
| Divorce justifiability (1–10) | 39,870 | 5.134 | 3.429 | 1 | 10 |
| Formative exposure (years) | 43,082 | 2.871 | 4.122 | 0 | 10 |
| Cumulative exposure (years) | 43,082 | 14.825 | 10.559 | 0 | 37 |
| Age (years) | 43,082 | 44.91 | 13.94 | 26 | 99 |

*Notes.* Completed-window estimation universe (respondents aged 26 and over, for whom the 16–25 formative window has fully elapsed). Binary outcomes are coded one for positive agreement and zero otherwise; the trust-index composite is the within-respondent mean of five binarized institutional-confidence items. Tolerance, abortion, and divorce are the World Values Survey one-to-ten justifiability scales; the Latinobarómetro tolerance item is the harmonized homosexuality-justifiability scale fielded in four waves. Formative exposure is the overlap of the 16–25 window with post-transition years, bounded at zero and ten; cumulative exposure is total years of life under democracy at the survey date (constructed for the World Values Survey panels). Within-sample $N$ varies by item because outcomes are fielded in different waves.

The cohort gradient that motivates the analysis is visible in the raw data, before any fixed effects. Figure 1 plots the pooled, standardized cohort means of tolerance and the two World Values Survey regime-support items against birth cohort. Tolerance rises smoothly and substantially across cohorts — roughly six-tenths of a standard deviation from the oldest to the youngest — while the regime items show no comparable rising gradient. The smoothness of the tolerance profile, and its presence across the entire cohort range including cohorts who came of age long before any democratic transition, is a first sign that the gradient is a secular cohort trend rather than an artifact of the fixed-effects specification. Whether any part of it is a formative effect of democratic exposure is the question Section 5.3 takes up.

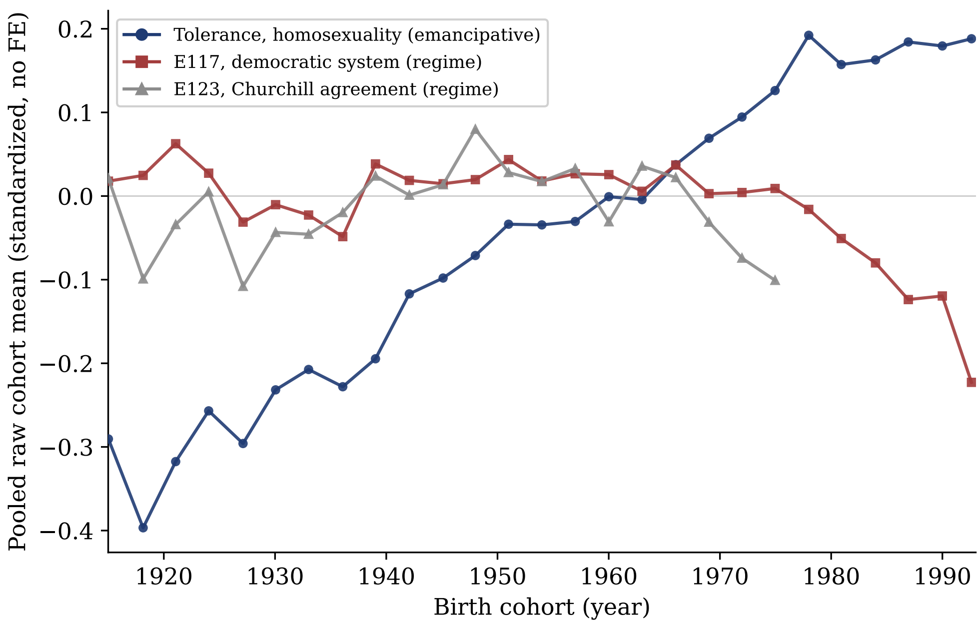


Figure 1: Raw cohort profiles of tolerance and regime support (WVS global, completed-window sample, pooled across all fifty countries with no fixed effects). Each series is the pooled mean of the outcome within three-year birth-cohort bins, standardized to its own mean and standard deviation so that the one-to-ten tolerance scale and the binary regime items share an axis. Tolerance rises smoothly and monotonically across the full cohort range; the regime-support items E117 ("having a democratic political system") and E123 (the Churchill statement) show no comparable rising gradient. The figure is descriptive and pre-decomposition: it establishes that the emancipative gradient exists and is smooth in the raw data, before the fixed-effects machinery, leaving its interpretation to Section 5.3.

# 4 Empirical strategy

## 4.1 Formative-window specification

For each outcome $y_{ict}$ of respondent $i$ in country $c$ surveyed in wave $t$, the formative specification is

$$y_{ict} = \beta \, E_{ic}^{\text{form}} + \lambda_{ct} + \gamma_{b(i)} + \varepsilon_{ict}, \tag{1}$$

where $\lambda_{ct}$ are country-by-survey-wave fixed effects and $\gamma_{b(i)}$ are birth-year fixed effects. The country-by-wave effects absorb every contemporaneous shock common to a country in a given survey wave. The coefficient of interest is $\beta$, which measures the average per-year effect of formative-window exposure to democracy on the outcome.

The identifying variation in $E_{ic}^{\text{form}}$ comes from the interaction of birth cohort with country-of-transition timing. Within a country-by-wave cell, respondents of different birth cohorts have different formative exposure, because each cohort's 16–25 window overlaps the country's transition by a different amount; the birth-year fixed effects net out the common cohort trajectory, so that $\beta$ is identified from how a given cohort's exposure in one country departs from the same cohort's exposure in countries that transitioned at other dates. The identifying assumption is that, conditional on country-by-wave and birth-year fixed effects, this cohort-by-timing interaction is not systematically correlated with omitted determinants of the outcome. We acknowledge the canonical age-period-cohort identification problem: because age, period, and cohort are related by the identity age = wave − birth, additive fixed effects in any two of the three exhaust the linear part of the third. The specification therefore does not separately identify a "pure" linear age effect from a linear cohort effect or a linear period effect; whatever variation in the outcome is attributable to a linear age trend is absorbed jointly by the birth-year fixed effects and the wave dimension of the country-by-wave fixed effects. The coefficient on formative exposure is identified off non-linear cohort-by-country variation rather than off a linear age trend, and its causal interpretation requires the assumption stated above.

This identifying assumption bears asymmetrically on the two attitude classes, and the asymmetry is diagnostic rather than dispositive. A positive gradient is especially vulnerable to secular cohort change correlated with transition timing, because such a trend can mimic the shape of formative exposure. A null on regime support is less vulnerable to that particular concern, especially once country-by-wave shocks and common birth-year profiles are absorbed. A null can nonetheless arise for reasons unrelated to a true absence of effect — attenuation from measurement error in transition dates or exposure, heterogeneous effects that offset across cohorts or countries, ceiling effects on items with little room to move, or a coarse linear treatment that misses a nonlinear response. We therefore read the regime-support result as an informative null within the identifying variation of this design, bounded by the equivalence interval below (Section 5.2), rather than as proof of universal absence.

## 4.2 Cumulative-exposure specification

The cumulative specification follows the identification logic of **?**, dropping birth-year fixed effects (since cumulative exposure varies in part through the cohort margin that birth-year FE would absorb) and adding a flexible control for current age:

$$y_{ict} = \beta \, E_{ic}^{\text{cum}} + g(\text{age}_i) + \alpha_c + \delta_t + \varepsilon_{ict}, \tag{2}$$

where $g(\cdot)$ is a cubic polynomial in current age. The coefficient of interest is now interpreted as the per-year effect of cumulative life-time exposure to democracy, conditional on a smooth flexible age trend, country level, and contemporary wave shock.

The identification of $\beta$ in this specification draws partly on the cohort margin — older respondents in countries with earlier transitions have accumulated more years of democratic exposure than younger respondents in the same country — and partly on the country margin, holding age fixed across countries with different transition years. The cubic polynomial in age is the smoothing restriction that allows the specification to attribute the residual variation to cumulative exposure rather than to a flexible age trend; if the true age effect is substantially non-monotonic or non-polynomial, the cumulative coefficient may absorb part of that misspecification. This is a known limitation of the cumulative specification under cross-country observational identification, discussed further in Section 6.1.

