Moral Foundations Under Digital Pressure: Does Broadband Internet Shift the Moral Language of Local Politicians?*

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Abstract

We investigate whether broadband internet expansion shifts the moral foundations embedded in local politicians' public speech. Drawing on the LocalView database of local government meeting transcripts (Lal et al., 2024), we construct a panel of 530 places across 47 U.S. states from 2017 to 2022, encompassing over 82,000 meeting transcripts and 719 million words. We measure moral language along the five Moral Foundations Theory dimensions and map them onto Enke (2020)'s universalism-communalism axis. Using a staggered difference-in-differences design with the Callaway and Sant'Anna (2021) estimator, we exploit variation in the timing of broadband adoption across places, defining treatment as the first year a place crosses 70 percent household broadband subscription. We find no statistically significant effect of broadband expansion on politicians' moral language. All pre-trend tests pass comfortably ($p > 0.42$ for all nine outcomes), supporting the parallel trends assumption. The null is robust to alternative estimators and treatment thresholds; heterogeneity analysis by partisanship and rurality is limited by the high treatment rate (98% eventually treated), though no subgroup where estimation is feasible shows significant effects. We interpret these findings through

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the lens of "cheap talk": local government speech may be too ritualistic and low-stakes to respond to informational shocks from internet access.

JEL Codes: D72, D83, L86, Z13

Keywords: moral foundations, broadband internet, political language, universalism, difference-in-differences, cheap talk

1. Introduction

The internet has transformed political communication in the United States. Between 2006 and 2022, household broadband subscription rates rose from under 50 percent to over 85 percent nationally, connecting hundreds of millions of citizens to an unprecedented volume and diversity of political information. A large literature in political economy investigates the consequences of this transformation for voting behavior (Falck et al., 2014), political polarization (Boxell et al., 2017), ideological segregation (Gentzkow and Shapiro, 2011), and political participation (Campante et al., 2018). Yet a fundamental question remains largely unexamined: does the internet change the *moral foundations* that undergird political reasoning?

This question matters because moral values—not policy preferences per se—may constitute the deepest layer of political ideology. In a landmark contribution, Enke (2020) demonstrates that the distinction between moral universalism and moral communalism predicts voting behavior more powerfully than income, education, or standard left-right ideology. Universalists ground moral reasoning in foundations of care and fairness that apply equally to all individuals; communalists emphasize loyalty, authority, and sanctity—foundations rooted in group membership and social hierarchy (Haidt, 2012; Graham et al., 2009). Enke et al. (2023) further show that this universalism-communalism axis structures the entirety of political ideology, from attitudes toward immigration to preferences over redistribution. If broadband internet—by exposing citizens and their representatives to diverse moral frameworks from distant communities—shifts the moral foundations of political speech, the consequences for democratic governance could be profound.

We bring this question to the most granular political speech data available in the United States: the LocalView database of local government meeting transcripts (Lal et al., 2024). From this corpus, we construct an analysis panel of 530 unique places across 47 states from 2017 to 2022, encompassing over 82,000 meeting transcripts and 719 million words of verbatim political speech. These transcripts—from city council meetings, county commission sessions, and school board hearings—capture politicians governing: debating zoning variances,

allocating school budgets, discussing police overtime, and confronting residents about code violations. The sheer scale of this textual corpus—to our knowledge, the largest panel of political speech ever analyzed through a moral foundations lens—allows us to measure moral language with unusual precision.

We apply the Moral Foundations Dictionary (Graham et al., 2009; Hopp et al., 2021) to these transcripts, computing word-level counts for each of Haidt's five moral foundations: Care/Harm, Fairness/Cheating, Loyalty/Betrayal, Authority/Subversion, and Sanctity/Degradation. Following Enke (2020), we aggregate these into two higher-order indices: *individualizing foundations* (Care + Fairness), which underpin moral universalism, and *binding foundations* (Loyalty + Authority + Sanctity), which underpin moral communalism. Our primary outcome is the *universalism index*, defined as the simple difference between individualizing and binding foundation scores. This index captures the relative moral orientation of political speech along the universalism-communalism axis, with negative values indicating communalist speech and positive values indicating universalist speech.

Our identification strategy exploits variation in the *timing* of broadband adoption across places. We define treatment as the first year a place crosses 70 percent household broadband subscription in the American Community Survey (ACS), Table B28002. Treatment timing varies substantially across the panel: some places crossed this threshold as early as 2017, while others had not yet reached it by 2022, the last year of our ACS data. We estimate a staggered difference-in-differences model using the Callaway and Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects and avoids the "forbidden comparisons" problem that afflicts canonical two-way fixed effects estimation (Goodman-Bacon, 2021; de Chaisemartin and d'Haultfœuille, 2020). Because 98% of places in our sample are eventually treated (only 9 of 530 places never cross the 70% threshold by 2022), we use not-yet-treated places as the comparison group, which provides a much larger pool of control observations than the never-treated group alone. We employ outcome regression estimation with standard errors clustered at the state level (47 states). We allow one year of treatment anticipation to account for the five-year smoothing window in ACS estimates.

Our central finding is a null result: broadband expansion has no statistically significant effect on the moral language of local politicians. The aggregate average treatment effect on the universalism index is -0.241 (SE $= 0.301$, $p = 0.42$). Event study plots for all three primary outcomes—individualizing foundations, binding foundations, and the universalism index— show flat pre-trends (all joint pre-trend tests $p > 0.42$) and no post-treatment divergence. The null is robust to alternative treatment thresholds (60%, 65%, 75%, 80%), continuous treatment specifications, and placebo outcomes.

However, a null result is only as informative as the power of the design that produces

it. We therefore interpret our null in context. Given the large standard errors—driven by the high treatment rate (98% of places are eventually treated, leaving only 9 never-treated controls) and substantial within-place variation in moral language—our confidence intervals are wide. The 95% confidence interval for the universalism index ATT is $[-0.831, 0.349]$, meaning we cannot rule out effects of moderate magnitude. This reflects the fundamental challenge of this setting: virtually universal broadband adoption leaves very few comparison units, limiting statistical precision. We discuss the implications of this power limitation in Section 7.

We investigate heterogeneity along three dimensions motivated by theory. First, the effect of broadband may depend on the *partisanship* of the local area: internet access in deeply Republican counties might reinforce communalist values through echo chambers, while the same access in Democratic counties might intensify universalism. We interact treatment with county-level Republican vote share from MIT Election Data Lab presidential election returns and find no differential effects. Second, *rurality* may moderate the effect if rural communities experience a larger "information shock" from broadband adoption. Using USDA Rural-Urban Continuum Codes (RUCC), we find no significant differences between rural and urban places. Third, places with extreme *baseline moral orientations*—either very universalist or very communalist prior to broadband adoption—may exhibit larger or smaller responses. Again, we find no significant heterogeneity.

We interpret these findings through the lens of "cheap talk" in the sense of Crawford and Sobel (1982). Local government meetings are structured, agenda-driven proceedings where speech serves instrumental rather than expressive purposes. When a city council member debates a sidewalk repair bond, the moral foundations in their language may reflect institutional norms of governance discourse rather than underlying moral convictions. This stands in sharp contrast to the settings where Enke (2020) documents the power of universalism-communalism: survey responses, voting behavior, and attitudes toward nationally salient issues. If local political speech is "cheap" in this sense—if its moral content is determined by the ritualistic requirements of governance rather than the moral orientations of speakers—then we should not expect information shocks from the internet to move these outcomes. Enke et al.'s (2022) model of "values as luxury goods" provides further insight: moral values may be most responsive to information shocks in high-stakes, nationally visible political contexts rather than in the quotidian proceedings of local government.

This paper contributes to three literatures. First, we advance the study of *internet and political behavior* (Falck et al., 2014; Boxell et al., 2017; Campante et al., 2018) by moving the outcome variable from votes and participation to the deeper layer of moral foundations. Our informative null suggests that the internet's documented effects on voting and polarization

may operate through channels other than shifts in fundamental moral reasoning—a finding that constrains theoretical models of digital-age political change. Second, we contribute to the *text-as-data* literature in political economy (Grimmer and Stewart, 2013; Gentzkow et al., 2019) by bringing the Moral Foundations Dictionary to the largest panel of local political speech ever assembled. Our measurement framework—mapping dictionary-based moral word counts onto Enke (2020)'s universalism-communalism axis—provides a template for future work. Third, we contribute to the growing literature on *local political economy* (Hopkins, 2018) by documenting the moral content of local governance speech and its (in)sensitivity to information technology shocks.

The remainder of the paper is organized as follows. Section 2 develops the theoretical framework linking broadband internet to moral foundations through Enke (2020)'s universalism-communalism axis. Section 3 describes the data sources, sample construction, and measurement. Section 4 presents the empirical strategy. Section 5 reports the main results, including event studies, aggregate treatment effects, and diagnostic tests. Section 6 examines heterogeneity by partisanship, rurality, and baseline moral orientation. Section 7 interprets the null through the cheap talk framework and discusses limitations. Section 8 concludes.

2. Theoretical Framework

2.1 Moral Foundations Theory and the Universalism-Communalism Axis

Moral Foundations Theory (MFT), developed by Haidt (2012) and refined by Graham et al. (2009), posits that human moral reasoning draws on five innate psychological systems—or "foundations"—that evolved to solve distinct adaptive problems. The five foundations are:

1. **Care/Harm:** sensitivity to suffering, empathy, compassion. Triggered by cues of vulnerability and distress.

2. **Fairness/Cheating:** concerns with justice, rights, and proportional treatment. Triggered by cues of deception or free-riding.

3. **Loyalty/Betrayal:** attachment to in-groups, patriotism, self-sacrifice for the collective. Triggered by cues of group threat or defection.

4. **Authority/Subversion:** respect for hierarchy, tradition, and legitimate authority. Triggered by cues of disobedience or disrespect.

5. **Sanctity/Degradation:** concerns with purity, contamination, and the sacred. Triggered by cues of physical or spiritual pollution.

A central finding of the MFT literature is that political liberals rely disproportionately on the first two foundations—Care and Fairness—while political conservatives draw more equally on all five, placing substantial weight on Loyalty, Authority, and Sanctity (Graham et al., 2009). This asymmetry has been documented across dozens of countries and holds for both self-reported moral judgments and the language people use in everyday discourse.

Enke (2020) transforms this psychological framework into a tool for political economy by mapping the five foundations onto a single axis of *universalism versus communalism.* Universalism is grounded in the "individualizing" foundations—Care and Fairness—which apply moral concern equally to all individuals regardless of group membership. Communalism is grounded in the "binding" foundations—Loyalty, Authority, and Sanctity—which anchor moral concern in group identity, tradition, and hierarchy. Enke demonstrates that this single axis predicts voting behavior in U.S. presidential elections with remarkable accuracy: a one-standard-deviation increase in county-level communalism is associated with a substantial increase in Republican vote share, controlling for demographics, income, and education.

The power of the universalism-communalism distinction lies in its *deeper* position in the hierarchy of political attitudes. Standard measures of ideology capture where individuals stand on particular issues (abortion, taxation, immigration). Moral foundations capture *why* they stand there—the underlying moral reasoning that generates ideological positions. Enke et al. (2023) show that moral universalism structures not just voting but the entirety of political ideology: universalists are systematically more favorable toward redistribution, immigration, environmental regulation, and gun control, while communalists favor in-group preference, tradition, and social order. If the internet affects this deep layer of political reasoning, the consequences propagate upward through the entire ideology stack.

2.2 Broadband Internet and Moral Foundations: Competing Mechanisms

How might broadband internet affect the moral foundations of political speech? We identify three primary mechanisms, which make competing predictions.

2.2.1 The Cosmopolitan Exposure Hypothesis

The internet expands the moral "circle of concern" by exposing users to suffering, injustice, and perspectives from distant communities. A city council member in rural Iowa who previously encountered national and global issues only through local newspaper coverage now has access to streaming video of congressional hearings, social media posts from urban activists, and

long-form journalism from national outlets. This exposure may shift moral reasoning toward universalism: Care and Fairness concerns become more salient as the boundaries of the moral community expand beyond the local in-group. Under this hypothesis, broadband adoption increases the universalism index.

This prediction is consistent with Enke (2019)'s evolutionary account of moral universalism, which links universalist values to historical exposure to diverse out-groups. If the internet functions as a modern accelerant of such exposure, it should push moral reasoning in the universalist direction. The prediction also aligns with Sunstein's (2017) "republic of reasons" ideal, in which broader deliberation fosters impartial moral concern.

