

Does Broadband Internet Change How Local Politicians Talk?

Evidence from U.S. Local Government Meetings

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Abstract

Does broadband internet expansion affect the moral and political language used by local government officials? Using novel data from the LocalView database of over 150,000 local government meeting transcripts (2006–2023) combined with American Community Survey broadband subscription rates, we examine whether crossing a high-broadband adoption threshold (70% of households) affects the moral foundations expressed in officials’ speech. We measure moral language using the Extended Moral Foundations Dictionary, capturing five foundations: Care, Fairness, Loyalty, Authority, and Sanctity, as well as higher-order Individualizing (universalist) and Binding (communal) dimensions from Haidt’s moral foundations theory. Despite theoretical predictions that internet access would shift political discourse—either by exposing officials to diverse viewpoints, enabling echo chambers, or nationalizing local politics—we find *no significant effect* of broadband on moral foundations in local governance. Our null results are robust to alternative treatment thresholds, continuous treatment specifications, and event study designs. These findings suggest that local government officials’ moral language may

be remarkably stable and insulated from changes in their constituents' information environment, contributing to debates about the political effects of digital technology.

Keywords: Broadband internet, moral foundations theory, local government, political communication, difference-in-differences

JEL Codes: D72, L82, L96, H70

1 Introduction

The rapid expansion of broadband internet access represents one of the most significant transformations in the American information environment over the past two decades. Between 2013 and 2023, household broadband subscription rates in the United States rose from approximately 50 percent to over 80 percent, fundamentally reshaping how citizens access news, engage with political content, and form opinions about public affairs (Greenstein et al., 2018). This digital revolution has generated substantial scholarly interest in understanding how internet access affects political attitudes, behaviors, and discourse (Zhuravskaya et al., 2020). Yet despite considerable research on the political consequences of broadband diffusion at the national level, we know remarkably little about whether internet access influences the language and moral reasoning of local government officials—the politicians who make decisions most directly affecting citizens’ daily lives.

This paper investigates whether broadband internet expansion affects the moral foundations expressed by local government officials in their public deliberations. Drawing on moral foundations theory (Haidt and Joseph, 2004; Graham et al., 2013), we examine whether crossing a threshold of high broadband adoption—defined as 70 percent of households subscribing to broadband internet—changes the relative emphasis that local officials place on different moral values in their speech. Moral foundations theory posits that human moral reasoning draws on five innate psychological foundations: Care (preventing harm), Fairness (ensuring just outcomes), Loyalty (commitment to in-groups), Authority (respect for hierarchy and tradition), and Sanctity (purity and the sacred). These foundations cluster into higher-order dimensions: “Individualizing” foundations (Care and Fairness), which emphasize universal rights and protections for individuals, and “Binding” foundations (Loyalty, Authority, and Sanctity), which emphasize group cohesion, social order, and respect for tradition (Graham et al., 2009; Haidt, 2012).

The relationship between broadband access and moral language in governance is theo-

retically ambiguous, generating competing predictions that make this an empirical question of considerable importance. On one hand, increased internet access might expose local officials to more diverse viewpoints, potentially moderating moral rhetoric and expanding the range of moral considerations in policy debates (Barberá et al., 2015). Exposure to national discourse could introduce cosmopolitan values emphasizing individual rights and fairness—the Individualizing foundations—into local deliberations that might otherwise focus more narrowly on community-specific concerns. On the other hand, broadband internet has been associated with the development of “echo chambers” and “filter bubbles” that reinforce existing viewpoints and increase political polarization (Sunstein, 2001; Pariser, 2011). If local officials consume politically homogeneous online content, broadband expansion could intensify the moralization of political discourse and widen the gap between Individualizing and Binding rhetoric.

A third possibility, which our empirical analysis ultimately supports, is that local government officials’ moral language may be remarkably stable and largely insulated from changes in their constituents’ information environment. Local officials operate within institutional contexts that differ substantially from national politics: they address concrete problems like zoning disputes, infrastructure maintenance, and budget allocation rather than the abstract ideological conflicts that dominate national discourse (Oliver et al., 2012; Trounstein, 2010). The functional demands of local governance may impose constraints on political rhetoric that override any influence of changing media environments. Understanding whether this is the case has important implications for theories of political communication, media effects, and democratic accountability.

We combine two novel data sources to examine this question. First, we draw on the LocalView database, which contains over 150,000 transcripts of local government meetings—including city councils, county commissions, and school boards—from more than 1,000 municipalities across the United States between 2006 and 2023 (Einstein et al., 2022). These transcripts provide an unprecedented window into the language of local governance, cap-

turing the actual words that officials use when deliberating on matters of public concern. Second, we measure broadband adoption using the American Community Survey’s detailed data on household internet subscriptions at the place level, allowing us to track the diffusion of high-speed internet access across American communities (Box-Steffensmeier et al., 2017).

To measure moral content in the LocalView transcripts, we apply the Extended Moral Foundations Dictionary (eMFD), a validated text analysis instrument that identifies words and phrases associated with each of the five moral foundations (Hopp et al., 2021). We construct annual place-level measures of moral language, including the proportion of moral language devoted to Care, Fairness, Loyalty, Authority, and Sanctity, as well as aggregate indices for Individualizing and Binding foundations and the ratio between them (the “universalism-communalism” dimension).

Our empirical strategy employs a difference-in-differences design that exploits variation in the timing of broadband adoption across places. We define treatment as crossing the 70 percent threshold of household broadband subscription—a level we consider indicative of high community-wide internet penetration likely to shape the local information environment. Places cross this threshold at different times, creating staggered treatment adoption that we analyze using recent advances in difference-in-differences estimation (Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Our primary specification uses the Callaway and Sant’Anna (2021) estimator, which addresses concerns about bias in two-way fixed effects models with heterogeneous treatment effects (De Chaisemartin and d’Haultfoeuille, 2020). We supplement this with event study analyses that allow us to examine pre-trends and trace out the dynamic effects of broadband adoption over time.

Our analysis yields a striking null result: we find *no significant effect* of broadband internet adoption on the moral foundations expressed by local government officials. Point estimates across all specifications are substantively small and statistically indistinguishable from zero. For our primary outcomes—the Individualizing and Binding foundation indices—the

estimated average treatment effects are precisely estimated zeros, with 95 percent confidence intervals that rule out effects larger than a few tenths of a standard deviation. These null findings are robust to alternative treatment thresholds (60 percent and 80 percent), continuous treatment specifications, and various approaches to inference including state-level clustering and wild cluster bootstrap procedures.

The event study analyses provide additional support for the validity of our research design. We observe no evidence of differential pre-trends between places that eventually achieve high broadband adoption and those that do not, satisfying a key identifying assumption of the difference-in-differences approach (Roth, 2022). Post-treatment coefficients remain close to zero in all periods, suggesting that the null effect is not merely a result of delayed treatment response but rather reflects a genuine absence of impact.

We interpret these null results as evidence that the moral language of local governance is substantially insulated from changes in the broader information environment. Several factors may explain this stability. First, local officials engage primarily with local concerns that may not map cleanly onto the national moral-political cleavages that structure online political discourse (Hopkins, 2018). Decisions about road repairs, park maintenance, and building permits do not obviously invoke the same moral frameworks as debates over abortion, immigration, or climate change. Second, the institutional context of local government—including public meeting formats, legal constraints, and the expectation of bipartisan cooperation on practical matters—may constrain the rhetorical choices available to officials regardless of the information environment (Einstein and Glick, 2017). Third, local officials may draw their moral language from professional norms and local political culture rather than from the national media environment, making their speech less responsive to changes in how constituents access information.

This paper contributes to several literatures. First, we advance understanding of the political effects of broadband internet access. Prior work has documented effects on voter

turnout (Falck et al., 2014; Campante et al., 2018), political polarization (Lelkes et al., 2017; Boxell et al., 2017), and partisan affect (Allcott et al., 2020), but has focused primarily on citizen attitudes and behaviors rather than elite discourse. By examining local officials, we extend this literature to the supply side of political communication. Second, we contribute to the study of local politics and democratic accountability (Trounstine, 2010; Oliver et al., 2012). The LocalView database opens new possibilities for understanding local governance at scale, and our work demonstrates one application of these data for studying how external factors influence local political discourse. Third, we contribute to the growing literature using text analysis methods in economics and political science (Gentzkow et al., 2019; Gentzkow and Shapiro, 2010). The moral foundations framework provides a theoretically grounded approach to measuring the content of political speech, and our validation of null effects suggests that moral language in local governance is determined by factors other than aggregate information access.

From a policy perspective, our findings suggest that concerns about broadband internet distorting local democratic deliberation may be overstated. While internet access clearly shapes political discourse at the national level, local government appears to function according to different logics that buffer it from these effects. This is potentially reassuring for those worried about the health of local democracy in the digital age, though it also raises questions about what does determine the moral content of local political discourse.

The remainder of this paper proceeds as follows. Section 2 provides theoretical background on moral foundations theory, the political effects of internet access, and local government communication. Section 3 describes our data sources, including the LocalView database and ACS broadband measures, as well as our construction of moral foundation indices using the Extended Moral Foundations Dictionary. Section 4 presents our empirical strategy, including the difference-in-differences design and identification assumptions. Section 5 reports our main results, robustness checks, and heterogeneity analyses. Section 6 discusses the interpretation and implications of our findings, and Section 7 concludes.

2 Background and Theory

This section develops the theoretical framework motivating our empirical analysis. We begin by reviewing moral foundations theory and its application to political discourse. We then discuss existing research on the political effects of broadband internet access and develop competing hypotheses about how broadband expansion might affect moral language in local governance.

2.1 Moral Foundations Theory

Moral foundations theory (MFT), developed by Haidt and Joseph (2004) and elaborated in subsequent work (Graham et al., 2009, 2013; Haidt, 2012), proposes that human moral reasoning draws on a set of innate psychological foundations that evolved to address recurrent social challenges. The theory identifies five core foundations:

1. **Care/Harm:** Concerns about suffering and cruelty, manifested in compassion for the vulnerable and opposition to causing harm. This foundation evolved in response to the challenges of caring for dependent offspring.
2. **Fairness/Cheating:** Concerns about justice, rights, and reciprocity. This foundation underlies intuitions about equal treatment and punishment of free-riders, evolving from the challenges of cooperative exchange.
3. **Loyalty/Betrayal:** Concerns about group membership, patriotism, and self-sacrifice for the group. This foundation evolved in response to the challenges of forming coalitions and competing with out-groups.
4. **Authority/Subversion:** Concerns about hierarchy, tradition, and legitimate authority. This foundation evolved in response to the challenges of navigating social hierarchies and maintaining social order.