The cumulative specification retains additive country and survey-wave fixed effects rather than the country-by-wave interaction used as the formative baseline (Section 4.1). This is a matter of identification rather than convenience: the two estimands draw on different variation. The formative coefficient is identified within country-by-wave cells, across birth cohorts whose formative windows overlap the transition by different amounts, and so survives the interaction. Cumulative exposure, by contrast, is by construction a near-deterministic function of age within a country-by-wave cell — the years lived under democracy at the survey date move almost one-for-one with current age once country and wave are held fixed — so the country-by-wave interaction, together with the flexible age control, absorbs the cumulative margin and the coefficient is no longer estimable. That the same interaction leaves the formative coefficient well identified while dissolving the cumulative one is itself evidence that the two specifications target distinct objects rather than two labels for a single cohort pattern.

## 4.3 Inference

Standard errors are clustered at the country level throughout. Cluster counts vary by sample: fourteen for the Latinobarómetro main sample, eleven for the WVS LATAM-14 subsample on the canonical regime-support item E117 (six for the secondary E123 item), nineteen for the post-communist bloc analyses, and fifty for the WVS global subset. Conventional cluster-robust standard errors are known to over-reject under the null when the number of clusters is small (?): the over-rejection is severe at $G \leq 10$ and material up to $G \approx 30$–$40$, while at $G = 50$ or above the cluster-robust inference is well behaved under standard conditions.

We report wild cluster bootstrap p-values with Webb six-point weights and 9,999 replications, implemented via `boottest` (?), on every main coefficient in the regime-support and emancipative-values cells, regardless of cluster count, for internal consistency. For the LATAM sample and the post-communist bloc, the wild cluster bootstrap is the primary inference; the cluster-robust p-values are reported alongside as a reference for readers familiar with that convention. For the WVS global sample with fifty clusters, cluster-robust and wild cluster bootstrap p-values agree closely, and we report both. Reported results on the WVS E123 LATAM-14 subset with six clusters are treated as a cross-check rather than as primary inference, as no small-sample correction is informative at that cluster count.

The leave-one-out robustness exercise on the emancipative-values outcome is reported with cluster-robust standard errors and wild cluster bootstrap p-values on each of the leave-one-out subsamples. The post-communist bloc analyses of the cumulative specification on the Churchill item are likewise reported with leave-one-out and a continuous gradient interaction, both with wild cluster bootstrap inference. We do not invoke a small-sample correction to a wild cluster bootstrap below the cluster counts at which the bootstrap itself becomes unreliable.

## 4.4 Minimum detectable effect

For each outcome on which we report a null formative effect, we compute the minimum detectable effect at 80% statistical power and conventional two-sided significance, using the standard frequentist formula

$$\text{MDE} = (z_{1-\alpha/2} + z_{1-\beta}) \cdot \text{SE}(\hat{\beta}) \approx 2.802 \cdot \text{SE}(\hat{\beta}). \tag{3}$$

The MDE is reported in the natural metric of the estimator and in standard-deviation units of the outcome.

The benchmark for whether a given MDE counts as informative requires a reference for what magnitude the literature treats as substantive. We take this reference from the magnitude reported by **?**, who express their estimated effect of cumulative democratic exposure as equivalent to the gap between primary and secondary education in support for the democratic regime. The authors do not convert this gap to standard-deviation units. To translate it into a per-annum metric comparable to our MDE, we assume that the primary-to-secondary education gradient on support for democracy is approximately 0.15 standard deviations, the mid-range of comparable estimates in the comparative literature. This yields a per-annum scale of 0.0176 standard deviations as a reference of what counts as substantively interesting. We are explicit that this conversion is our assumption rather than a number reported by Fuchs-Schündeln and Schündeln, who studied cumulative not formative exposure: their figure serves here as a reference of scale for what the literature treats as a magnitude worth recovering, not as the effect against which our MDE is measured. The qualitative conclusion of Section 5.2 — that the MDE is calibrated below substantively interesting formative magnitudes — is robust to substantially more conservative choices of the education gradient.

# 5 Results

Results are organized in five subsections. Section 5.1 reports the formative-exposure coefficient on diffuse regime support across three samples. Section 5.2 converts the cluster-robust standard errors into minimum detectable effects and compares them to the scale implied by **?**. Section 5.3 applies the same specification to tolerance toward homosexuality, the canonical **?** item, and shows that its apparent cohort gradient is not identified as a formative effect. Section 6.1 replaces the formative window with cumulative lifetime exposure. Section 6.2 consolidates the robustness layers.

## 5.1 Formative exposure does not predict support for democracy

The formative-window specification of Equation 1, estimated on the completed-window sample, returns coefficients in the fourth decimal place across every Latinobarómetro outcome, the WVS LATAM-14 restriction, and the WVS global sample of fifty first-transition countries. None of the six coefficients approaches conventional significance under the wild cluster bootstrap. Table 4 reports the estimates.

In the Latinobarómetro panel, Churchill agreement loads a small negative coefficient, satisfaction with democracy a small positive one, and the trust-index composite a near-zero positive one. The three signs are inconsistent and the $t$-statistic on each coefficient is below one in absolute value. The WVS LATAM-14 restriction reproduces the same pattern on the canonical item E117 ("Having a democratic political system"). Extending the same specification to the WVS global panel of fifty first-transition countries leaves the conclusion intact on both E117 and the Churchill item E123. We read the six estimates as a single null on regime-related attitudes, clearest for the two generic democratic-support items and extending in the same direction to satisfaction with democracy and institutional confidence, replicated across two independent survey programs and two geographic scopes (Figure 2).

Table 4: Formative-exposure coefficient on regime-related attitudes — ranging from generic democratic support to performance-adjacent satisfaction and institutional confidence — across samples and outcomes.

| Sample | Outcome | Coefficient | SE | $p_{\text{WCB}}$ | N | Clusters |
|---|---|---|---|---|---|---|
| Latinobarómetro LATAM-14 | Churchill agreement | −0.00033 | 0.00075 | 0.68 | 231,562 | 14 |
| Latinobarómetro LATAM-14 | Satisfaction with democracy (high) | +0.00013 | 0.00148 | 0.94 | 240,734 | 14 |
| Latinobarómetro LATAM-14 | Trust-index composite | +0.00012 | 0.00083 | 0.89 | 251,828 | 14 |
| WVS LATAM-14 | E117 (Having a democratic political system) | −0.00001 | 0.00108 | 0.99 | 34,643 | 11 |
| WVS global 50 countries | E117 (Having a democratic political system) | +0.00047 | 0.00068 | 0.52 | 141,991 | 50 |
| WVS global 50 countries | E123 (Churchill agreement) | −0.00064 | 0.00149 | 0.69 | 47,735 | 34 |

*Notes.* Coefficient on years of formative-window exposure (the overlap between ages 16–25 and post-transition life, bounded at zero and ten) from the formative specification of Equation 1 with country-by-wave and birth-year fixed effects, estimated on the completed-window sample (respondents aged 26 and over, for whom the formative window has fully elapsed). Standard errors clustered at the country level; $p_{\text{WCB}}$ is the wild cluster bootstrap $p$-value with Webb six-point weights and 9,999 replications. Binary outcomes (Latinobarómetro Churchill and Satisfaction; WVS E117_support and E123_agree) are coded as one for positive agreement and zero for negative agreement; the trust-index composite is the within-respondent mean of five binarized institutional confidence items (Congress, national government, police, armed forces, political parties) described in Section 3.5.