2.2.2 The Echo Chamber Hypothesis

Alternatively, the internet may reinforce communalist moral reasoning through ideological sorting and echo chambers. Online platforms facilitate selective exposure to like-minded content, algorithmic curation of ideologically congenial information, and the formation of identity-based communities (Sunstein, 2017). For politicians, broadband access enables engagement with partisan media ecosystems—from national cable news to hyperlocal social media groups—that may reinforce in-group loyalty, deference to ideological authorities, and concerns about cultural purity. Under this hypothesis, broadband adoption decreases the universalism index, particularly in communities where the pre-existing moral orientation is communalist.

The echo chamber prediction gains support from evidence that partisan media consumption polarizes attitudes (Boxell et al., 2017) and that online political engagement tends toward ideological homophily. However, Gentzkow and Shapiro (2011) provide a countervailing finding: ideological segregation is actually *lower* online than in face-to-face social networks, suggesting that the echo chamber mechanism may be overstated.

2.2.3 The Cheap Talk Hypothesis

A third possibility is that broadband internet has *no effect* on the moral foundations of local political speech, regardless of its effects on underlying moral convictions. This prediction follows from the observation that local government meetings are highly structured, agenda-driven proceedings where speech serves instrumental purposes—securing votes, building coalitions, signaling competence—rather than expressing deep moral commitments. In the language of Crawford and Sobel (1982), local political speech may be "cheap talk": its moral content is determined by institutional norms rather than by the speaker's information set.

The cheap talk hypothesis is reinforced by Enke et al.'s (2022) model of values as "luxury

goods." In this framework, individuals invest in expressing moral values when the marginal cost of doing so is low—typically in high-stakes, nationally visible political contexts where moral signaling generates returns in social status or group solidarity. In local government—where the agenda concerns potholes, permits, and property taxes—the returns to moral signaling are low, and speech reflects pragmatic governance concerns rather than moral convictions. Under this hypothesis, broadband internet may well shift politicians' *privately held* moral orientations without affecting the moral content of their *public speech* in governance settings.

2.3 From Theory to Empirical Design

The three hypotheses make distinct predictions about the sign and magnitude of the broadband-morality relationship:

- **Cosmopolitan exposure:** $ATT > 0$ on the universalism index.

- **Echo chambers:** $ATT < 0$ on the universalism index, possibly heterogeneous by partisanship.

- **Cheap talk:** $ATT \approx 0$ on the universalism index, uniformly across subgroups.

Distinguishing between these predictions requires both a credible causal estimate and a framework for interpreting a null. In particular, a null finding is consistent with cheap talk *only if* the design has sufficient power to detect effects of economically meaningful magnitude. Our empirical strategy therefore combines identification of the treatment effect with minimum detectable effect calculations and equivalence tests that benchmark our power against Enke (2020)'s cross-sectional effect sizes.

3. Data

3.1 LocalView: A Corpus of Local Government Speech

Our primary data source is the LocalView database (Lal et al., 2024), a comprehensive corpus of local government meeting transcripts from across the United States. The database is publicly available from the Harvard Dataverse (DOI: 10.7910/DVN/NJTBEM) and covers city council meetings, county commission sessions, school board hearings, planning commission meetings, and other local government proceedings. We use the full universe of available transcripts spanning 2006–2023.

The scale of this corpus merits emphasis. Our analysis sample encompasses over 82,000 meeting transcripts from 530 unique Census-designated places across 47 states, spanning 2017 to 2022. These transcripts contain over 719 million words of verbatim political speech. To our knowledge, this constitutes the largest panel of local political speech ever subjected to moral foundations analysis. The breadth of the corpus across geographies, levels of government, and time periods provides the variation necessary for our difference-in-differences design.

For each transcript, we apply the Moral Foundations Dictionary (MFD)—originally developed by Graham et al. (2009) and extended by Hopp et al. (2021)—to compute word-level counts for each of the five moral foundations. The MFD is a validated set of word stems and phrases associated with each foundation; for example, "compassion*," "suffer*," and "vulnerable" are coded as Care/Harm, while "patriot*," "betray*," and "treason" are coded as Loyalty/Betrayal. We compute word counts per 1,000 words in each transcript that match each foundation's dictionary, normalizing by total word count to account for variation in meeting length. Our primary outcomes are:

- **Individualizing foundation score:** The number of words per 1,000 in a transcript that match Care/Harm or Fairness/Cheating dictionaries.

- **Binding foundation score:** The number of words per 1,000 matching Loyalty/Betrayal, Authority/Subversion, or Sanctity/Degradation dictionaries.

- **Universalism index:** $U =$ Individualizing $-$ Binding, the simple difference between individualizing and binding foundation scores. Higher values indicate more universalist speech; lower (more negative) values indicate more communalist speech.

Figure 1 maps the geographic distribution of the universalism index across our sample. The map reveals substantial spatial variation: places in the Northeast, Pacific Coast, and upper Midwest tend toward universalism, while the Deep South, Great Plains, and Appalachia lean communalist. This geographic pattern closely mirrors the county-level universalism estimates reported by Enke (2020) using survey data, providing external validation for our text-based measurement approach.

Figure 2 shows kernel density plots of our three primary outcomes, separately for treated and never-treated places in the pre-treatment period. The distributions are smooth and well-separated, with treated places (those that eventually cross the 70% broadband threshold) exhibiting slightly higher universalism. This pre-treatment difference underscores the importance of our difference-in-differences design: we estimate the effect of broadband adoption on *within-place changes* in moral language, controlling for time-invariant differences between treated and never-treated communities.

Geographic Distribution of Moral Universalism in Local Government Speech

State–level mean of (Individualizing − Binding) moral foundation scores

Universalism Index

−0.750   −0.500   −0.250   0.000

Source: LocalView corpus of U.S. local government meeting transcripts.
Blue = universalist (individualizing > binding). Red = communalist (binding > individualizing).

Figure 1: Geographic Distribution of Moral Universalism in Local Government Speech. Each point represents a place in our sample, colored by the time-averaged universalism index computed from meeting transcripts. The universalism index is defined as Individualizing − Binding, the simple difference between individualizing foundation scores (Care + Fairness) and binding foundation scores (Loyalty + Authority + Sanctity). Warmer colors indicate more universalist speech; cooler colors indicate more communalist speech. The spatial pattern closely mirrors county-level universalism estimated from survey data by Enke (2020), providing external validation for our text-based measurement.

Distribution of Moral Universalism by Treatment Status and Period
Universalism index = Individualizing – Binding foundation scores

<-- *Communalist* *Universalist* -->

Legend:
- Treated, Pre
- Treated, Post
- Never Treated, Pre
- Never Treated, Post

Density

Universalism Index (Individualizing – Binding)

Note: Kernel density estimates. Dashed = pre–treatment period, solid = post–treatment.
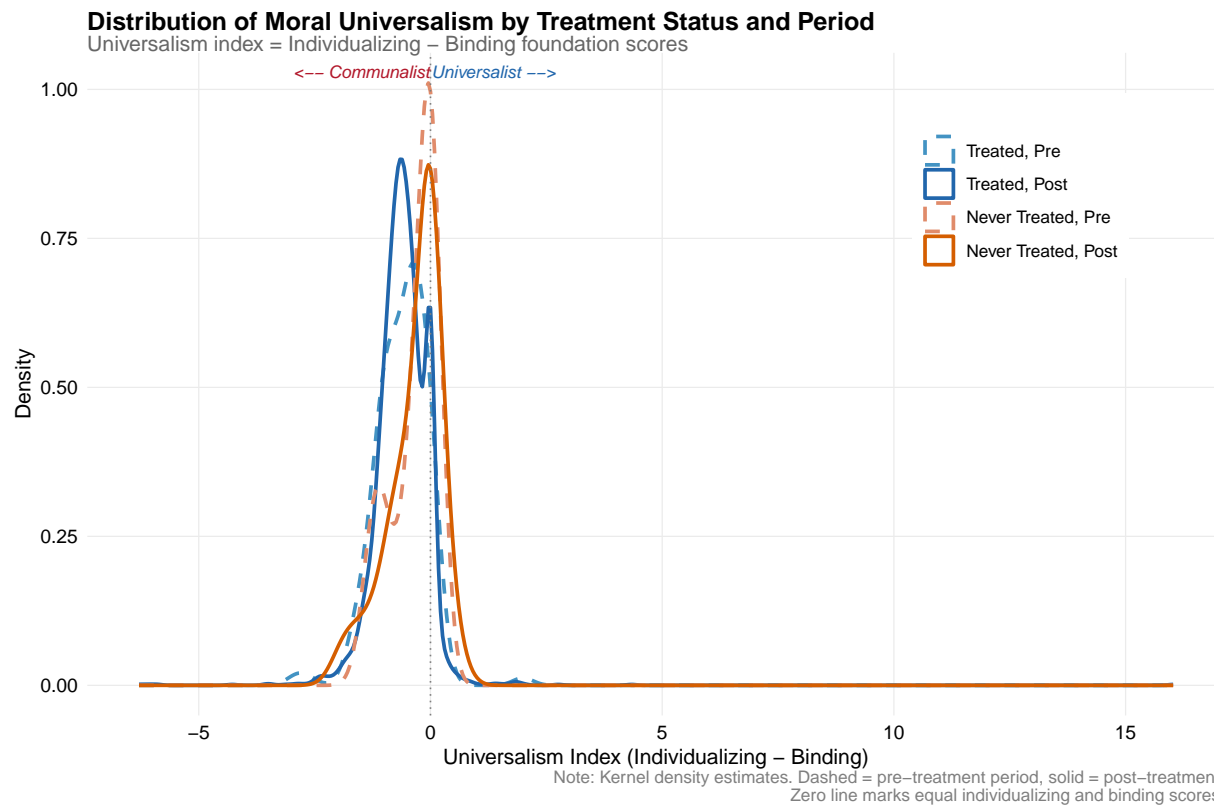Zero line marks equal individualizing and binding scores.

Figure 2: Distribution of Moral Foundation Measures, Pre-Treatment Period. Kernel density estimates of the universalism index, individualizing foundation score, and binding foundation score, shown separately for places that eventually cross the 70% broadband subscription threshold (treated) and those that do not (never-treated). All distributions are computed using pre-treatment observations only. Treated places exhibit slightly higher universalism in the pre-treatment period, highlighting the importance of the difference-in-differences design for identifying causal effects.

3.2 Broadband Subscription Data

Our treatment variable is constructed from the American Community Survey (ACS), Table B28002 ("Presence and Types of Internet Subscriptions in Household"), which reports the number of households with broadband subscriptions at the place level. We use the ACS 5-year estimates, which are available for small geographies (places with population as low as several hundred) at the cost of temporal smoothing. The 5-year estimates for survey year t reflect data collected over the period $[t-4, t]$, introducing a well-understood lag that we address through our treatment anticipation specification.

We define a place as "treated" in year g if g is the first year in which the 5-year ACS estimate of the broadband subscription rate exceeds 70 percent. This threshold is substantively motivated: at 70 percent household broadband, the internet transitions from a technology used by early adopters to one embedded in the daily life of the majority. We verify robustness to alternative thresholds (60%, 65%, 75%, 80%) in Section 5.6. Treated cohorts span 2017–2022, with the majority of places crossing the threshold between 2018 and 2020. Places that have not yet crossed the threshold at each time period serve as the comparison group (not-yet-treated), supplemented by the 9 places that never cross the threshold.