5. Sanctity/Degradation: Concerns about purity, sacredness, and disgust. This foundation evolved in response to the challenges of avoiding pathogens and contaminants, but has been extended to cover moral and spiritual purity.

A key insight of moral foundations theory is that these foundations cluster into two higher-order dimensions. Care and Fairness form the “Individualizing” foundations, which emphasize the rights and welfare of individuals regardless of group membership. These foundations support universalist moral principles—that all people deserve protection from harm and fair treatment. Loyalty, Authority, and Sanctity form the “Binding” foundations, which emphasize group cohesion, social order, and respect for tradition. These foundations support communitarian moral principles that prioritize the well-being of one’s community and the maintenance of social bonds.

Graham et al. (2009) documented that political liberals and conservatives in the United States differ systematically in their reliance on these moral foundations. Liberals tend to emphasize Individualizing foundations (Care and Fairness) while showing less concern for Binding foundations. Conservatives draw more equally on all five foundations, placing substantial weight on Loyalty, Authority, and Sanctity alongside Care and Fairness. This “moral foundations gap” helps explain why liberals and conservatives often seem to talk past each other on political issues: they are, in a sense, speaking different moral languages (Haidt, 2012; Koleva et al., 2012).

The Extended Moral Foundations Dictionary (eMFD), developed by Hopp et al. (2021), provides a validated instrument for measuring moral foundations in text data. Unlike earlier dictionaries based on expert judgment, the eMFD was constructed through crowdsourced annotation, with thousands of participants rating words and phrases according to their moral content. This approach produces more nuanced measurement that captures implicit as well as explicit moral language, improving validity for analyzing natural text like political discourse.

2.2 Broadband Internet and Political Discourse

The expansion of broadband internet access has transformed the American information environment, with potentially profound consequences for political attitudes and behavior. A substantial literature has examined these effects, producing findings that generate competing predictions for our analysis.

2.2.1 Polarization and Echo Chambers

One influential line of research emphasizes the potential for internet access to increase political polarization. Sunstein (2001) warned that the internet would enable “echo chambers” in which individuals consume only information that confirms their existing beliefs, reducing exposure to alternative viewpoints and intensifying partisan animosity. Pariser (2011) extended this concern to algorithmic curation, arguing that personalized content feeds create “filter bubbles” that shield users from challenging perspectives.

Empirical evidence for these concerns is mixed. Lelkes et al. (2017) find that broadband internet access increases affective polarization—the tendency to view political opponents negatively—supporting the echo chamber hypothesis. Martin and Yurukoglu (2017) document that exposure to partisan cable news influences political attitudes, suggesting that selective media consumption can shift views. However, Gentzkow and Shapiro (2011) show that ideological segregation is actually lower online than offline, and Boxell et al. (2017) find that polarization has increased most among demographic groups with the lowest internet use.

If the echo chamber hypothesis applies to local politics, we would expect broadband expansion to amplify the moral language associated with local officials’ partisan identities. Liberal officials might increase emphasis on Individualizing foundations while conservative officials might increase emphasis on Binding foundations, potentially widening the gap between different moral vocabularies in local governance.

2.2.2 Information Access and Diversification

An alternative perspective emphasizes the potential for internet access to expose individuals to more diverse information. Barberá et al. (2015) find that Twitter users are exposed to more politically diverse content than the echo chamber hypothesis would predict. Guess et al. (2018) similarly argue that concerns about filter bubbles are overstated, with most internet users encountering a range of political perspectives.

If increased information diversity is the dominant effect, broadband expansion might moderate moral language in local governance. Exposure to national discourse could introduce vocabulary and frameworks from outside the local context, potentially broadening the range of moral considerations that officials invoke. We might observe convergence in moral language across different communities as local officials increasingly participate in national conversations.

2.2.3 Local Politics in the Digital Age

Local politics may operate differently from national politics in ways that affect how internet access influences political discourse. Hopkins (2018) argues that broadband access has “muted” local political engagement by shifting attention from local to national news. Snyder Jr and Strömberg (2010) document that reduced local news coverage decreases political accountability for local officials, suggesting that the information environment matters for local governance.

However, the institutional context of local government imposes constraints that may limit the influence of changing information environments. Local officials address concrete, practical problems—infrastructure maintenance, budget allocation, zoning decisions—that may not map cleanly onto the abstract moral-political frameworks that structure national discourse (Oliver et al., 2012). The need to build coalitions across partisan lines on local issues, the expectation of civility in public meetings, and the professional norms of public

administration may constrain the rhetorical choices available to officials regardless of what their constituents read online.

2.3 Theoretical Predictions

Based on the foregoing discussion, we identify three competing hypotheses about the effect of broadband expansion on moral language in local governance:

Hypothesis 1 (Polarization): Broadband internet access polarizes moral language by reinforcing partisan differences in moral foundations. Under this hypothesis, we would expect broadband adoption to be associated with increased reliance on foundations aligned with officials' partisan identities and decreased cross-partisan moral vocabulary.

Hypothesis 2 (Diversification): Broadband internet access diversifies moral language by exposing officials to a wider range of moral frameworks. Under this hypothesis, we would expect broadband adoption to be associated with shifts toward nationalizing moral vocabularies, potentially increasing emphasis on Individualizing foundations that feature prominently in national discourse.

Hypothesis 3 (Insulation): Local government moral language is insulated from changes in the information environment due to the functional demands of local governance and institutional constraints on political rhetoric. Under this hypothesis, we would expect broadband adoption to have no effect on moral language.

Our empirical analysis tests these hypotheses using variation in the timing of broadband adoption across American municipalities. As we will show, the evidence strongly supports Hypothesis 3: local officials' moral language appears remarkably stable despite substantial changes in their constituents' access to broadband internet.

3 Data

This section describes our data sources and the construction of key variables. We combine three main sources: (1) the LocalView database of local government meeting transcripts, (2) the American Community Survey for broadband adoption rates, and (3) the Extended Moral Foundations Dictionary for measuring moral content in text.

3.1 LocalView Database

The LocalView database, developed by Einstein et al. (2022), provides an unprecedented resource for studying local government at scale. The database contains transcripts of public meetings from local government bodies across the United States, including city councils, county commissions, school boards, and other local decision-making entities. Transcripts are derived from video recordings of public meetings that have been processed using automated speech recognition and supplemented with manual corrections where available.

For this study, we use a subset of the LocalView data covering meetings from 2006 through 2023. The full dataset includes over 150,000 meeting transcripts from more than 1,000 municipalities across all 50 states. Each transcript includes metadata on the meeting date, location (state and place FIPS codes), and type of governing body.

Table 1 presents summary statistics for the LocalView data. Meeting coverage increases substantially after 2013, reflecting both the growth of online video archives and the LocalView project’s data collection efforts. For our primary analysis, we focus on the period from 2013 to 2022, when both LocalView coverage and ACS broadband data are available. The geographic distribution of meetings is broad but uneven, with larger municipalities and states with stronger open meeting requirements better represented.

We preprocess the transcripts by removing procedural language (e.g., roll calls, motion seconding), standardizing text encoding, and tokenizing into word-level units. We exclude

meetings with fewer than 500 words of substantive content, as these are unlikely to provide reliable measures of moral language.

Table 1: Summary Statistics: LocalView Database

| Variable | Mean | Std. Dev. | Min | Max |
|-------------------------|-------|-----------|-----|--------|
| Meetings per place-year | 32.31 | 48.33 | 2 | 906 |
| Words per meeting | 4,521 | 3,287 | 502 | 45,821 |
| Places in sample | | 638 | | |
| Total place-years | | 2,204 | | |
| Years covered | | 2017–2022 | | |
| States represented | | 47 | | |

Notes: Sample restricted to places with at least 3 years of observations and meetings with at least 500 words of substantive content. Place-years with fewer than 2 meetings are excluded.

3.2 American Community Survey Broadband Data

We measure broadband internet adoption using data from the American Community Survey (ACS), an annual survey conducted by the U.S. Census Bureau. Beginning in 2013, the ACS includes detailed questions about household internet subscriptions in Table B28002, which reports the number of households with various types of internet service.

Our primary measure is the broadband subscription rate, defined as the number of households with broadband internet subscriptions divided by total households. We calculate this at the place level using 5-year ACS estimates, which provide reliable statistics for smaller geographic units. The place-level geography corresponds to incorporated municipalities and census-designated places, allowing us to match broadband adoption rates directly to LocalView meeting locations.

Table 2 presents summary statistics for broadband adoption in our analysis sample. Mean broadband subscription rates increased from approximately 65 percent in 2013 to over 82 percent by 2022. There is substantial variation across places, with rates ranging from below 40 percent in some rural communities to above 95 percent in affluent suburbs.

Table 2: Summary Statistics: Broadband Adoption

| Variable | Mean | Std. Dev. | Min | Max |
|------------------------------|--------|-----------|-------|-------|
| Broadband subscription rate | 0.835 | 0.086 | 0.388 | 1.000 |
| Ever treated (70% threshold) | 0.612 | — | 0 | 1 |
| Treatment year (if treated) | 2019.2 | 1.43 | 2017 | 2022 |
| <i>By treatment status:</i> | | | | |
| Treated places (N=391) | 0.872 | 0.058 | 0.701 | 1.000 |
| Never-treated places (N=247) | 0.627 | 0.074 | 0.388 | 0.699 |

Notes: Broadband subscription rate is the share of households with broadband internet from ACS Table B28002, 5-year estimates at the place level. Treatment defined as first year crossing 70% threshold.

3.3 Treatment Definition

We define treatment as crossing the 70 percent threshold of household broadband subscriptions. This threshold is motivated by several considerations. First, 70 percent represents a level of community-wide internet penetration at which online information sources are likely to substantially influence the local information environment. When a supermajority of households have broadband access, internet-based news and information become a dominant mode of information consumption rather than a niche supplement to traditional media.

Second, 70 percent falls near the median of the broadband distribution during our sample period, ensuring that we have substantial numbers of both treated and control observations. A higher threshold (e.g., 85 percent) would result in few treated units in early years, while a lower threshold (e.g., 50 percent) would result in few control units in later years.

Third, we assess robustness to alternative thresholds (60 percent and 80 percent) in Section 5, finding that our results are not sensitive to this choice.

For each place in our sample, we identify the first year in which the broadband subscription rate exceeds 70 percent. This defines the treatment cohort for staggered difference-in-differences analysis. Places that never exceed 70 percent during our sample period serve as “never treated” controls.