The regime null is not an artifact of an insensitive estimator, and the cleanest demonstration is a single control. The country-specific linear cohort-trend control that absorbs roughly sixty-eight percent of the emancipative gradient — driving it from 0.0178 to 0.0058 standard deviations per year, below the reference scale (Section 5.3) — leaves the regime-support nulls intact: under the control all six coefficients remain statistically indistinguishable from zero, the closest being the global Churchill item E123 and the Latinobarómetro trust index, and none approaches conventional significance. The wild cluster bootstrap $p$-values under the control are 0.88 and 0.10 on the global E117 and E123 items, 0.72 on the WVS Latin American E117, and 0.89, 0.43, and 0.11 on the three Latinobarómetro outcomes.

## 5.2 Minimum detectable effects and the Fuchs-Schündeln reference scale

A null is informative only if the design could plausibly have detected the effect the literature would have us look for. For each Latinobarómetro outcome we compute the per-year minimum detectable effect at conventional power and rescale it to per-year standard-deviation units using the sample standard deviation of the dependent variable. The three MDEs lie in a tight band between roughly four-tenths and nine-tenths of one percent of an outcome standard deviation per year of formative exposure. Section 4.4 converted the **?** cumulative-exposure finding into a per-year scale of approximately 0.0176 standard deviations per year using a primary-versus-secondary education-gradient bridge that we restated as our assumption rather than a quantity reported in the original paper. Against that reference of scale, the three MDEs sit between roughly a quarter and a half. A formative-exposure effect on diffuse regime support of the per-year magnitude implied by Fuchs-Schündeln would have been detected in our samples at conventional power.

Table 5: Minimum detectable effects on Latinobarómetro regime-support outcomes against the Fuchs-Schündeln reference scale.

| Outcome | SD of outcome | MDE per year (raw) | MDE per year (SD/year) | Ratio to FS reference |
|---|---|---|---|---|
| Churchill agreement | 0.4894 | 0.00210 | 0.0043 | 0.24 |
| Satisfaction (high) | 0.4807 | 0.00413 | 0.0086 | 0.49 |
| Trust-index composite | 0.3252 | 0.00232 | 0.0071 | 0.40 |
| Fuchs-Schündeln reference scale | — | — | 0.0176 | 1.00 |

*Notes.* Minimum detectable effect computed as MDE $= 2.802 \times$ SE at conventional 80% power and 5% size on the per-year formative-exposure coefficient. The SD-per-year column rescales the raw MDE by the sample standard deviation of the dependent variable. The Fuchs-Schündeln reference scale of 0.0176 standard deviations per year is our bridge from the cumulative-exposure effect they report to per-year formative units, constructed via the primary-versus-secondary education gradient described in Section 4.4, and is not a quantity reported by **?**.

The minimum detectable effect bounds what the design could have detected; an equivalence criterion bounds what the data are consistent with. Across the two well-powered samples, the wild cluster bootstrap 95% confidence interval on every regime-support estimate lies entirely within $\pm 0.0176$ standard deviations per year — the Fuchs-Schündeln reference scale, and thus the equivalence criterion for rejecting formative effects larger than the magnitude the literature treats as substantive. In Latinobarómetro the intervals are $[-0.0046, 0.0031]$ (Churchill), $[-0.0076, 0.0078]$ (satisfaction), and $[-0.0062, 0.0063]$ (trust index); in the WVS global fifty-country sample the interval on E117 is $[-0.0032, 0.0061]$. The null is informative in the equivalence sense, not merely a failure to reject zero.

The equivalence conclusion does not rest on the Fuchs-Schündeln calibration. Table 6 reports, for each well-powered regime outcome, whether the wild cluster bootstrap interval falls inside successively tighter equivalence bounds. Every interval is contained at the Fuchs-Schündeln scale of $\pm 0.0176$ standard deviations per year, the containment survives at the substantially tighter $\pm 0.010$ bound for all four outcomes, and it holds at $\pm 0.005$ for the most precisely estimated outcome. The design rules out substantively meaningful formative effects on a criterion that does not depend on the education-gradient bridge.

Table 6: Equivalence of the regime-support null at alternative thresholds (country-by-wave baseline).

| Outcome | WCB 95% CI (SD/yr) | $\pm 0.005$ | $\pm 0.010$ | $\pm 0.0176$ |
|---|---|---|---|---|
| Churchill agreement (Lat.) | $[-0.0046, 0.0031]$ | Yes | Yes | Yes |
| Satisfaction, high (Lat.) | $[-0.0076, 0.0078]$ | No | Yes | Yes |
| Trust-index composite (Lat.) | $[-0.0062, 0.0063]$ | No | Yes | Yes |
| WVS global E117 | $[-0.0032, 0.0061]$ | No | Yes | Yes |

*Notes.* "Yes" indicates the wild cluster bootstrap 95% confidence interval on the formative-exposure coefficient (in standard deviations per year, country-by-wave baseline) lies entirely within $\pm\delta$, so the design rejects formative effects larger than $\delta$ at that threshold. Intervals are the completed-window estimates underlying Table 4, on the two well-powered samples. The $\pm 0.0176$ column is the Fuchs-Schündeln reference scale (Section 4.4).

## 5.3 The emancipative gradient is not identified as a formative effect

The same formative specification applied to the canonical Inglehart-Welzel item (F118, "Justifiable: Homosexuality," a one-to-ten scale) returns an apparent positive gradient. On the completed-window sample the coefficient is +0.049 per formative year in the WVS global fifty-country sample ($p_{\text{WCB}} < 0.001$) and +0.048 in Latinobarómetro ($p_{\text{WCB}} = 0.034$). Taken at face value, this is the cohort effect the modernization tradition predicts. It does not, however, survive the tests that distinguish a formative effect from a secular cohort trend. Table 7 reports the estimates.

Table 7: Formative-exposure coefficient on Inglehart-Welzel emancipative outcomes.

| Sample | Outcome | Coefficient | $p_{\text{WCB}}$ | N | Clusters |
|---|---|---|---|---|---|
| WVS global 50 countries | F118 tolerance, continuous (1–10) | +0.0489 | $< 0.001$ | 154,304 | 50 |
| WVS global 50 countries | F118 tolerance, high (F118 $\geq 6$) | +0.0061 | $< 0.001$ | 154,304 | 50 |
| Latinobarómetro LATAM-14 | Homosexuality justifiability, continuous | +0.0483 | 0.034 | 40,778 | 14 |
| Latinobarómetro LATAM-14 | Homosexuality justifiability, high | +0.0066 | 0.031 | 40,778 | 14 |
| WVS global 50 countries | F120 abortion justifiability, continuous | +0.0178 | 0.018 | 157,210 | 50 |
| WVS global 50 countries | F121 divorce justifiability, continuous | +0.0086 | 0.375 | 156,557 | 50 |

*Notes.* Coefficient on years of formative-window exposure from the formative specification of Equation 1, estimated on the completed-window sample (age $\geq$ 26). Wild cluster bootstrap $p$-values computed with Webb six-point weights and 9,999 replications. F120 (abortion) and F121 (divorce) are reported to show that the items do not cohere into the broad emancipative shift a formative mechanism would predict.

The formative mechanism makes a sharp prediction (Section 2.2): the effect should concentrate at the cohort boundary defined by the transition, producing a discontinuity between cohorts whose formative window fell before the transition and those whose window fell after. It predicts nothing about cohorts who never experienced formative democracy. Three features of the gradient violate the first prediction.

First, the gradient is present among never-exposed cohorts. Restricting to respondents whose entire 16–25 window closed before their country's transition — cohorts with zero years of formative democratic exposure — a placebo regression of tolerance on the overlap of that window with the pre-transition decade returns $+0.030$ per year on the one-to-ten scale ($p_{\text{WCB}} < 0.001$), roughly three-fifths the headline gradient. The gradient is present among cohorts who experienced no formative democracy at all.

Second, the cohort profile shows no discontinuity at the transition. Figure 3 plots tolerance against birth cohort relative to the transition, with country and survey-wave fixed effects; it rises smoothly and monotonically across the entire cohort range, including across the pure pre-transition cohorts (the rise across never-exposed cohorts is itself large and jointly significant, $F = 33.2$, $p < 0.001$), with no kink at the boundary the formative mechanism would mark. A smooth ramp through the pre-exposure region is the signature of a secular cohort trend, not of formation.