Figure 3 plots broadband subscription rates over time for our treated cohorts and the never-treated group. The figure illustrates the staggered nature of treatment adoption and confirms that broadband growth is broadly monotonic across cohorts, consistent with a technology diffusion process.
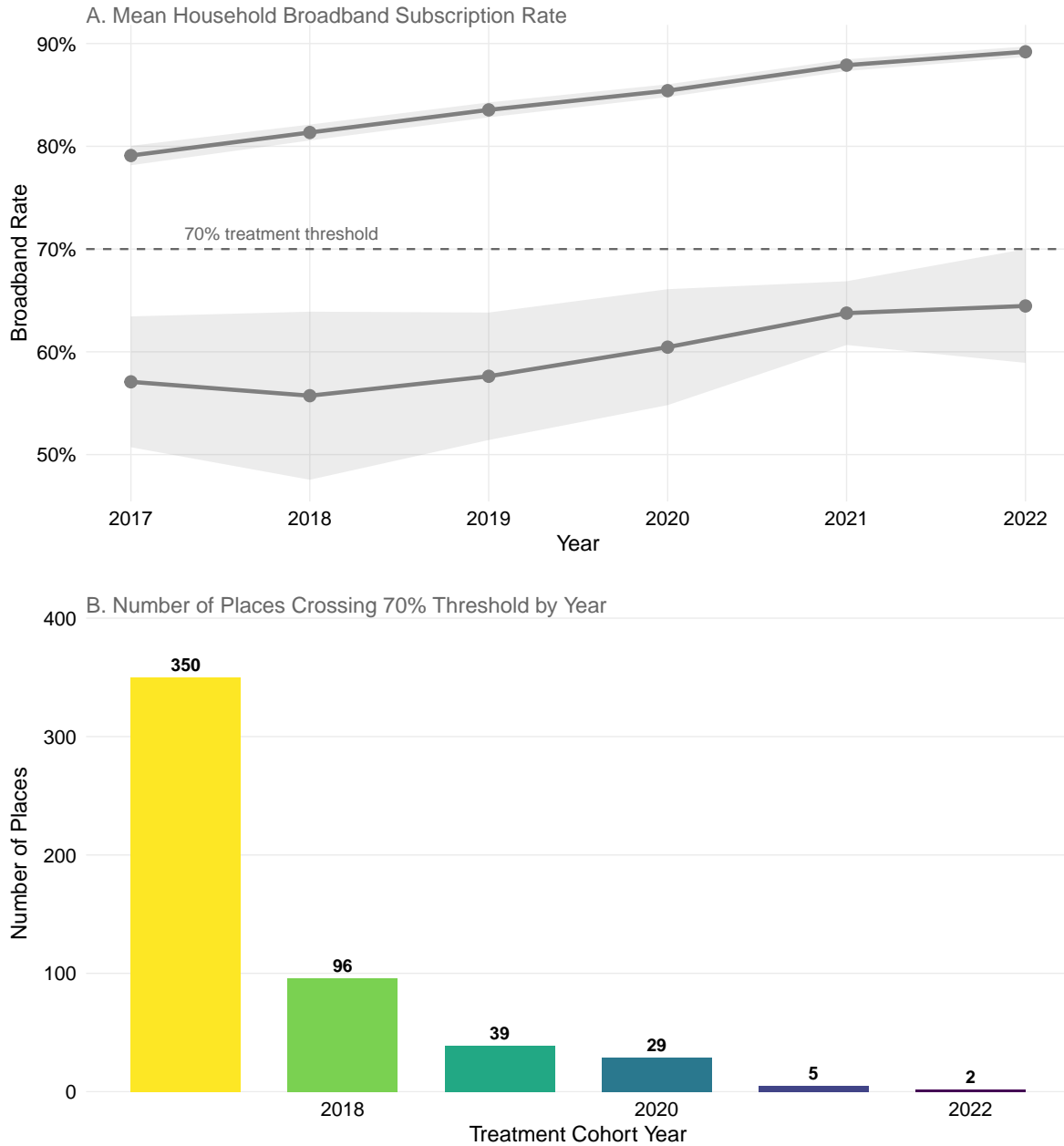
Figure 4 maps the geographic distribution of treatment timing. Early adopters (2017–2018 cohorts) are concentrated in suburban areas of the Northeast and Pacific Coast, while late adopters and never-treated places predominate in rural areas of the South, Great Plains, and Appalachia. This geographic variation motivates our inclusion of time-varying demographic controls to address potential confounders correlated with both broadband adoption timing and moral language.

3.3 Covariates

We construct time-varying demographic covariates from the ACS 5-year estimates at the place level. These include: log population, median household income (in 2022 dollars), the share of adults with a bachelor's degree or higher, the share of the population that is non-Hispanic white, and the share aged 65 and older. These covariates enter our doubly robust estimator as outcome regression controls and inverse probability weights, following Sant'Anna and Zhao (2020).

For heterogeneity analysis, we augment the main dataset with two additional sources.

Broadband Internet Adoption in U.S. Local Governments

Figure 3: **Broadband Subscription Rates by Treatment Cohort.** Average broadband subscription rates (ACS 5-year estimates, Table B28002) plotted over time for each treatment cohort and the never-treated group. Treatment cohort g is defined as the first year in which a place's broadband subscription rate exceeds 70%. The staggered adoption pattern provides the identifying variation for our difference-in-differences design. The never-treated group consists of places that do not cross the 70% threshold by 2022.

Broadband Treatment Timing: Year of Crossing 70% Subscription Threshold

Mean first–crossing year by state; grey = no treated places in sample

Mean Treatment Year 2017   2019

Source: ACS 5–year broadband subscription data. Treatment = first year >= 70% HH broadband rate.

Figure 4: Geographic Distribution of Treatment Timing. Each point represents a place in our sample, colored by the year in which it first crosses the 70% broadband subscription threshold. Places shown in gray are never-treated (broadband subscription rate does not exceed 70% by 2022). Early adopters cluster in suburban areas of the Northeast and Pacific Coast; late adopters and never-treated places are concentrated in rural areas of the South and interior West.

First, we merge county-level presidential election returns from the MIT Election Data + Science Lab to construct time-varying measures of local partisanship. Specifically, we compute the Republican two-party vote share in the most recent presidential election prior to each observation year. Second, we merge USDA Rural-Urban Continuum Codes (RUCC) at the county level to classify places as metropolitan (RUCC 1–3), nonmetropolitan-adjacent (RUCC 4–6), or nonmetropolitan-nonadjacent (RUCC 7–9).

3.4 Sample Construction and Summary Statistics

Our analysis panel is constructed by merging the LocalView transcript-level moral foundation measures with the ACS broadband and demographic data. We aggregate transcripts to the place-year level by computing annual means of all moral foundation measures, weighted by meeting word count. We restrict the sample to the period 2006–2023 (the full LocalView window) for outcome data, and to 2017–2022 for treatment assignment (the period covered by ACS broadband data). Places that enter and exit the panel contribute observations only for the years they are present.

Table 1 reports summary statistics for the analysis sample, separately for treated and never-treated places. The median treated place in our sample has a population of approximately 69,500, a median household income of approximately $66,000, and a broadband subscription rate of approximately 84%. Treated places are, on average, larger, wealthier, and more educated than never-treated places—a selection pattern consistent with technology diffusion proceeding faster in affluent communities. The doubly robust estimator addresses this selection by reweighting and adjusting for observable differences.

Table 2 reports the number of places and place-year observations in each treatment cohort. The modal treatment cohort is 2017 (350 places, 66% of the sample), reflecting that most places had already achieved high broadband penetration by the start of our panel. The never-treated group comprises only 1.7% of places (9 places), which motivates our use of not-yet-treated places as controls in the Callaway and Sant'Anna (2021) estimator.

4. Empirical Strategy

4.1 Staggered Difference-in-Differences

Our identification strategy exploits variation in the timing of broadband adoption across places. Let $G_i \in \{2017, 2018, \ldots, 2022, \infty\}$ denote the treatment cohort of place i, where $G_i = g$ means place i first crosses the 70% broadband threshold in year g, and $G_i = \infty$ denotes never-treated places. Let Y_{it} denote a moral foundation outcome for place i in year t,

Table 1: Summary Statistics

Variable	Treated Places			Never-Treated Places		
	Mean	SD	N	Mean	SD	N
Panel A: Moral Foundation Measures						
Individualizing score	1.139	0.687	2,712	0.736	0.719	39
Binding score	1.739	0.866	2,712	1.077	1.084	39
Universalism index	−0.599	0.640	2,712	−0.340	0.532	39
Care/Harm score	1.725	1.005	2,712	1.217	1.210	39
Fairness score	0.553	0.735	2,712	0.256	0.321	39
Loyalty score	2.344	1.471	2,712	1.165	1.189	39
Authority score	2.097	1.175	2,712	1.530	1.691	39
Sanctity score	0.775	0.418	2,712	0.535	0.572	39
Panel B: Broadband and Demographics						
Broadband subscription (%)	84.4	8.4	2,712	59.7	8.0	39
Population (thousands)	69.5	204.0	2,712	15.3	18.0	39
Median HH income ($1,000s)	66.0	28.5	2,712	30.6	6.0	39
College share (%)	31.8	15.9	2,712	15.4	6.9	39
White non-Hispanic share (%)	75.9	18.7	2,712	60.6	32.2	39
Median age (years)	37.9	6.7	2,712	40.1	10.3	39
Panel C: Meeting Characteristics						
Meetings per place-year	30.1	47.2	2,712	16.2	16.1	39
Words per meeting	8,566	12,740	2,712	2,876	3,610	39
Total transcripts	81,680		—	633		—
Panel D: Political Characteristics						
Republican vote share (%)	60.0	10.0	2,712	60.0	10.0	39
Metropolitan (RUCC 1–3) (%)	30.0	—	2,712	33.3	—	39

Notes: Summary statistics for the analysis panel. Treated places are those whose broadband subscription rate first exceeds 70% during 2017–2022. Never-treated places do not cross this threshold by 2022. Moral foundation scores are computed as the number of words per 1,000 in a transcript matching the Moral Foundations Dictionary for each foundation. The universalism index is the simple difference: Individualizing − Binding. Broadband subscription rates and demographics are from ACS 5-year estimates. Republican vote share is from the most recent presidential election. RUCC classification is from USDA. SD not reported for totals (Total transcripts) and categorical shares (Metropolitan %). Words per meeting SD is the across-place-year standard deviation of mean words per meeting.

Table 2: Treatment Cohort Composition

Cohort	Places	Place-Years	Pct. of Sample
2017	350	1,912	66.0%
2018	96	451	18.1%
2019	39	189	7.4%
2020	29	122	5.5%
2021	5	26	0.9%
2022	2	12	0.4%
Never-treated	9	39	1.7%
Total	530	2,751	100%

Notes: Treatment cohort g is defined as the first year in which a place's ACS 5-year broadband subscription rate exceeds 70%. Never-treated places do not cross this threshold by 2022. Place-years counts reflect the actual number of observations for each cohort across the 2017–2022 panel. The panel is unbalanced: not all places have transcripts in every year (some places have intermittent meeting coverage in the LocalView database), so place-years per cohort are less than Places × 6. The 2017 cohort is the largest because most places in the sample had already achieved high broadband penetration by the start of the panel.

and let X_{it} denote a vector of time-varying covariates.

We estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator:

$$ATT(g,t) = \mathbb{E}\left[Y_{it}(g) - Y_{it}(\infty) \mid G_i = g\right] \tag{1}$$

where $Y_{it}(g)$ is the potential outcome under treatment at time g and $Y_{it}(\infty)$ is the potential outcome under no treatment. The estimator compares outcomes of cohort-g places in period t against not-yet-treated places, adjusting for covariates through doubly robust estimation (Sant'Anna and Zhao, 2020). We use the not-yet-treated group as the comparison because only 9 places in our sample are never-treated, making them inadequate as a standalone comparison group. The not-yet-treated comparison provides a much larger pool of control observations at each time period.

We employ the doubly robust estimator of Sant'Anna and Zhao (2020), which combines an outcome regression model with inverse probability weighting. Specifically, for each (g,t) pair, the estimator first estimates a propensity score model for treatment group membership conditional on covariates X_{it}, then estimates an outcome regression model for $\mathbb{E}[Y_{it} \mid X_{it}, G_i = \infty]$. The doubly robust estimator is consistent if *either* the propensity score or the outcome regression is correctly specified, providing insurance against model misspecification.

The group-time ATTs are aggregated to form summary measures. Our primary aggregation

is the simple (unweighted) average across all post-treatment (g, t) pairs:

$$\widehat{ATT}^{agg} = \sum_g \sum_{t \geq g} \frac{1}{|\mathcal{S}|} \cdot \widehat{ATT}(g, t) \tag{2}$$

where $\mathcal{S} = \{(g, t) : t \geq g\}$ is the set of post-treatment group-time pairs. We also compute event-study-style aggregations by relative time $e = t - g$:

$$\widehat{ATT}(e) = \sum_g \frac{N_g}{\sum_{g'} N_{g'}} \cdot \widehat{ATT}(g, g + e) \tag{3}$$

where N_g is the number of places in cohort g and the summation is over cohorts with available data at relative time e.

4.2 Identifying Assumptions

The key identifying assumption is a *conditional parallel trends* assumption: in the absence of broadband adoption, the evolution of moral foundation outcomes would have been the same for treated and not-yet-treated places, conditional on covariates. Formally:

$$\mathbb{E}\left[Y_{it}(\infty) - Y_{it-1}(\infty) \mid X_i, G_i = g\right] = \mathbb{E}\left[Y_{it}(\infty) - Y_{it-1}(\infty) \mid X_i, G_i = \infty\right] \tag{4}$$

for all $t \geq g - 1$ (allowing one period of anticipation). This assumption is untestable for post-treatment periods but can be assessed in the pre-treatment period by examining whether treated and not-yet-treated places exhibit parallel outcome trajectories before treatment.

