

Two Logics of Political Action: The Structure and Social Correlates of Democratic and Coercive Action Likelihood

Matthew DeMichele

mdemichele@rti.org

RTI International

William Parkin

RTI International

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Abstract

Many accounts infer substantial public support for political violence from survey responses, but the structure and meaning of those responses are rarely tested. We use a survey of 34,300 U.S. adults from a Census-calibrated online opt-in panel who rated how likely they would be to take each of nine actions, from sharing information to lethal violence, if doing so protected each of fourteen politically salient causes. Three findings emerge. First, prevalence depends on the estimand by an order of magnitude: sustained elevation on the pooled violence score is uncommon (3.9 percent), while 41.8 percent rate violence at least “somewhat likely” for at least one cause. Second, the ratings are better represented by two correlated dimensions than by a single continuum, civic participation and protest forming a democratic-action dimension and disruptive and violent acts a coercive-action dimension; the structure reproduces within each of the fourteen causes and across confirmatory and split-sample analyses, though voting and protest retain secondary relations with the coercive dimension. Third, the dimensions have different correlates: economic grievance is associated with protest only, while vote choice and religious affiliation show larger, differentiated associations with coercive-action likelihood that survive coding, response-quality, salience, and weight-sensitivity checks. All adjusted group means at the violence tier fall between 1.5 and 2.0 on the six-point scale, below its “unlikely” anchor. County measures add little explanatory power and are predominantly null. The findings establish a multidimensional response structure and a set of conditional associations, not a causal account of radicalization.

INTRODUCTION

One line of research reports that substantial minorities of partisans express support for political violence (Kalmoe and Mason 2022). Another shows that such estimates shrink dramatically once inattentive respondents are accounted for (Westwood et al. 2022). Largely missing from this debate is a prior question of whether orientations toward political violence lie at the far end of a participation continuum or form a distinct dimension with its own correlates. If coercive orientations are continuous with participation, the correlates of engagement should also predict coercive likelihood. If they are distinct, models of participation and models of coercive orientations need different variables, and single-item violence measures sit on an unexamined dimension.

The distinction speaks to a standing puzzle as affective polarization is only weakly related to support for political violence (Westwood et al. 2022; Broockman, Kalla, and Westwood 2023). A two-dimensional structure offers a candidate explanation. If affective polarization is more closely related to democratic engagement than to coercive-action likelihood, its weak association with violence support follows from the modest correlation between the dimensions. We test the dimensional structure of political-action responses across the full civic-to-violence range and then ask how social position, partisan behavior, religious affiliation, and community context relate to each dimension.

For every cause, respondents read that “some people believe” the cause matters and will act to protect it, and were then asked, “If it protects [the cause], how likely are you to...” perform each action, on a fully

labeled scale from “no chance at all” to “certain to happen.” These are conditional self-reported action likelihoods, likelihood under the premise that the action would protect something of value, not measures of approval, willingness, or moral endorsement. The distinction is consequential as a respondent may report some likelihood of an action they condemn or approve of an action they consider unlikely. Where we compare our results to studies of explicit approval, we treat the constructs as related but distinct.

The paper proceeds as follows. We first develop the theoretical distinction between democratic and coercive logics of political action and derive seven hypotheses. After describing the survey, the measures, and the analytic strategy, we test the dimensional structure of the action-likelihood items, establish how the prevalence of elevated coercive likelihood depends on measurement choices, and then examine how partisan behavior, religious affiliation, dominant-group identity, and community context relate to each dimension. We close by considering what a two-dimensional structure implies for the study of political violence and for the interpretation of single-item violence measures.

THEORETICAL FRAMEWORK AND HYPOTHESES

From Continuum to Structure: Two Logics of Political Action

Political action refers to behaviors through which individuals seek to influence political outcomes or power relations (Verba and Nie 1972; Verba, Scholzman, and Brady 1995), encompassing practices from conventional participation to contentious and disruptive forms of collective action (McAdam, Tarrow, and Tilly 2001; Tilly 2003). We recast this variation as an empirical problem by drawing on democratic theory to distinguish qualitatively different types of political action and the logics that govern them.

Deliberative democratic theory treats civic engagement, voting, information exchange, participation in institutional practices, as the paradigmatic form of political action because these activities operate through public reason: they seek to influence collective outcomes through claims that can be evaluated, contested, and revised within shared arenas (Dryzek 2000; Habermas 1984). Participatory approaches counter that structured inequalities shape access to those arenas and that disruptive collective action is a legitimate democratic response when formal channels systematically exclude (Fung 2006; Mansbridge et al. 2012; Piven 2008). Protest and civil disobedience are distinct but still democratically oriented forms of engagement, as in the civil rights movement, labor mobilization, and anti-war protest (Morris 1984; Tarrow 2011).

Illegal disruption and political violence occupy a qualitatively different position. Whereas deliberative and participatory modes operate within a shared framework of democratic contestation, coercive action rejects that framework by substituting force for persuasion. Social-psychological research has long drawn a kindred boundary between normative and nonnormative collective action (Wright, Taylor, and Moghaddam 1990; Tausch et al. 2011), and the activism–radicalism intention literature distinguishes legal activism from radical action as separate, correlated dimensions (Moskalenko and McCauley 2009).

The activism–radicalism literature establishes this two-dimensional structure on a single battery of items. Because our instrument crosses nine actions with fourteen causes, we can ask a question single-battery designs cannot: whether the structure is invariant across the political content that motivates action, or whether it holds only for particular causes. We then ask whether the two dimensions have distinct social correlates. Prior survey research, including the designs on which it rests, has typically assumed the ordering of political actions rather than tested it. We treat it as an empirical question; this structural expectation appears as H1 in the hypothesis summary below.

Elite Polarization, Partisan Identity, and Extreme Action Likelihood

A prominent account holds that contemporary polarization is substantially elite-constructed, driven by partisan sorting and cue-taking among the most engaged citizens (Levendusky 2009). On this view the strongest partisan identifiers should show the most extreme action-likelihood ratings. We do not measure elite-message exposure or media content directly; vote choice, party identification, ideology, and general media use are the observable traces of this process available here.

Mason's (2018) account of social sorting captures the mechanism: when partisan identity aligns with other social identities, the result is an identity package activated by partisan stimuli, producing uniformly strong responses across constituent identities. Kalmoe and Mason (2022) show that this identity fusion produces lethal mass partisanship, willingness to endorse violence against political opponents among a meaningful minority of the electorate. Their findings implicate partisan identity rather than ideological extremism as the proximate cause: it is not that ideological conservatives or liberals endorse violence, but that strongly identified partisans of both parties do so.

Vote choice may index electoral alignment, candidate evaluation, political engagement, or identity intensity. The 2024 election, between candidates whose rhetoric about political means and democratic legitimacy diverged sharply, provides a sharp test of whether electoral behavior is associated with action likelihood independently of party identification, and we expect its associations to differ across the two dimensions.

Religious Affiliation and Action-Likelihood Heterogeneity

Religious identity has served as both a mobilizer and a constrainer of political action. Religious communities supply organizational resources, moral framings, and social networks that can channel engagement toward institutional or contentious forms (Morris 1984; Smith 1996; Munson 2008). Because affiliations differ in organizational structure, majority or minority position, and the salience of contested issues, they may relate differently to democratic and coercive action likelihood.

The standard finding that religious affiliation predicts conventional participation may conceal heterogeneity across tiers. Affiliations differ on several dimensions that plausibly matter for conditional action likelihood: organizational integration and the resources it provides; majority versus minority

position, and the political targeting that can accompany the latter; degree of political incorporation; the salience of currently contested issues to the tradition; and the availability of sacred-value framings that turn political conflicts into obligations (Morris 1984; Smith 1996; Juergensmeyer 2003; Bail 2015). These considerations predict affiliation heterogeneity in general; they do not single out any affiliation in advance. This motivates the religion hypothesis (H4), assessed by an omnibus test and pairwise contrasts, and reserve group-specific interpretations, moral shock (Jasper 1997), discrimination exposure, perceived protective exposure, moral authorization, for the discussion, as competing readings of whatever heterogeneity appears.

Status Threat, Dominant-Group Identity, and Coercive-Action Likelihood

Status-threat theory locates the roots of political extremism not in material deprivation but in the perceived erosion of group position (Blumer 1958; Blalock 1967; Mutz 2018). Dominant-group members who perceive their status as declining may be more receptive to extreme action. We test a group-membership version of this claim using the intersecting white, male, and Protestant category; we do not measure subjective threat or identity salience directly, a limitation we return to.