Third, and most directly, the gradient does not flatten where formative exposure stops moving (Figure 4). Completed-window cohorts fall into three regions by their formative exposure: never-exposed (the 16–25 window closed before the transition, exposure zero), straddling (the window overlaps the transition, exposure rising toward ten), and fully-exposed (the window falls entirely after the transition, exposure pinned at the cap of ten). In the fully-exposed region formative exposure is constant by construction, so any cohort gradient there cannot be formative. Yet tolerance keeps rising across fully-exposed cohorts, at 0.0055 standard deviations per birth year ($p_{\text{WCB}} = 0.011$, thirty-seven country-clusters), and the estimate is unchanged when the comparison is restricted to the countries that contribute to all three regions. A non-formative cohort trend of substantial size is therefore present in the data, established without any control that could absorb a true formative effect.

On the common set of thirty-seven countries present in all three regions, so that the comparison holds country composition fixed (Figure 4), the cohort slopes are 0.0089, 0.0118, and 0.0055 standard deviations per birth year (the never-exposed slope is the same secular trend the placebo above detects, measured directly on birth cohort rather than on pre-window overlap). The profile is two-sided. A secular trend is unmistakably present, significant in both treatment-static regions where formation cannot operate. But the straddling region, where exposure is actively changing, carries the steepest slope — the qualitative signature of a formative increment. That excess steepness is not statistically separable from the flanks: in a pooled specification that interacts the cohort slope with exposure region and is estimated under the same wild cluster bootstrap inference, none of the pairwise slope differences reaches significance — straddling versus never-exposed ($p_{\mathrm{WCB}} = 0.52$), straddling versus fully-exposed ($p_{\mathrm{WCB}} = 0.18$), and the two treatment-static flanks against each other ($p_{\mathrm{WCB}} = 0.15$). The data are consistent with a small formative component concentrated in the straddling cohorts, but the cross-country design cannot distinguish it from sampling noise.

The country-by-wave baseline is already informative on this point: moving from additive country and survey-wave effects to their interaction removes about a quarter of the apparent gradient on its own, the first indication that part of what looks like a cohort imprint is contemporaneous country-by-wave variation rather than formation — variation that a genuine within-cohort formative effect, orthogonal to such aggregates as the regime null is, would not display. A country-specific linear cohort-trend control points the same way from the other side, cutting the global gradient from 0.0178 to 0.0058 standard deviations per year (Appendix Figure 5), a sixty-eight-percent reduction to a marginal residual ($p_{\mathrm{WCB}} = 0.030$) below the 0.0176 reference scale. We read this as an upper bound on the secular share rather than as the identification: a linear control absorbs the linear projection of any true formative effect along with the secular trend, which is why we rest the claim on the constant-treatment slope, an uncontaminated secular estimate, rather than on the residual. Under the country-by-wave baseline the two decompositions converge: the never-exposed placebo attributes roughly three-fifths of the gradient and the country-specific linear control roughly two-thirds. The two independent routes to the secular share — one using only cohorts the treatment never reached, the other removing each country's smooth cohort drift — now fall in a narrow band between three-fifths and two-thirds, where the additive specification left them spread from half to seven-tenths. The tighter the two routes agree, the less of the gradient is left for a formative interpretation to claim.

Together these establish that the emancipative gradient is observationally equivalent to a secular cohort trend correlated with transition timing — a trend the cross-country design cannot distinguish from a formative effect. This is not a refutation of the Inglehart-Welzel claim: a within-country or natural-experimental design that isolates formative exposure from secular cohort change might recover a formative component our design cannot identify. What our evidence withdraws is the cross-country cohort gradient often read as confirming it.

Nor does the pattern cohere into a general emancipative cohort effect. The two further items, F120 (abortion) and F121 (divorce), carry smaller gradients that do not move together with F118 or with each other — +0.018 ($p_{\text{WCB}} = 0.018$) and +0.009 (not significant, $p_{\text{WCB}} = 0.375$). The three items do not cohere into the broad emancipative shift a formative mechanism would predict, which is what a secular trend concentrated on the most salient attitudes produces rather than a uniform formative imprint.

# 6 Robustness

## 6.1 Cumulative exposure: the Fuchs-Schündeln pattern is not recovered

A cumulative-exposure specification (Equation 2) does not recover the Fuchs-Schündeln pattern in our cross-country observational variation. Across the five sample-pooled estimates, one (Latinobarómetro Churchill) is marginally positive, one (WVS LATAM-14 E117) positive but not significant, and three are indistinguishable from zero (Appendix D). Because Fuchs-Schündeln and Schündeln identify the cumulative effect most sharply at the post-communist transition, we examined the bloc most analogous to their German case — nineteen post-Soviet and Eastern-European country-clusters — in detail. There the cumulative coefficient on the Churchill item is positive and significant at face value (+0.0131 per year, $p_{\text{WCB}} = 0.039$), but it does not survive the routine verification a referee would request: it collapses to zero when either Albania or Moldova is dropped from the bloc, and a continuous gradient interaction with transition timing fails the wild cluster bootstrap (Appendix D). We read this as non-recovery on our cross-country identification, not as a refutation of the natural-experimental result: the within-Germany comparison holds country and culture fixed while exposure varies, an identifying source our observational design lacks.

## 6.2 Sample composition and cross-source agreement

The results survive two robustness layers, each of which we used along the way and consolidate here. Neither strengthens the headline. The role of the section is to make internal consistency visible.

Composition of the Latinobarómetro panel. Section 3.1 defined the first-transition sample of fourteen Latin American countries by excluding Costa Rica (continuously democratic across the sample window), Venezuela (de-democratization in the post-2010 period generates outward selection of the sample population that confounds the cohort-exposure identification), Peru (re-democratization in 2001 rather than first transition), and Ecuador (interrupted transition). Re-estimating the formative specification on a panel that adds Costa Rica as a never-treated comparison leaves the Churchill, satisfaction, and trust-index coefficients in the same fourth-decimal-place band reported in Section 5.1; re-estimating on a panel that adds Venezuela with the post-2010 selection-bias caveat acknowledged in Section 3.1 likewise leaves the formative null in place. Peru, estimated separately as a single country, returns an under-identified formative specification (one cluster) and is not pooled.

Cross-source agreement on the formative null. Section 5.1 reported the formative coefficient on Latinobarómetro Churchill, Latinobarómetro satisfaction, Latinobarómetro trust-index, World Values Survey LATAM-14 E117, World Values Survey global E117, and World Values Survey global E123 in a single table. The two survey programs, designed and fielded by different teams with different sampling frames and different question wordings of the regime-support concept, return the same null on the same formative-window specification across the same fourteen first-transition countries (where the two samples overlap) and across the fifty first-transition countries (where the World Values Survey panel extends). We do not add a separate cross-source robustness exercise. The consistency is in the headline table.

# 7 Discussion

## 7.1 What cross-country variation can and cannot recover

The design poses one question of each tradition — is the formative cohort effect it implies recoverable from cross-country variation? — and returns two answers.

On regime support, we find no evidence that formative exposure raises it: a precise null across six regime-related outcomes in three independent samples (Section 5.1), bounded by minimum detectable effects and equivalence bounds below the magnitude the literature treats as substantive (Section 5.2). This confirms, with quasi-causal identification across fifty first-transition countries, the Mishler-Rose prediction that diffuse support is not crystallized in the formative years.

On emancipative values, the apparent cohort gradient on the canonical Inglehart-Welzel item is not identified as a formative effect (Section 5.3): it is present among never-exposed cohorts, shows no discontinuity at the transition, and persists where formative exposure is fixed at its cap and formation can no longer move it, leaving it observationally equivalent to a secular trend the cross-country design cannot distinguish from formation. We do not refute the Inglehart-Welzel claim — a within-country or natural-experimental design might recover a formative component ours cannot identify — but we withdraw the cross-country cohort evidence often read as confirming it.

