How Did the Internet Change Campaign Fundraising?*

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Abstract

Since the advent of broadband internet, the number of individual campaign contributors has quadrupled. While journalistic and scholarly accounts often emphasize the internet's role in this transformation, a large body of literature also argues that its effect on political participation was small. I clarify why these seemingly contradictory findings coexist. I first show that the introduction of broadband itself induced only modest changes. Using two panel studies leveraging the rollout of broadband and household web-browsing data, I find that access to fast internet did little to increase households' time spent online on political content, and broadband adoption accounts for only 4.2% of the growth in political contributions between 1996 and 2008. The internet's more consequential role lies in amplifying traditional campaign strategies by making it easier to contribute. Exploiting exogenous variation in television ad exposure along media market boundaries, I show that exposure to presidential ads doubled contributions in places with high broadband access, accounting for 43% of the increase over time. In contrast, TV advertising had no measurable effect in the early 1990s or in regions without broadband in the 2000s. These results imply that by making offline appeals actionable, the internet reshaped campaign incentives and ushered in an era where all publicity isn't just good—it's profitable.

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1 Introduction

Since broadband became widely available in the 1990s, the number of individual campaign contributors has increased fourfold.[1] Campaign professionals often argue that the internet had a profound impact on elections by lowering barriers to participation, arming citizens with information, and enabling microtargeted mobilization (Trippi, 2004; Morris, 2011; Issenberg, 2013). Yet scholars disagree on the internet's role in this transformation. Some highlight the innovative role the internet played in fundraising strategies (Bimber, 2014; Magleby et al., 2018). In contrast, a growing body of research finds the internet's direct effects on participation to be modest and largely reinforcing existing dynamics (Bimber and Davis, 2003; Vaccari, 2010; Boulianne, 2009; Stromer-Galley, 2019; Boulianne, 2020; Lorenz-Spreen et al., 2023). Multiple meta-analyses of hundreds of studies report a positive, but small effect (Boulianne, 2009, 2020), which likely represents an upper bound given reliance on self-reports and potential confounding. How can we reconcile these diverging accounts of the internet's impact on elections?

Explaining this surge matters because individual donations can change whose voices politicians attend to and which candidates win elections. Prior research shows that strengthening the role of individual donors advantages ideologically extreme candidates and deepens polarization by replacing moderates (Barber, 2016; Kilborn and Vishwanath, 2021). In the United States, longstanding inequalities along social and economic lines have also shaped who votes, who speaks, and who donates (Verba and Nie, 1987; Brady et al., 1995; Schlozman et al., 2012). Depending on how the internet affects political participation, these dynamics may also distort who politicians listen to (Kalla and Broockman, 2016).

This paper clarifies the internet's impact on modern campaign fundraising by highlighting an underexplored mechanism: its interaction with existing campaign strategies. I argue that the internet's most important contribution was not primarily to increase political information or directly generate new donors, but to make offline campaign appeals easier to act on by lowering barriers to giving money. I test this idea using television advertising, which has long been one of the largest categories of campaign spending. I show that in the broadband era, television ads were no longer

[1] Based on McDonald (2025) and Center for Responsive Politics (2025), comparing 2016 to 1992. Individual contributions rose 3.2-fold as a share of the population, and total dollars contributed nearly ninefold. Turnout, by contrast, rose only slightly, from 58.8% in 1992 to 60.1% in 2016. In 2024, turnout was 64.1%, up 9% from 1992, while the number of contributors rose eightfold to 3.3 million and contributions thirty-fold to $10.3 billion.

only a medium for persuasion but also a driver for donations. More broadly, my findings suggest that digital infrastructures enable campaigns to combine online and offline strategies, turning publicity into dollars more efficiently.

To support these claims, the paper focuses on three causal effects. To begin, I use the varying speed of the expansion of broadband across the United States from 1996 to 2008 to understand its effect on political contributions. Studying the causal impact of broadband internet in an observational setting has proven difficult due to the lack of credible empirical designs. A simple comparison of areas with differing levels of broadband access risks conflating the effects of broadband with underlying demographic differences and market dynamics that influence both donation behavior and broadband provider expansion.[2] To address these issues, I construct a panel-based identification strategy where I compare areas that are similar in terms of observable market characteristics like income and the pre-existing number of broadband providers but experience different trajectories in broadband rollout over time.

Using this variation in broadband availability, I establish that internet provision is positively correlated with individual political contributions. These increases are primarily driven by repeated donations, evidenced by the effect of broadband being most pronounced in ZIP codes that already exhibited high levels of political giving at baseline. While these findings exhibit no pre-trends according to placebo tests, the magnitude of the effect is substantially small. I estimate that a one-standard deviation increase in the treatment corresponds to a 0.015 standard deviation increase in donations. Back-of-the-envelope calculations suggest that broadband expansion alone accounts for roughly 4.2% of campaign contributions during the sample period.

To put the previous findings in perspective, I next examine whether broadband spurred political engagement through changes in the online media consumption. Using household-level web-browsing data from Comscore (2002–2010), I implement a difference-in-differences design that compares respondents within the same city before and after broadband adoption. I track visits to over 400 political websites (Gentzkow and Shapiro, 2011; Lelkes et al., 2017), presidential campaign sites, and the donation platform ActBlue. This within-city comparison isolates the effect of

[2] As one review of broadband's economic consequences notes, "There appears to be a positive economic impact from expanded broadband deployment and adoption. However, various research challenges, including methodological problems and access to sufficiently granular data, have prevented the authors from drawing more definitive conclusions from the US broadband experience" (Holt and Jamison, 2009). The same challenges complicate efforts to understand broadband's broader social and political consequences.

new technology while holding the offline media and campaign environment constant.

Broadband adoption is associated with 8.5 additional visits to political news websites per household per year. These effects are statistically significant, and dynamic specifications show no evidence of pre-trends, suggesting the estimates are plausibly causal. Yet the magnitude is, again, modest: 8.5 visits translate into roughly 45 minutes per year, or reading an extra online article every one or two months for about 5 minutes. By contrast, broadband increases overall internet use by more than 24 hours per household per month. Moreover, I find no evidence that broadband adoption increased visits to presidential campaign websites or ActBlue. Together, these results suggest that while broadband mechanically increased exposure to political information by expanding total time online, access alone did not make households more politically engaged.

If broadband did not substantially increase exposure to online political content, what explains the rise in campaign contributions? Part of the answer lies in the way the internet reduced the transaction costs of giving (Farrell, 2012; Zhuravskaya et al., 2020). By the mid-2000s, online donations had become both viable and common: campaign websites integrated payment portals (Williams et al., 2009; Druckman et al., 2009), and in 2004 ActBlue launched as a centralized platform for small-dollar Democratic donations. Whereas contributions were once primarily mailed in, the internet created a low-friction channel where supporters could locate a candidate and contribute with a few clicks.

Still, the web-browsing evidence shows that lowering barriers to giving did not by itself generate new political interest or substantially increase traffic to donation platforms. To account for the observed surge in contributions, I turn to variation in campaign activity and show that television ads became effective as a fundraising tool with the advent of the internet.

Television advertising constitutes a substantial share of campaign expenditures. In the 2000s, federal campaigns allocated about 16% of their budgets to traditional media, making it the second-largest spending category after administrative costs.[3] To link campaign activity to contributions, I adapt a design that leverages mismatches between Designated Media Markets (DMA) and state boundaries to generate plausibly exogenous variation in presidential television advertisement expo-

[3]Statistic calculated by the author using data from Sheingate et al. (2022). Reported value is the average share of campaign budgets allocated to media by presidential, Senate, and House campaigns across the 2004–2010 election cycles. The dataset also reports the following categories: administrative (45.4%), consulting (9.4%), fundraising (7.3%), legal (4.6%), digital (4.4%), travel (3.8%), field (2.2%), and polling (1.7%).

sure (Huber and Arceneaux, 2007; Urban and Niebler, 2014; Spenkuch and Toniatti, 2018). This analysis reveals exposure to presidential ads has large, measurable effects on donations: in ZIP codes with high volumes of presidential advertising, broadcast TV approximately doubles the total dollar value of contributions. Over time, this effect accounts for 43% of the total dollar amount of donations in areas exogenously exposed to presidential ads, and about one-third of this increase comes from first-time contributors.

I extend these results in three ways to demonstrate that this television effect would likely not exist without the internet. First, presidential ads generate donations only in areas that have high levels of broadband coverage. Second, despite substantial use of television advertising in the 20th century, I find that television ads had no effect on campaign contributions until the 2004 election cycle, coinciding with the emergence of campaign websites with integrated donation tools and the launch of ActBlue. These temporal patterns suggest that the development of online infrastructure was necessary for making it easier for supporters to respond to campaign appeals with contributions. Third, ActBlue donations themselves increase as a result of presidential advertising, but only in broadband-connected areas. The null effect in low-connectivity areas serves as a negative control: if the internet is the key mechanism, we should not observe an effect where online giving is difficult.

The structure of the paper is as follows. The next section discusses existing theories of the political effects of the internet and highlights related research. Section 3 presents the data sources. Section 4 describes the empirical strategy of studying the effect of broadband on donations, and Section 5 presents these results. Section 6 studies the impact of broadband subscription on online browsing behavior. Section 7 investigates how broadband and television advertising interact. Section 8 concludes.