Place, Decline, and Geographic Accounts of Coercive-Action Likelihood

The qualitative literature on place-based resentment offers the most sustained sociological account of community conditions producing extremism. Cramer (2016) documents the rural consciousness of economically declining Wisconsin communities; Hochschild (2016) identifies the “deep story” of queue-jumping animating resentment in Louisiana petrochemical towns; and Gest (2016) traces similar dynamics in post-industrial communities, reading the politics of the abandoned as a coherent response to structural displacement.

These accounts emphasize decline relative to expectation and to others, not absolute deprivation: a community that was prosperous and has declined occupies a different political position than one that has always been poor, and it is where the gap between memory and present is most acute that resentment politics is theorized to take root (Gidron and Hall 2017). That trajectory emphasis has rarely been operationalized quantitatively, cross-sectional studies test levels, not trajectories. We construct county measures of change between 2010 and 2023/2024 in racial composition, foreign-born population, real median household income, and presidential vote margin, and test them alongside static structural conditions, treating the exercise as a trajectory-based assessment of the decline thesis at scale rather than a refutation of the ethnographic record it draws on. Because pooling likelihood ratings across causes spanning the ideological spectrum may obscure directional effects that cancel, all focal analyses are replicated within right-coded, left-coded, and cross-cutting cause clusters (supplement Table S19).

Hypotheses.

H1. The nine action-likelihood items form two distinct but correlated dimensions in their primary loadings, democratic (deliberative and participatory action) and coercive (disruptive and violent action), rather than a single continuum, with the primary-loading boundary at the transition from protest to disruptive or illegal action.

H2. Both Trump and Harris voters show elevated democratic-action likelihood relative to nonvoters, consistent with vote choice as behavioral political engagement.

H3. The Trump–Harris difference reverses sign across the two dimensions: Harris voters are higher on democratic action and Trump voters higher on coercive action, with the contrast tested at and across tiers.

H4. Religious affiliations differ in their conditional associations with democratic and coercive action likelihood, after adjustment for ideology, partisanship, and socioeconomic position.

H5. Respondents in the intersecting white, male, and Protestant category show elevated coercive-action likelihood relative to all other respondents, net of the constituent main effects.

H6. The dominant-group-identity association differs by county context: it is larger in counties experiencing white population loss than in counties that are not, tested by the cross-level interaction between the compound indicator and the county white-population-loss indicator.

H7. County economic decline, demographic change, and political-sorting trajectories are associated with coercive-action likelihood net of individual characteristics.

METHODS

Data and Study Design

Data come from an online survey administered by Ipsos to 34,300 American adults aged 18 and older recruited from its double opt-in panel. Calibration weights raked to Census Current Population Survey benchmarks (age, sex, race and ethnicity, education, region, household income) are applied to all descriptive and regression estimates; measurement models are unweighted, with a weighted sensitivity check. The online supplement benchmarks weighted estimates against probability-sample targets not used in calibration, ideological self-placement, gun ownership, religious affiliation, and 2024 vote, drawn from the General Social Survey, the American National Election Studies, and Pew’s National Public Opinion Reference Survey (Table S3). Because recruitment was nonprobability, calibration does not make the panel probability-representative.

Two properties of opt-in samples bear on our conclusions. First, panel participation plausibly selects on political interest and expressive engagement. That selection may inflate expressed action likelihood,

which would make our rarity estimates conservative; but selection into opt-in panels operates through multiple, partly unknown mechanisms, so we offer this as a directional conjecture rather than an established bound (Meng 2018). Second, opt-in panels are vulnerable to inattentive and acquiescent responding, which can inflate estimates of support for political violence specifically (Westwood et al. 2022; Kennedy et al. 2020; Yeager et al. 2011; MacInnis et al. 2018). We address this with the response-quality procedures described under Analytic Strategy and the exclusion analyses in supplement Table S10, framed as sensitivity analyses, because the high-anchor indicator is defined partly by response extremity and may remove some genuine high-end respondents along with acquiescent ones.

The survey measures self-reported likelihood of political action across a civic-to-violence range. For each of fourteen politically consequential causes, a short preamble states that “some people believe” the cause matters and will act to protect it, followed by the stem “If it protects [the cause], how likely are you to...” applied to nine actions: share information; donate time or money; vote in an election; attend a protest; harass or obstruct government officials; destroy property; threaten someone; seriously hurt someone; and kill someone. Every item uses the same fully labeled six-point scale from “No chance at all” to “Certain to happen.” Cause blocks were presented in randomized order, and the nine actions were presented in randomized order within each block, so the severity gradient in item means cannot be attributed to a fixed presentation order. Twelve causes name a value or right to protect (family, race and ethnicity, religious beliefs, political beliefs, protection from the government, protection from law enforcement, the environment, unborn children, women’s bodily autonomy, gun rights, freedom of speech, and peaceful assembly); two are explicitly threat-framed, protection from “illegal immigrants,” and an item whose preamble states an antisemitic premise (“the Jewish people hold a disproportionate amount of money and power...”) and asks about action “if it protects you from Jewish people.” The complete verbatim instrument, including all fourteen preambles, the randomization scheme, and the shorthand cause labels used in our tables, appears in the supplement’s instrument documentation; the fielded questionnaire accompanies the replication deposit.

County-level contextual variables were linked to respondents using five-digit FIPS codes derived from zip codes. County data were drawn from the U.S. Census Bureau’s American Community Survey (five-year estimates for 2019–2023 and 2006–2010), the MIT Election Lab’s county presidential returns file (2000–2024), the USDA Economic Research Service Rural-Urban Continuum Codes (2023), and Rupasingha, Goetz, and Freshwater’s (2006, updated 2014) social capital index. Table S1 in the online supplement provides the full variable inventory.

Dependent Variables

The four dependent variables are pooled tier scores averaging likelihood ratings across the fourteen causes within each action tier: civic (sharing information, giving time or money, voting in elections), protest (attending demonstrations), disruptive/illegal (harassing or obstructing, destroying property, threatening), and violence (seriously hurting, killing). We adopt the label “disruptive/illegal” in place of “illegal” because the harass-or-obstruct item combines behavior that may be unlawful with tactics, such

as obstruction, that can constitute civil disobedience; a sensitivity analysis dropping this item leaves the measurement structure unchanged. Tier reliabilities and all composite reliabilities are reported in the supplement.

Because the pooled tier scores average across causes spanning the ideological spectrum, they capture an individual's general perceived likelihood for each tier of action, abstracting above the specific cause that motivates it. The pooled outcome is therefore a cause-general measure rather than cause-specific mobilization. Cause-disaggregated robustness analyses partitioning the fourteen causes into right-coded, left-coded, and cross-cutting subsets are summarized in the online supplement (Table S19), with full grids in the replication archive. Crucially, because both the tier construction and the escalation language presuppose claims about how the nine actions relate to one another, we test the dimensional structure directly, factor analysis, cumulative-ordering analysis, and latent class analysis, before estimating any regression model (Table 2; supplement Table S4).

Table 2
Two-Factor Structure of Political Action (Oblimin-Rotated Loadings, Cause-Mean Scores)

Action type	Coercive	Democratic
Share information	-.06	.83
Give time or money	.06	.81
Vote in an election	-.24	.59
Attend a protest	.25	.67
Harass/obstruct officials	.80	.15
Destroy property	.96	.01
Threaten	.94	.03
Seriously hurt	.98	-.03
Kill	.94	-.07

Note. Principal-axis loadings with oblique (oblimin) rotation from the correlation matrix of the nine cause-mean scores treated as approximately continuous (N = 34,300). Eigenvalues 5.12, 2.07; RMSR one-factor .178, two-factor .016; factor correlation $\varphi = .35$. Ordinal robustness: polychoric eigenvalues 5.40, 2.08; ordinal parallel analysis retains two factors; polychoric two-factor RMSR .014, $\varphi = .34$. Confirmatory comparisons, bifactor diagnostics (specific ECV .51, omega- $HS .74$, $H .98$), and split-sample and within-cause replications appear in supplement Section B (measurement validation); the latent class diagnostics appear in supplement Table S7.

Individual-Level Predictors

The individual predictors enter in four theoretically motivated blocks corresponding to the framework above: demographic and socioeconomic controls; political attitudes and media exposure; partisan identity and 2024 vote choice (the fusion account); and religious affiliation with the compound white-

male-Protestant indicator (the moral-authorization and status-threat accounts). The three issue attitudes, abortion, gun ownership, and immigration, are included because they correspond directly to causes measured in the likelihood battery (pro-life and pro-choice advocacy, gun rights, immigration restriction); comparable attitude items for the remaining causes were not fielded, a constraint of the instrument rather than a specification choice.