The boundary of this second finding is also the reason for it, and it is worth stating plainly. Our pre-trend tests rule out that the emancipative gradient is a secular trend *predating* the transition. They do not, and no cross-country design can, rule out a modernization process *coincident* with transition timing — rising incomes, the expansion of secondary and tertiary education, the liberalization of media — that lifts the tolerance of younger cohorts and brings democratization in the same calendar window. In cross-country variation, formative democratic exposure and this contemporaneous modernization are not two variables but one: both switch on at the transition, both load on the cohorts coming of age around it. That is why finding two is a non-identification rather than a demonstrated secular trend — we cannot assert that the gradient is modernization rather than formation any more than we can assert the reverse. What we establish is that the cross-country cohort design does not separate them, and that the evidence routinely read as recovering the formative effect is also consistent with a modernization trend the design cannot strip out. The limitation is not a caveat appended to the finding; it is the finding.

The two findings are bound by a single diagnostic: the country-specific cohort-trend control that absorbs most of the emancipative gradient leaves the regime null intact. The cumulative mechanism of **?** is, separately, not recovered in our cross-country variation either (Section 6.1), including in the post-communist bloc where a marginal positive signal on E123 fails leave-one-out and a continuous gradient test; we do not read this as a refutation of their natural-experimental result, which identifies the cumulative question off a within-country discontinuity our observational design does not have. We do not reconcile the three traditions into a settled scheme. We report what their shared formative prediction can and cannot recover from this design.

## 7.2 The European precedent and external validity

**?** document a two-part finding in established European democracies. The first part is the cohort-level stability of generic democratic support: across the cohorts they observe, generic measures of regime support do not decline with the arrival of younger cohorts. The second part is a localized erosion in the liberal-democratic components of support — minority rights, judicial constraints on executives, separation of powers — among certain younger European cohorts, which they characterize as "democrats in name only." Their two-part finding maps cleanly onto the dimensional disaggregation we adopt: the generic-support part falls on our diffuse-support outcomes; the liberal-component part is outside what our design measures.

Our contribution to the European precedent is to extend the first part beyond the consolidated-European setting. The fifty first-transition countries in our WVS global sample have V-Dem first-transit dates listed in Appendix Table 9 that span three decades, from the Dominican Republic (1982) and Argentina (1984) at one end through Indonesia (1999), South Africa (1995), and Tunisia (2012) at the other, with the post-communist transitions of 1989–1991 (Poland, Hungary, Czech Republic, Bulgaria, Romania) forming one cluster within that wider window. The cohort-null on generic regime support is observed across this wider window and not only in the consolidated-European cluster.

The strength of the null is not uniform across the regional partition of the global sample. Its informativeness rests on power: where the minimum detectable effect is calibrated below substantive magnitudes — the global aggregate of fifty country-clusters and the nineteen-cluster post-communist sub-bloc — the cohort-null on diffuse regime support is informative; in regional sub-samples with smaller country coverage the design has correspondingly less power and the null is correspondingly less informative. The external-validity claim we attach to the regime-support null is strongest at the global aggregate and weaker in thinner regional slices. The result that cohorts arriving after democratic transition do not form a stronger diffuse attachment to the regime than the cohorts preceding it is, on our data, a pattern that holds in the global aggregate of first-transition countries rather than a European peculiarity, with the caveat that regional power is uneven.

We do not claim to replicate the second part of the Wuttke-Gavras-Schoen finding. Our outcomes measure regime-related attitudes and the canonical Inglehart-Welzel emancipative item; they do not measure attitudes toward judicial review, minority protections, or executive constraints. The "democrats in name only" qualification of their European result is consistent with — and disjoint from — the regime-support null we extend.

# 8 Conclusion

We asked whether coming of age under democracy durably shapes political attitudes, bringing the formative-window prediction common to three traditions to a single cross-country design. Two findings follow.

First, we find no evidence that formative-window exposure builds support for the democratic regime. The null holds across six regime-related outcomes in three samples, is bounded below the magnitude the comparative literature treats as substantive, and is robust to country-specific cohort trends — confirming, with quasi-causal identification across fifty first-transition countries, the Mishler-Rose prediction that diffuse support is not crystallized in the formative years.

Second, the emancipative cohort effect the modernization literature attributes to formative exposure is not recoverable from cross-country observational variation. The apparent gradient on the canonical Inglehart-Welzel item appears among cohorts who never experienced formative democracy, lacks the discontinuity at the transition that a formative mechanism predicts, and persists undiminished among cohorts whose exposure is fixed at its maximum, where formation can no longer operate — observationally equivalent to a secular cohort trend correlated with transition timing. We do not refute Inglehart-Welzel: a within-country or natural-experimental design might recover a formative component ours cannot identify. What we withdraw is the cross-country cohort evidence often read as confirming it.

The regime null is informative on its own terms, bounded by the minimum detectable effect and the equivalence criterion below substantive magnitudes. The identification is asymmetric (Section 4.1): the same country-specific cohort-trend control that absorbs the emancipative gradient leaves the regime null standing, which is what lets us read one as an informative null and the other as a non-identification.

If diffuse support is not laid down in the formative years, the standing alternative is that it is sustained by contemporary determination — the Mishler-Rose and **?** position that current performance, not biography, drives system support. We framed this at the outset as the cell our design does not estimate; the null sharpens it into the live alternative. Isolating the effect of present conditions on regime support is a distinct identification exercise — within-country temporal variation, with the joint endogeneity of performance and support to confront — that we leave to future work.

# Appendices

## A Latinobarómetro coverage and harmonization

### A.1 Harmonization procedures

The country-code variable changes name between waves: it is `pais` in 1995 and 1996, `idenpa` from 1997 onward, with one further capitalization variant (`IDENPA`) in 2018. The current-age variable changes more frequently. Rather than relying on variable names, we identify the raw-age variable in each wave through a shape-validation rule: we accept as the age variable any numeric variable in a given wave whose maximum value is at least 70 and whose median lies in the interval $[25, 55]$, the signature of an adult-population age distribution. This rule identifies, depending on the wave, one of `s2`, `s7`, `s6`, `edad`, `EDAD`, `S11`, or `S13`. Several older waves contain a binned-age variable with a similar name (e.g., `s10` in 1997), which the rule correctly excludes. The full wave-by-wave variable mapping is reported in the Online Appendix that accompanies this paper.

The 2018 Latinobarómetro file presents a technical complication: 291 of its variables have literal dots in their names (e.g., `P15STGBSC.A`). Stata, in which the bulk of our analysis is conducted, can load such variables but cannot reference them through standard syntax. We preprocess the 2018 file in Python using the `pyreadstat` package, renaming all dotted variables to underscored equivalents (e.g., `P15STGBSC_A`), and use this cleaned file in the stacking pipeline. The clean file is identical in content to the original.

The crime-victimization item used in our sensitivity analysis underwent a scale change between 2009 and 2010 that requires explicit harmonization. In waves 1995–2008 the item is binary (1 = yes, any household member victim of crime in the last twelve months; 2 = no). In 2009 a three-category transitional scale appears (1 = respondent, 2 = relative, 3 = no). From 2010 onward a four-category scale is used (1 = respondent, 2 = relative, 3 = both, 4 = no). We harmonize across the break by collapsing the post-2009 scales to the original binary: a respondent is coded as victim if any positive-category response appears and as non-victim if the explicit "no" category is chosen. The unharmonized recoding using only category 1 across waves would have produced a sharp post-2010 drop in the victim rate driven entirely by scale change rather than by any underlying shift, which is exactly the kind of measurement-induced cohort-by-wave artifact this design is most vulnerable to.

Item-level wave-specific variable codes for each outcome concept were extracted from Latinobarómetro's official 1995–2020 time-series dictionary and supplemented for the 2023 wave through manual identification against the codebook. The full mapping covers 23 waves × 10 outcome concepts and is reported in the Online Appendix, with diagnostic counts grouped by outcome family.