2 Understanding of the political effects of the internet

Scholars have proposed three broad explanations for how the internet could shape contribution behavior. The first emphasizes campaign adaptation, focusing on how candidates adopted digital tools to mobilize supporters. The second highlights changes to the information environment: the internet expanded the supply of news by competing with traditional outlets, and enabled selective patterns of online consumption. The third centers on reduced participation costs, as online platforms lowered the barriers to engaging in politics. Together, these perspectives yield mixed expectations, leaving open the question of how the underlying mechanisms interact to influence campaign finance.

The first explanation emphasizes how campaigns adopted digital strategies to reach and mobilize supporters in the 2000s. Building on the long-standing role of campaigns as recruiters in political participation (Brady et al., 1999), the internet provided yet another avenue for this outreach. These efforts quickly grew sophisticated, employing data-driven targeting and behavioral modeling to spur action (Issenberg, 2012; Kreiss, 2012; Bimber, 2014). Some accounts highlight its pivotal role, showing how the Obama campaign in particular leveraged digital tools to communicate with voters and raise funds (Magleby et al., 2018). Yet other scholars remain skeptical about the internet's transformative impact. They argue that while campaigns did adopt new tools, these did not fundamentally alter campaign dynamics or provide decisive advantages (Vaccari, 2010; Bimber, 2014; Margolis and Resnick, 2000; Stromer-Galley, 2019). As Vaccari (2010) notes, "The Internet cannot be seen as a 'magic bullet' capable in and of itself of driving support and resources to a candidate."

Beyond campaign-driven mobilization, another potential channel concerns the internet's informational effects. The second class of explanations emphasizes its role in substituting for traditional news and enabling online search. These theories argue that access to digital media shifts the resources and motivations individuals bring to participation. By disrupting the revenue models that sustained journalism (Djourelova et al., 2023), the internet altered the supply of news and accelerated the shift from print and broadcast to digital sources. At the same time, the online environment—with its high degree of choice—changed the information individuals encounter. However, here too, scholars disagree about the resulting implications for political engagement.

Some research suggests that expanded choice encourages avoidance of political content, with individuals substituting toward entertainment and reducing overall consumption of political information (Prior, 2005). A handful of studies even find that internet access can depress turnout under certain conditions (Falck et al., 2014; Campante et al., 2018; Gavazza et al., 2019), implying that increased internet use might reduce campaign contributions. Conversely, other studies argue that the internet increases exposure to political content and participation. Broadband access is associated with higher internet use (Hitt and Tambe, 2007), which can mechanically increase exposure to partisan news (Lelkes et al., 2017) or reinforce prior beliefs through curated media diets (Negroponte, 1995; Bakshy et al., 2015; Peterson et al., 2021).

The third explanation highlights how the internet lowers the costs of participation. Scholars have often noted this mechanism (Schlozman et al., 2010; Farrell, 2012; Zhuravskaya et al., 2020), but relatively little empirical work directly tests it. From the perspective of the resource model (Brady et al., 1995; Schlozman et al., 2010), reduced transaction costs represent one of the clearest ways technology might increase contributions. Yet if citizens' underlying interest in politics is limited, simply lowering barriers may be insufficient to generate substantial increases in giving.

Given these mechanisms, theory does not yield a clear prediction for the net effect of broadband on campaign contributions, and prior empirical evidence remains mixed. With respect to campaign donations, two unpublished manuscripts are particularly relevant, but diverge in their conclusions. Larcinese and Miner (2018) rely on a state-level right-of-way law instrument to study the effect of increased broadband in the 2008 election and report null results. Jaber (2013) uses geographic terrain as an instrument instead and finds that broadband explains 40% of contributions in his sample, attributing this effect to increases in political knowledge due to access to the internet. In general, the literature also generally reports positive but modest effects on political knowledge and engagement (Boulianne, 2009, 2020; Lorenz-Spreen et al., 2023). Survey-based analyses of donation behavior similarly suggest that broadband access did not fundamentally reshape patterns of political giving (Panagopoulos and Bergan, 2009; Schlozman et al., 2012).

This paper argues that existing work has focused too narrowly on what happens online when assessing the internet's political effects. In the case of campaign contributions, no single mechanism—whether informational effects or reduced participation costs—fully explains observed patterns. I show that the internet's influence emerges from the interaction of lowered logistical barriers and offline campaign interventions, such as television advertising that becomes actionable online. In this sense, my work builds on prior ideas by Farrell (2012), by providing new evidence on testing the complex and interrelated causal mechanisms that the internet introduces. It also provides a coherent explanation for why the internet appears transformative qualitatively, even when measured effects in isolation appear small and campaign dynamics remain largely stable.

3 Data

My analysis draws on five data sources: (i) broadband expansion across the United States, (ii) itemized campaign contribution records, (iii) ZIP-code–level covariates, (iv) household-level web-browsing data, and (v) presidential advertising data linked to Designated Media Areas (DMAs). To estimate the impact of broadband internet on campaign contributions, I merge broadband provider data with contribution records at the ZIP-code level and supplement these with ZIP-level covariates. To examine the role of presidential advertising, I further combine advertising data with these sources.

Broadband provider data: Data on the availability of broadband internet comes from the Federal Communications Commission (FCC). This data source is well-established for this time period and has been widely used in previous social science studies. Under Form 477, the FCC requires internet service providers (ISPs) offering speeds above 200 kilobits per second (downstream or upstream) to report their coverage twice annually. I use these records to observe the number of registered ISPs at the ZIP code level until 2008.

Campaign contributions: I combine the broadband data with campaign contribution records from Bonica (2019), which compiles and cleans individual donation data from the Federal Election Commission (FEC), covering federal, state, and local elections. For each election cycle between 1990 and 2008, I aggregate the number of individual contributions originating from each ZIP code. I include previous years because in some analysis I use extensive lags in contribution data.

All analyses come with the important caveat that contributions totaling less than $200 per donor-recipient pair over the course of a campaign cycle are not required to be itemized by the FEC and thus are not included in the dataset. For example, a $30 donation would be captured if the contributor donates at least seven times to the same recipient (bringing the cumulative total above $200), but would not appear if they donate less than $200 in total to any single recipient, or small amounts across multiple recipients. As a result, my estimates likely understate the true effect. To assess the extent of this underreporting, Appendix Figure C3 shows the aggregate dollar value of unitemized contributions to congressional candidates from 1998 to 2025. In the period analyzed, unitemized donations make up, on average, 18.6% of total contribution dollars, but this share declines, reaching 13.4% in 2008.

In addition, ZIP code information is required for geocoding contributions. Fortunately, ZIP codes are reported for the vast majority of itemized contributions. After correcting apparent formatting errors, only a small fraction of donations are missing ZIP codes in election cycles up to 2000. Between 2002 and 2006, the share of itemized contributions missing ZIP codes hovers around 10%, and rises to approximately 17% in the 2008 cycle.

Household-level web browsing data: Comscore collects online browsing data from a large, nationally representative sample of American internet users. For each panelist, the data include their type of internet connection (broadband or dial-up), basic household-level covariates, and detailed records of web browsing activity. I use data spanning the years 2002 to 2010. Following Lelkes et al. (2017), I aggregate the number of visits to political websites, as defined by the domain list in Gentzkow and Shapiro (2011), for each panelist in each year. I also measure visits to presidential campaigns websites listed in the Library of Congress.

Campaign advertising on television: Data on political advertisements aired on broadcast television come from the Wisconsin Advertising Project. Following Urban and Niebler (2014), I focus on campaign ads aired during the general election periods of the 2000, 2004, and 2008 presidential races. The coverage of the Wisconsin Advertising Project expanded over time, from 75 of the largest Designated Market Areas (DMAs) in 2000 to all 210 DMAs by 2008. DMAs are a group of counties that determine television and radio markets, within which consumers can receive the same broadcast content.

I match the geographic placement of these ads from the DMA level to counties using the mapping provided by Sides et al. (2022). For the purposes of my analysis, I restrict attention to counties located in non-battleground states that received advertising because they fall within a battleground DMA. I define battleground states as those rated as "toss-up" or "lean" by the *Cook Political Report's Electoral College Ratings* for the respective election year.

ZIP-level demographics: I construct ZIP-code-level covariates for population, demographics, personal income, and land use. Population, demographic, and income variables are derived from county-level government statistics, which I disaggregate to 2000 ZIP Code Tabulation Areas (ZCTAs) using the LandScan Global dataset. LandScan provides remote-sensing-based estimates of average 24-hour population at a 1 km resolution. I rasterize the county-level data based on population distributions and then aggregate it to the ZCTA level. This method follows prior work on fine-grained geographic analysis of technology adoption (Guriev et al., 2021)[4].

Land use data are obtained from the Historical Settlement Data Compilation for the United States (HISDAC-US), which provides rasterized classifications at 250m resolution across six land use categories: agricultural, commercial, industrial, owned-residential, rented-residential, and recreational. I aggregate these data to ZCTAs and compute the proportion of land in each category. Additional details on the procedures used to aggregate or disaggregate variables by geography can be found in Appendix 8.