Demographics. Age (years), sex (female = 1), employed (binary), married (binary) and single, never married (binary; reference includes cohabiting, widowed, and divorced or separated respondents), children in household (binary), household income (eight categories), educational attainment (five categories), and military service (binary). The marital variables correct a transposed-value-label error in the source data (weighted shares: married 51.5 percent, single never married 24.7 percent); a complete frequency audit of every recoded variable accompanies the replication package.

Race and ethnicity. Race and ethnicity are coded as mutually exclusive Census-style categories: Hispanic (any race), and among non-Hispanic respondents, single-race White (reference), Black, Native American, Asian, Pacific Islander or other race, and multiracial (two or more races). Weighted proportions sum to 100 percent (Table 1).

Table 1
Weighted Descriptive Statistics (N = 34,300)

Variable	M	SD
Demographics		
Age (years)	48.104	17.838
Female	0.513	0.500
Employed	0.585	0.493
Married	0.515	0.500
Single, never married	0.244	0.429
Has children	0.313	0.464
Income (1–8 scale)	4.793	1.682
Education (1–5 scale)	3.089	1.108
Military service	0.103	0.304
Race and Ethnicity (mutually exclusive)		
Non-Hispanic White	0.592	0.492
Hispanic (any race)	0.182	0.386
Black (non-Hispanic)	0.116	0.321
Native American (non-Hispanic)	0.009	0.092
Asian (non-Hispanic)	0.066	0.248
Pacific Islander/Other (non-Hispanic)	0.008	0.089
Multiracial (non-Hispanic)	0.028	0.164
Political Attitudes and Behavior		
Ideology (1 = liberal, 7 = conservative)	4.138	1.722
Abortion scale (0 = oppose, 1 = support)	0.665	0.343
Economic grievance (composite)	–0.082	0.778
Gun owner	0.382	0.486
Immigration attitudes (restrictive →)	3.349	1.176

Note. All means and standard deviations are weighted using Ipsos calibration weights raked to 2020 Census CPS benchmarks; calibration weights applied to a nonprobability panel are not probability weights. Race and ethnicity categories are mutually exclusive (Hispanic any race; remaining categories non-Hispanic) and sum to 100 percent. Reference categories in parentheses. N = 34,300.

Variable	M	SD
Demographics		
Media: traditional (1–5)	3.275	1.073
Media: online (1–5)	4.126	1.052
Media: verbal (1–5)	3.881	1.223
Partisan Identity and Vote Choice		
Republican (ref: Independent)	0.366	0.482
Democrat (ref: Independent)	0.340	0.474
Voted Trump 2024 (ref: Did not vote)	0.423	0.494
Voted Harris 2024 (ref: Did not vote)	0.369	0.482
Social class (1 = lower, 4 = upper)	2.505	0.722
Religious Affiliation (ref: Protestant)		
Protestant	0.346	0.476
Catholic	0.252	0.434
Other Christian	0.033	0.179
Jewish	0.024	0.153
Muslim	0.019	0.138
No religion	0.256	0.437
Other religion	0.069	0.253
White × Male × Protestant	0.087	0.282
Dependent Variables: Action Likelihood (1–6 scale)		
Civic actions (pooled)	3.655	1.071
Protest actions (pooled)	2.703	1.318
Disruptive/illegal actions (pooled)	1.720	0.919
Violence actions (pooled)	1.583	0.888
<p>Note. All means and standard deviations are weighted using Ipsos calibration weights raked to 2020 Census CPS benchmarks; calibration weights applied to a nonprobability panel are not probability weights. Race and ethnicity categories are mutually exclusive (Hispanic any race; remaining categories non-Hispanic) and sum to 100 percent. Reference categories in parentheses. N = 34,300.</p>		

Attitudinal. Political ideology is measured on a seven-point scale (1 = extremely liberal, 7 = extremely conservative); supplement Table S11 reports a parameterization using extremity indicators. Abortion attitudes are scaled 0–1 (support-leaning high). The economic grievance composite is the mean of three z-scored components, financial dissatisfaction, worsening financial trajectory (better/–1, same/0, worse/+1), and below-average relative household income, with higher values indicating greater grievance (reliability in supplement). Gun ownership (binary), immigration attitudes (restrictionist high), and three media-exposure scales complete the block; the three issue attitudes correspond directly to causes in the likelihood battery.

Partisan identity, vote choice, religion, and dominant identity. Party identification is captured by Republican and Democrat dummies (Independent=reference). 2024 vote choice is captured by Trump, Harris, Other candidate, and not eligible dummies (Did not vote=reference). Social class is the four-category subjective class identification (1 = Lower to 4 = Upper), used as a continuous ordinal predictor. Religious affiliation collapses twelve original categories to seven: Protestant (reference), Catholic, Other Christian (Mormon, Orthodox), Jewish, Muslim, No Religion (Agnostic, Atheist, None), and Other Religion (Buddhist, Hindu, Other). The white-male-Protestant interaction equals 1 only when all three conditions are met (8.7 percent of the weighted sample).

County-Level Predictors

The county predictor set comprises nine predictors plus three theoretical blocks of trajectory variables. Online supplement Table S2 documents the full missingness audit underlying these specification choices.

Block 1, Demographic change (group threat). Following Blalock (1967), Block 1 captures change in county composition between ACS 2006–2010 and 2019–2023: change in percent non-Hispanic White, percent Hispanic, and percent foreign-born (each in percentage points) and percent change in total population, plus binary indicators for white population loss and population decline.

Block 2, Economic trajectory (relative deprivation). Median household income change (inflation-adjusted to 2023 dollars using BLS CPI-U) and poverty rate change between 2010 and 2023, plus a binary income-decline indicator.

Block 3, Political sorting trajectory. Constructed from MIT Election Lab returns: binary indicators for rightward shift (margin increased by more than 10 points between 2016 and 2024; N = 137 counties) and sorted out (competitive in 2016, noncompetitive in 2024; N = 106 counties). Block 3 variables are available for 76.6 percent of respondents.

Analytic Strategy

Design and inference. The design is cross-sectional at the individual level, and we state at the outset what that permits and precludes. H1 concerns the covariance structure of the nine action-likelihood items and is identified within a single cross-section. H2 through H5 concern conditional associations

between individual characteristics and action-likelihood ratings measured at the same moment; they are estimated and interpreted as associations, and carry no claim about temporal order or causal direction. H6 and H7 pair county measures of change between 2010 and 2023/24 with outcomes observed once, so they test whether respondents living in differently trending counties differ at a point in time; they cannot establish that county trajectories produced those differences, and we do not read them that way. No hypothesis tested here requires panel data, and none is offered as a model of how coercive-action likelihood develops within individuals over time.

The analysis proceeds in three progressive specifications, estimated by weighted least squares with cluster-robust standard errors on county FIPS (Liang and Zeger 1986; Cameron and Miller 2015), appropriate because several focal predictors vary only at the county level. Treating the multi-item tier scores as approximately continuous is supported by evidence on parametric procedures with Likert-type data (Norman 2010). Calibration weights are applied to all descriptive and regression estimates; measurement models are unweighted, with a weighted check in Table S4.

Measurement structure. Before any regression, we test whether the nine action types form the assumed continuum. The primary analysis treats each action-type score, the respondent's mean across the fourteen causes, as approximately continuous and factor-analyzes the correlation matrix of these scores; with fourteen-item averages, the continuous treatment is defensible and avoids discarding information through re-categorization. Two ordinal analyses serve as robustness checks: polychoric factor analysis of the scores rounded to the six response categories, and within-cause analyses of the original ordinal responses for each cause separately. Confirmatory models, one factor, two correlated factors, two factors with two theoretically motivated cross-loadings, and a bifactor model with a general factor plus a coercion-specific factor, are estimated by diagonally weighted least squares on the polychoric correlations, with mean- and variance-adjusted (scaled-shifted, WLSMV-type) test statistics computed from a bootstrap estimate ($B = 200$ resamples) of the asymptotic covariance matrix of the polychoric correlations; normal-theory maximum likelihood fits to the same matrix, which order the models identically. We assess replication in a random split-sample exploratory/confirmatory design and examine loading-pattern similarity across vote, sex, and age subgroups with Tucker's congruence coefficient; congruence indexes pattern similarity and does not establish measurement invariance, which would require multi-group ordinal tests of loading and threshold equality that we leave to future work. Cumulative ordering is assessed overall and conditionally among respondents with any above-threshold upper-tier response, with Loevinger's H. Ordinal latent class models (three to seven classes) are fit to the quality-screened sample with twenty random initializations, reporting entropy, classification uncertainty, and cross-seed stability (supplement Table S7).