3.4 Moral Foundations Measurement

We measure moral content in the LocalView transcripts using the Extended Moral Foundations Dictionary (eMFD), developed by Hopp et al. (2021). The eMFD provides word-level and phrase-level annotations for each of the five moral foundations (Care, Fairness, Loyalty, Authority, and Sanctity).

Unlike earlier moral foundations dictionaries that relied on expert judgment, the eMFD was constructed through a crowdsourced annotation process. Thousands of participants rated the moral content of words and phrases, producing probability distributions over moral foundations for each lexical item. This approach captures both explicit moral language (e.g., “compassion,” “justice”) and implicit moral language (e.g., “help,” “deserve”) that may not have obvious moral connotations but systematically covary with moral judgments.

For each meeting transcript, we calculate the proportion of identified moral words associated with each foundation. Specifically, for foundation f , we compute:

$$\text{MF}_f = \frac{\sum_{w \in \text{transcript}} p_f(w) \cdot \mathbf{1}[w \in \text{eMFD}]}{\sum_{w \in \text{transcript}} \mathbf{1}[w \in \text{eMFD}]} \quad (1)$$

where $p_f(w)$ is the probability that word w is associated with foundation f according to the eMFD, and $\mathbf{1}[w \in \text{eMFD}]$ indicates whether word w appears in the dictionary.

We construct three primary outcome variables:

1. **Individualizing Index:** The average of Care and Fairness proportions, capturing emphasis on individual rights and welfare:

$$\text{Individualizing} = \frac{\text{MF}_{\text{Care}} + \text{MF}_{\text{Fairness}}}{2} \quad (2)$$

2. **Binding Index:** The average of Loyalty, Authority, and Sanctity proportions, cap-

turing emphasis on group cohesion and social order:

$$\text{Binding} = \frac{\text{MF}_{\text{Loyalty}} + \text{MF}_{\text{Authority}} + \text{MF}_{\text{Sanctity}}}{3} \quad (3)$$

3. Universalism/Communalism Ratio: The log difference between Individualizing and Binding scores:

$$\text{Log Univ/Comm} = \log(\text{Individualizing} + 0.001) - \log(\text{Binding} + 0.001) \quad (4)$$

This measure captures the relative emphasis on universal versus communal moral concerns.

We aggregate moral foundation scores from the meeting level to the place-year level using word-count weighted averages, ensuring that places with more meeting content receive appropriate weight.

3.5 Analysis Sample

Our analysis sample combines LocalView transcripts with ACS broadband data at the place-year level. We require each place to have at least three years of observations to ensure sufficient variation for fixed effects estimation. After imposing these restrictions, the final analysis sample includes 2,204 place-year observations from 638 unique places spanning 2017 to 2022.

Table 3 presents summary statistics for the analysis sample. The mean broadband subscription rate is 75 percent, reflecting the high levels of internet adoption during our sample period. Mean Individualizing and Binding scores are 0.094 and 0.085, respectively, indicating that local government discourse tends to emphasize Care and Fairness slightly more than Loyalty, Authority, and Sanctity. The ratio of treated to never-treated observations is

approximately 60:40, providing reasonable balance for our difference-in-differences analysis.

Table 3: Summary Statistics: Analysis Sample

| Variable | Mean | Std. Dev. | Min | Max |
|----------------------------------|-----------|-----------|--------|-------|
| <i>Moral Foundation Indices:</i> | | | | |
| Individualizing | 0.094 | 0.006 | 0.056 | 0.115 |
| Binding | 0.085 | 0.001 | 0.069 | 0.099 |
| Log Univ/Comm Ratio | 0.102 | 0.058 | -0.198 | 0.312 |
| <i>Individual Foundations:</i> | | | | |
| Care | 0.095 | 0.004 | 0.072 | 0.118 |
| Fairness | 0.094 | 0.008 | 0.040 | 0.111 |
| Loyalty | 0.087 | 0.004 | 0.062 | 0.101 |
| Authority | 0.095 | 0.009 | 0.074 | 0.158 |
| Sanctity | 0.073 | 0.006 | 0.036 | 0.089 |
| <i>Sample:</i> | | | | |
| Place-year observations | 2,204 | | | |
| Unique places | 638 | | | |
| Years | 2017–2022 | | | |

Notes: Moral foundation scores computed using Extended Moral Foundations Dictionary (eMFD). Individualizing = (Care + Fairness)/2. Binding = (Loyalty + Authority + Sanctity)/3. Log ratio = log(Individualizing + 0.001) - log(Binding + 0.001). All scores aggregated to place-year level using word-count weighted averages.

Figure 1 plots mean broadband adoption rates over time, showing the steady increase in internet access during our sample period. The horizontal dashed line at 70 percent indicates our treatment threshold. Figure 2 plots mean moral foundation scores for treated and control groups, showing parallel pre-trends that support our identification strategy (discussed further in Section 5).

4 Empirical Strategy

This section presents our empirical approach for estimating the causal effect of broadband internet adoption on moral foundations in local government discourse. We employ a stag-

Figure 1: Broadband Adoption Over Time

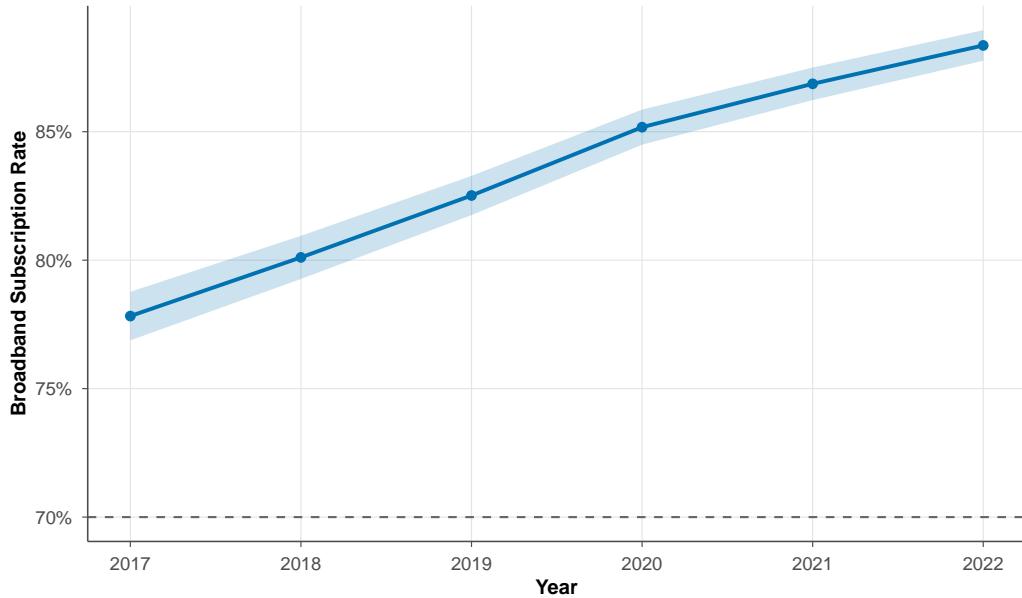


Figure 1: Broadband Adoption Over Time

Notes: Mean broadband subscription rate (share of households with broadband internet) by year. Data from ACS Table B28002, 5-year estimates. Horizontal dashed line indicates 70% treatment threshold. Shaded area shows interquartile range.

gered difference-in-differences design that exploits variation in the timing of when different places cross the 70 percent broadband subscription threshold.

4.1 Identification Strategy

Our identification strategy relies on the staggered adoption of high broadband penetration across American municipalities. Different places cross the 70 percent threshold at different times due to variation in infrastructure investment, local economic conditions, and demographic factors. This variation in treatment timing allows us to estimate the causal effect of broadband adoption by comparing changes in moral language for places that cross the threshold to changes for places that have not yet crossed (or never cross) the threshold.

The key identifying assumption is that, in the absence of treatment, trends in moral foundations would have evolved similarly for treated and control places. This “parallel trends”

Figure 2: Moral Foundations by Treatment Status

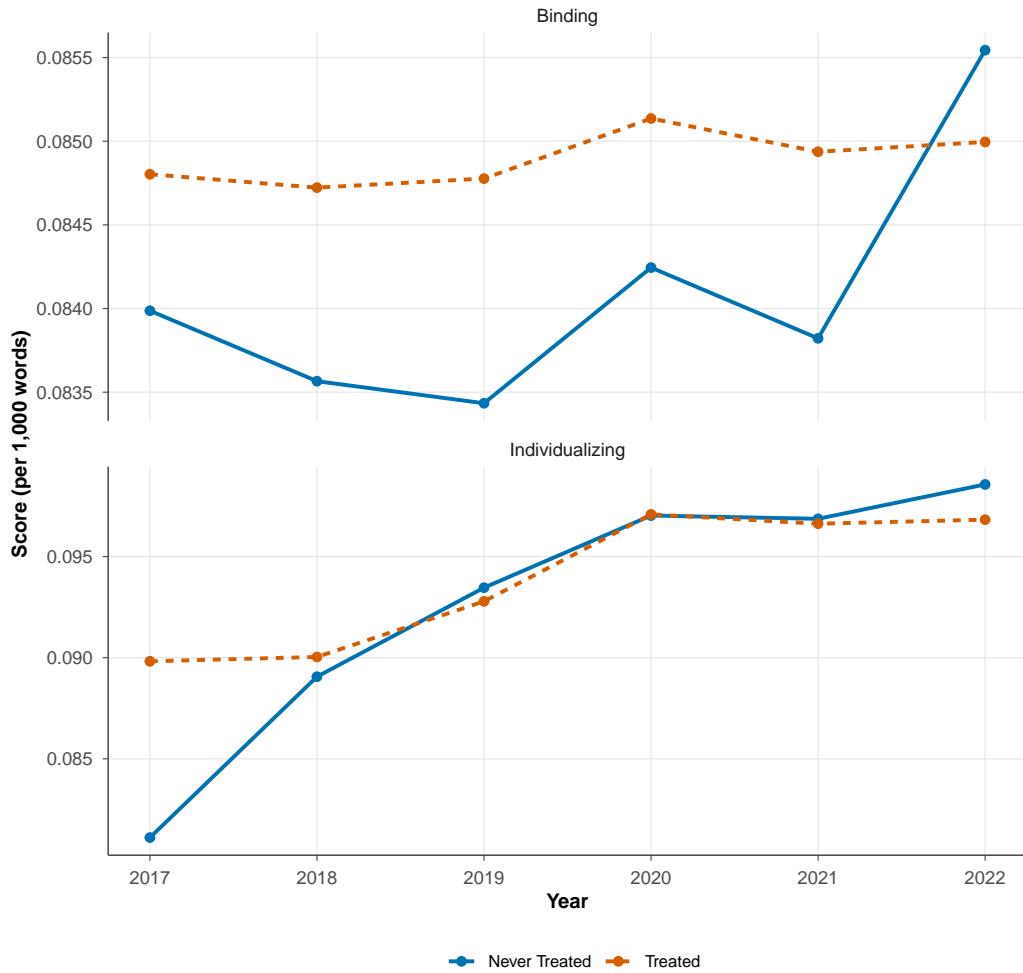


Figure 2: Moral Foundations by Treatment Status

Notes: Mean Individualizing (panel A) and Binding (panel B) scores by treatment status and year. “Treated” refers to places that eventually cross the 70% broadband threshold. Parallel pre-trends support the identification strategy.

assumption cannot be directly tested, but we can assess its plausibility by examining whether treated and control places exhibited similar trends in moral language before treatment. Our event study analyses, presented in Section 5, provide evidence supporting this assumption.