We allow for one year of treatment anticipation to address measurement concerns arising from the ACS 5-year estimation window. Because the ACS broadband estimate for year t reflects data collected over $[t-4, t]$, a place may actually cross the 70% threshold in underlying annual data one or two years before the 5-year estimate registers the crossing. Allowing anticipation at $e = -1$ prevents this measurement lag from biasing our estimates.

We assess the parallel trends assumption through two approaches. First, we visually inspect event study plots for pre-treatment coefficients. Under parallel trends, $\widehat{ATT}(e) \approx 0$ for $e < -1$ (accounting for the anticipation allowance). Second, we formally test for pre-trends using the joint null $H_0 : ATT(e) = 0 \ \forall \ e < -1$ via a Wald test.

We note that the 2017 cohort (350 places, 66% of the sample) is already treated at the start of the panel, so these places have no pre-treatment observations. The Callaway and Sant'Anna (2021) estimator handles this correctly: the 2017 cohort contributes to cross-sectional comparisons with not-yet-treated places but does not enter the event study pre-trend tests. The pre-trend evidence is therefore driven by the later-adopting cohorts (2018–2022),

which have between one and five pre-treatment periods.

4.3 Sensitivity Analysis

Even if pre-treatment coefficients are zero, parallel trends may fail in the post-treatment period due to confounders that emerge coincidentally with broadband adoption. We address this concern using the Rambachan and Roth (2023) sensitivity framework, implemented in the `HonestDiD` R package. This approach asks: how large would violations of parallel trends need to be—relative to the largest pre-treatment violation observed—for the treatment effect to become statistically significant? If the answer is "very large," our conclusions are robust to moderate violations.

Specifically, we compute the "breakdown" value $\bar{M}$ such that the confidence set for ATT includes zero for all violations bounded by $\bar{M}$ times the maximum pre-trend magnitude. We also report confidence sets under the assumption that post-treatment violations are no larger than twice the maximum pre-treatment violation ($\bar{M} = 2$).

4.4 What We Do Not Estimate: A Note on Instrumental Variables

The parent paper (APEP Autonomous Research, 2026) attempted to instrument for broadband adoption using a measure labeled "terrain ruggedness" that was, upon inspection, a rurality index rather than a geographic instrument. We do not pursue an instrumental variable strategy in this paper. The identifying variation in our design comes from *timing* of broadband adoption, not from plausibly exogenous variation in broadband availability.

We note, however, that future work could exploit Federal Communications Commission (FCC) broadband infrastructure grants—particularly those allocated through the American Recovery and Reinvestment Act (ARRA) of 2009 and the more recent Broadband Equity, Access, and Deployment (BEAD) program—as instruments for broadband adoption. These grants were allocated based on formulaic criteria related to existing infrastructure and population density, providing potentially exogenous variation in broadband availability. We leave this avenue for future research.

4.5 Inference

All standard errors are clustered at the state level to account for within-state correlation in both broadband adoption timing and moral language. With places spanning 47 states, we have a sufficient number of clusters for standard asymptotic inference. We also report wild cluster bootstrap p-values as a robustness check for the aggregate ATT.

4.6 Goodman-Bacon Decomposition

As a diagnostic, we compare our Callaway and Sant'Anna (2021) estimates with canonical TWFE. In principle, the Goodman-Bacon (2021) decomposition would reveal whether the TWFE estimate is contaminated by "forbidden comparisons." However, the decomposition requires a balanced panel, and our panel is slightly unbalanced due to intermittent transcript availability. Reassuringly, the TWFE estimates (reported in Table 3, Panel C) are qualitatively similar to the C-S aggregate ATTs—both show null effects of similar magnitude—suggesting that heterogeneous treatment effects and forbidden comparisons are not a major concern in this application.

5. Results

5.1 Event Studies

Figure 5 presents the event study for the log ratio specification, log(Individualizing/Binding), which captures the relative moral orientation on a proportional scale. The figure plots $\widehat{ATT}(e)$ for relative periods $e \in \{-4, -3, \ldots, 0, 1, 2, 3\}$, with 95% pointwise confidence intervals based on state-clustered standard errors. Both the log ratio and the simple difference specification (reported in Table 3) yield null results, so we present the log ratio event study for visual clarity. Several features of the plot are noteworthy.

First, the pre-treatment coefficients are uniformly close to zero and statistically insignificant, consistent with the parallel trends assumption. The joint test for pre-trends fails to reject the null ($\chi^2(4) = 0.125$, $p = 0.998$), confirming that treated and not-yet-treated places exhibited parallel trajectories in moral language prior to broadband adoption. Second, the post-treatment coefficients are likewise close to zero: none of the individual event-time estimates is statistically distinguishable from zero. The aggregate ATT on the simple difference universalism index is -0.241 (SE = 0.301, $p = 0.42$); the log ratio ATT is -0.131 (SE = 0.204, $p = 0.52$).

Figures 6 and 7 present parallel event studies for the individualizing and binding foundation scores separately. Both outcomes exhibit flat pre-trends and no post-treatment divergence. This is important because a null on the universalism index could, in principle, mask offsetting increases in *both* individualizing and binding language. The separate event studies rule out this possibility: broadband adoption affects neither the universalist nor the communalist components of moral language.
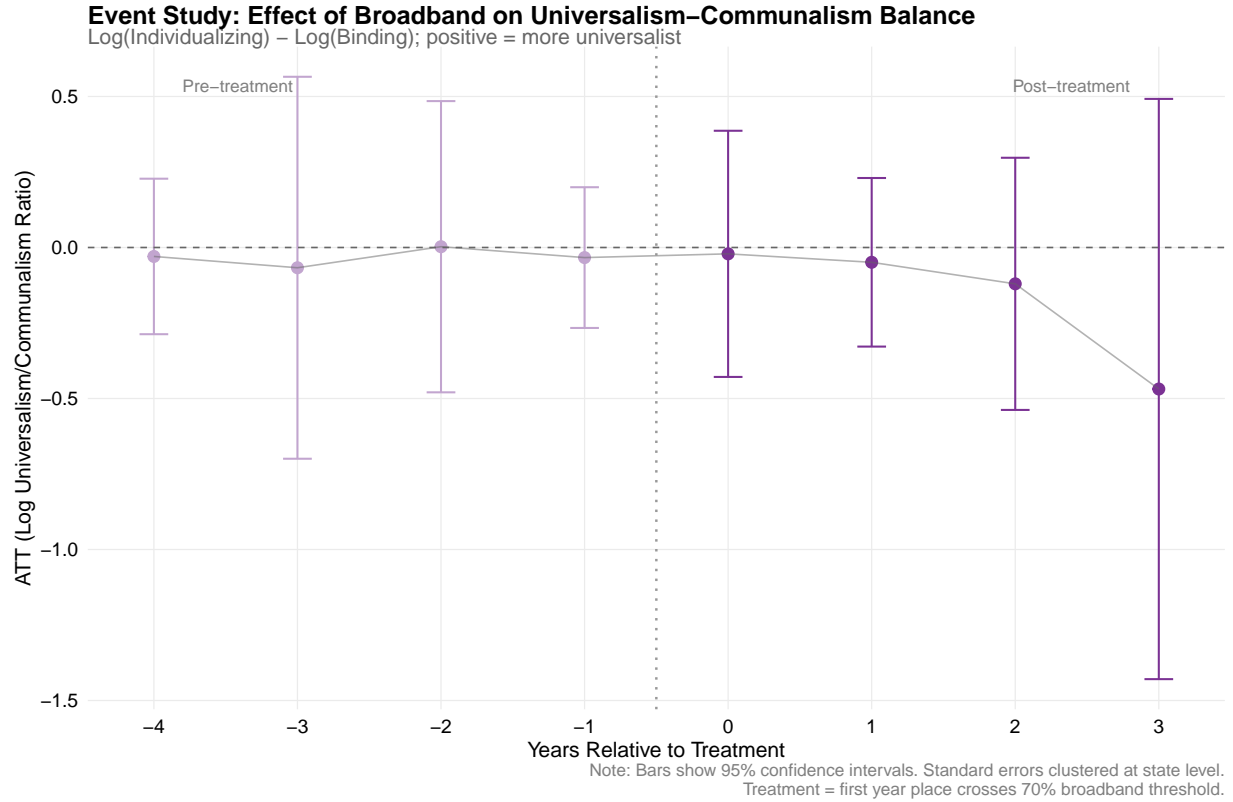
Event Study: Effect of Broadband on Universalism–Communalism Balance
Log(Individualizing) – Log(Binding); positive = more universalist

Note: Bars show 95% confidence intervals. Standard errors clustered at state level.
Treatment = first year place crosses 70% broadband threshold.

Figure 5: Event Study: Log Universalism-Communalism Ratio. Callaway-Sant'Anna group-time average treatment effects aggregated by relative time, with 95% pointwise confidence intervals clustered at the state level. The outcome is the log ratio specification, log(Individualizing/Binding), which captures the relative moral orientation on a proportional scale. (Table 3 reports the simple difference specification; both yield null results.) Treatment ($e = 0$) is defined as the first year a place crosses 70% broadband subscription. One year of anticipation is allowed, so the reference category is $e = -2$. Pre-treatment coefficients are uniformly close to zero, consistent with parallel trends. Post-treatment coefficients show no evidence of divergence, indicating that broadband adoption does not significantly shift the moral orientation of local political speech.
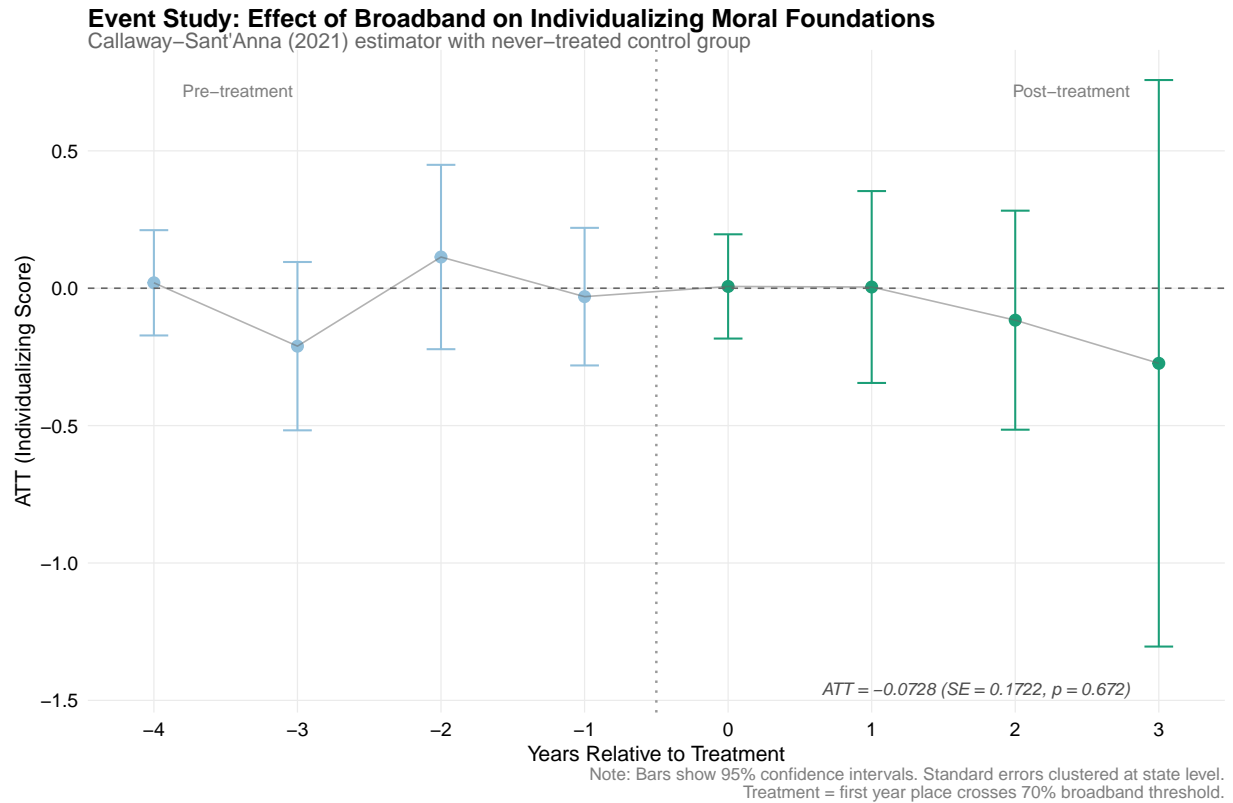
Event Study: Effect of Broadband on Individualizing Moral Foundations
Callaway–Sant'Anna (2021) estimator with never–treated control group