Response quality. Opt-in panels are vulnerable to inattentive and acquiescent responding that can inflate violence-related estimates (Westwood et al. 2022; Kennedy et al. 2020). All retained respondents answered four randomly assigned embedded attention checks correctly; initial failure and screen-out counts are reported in the Ipsos field documentation accompanying the ICPSR deposit. We construct two further indicators: completion speed (bottom decile, at or below 607 seconds against a 330-second

fielding floor) and high-anchor straight-lining, an entire nine-item cause block answered at “likely” or “certain,” shown by 4.8 percent of respondents and correlated with violence-tier scores at $r = .38$. Because the high-anchor indicator is defined partly by response extremity, exclusions based on it are sensitivity analyses rather than validity guarantees; all focal models are re-estimated excluding speeders, high-anchor straight-liners, and both (supplement Table S10), and the one result that does not survive is flagged in the text.

Model 1 is the individual-level specification (40 predictors covering demographics, race/ethnicity, attitudes, news consumption, partisan identity, 2024 vote choice, social class, religious affiliation, and the white-male-Protestant interaction; $N = 28,919$). Model 2 adds nine original county-level structural predictors: unemployment, poverty, Gini coefficient, percent college, percent non-Hispanic White, a winsorized social capital index, log median income, log population, and a rural dichotomy ($N = 28,268$). Model 3 adds the Block 1–3 trajectory variables ($N = 21,588$). Sample size falls across models for two reasons: incomplete county-level coverage in the linked administrative data (notably MIT Election Lab returns, which lack county results for Alaska and parts of New England) and listwise deletion on the individual predictors. Online supplement Table S2 documents the cumulative attrition cascade and a comparison of retained and dropped respondents on focal predictors and outcomes.

Estimation and testing details. Because listwise deletion produces different samples across specifications, all nested R^2 comparisons are re-estimated on the common Model 3 sample. Claims that a predictor's association differs across tiers are tested directly with stacked-equation Wald contrasts clustered on respondent, and the Trump–Harris comparison is tested as an explicit contrast at every tier. Religion is assessed first by an omnibus Wald test. County trajectory blocks are evaluated jointly by block F-tests, individual coefficients in the disaggregated grid are subjected to Benjamini–Hochberg false-discovery control, and variance inflation diagnostics are reported for the trajectory block. Item nonresponse before Model 1 (15.7 percent of respondents) is addressed with an attrition comparison and re-estimation on the full sample after a single-pass iterative imputation, a bound check that does not propagate imputation uncertainty and is not multiple imputation (imputation model, variables, and complete-case comparisons in supplement Table S16).

Selection on county characteristics. Observed differences between respondents retained in and excluded from Model 3 are concentrated in county characteristics rather than in the measured individual covariates or outcomes. Comparing the 21,588 retained respondents with the 7,331 dropped from Model 1 (primarily for incomplete MIT Election Lab county coverage) shows trivial differences on the four outcomes (largest $|\text{Cohen's } d| = 0.025$) and on focal individual predictors (largest $|d| = 0.059$, the white $\times$ male $\times$ Protestant contrast). Dropped respondents do reside in counties that are modestly smaller ($|d| = 0.25$), with smaller differences on rurality, inequality, and racial composition ($|d| = 0.09\text{--}0.21$). The attrition primarily reflects the geographic reporting structure of the election data, which lacks county-level returns for Alaska and parts of New England. Supplement Table S2 Panel C reports the full comparison.

Cause-disaggregated robustness analyses. To address whether pooling obscures directionally opposed mobilization, we replicate Model 3 on three cause-valence subsets defined a priori: right-coded (family values, religious freedom, pro-life, gun rights, immigration restriction; n = 5), left-coded (racial justice, environmental protection, pro-choice, police accountability; n = 4), and cross-cutting/other causes (political reform, government accountability, free speech, right of assembly, the antisemitic-premise item (verbatim in the supplement); n = 5). Cluster classification rationale and condensed results are reported in the online supplement (Table S19), with full disaggregated estimates and alternative cause-classification models in the replication archive.

Geographic clustering assessment. Prior to individual-level estimation, we assess geographic clustering using ANOVA-based intraclass correlation coefficients (Bliese 2000), preferred over mixed-effects estimation because the sparse county design (median of four respondents per county across 2,572 counties) produces zero-variance solutions in mixed-effects models.

RESULTS

Descriptive Statistics and Action Tier Distributions

Table 1 presents weighted descriptive statistics for the full survey sample (N = 34,300): 51.5 percent of respondents are married and 24.7 percent single never married; the mutually exclusive race and ethnicity distribution (59.2 percent non-Hispanic White, 18.2 percent Hispanic, 11.6 percent Black) appears in full in the table. Mean ideology is 4.14 on the seven-point scale, 38.2 percent report a gun in the household, and 42.3 and 36.9 percent report 2024 votes for Trump and Harris respectively. The economic grievance composite is standardized by construction. Comparisons of the weighted sample to external probability-sample benchmarks are reported in supplement Table S3; the weighted sample tracks these targets imperfectly, over-representing ideological extremes in particular. The pooled action-tier means decline monotonically across tiers: civic 3.66, protest 2.70, disruptive/illegal 1.72, violence 1.58 on the six-point likelihood scale.

Table 1 about here

The Dimensional Structure of Perceived Political Action

Before asking whose action likelihood is elevated, we ask whether the nine actions form a single ordered continuum. They do not. In the primary analysis, factor analysis of the nine cause-mean scores treated as continuous, two eigenvalues exceed one (5.12 and 2.07), and the two-factor oblique solution (Table 2) separates a democratic-action dimension (sharing information, giving time or money, voting in elections, attending demonstrations) from a coercive-action dimension (harassing or obstructing officials, property destruction, threats, serious injury, killing). Root mean square residual falls from .178 to .016, with an inter-factor correlation of .35. The ordinal analyses agree: polychoric eigenvalues are 5.40 and 2.08, ordinal parallel analysis retains exactly two factors, and the polychoric two-factor solution reproduces the pattern (RMSR .014, $\phi = .34$).

We compared one-factor, correlated two-factor, cross-loading, and bifactor models using ordinal confirmatory factor analysis with DWLS/WLSMV estimation (full estimator details and standardized loadings in Table S4). The one-factor model fits poorly. A correlated two-factor model improves fit substantially, and a model allowing protest and voting secondary loadings on the coercive factor fits best. The preferred model has strong CFI, TLI, and SRMR values, though its RMSEA of .091 remains above conventional close-fit thresholds; the evidence is therefore comparative, showing that two correlated dimensions represent the data better than one, rather than establishing uniformly strong absolute fit. The bifactor alternative does not absorb the coercive dimension: it fits worse than the cross-loading model, its coercion-specific factor retains about half the common variance, and specific loadings exceed general loadings on every coercive item. In the simple two-factor model the single protest indicator reaches the unit boundary, a strain the cross-loading model resolves with all estimates admissible.

The two-dimensional pattern is not an artifact of estimator, sample, or scoring. It replicates within every one of the fourteen causes (two-factor RMSR .010–.024 versus one-factor .136–.186), across a random split-sample design, and with highly similar loadings across Trump and Harris voters, men and women, and younger and older respondents. Dropping the heterogeneous harass-or-obstruct item leaves the structure unchanged. H1 is therefore supported: civic participation and protest load primarily on one dimension, disruptive and violent actions on a second, though voting and protest retain secondary relations with the coercive dimension. The full measurement battery, including the confirmatory, bifactor, split-sample, and within-cause results, is reported in supplement Section B.

The dimensional structure carries directly into the covariate analysis. Regressing factor scores for the two dimensions on the full individual-level specification reproduces the story the tier models tell in detail (Fig. 1): on the coercive dimension, Trump vote, Muslim and Other Christian affiliation, and military service are positive while Harris vote is negative and economic grievance is null; on the democratic dimension both voter groups are positive. Economic grievance is the one association that is tier-specific rather than dimension-general, relating to protest but not to the civic-dominated democratic factor. These factor-score regressions treat estimated scores as observed and are secondary checks; the tier models that follow add resolution within each dimension.

Figure 1 about here

Table 2 about here

How common is elevated coercive-action likelihood depends entirely on what one counts, and the difference is not a technicality but the central descriptive fact. Measured as sustained elevation, a respondent whose average rating across the fourteen causes reaches “somewhat likely,” only 3.9 percent qualify on the pooled violence score. Measured as any cause-specific elevation, at least one violence item rated “somewhat likely” for at least one cause, the figure rises to 41.8 percent (37.7 percent after quality screening). The gap between these numbers is the finding: a large minority gives an elevated violence-likelihood response for at least one cause, while only a small minority reports elevated violence

likelihood across causes. Neither number is the prevalence of approval of violence, and the permissive definitions are mechanically generous, the same any-cause rule yields 94 percent for voting; nearly three-quarters of all violence-item ratings are “no chance at all.” Table 3 reports the full grid with exact labels and denominators.