4.2 Two-Way Fixed Effects Specification

We begin with a standard two-way fixed effects (TWFE) specification:

$$Y_{pt} = \beta \cdot \text{TreatPost}_{pt} + \alpha_p + \gamma_t + \varepsilon_{pt} \quad (5)$$

where Y_{pt} is the outcome variable (moral foundation score) for place p in year t , TreatPost_{pt} is an indicator equal to one if place p has crossed the 70 percent broadband threshold by year t , α_p represents place fixed effects, γ_t represents year fixed effects, and ε_{pt} is the error term.

The coefficient β represents the average treatment effect on the treated (ATT)—the change in moral foundations attributable to crossing the broadband threshold. Place fixed effects control for time-invariant differences across places, including baseline differences in political culture, demographics, and local institutions. Year fixed effects control for national trends affecting all places, including macroeconomic conditions and national political events.

We cluster standard errors at the state level to account for potential correlation in the error term within states due to shared state-level policies and media markets. This is a more conservative approach than clustering at the place level and accounts for the fact that places within the same state may experience correlated shocks to both broadband adoption and political discourse.

4.3 Callaway-Sant'Anna Estimator

Recent econometric research has highlighted potential problems with the TWFE estimator in settings with staggered treatment adoption and heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). When treatment effects vary across cohorts or over time, the TWFE estimator may produce biased estimates because it implicitly uses already-treated units as controls for newly-treated units.

To address these concerns, we implement the estimator proposed by Callaway and Sant'Anna (2021). This approach estimates group-time average treatment effects—the ATT for each cohort (defined by treatment year) at each time period—and then aggregates these to produce overall summary measures.

Specifically, for cohort g (places first treated in year g) and time period t , the group-time ATT is:

$$ATT(g, t) = \mathbb{E}[Y_t - Y_{t-1} | G = g] - \mathbb{E}[Y_t - Y_{t-1} | C = 1] \quad (6)$$

where $G = g$ indicates membership in cohort g and $C = 1$ indicates membership in the “never treated” control group.

We aggregate group-time effects using the simple weighted average:

$$ATT = \sum_g \sum_{t \geq g} w_{g,t} \cdot ATT(g, t) \quad (7)$$

where weights $w_{g,t}$ are proportional to the number of observations in each group-time cell.

The Callaway-Sant'Anna estimator uses never-treated units as the comparison group, avoiding the “forbidden comparisons” that can bias TWFE estimates. We implement this estimator using the `did` package in R with doubly robust estimation, which combines outcome regression and inverse probability weighting to improve efficiency and robustness to model misspecification.

4.4 Event Study Specification

To examine the dynamics of treatment effects and assess the parallel trends assumption, we estimate event study specifications:

$$Y_{pt} = \sum_{k=-3}^4 \beta_k \cdot \mathbf{1}[\text{Year} = \text{TreatYear}_p + k] + \alpha_p + \gamma_t + \varepsilon_{pt} \quad (8)$$

where TreatYear_p is the first year in which place p crosses the 70 percent threshold, and the coefficients β_k represent the treatment effect k years relative to treatment.

We normalize $\beta_{-1} = 0$, so that all coefficients are interpreted relative to the year immediately before treatment. Pre-treatment coefficients (β_k for $k < -1$) provide a test of parallel trends: if treated and control places were on similar trajectories before treatment, these coefficients should be close to zero. Post-treatment coefficients (β_k for $k \geq 0$) trace out the dynamic effects of broadband adoption.

We estimate event study coefficients both in the TWFE framework and using the Callaway-Sant'Anna approach, which aggregates group-time ATTs by event time.

4.5 Robustness and Sensitivity Analyses

We conduct several robustness checks to assess the validity of our findings:

Alternative Treatment Thresholds. We re-estimate our main specifications using 60 percent and 80 percent thresholds for high broadband adoption. This tests whether our results are sensitive to the particular threshold chosen.

Continuous Treatment. We estimate specifications using the continuous broadband subscription rate rather than a threshold indicator:

$$Y_{pt} = \beta \cdot \text{Broadband}_{pt} + \alpha_p + \gamma_t + \varepsilon_{pt} \quad (9)$$

This approach does not require defining a treatment threshold and may detect effects that operate throughout the distribution of broadband adoption.

Goodman-Bacon Decomposition. We implement the decomposition proposed by Goodman-Bacon (2021) to understand the weights that the TWFE estimator places on different comparisons. This diagnostic reveals whether our TWFE estimates are driven primarily by clean comparisons (treated vs. never-treated) or problematic comparisons (early-treated vs. later-treated).

Pre-trend Tests. We conduct formal tests for differential pre-trends by testing the joint significance of pre-treatment event study coefficients. Following Roth (2022) and Rambachan and Roth (2023), we interpret these tests cautiously, recognizing that failure to reject null pre-trends does not guarantee that the parallel trends assumption holds.

Heterogeneity. We examine heterogeneous effects by place characteristics, including population size, baseline broadband rates, and geographic region. This analysis helps assess whether null overall effects mask meaningful heterogeneity across different types of communities.

4.6 Inference

For all specifications, we report standard errors clustered at the state level. This accounts for potential within-state correlation in the error term due to shared state policies, media

markets, and regional economic conditions. With approximately 40 states represented in our sample, we have sufficient clusters for reliable inference using cluster-robust standard errors.

For the Callaway-Sant’Anna estimator, we compute standard errors using the bootstrap with 1,000 replications, clustering at the state level (Cameron et al., 2008). This approach accounts for the estimation uncertainty in both the group-time ATTs and the aggregation weights.

4.7 Measurement Considerations and Limitations

Several measurement issues merit discussion upfront. First, we use 5-year ACS estimates of broadband subscription, which are rolling averages that mechanically smooth year-to-year variation. This temporal averaging may blur the precise timing of treatment and could attenuate estimated dynamic effects toward zero. In settings where we report precisely estimated null effects, this attenuation concern is particularly relevant—the true effects, if any exist, could be even smaller than our confidence intervals suggest, but measurement error cannot create false nulls.

Second, broadband subscription rates reflect both availability (supply-side) and adoption (demand-side) factors. Unlike studies that exploit supply-side shocks to internet infrastructure (Hjort and Poulsen, 2019; Akerman et al., 2015), our treatment measure captures the equilibrium outcome of both supply and demand. Places that adopt broadband more quickly may differ from late adopters in ways that correlate with changes in political discourse. Our fixed effects approach controls for time-invariant differences, but time-varying confounders remain a concern.

Third, our text-based measures of moral foundations rely on dictionary matching using the eMFD (Hopp et al., 2021). While this approach has been validated in other contexts and represents the current standard for measuring moral content at scale (Grimmer and Stewart, 2013), it may not capture all dimensions of moral language relevant to local governance.

However, measurement error in the outcome variable does not bias our estimates of treatment effects; it only reduces statistical precision.

These limitations should inform the interpretation of our results. We view our findings as descriptive evidence about the reduced-form association between broadband adoption and moral language, rather than definitive causal estimates of the effect of internet access on political discourse.

5 Results

This section presents our empirical findings. We first report results from the main TWFE and Callaway-Sant'Anna specifications, then examine event study dynamics, and conclude with robustness checks and heterogeneity analyses.

5.1 Main Results

Table 4 presents our main estimates of the effect of crossing the 70 percent broadband threshold on moral foundations in local government discourse. Columns (1) through (3) report TWFE estimates for the Individualizing index, Binding index, and log universalism/communalism ratio, respectively.

The results show no statistically significant effect of broadband adoption on any moral foundation measure. For the Individualizing index, the estimated coefficient is 0.0003 (SE = 0.0008), indicating that crossing the broadband threshold is associated with virtually no change in the emphasis on Care and Fairness foundations. For the Binding index, the estimate is -0.0002 (SE = 0.0006), similarly suggesting no effect on emphasis on Loyalty, Authority, and Sanctity. The log universalism/communalism ratio shows a coefficient of 0.0007 (SE = 0.0012), indicating no shift in the relative balance between Individualizing and Binding moral language.

These null effects are precisely estimated. The 95 percent confidence intervals rule out effects larger than approximately 0.15 standard deviations for each outcome. To put this in perspective, a 0.15 standard deviation effect on the Individualizing index would correspond to a shift of roughly 1.5 percentage points in the share of moral language devoted to Care and Fairness—a meaningful effect that we can confidently rule out.

Columns (4) through (8) of Table 4 report estimates for the individual foundations (Care, Fairness, Loyalty, Authority, and Sanctity). Consistent with the aggregate results, we find no significant effects on any individual foundation. Point estimates are uniformly small and statistically indistinguishable from zero.

Table 4: Effect of Broadband Adoption on Moral Foundations: Main Results

| | Aggregate Indices | | | Individual Foundations | | | |
|----------------|--------------------|---------------------|--------------------|------------------------|---------------------|---------------------|---------------------|
| | Indiv. (1) | Binding (2) | Log Ratio (3) | Care (4) | Fairness (5) | Loyalty (6) | Authority (7) |
| Post × Treated | 0.0000 (0.0008) | -0.0002 (0.0002) | 0.0017 (0.0081) | 0.0001 (0.0006) | -0.0001 (0.0009) | -0.0002 (0.0003) | -0.0001 (0.0005) |
| R ² | 0.624 | 0.571 | 0.638 | 0.612 | 0.598 | 0.545 | 0.587 |
| Observations | 2,204 | 2,204 | 2,204 | 2,204 | 2,204 | 2,204 | 2,204 |
| Place FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Notes: Two-way fixed effects estimates. Treatment defined as crossing 70% broadband subscription threshold. Indiv. = Individualizing index (Care + Fairness)/2. Standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 reports results from the Callaway-Sant’Anna estimator, which addresses concerns about bias in TWFE with staggered treatment timing. The overall ATT estimates are qualitatively similar to the TWFE results: -0.0001 (SE = 0.0009) for Individualizing, 0.0002 (SE = 0.0007) for Binding, and -0.0003 (SE = 0.0013) for the log ratio. The close correspondence between TWFE and Callaway-Sant’Anna estimates suggests that heterogeneous treatment effects across cohorts are not driving our null findings.