I trim the dataset based on population estimates in three ways. First, I exclude ZCTAs with estimated populations under 30 to avoid inflating per capita outcomes due to small denominators. This removes approximately 2.3% of ZIP codes, and the main results are robust to the choice of threshold. Second, I exclude ZCTAs with implausibly high donation-to-population ratios, typically corresponding to downtown business districts or commercial areas where few people reside but donation records may be linked to office or P.O. box addresses. I remove 0.74% of ZIP codes where the ratio exceeds 1:3. Raising this threshold increases the magnitude of point estimates but also their variance. Finally, and importantly, I exclude ZIP codes with no reported population and those classified as rural. As will be discussed in the institutional background, this design is tailored to capture competitive entry among telephone-based broadband providers. In rural areas, these dynamics are less relevant: markets are often too small to support multiple entrants, and broadband access is more likely provided by satellite or cable technologies. In these contexts, the number of providers is unlikely to be conditionally random, and so they are excluded from the analysis.

4 A design for studying the effect of broadband

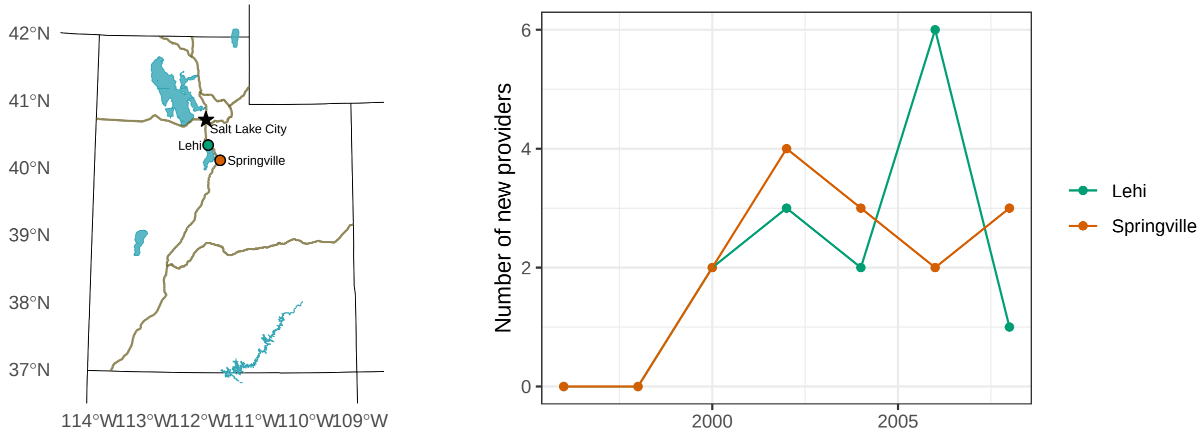
This study aims to estimate the average change in political donations associated with the entry of an additional broadband provider. In an ideal setting, one would observe household-level broadband subscriptions and compare donation behavior to similar households with only dial-up access. In the absence of such granular data, I estimate an intent-to-treat effect at the ZIP code level, under the assumption that more broadband providers lead to greater broadband penetration within an area.

[4]The American Community Survey did not exist prior to 2008.

Figure 1: Trajectory of Broadband Provider Entry in Two Cities in Utah Valley

(a) Geographic Location

(b) Number of New Providers Each Year



Note: Yearly count of new broadband entrants across two ZIP codes in each of two U.S. cities in the Utah Valley.The map on the left plots where each city is in the state, along major lakes, cities, and highways. The figure on the right plots data from the Federal Communications Commission's Form 477.

A key empirical challenge is that the entry of broadband providers is not random: larger and wealthier areas are both more attractive markets for broadband providers and more likely to generate higher levels of political donations. The task, then, is to isolate the causal effect of increased internet connectivity from these confounding demand-side factors.

The identifying variation comes from the difference in the year-to-year rate at which telephone broadband providers enter a ZIP code.[5]. In order to use broadband internet, U.S. residents needed to buy access from these broadband internet providers who built the critical "last mile" infrastructure connecting the user to an internet service provider's backend network.

To provide some intuition, Figure 1b visualizes the variation I use by plotting the number of new broadband providers for Lehi and Springville, two proximate, mid-size cities in the Utah Valley. Both of these areas received two broadband providers for the first time in 2000, and ultimately

[5]Over the period of time in question for this study, the two most common broadband providers were phone and cable companies. Although cable providers have historically offered one of the most cost-effective and popular methods of accessing the internet (Abe, 1997; Crandall, 2005), cable television operators formed monopolies around cities and municipalities (Abe, 1997; Laubach et al., 2002). Cable television operators often held exclusive franchises granted by local governments, which gave them sole rights to lay coaxial cables—either by stringing them from utility poles or burying them underground. As a result, cable broadband providers rarely competed within the same markets and often even cooperated with each other to develop and deploy broadband technology(Laubach et al., 2002; Crawford, 2010) This study relies on the intensive margin of broadband provider entry and cable companies likely did not contribute much to the variation necessary for identification.

received the same numbers of total broadband providers (14) by the end of 2008. Demographically, the cities are very similar across several variables, such as racial composition, income, family makeup, population density and total population. However, the pace of broadband roll out differed. Springville experienced an earlier spike in provider entry, while Lehi's growth was more concentrated in later years. The empirical strategy generalizes this idea across the United States by making short-term comparisons in donation behavior over time across observably similar ZIP codes after differential growth in broadband provision. The remainder of this section provides historical context on why this expansion varied idiosyncratically across places, then outlines the estimation strategy.

4.1 Institutional background

Broadband internet expanded rapidly across the United States from the late 1990s to the 2000s, marking a major shift from other dial-up connections. Unlike dial-up, which was slow and required users to choose between using the phone or the internet, broadband provided a high-speed, always-on connection. This allowed users to easily browse the web, stream and share multimedia, and access a broader range of online services, making the internet a more integral part of daily life for many Americans.

I begin my study in 1996, the year broadband adoption accelerated following the Telecommunications Act of 1996. This legislation disrupted existing market structures by enabling new firms to compete in broadband provision through telephone lines. Prior to the reform, Regional Bell Operating Companies— the incumbent local exchange carriers (ILECs) created by the breakup of AT&T in 1984—enjoyed local monopolies owing to the high entry costs of building telephone infrastructure. The 1996 Act sought to modernize decades-old broadcasting and telecommunications regulations to "let anyone enter any communications business—to let any communications business compete in any market against any other" (Federal Communications Commission, 2013). A key provision required ILECs to grant new competitive local exchange carriers (CLECs) "interconnection" with their networks and "unbundled access" to existing facilities at fair rates (Congress, 1996). This opened the door for thousands of new, small, and entrepreneurial CLECs to enter the broadband market(Kushnick, 2006).

Table 1: Over-Time Effect of Broadband Providers on Broadband Subscriptions

	% subscribed
N Broadband provider	0.07***
	(0.000)
N	45,153
ZIP FE	✓

Note: The dependent variable is the percentage of respondents in a ZIP code who subscribed to broadband, as measured by Comscore between 2002 and 2008. An observation is a Zip code-year. The regression includes ZIP code fixed effects. The standard errors are clustered at the ZIP code level. * p < 0.05, ** p < 0.01, *** p < 0.001 (two-tailed test)

Did this influx of broadband providers lead to more subscribers? A key assumption underlying the rest of the analysis is the existence of the first stage relationship where the entry of an additional broadband provider leads to an increase in broadband subscriptions. On the aggregate, it certainly seems so as the share of Americans using the internet grew rapidly from 10% in 1995 to 72% by 2010 (World Bank). There are also at least two theoretical reasons why we would expect this relationship to hold, at least in the short term. For one, the arrival of a new internet provider may indicate expanded coverage into previously unserved areas. For another, an additional provider may also reduce prices (Grubesic and Murray, 2004; U.S. Government Accountability Office, 2006), making broadband more affordable and encouraging more people to subscribe. Numerous prior studies have found a positive association between the number of broadband providers and subscriptions in the cross-section (Kolko, 2010; Jaber, 2013; Larcinese and Miner, 2018; Trussler, 2021; Djourelova et al., 2023). In Table 1, I provide an additional test that illustrates this, even over time and within the same zip code. I regress the fraction of broadband subscribers among all internet users against the number of broadband providers with ZIP code fixed effects. I estimate that an additional provider is associated with a 7 percentage point increase in broadband subscribers.

As mentioned earlier, the timing of entry for these broadband providers was generally predictable. A large body of research on the digital divide has established that local demographics such as wealth, population density, education levels, and favorable regulatory environments are strong predictors of where broadband is deployed early and extensively (Chaudhuri et al., 2005; Flamm and Chaudhuri, 2007; U.S. Government Accountability Office, 2006; Kolko, 2012). This was because delivering fast internet required significant infrastructure investment, and providers needed to accurately anticipate demand. For instance, to provide xDSL, the primary broadband service provided by telephone companies, the company would need to install a DSLAM in the central switching office (Abe, 1997). Because central offices function as a wire center for the copper telephone lines that serve an entire geographic area, wealthy, educated, and urban communities closer to these facilities were more likely to have broadband because they were also more likely to pay for premium speed internet.

Yet at the same time, providers did not have total control or foresight over where to upgrade equipment due to technological constraints. The locations of central switching offices were determined decades before the advent of the internet, since they served as the physical buildings where phone companies have historically hosted telephone switching boards. xDSL technology itself was also constrained by distance. It relied on the same copper line infrastructure as telephone technology, but signals used for high speed internet deteriorated beyond roughly 18,000 feet from a central office (Jackson, 2002; Grubesic and Murray, 2004; Xiao and Orazem, 2011). Thus, even if an area was demographically favorable, service provision depended on its physical proximity to existing infrastructure, on top of other factors such as regulatory environment and existence of competition. In addition, providers faced choices about where to upgrade first, given limited resources. For example, Grubesic and Murray (2002) simulate broadband rollout in Franklin County, Ohio using data and switching office locations, and show that multiple configurations of central switching offices upgrade plans yielded the same level of profit. Because it wasn't financially feasible to equip every switching office in the city at once, areas with similar market characteristics often experienced broadband rollout at different rates. This technological and geographic contingencies created the variation in broadband entry on which my empirical strategy relies.