Table 3
Prevalence of Elevated Action Likelihood under Alternative Definitions

Action	Pooled mean ≥ 4 (%)	Any cause ≥ 4 (%)	Any cause ≥ 4, screened (%)	Any cause certain (%)
Share information	39.2	85.8	85.3	41.6
Give time/money	29.0	84.9	84.3	36.7
Vote in an election	67.8	94.0	94.0	68.2
Attend protest	20.8	66.7	64.5	28.6
Harass/obstruct	5.0	36.6	32.1	12.2
Destroy property	4.4	31.9	26.7	12.0
Threaten	4.8	42.6	38.8	14.2
Seriously hurt	4.5	39.5	35.4	13.7
Kill	3.8	30.0	25.1	11.5
<p>Note. Weighted percentages, N = 34,300 per action. “Pooled mean ≥ 4”: respondent’s mean rating across the fourteen causes at “somewhat likely” or above. “Any cause ≥ 4”: at least one cause rated at that level; “screened” excludes speeders and high-anchor straight-liners. “Any cause certain”: at least one cause rated “certain to happen.” Violence-tier composites (hurt and kill items jointly): pooled 3.9; any-cause 41.8 (37.7 screened); any certain 15.9. The any-cause definitions are mechanically permissive, with fourteen opportunities to qualify. Full response-category distributions and exact denominators appear in the supplement.</p>				

Elevated coercive likelihood is also largely cumulative rather than a separate track: among respondents above the pooled-violence threshold, 87 percent also clear the civic threshold and 86 percent the disruptive/illegal threshold. Binarized tiers conform to strict Guttman ordering for 97.1 percent of respondents (Loevinger’s H = .84); among respondents with any above-threshold upper-tier response, strict conformity is lower, at 76.5 percent, so most, though not all, of those willing to imagine violence also report the ordinary forms of participation beneath it.

The pattern does not depend on the two explicitly threat-framed causes: dropping the immigration and antisemitic-premise items leaves both prevalence estimates and every focal coefficient essentially unchanged (no coefficient shifts by more than 0.02; supplement Table S20).

Table 3 about here

An exploratory latent class analysis on the quality-screened sample (N = 29,478) identifies a class of 8.1 percent of respondents elevated across all five coercive items while remaining engaged on democratic action; its diagnostics establish good classification conditional on five classes, not that five is the preferred number, and full enumeration statistics, composition, and sensitivity appear in supplement Table S7. The central dimensional result does not depend on this analysis.

County-Level Clustering: Intraclass Correlation Coefficients

Between-county clustering is modest. Pooled intraclass correlation coefficients from ANOVA models rise across tiers but remain small (from .01 at civic to roughly .05 at violence), locating most of the variance within rather than between counties and motivating the individual-level focus that follows.

Individual Predictors: Partisan Identity, Religion, and Dominant Group Identity

The clearest individual-level pattern is a partisan crossover (Table 4). Both Trump and Harris voters are far more likely than nonvoters to report democratic action, consistent with vote choice as engagement (H2), but the two diverge sharply on coercive action: relative to nonvoters, Harris voters are approximately indistinguishable at the disruptive/illegal tier and significantly lower at the violence tier, whereas Trump voters are higher at both, and the direct Trump–Harris contrast reverses sign from the democratic to the coercive dimension (H3 supported). Party identification does not behave the same way as vote choice: Republican identification is null across tiers once vote is included, while Democratic identification carries small positive associations throughout. The crossover, not the level, is the result: both vote choices mark greater democratic engagement, but their associations diverge sharply at the coercive tiers.

Table 4
Weighted OLS Regression: Individual Predictor Models

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Age (years)	-0.003*** (0.001)	-0.018*** (0.001)	-0.014*** (0.001)	-0.013*** (0.001)
Female	-0.052** (0.017)	-0.171*** (0.021)	-0.210*** (0.016)	-0.172*** (0.017)
Employed	-0.041* (0.017)	-0.018 (0.020)	-0.037** (0.014)	-0.014 (0.013)
Married	-0.063** (0.019)	-0.117*** (0.023)	-0.018 (0.016)	-0.003 (0.015)
Single, never married	-0.103*** (0.022)	-0.112*** (0.027)	-0.042* (0.021)	-0.011 (0.021)
Has children	+ 0.116*** (0.022)	+ 0.168*** (0.024)	+ 0.146*** (0.016)	+ 0.151*** (0.017)
Income	-0.002 (0.006)	-0.004 (0.007)	-0.019*** (0.005)	-0.022*** (0.005)
Education	+ 0.036*** (0.008)	+ 0.056*** (0.009)	+ 0.001 (0.008)	-0.018* (0.008)
Military service	+ 0.055* (0.024)	+ 0.106*** (0.032)	+ 0.240*** (0.026)	+ 0.264*** (0.027)
Northeast	-0.070* (0.032)	-0.072* (0.030)	-0.040† (0.023)	-0.038† (0.021)
Midwest	-0.044 (0.030)	-0.061* (0.030)	-0.000 (0.030)	-0.009 (0.030)
South	-0.031 (0.020)	-0.044† (0.024)	-0.047** (0.015)	-0.045** (0.014)
Hispanic (any race)	+ 0.040† (0.022)	+ 0.104*** (0.024)	+ 0.052** (0.019)	+ 0.075*** (0.018)
Black (non-Hispanic)	+ 0.142*** (0.027)	+ 0.215*** (0.033)	+ 0.121*** (0.029)	+ 0.199*** (0.027)
Native American (non-Hispanic)	+ 0.005 (0.094)	+ 0.114 (0.106)	+ 0.137† (0.083)	+ 0.150† (0.088)
Asian (non-Hispanic)	-0.062† (0.034)	-0.040 (0.040)	-0.074* (0.030)	-0.076* (0.030)

Note. Unstandardized weighted OLS coefficients with cluster-robust standard errors (county FIPS) in parentheses; Ipsos calibration weights. Mutually exclusive race and ethnicity (reference: non-Hispanic White), marital indicators (reference: cohabiting, widowed, or divorced/separated), and the economic-grievance composite. Reference categories: Independent (party), did not vote (2024 vote), Protestant (religion). N = 28,919. † p < .10; * p < .05; ** p < .01; *** p < .001.

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Pacific Isl./Other (non-Hispanic)	+ 0.018 (0.095)	-0.122 (0.134)	-0.139† (0.082)	-0.119 (0.084)
Multiracial (non-Hispanic)	+ 0.189*** (0.046)	+ 0.248*** (0.056)	+ 0.020 (0.039)	-0.007 (0.035)
Ideology (conservative →)	-0.011 (0.007)	-0.069*** (0.008)	-0.030*** (0.006)	-0.021*** (0.006)
Abortion attitudes (support →)	+ 0.075** (0.025)	+ 0.168*** (0.028)	+ 0.015 (0.021)	-0.027 (0.021)
Economic grievance	-0.001 (0.012)	+ 0.089*** (0.014)	+ 0.012 (0.010)	-0.010 (0.010)
Gun owner	+ 0.144*** (0.015)	+ 0.161*** (0.019)	+ 0.135*** (0.013)	+ 0.144*** (0.012)
Immigration attitudes (restrictive →)	+ 0.004 (0.007)	-0.089*** (0.009)	-0.100*** (0.007)	-0.096*** (0.007)
Media: traditional	+ 0.114*** (0.008)	+ 0.155*** (0.009)	+ 0.081*** (0.007)	+ 0.079*** (0.007)
Media: online	+ 0.092*** (0.007)	+ 0.069*** (0.008)	-0.017** (0.006)	-0.028*** (0.006)
Media: verbal	+ 0.078*** (0.007)	+ 0.061*** (0.007)	-0.001 (0.005)	-0.014** (0.005)
Republican	+ 0.032 (0.023)	+ 0.005 (0.025)	+ 0.013 (0.018)	+ 0.018 (0.018)
Democrat	+ 0.117*** (0.022)	+ 0.157*** (0.026)	+ 0.068*** (0.018)	+ 0.071*** (0.017)
Voted Trump 2024	+ 0.506*** (0.031)	+ 0.213*** (0.034)	+ 0.162*** (0.024)	+ 0.162*** (0.023)
Voted Harris 2024	+ 0.578*** (0.030)	+ 0.349*** (0.033)	-0.037 (0.024)	-0.081*** (0.022)
Voted other	+ 0.335*** (0.050)	+ 0.167** (0.053)	+ 0.106** (0.040)	+ 0.104** (0.040)
Ineligible to vote	+ 0.241*** (0.055)	+ 0.156* (0.061)	-0.028 (0.042)	-0.101* (0.042)
Social class	-0.003 (0.013)	-0.035* (0.014)	-0.042*** (0.011)	-0.045*** (0.011)

Note. Unstandardized weighted OLS coefficients with cluster-robust standard errors (county FIPS) in parentheses; Ipsos calibration weights. Mutually exclusive race and ethnicity (reference: non-Hispanic White), marital indicators (reference: cohabiting, widowed, or divorced/separated), and the economic-grievance composite. Reference categories: Independent (party), did not vote (2024 vote), Protestant (religion). N = 28,919. † p < .10; * p < .05; ** p < .01; *** p < .001.