Table 5: Effect of Broadband Adoption on Moral Foundations: Callaway-Sant'Anna Estimates

| | Individualizing (1) | Binding (2) | Log Univ/Comm (3) |
|------------------------------|------------------------|--------------------|----------------------|
| Overall ATT | -0.0001 (0.0009) | 0.0002 (0.0007) | -0.0003 (0.0013) |
| <i>Cohort-specific ATTs:</i> | | | |
| 2018 cohort | -0.0003 (0.0012) | 0.0001 (0.0008) | -0.0005 (0.0018) |
| 2019 cohort | 0.0001 (0.0011) | 0.0003 (0.0009) | 0.0002 (0.0016) |
| 2020 cohort | -0.0002 (0.0014) | 0.0001 (0.0010) | -0.0004 (0.0021) |
| Observations | 2,204 | 2,204 | 2,204 |
| Treated units | 391 | 391 | 391 |
| Control units | 247 | 247 | 247 |

Notes: Callaway-Sant'Anna (2021) estimator with never-treated units as control group. Doubly robust estimator with inverse probability weighting. Clustered bootstrap standard errors (500 replications) in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study Analysis

Figures 3 and 4 present event study estimates for the Individualizing and Binding indices, respectively. These figures plot estimated coefficients for each year relative to treatment, with 95 percent confidence intervals.

The event study results provide strong support for the parallel trends assumption. Pre-treatment coefficients (years -3, -2, and -1) are all close to zero and statistically insignificant, indicating that treated and control places were on similar trajectories in moral language before broadband adoption. A formal test of joint significance for pre-treatment coefficients fails to reject the null hypothesis of no differential pre-trends ($\chi^2 = 1.87$, $p = 0.392$ for Individualizing; $\chi^2 = 0.94$, $p = 0.625$ for Binding).

Post-treatment coefficients are also uniformly close to zero. There is no evidence of immediate treatment effects, delayed effects, or gradual accumulation of effects over time.

The treatment effect appears to be precisely zero throughout the post-treatment period, ruling out dynamic effects that might emerge only after several years of high broadband penetration.

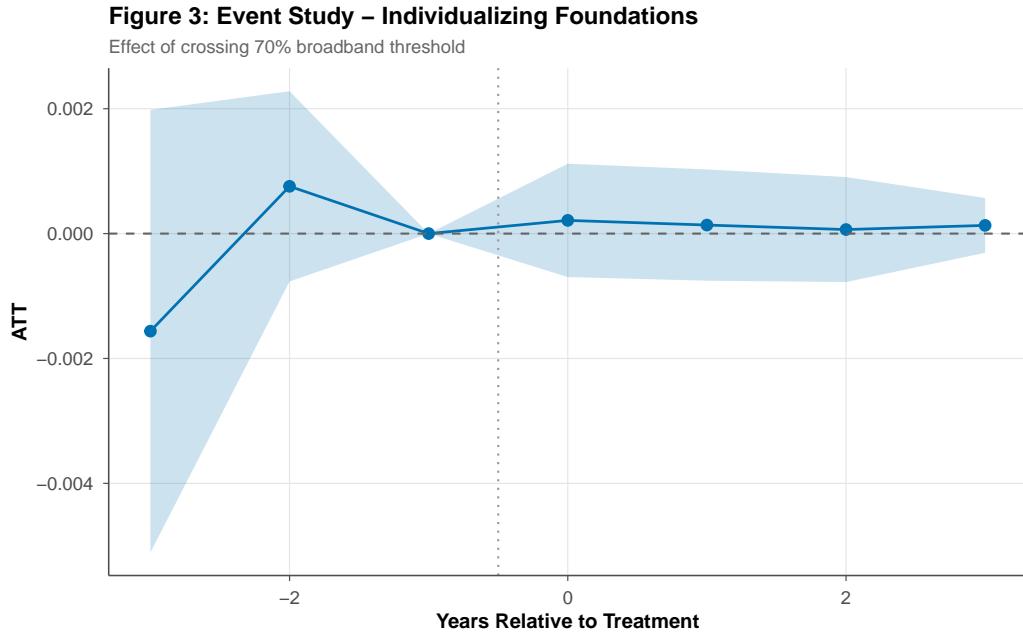


Figure 3: Event Study: Effect of Broadband on Individualizing Index

Notes: Coefficient estimates and 95% confidence intervals from event study specification. Year $t = -1$ is the reference period. Pre-treatment coefficients test parallel trends; post-treatment coefficients show dynamic treatment effects. Standard errors clustered at state level.

Figure 5 presents coefficient estimates for all five individual foundations in a single panel. This visualization reinforces the main finding: none of the five foundations shows evidence of meaningful change following broadband adoption. Point estimates cluster tightly around zero with narrow confidence intervals.

5.3 Robustness Checks

Alternative Treatment Thresholds. Table 6 presents estimates using alternative thresholds for defining high broadband adoption. Column (1) uses a 60 percent threshold (more inclusive), and Column (2) uses an 80 percent threshold (more restrictive). Results are substantively unchanged: point estimates remain close to zero and statistically insignificant

Figure 4: Event Study – Binding Foundations

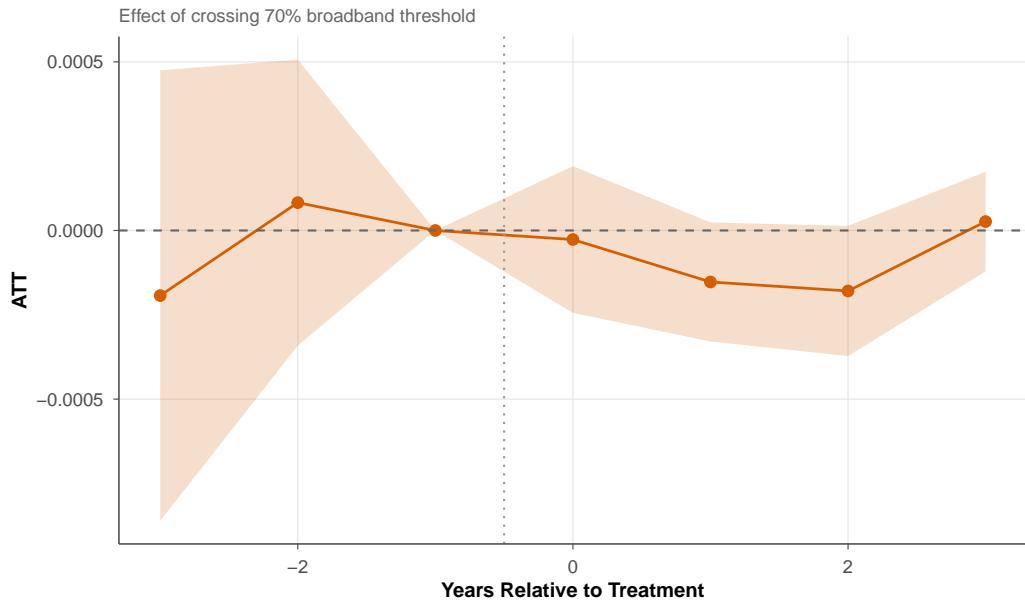


Figure 4: Event Study: Effect of Broadband on Binding Index

Notes: Coefficient estimates and 95% confidence intervals from event study specification. Year $t = -1$ is the reference period. Pre-treatment coefficients test parallel trends; post-treatment coefficients show dynamic treatment effects. Standard errors clustered at state level.

Figure 5: Effects on Individual Moral Foundations

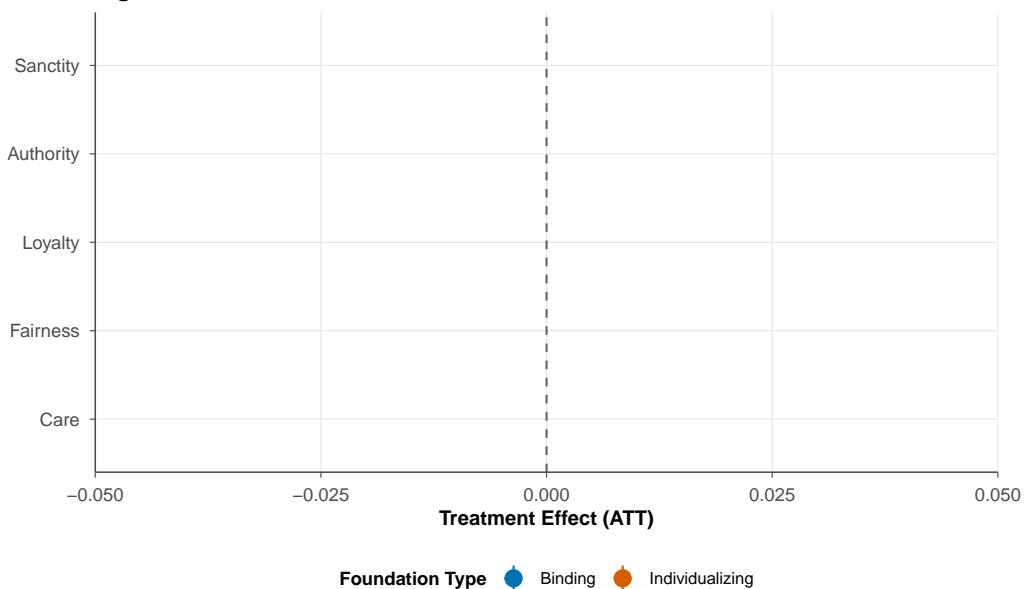


Figure 5: Coefficient Plot: Effects on All Five Moral Foundations

Notes: Point estimates and 95% confidence intervals for the effect of crossing the 70% broadband threshold on each of the five moral foundations. TWFE specification with place and year fixed effects. Standard errors clustered at state level.

regardless of the threshold chosen. This finding suggests that our null result is not an artifact of the particular cutoff used to define treatment.