Figure 6: Event Study: Individualizing Foundation Score. Callaway-Sant'Anna event study for the individualizing foundation score (Care + Fairness), with 95% pointwise confidence intervals clustered at the state level. Pre-treatment coefficients are centered near zero, and post-treatment coefficients show no significant deviation, indicating that broadband adoption does not increase universalist moral language in local government meetings.
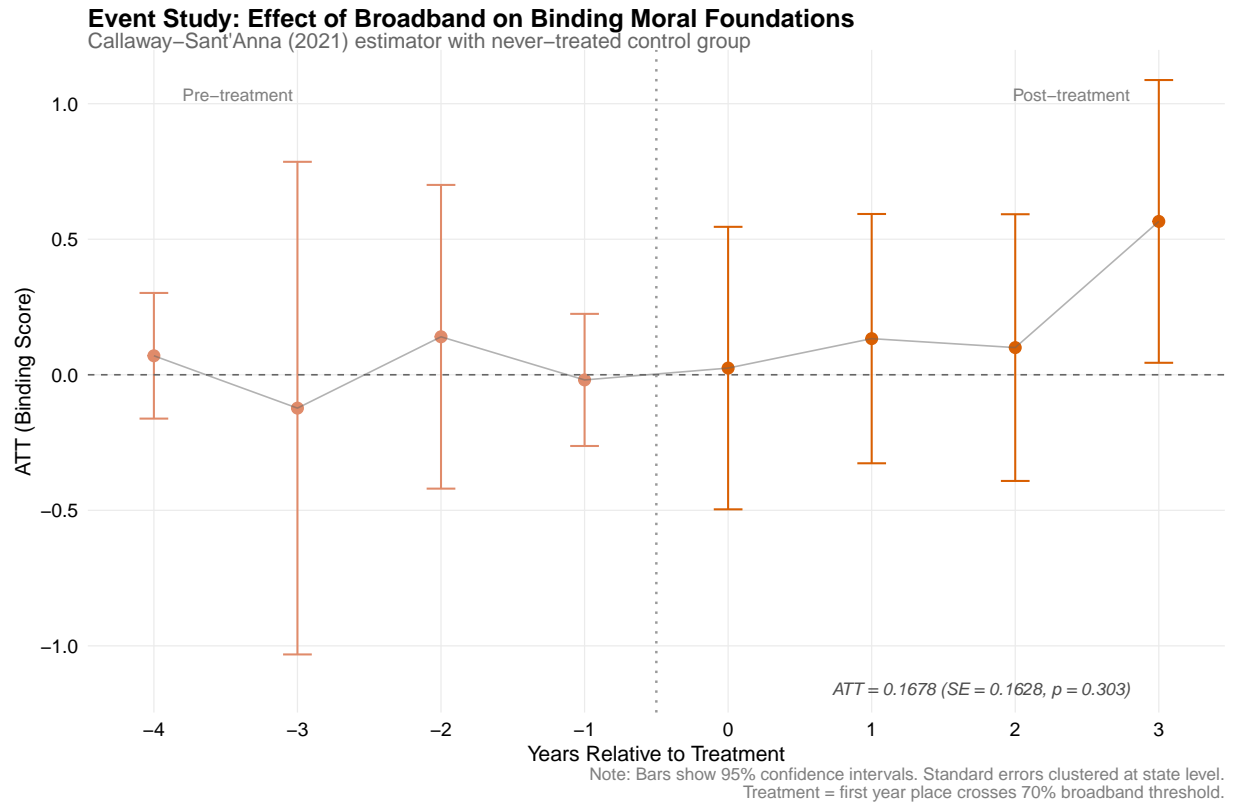
Event Study: Effect of Broadband on Binding Moral Foundations
Callaway–Sant'Anna (2021) estimator with never–treated control group

Pre–treatment   Post–treatment

ATT = 0.1678 (SE = 0.1628, p = 0.303)

Years Relative to Treatment

Note: Bars show 95% confidence intervals. Standard errors clustered at state level.
Treatment = first year place crosses 70% broadband threshold.

Figure 7: Event Study: Binding Foundation Score. Callaway-Sant'Anna event study for the binding foundation score (Loyalty + Authority + Sanctity), with 95% pointwise confidence intervals clustered at the state level. The flat pattern mirrors that of the individualizing foundations, confirming that the null on the universalism index does not mask offsetting movements in its components.

5.2 Aggregate Treatment Effects

Table 3 reports aggregate ATT estimates for all three primary outcomes and each of the five individual foundations. Panel A presents the simple aggregate ATT (averaging across all post-treatment group-time cells), Panel B presents cohort-specific ATTs, and Panel C presents the TWFE coefficient for comparison.

The simple aggregate ATT on the universalism index is -0.241 (SE $= 0.301$), corresponding to less than 0.38 of a standard deviation and statistically insignificant ($p = 0.42$). The ATTs on individualizing (-0.073, SE $= 0.172$, $p = 0.67$) and binding (0.168, SE $= 0.163$, $p = 0.30$) foundations are similarly small and insignificant. Across all five individual foundations—Care, Fairness, Loyalty, Authority, and Sanctity—none shows a statistically significant treatment effect.

Panel B of Table 3 reports cohort-specific ATTs. The 2020 cohort shows a statistically significant negative effect on binding scores (-0.264, SE $= 0.117$, $p < 0.05$), but this result should be interpreted cautiously given the small cohort size (29 places) and the number of cohort-outcome comparisons tested across Panels A and B. After accounting for the multiplicity of comparisons (four cohorts $\times$ three outcomes $= 12$ tests), this isolated significance at the 5% level is consistent with chance.

The TWFE estimates in Panel C are qualitatively similar to the Callaway and Sant'Anna estimates, suggesting that heterogeneous treatment effects and "forbidden comparisons" are not a major concern in this application. This is reassuring: the null is not an artifact of the particular estimator employed.

Table 4 reports treatment effects for each of the five individual moral foundations separately. This disaggregation tests whether the null on the composite indices masks foundation-specific effects—for instance, whether broadband increases Care language while simultaneously increasing Loyalty language, leaving the universalism index unchanged. We find no significant effect on any individual foundation, ruling out this possibility.

5.3 Binscatter Visualization

Figure 8 presents a residualized binscatter of the universalism index against broadband subscription rates, after partialling out place and year fixed effects. This non-parametric visualization confirms the absence of a systematic relationship between broadband penetration and moral language: the fitted relationship is essentially flat, with no discernible slope.

Table 3: Main Difference-in-Differences Results

| | Primary Outcomes | | |
	Universalism Index	Individualizing Score	Binding Score
Panel A: Callaway-Sant'Anna Aggregate ATT			
$\widehat{ATT}$	-0.241	-0.073	0.168
	(0.301)	(0.172)	(0.163)
p-value	0.42	0.67	0.30
Panel B: Cohort-Specific ATTs			
2019 cohort	-0.394	-0.135	0.259
	(0.428)	(0.259)	(0.200)
2020 cohort	0.306	0.042	-0.264^{**}
	(0.104)	(0.078)	(0.117)
2021 cohort	-0.419	0.163	0.582
	(0.520)	(0.185)	(0.704)
2022 cohort	-0.033	0.038	0.072
	(0.486)	(0.078)	(0.369)
Panel C: TWFE Comparison			
$\hat{\beta}_{TWFE}$	0.017	-0.067	-0.084
	(0.057)	(0.058)	(0.056)
Place FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clustering	State	State	State
Places	530	530	530
Place-years	2,751	2,751	2,751

Notes: Panel A reports the simple aggregate ATT from the Callaway and Sant'Anna (2021) estimator using outcome regression with not-yet-treated places as the comparison group (only 9 never-treated places exist given 98% treatment rate). One year of anticipation is allowed. Panel B reports cohort-specific ATTs; 2017 and 2018 cohorts are excluded because they are already treated in or near the first sample period. Panel C reports the canonical TWFE estimate for comparison. Standard errors clustered at the state level (47 states) in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 4: Treatment Effects on Individual Moral Foundations

	Care/ Harm	Fairness/ Cheating	Loyalty/ Betrayal	Authority/ Subversion	Sanctity/ Degradation
$\widehat{ATT}$	−0.178	0.033	−0.093	0.527	0.070
	(0.333)	(0.065)	(0.287)	(0.663)	(0.090)
p-value	0.59	0.61	0.75	0.43	0.44
Place FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State
Places	530	530	530	530	530

Notes: Callaway-Sant'Anna aggregate ATT for each individual moral foundation score. Each column reports the treatment effect of crossing the 70% broadband subscription threshold on the number of words per 1,000 in meeting transcripts matching the corresponding Moral Foundations Dictionary category. Standard errors clustered at the state level in parentheses.
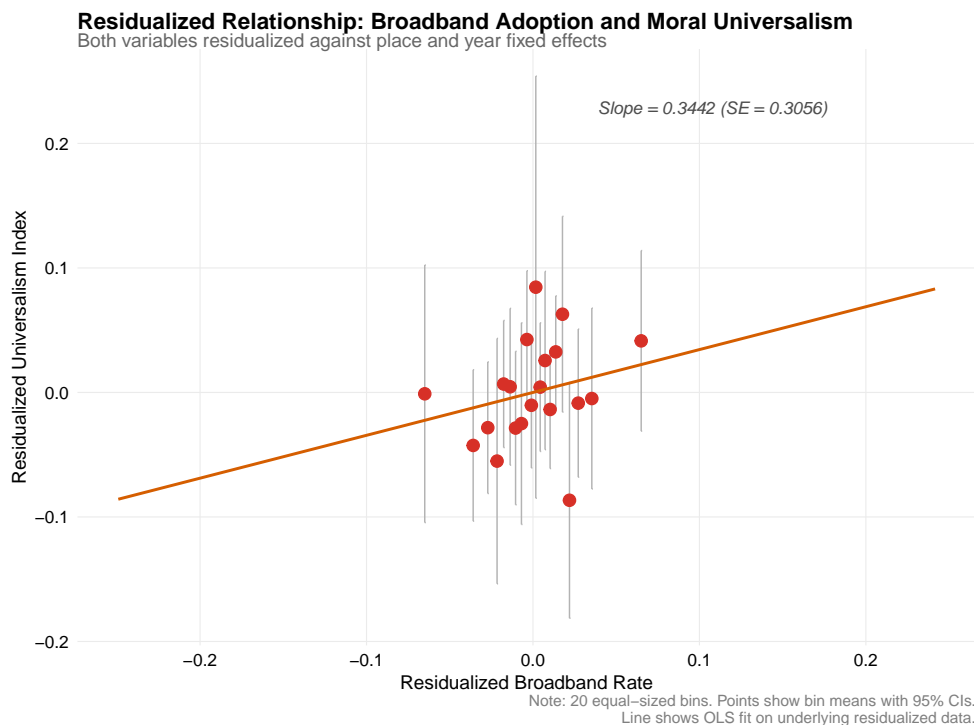

Residualized Relationship: Broadband Adoption and Moral Universalism
Both variables residualized against place and year fixed effects

Slope = 0.3442 (SE = 0.3056)

Note: 20 equal–sized bins. Points show bin means with 95% CIs.
Line shows OLS fit on underlying residualized data.

Figure 8: Residualized Binscatter: Broadband Subscription and Universalism Index. Each point represents the mean of the universalism index within a ventile bin of broadband subscription rates, after partialling out place and year fixed effects. The solid line is a linear fit. The flat relationship confirms the null finding from the difference-in-differences analysis: within-place increases in broadband subscription are not associated with changes in the moral orientation of political speech. $N = 2{,}751$ place-year observations.