4.2 Estimation

I estimate a panel regression that leverages short-term variation in the number of broadband providers, controlling for observable, time-varying market characteristics such as average income and baseline provider counts. The intuition is that, conditional on market attractiveness and existing saturation, the marginal entry of a broadband provider in a given year is largely idiosyncratic and due to supply-side shocks.

The estimation is summarized in the following equation:

$$\Delta Donations_{zy} = \tau N\ new\ provider_{zy} + \beta_1 N\ existing\ providers_{zy} + \beta_2 Income_{zy} + \alpha_z + \gamma_{y \times s} + \varepsilon_{zy}$$

The outcome of interest, $\Delta Donations$, is the change in the number of donations per capita in ZIP code z relative to the previous election cycle. The main explanatory variable, *N new provider*, captures the number of new broadband providers entering ZIP code z in the most recent two-year period. *N existing providers* and *Income* represent the number of already-existing broadband providers and the average personal income in ZIP code z in year y. α_z and $\gamma_{y \times s}$ are ZIP code and election year by state fixed effects. This specification lets me focus on changes along the intensive margin of broadband providers within a ZIP code in a two-year period. The year-by-state fixed effect restricts the comparisons to ZIP codes of the same year within the state—a specification motivated by broadband providers facing different sets of state regulations and anti-competitive behavior by regional ILECs. Lastly, I cluster standard errors at the ZIP level.

Three features of the regression specification require discussion. First, I estimate the model in first differences rather than levels. Long-term comparisons using levels may be biased if broadband has broader positive effects on the local economy, thus violating all else equal comparisons over time. Focusing on short-term effects is also historically appropriate. The 1996 Telecommunications Act sought to foster competition, but the market soon re-consolidated due to litigation, anticompetitive behavior by ILECs, and the post-dot-com crash bankruptcy of many CLECs (Ferguson, 2002; Grubesic and Murray, 2002; Starr, 2002). The 2005 FCC unbundling order further reduced competition by limiting the obligations of incumbent carriers (Bauer, 2005; Ford and Spiwak, 2016). As a result, the relationship between provider count and broadband penetration likely weakened over time. First-differencing also addresses the challenge of non-stationary time series, given that both broadband availability and donations tend to rise monotonically over time.

Second, I include unit fixed effects after differencing. This means I estimate a two-way fixed effects model in changes, rather than levels. The ZIP fixed effect guards against any time-invariant location attributes that affect the growth trajectory of donation behaviors and provider entry. It compares changes within a ZIP code relative to a unit-specific growth trend, using similar units as counterfactuals. In Appendix B, I show that this specification performs better in placebo pre-trend tests than either a levels-based two-way fixed effects model or a simple first-differences design.

Third, I retain the integer-valued treatment variable (i.e., the number of broadband providers), rather than converting it into a binary indicator. Discretization risks introducing arbitrary thresholds, especially in contexts where broadband provision is unevenly distributed. Urban ZIP codes can have up to 30 providers in a given year, while the median ZIP code has no more than 8. Binarizing this variation could lead to over or under-estimate the effect depending on an arbitrary value whose significance varies depending on the location in the country.

5 Broadband Leads to Small Increases in Campaign Donations

Table 2a presents evidence of a positive relationship between the availability of broadband service providers and campaign donations at the ZIP code level. Each column estimates a version of the equation outlined in Section 4.2, using different specifications or dependent variables.

In Column 1, I regress the change in per capita donations on the growth in broadband providers. To account for variation in ZIP code population sizes, I normalize the outcome to counts per 100,000 residents. The results show that each additional broadband provider is associated with 30.2 more donations per 100,000 people, equivalent to a 0.015 standard deviation increase in response to a 1 standard deviation increase in the within-unit broadband provision. When multiplied by the observed number of broadband providers, the estimate implies that broadband expansion accounts for approximately 4.2% of total donations over the study period.

Column 2 examines whether broadband growth expands the donor base by increasing the number of unique donors in a ZIP code. The average ZIP code sees a change of 21.3 unique donors between elections, so the estimate implies each additional provider corresponds to a roughly 5% increase relative to this mean. Column 4 turns to the other side of the donor-recipient relationship: I count the number of unique donation recipients in each ZIP code and find that broadband also broadens the set of supported candidates and organizations. The results in Table 2a demonstrate that broadband internet increased both the number of individuals who donate and the variety of candidates and organizations they support.

I implement a version of the standard "pre-trends" test common in panel data settings. Under the assumption that a marginal increase in broadband providers at time t_0 should not affect cam-

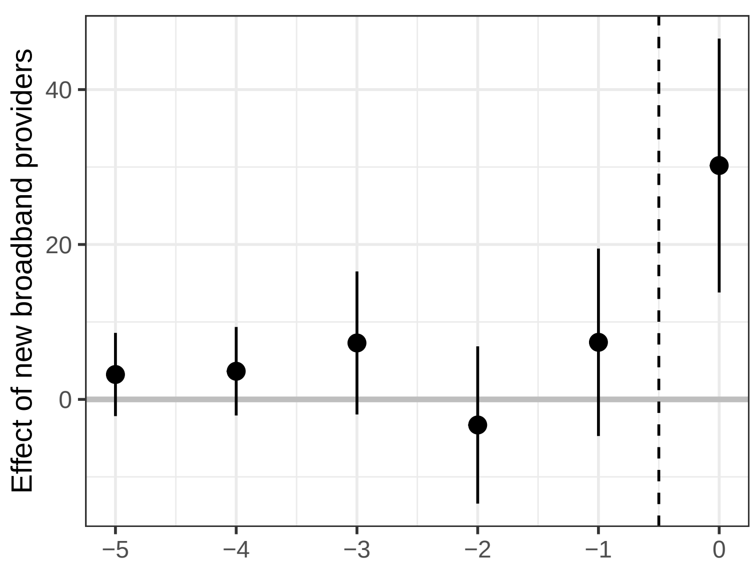
Figure 2: The Effect of Broadband on Donations

(a) Regression results

	Count per capita	N unique donors	N recipients
N new providers	30.19***	1.08***	0.19***
	(8.36)	(0.26)	(0.04)
Personal income	676.51***	21.05***	1.04**
	(58.61)	(2.01)	(0.33)
Baseline N providers	38.72***	2.57***	−0.03
	(6.51)	(0.16)	(0.03)
N	136.388	136.388	136.388
ZIP FE	✓	✓	✓
Year × State FE	✓	✓	✓

Note: The table presents regression results where the dependent variables are the change in the number of donations per 100,000 population, the change in the number of unique donors, and the change in the number of unique donation recipients for each ZIP code. An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

(b) Placebo Test Using Lagged Change in the Number of Donations



Note: This figure plots coefficients from regressions of lagged values of the change in the number of donations on contemporaneous growth in the number of broadband providers. The specification follows that in Table 2a, Column 1; see the table for details. Error bars represent 95% confidence intervals.

paign donations at earlier periods $t-1$, $t-2$ and so on, I regress lagged donation outcomes on the same main specification.[6] Figure 2b plots the coefficients on broadband provider growth, with each point estimated using a separate regression and a different lagged (placebo) outcome. Unlike a typical event-study plot, this approach does not rely on a dynamic treatment effect structure, but instead estimates standalone placebo regressions. The results show no evidence of significant pre-trends: the marginal increase in broadband provision does not predict prior levels of campaign giving, and the effect appears to occur "on impact."

The robustness of the main results and the underlying research design is evaluated through several empirical checks, reported in the Appendix B. First, I demonstrate that the main findings are robust across a range of alternative, plausible specifications. In one such test, I interact year dummies with several ZIP-level covariates that predict broadband rollout, allowing for flexible time trends based on potential confounders. Across these specifications, the estimated effect of broadband remains positive and statistically significant. Second, I show that including lagged dependent variable as a control does not change the results reported in Table 2a. This suggests that previous levels of giving are unrelated to broadband provision, mitigating concerns of feedback.

Next, I examine whether the expansion of broadband internet changed existing patterns in political giving. Table 2, column 1, tests whether broadband growth increases the number of first-time donors. The indicator for first-time donors is taken directly from Bonica (2019). I find that each additional broadband provider is associated with three more first-time donations per 100,000 residents. This effect accounts for approximately 10% of the total impact reported in Table 2a, suggesting that broadband helped introduce some number of new donors into the system.

Yet, the more substantial effect of broadband appears to come from existing donors. In column 2, I interact broadband growth with a binned measure of baseline donor activity, specifically, the average number of donations per ZIP code in pre-broadband years (prior to 1996). The results show that the effect of broadband is concentrated entirely in the top tercile of ZIP codes that were already high-contributing before broadband expansion. To more directly illustrate this, in column 3, I show that broadband increases the average number of contributions made per donor within a ZIP code and in column 4, I show that it reduces the relative fraction of donations coming from new donors each year. While both of these estimates are small, directionally, they point towards

[6] In Appendix B, I provide additional visual intuition behind these placebo tests.