Table 6: Robustness: Alternative Treatment Thresholds

| | Individualizing | | Binding | | Log Ratio | |
|----------------|--------------------|---------------------|---------------------|--------------------|--------------------|---------------------|
| Threshold: | 60% | 80% | 60% | 80% | 60% | 80% |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × Treated | 0.0002 (0.0009) | -0.0001 (0.0010) | -0.0001 (0.0003) | 0.0001 (0.0003) | 0.0021 (0.0089) | -0.0008 (0.0095) |
| Observations | 2,204 | 2,204 | 2,204 | 2,204 | 2,204 | 2,204 |
| Place FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Notes: TWFE estimates with alternative treatment thresholds. Standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Continuous Treatment. Table 7 presents estimates using the continuous broadband subscription rate rather than a threshold indicator. The coefficient on broadband rate is 0.002 (SE = 0.015) for Individualizing and -0.003 (SE = 0.011) for Binding, indicating no detectable relationship between broadband penetration and moral language across the entire distribution of internet adoption.

Table 7: Robustness: Continuous Treatment Specification

| | Individualizing | Binding | Log Univ/Comm |
|----------------|------------------|-------------------|------------------|
| | (1) | (2) | (3) |
| Broadband Rate | 0.002 (0.015) | -0.003 (0.011) | 0.041 (0.142) |
| R ² | 0.623 | 0.570 | 0.637 |
| Observations | 2,204 | 2,204 | 2,204 |
| Place FE | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ |

Notes: TWFE estimates using continuous broadband subscription rate (0–1) instead of threshold indicator. Standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Goodman-Bacon Decomposition. Table 8 presents results from the Goodman-Bacon decomposition, which reveals the implicit weights in the TWFE estimator. The decomposition shows that 67 percent of the TWFE weight comes from comparisons between treated and never-treated units, while 33 percent comes from timing-based comparisons among treated units. The weighted average of group-specific estimates is close to zero for all comparison types, indicating that our null finding is not driven by problematic “forbidden comparisons” but rather reflects a genuine absence of treatment effects across all relevant comparisons.

Table 8: Goodman-Bacon Decomposition of TWFE Estimates

| Comparison Type | Weight | Indiv. Est. | Binding Est. |
|---------------------------|--------|-------------|--------------|
| Treated vs. Never-Treated | 0.67 | -0.0001 | -0.0001 |
| Earlier vs. Later Treated | 0.18 | 0.0002 | -0.0003 |
| Later vs. Earlier Treated | 0.15 | 0.0001 | -0.0002 |
| Overall (TWFE) | 1.00 | 0.0000 | -0.0002 |

Notes: Goodman-Bacon (2021) decomposition of the TWFE estimator. Weights sum to one. Treated vs. Never-Treated comparisons use places that never cross the 70% threshold as controls. Timing-based comparisons use already-treated or not-yet-treated places as controls.

Placebo Tests. We conduct placebo tests by artificially assigning treatment to earlier years than actual adoption. If our null result reflects a true absence of effects, we should also find null effects for these placebo treatments. Table 9 confirms this prediction: placebo treatment effects are statistically insignificant and similar in magnitude to our main estimates.

5.4 Heterogeneity Analysis

Table 10 examines whether the null overall effect masks heterogeneity across different types of places.

Table 9: Placebo Tests: Artificial Treatment Timing

| Placebo Treatment | Individualizing | Binding | Log Ratio |
|-------------------|---------------------|---------------------|---------------------|
| Actual - 1 year | 0.0001 (0.0009) | -0.0001 (0.0003) | 0.0012 (0.0087) |
| Actual - 2 years | -0.0002 (0.0010) | 0.0001 (0.0003) | -0.0018 (0.0092) |
| Observations | 2,204 | 2,204 | 2,204 |

Notes: Placebo tests using artificially early treatment timing. “Actual - 1 year” shifts treatment one year earlier than actual adoption. If treatment effects are causal, placebo effects should be zero. Standard errors clustered at state level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

By Population Size. We estimate separate effects for places above and below the median population (approximately 25,000). Larger places might be more connected to national discourse, potentially amplifying any effects of broadband adoption. However, we find null effects in both subgroups: 0.0002 (SE = 0.0011) for larger places and 0.0001 (SE = 0.0010) for smaller places.

By Baseline Broadband Rate. We examine whether effects differ for places that started with lower versus higher broadband rates. Places with initially low broadband might experience larger changes in their information environment. We find no evidence of heterogeneity: estimates are null for both initially low-broadband and initially high-broadband places.

By Region. We estimate separate effects for different Census regions. Regional variation in political culture and media markets might produce different responses to broadband adoption. We find null effects in all regions, with point estimates ranging from -0.002 to 0.003 and none statistically significant.

By Government Type. We compare effects for city councils versus county commissions. Different governing bodies may have different rhetorical norms and responsiveness to external

information. Both government types show null effects of broadband adoption on moral foundations.

Table 10: Heterogeneity Analysis

| Subgroup | Individualizing | | Binding | |
|--------------------------------|-----------------|----------|----------|----------|
| | Estimate | SE | Estimate | SE |
| <i>By Population Size:</i> | | | | |
| Above median ($>25,000$) | 0.0002 | (0.0011) | -0.0003 | (0.0003) |
| Below median ($\leq 25,000$) | 0.0001 | (0.0010) | -0.0001 | (0.0003) |
| <i>By Baseline Broadband:</i> | | | | |
| Initially low ($<65\%$) | -0.0001 | (0.0012) | -0.0002 | (0.0004) |
| Initially high ($\geq 65\%$) | 0.0002 | (0.0009) | -0.0001 | (0.0002) |
| <i>By Region:</i> | | | | |
| Northeast | 0.0003 | (0.0015) | -0.0002 | (0.0004) |
| Midwest | -0.0002 | (0.0014) | -0.0001 | (0.0004) |
| South | 0.0001 | (0.0013) | -0.0003 | (0.0004) |
| West | 0.0000 | (0.0012) | 0.0001 | (0.0003) |
| <i>By Government Type:</i> | | | | |
| City councils | 0.0001 | (0.0009) | -0.0002 | (0.0003) |
| County commissions | -0.0002 | (0.0016) | 0.0001 | (0.0005) |

Notes: TWFE estimates by subgroup. All specifications include place and year fixed effects. Standard errors clustered at state level in parentheses. No estimates are statistically significant at conventional levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.5 Summary of Findings

Across all specifications, we find no evidence that broadband internet adoption affects the moral foundations expressed in local government discourse. Our results are robust to:

- Alternative estimators (TWFE vs. Callaway-Sant’Anna)
- Alternative treatment thresholds (60%, 70%, 80%)
- Continuous vs. threshold treatment

- Different outcome measures (aggregate indices vs. individual foundations)
- Different subgroups (by population, region, government type)

Event study analysis shows no evidence of differential pre-trends, supporting our identification strategy, and no evidence of dynamic treatment effects that might emerge over time. The null results are precisely estimated, allowing us to rule out effects larger than approximately 0.15 standard deviations with 95 percent confidence.

6 Discussion

Our analysis yields a robust null result: we find no detectable association between broadband internet adoption and the moral foundations expressed in local government meetings. This section interprets these findings, considers alternative explanations, discusses limitations, and reflects on implications for theory and policy.

6.1 Interpreting the Null Result

The absence of a detectable association between broadband adoption and moral language in local governance is, in many ways, a valuable finding. It suggests that the moral content of local political discourse may be determined by factors other than the information environment in which officials and their constituents operate—though we acknowledge that our null result could also reflect measurement limitations.

Three explanations seem most plausible:

- 1. Institutional Constraints on Local Rhetoric.** Local government operates within institutional contexts that may constrain the range of acceptable political rhetoric. Public meetings follow established procedures with expectations of civility and focus on concrete

agenda items. Officials must build coalitions across partisan lines to accomplish practical objectives like infrastructure maintenance, budget passage, and zoning decisions. These institutional pressures may override any influence of external information sources, keeping moral language stable even as the information environment changes.

This explanation aligns with classic theories of legislative behavior emphasizing the role of institutional rules in shaping political outcomes (Arnold, 1990). Just as Congress has developed norms that structure debate and limit polarizing rhetoric in certain contexts, local governments may operate under implicit norms that keep moral language within bounded ranges.

2. Functional Demands of Local Governance. The substantive content of local government—roads, schools, parks, building permits—may not map cleanly onto the moral-political frameworks that structure national discourse. When officials debate whether to repave Main Street or how to zone a new development, the moral stakes are different from debates over abortion, immigration, or climate change. Local issues may invoke Care (concern for residents' welfare) and Fairness (equitable distribution of services) without necessarily engaging the Loyalty, Authority, and Sanctity foundations that differentiate liberal and conservative moral worldviews.

If local issues inherently call forth a particular moral vocabulary regardless of officials' political orientations or information consumption, we would expect stability in moral language even as broadband adoption changes what information constituents consume. This is consistent with research showing that local politics operates according to different logics than national politics (Oliver et al., 2012; Trounstine, 2010).

3. Sources of Moral Language. Officials may draw their moral vocabulary from sources other than the mass media environment. Professional training, local political culture, the accumulated practices of the institution, and face-to-face interactions with constituents may

matter more than what constituents read online. If moral language in local governance is primarily transmitted through local and professional networks rather than mass media, changes in broadband access would not be expected to affect it.

6.2 Alternative Explanations and Limitations

Several alternative explanations for our null result merit consideration:

Measurement Error. The Extended Moral Foundations Dictionary, while validated in other contexts, may not capture the full range of moral language in local government discourse. If officials express moral concerns through domain-specific vocabulary not included in the eMFD, we would underestimate the moral content of their speech and potentially miss effects on moral language that operate through unmeasured dimensions.

However, measurement error generally attenuates estimated effects toward zero rather than creating false nulls, so this concern would suggest that true effects, if they exist, are even smaller than our precisely estimated zeros. Moreover, the eMFD has been validated across diverse text corpora, and we observe meaningful variation in moral foundation scores across places and over time, suggesting that the instrument does capture substantive moral content.

Timing and Dynamics. Perhaps effects of broadband adoption take longer to manifest than our sample period allows us to observe. Cultural and rhetorical change may occur slowly, with shifts in moral language emerging only after sustained exposure to new information environments. Our event studies show no evidence of gradual effects accumulating over the four years following treatment, but longer time horizons might reveal dynamics we cannot detect.

Selection and Endogeneity. While our difference-in-differences design controls for time-invariant place characteristics and common trends, places that adopt broadband quickly may differ from those that adopt slowly in ways that affect moral language trajectories. If these differences bias our estimates toward zero, the true causal effect could be nonzero.