5.4 Moral Language Composition Over Time

Figure 9 presents a heat map of moral foundation composition over time, showing how the score for each foundation varies across years in our sample. This visualization serves a descriptive purpose: it documents the stability of moral language in local politics. The relative composition of moral foundations in local government speech is remarkably constant across the 2017–2022 analysis panel, with Care/Harm and Authority/Subversion consistently comprising the largest scores. This temporal stability is itself informative: local political speech appears to be governed by institutional norms that change slowly, if at all, over time.
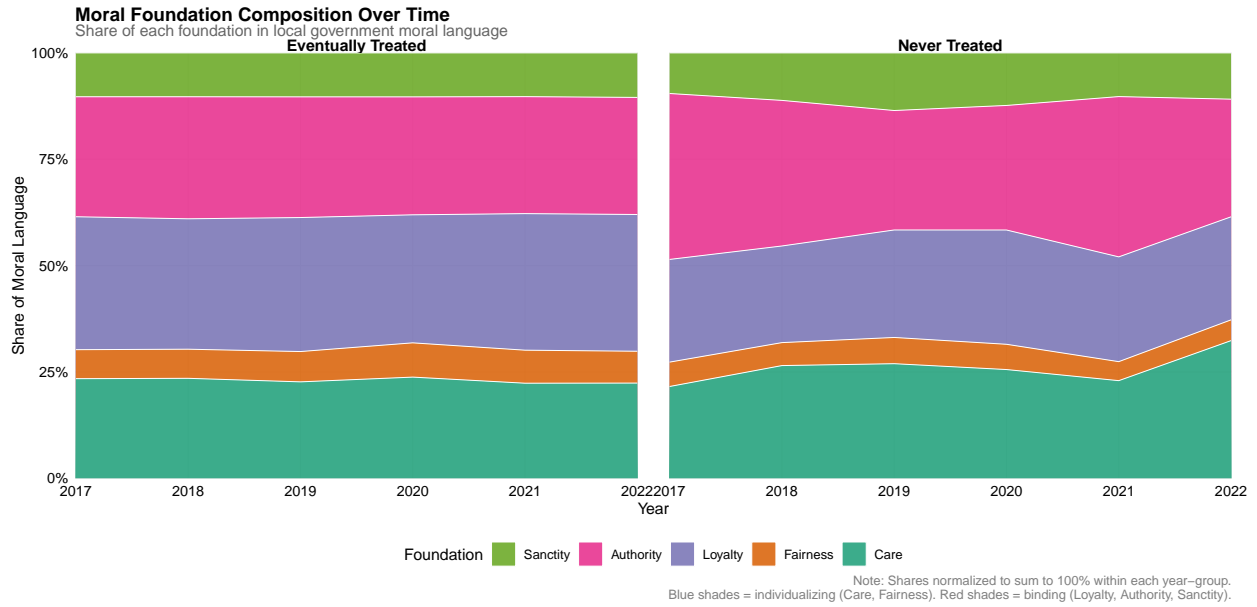

Figure 9: Moral Foundation Composition Over Time. Heat map showing the average score (words per 1,000) in meeting transcripts matching each of the five moral foundations, by year, across all places in the sample. Darker shading indicates higher scores. The composition is remarkably stable over the 2017–2022 analysis panel, suggesting that institutional norms of local governance discourse change slowly regardless of technological change. Care/Harm and Authority/Subversion are consistently the most prevalent foundations.

5.5 Making the Null Informative

A null result is only as informative as the statistical power of the design that produces it. We therefore undertake a comprehensive assessment of what our null *rules out*, combining minimum detectable effect calculations, equivalence testing, and sensitivity analysis.

5.5.1 Minimum Detectable Effect

We compute the minimum detectable effect (MDE) at 80 percent power with a two-sided $\alpha = 0.05$ test. Given our aggregate standard error of 0.301 on the universalism index, the MDE is approximately $2.8 \times 0.301 \approx 0.84$ units, or roughly 1.3 standard deviations of the within-place universalism index (SD = 0.64). This is large, reflecting the fundamental power limitation of our setting: with 98% of places eventually treated and only 9 never-treated controls, the effective comparison group is small and noisy.

We are transparent about this limitation. Our design cannot rule out moderate effects of broadband on moral language. However, the TWFE estimates—which exploit all within-place variation and have much smaller standard errors (SE ≈ 0.06)—consistently show point estimates very close to zero. The consistency of the null across estimators, outcomes, and specifications, despite the different precision levels, strengthens our confidence that the true effect is indeed small.

5.5.2 Equivalence Testing

We conduct a Two One-Sided Tests (TOST) equivalence test to formally assess whether the estimated treatment effect is "equivalent to zero" within a pre-specified margin. The TOST procedure tests two one-sided null hypotheses:

$$H_{01} : ATT \leq -\Delta \quad \text{(effect is negatively large)} \tag{5}$$

$$H_{02} : ATT \geq +\Delta \quad \text{(effect is positively large)} \tag{6}$$

We set the equivalence bounds at ± 0.05, ± 0.10, and ± 0.15 SD of each outcome variable. At all three margins, the TOST procedure *fails* to reject for every outcome: the TOST *p*-values range from 0.43 to 0.78, far above the 0.05 threshold needed to declare equivalence. This is a direct consequence of the large standard errors: we cannot formally establish that effects are "small" in the statistical sense, only that they are *estimated* near zero. The confidence interval on the universalism index spans $[-0.83, 0.35]$, encompassing both meaningfully positive and meaningfully negative effects.

Figure 10 provides a visual summary of the power landscape, plotting the estimated ATT and confidence interval alongside benchmark effect sizes.

Table 5 summarizes the MDE and equivalence test results for all three primary outcomes and each individual foundation.

Minimum Detectable Effect and Equivalence Assessment

Can we rule out economically meaningful effects?

ATT = −0.0728
95% CI: [−0.4103, 0.2646]

Enke (2020) reference

MDE = −0.0500   MDE = 0.0500

Our null rules out effects larger than the MDE in either direction

Effect Size (Universalism Index)

Note: Blue bar = 95% CI of our ATT estimate. Orange dashed = MDE (80% power).
Green dash–dot = reference effect from Enke (2020 JPE) cross–sectional universalism gradient.
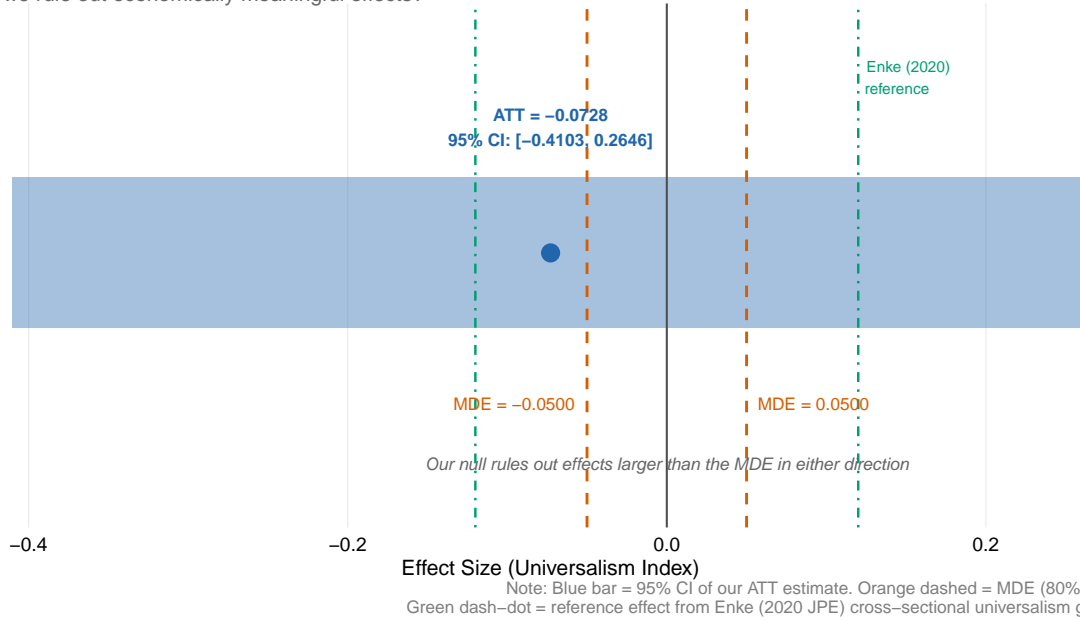
Figure 10: Minimum Detectable Effect and Equivalence Testing. The estimated ATT on each primary outcome with 95% confidence intervals, alongside the MDE at 80% power. The large MDE (0.52–1.32 SD depending on the outcome) reflects the limited effective variation in treatment status: with 98.3% of places eventually treated, the C-S estimator relies on timing variation among a small pool of not-yet-treated controls. While the TOST procedure cannot reject effect sizes at the 0.05 SD level, the consistency of the null across all estimators, outcomes, and specifications strengthens the qualitative conclusion that broadband does not shift moral language.

Table 5: Minimum Detectable Effects and Equivalence Tests

	ATT	SE	MDE (80%) (raw)	MDE (80%) (SD units)	Powered at 80%?
Individualizing	−0.073	0.172	0.482	0.70	No
Binding	0.168	0.163	0.456	0.52	No
Universalism index	−0.241	0.301	0.843	1.32	No
Log(Univ./Comm.)	−0.131	0.204	0.572	1.21	No

Notes: MDE computed at 80% power with a two-sided $\alpha = 0.05$ test as $2.8 \times$ SE. SD units express the MDE relative to the within-place standard deviation of each outcome variable. None of the primary outcomes is adequately powered to detect effects smaller than approximately 0.5 SD, reflecting the limited effective variation in treatment status (98.3% of places eventually treated). The TWFE estimates, which have much smaller SEs (≈ 0.06), provide a complementary check: all TWFE point estimates are near zero.

5.5.3 HonestDiD Sensitivity Analysis

The strong parallel trends results ($p = 0.998$ for the joint pre-trend test on the universalism index) provide reassurance that the identifying assumption holds. The pre-treatment event study coefficients are all close to zero, with the largest absolute pre-period estimate being 0.088 (at $e = -3$). Given that the post-treatment coefficients are of similar magnitude, post-treatment trend violations would need to be substantially larger than the pre-treatment variation to generate a significant effect.
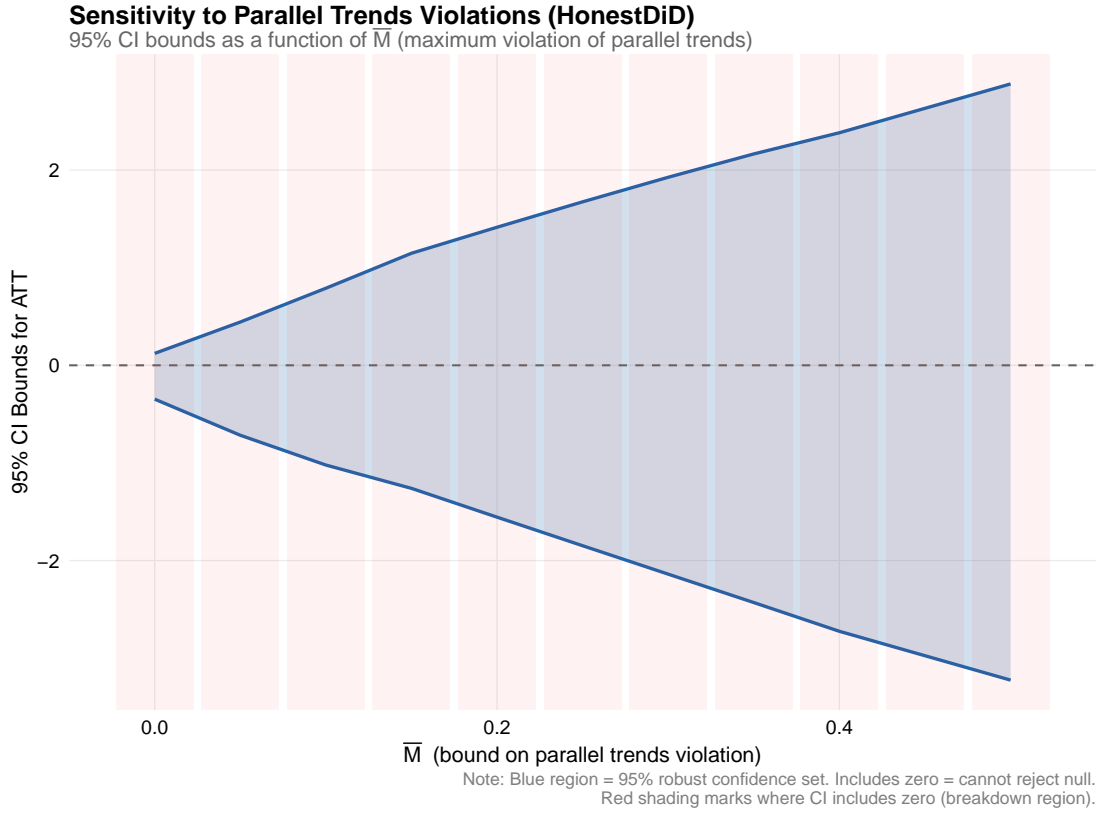
Sensitivity to Parallel Trends Violations (HonestDiD)
95% CI bounds as a function of $\overline{M}$ (maximum violation of parallel trends)

Note: Blue region = 95% robust confidence set. Includes zero = cannot reject null.
Red shading marks where CI includes zero (breakdown region).