## A.2 Coverage tables

Table 8: Latinobarómetro respondent count by country and wave, LATAM-14 sample.

| Country | 1997 | 1998 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 | 2013 | 2015 | 2016 | 2017 | 2018 | 2020 | 2023 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| DOM | — | — | — | — | — | — | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 15,000 |
| ARG | 1,196 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 25,196 |
| URY | 1,189 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 25,189 |
| BOL | 792 | 792 | 1,080 | 1,075 | 1,242 | 1,200 | 1,201 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 24,182 |
| BRA | 963 | 1,000 | 1,000 | 1,000 | 1,000 | 1,200 | 1,204 | 1,204 | 1,204 | 1,204 | 1,204 | 1,204 | 1,204 | 1,204 | 1,204 | 1,250 | 1,204 | 1,200 | 1,204 | 1,204 | 1,204 | 24,265 |
| CHL | 1,200 | 1,200 | 1,183 | 1,174 | 1,196 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 25,153 |
| NIC | 1,001 | 1,000 | 1,000 | 1,005 | 1,016 | 1,010 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | — | 20,032 |
| COL | 1,200 | 1,198 | 1,200 | 1,199 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 25,197 |
| PAN | 1,021 | 1,000 | 993 | 1,000 | 1,010 | 1,004 | 1,000 | 1,008 | 1,008 | 1,008 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 999 | 1,000 | 1,000 | 21,051 |
| PRY | 575 | 600 | 602 | 604 | 600 | 600 | 600 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 20,981 |
| HND | 1,011 | 1,000 | 997 | 1,000 | 1,004 | 1,006 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 21,018 |
| MEX | 1,105 | 1,200 | 1,166 | 1,253 | 1,210 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 1,200 | 25,134 |
| SLV | 1,007 | 999 | 1,001 | 1,000 | 1,014 | 1,008 | 1,000 | 1,010 | 1,020 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 21,059 |
| GTM | 1,000 | 1,000 | 989 | 1,002 | 1,000 | 1,006 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 20,997 |
| Total | 13,260 | 13,389 | 13,611 | 13,712 | 13,892 | 14,034 | 15,005 | 15,622 | 15,632 | 15,612 | 15,604 | 15,604 | 15,604 | 15,604 | 15,604 | 15,650 | 15,604 | 15,600 | 15,603 | 15,604 | 14,604 | 314,454 |

*Notes.* Respondent count by country-by-wave in the Latinobarómetro 1995–2023 LATAM-14 panel. Latinobarómetro did not field a survey in 1999, 2012, 2014, 2019, or 2022; those columns are absent. The 1995 and 1996 waves used the variable `pais` for country identification and are reported under separate constraints in Section 3.1.

# B World Values Survey global sample

Table 9: WVS global sample of first-transition countries: V-Dem first-transit year, by region.

| Region | ISO3 | Country | First-transition year |
|---|---|---|---|
| Africa | MLI | Mali | 1993 |
| Africa | ZAF | South Africa | 1995 |
| Africa | GHA | Ghana | 1996 |
| Africa | BFA | Burkina Faso | 2000 |
| Africa | KEN | Kenya | 2003 |
| Africa | ZMB | Zambia | 2006 |
| Africa | NGA | Nigeria | 2012 |
| Asia | KOR | Republic of Korea | 1988 |
| Asia | PHL | Philippines | 1988 |
| Asia | MNG | Mongolia | 1991 |
| Asia | BGD | Bangladesh | 1992 |
| Asia | TWN | Taiwan | 1996 |
| Asia | THA | Thailand | 1998 |
| Asia | IDN | Indonesia | 1999 |
| Asia | MYS | Malaysia | 2023 |
| Eastern Europe | BGR | Bulgaria | 1990 |
| Eastern Europe | CZE | Czech Republic | 1990 |
| Eastern Europe | HUN | Hungary | 1990 |
| Eastern Europe | POL | Poland | 1990 |
| Eastern Europe | ROU | Romania | 1990 |
| Eastern Europe | SVN | Slovenia | 1990 |
| Eastern Europe | EST | Estonia | 1993 |
| Eastern Europe | SVK | Slovakia | 1994 |
| Eastern Europe | BIH | Bosnia and Herzegovina | 1997 |
| Eastern Europe | MKD | North Macedonia | 1998 |
| Eastern Europe | HRV | Croatia | 2000 |
| Eastern Europe | SRB | Serbia | 2001 |
| Eastern Europe | ALB | Albania | 2005 |
| Eastern Europe | MNE | Montenegro | 2005 |
| Latin America | DOM | Dominican Republic | 1982 |
| Latin America | ARG | Argentina | 1984 |
| Latin America | URY | Uruguay | 1985 |
| Latin America | BOL | Bolivia | 1986 |
| Latin America | BRA | Brazil | 1987 |
| Latin America | CHL | Chile | 1990 |
| Latin America | NIC | Nicaragua | 1990 |
| Latin America | COL | Colombia | 1991 |
| Latin America | MEX | Mexico | 1996 |
| Latin America | SLV | El Salvador | 1999 |
| Latin America | GTM | Guatemala | 2000 |
| Latin America | PER | Peru | 2001 |
| MENA | TUR | Türkiye | 1990 |
| MENA | PSE | Palestine | 2003 |
| MENA | TUN | Tunisia | 2012 |
| MENA | LBY | Libya | 2013 |
| Post-Soviet | BLR | Belarus | 1992 |
| Post-Soviet | UKR | Ukraine | 1992 |
| Post-Soviet | MDA | Moldova | 1993 |
| Post-Soviet | GEO | Georgia | 2004 |
| Post-Soviet | ARM | Armenia | 2018 |

*Notes.* Countries with a V-Dem-identified first transition from non-democracy to democracy after 1980, observed in the WVS Time Series 1981–2022 v5.0. Regional partition matches the one used in the regional-heterogeneity check of Section 6.2. Coverage and transit years from the V-Dem v15 binary regime series.

# C Institutional confidence index

Table 10: Constituent items of the institutional confidence index (`trust_index`), Latinobarómetro LATAM-14.

| Item | Institution | Wave coverage |
|---|---|---|
| Congress | Congress / National Legislature | All twenty-three waves |
| Government | National government / executive branch | Twenty-one waves; not asked 1997–2001 |
| Police | National police force | All twenty-three waves |
| Armed forces | Armed forces | All twenty-three waves |
| Political parties | Political parties | All twenty-three waves |

*Notes.* Each item is asked on a four-point trust scale (a lot, some, little, none) and binarized to one if the respondent reports a lot or some trust, zero otherwise. The `trust_index` variable is the within-respondent mean of the available items. In the 1997–2001 waves the national-government item is not asked and the index is computed on the remaining four items. Within-wave Cronbach's $\alpha$ for the five-item battery ranges from 0.664 to 0.760; for the four-item version in 1997–2001 it ranges from 0.699 to 0.743.

# D Cumulative-exposure analysis

This appendix reports the cumulative-exposure estimates summarized in Section 6.1: the sample-pooled coefficients across the three samples and the post-communist sub-bloc (Table 11), and the two verifications of the bloc signal — leave-one-out re-estimation across the nineteen bloc countries (Table 12) and the continuous gradient interacting cumulative exposure with transition timing (Table 13).