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Catholic	-0.065*** (0.019)	+ 0.003 (0.025)	+ 0.071*** (0.017)	+ 0.080*** (0.016)
Other Christian	-0.103* (0.045)	+ 0.044 (0.061)	+ 0.232*** (0.052)	+ 0.266*** (0.056)
Jewish	-0.034 (0.043)	+ 0.078 (0.055)	+ 0.139*** (0.036)	+ 0.170*** (0.034)
Muslim	+ 0.058 (0.060)	+ 0.300*** (0.071)	+ 0.402*** (0.075)	+ 0.426*** (0.078)
No religion	-0.229*** (0.023)	-0.074** (0.027)	+ 0.043* (0.017)	+ 0.029† (0.017)
Other religion	-0.062* (0.031)	+ 0.044 (0.036)	+ 0.078** (0.028)	+ 0.079** (0.028)
White × Male × Protestant	+ 0.056† (0.031)	+ 0.101** (0.037)	+ 0.043† (0.023)	+ 0.017 (0.023)
N	28,919	28,919	28,919	28,919
R²	0.128	0.212	0.210	0.225
<p>Note. Unstandardized weighted OLS coefficients with cluster-robust standard errors (county FIPS) in parentheses; Ipsos calibration weights. Mutually exclusive race and ethnicity (reference: non-Hispanic White), marital indicators (reference: cohabiting, widowed, or divorced/separated), and the economic-grievance composite. Reference categories: Independent (party), did not vote (2024 vote), Protestant (religion). N = 28,919. † p < .10; * p < .05; ** p < .01; *** p < .001.</p>				

Religious affiliations differ, and the differences concentrate at the upper tiers (H4). Affiliations differ at several tiers, but the largest and most consistent differences occur for disruptive and violent action likelihood: an omnibus test rejects homogeneity at both upper tiers (disruptive/illegal $F = 10.4$; violence $F = 14.0$; $p < .001$). Comparing adjusted predictions rather than raw coefficients, violence-tier likelihood rises from 1.53 for Protestant respondents (the reference) to 1.61 for Catholic, 1.70 for Jewish, 1.79 for Other Christian, and 1.95 [95% CI 1.80, 2.10] for Muslim respondents, the last estimated on a small sample (analytic $n = 490$; effective weighted $n = 361$; 231 counties). Every one of these adjusted predictions falls between “no chance at all” (1) and “unlikely” (2) on the six-point scale, so the affiliation differences are differences among uniformly low absolute levels. These are conditional associations, not mechanisms, and they are stable across the coding, response-quality, salience, imputation, and weight-sensitivity checks (the Muslim violence coefficient moves only between + .410 and .426 across them). Broad affiliation categories cannot identify doctrine, religiosity, attendance, sacred values, discrimination, or moral authorization; the full margins, contrasts, and robustness grids appear in supplement Section C.

Dominant-group identity does not behave as status-threat accounts predict. As a main effect, the compound white-male-Protestant indicator is not the highest-scoring group and is not significantly

elevated at the violence tier (average marginal prediction 1.60 for the compound group versus 1.58 pooled; the coefficient is + .017, $p = .47$), so H5 is unsupported at the coercive tiers.

The one place dominant-group identity does matter is conditional on local decline (H6). The a priori cross-level interaction between the compound indicator and county white-population loss, a share decline greater than five percentage points, is positive at the violence tier (+ .094, SE .041, $p = .021$): compound-identity respondents are no more coercive than others in stable counties but modestly more so where the White population share has fallen. The result is suggestive rather than conclusive. It also appears under a more permissive 2.5-point threshold (+ .184, $p = .021$) but weakens to marginal under a continuous specification ($p = .060$) and relative-trend cutpoints, and it cannot establish a status-threat mechanism because subjective threat, identity salience, and personal exposure to county change are unmeasured. Panel B of Table 5 and supplement Table S15 report the full interaction, marginal predictions, and sensitivity analyses.

Table 5
Weighted OLS Regression: County Structural Conditions and Trajectories

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Structural conditions (Model 2 and 3)				
Unemployment rate	+ 0.009 (0.012)	+ 0.015 (0.016)	+ 0.009 (0.011)	+ 0.006 (0.010)
Poverty rate	-0.010 (0.028)	+ 0.005 (0.030)	+ 0.002 (0.023)	-0.008 (0.023)
Gini coefficient	+ 0.003 (0.015)	-0.005 (0.017)	+ 0.029† (0.016)	+ 0.035* (0.016)
% college educated	-0.006 (0.022)	+ 0.014 (0.026)	-0.026 (0.018)	-0.023 (0.017)
% non-Hispanic White	-0.004 (0.021)	-0.002 (0.022)	-0.025 (0.017)	-0.026 (0.017)
Social capital index	+ 0.024* (0.011)	+ 0.024† (0.013)	+ 0.018† (0.010)	+ 0.008 (0.010)
Log median household income	-0.021 (0.032)	-0.018 (0.035)	+ 0.019 (0.028)	+ 0.006 (0.027)
Log total population	+ 0.016 (0.017)	+ 0.023 (0.020)	-0.033* (0.016)	-0.042* (0.016)
Rural	+ 0.005 (0.030)	+ 0.012 (0.035)	+ 0.002 (0.025)	-0.019 (0.026)
Trajectories, 2010–2023/24 (Model 3)				
% non-Hispanic White change	+ 0.017 (0.019)	+ 0.035† (0.021)	-0.011 (0.016)	-0.026 (0.016)
% Hispanic change	-0.025 (0.016)	+ 0.000 (0.017)	-0.011 (0.013)	-0.015 (0.014)
% foreign-born change	+ 0.017 (0.014)	+ 0.006 (0.017)	-0.000 (0.012)	-0.005 (0.012)

Note. Unstandardized coefficients with cluster-robust standard errors (county FIPS); Model 3 specification with all individual predictors (Table 4). Model 2 structural-only estimates (N = 28,268) are substantively identical (full estimates in the replication archive). Nested R² on the common Model 3 sample (N = 21,588; supplement Table S8): .210/.213/.213 (disruptive/illegal) and .225/.227/.228 (violence) across Models 1–3; Model 3 covariates account for 83 and 79 percent of between-county variance at those tiers respectively. Panel B: compound-identity respondents number 740 in no-loss counties (410 counties) and 1,137 in loss counties (376 counties); average marginal predictions and the marginal-effects display appear in supplement Table S15 and Figure S1; the interaction is + 0.184* at a 2.5-percentage-point share-decline threshold, marginal for continuous (p = .060) and relative-trend specifications. The sorted-out indicator is retained for transparency but is not robust to threshold or multiplicity scrutiny (see text). † p < .10; * p < .05; ** p < .01; *** p < .001.