Table 2: Broadband Effect on Donor Pool

	Count per capita (First-time donations)	Count per capita (All donations)	Avg N per donor (All donations)	Percent new (All donations)
N new providers	3.06*	-0.47	0.01*	-0.13***
	(1.54)	(11.17)	(0.00)	(0.01)
Baseline N providers	7.81***	35.50***	0.03***	-0.39***
	(0.42)	(6.71)	(0.00)	(0.01)
Personal income	40.75***	686.33***	0.16***	0.27***
	(5.48)	(60.38)	(0.03)	(0.05)
Growth ×		1.15		
Baseline donations: Medium		(10.21)		
Growth ×		49.98***		
Baseline donations: High		(13.41)		
N	136,388	123,130	128,082	128,089
ZIP FE	✓	✓	✓	✓
Year × State FE	✓	✓	✓	✓

Note: The table presents regression results where the dependent variables are the change in number of donations per 100,000 population (Columns 1 and 2), the change in the average number of times a donor gives within an election cycle for each ZIP code (Column 3), and the percentage of donations that come from new donors (Column 4). An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * p < 0.05, ** p < 0.01, *** p < 0.001 (two-tailed test)

Table 3: Heterogeneity by Recipient and Donor Type

	President	Congress	Committees	In-state	Out-of-state
N new providers	11.98***	−0.61	15.73**	3.33	14.89*
	(2.64)	(1.85)	(5.90)	(1.79)	(7.30)
Baseline N providers	16.83***	−2.30	34.48***	2.51*	19.38***
	(2.15)	(1.84)	(3.57)	(1.18)	(5.34)
Personal income	472.92***	34.47**	133.71***	21.74*	181.85***
	(28.88)	(10.66)	(30.81)	(9.44)	(47.42)
N	136.388	136.388	136.388	136.388	136.388
ZIP FE	✓	✓	✓	✓	✓
Year × State FE	✓	✓	✓	✓	✓

Note: The table presents regression results where the dependent variable is the change in the number of donations per 100,000 population for a specific subgroup. In-state and out-of-state donations exclude contributions to presidential candidates, as geographic attribution is not well-defined. An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

the trend that broadband primarily reinforced preexisting patterns of political engagement.

One concern is that my analysis does not capture donations under $200, which are not itemized in FEC records and thus unobservable in the main dataset. Since these small donations could plausibly be more sensitive to lower transaction costs, their exclusion might understate the effect of broadband. To address this, I present two checks in Appendix C. First, I analyze contributions reported by ActBlue, a Democratic conduit organization required to disclose all donations regardless of size. Second, I examine the frequency of sub-$200 contributions when such data are available. In both cases, I find that broadband has a positive but still relatively small effect on these outcomes, which supports the conclusion that the increase in donation due to the introduction of broadband was not primarily driven by small-dollar giving during this period.

Finally, I examine how the effect of broadband varies across types of electoral races and geographic targeting. Table 3 reports the coefficients on the broadband ISP measure for different categories of donation recipients. Columns 1–3 show that broadband expansion is positively associated with increased contributions to presidential candidates and non-candidate political committees, but has no significant relationship with donations to congressional candidates.

This pattern aligns with the idea that online information flows are not constrained by geographic proximity. The internet makes it easier for individuals to access information about distant races, thus lowering informational barriers to participation in national politics. To further test this, columns 5–6 distinguish between in-state and out-of-state donations (excluding presidential candidates, for whom geographic definitions are ambiguous). The results show that broadband access more strongly predicts donations to out-of-state candidates than to those within a donor's home state. These findings suggest that the expansion of high-speed internet likely did provide information about primarily prominent, nationally salient races. Broadband appears to have broadened the geographic and institutional reach of donors and nationalized patterns of political giving.

6 Effects of Broadband Subscription on Online Behavior are Modest

The patterns observed in the previous section suggest that broadband's effect on campaign contributions is modest and largely driven by existing donors. To understand why, it is useful to examine the underlying mechanisms. I show that broadband primarily extends users' existing online habits, increasing time spent online but not fundamentally changing them. Furthermore, only a small fraction of this additional time is devoted to political news. These findings indicate that broadband alone does not substantially alter the informational environment of users.

The evidence comes from an online panel dataset that passively tracks households' web browsing behavior between 2002 and 2010. Crucially, each household reports whether it has access to broadband internet. While the dataset is primarily composed of repeated cross-sections, a subset of households appears in multiple years, allowing me to construct a short panel and observe within-household changes over time.[7] For each household, the data include the website domains visited, the number of pages viewed, and the total time spent on each domain. I record traffic to 406 online news websites classified by Gentzkow and Shapiro (2011), presidential campaign websites, and ActBlue. To assess how broadband affects political engagement online, I implement a

[7] I exclude households that always report having broadband access ("always treated" units).

difference-in-differences design by estimating the regression:

$$y_{hy} = \alpha_h + \gamma_{y\times c} + \tau Broadband_{yh} + \beta X_{yh} + \varepsilon_{yh}$$

This specification exploits within-household variation by including household fixed effects.[8] Year-by-city fixed effects further ensure that identification comes from households that change their internet access relative to others in the same location and year. Critically, the within-city comparisons ensure that I am comparing households that are placed in the same media market and face the same set of political races to vote for. I additionally control for household size and whether the household has a child or not.

Table 4a presents the main results. Columns 1 and 2 report the total number of visits to, and time spent (in minutes) on, online news websites. Broadband subscription increases the annual number of visits to online news by 8.5 times. The corresponding increase in time, 46 minutes per year, is not statistically significant, and translated to just under one minute per week. Column 3 examines the share of visits directed to news relative to total internet use. I find a precise null effect, indicating that the increase in news browsing is proportional to the overall growth in time spent online following broadband adoption.

Figure 4b plots dynamic estimates from the panel regression in Column 1 of Table 4a. No statistically significant pre-trends are evident before broadband adoption, supporting a causal interpretation. However, the slight upward slope could reflect either sampling variation or unobserved confounding. If the latter, the Column 2 estimates may be upwardly biased. Even under the more favorable interpretation, the effect remains small relative to natural year-to-year variation in search behavior. In short, the increase in news browsing following broadband adoption is modest at best.

Broadband adoption does, however, lead households to increase their overall time online substantially. Table D7 shows that subscribers spend more than 24 additional hours per month online after adoption. Yet this expansion does not translate into large increases in news consumption,

[8] Lelkes et al. (2017) report similar results using Comscore data, but my analysis differs in two ways. First, while they rely on cross-sectional comparisons in 2004, I focus on panel respondents observed across multiple waves. Second, they restrict attention to "partisan" outlets, while I use the broader set classified by Gentzkow and Shapiro (2011). When replicating their approach—excluding household fixed effects and limiting attention to partisan sites—I obtain nearly identical estimates. Under the panel specification, however, the effect falls to less than half and is no longer statistically significant.

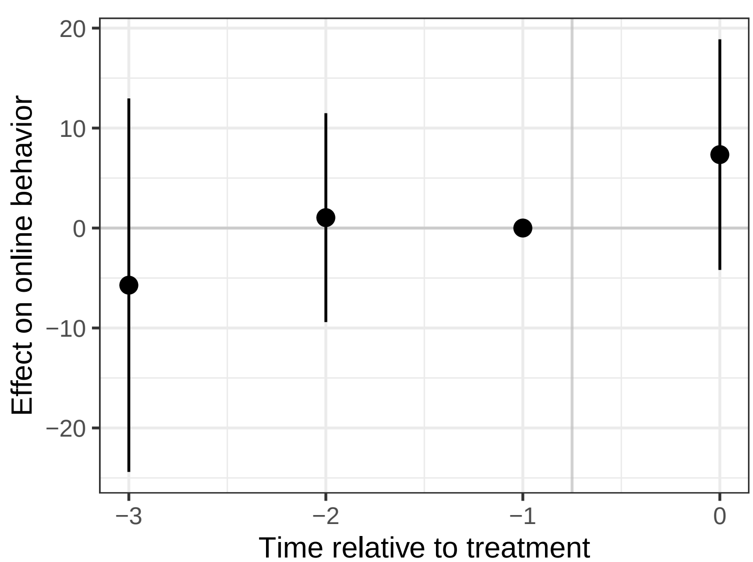
Table 4: The Effect of Broadband on Online News

(a) Regression results

	N access	Minutes	Ratio access	N access	Minutes
Broadband	8.47*	46.33	0.00	10.59**	69.85
	(3.78)	(30.60)	(0.00)	(3.72)	(39.01)
Broadband ×				−2.44	−20.48
Below median news reader				(4.74)	(48.80)
Broadband ×				−2.97	−36.07
Above median news reader				(6.97)	(69.09)
Household size	1.18	0.06	0.00	1.19	0.05
	(1.82)	(16.96)	(0.00)	(1.82)	(16.97)
Children indicator	2.64	15.69	0.00	2.67	16.08
	(4.94)	(35.46)	(0.00)	(4.94)	(35.57)
N	24.564	24.564	24.564	36.185	36.185
Household FE	✓	✓	✓	✓	✓
Year × City FE	✓	✓	✓	✓	✓

Note: "Broadband" is an indicator for whether the respondent reports subscribing to broadband. The table presents regression results where the dependent variables are: (1) the number of times a machine accessed a online news domain (Columns 1 and 4), (2) the total number of minutes spent on political news domains (Columns 2 and 5), and (3) the percentage of total accesses directed to political news domains (Column 3). All outcomes are measured on a yearly basis. An observation is a household–year. Standard errors are clustered at the household level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

(b) Dynamic Effect of Broadband Subscription



Note: This figure plots the estimated treatment effect of subscribing to broadband using a dynamic specification of Table 5, Column 2. Error bars represent 95% confidence intervals.

since news accounts for only a very small share of total activity. Throughout the 2000s, visits to news websites consistently comprised just 0.4–0.8% of online activity, with no discernible trend over time. These results align with Hitt and Tambe (2007), who, using the same Comscore dataset between 2002 and 2004, show that broadband adoption most strongly increased visits to entertainment, sports, and business sites. Taken together, the findings suggest that while broadband may slightly increase exposure to political content online, its overall impact on media diets was modest.