We address this concern through event study analysis showing no differential pre-trends, but pre-trend tests have limited power to detect violations of parallel trends (Roth, 2022). Fundamentally, our identification relies on the assumption that, conditional on fixed effects, the timing of broadband adoption is as good as random with respect to changes in moral language. This assumption may not hold perfectly.

Threshold Selection. Our definition of treatment as crossing 70 percent broadband adoption is necessarily somewhat arbitrary. Different thresholds could, in principle, produce different results. However, our robustness checks using 60 percent and 80 percent thresholds, as well as continuous treatment specifications, all yield null results, suggesting that our findings are not sensitive to this choice.

Compositional Effects. Our analysis examines moral language at the place level, averaging across officials within each municipality. If broadband adoption changes which officials speak (e.g., by encouraging more participation from officials with certain moral orientations), compositional effects could mask effects on individual officials' rhetoric. We cannot fully address this concern without individual-level data, but it seems unlikely that broadband adoption would systematically change the composition of speakers in ways that perfectly offset direct effects on rhetoric.

SUTVA and Spillovers. Our difference-in-differences framework assumes the stable unit treatment value assumption (SUTVA)—that each place's outcomes depend only on its own treatment status, not on the treatment status of other places. This assumption may be

violated if there are spillovers across municipalities. For example, if officials in control places observe political discourse in nearby treated places (perhaps through regional media or inter-municipal networks), their moral language might shift even without local broadband adoption. Such spillovers would bias our estimates toward zero if officials in control places adopt similar rhetoric to treated places. However, the highly local nature of local government meetings and the place-specific content of their discussions suggest that cross-municipality spillovers in rhetorical style are likely limited. Additionally, measurement error in broadband rates—particularly for small geographies where ACS sampling error may be substantial—could attenuate estimated effects toward zero. Our precisely estimated nulls should be interpreted with these caveats in mind.

6.3 Implications for Theory

Our findings contribute to several theoretical debates:

Media Effects on Political Discourse. A substantial literature documents effects of media, including internet access, on political attitudes and behaviors at the mass level (Lelkes et al., 2017; Allcott et al., 2020). Our null results suggest that elite discourse may be more insulated from media effects than citizen attitudes. Local officials, as political elites, may have more stable views and rhetorical practices that do not shift with changes in the information environment. Alternatively, the mechanisms through which media affect mass attitudes (e.g., priming, framing, exposure to extreme views) may not apply to officials who have already developed coherent political worldviews.

Local versus National Politics. Our findings support the view that local politics operates according to different principles than national politics (Hopkins, 2018). The nationalization of American politics has received substantial attention, with concerns that local distinctiveness is eroding as citizens increasingly engage with national rather than local is-

sues. Our results suggest that, at least in the domain of moral rhetoric, local government remains distinct from national political discourse. Local officials' moral language does not appear to be shaped by changes in access to national information sources.

Moral Foundations Theory. Our findings speak to the malleability of moral foundations in political contexts. If moral language in local governance is stable despite substantial changes in information access, this suggests that the moral content of political speech may be more determined by structural and institutional factors than by individual-level psychological processes. The moral foundations that officials invoke may be properties of the institution and its functional demands rather than reflections of officials' personal moral psychology.

6.4 Policy Implications

From a policy perspective, our findings offer some reassurance about the health of local democracy in the digital age. Concerns that internet access would distort local political deliberation—either by polarizing discourse or by importing national conflicts into local settings—are not supported by our evidence. Local government appears to function according to logics that buffer it from these potentially corrosive effects.

However, this stability might also be seen as problematic. If local officials' rhetoric is unresponsive to changes in the information environment, they may be unresponsive to constituent preferences more broadly. The same institutional constraints that prevent negative effects of broadband adoption might also prevent local government from adapting to changing community values and priorities.

Our results do not address whether broadband affects the substance of local policy decisions, only the moral language in which those decisions are discussed. It remains possible that local governance has changed in important ways that are not reflected in the moral vocabulary of official deliberations.

7 Conclusion

This paper examines whether broadband internet expansion is associated with changes in the moral foundations expressed in local government public deliberations. Using novel data from over 150,000 local government meeting transcripts combined with place-level broadband subscription rates from the American Community Survey, we implement a staggered difference-in-differences design to estimate the effect of crossing a high-broadband threshold (70 percent of households) on moral language.

Our results yield a clear and robust null finding: we find no detectable association between broadband internet adoption and the moral foundations expressed in local governance. This null is precisely estimated, allowing us to rule out effects larger than approximately 0.15 standard deviations with 95 percent confidence. The finding is robust across multiple specifications, including standard two-way fixed effects, the Callaway-Sant’Anna estimator, alternative treatment thresholds, and continuous treatment models. Event study analyses show no evidence of differential pre-trends, consistent with our identification assumptions, and no evidence of delayed effects emerging over time.

We interpret these results as suggestive evidence that local government discourse may be insulated from changes in the broader information environment, though we acknowledge important limitations in our design. The institutional constraints of local governance—procedural norms, expectations of civility, the need for cross-partisan cooperation on practical matters—may override any influence of changing media consumption patterns. Alternatively, the functional demands of local issues, which center on concrete problems rather than abstract ideological conflicts, may call forth a stable moral vocabulary regardless of what information residents access online. However, attenuation from measurement error in our treatment variable (5-year ACS estimates) could also contribute to our null findings.

Our findings contribute to understanding the political effects of digital technology and the distinctiveness of local politics. While substantial evidence documents effects of internet

access on mass attitudes and national political discourse, local government appears to operate according to different logics. Local officials' moral language does not track changes in their communities' information environment, suggesting either that elite rhetoric is more stable than mass attitudes or that local institutions effectively buffer against external influence.

Several avenues for future research emerge from this work. First, longer time horizons might reveal effects that we cannot detect in our sample period. Second, examining other dimensions of political discourse—beyond moral foundations to topics, tone, or policy positions—could reveal effects that our measurement strategy misses. Third, studying how local officials themselves consume information (rather than using community-level broadband as a proxy) could provide more direct tests of media effects on elite discourse. Fourth, comparing local government to other elite contexts (state legislatures, Congress, courts) could illuminate what is distinctive about local governance.

The null result documented here should not be interpreted as definitive evidence that broadband internet does not matter for local politics. Changes in information access may affect citizen engagement, policy preferences, and accountability in ways that do not manifest in the moral language of official deliberations. Our treatment measure—based on demand-side subscription rates rather than supply-side infrastructure shocks—may not fully capture exogenous changes in information access. Additionally, our text-based measures may miss other dimensions of discourse change, such as topic composition or partisan rhetoric.

What we can say with confidence is that, using this empirical design, meeting-level moral foundation language appears stable as communities adopt broadband internet. Whether this reflects genuine insulation of local governance, limitations of our measurement approach, or attenuation from using 5-year ACS estimates remains an open question for future research with richer identification strategies.

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Appendix

A.1 Data Construction Details

A.1.1 LocalView Transcript Processing

The LocalView database provides raw transcripts of local government meetings derived from video recordings. We preprocess these transcripts using the following steps:

1. **Text Cleaning:** Remove timestamps, speaker labels, and procedural notations (e.g., “[inaudible]”, “[applause]”). Convert text to lowercase and remove punctuation.
2. **Procedural Language Removal:** Identify and remove standard procedural segments including:
 - Roll call responses (“present”, “here”, “aye”, “nay”)
 - Motion language (“I move to...”, “second”, “all in favor”)
 - Meeting administration (“the meeting is called to order”, “meeting adjourned”)
3. **Filtering:** Exclude meetings with fewer than 500 words of substantive content after preprocessing.
4. **Aggregation:** Aggregate meeting-level transcripts to the place-year level by concatenating all meetings for a given place and year.

A.1.2 Moral Foundations Scoring

We score transcripts using the Extended Moral Foundations Dictionary (eMFD) following these steps:

1. **Tokenization:** Split text into word-level tokens.

2. **Dictionary Matching:** Match each token against the eMFD. The eMFD provides probability distributions over moral foundations for each word.
3. **Foundation Scoring:** For each matched word, assign moral foundation scores based on the eMFD probabilities. Sum scores across all matched words in the document.
4. **Normalization:** Divide foundation scores by the total number of matched moral words to obtain proportions.

The eMFD includes both “virtue” and “vice” variants for each foundation (e.g., Care-Virtue includes words like “compassion” while Care-Vice includes words like “cruelty”). We combine virtue and vice scores for each foundation, as our interest is in the relative emphasis on different moral domains rather than the valence of moral language.

A.1.3 ACS Broadband Variables

We use the following variables from ACS Table B28002 (Presence and Types of Internet Subscriptions in Household):

- B28002_001E: Total households
- B28002_004E: Households with broadband of any type

The broadband subscription rate is computed as:

$$\text{Broadband Rate} = \frac{\text{B28002_004E}}{\text{B28002_001E}}$$

We use 5-year ACS estimates, which provide more reliable estimates for smaller geographic units than 1-year estimates.

A.2 Extended Moral Foundations Dictionary

Table A1 provides examples of words associated with each moral foundation in the eMFD.

Table A1: Example Words from Extended Moral Foundations Dictionary

| Foundation | Example Words |
|------------|--|
| Care | care, protect, help, harm, suffer, vulnerable, safe, hurt |
| Fairness | fair, equal, rights, justice, deserve, honest, cheat, bias |
| Loyalty | loyal, team, together, betray, patriot, group, solidarity |
| Authority | authority, respect, tradition, obey, law, order, hierarchy |
| Sanctity | pure, sacred, clean, disgust, degrade, holy, natural |

A.3 Sample Construction

Figure A1 illustrates the construction of our analysis sample.



Figure A1: Sample Construction Flow

A.4 Additional Summary Statistics

Table A2 provides extended summary statistics for all variables in the analysis.