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Structural conditions (Model 2 and 3)				
Total population change	+ 0.019 (0.015)	+ 0.014 (0.016)	+ 0.007 (0.013)	-0.003 (0.014)
White population loss (binary)	-0.001 (0.031)	-0.016 (0.036)	-0.021 (0.024)	-0.037 (0.023)
Population decline (binary)	-0.018 (0.030)	+ 0.046 (0.034)	+ 0.021 (0.024)	+ 0.023 (0.024)
Real income change	+ 0.035† (0.021)	+ 0.041 (0.025)	+ 0.026 (0.018)	+ 0.029† (0.017)
Poverty rate change	+ 0.024 (0.022)	+ 0.044† (0.025)	+ 0.011 (0.024)	+ 0.011 (0.025)
Income decline (binary)	+ 0.036 (0.031)	+ 0.010 (0.036)	+ 0.019 (0.026)	+ 0.003 (0.024)
Rightward shift (binary)	+ 0.014 (0.038)	-0.018 (0.042)	+ 0.004 (0.024)	-0.005 (0.024)
Sorted out (10pp definition)	-0.024 (0.028)	+ 0.007 (0.040)	-0.050† (0.026)	-0.053* (0.021)
N / R² (Model 3)	21,588 / 0.133	21,588 / 0.220	21,588 / 0.213	21,588 / 0.228
Panel B: Cross-level interaction (H6), violence tier				
WMP × white population loss				+ 0.094* (0.041)
WMP main effect (no-loss counties)				-0.038 (0.036)
White population loss main effect				-0.046* (0.023)
<p>Note. Unstandardized coefficients with cluster-robust standard errors (county FIPS); Model 3 specification with all individual predictors (Table 4). Model 2 structural-only estimates (N = 28,268) are substantively identical (full estimates in the replication archive). Nested R² on the common Model 3 sample (N = 21,588; supplement Table S8): .210/.213/.213 (disruptive/illegal) and .225/.227/.228 (violence) across Models 1–3; Model 3 covariates account for 83 and 79 percent of between-county variance at those tiers respectively. Panel B: compound-identity respondents number 740 in no-loss counties (410 counties) and 1,137 in loss counties (376 counties); average marginal predictions and the marginal-effects display appear in supplement Table S15 and Figure S1; the interaction is + 0.184* at a 2.5-percentage-point share-decline threshold, marginal for continuous (p = .060) and relative-trend specifications. The sorted-out indicator is retained for transparency but is not robust to threshold or multiplicity scrutiny (see text). † p < .10; * p < .05; ** p < .01; *** p < .001.</p>				

Predictor	Civic	Protest	Disruptive/Illegal	Violence
Structural conditions (Model 2 and 3)				
WMP simple association, loss counties				+ 0.056† (0.030)
<p>Note. Unstandardized coefficients with cluster-robust standard errors (county FIPS); Model 3 specification with all individual predictors (Table 4). Model 2 structural-only estimates (N = 28,268) are substantively identical (full estimates in the replication archive). Nested R² on the common Model 3 sample (N = 21,588; supplement Table S8): .210/.213/.213 (disruptive/illegal) and .225/.227/.228 (violence) across Models 1–3; Model 3 covariates account for 83 and 79 percent of between-county variance at those tiers respectively. Panel B: compound-identity respondents number 740 in no-loss counties (410 counties) and 1,137 in loss counties (376 counties); average marginal predictions and the marginal-effects display appear in supplement Table S15 and Figure S1; the interaction is + 0.184* at a 2.5-percentage-point share-decline threshold, marginal for continuous (p = .060) and relative-trend specifications. The sorted-out indicator is retained for transparency but is not robust to threshold or multiplicity scrutiny (see text). † p < .10; * p < .05; ** p < .01; *** p < .001.</p>				

Several additional patterns merit attention. Military service, an exploratory association without a hypothesis stated in advance, shows accelerating positive associations across tiers, from a modest civic coefficient to an increasingly positive association across tiers, reaching $b = .264$ at violence ($b = .264$, $p < .001$). Online news consumption reverses sign, positive at civic and protest, negative at the upper tiers, though the measure records frequency of general online news exposure, not extremist or social-media content, so this association does not test a radicalization-through-media account. The economic grievance composite is associated with protest likelihood ($b = .089$, $p < .001$) but with neither civic ($b = -.001$) nor disruptive/illegal ($b = .012$) nor violence likelihood ($b = -.010$): grievance is associated specifically with protest likelihood, not participation generally and not coercion. Higher subjective social class is negatively associated with the upper tiers (violence $b = -.045$, $p < .001$); gun ownership is positively associated at every tier (violence $b = .144$, $p < .001$); Black respondents show elevated adjusted likelihood at the upper tiers (violence $b = .199$, $p < .001$), while Native American estimates are positive but imprecise (violence $b = .150$, $p = .09$); and female respondents are substantially lower at every tier (violence $b = -.172$, $p < .001$).

Table 4 about here

County Context: Structural Conditions and Trajectories of Change

Community context adds little. Adding nine structural county measures and then eleven trajectory measures of demographic, economic, and political change barely moves explained variance (Table 5): the R² at the violence tier rises only from .225 to .228 across the three models. A variance decomposition tells the same story, adjustment for observed individual characteristics substantially reduces the estimated between-county component, static county measures reduce it a little more, and change trajectories almost not at all (supplement Section D). No individual trajectory coefficient is stable across specifications.

In short, the measured features of place, its economy, its demographic composition, and its recent trajectory, do little to distinguish who reports coercive-action likelihood, after adjustment for the observed individual characteristics of county residents. The one conditional exception is the identity-by-decline interaction above.

Table 5 about here

Static county measures do include a few significant coefficients (income inequality and population size at the upper tiers), and the joint trajectory block is significant at the violence tier, but no individual trajectory coefficient is stable: the one that reaches significance, a negative “sorted-out” association, appears only at a single competitiveness threshold and does not survive multiplicity correction, so we do not interpret it.

DISCUSSION

Public discourse describes a citizenry increasingly willing to embrace political violence (Levitsky and Ziblatt 2018; Walter 2022; Kalmoe and Mason 2022). Read with the measurement care they require, our findings complicate that narrative. Structurally, conditional action likelihood is better represented by two correlated dimensions, democratic and coercive, than by a single continuum, with primary loadings dividing at the transition from protest to disruptive/illegal action. Substantively, sustained elevation on the pooled violence score is uncommon (3.9 percent), an exploratory latent class elevated across all five coercive items comprises 8.1 percent, and yet a substantially larger share reports elevated likelihood for at least one cause (41.8 percent), a duality the prevalence table makes explicit rather than collapsing into a headline. Elevated coercive-action likelihood is not adequately represented as the far end of one participation spectrum (cf. Converse 1964 on the loosely engaged mass public); it appears as a subset with generally elevated coercive ratings and identifiable correlates. These are perceived likelihoods, not approval, intention, or behavior, and the discussion that follows respects that boundary.

Partisan Identity, Vote Choice, and the Limits of the Fusion Account

The partisan results are consistent with, but do not establish, identity-based accounts of extreme partisanship; identity fusion itself is unmeasured. What the estimates show is behavioral differentiation: vote choice displays larger and more sharply differentiated associations across the two dimensions than party identification, while party identification does not behave uniformly, Republican identification is null across tiers once vote choice is included, but Democratic identification retains smaller positive associations at every tier (violence + .071, $p < .001$). The Trump–Harris contrast reverses sign across dimensions (– .072 civic, – .136 protest, + .199 disruptive/illegal, + .244 violence), and the factor-score models reproduce the crossover at the dimension level. Vote choice may index candidate alignment, issue positions, political interest, identity intensity, or omitted variables correlated with any of these; the design cannot distinguish among them, and whether the pattern reflects 2024 specifically or a general

feature of electoral alignment is a question for longitudinal data. Re-expressing ideology as extremity indicators shows both poles elevated relative to all non-extreme categories at every tier above civic, with the extremely-liberal coefficient larger at protest, disruptive/illegal, and violence (supplement Table S11).

Religious Affiliation and Heterogeneity in Action Likelihood

Muslim respondents show the highest adjusted violence-tier predictions of any affiliation category (average marginal predictions: 1.95 [95% CI 1.80, 2.10] against 1.53 for the Protestant reference, with Other Christian at 1.79 and Jewish at 1.70; supplement Table S13), associations that persist across the robustness analyses reported above and after statistical adjustment for ideology, partisanship, and socioeconomic position. In absolute terms every affiliation category sits below the scale's "unlikely" anchor, and the widest gap between categories is roughly four-tenths of a scale point on a six-point instrument. What they mean is a question these measures cannot settle. Self-reported religious affiliation is not membership, attendance, religiosity, doctrine, identity salience, moral conviction, perceived discrimination, or identity fusion. Under the conditional-likelihood construct a further alternative is available: the stem conditions on efficacy ("if it protects..."), and members of minority religious communities may perceive more circumstances in which such action would be protective, without any difference in approval. Age and migration composition, response style, and unmeasured political engagement remain candidates as well. We therefore present moral authorization, the possibility that religious community provides frameworks within which coercion becomes thinkable when sacred values are at stake (Juergensmeyer 2003; Skitka 2010; Swann et al. 2012), as one hypothesis these findings generate, to be adjudicated by direct measurement. The affiliation differences generate hypotheses about political incorporation, perceived protection, discrimination, and moral authorization, but distinguishing among them requires direct measures beyond the scope of our data.

Status Threat and Dominant Group Identity

The status-threat account fares poorly as an unconditional claim and receives suggestive, definition-dependent support as a conditional one. The compound white-male-Protestant contrast does not survive corrected coding or response-quality screening at the coercive tiers (H5 unsupported), while the same diagnostics leave the vote and religion estimates similar, the asymmetry expected if the latter reflect substantive variation. The a priori cross-level interaction (H6) is present at the primary share-decline definition and a 2.5-point alternative but marginal under continuous and relative-trend specifications, and the implied simple association within loss counties is itself marginal. The evidence is consistent with a context-contingent identity association but does not establish a status-threat mechanism: subjective threat and white identity salience are unmeasured, personal exposure to county change is unknown, and the finding awaits replication.