Columns 4 and 5 of Table 4a test for heterogeneous effects in political news browsing. One possibility is that broadband enables a subset of "super-users" to dramatically increase their consumption of political news. The results clearly reject this hypothesis. Splitting households by prior news readership—none, below-median, and above-median visits—I find no meaningful heterogeneity in either visits or time spent.

Finally, to further show that individual broadband subscription is not the causal driver of increased donations, Appendix Table D7 reports that subscription to broadband does not increase traffic to presidential campaign websites or the online payment platform ActBlue. This supports the idea that access to technology by itself does not motivate citizens to contribute, and as I will demonstrate in the next section, campaign activity is a key factor that drives donations.[9]

7 Broadband Strengthens Campaign Advertising over TV

In this final section, I examine how televised campaign advertisements became a more effective tool for raising donations in the broadband era. To estimate the causal effect of campaign ads on donations, I leverage the geographic incongruities between media market (DMA) boundaries and state boundaries. In a winner-take-all Electoral College system, presidential campaigns concentrate resources on swing states, with ad purchases allocated at the media market level. However, because DMA boundaries often spill across state lines, some counties in non-battleground states inadvertently receive a high volume of ads targeted at neighboring battleground areas. These "spillover" counties are not the intended targets of campaign messages, yet they are exposed to them. Urban and Niebler (2014) use this boundary mismatch and propensity score matching to show that pres-

[9]The fixed effects specification also keeps constant the campaigning environment, isolating the effect of broadband subscription.

idential ads increased donations in non-battleground counties during the 2008 election. I extend their analysis to demonstrate that broadband access played a key role in enabling these ad-driven effects.

The basic research design has been widely used to estimate the effects of campaign advertising on persuasion and vote share (Huber and Arceneaux, 2007; Spenkuch and Toniatti, 2018; Sides et al., 2022). I focus on estimating the average treatment effect on the treated (ATT) among ZIP codes in non-battleground states that are part of a battleground DMA. Figure 3 visualizes the the distribution of ads television advertisements in non-battleground states. To estimate the design, I follow the specification developed in Spenkuch and Toniatti (2018) by comparing adjacent counties within the same state, but cross DMA boundaries. The estimating equations are:

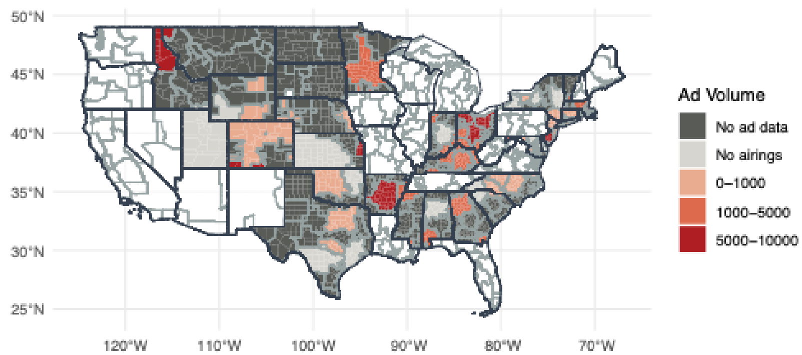
$$y_{zpy} = \tau AdAdvantage_{py} + X\beta_{zpy} + \xi_{y\times p} + \varepsilon_{zpy}$$
$$y_{zpy} = \tau AdAdvantage_{py} \times NBroadband_{zpy} + X\beta_{zpy} + \xi_{y\times p} + \varepsilon_{zpy}$$

The unit of observation is a ZIP code, indexed by year and border pair. The dependent variable is the total dollar amount of donations to presidential candidates from a ZIP code during the general election period. As shown in Appendix E, the results are robust to using alternative outcomes such as the number of donations per capita. The key treatment variable is a binary indicator for whether the ZIP code lies within a battleground DMA that received campaign advertisements during the general election. In many of the specifications, I interact the treatment with the number of broadband providers to estimate conditional average treatment effects on the treated. I include ZIP-level demographic controls in some specifications; while they do not affect point estimates, they reduce standard errors. Standard errors are clustered at the DMA level.

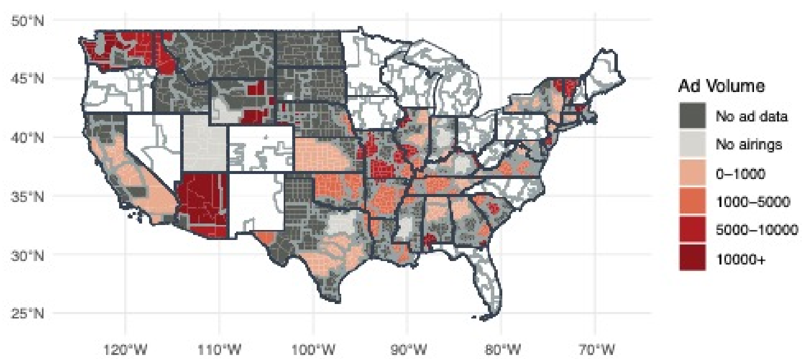
Two aspects of the fixed-effects structure are worth highlighting. First, the border-pair fixed effects allow comparisons between cross-DMA counties within the same state. The specification is "stacked," meaning that each ZIP code can appear multiple times when the county it resides in is adjacent to multiple counties across DMA lines. For example, Nevada was a battleground state in 2008, and the Reno DMA extends into Lassen County, California. To construct credible counterfactual units, I compare Lassen only to neighboring counties like Sierra, Plumas, and Shasta, and not to Los Angeles or San Francisco, which differ along many unobserved dimensions.

Figure 3: Presidential Campaign Ads in Non-Battleground States

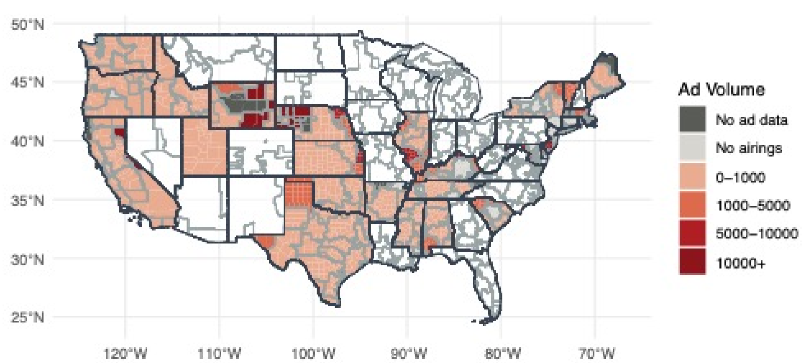
(a) 2000



(b) 2004



(c) 2008



Note: Maps show the number of presidential campaign advertisements in non-battleground states. Battleground states are depicted in white, and media market boundaries are outlined in grey. Data from the Wisconsin Advertising Project and the Cook Political Report.

Second, I interact the border-pair fixed effects with year fixed effects to focus solely on cross-sectional variation. This avoids within-unit comparisons over time, which could be confounded by changing electoral competitiveness. For instance, Washington was a battleground state in 2000, but a non-battleground neighbor to Oregon (a battleground) in 2004. These temporal shifts in status may reflect changing demographic or political conditions rather than arbitrary media market mismatches.

Column 1 of Table 5a presents results from the stacked border design. ZIP codes in non-battleground states that receive presidential campaign advertisements via a battleground media market donate approximately $5,300 more during the general election period than nearby ZIP codes that do not receive such ads.[10] Substantively, this effect is large. On average, ZIP codes located near a battleground media market donate $11,147, compared to $5,540 for adjacent ZIP codes on the other side of the border. In other words, exposure to presidential television advertisements doubles the amount of money raised from a ZIP code on average. Moreover, television proved effective at mobilizing new donors: Column 2 of Table 5a shows that ads increased contributions from first-time donors, accounting for nearly one-third of the total effect. Recall, broadband expansion generated fewer new donors—less than 10% of the increase. This distinction is important, since the share of contributions from first-time donors was already declining over this period. Overall, the impact of TV ads is striking from a historical context: the implied effect size suggests that television ads account for 43% of all donations from areas where presidential campaign advertisements aired.

I show four pieces of additional evidence consistent with the idea that this significant impact would not have arisen without internet technology. As a first test, Table 5b adds an interaction between campaign ad exposure and the number of broadband providers in the ZIP code. Across all models, the interaction terms are both large and statistically significant. By contrast, the coefficient on the lower-order term, the effect of ads in areas with zero broadband, is negative. This suggests that the returns to television advertisements are concentrated in areas with broadband internet access.[11] Appendix E reports results from a specification in which broadband access is binned

[10]Using an expanded sample and different estimation strategy, I obtain estimates consistent with results from Urban and Niebler (2014), who find effects between $6,117 (SE = 1,771) and $7,213 (SE = 1,436), depending on specification.