Table 11: Cumulative-exposure coefficient on diffuse regime support across samples, outcomes, and the post-communist sub-bloc.

| Sample | Outcome | Coefficient | SE | $p_{\text{cl}}$ | $p_{\text{WCB}}$ | N | Clusters |
|---|---|---|---|---|---|---|---|
| Latinobarómetro LATAM-14 | Churchill agreement | +0.00351 | 0.00185 | 0.08 | — | 306,240 | 14 |
| Latinobarómetro LATAM-14 | Satisfaction with democracy (high) | −0.00062 | 0.00162 | 0.71 | — | 318,162 | 14 |
| WVS LATAM-14 | E117 (Having a democratic political system) | +0.00246 | 0.00168 | 0.17 | — | 45,285 | 11 |
| WVS LATAM-14 | E123 (Churchill agreement, six-cluster diagnostic) | — | — | — | — | 9,247 | 6 |
| WVS global 50 countries | E117 (Having a democratic political system) | +0.00017 | 0.00094 | 0.86 | — | 180,649 | 50 |
| WVS global 50 countries | E123 (Churchill agreement) | −0.00025 | 0.00380 | 0.95 | — | 60,785 | 34 |
| Post-communist bloc | E117 (Having a democratic political system) | −0.00110 | 0.00264 | 0.68 | 0.768 | 51,555 | 19 |
| Post-communist bloc | E123 (Churchill agreement) | +0.01310 | 0.00162 | 0.00 | 0.039 | 26,280 | 19 |

*Notes.* Coefficient on cumulative years of post-transition exposure at the survey date from the specification of Equation 2 with country and survey-wave fixed effects and a cubic polynomial in current age. Wild cluster bootstrap $p$-values are reported for the post-communist sub-bloc, where the small number of clusters makes the bootstrap correction informative. The WVS LATAM-14 E123 row is omitted from the table body because the six-cluster estimation is not used for inference. The cumulative specification uses the full sample, with no completed-window restriction, because that restriction is specific to the formative-window estimand (Section 4.2). Its sample sizes accordingly exceed the completed-window $N$'s reported in Table 4.

Table 12: Leave-one-out re-estimation of the cumulative-exposure coefficient on E123 in the post-communist sub-bloc.

| Dropped country | Coefficient | SE | $p_{\mathrm{cl}}$ | $p_{\mathrm{WCB}}$ |
|---|---|---|---|---|
| Albania (ALB) | 0.00000 | 0.00000 | — | 0.120 |
| Armenia (ARM) | +0.01315 | 0.00161 | 0.00 | 0.034 |
| Bulgaria (BGR) | +0.01309 | 0.00162 | 0.00 | 0.035 |
| Bosnia and Herzegovina (BIH) | +0.01553 | 0.00056 | 0.00 | 0.076 |
| Belarus (BLR) | +0.01309 | 0.00162 | 0.00 | 0.035 |
| Czech Republic (CZE) | +0.01308 | 0.00163 | 0.00 | 0.035 |
| Estonia (EST) | +0.01309 | 0.00162 | 0.00 | 0.035 |
| Georgia (GEO) | +0.01306 | 0.00162 | 0.00 | 0.035 |
| Croatia (HRV) | +0.01308 | 0.00162 | 0.00 | 0.035 |
| Hungary (HUN) | +0.01310 | 0.00162 | 0.00 | 0.035 |
| Moldova (MDA) | 0.00000 | 0.00000 | — | 0.155 |
| North Macedonia (MKD) | +0.01123 | 0.00056 | 0.00 | 0.058 |
| Montenegro (MNE) | +0.01262 | 0.00152 | 0.00 | 0.104 |
| Poland (POL) | +0.01309 | 0.00162 | 0.00 | 0.036 |
| Romania (ROU) | +0.01308 | 0.00162 | 0.00 | 0.036 |
| Serbia (SRB) | +0.01467 | 0.00152 | 0.00 | 0.188 |
| Slovakia (SVK) | +0.01311 | 0.00162 | 0.00 | 0.035 |
| Slovenia (SVN) | +0.01309 | 0.00162 | 0.00 | 0.035 |
| Ukraine (UKR) | +0.01313 | 0.00161 | 0.00 | 0.035 |

*Notes.* Cumulative-exposure coefficient on the Churchill item E123 in the post-communist sub-bloc, re-estimated dropping each country in turn. Dropping Albania or Moldova collapses the coefficient to exactly zero, indicating that the bloc-level cumulative signal rests on the variation introduced by those two countries.

Table 13: Continuous gradient: cumulative exposure interacted with distance of country's V-Dem transit year from 1990, post-communist sub-bloc.

| Term | Coefficient | SE | $p_{\text{cl}}$ | $p_{\text{WCB}}$ |
|---|---|---|---|---|
| Cumulative exposure (main) | $-0.04542$ | 0.00112 | 0.00 | 0.306 |
| Cumulative exposure × (transit year minus 1990) | $+0.01306$ | 0.00021 | 0.00 | 0.258 |

*Notes.* Continuous gradient specification on the Churchill item E123 in the nineteen-country post-communist bloc. The interaction term replaces the binary post-communist indicator with the distance of the country's V-Dem transit year from 1990. Cluster-robust $p$-values on both the main coefficient and the interaction term lie below the 1% threshold, but the wild cluster bootstrap returns $p$-values above 0.25 on each, indicating that the cluster-robust significance does not survive the bootstrap appropriate for nineteen clusters.

# E Linear-control decomposition of the emancipative gradient

This appendix reports the linear cohort-trend decomposition referenced in Section 5.3. It is a secondary check: the identification of the secular share rests on the constant-treatment slope of Figure 4, not on the linear-control residual, because a country-specific linear control absorbs the linear projection of any true formative effect along with the secular trend and so can only bound the secular share from above.

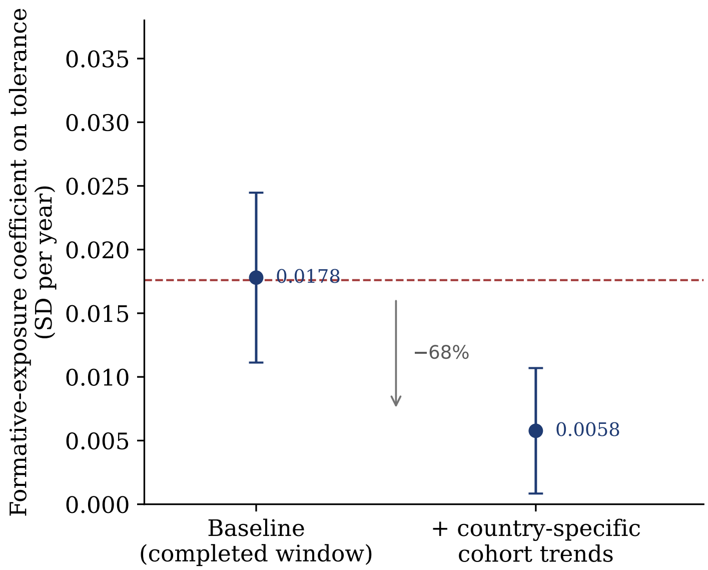


Figure 5: Secular decomposition of the global emancipative gradient (WVS global, completed-window sample). The formative-exposure coefficient on tolerance, in standard deviations per year, falls from 0.0178 to 0.0058 — a sixty-eight-percent reduction — when each country is allowed its own linear birth-cohort trend, so that the coefficient is identified only off departures from a smooth within-country cohort path. Bars are 95% analytic cluster-robust confidence intervals, shown for display, while in-text inference uses the wild cluster bootstrap. The residual lies below the Fuchs-Schündeln reference scale of 0.0176 SD per year (dashed line). This linear control over-absorbs (it removes the linear part of any formative effect too); the uncontaminated secular estimate is the constant-treatment slope of Figure 4.