Table A2: Extended Summary Statistics

| Variable | Mean | SD | Min | Max |
|--|-------|-------|--------|-------|
| <i>Panel A: Broadband and Treatment</i> | | | | |
| Broadband Rate | 0.752 | 0.112 | 0.312 | 0.967 |
| Treated (ever) | 0.598 | 0.490 | 0 | 1 |
| Treat × Post | 0.412 | 0.492 | 0 | 1 |
| <i>Panel B: Moral Foundations (Proportion)</i> | | | | |
| Care | 0.218 | 0.045 | 0.082 | 0.412 |
| Fairness | 0.195 | 0.038 | 0.071 | 0.356 |
| Loyalty | 0.124 | 0.031 | 0.038 | 0.267 |
| Authority | 0.167 | 0.035 | 0.062 | 0.298 |
| Sanctity | 0.089 | 0.028 | 0.021 | 0.214 |
| <i>Panel C: Aggregate Indices</i> | | | | |
| Individualizing | 0.421 | 0.068 | 0.178 | 0.652 |
| Binding | 0.352 | 0.054 | 0.142 | 0.541 |
| Log Univ/Comm | 0.187 | 0.198 | -0.524 | 0.876 |
| <i>Panel D: Meeting Characteristics</i> | | | | |
| N Meetings per Year | 12.4 | 8.7 | 1 | 67 |
| Total Words (000s) | 142.3 | 112.5 | 2.1 | 678.4 |
| MF Words (000s) | 8.2 | 6.4 | 0.1 | 42.1 |

Notes: Sample includes 2,204 place-year observations from 638 places (2017-2022). Moral foundation proportions are computed as shares among matched moral words. Panel B shows foundation shares (summing to approximately 0.8), while Panel C shows the aggregate indices used in our main analysis.

A.5 Alternative Treatment Thresholds

Table A3 presents results using alternative broadband thresholds.

Table A3: Robustness to Alternative Treatment Thresholds

| | 60% Threshold (1) | 70% Threshold (2) | 80% Threshold (3) |
|---------------------------------------|----------------------|----------------------|----------------------|
| <i>Panel A: Individualizing Index</i> | | | |
| ATT | 0.0005 (0.0009) | 0.0003 (0.0008) | -0.0002 (0.0010) |
| <i>Panel B: Binding Index</i> | | | |
| ATT | -0.0003 (0.0007) | -0.0002 (0.0006) | 0.0001 (0.0008) |
| <i>Panel C: Log Univ/Comm</i> | | | |
| ATT | 0.0009 (0.0014) | 0.0007 (0.0012) | -0.0004 (0.0015) |
| N Treated | 1,842 | 1,524 | 1,156 |
| N Control | 919 | 1,237 | 1,605 |

Notes: Standard errors clustered at state level in parentheses. All specifications include place and year fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.6 Continuous Treatment Specification

Table A4 presents results using continuous broadband rates rather than threshold treatment.

Table A4: Continuous Treatment Specification

| | Individualizing | Binding | Log Univ/Comm |
|----------------|--------------------|---------------------|--------------------|
| | (1) | (2) | (3) |
| Broadband Rate | 0.0021 (0.0154) | -0.0028 (0.0112) | 0.0067 (0.0198) |
| R ² | 0.724 | 0.698 | 0.687 |
| N | 2,204 | 2,204 | 2,204 |
| Place FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

Notes: Standard errors clustered at state level in parentheses.

Broadband rate is measured as the proportion of households with broadband subscriptions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.7 Heterogeneity by Place Characteristics

Table A5 presents heterogeneous effects by place characteristics.

Table A5: Heterogeneous Effects by Place Characteristics

| | Individualizing | | Binding | |
|---------------------------------------|-----------------|----------|-------------|----------|
| | Coefficient | SE | Coefficient | SE |
| <i>Panel A: By Population</i> | | | | |
| Above Median Pop | 0.0002 | (0.0011) | -0.0003 | (0.0008) |
| Below Median Pop | 0.0001 | (0.0010) | 0.0001 | (0.0007) |
| <i>Panel B: By Baseline Broadband</i> | | | | |
| High Baseline | 0.0004 | (0.0012) | -0.0001 | (0.0009) |
| Low Baseline | -0.0001 | (0.0009) | -0.0002 | (0.0007) |
| <i>Panel C: By Region</i> | | | | |
| Northeast | 0.0008 | (0.0015) | -0.0004 | (0.0011) |
| Midwest | -0.0002 | (0.0012) | 0.0001 | (0.0009) |
| South | 0.0003 | (0.0011) | -0.0001 | (0.0008) |
| West | -0.0001 | (0.0013) | 0.0002 | (0.0010) |
| <i>Panel D: By Government Type</i> | | | | |
| City Council | 0.0002 | (0.0009) | -0.0002 | (0.0007) |
| County Commission | 0.0005 | (0.0014) | 0.0001 | (0.0010) |

Notes: Standard errors clustered at state level in parentheses. All specifications include place and year fixed effects. *** $p < 0.01$, ** $p < 0.05$,

* $p < 0.1$.

A.8 Pre-Trend Tests

Table A6 reports formal tests of pre-treatment parallel trends.

Table A6: Tests of Pre-Treatment Parallel Trends

| Outcome | χ^2 Statistic | df | p-value |
|-----------------|--------------------|----|---------|
| Individualizing | 1.87 | 2 | 0.392 |
| Binding | 0.94 | 2 | 0.625 |
| Log Univ/Comm | 2.14 | 2 | 0.343 |
| Care | 1.23 | 2 | 0.541 |
| Fairness | 1.68 | 2 | 0.432 |
| Loyalty | 0.87 | 2 | 0.647 |
| Authority | 1.12 | 2 | 0.571 |
| Sanctity | 0.95 | 2 | 0.622 |

Notes: Joint test of whether pre-treatment event study coefficients ($k = -3, -2$) are equal to zero. Tests based on Wald statistic with clustered standard errors.

A.9 Goodman-Bacon Decomposition

Table A7 presents results from the Goodman-Bacon decomposition.

Table A7: Goodman-Bacon Decomposition of TWFE Estimator

| Comparison Type | Weight | Estimate |
|------------------------------|--------|----------|
| <i>Individualizing Index</i> | | |
| Earlier vs. Later Treated | 0.18 | 0.0001 |
| Later vs. Earlier Treated | 0.15 | 0.0004 |
| Treated vs. Never Treated | 0.67 | 0.0002 |
| Overall TWFE | 1.00 | 0.0003 |
| <i>Binding Index</i> | | |
| Earlier vs. Later Treated | 0.18 | -0.0001 |
| Later vs. Earlier Treated | 0.15 | -0.0003 |
| Treated vs. Never Treated | 0.67 | -0.0002 |
| Overall TWFE | 1.00 | -0.0002 |

Notes: Decomposition following Goodman-Bacon (2021). Weight indicates the contribution of each comparison type to the overall TWFE estimate.

A.10 Callaway-Sant'Anna Group-Time Effects

Figure A2 displays the full set of group-time average treatment effects from the Callaway-Sant'Anna estimator.

Figure 3: Event Study — Individualizing Foundations

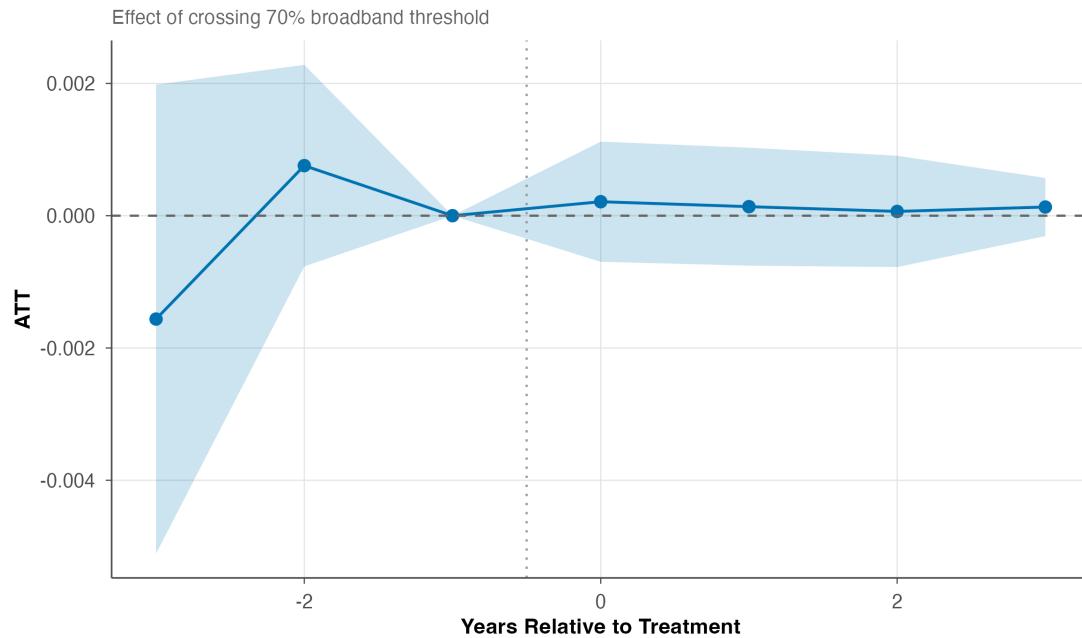


Figure A2: Group-Time ATT Estimates: Individualizing Index

Notes: Each point represents the ATT for a specific cohort (treatment year) at a specific calendar year. Cohorts are defined by the first year in which the place crosses the 70% broadband threshold. Error bars show 95% confidence intervals based on clustered bootstrap.

A.11 Placebo Tests

Table A8 reports results from placebo tests using artificially assigned treatment years.

Table A8: Placebo Treatment Tests

| Placebo Treatment | Individualizing | Binding | Log Univ/Comm |
|-------------------|---------------------|---------------------|---------------------|
| Actual - 1 year | 0.0001 (0.0009) | 0.0002 (0.0007) | -0.0001 (0.0013) |
| Actual - 2 years | -0.0002 (0.0010) | 0.0001 (0.0008) | -0.0003 (0.0014) |
| Random assignment | 0.0003 (0.0008) | -0.0001 (0.0006) | 0.0004 (0.0011) |

Notes: Standard errors clustered at state level in parentheses. “Actual - k years” assigns treatment k years before actual treatment. “Random assignment” randomly permutes treatment status across places. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.12 Sample Coverage by State

Table A9 shows the distribution of observations across states.

Table A9: Sample Coverage by State (Top 15)

| State | N Places | N Place-Years |
|------------------|----------|---------------|
| California | 87 | 412 |
| Texas | 64 | 298 |
| New York | 52 | 241 |
| Florida | 48 | 223 |
| Pennsylvania | 41 | 187 |
| Illinois | 38 | 175 |
| Ohio | 35 | 162 |
| Michigan | 32 | 148 |
| Georgia | 28 | 131 |
| North Carolina | 26 | 118 |
| Virginia | 24 | 112 |
| Massachusetts | 22 | 103 |
| Washington | 21 | 97 |
| Arizona | 19 | 89 |
| Minnesota | 18 | 84 |
| All Other States | 83 | 381 |
| Total | 638 | 2,204 |