[11]This negative baseline estimate should be interpreted with caution. ZIP codes with zero broadband providers represent roughly 17% of the sample and are disproportionately observed in the year 2000. It is likely that this

Table 5: The Effect of Presidential Ads on Donations

(a) Effect of Advertisements

	Sum dollars (All donations)	Sum dollars (New donations)	Sum dollars (ActBlue)
Battleground DMA Ads	5341.09*	1735.71*	291.20*
	(2319.90)	(730.56)	(142.38)
N	7.410	7.410	6.174
Pair × Year FE	✓	✓	✓

(b) Effect of Advertisements Conditional on Internet Penetration

	Sum dollars (All donations)	Sum dollars (New donations)	Sum dollars (ActBlue)
Battleground DMA Ads	−16 279.63*	−4503.50*	−1457.52*
	(7316.12)	(2059.04)	(567.07)
N broadband provider	2872.11**	1041.75***	154.40***
	(874.48)	(259.52)	(43.41)
Battleground DMA Ads ×	3574.87**	1022.18**	258.91**
N broadband provider	(1296.42)	(369.42)	(83.85)
N	7.410	7.410	6.174
Pair × Year FE	✓	✓	✓

Note: "Battleground DMA Ads" is an indicator for whether the ZIP code is located within a Designated Media Area (DMA) in a battleground state where presidential television advertisements were aired. The table presents regression results where the dependent variable is the total dollar amount of donations from individuals during the presidential general election cycle. Across both tables, Column 1 includes all donations made in border-county ZIP codes from 2000 to 2008, Column 2 includes all donations made by first-time contributors from 2000 to 2008, and Column 3 is restricted to donations made via ActBlue in 2004 and 2008. An observation is a ZIP code–border pair-year. Standard errors are clustered at the DMA level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test) into terciles. The interaction pattern remains consistent: the effect of campaign advertisements is concentrated in areas with high broadband penetration, while the estimate in low-broadband areas not statistically different from zero.

Second, I show that campaign advertisements increased donations made through the online platform ActBlue. Because ActBlue launched in 2004, I restrict attention to post-2004 samples. Consistent with the broader pattern for total and new donations, ActBlue contributions rise only in places with substantial broadband access. Equally important is the absence of any effect in areas with little or no broadband, which serve as a natural negative control. Since ActBlue donations can only be made online, the null result reinforces the conclusion that the internet was the key mechanism making television ads profitable.

Third, despite the long-standing use of television advertising in American campaigns, I show that its fundraising effect was essentially zero until 2004. The Wisconsin Media Project provides reliable ad-level data only from 2000 onward. To extend the analysis to earlier cycles, I exploit the fact that battleground DMAs are systematically more likely than non-battleground DMAs to receive presidential ads. Without using the actual ad data, I estimate the effect of residing in a non-battleground state county that overlaps with a battleground DMA, using the same regression specification as before, but interacting treatment indicators with year dummies. Figure 4 panel (a) plots these year-by-year estimates in orange.

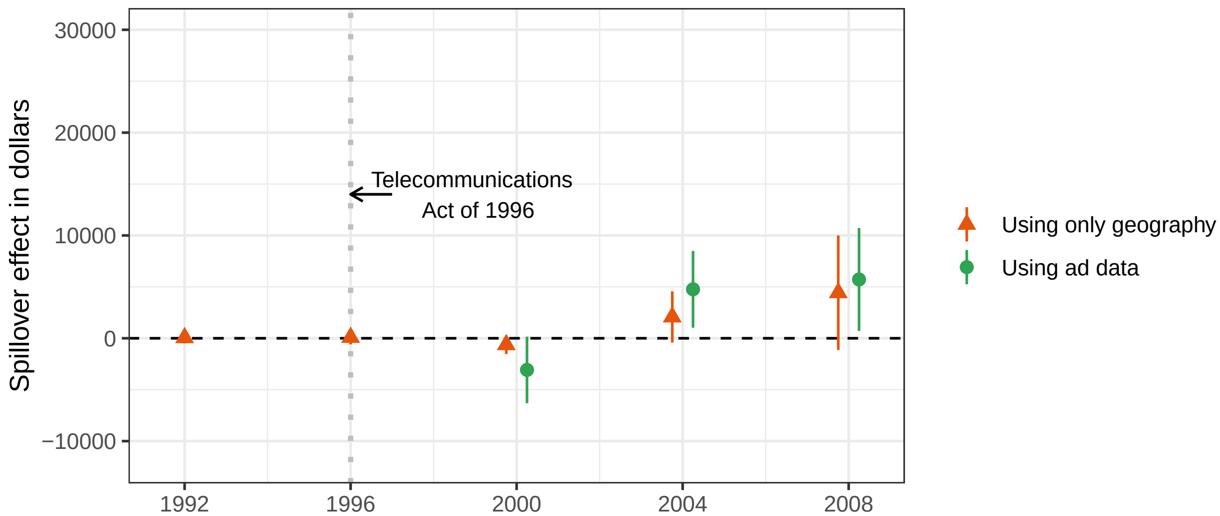
The results indicate that exposure to battleground DMAs had virtually no effect until 2004, emphasizing the idea that television by itself does not cause more donations from viewers. These estimates, based only on DMA geography, are admittedly noisy. For example, the year-to-year differences (ex. 2000 vs 2004) are not statistically significant. Still, two validation checks suggest the pattern is meaningful. First, the 2008 point estimate ($4,430) aligns closely with the magnitude found in Table 5a. Second, when restricting to 2000–2008 and incorporating actual ad data, the yearly trends mirror those of the geography-only specification and achieve statistical significance. These estimates are plotted in green points in the same figure.

This timing of the growth of the TV effect, aligns with important shifts in campaign-side online infrastructure. For instance, Druckman et al. (2009) show that between 2002 and 2004 congressional candidate websites evolved from static brochures to dynamic tools with moving content,

estimate reflects linear extrapolation from a subset of the data.

Figure 4: Heterogeneity in Ad Effect Over Time

(a) Effect Pre-Broadband



(b) Effect Across Levels of Broadband

Note: This figure presents results from three regression models. Triangle points indicate the estimated treatment effect of being located in a battleground state DMA over time. Circle points report the same effect using actual advertisement data from the Wisconsin Advertising Project. The lower panel show heterogeneity in the green, circular estimates in the first panel by distinguishing high-broadband areas. The full regression specification can be found in Appendix Table E9. All regressions includes controls for income, total population, percentage of white population, and percentage of children. Standard errorrs are clustered at the DMA level. Error bars represent 95% confidence intervals.

targeted messaging, and external links. The 2004 cycle also saw the launch of ActBlue, which centralized and streamlined online giving for Democratic candidates. Together, these developments lowered the logistical costs of contributing, making it easier for voters to act on televised appeals. This strengthen the argument that the internet's contribution to campaign fundraising came not only from increased household access, but also from the growing sophistication of campaign-side technology.

Finally, I show that over time, the effect of presidential ads grew only in areas with high broadband provision. I estimate this using a triple interaction between television advertisements, year, and broadband provision terciles, capturing returns to TV ads by year and broadband level. Again, areas with little or no broadband provision serve as a negative control: if the internet is a necessary mechanism, donations should not increase where access is absent. Panel (b) confirms this expectation, showing that the effect of campaign ads on donations rose sharply after 2004, but only in high-broadband areas.

8 Conclusion

One of the puzzles motivating this study was the apparent disagreement over the internet's impact on campaign donations: was it large or small? I resolve this tension by showing that previous work on the internet each capture part of the truth, but none quite tell the full story. Broadband internet access leads to modest increases in donations, concentrated among existing contributors, consistent with similarly small rises in political interest or online news consumption. Its more consequential effect, however, lies in reducing logistical frictions for donors and amplifying the effectiveness of traditional campaign outreach. The internet did not replace legacy media; instead, it enabled individuals to respond more readily to campaign appeals delivered through established channels and made television a new engine for campaign fundraising. Understanding this interplay between digital infrastructure and offline mobilization is central to assessing the internet's role in political participation.

These findings have broader implications for technology and political engagement. Campaign donations remain a relatively rare form of participation, yet their prevalence has grown dramatically in the 21st century. Brady et al. (1995) identify three barriers to civic engagement: individuals can't, don't want to, or aren't asked. My research suggests that the internet reduced barriers to the first, likely did not alter the second, and amplified the third by making offline campaign outreach more effective.

The analysis underscores the importance of considering media interactions. Rather than operating in isolation, different channels often enhance or constrain each other's influence. In this case, the internet magnified the fundraising potential of television ads by creating a more immediate channel for viewers to take action. This insight suggests that evaluating the political effects of new communication technologies requires attention to the broader information environment and the interactions among technologies.

The results further suggest that the internet may have changed the incentives behind televised campaign advertising. Using a simple maximization problem and estimates from 2008 data, Urban and Niebler (2014) argue that campaigns gain revenue by airing more ads in large media markets. My evidence indicates the campaign ads in the broadband era erved a dual purpose: to persuade voters and to raise money.