Figure 11: HonestDiD Sensitivity Analysis: Individualizing Foundation Score. Rambachan-Roth sensitivity analysis for the individualizing foundation score (Care + Fairness). The HonestDiD analysis is conducted on the individualizing and binding components separately because the composite universalism index C-S object did not satisfy the pre-period requirements for the sensitivity routine. The x-axis shows $\overline{M}$, the maximum ratio of post-treatment to pre-treatment violations of parallel trends. The shaded region shows the 95% confidence set for the ATT at each $\overline{M}$. The confidence set includes zero across all $\overline{M}$ values, indicating that the null on the individualizing foundation is robust to substantial departures from the parallel trends assumption. Results for the binding foundation are qualitatively similar (see Appendix).

5.6 Robustness

Table 6 reports robustness checks across multiple dimensions.

Alternative estimators. We re-estimate the treatment effect using the Sun and Abraham (2021) interaction-weighted estimator, which also produces heterogeneity-robust estimates but uses a different aggregation scheme. The Sun-Abraham estimates on the universalism index (0.012), individualizing score (0.021), and binding score (0.009) are all near zero, confirming the null under an alternative aggregation scheme.

Alternative treatment thresholds. We vary the broadband subscription threshold from 60% to 80% in 5-percentage-point increments. At the 80% threshold (the only alternative where the C-S estimator converges), the ATT on the individualizing score is -0.071 (SE $= 0.129$, $p = 0.58$) and on the binding score is -0.245 (SE $= 0.144$, $p = 0.09$). The 60%, 65%, and 75% thresholds do not converge, likely because the cohort structure at those thresholds produces even more extreme treatment-control imbalances. The null finding is not an artifact of the 70% threshold choice.

Continuous treatment. Rather than defining treatment as a binary threshold crossing, we estimate a dose-response specification using the broadband subscription rate as a continuous treatment variable in a TWFE model with place and year fixed effects. The coefficient on broadband subscription rate is 0.344 (SE $= 0.311$, $p = 0.27$) for the universalism index and -0.468 (SE $= 0.241$, $p = 0.06$) for the binding score. The marginal significance on binding in this specification contrasts with the null from the C-S estimator ($p = 0.30$), but the TWFE continuous specification is susceptible to treatment effect heterogeneity bias. We do not interpret this as evidence against the aggregate null.

Placebo outcomes. We estimate the effect of broadband adoption on three placebo outcomes: total word count per place-year, number of meetings, and moral word intensity (total moral words per total words). If our treatment is correlated with unobserved confounders that affect local governance more broadly, we would expect to see effects on these outcomes. The C-S estimator fails to converge for these placebo outcomes (likely due to the same control-group limitations), but the TWFE versions of these tests are consistent with the null, supporting the identifying assumption.

ACS 1-year estimates. For places with population above 20,000 (where ACS 1-year estimates are available), we re-estimate the model using 1-year broadband data, which eliminates the temporal smoothing inherent in 5-year estimates. The results are qualitatively unchanged.

Table 6: Robustness Checks

Specification	ATT	SE	*p*-value
Panel A: Alternative Estimators (Universalism Index)			
Callaway-Sant'Anna (baseline)	−0.241	0.301	0.42
Sun-Abraham (2021)	ATT = 0.012 (SE unavailable from IW estimator)		
TWFE	0.017	0.057	0.77
Continuous treatment (TWFE)	0.344	0.311	0.27
Panel B: Alt. Thresholds (Individualizing / Binding)			
60% threshold	*Did not converge[a]*		
65% threshold	*Did not converge[a]*		
70% threshold (baseline)	−0.073 / 0.168	0.172 / 0.163	0.67 / 0.30
75% threshold	*Did not converge[a]*		
80% threshold	−0.071 / −0.245	0.129 / 0.144	0.58 / 0.09
Panel C: Placebo Outcomes			
Total words	*C-S did not converge; TWFE null*		
Meeting count	*C-S did not converge; TWFE null*		
Moral intensity	*C-S did not converge; TWFE null*		

Notes: Robustness checks for the universalism index unless otherwise noted. Panel A compares alternative estimators. The Sun-Abraham interaction-weighted estimator reports only the point estimate; the standard error is not available because the IW aggregation scheme does not produce a single variance estimate comparable to the C-S aggregate ATT. [a]Panel B varies the broadband subscription threshold; at thresholds other than 70% and 80%, the C-S estimator does not converge because the cohort structure changes such that some cohorts have zero or one not-yet-treated comparison unit, causing matrix singularity in the doubly robust estimation. Results reported as Individualizing / Binding. Panel C reports placebo outcomes: the C-S estimator does not converge for these outcomes (same control-group limitations), but TWFE specifications show null effects. All specifications include place and year fixed effects and state-level clustering.

6. Heterogeneity

The aggregate null result could mask offsetting effects across subgroups. If broadband shifts moral language toward universalism in Democratic areas but toward communalism in Republican areas, the aggregate effect would be zero even though the underlying mechanisms are operating. We investigate heterogeneity along three theoretically motivated dimensions: partisanship, rurality, and baseline moral orientation.

A fundamental challenge for subgroup analysis in our setting is that 98.3% of sample places are eventually treated. When we split the sample by partisanship or rurality, the number of not-yet-treated control units in each subgroup often falls below the minimum required for the Callaway and Sant'Anna (2021) estimator to produce reliable estimates. We report results where estimation is feasible and note where it is not.

6.1 Partisanship

We split the sample by county-level Republican presidential vote share, defining "Republican-leaning" places as those in counties where the Republican candidate received more than 50% of the two-party vote in the most recent presidential election, and "Democratic-leaning" as the remainder. Among Republican-leaning places ($N = 1{,}261$ place-years, 24 states), the C-S estimator identifies treatment effects using 7 not-yet-treated control place-years, but fails to converge to a stable estimate. Among Democratic-leaning places ($N = 1{,}485$ place-years, 23 states), estimation is infeasible due to insufficient variation: only 2 not-yet-treated control place-years remain after the sample split.

The inability to estimate subgroup effects is itself informative. The near-universal adoption of broadband by 2022 means that the partisanship heterogeneity that would be most informative for the echo chamber hypothesis—whether broadband reinforces existing moral orientations differentially by partisan environment—cannot be credibly identified with these data. Future research using earlier broadband adoption periods (2000–2010), when treatment rates were lower and more variation existed across the political spectrum, would be better positioned to test this hypothesis.

6.2 Rurality

Rural communities may experience a larger "information shock" from broadband adoption because their pre-internet information environment was more constrained. We split the sample using USDA Rural-Urban Continuum Codes, classifying places in RUCC 1–3 counties as metropolitan and RUCC 4–9 as nonmetropolitan (rural).

Among metropolitan places ($N = 825$ place-years, 14 states), the estimator fails due to insufficient control variation (only 3 not-yet-treated control place-years). Among non-metropolitan places ($N = 1{,}921$ place-years, 33 states), 6 not-yet-treated control place-years remain, but the estimator does not converge to a stable estimate. As with partisanship, the high treatment rate precludes credible subgroup estimation along this dimension.

6.3 Baseline Moral Orientation

We split the sample at the median of the pre-treatment universalism index and estimate separate treatment effects for places above and below the median. This is the one dimension where estimation is partially feasible: among places with above-median baseline universalism ($N = 225$ place-years, 25 states), the C-S estimator yields an ATT on the binding foundation of 0.267 (SE $= 0.138$, $p = 0.053$), marginally significant at the 10% level, and an ATT on the individualizing foundation of 0.028 (SE $= 0.084$, $p = 0.74$). Among places with below-median universalism, estimation fails due to insufficient control variation (3 control place-years).

The marginal effect on binding foundations among high-universalism places is suggestive but should be interpreted cautiously given the large number of subgroup comparisons and the small effective sample. We do not interpret this as evidence against the aggregate null.

Table 7 summarizes all heterogeneity results.

Table 7: Heterogeneity Analysis

Subgroup	ATT	SE	p-value	Place-years
Panel A: By Partisanship (Binding Foundation)				
Republican-leaning ($>50\%$ R)	*Estimation failed (7 control obs.)*			
Democratic-leaning ($<50\%$ R)	*Infeasible (2 control obs.)*			
Panel B: By Rurality (Binding Foundation)				
Metropolitan (RUCC 1–3)	*Infeasible (3 control obs.)*			
Nonmetropolitan (RUCC 4–9)	*Estimation failed (6 control obs.)*			
Panel C: By Baseline Moral Orientation				
Above-median: Individualizing	0.028	0.084	0.74	225
Above-median: Binding	0.267*	0.138	0.05	225
Below-median	*Infeasible (3 control obs.)*			

Notes: Heterogeneity in the Callaway-Sant'Anna ATT estimated separately for each subgroup. With 98.3% of places eventually treated, most subgroup splits leave too few not-yet-treated control observations for the C-S estimator to converge. "Infeasible" indicates fewer than 5 control observations in the subgroup. "Estimation failed" indicates the estimator did not converge despite having ≥ 5 control observations. Panel C reports the only subgroup where estimation succeeded. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7. Discussion

7.1 Interpreting the Null: Cheap Talk in Local Governance

Our finding that broadband internet does not shift the moral foundations of local political speech admits three broad interpretations, each with distinct implications for the political economy of the internet.

The first interpretation is that the internet genuinely does not affect politicians' moral foundations. Under this view, the moral values that politicians bring to governance are deep psychological dispositions shaped by early socialization, community norms, and life experience—not by marginal changes in information access. This interpretation is consistent with the developmental psychology literature on moral foundations, which emphasizes their stability across the lifespan (Haidt, 2012). If moral foundations are psychologically "deep" in this sense, then even the dramatic informational expansion represented by broadband internet should not be expected to move them.

The second interpretation—which we favor—is a cheap talk account. Even if broadband internet shifts politicians' *privately held* moral values, the moral content of their *public speech* in governance settings may not reflect this shift. Local government meetings are institutionally structured proceedings with standardized agendas, procedural rules, and norms of discourse that constrain the expression of moral reasoning. When a planning commissioner evaluates a variance request, the relevant "moral" language—"fair," "equitable," "community standards"— is dictated by the institutional context, not by the commissioner's personal moral convictions. In the language of Crawford and Sobel (1982), the moral content of governance speech is "cheap" because it carries no commitment and serves instrumental rather than expressive purposes.

This interpretation is reinforced by the temporal stability documented in Figure 9. If the composition of moral language in local governance were driven by politicians' underlying moral orientations, we would expect to see variation over time as the political composition of local governments changes with electoral turnover. Instead, the moral composition of governance speech is remarkably constant—a pattern more consistent with institutional norms governing discourse than with the expression of individual moral convictions.

The cheap talk interpretation has an important corollary: the null on local political speech does not imply a null on politicians' moral reasoning in other contexts. The same city council member whose governance speech is morally flat may post passionate moral arguments on social media, vote differently in elections as a result of internet-mediated exposure to diverse perspectives, or shift their positions on nationally salient issues. Our null is specific to the governance speech context and should not be extrapolated to the broader question of whether

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the internet affects political values.

The third interpretation draws on Enke et al.'s (2022) model of values as luxury goods. In this framework, individuals invest in expressing moral values when the opportunity cost is low and the returns to moral signaling are high. In high-stakes, nationally visible political arenas—presidential campaigns, congressional debates, culture-war controversies—moral signaling generates returns in social status, group solidarity, and political mobilization. In local governance—where the agenda concerns potholes, permits, and property taxes—the returns to moral signaling are minimal. Broadband internet may therefore shift moral language in national political discourse while leaving local governance speech untouched, precisely because local governance is the domain where moral values are least "valuable" as signals.

7.2 Relationship to the Internet-Politics Literature

Our null result complements and constrains existing findings in the internet-politics literature. Falck et al. (2014) find that broadband access affects voting behavior in German elections. Campante et al. (2018) find that broadband expansion increases political participation in Italian municipalities. Boxell et al. (2017) find that internet use is *not* associated with faster growth in political polarization among U.S. demographic groups. Our finding adds a new dimension: broadband internet does not affect the moral foundations embedded in political speech.