Place-Based Accounts and the Limits of County Measures

The place-based account, in the operationalizations available here, receives little support. The measured county structural and trajectory variables add little individual-level explanatory power, no trajectory

coefficient is stable across specifications, and the trajectory block shows only aggregate signal (violence-tier joint $F = 2.34$, $p = .007$). Cause-disaggregated replications (supplement Table S19; full grids in the replication archive) reinforce the pooled conclusions: across 132 trajectory tests, five reach $p < .05$ uncorrected and none survives false-discovery control, so the pooled null is not an artifact of directional cancellation. These are conclusions about county compositional measures, not about the ethnographic literature's claims: perceived decline, local narrative, remembered status, and social networks are not measured by ACS change scores, residence duration is unknown, and county aggregation may sit on the wrong scale. The one conditional result, the identity-by-white-population-loss interaction, is consistent with a context-contingent identity association rather than with place as an independent cause.

Theoretical Implications

Three broader implications follow. First, measurement models of perceived political-action likelihood should allow multiple correlated dimensions. The transition from contentious to coercive action appears as a division in the primary loading pattern rather than an intensification of a single dimension, though the correlated factors, cumulative upper-tier patterns, and residual general factor show continuity as well as differentiation (Moghaddam 2005; McCauley and Moskalenko 2011; Moskalenko and McCauley 2009). This structure also offers a candidate account of affective polarization's weak relation to violence support, if affective polarization is more closely tied to democratic engagement than to coercive-action likelihood, the weak association follows from the structure, a conjecture these data cannot test directly. Second, the grievance results sharpen a classic social-movements claim in associational form: economic grievance is positively associated with protest likelihood but with neither routine participation nor coercion, while coercive-action likelihood is associated with vote choice, religious affiliation, and, in an exploratory association, military service. Third, the findings counsel measurement humility for the violence-support literature: prevalence is estimand-dependent by an order of magnitude, latent structure is sensitive to response-quality criteria, and the construct measured, likelihood, approval, intention, must discipline the conclusions drawn.

Limitations and Future Directions

Several limitations bound these conclusions. First and most fundamental, the outcome is conditional self-reported action likelihood: the stem's efficacy premise ("if it protects...") and normalizing preamble ("some people believe...") may elevate absolute levels, so raw levels should not be read as intention prevalence, and nothing here measures approval, intention, or behavior. Second, the sample is a nonprobability opt-in panel; response-quality analyses address inattention, and benchmark comparisons against probability-sample targets (supplement Table S3) show the weighted sample over-represents ideological extremes and the Republican share of the two-party 2024 vote while under-representing Protestant affiliation, so descriptive estimates, especially those tied to ideological extremity, carry that caveat; inference to the national population rests on modeling assumptions regardless (Meng 2018), and the direction of panel-selection bias is a conjecture. Third, the design is cross-sectional and the county analysis is limited by sparse within-county samples and unknown residence duration. Fourth, the

mechanisms our interpretations point toward, moral authorization, identity fusion, threat activation, are unmeasured. The study these findings make necessary pairs the action-likelihood battery with validated instruments for identity fusion (Swann et al. 2012), group identification, moral conviction (Skitka 2010), perceived discrimination, and explicit approval items, on a probability sample, with qualitative work capable of capturing the narratives through which coercion becomes thinkable.

Conclusion

Conditional action likelihood is not adequately represented by a single civic-to-violence continuum. Across fourteen causes, civic participation and protest form one dimension, while disruptive and violent actions form a second, correlated dimension. Sustained elevation on the pooled violence score is uncommon, although cause-specific perceptions of coercive possibility are considerably more widespread. The correlates also differ across dimensions: economic grievance is associated primarily with protest likelihood, whereas vote choice, religious affiliation, and, in an exploratory association, military service show larger associations with upper-tier likelihood. The county measures add little explanatory power and do not identify a stable place-based trajectory, with one small, definition-sensitive cross-level interaction, compound dominant-group identity and county white population-share decline, that merits replication. Because the outcomes measure conditional self-reported action likelihood rather than approval, intention, or action, these findings establish a multidimensional response structure and a set of conditional associations, not a causal account of political radicalization.

Declarations

Author Contribution

The authors contributed mutually developed the study and collected and processed the data. Dr. DeMichele was the lead writer and analyst for this manuscript. Dr. Parkin conducted editing and validation.

Data Availability

The data collected and analyzed for this study will be deposited at the Inter-university Consortium for Political and Social Research at the University of Michigan as required through our research funding by the National Institute of Justice.

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Figures

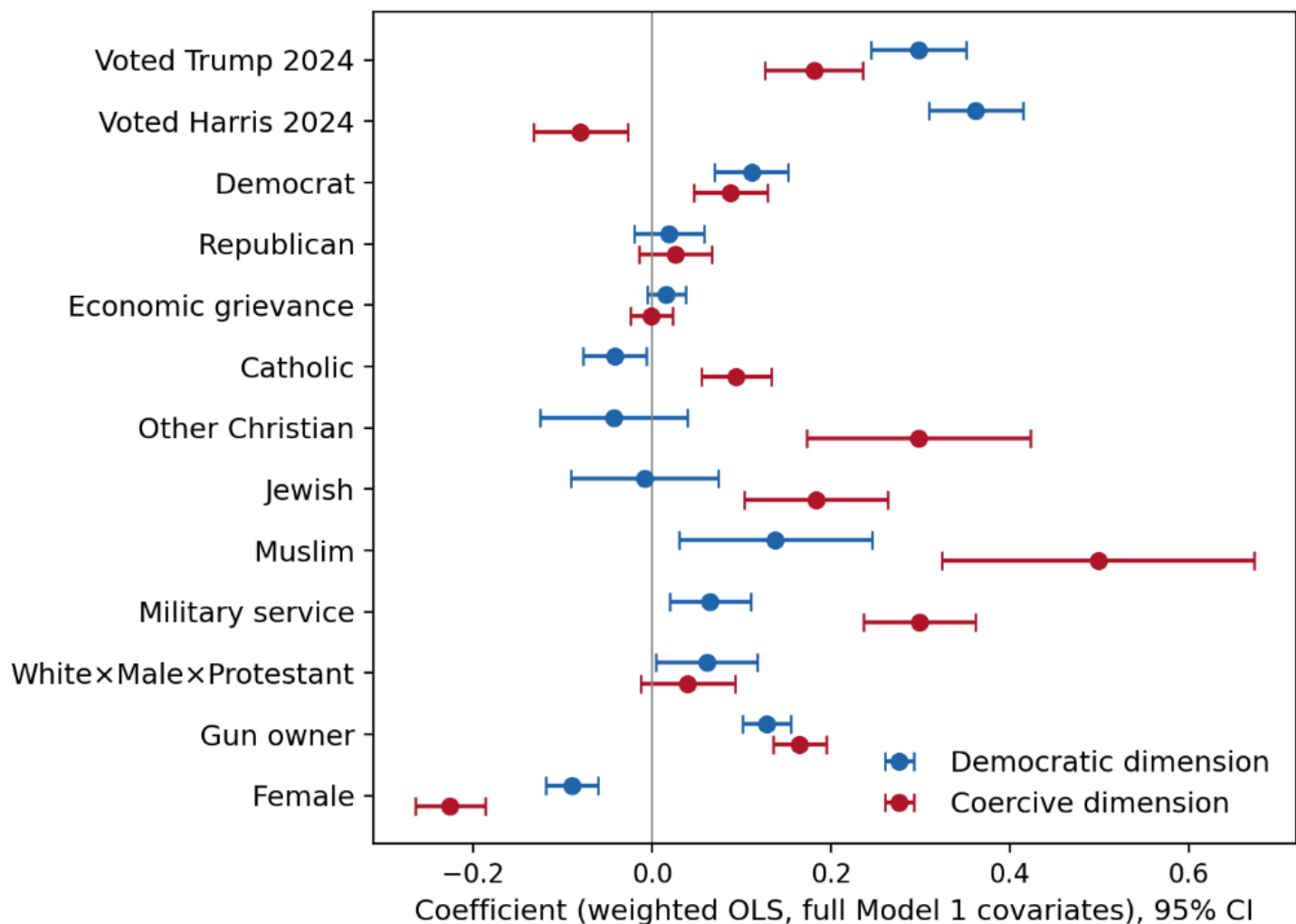


Figure 1

Adjusted associations of focal predictors with democratic and coercive factor scores

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