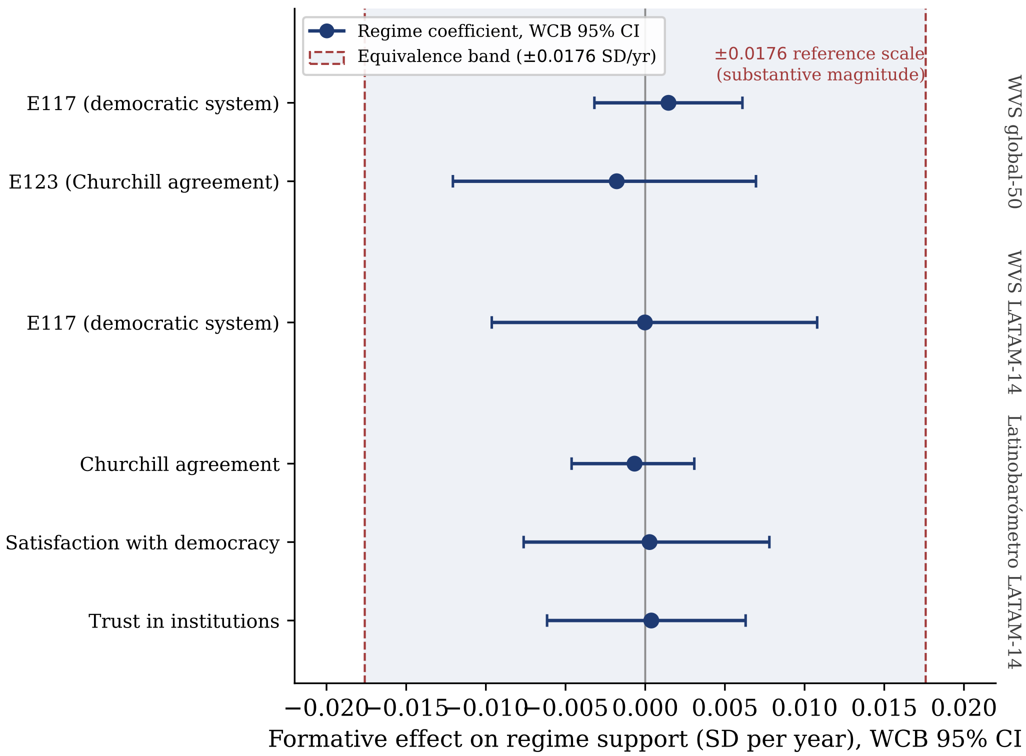


Figure 2: Formative-exposure coefficient on diffuse regime support, six outcomes across three samples, against the substantive-magnitude reference scale. Points are the per-year formative coefficients in standard deviations per year; horizontal bars are wild cluster bootstrap 95% confidence intervals (Webb six-point weights, 9,999 replications). The shaded region bounded by the dashed lines is the $\pm 0.0176$ SD-per-year Fuchs-Schündeln reference scale — the magnitude the comparative literature treats as substantive (Section 4.4). Every confidence interval falls entirely inside the band: the design rules out a formative effect on regime support as large as the substantive benchmark in all six outcomes. This is an informative null, not an underpowered one. Items E117 ("having a democratic political system") and E123 (the Churchill statement) are the World Values Survey regime-support items; the band is a region, not a confidence interval.

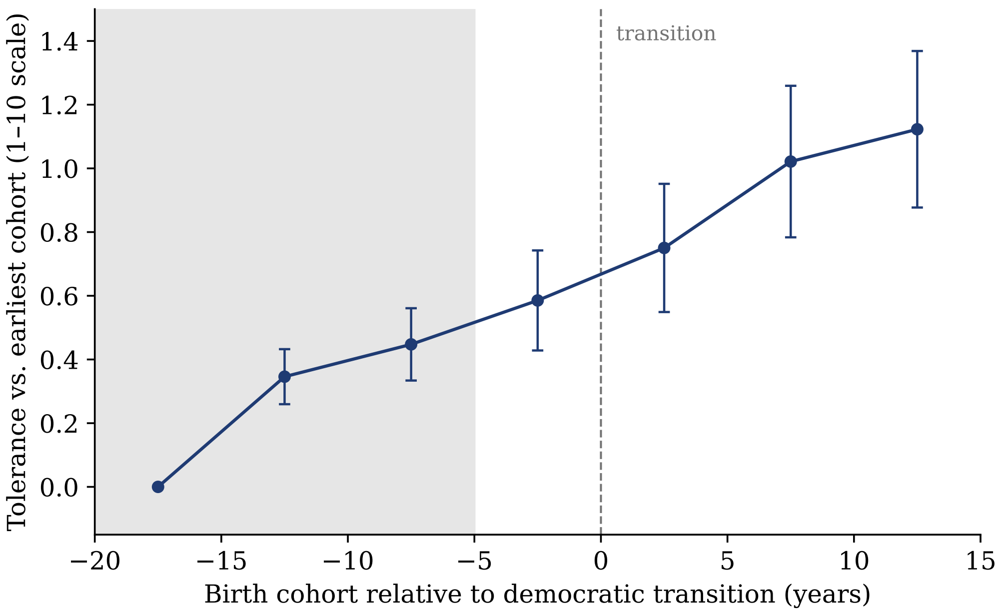


Figure 3: Tolerance by birth cohort relative to the democratic transition (WVS global, completed-window sample). Points are coefficients from a cohort event-study with country and survey-wave fixed effects, measured relative to the deepest pre-transition cohort. Bars are 95% analytic cluster-robust confidence intervals (fifty country-clusters), shown for display, while in-text inference uses the wild cluster bootstrap. The shaded region marks cohorts with zero formative democratic exposure — those whose 16–25 window closed before the transition. The dashed line marks the transition itself. Tolerance rises smoothly and monotonically across the never-exposed region and shows no discontinuity at the transition, the signature of a secular cohort trend rather than a formative effect.

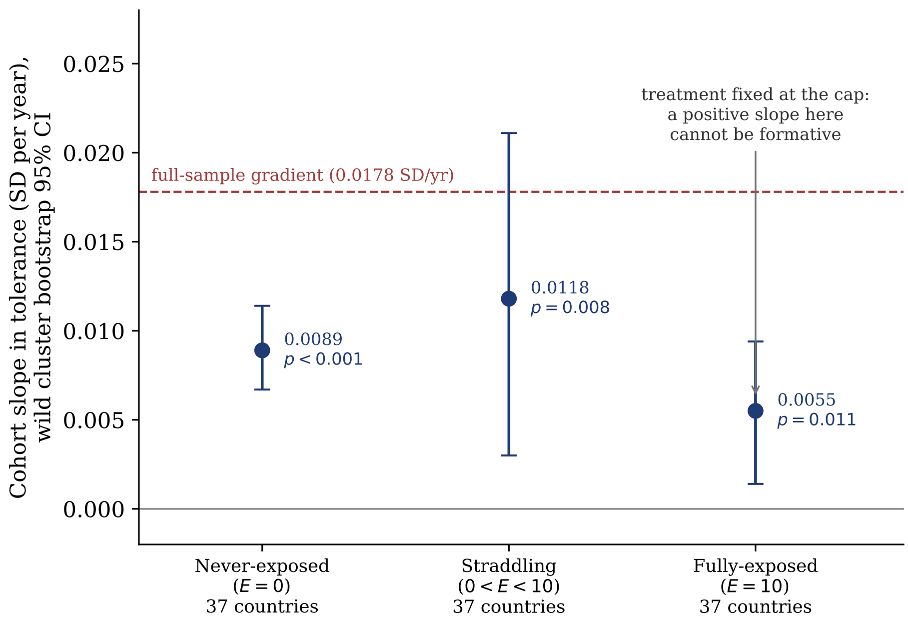


Figure 4: Cohort slope in tolerance by formative-exposure region (WVS global, completed-window sample, the thirty-seven countries present in all three regions). Each point is the within-region coefficient of tolerance on birth cohort with country and survey-wave fixed effects, in standard deviations per year; bars are wild cluster bootstrap 95% confidence intervals (Webb six-point weights, 9,999 replications). All three slopes are positive and bounded away from zero, so the cohort gradient does not plateau where formative exposure stops moving. The fully-exposed region holds formative exposure fixed at the ten-year cap, so its positive slope (0.0055 SD per year, $p_{\mathrm{WCB}} = 0.011$) cannot be a formative effect — it is a secular cohort trend established without any control that could absorb a true formative effect. The straddling region, where exposure is actively changing, is steepest, the qualitative signature of a formative increment, but its excess over the flanks is not statistically separable. The dashed line is the full-sample emancipative gradient (0.0178 SD per year); the three regions each recover a fraction of it.