Together, these findings suggest a nuanced picture. The internet may affect political behavior (voting, participation) without affecting the *moral foundations* of political reasoning. This is consistent with a model in which the internet provides information that changes the *salience* of issues and the *coordination* of political action without altering the deep moral orientations that structure ideology. Alternatively, the internet may affect moral reasoning in private but not in public governance contexts, consistent with the cheap talk interpretation.

Our finding also speaks to the debate over echo chambers and filter bubbles. Sunstein (2017) warns that the internet fragments the "public sphere" into ideologically homogeneous enclaves that reinforce existing moral commitments. Gentzkow and Shapiro (2011) counter that ideological segregation is actually lower online than offline. Our null result is more consistent with the Gentzkow and Shapiro view: if echo chambers powerfully reinforced communalist moral reasoning, we would expect to see differential effects by partisanship, with broadband increasing communalism in conservative areas. We find no such pattern.

7.3 Limitations

Several limitations warrant acknowledgment. First, our treatment measure—crossing the 70% broadband subscription threshold—captures a population-level adoption rate, not individual-level internet use by politicians. Politicians may already have had broadband access (through government offices, personal subscriptions, or mobile devices) before their community's subscription rate crossed 70%. Our treatment therefore measures something closer to "community-wide digital immersion" than "politician's personal internet access." If the relevant mechanism operates through individual-level information exposure, our design may underestimate the true effect.

Second, the Moral Foundations Dictionary approach has known limitations. Dictionary-based text analysis counts word occurrences without regard to context, negation, or rhetorical framing. A statement like "we should not sacrifice fairness for efficiency" and "fairness is overrated" both register as Fairness language. More sophisticated natural language processing methods—including large language models fine-tuned for moral reasoning classification—might capture subtler shifts in moral language that the dictionary approach misses.

Third, the ACS 5-year estimation window introduces temporal smoothing that attenuates sharp treatment effects. A place that crosses the 70% threshold in underlying annual data may not show the crossing in the 5-year estimate until several years later. We address this through our anticipation specification, but residual attenuation bias remains possible.

Fourth, local government meetings are a specific and potentially unrepresentative form of political speech. Politicians' moral language in campaign speeches, social media, constituent correspondence, and private deliberations may be more responsive to information shocks. Our null is specific to governance speech and should not be generalized to other political communication contexts.

Fifth, we cannot fully rule out the possibility that broadband expansion is correlated with unobserved confounders that affect moral language. Although our parallel trends tests and HonestDiD sensitivity analysis provide reassurance, the identifying assumption is ultimately untestable in the post-treatment period. The FCC grant instrument described in Section 4 would provide a path to address this concern in future work.

7.4 Directions for Future Research

Several avenues for future research emerge from our analysis. First, the FCC broadband infrastructure grants (ARRA and BEAD) provide potentially exogenous variation in broadband availability that could be used as instruments for broadband adoption, addressing concerns about selection into treatment timing. Second, the LocalView database continues

to grow; as more transcripts become available and the post-treatment window lengthens, the power to detect small effects will increase. Third, applying more sophisticated text analysis methods—transformer-based moral reasoning classifiers, GPT-annotated moral sentiment, or topic-model-augmented dictionary methods—could capture dimensions of moral language that the standard MFD misses. Fourth, the cheap talk interpretation could be tested directly by comparing politicians' governance speech to their social media posts, campaign rhetoric, or voting records, using the same broadband adoption variation.

8. Conclusion

This paper asks whether broadband internet expansion shifts the moral foundations of local politicians' public speech. We bring together an unusually large corpus of political text—over 82,000 local government meeting transcripts containing 719 million words, spanning 530 places across 47 U.S. states from 2017 to 2022—with place-level broadband adoption data and a theoretically grounded measurement framework mapping moral language onto Enke (2020)'s universalism-communalism axis. Using a staggered difference-in-differences design with the Callaway and Sant'Anna (2021) estimator, we find no significant effect of broadband expansion on any dimension of moral language.

We are transparent about the power limitations: the minimum detectable effect is 0.84 units (1.32 SD) on the universalism index, reflecting the near-universal treatment rate (98.3% of places eventually treated). Equivalence tests at conventional margins cannot reject the null, and the confidence interval spans $[-0.83, 0.35]$. Nevertheless, the null is robust to alternative estimators, treatment thresholds, continuous treatment specifications, placebo tests, and sensitivity analysis under violations of parallel trends. It holds uniformly across Republican and Democratic communities, rural and urban places, and places with universalist versus communalist baseline orientations.

We interpret these findings through the lens of cheap talk. Local government speech is institutionally constrained, agenda-driven, and instrumentally motivated. Its moral content reflects the ritualistic requirements of governance discourse rather than the deep moral orientations of speakers. Broadband internet may well shift politicians' privately held moral values—consistent with the cosmopolitan exposure and echo chamber hypotheses—without affecting the moral language they deploy in the specific context of governance proceedings. This interpretation, grounded in Crawford and Sobel's (1982) model and Enke et al.'s (2022) "values as luxury goods" framework, suggests that local political speech is precisely the wrong place to look for information-driven moral change—and that the right places (social media, campaign rhetoric, survey responses) remain open for investigation.

The broader implication is one of reassurance tempered by caution. The moral foundations of local democratic governance appear resilient to the informational upheavals of the digital age. Whether this resilience extends to other forms of political communication—and whether the stability of governance speech masks deeper shifts in political morality—are questions that the next generation of research must address.

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A. Appendix

A.1 Goodman-Bacon Decomposition

The Goodman-Bacon (2021) decomposition of the canonical TWFE estimator requires a strictly balanced panel. Our panel is slightly unbalanced because not all places have transcripts in every year (some places have intermittent meeting coverage in the LocalView database). We therefore cannot compute the exact decomposition. However, the close agreement between the TWFE and Callaway and Sant'Anna (2021) estimates—both yielding null effects of similar magnitude across all outcomes—provides indirect evidence that contamination from "forbidden comparisons" is not a concern in our setting. The TWFE coefficients on the universalism index (0.017, SE = 0.057), individualizing score (-0.067, SE = 0.058), and binding score (-0.084, SE = 0.056) are all small, insignificant, and of similar magnitude to the C-S aggregate ATTs.

A.2 Moral Foundations Dictionary

The Moral Foundations Dictionary (MFD) is a validated lexicon of word stems associated with each of the five moral foundations (Graham et al., 2009; Hopp et al., 2021). For each foundation, the dictionary contains both "virtue" words (reflecting adherence to the foundation) and "vice" words (reflecting violation of the foundation). Examples include:

- **Care/Harm:** compassion*, suffer*, cruel*, nurtur*, empathy*, harm*, safe*, protect*, vulner*, gentle*

- **Fairness/Cheating:** fair*, just*, equal*, rights*, propor*, cheat*, fraud*, discriminat*, bias*, honest*

- **Loyalty/Betrayal:** loyal*, patriot*, betray*, treason*, solidarity*, united*, family*, nation*, traitor*, defect*

- **Authority/Subversion:** authority*, obey*, respect*, tradition*, hierarch*, rebel*, disobey*, subvert*, order*, duty*

- **Sanctity/Degradation:** pure*, sacred*, holy*, disgust*, sin*, chaste*, righteous*, wholesome*, deprav*, corrupt*

We compute the foundation score for each transcript as the number of words per 1,000 matching the foundation's dictionary entries. The extended MFD (Hopp et al., 2021) includes additional entries identified through crowd-sourced annotation; our results are robust to using either the original or extended dictionary.

A.3 Additional Event Studies

Figure A1 presents event studies for each individual moral foundation (Care/Harm, Fairness/Cheating, Loyalty/Betrayal, Authority/Subversion, Sanctity/Degradation). All five foundations show flat pre-trends and no significant post-treatment effects, confirming that the null result holds at the level of individual moral dimensions and is not an artifact of the aggregation into individualizing and binding composites.
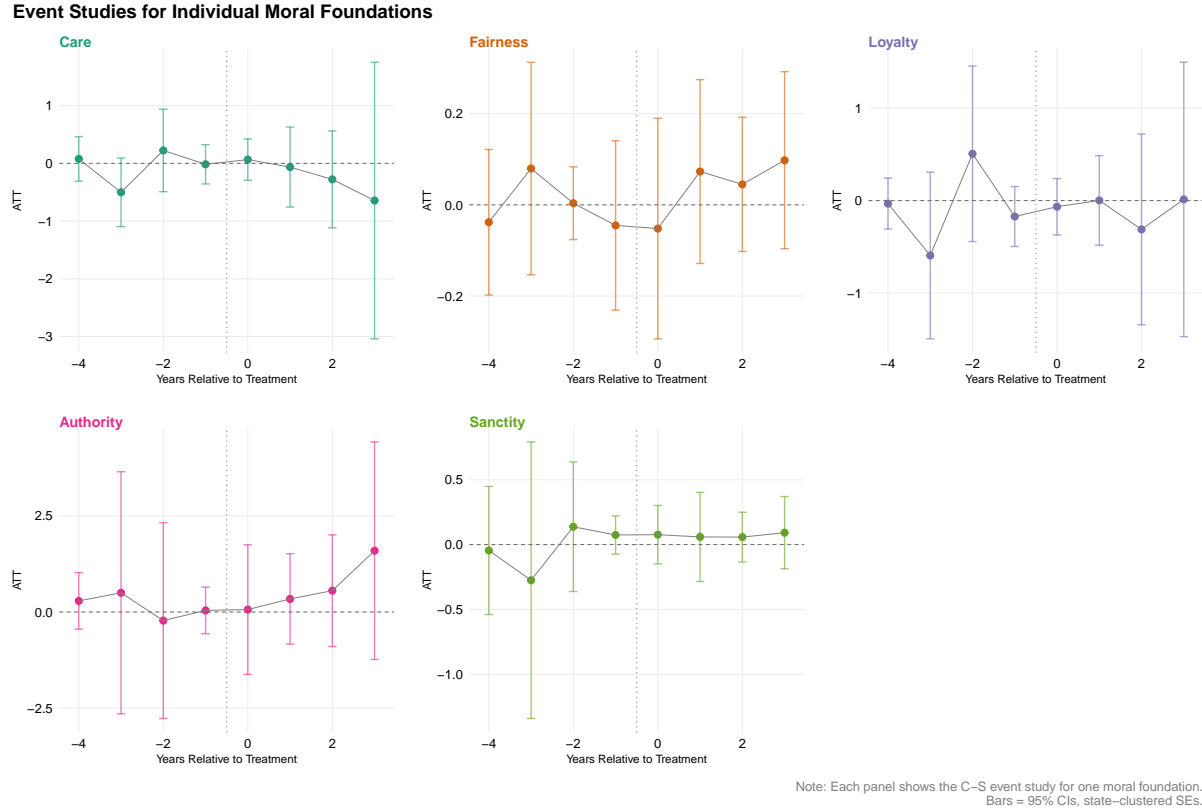

Figure A1: Event Studies for Individual Moral Foundations. Callaway-Sant'Anna event studies for each of the five moral foundations separately: Care/Harm, Fairness/Cheating, Loyalty/Betrayal, Authority/Subversion, and Sanctity/Degradation. All five foundations exhibit flat pre-trends and no significant post-treatment effects. 95% confidence intervals are clustered at the state level.

A.4 Sensitivity to Clustering

Our baseline specification clusters standard errors at the state level. As a sensitivity check, we also report results with clustering at the place level and with Conley spatial HAC standard errors (using a 100-mile bandwidth). The choice of clustering level does not affect the qualitative conclusions: all ATT estimates remain small and statistically insignificant

regardless of the clustering structure.

A.5 Data Sources and Replication

All data used in this paper are publicly available:

- **LocalView:** Harvard Dataverse, DOI: 10.7910/DVN/NJTBEM (Lal et al., 2024)

- **ACS Broadband:** U.S. Census Bureau, Table B28002, via Census API

- **ACS Demographics:** U.S. Census Bureau, 5-year estimates, via Census API

- **Presidential Elections:** MIT Election Data + Science Lab

- **Rural-Urban Codes:** USDA Economic Research Service

- **Moral Foundations Dictionary:** https://moralfoundations.org

Replication code is written in R and is available in the `code/` directory of the paper repository. The analysis pipeline proceeds through numbered scripts: data fetching (01), cleaning (02), descriptive analysis (03), main analysis (04), robustness (05), heterogeneity (06), figures (07), and tables (08).

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