More broadly, the findings have potential implications for political inequality and polarization. Individual contributions are lead by more ideologically active donors, shaping candidate selection and electoral outcomes (Barber, 2016; Kilborn and Vishwanath, 2021; Yorgason, 2025). By lowering barriers to giving and amplifying the reach of digital fundraising tools, the internet may have helped campaigns more effectively mobilize an engaged donor base, contributing indirectly to the replacement of moderate representatives with more ideologically extreme ones (McCarty, 2019).

Several limitations should be acknowledged. First, while I measure changes in online behavior after broadband subscription, I do not observe broader shifts in the overall information environment, such as how internet access might influence what content individuals consume on television, radio, or in print. That said, broadband adoption substantially increases time spent online, and if we assume individuals have relatively fixed daily information budgets, any unmeasured spillovers may be limited. Second, this analysis excludes small donations that fall below the $200 itemization threshold, meaning that the behavior of a group of small-dollar donors remains unobserved. Finally, these findings are specific to the early period of broadband diffusion, from 1996 to 2008. Since then, the internet has changed dramatically, with the rise of smartphones, social media platforms, and algorithmic content delivery, potentially complicating the mechanisms identified in this paper. Nonetheless, the broader insight remains: digital infrastructure functions both as a media channel and as an enabler of participation. Understanding how technologies interact with existing communication channels will be essential for studying the future of political engagement, including emerging platforms and decentralized applications.

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Appendix

A Construction of control variables

I use four main data sources to construct ZIP code level covariates. These data sources are summarized below:

Table A1: Summary of Data Sources

Source	**Data**	**Format/Unit**
Harvard geospatial library	2000 ZCTA boundaries	Polygon shape file
National Cancer Institute	Demographic variables	County-by-year
Historical Settlement Data Compilation for the United States	Land-use variables	Raster files by year
LandScan Global (2000)	Population distribution	Raster file

The goal is to estimate various demographic and geographic variables at the ZIP Code Tabulation Area (ZCTA) level, even when the original data is available at a coarser geographic unit such as counties. To do this, I first disaggregate county-level variables by redistributing them across space according to the 2000 population distribution from the LandScan Global dataset. This step assumes that each covariate follows the spatial distribution of population within a county.

Next, I convert these disaggregated values into raster files representing demographic or land use characteristics. I then re-aggregate these raster layers to the ZCTA level by overlaying ZCTA shapefiles and summing values proportionally based on spatial overlap. All spatial processing is conducted in R.

B Robustness of main results

In this section, I assess the robustness of the main results presented in Table 2a.

Table B2 reports estimates of the relationship between broadband provider growth and political contributions across several alternative specifications. Columns 1 and 2 show results from a levels regression with ZIP code and year fixed effects. These estimates are approximately twice as large as those in the main specification, likely reflecting long-term, unadjusted trends. Columns 3 and 4 present results using a first-difference specification without ZIP code fixed effects. These are slightly larger than my baseline estimates, but remain broadly consistent. Column 5 re-estimates the main specification, but switches the outcome to total dollar amounts raised per ZIP code rather than donation counts. The results remain positive and statistically significant.

Next, I illustrate the intuition behind the placebo regressions in 2b. I begin by residualizing both the treatment variable (the number of new broadband providers) and the outcome variable (the change in the number of donations) with respect to the control variables and fixed effects specified in 2a. I then partition the residualized treatment variable into 200 bins, compute the mean of the residualized outcome within each bin, and plot these averages. Panel (a) of B1 shows the resulting relationship, which displays a positive linear association. In the remaining panels, I repeat the exercise using lagged outcomes. Here the relationship is flat, consistent with the absence of pretrends. 2b in the main text extends this exercise by plotting these correlations over time for multiple lagged outcomes.

Figure B2 visualizes the results from placebo regressions similar to those in Figure 2b, using alternative specifications. Panel A shows the pre-treatment coefficients from a levels two-way fixed effects model, which reveals a clear and significant upward pre-trend—explaining the inflated effect sizes in Columns 1 and 2 of Table B2. Panel B shows the same placebo test using a simple first-differences specification (without ZIP fixed effects). While pre-trends are generally attenuated, several pre-treatment periods still exhibit statistically significant effects, highlighting the importance of controlling for unit-specific trends in the final specification.

To better understand treatment assignment patterns, Table B3 reports regressions of changes in broadband providers on a set of observable covariates. Column 1 shows results after residualizing only unit and year fixed effects. Column 2 adds controls for average income and the baseline number of broadband providers, in line with the main specification. Including these two key covariates substantially improves covariate balance across treatment conditions. However, some observables, such as total population, the proportion of children, and the proportion of seniors in a ZIP code, still positively predict changes in broadband provision.

To account for these remaining imbalances, Table B5 interacts these covariates with year dummies, allowing for flexible time trends by observable characteristics. Across all model configurations, the estimated effect of broadband on donations remains positive and statistically significant, suggesting that the results are not driven by residual confounding along these dimensions.

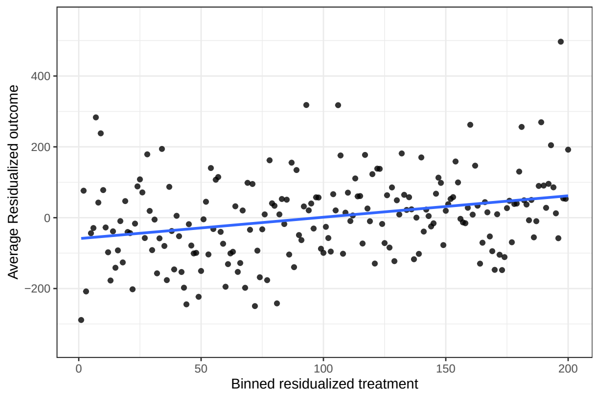
Table B2: Alternative Specifications of Broadband Effect

	N donations (levels)	N donations (levels)	N donations (1st diff)	N donations (1st diff)	Sum dollars (1st diff)
N new providers	64.23***	83.16***	32.23***	27.92+	5140.68+
	(9.35)	(21.13)	(7.71)	(16.67)	(2857.53)
Personal income	3012.71***	2261.34***	460.10***	456.89***	42324.91**
Baseline N providers	258.80***	303.19***	49.11***	72.42***	6870.85***
	(13.83)	(30.25)	(6.28)	(12.58)	(1699.02)
	(137.25)	(295.16)	(18.45)	(38.13)	(16429.71)
N	136,388	136,388	136,388	136,388	136,388
ZIP FE	✓	✓			✓
Year × State FE	✓		✓		✓
Year × City FE		✓		✓	

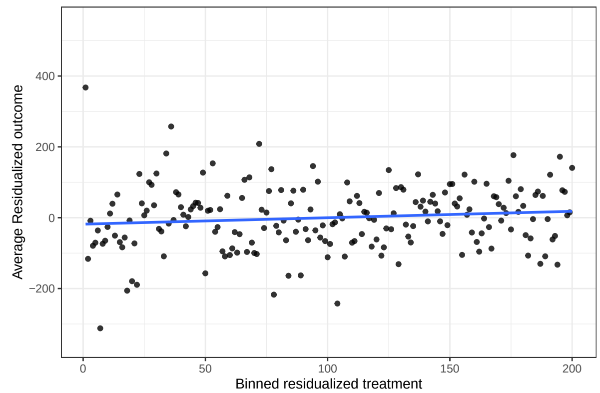
Note: The table presents regression results where the dependent variables are: the number of donations per 100,000 population (Columns 1–2), the change in the number of donations per 100,000 population (Columns 3–4), and the change in the total dollar amount of donations (Column 5). An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

Figure B1: Binned residualized scatterplot of broadband and contributions

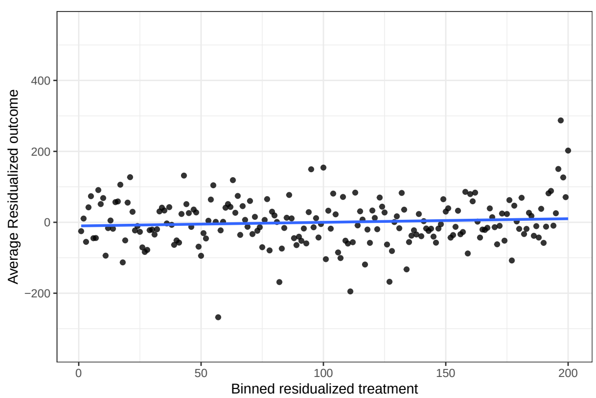
(a) Contemporaneous effect



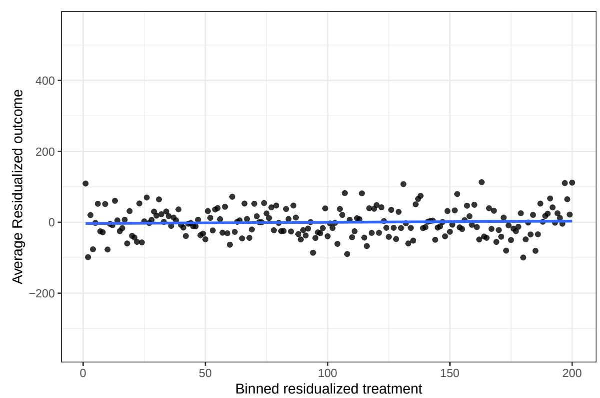
(b) 1st lag



(c) 3rd lag

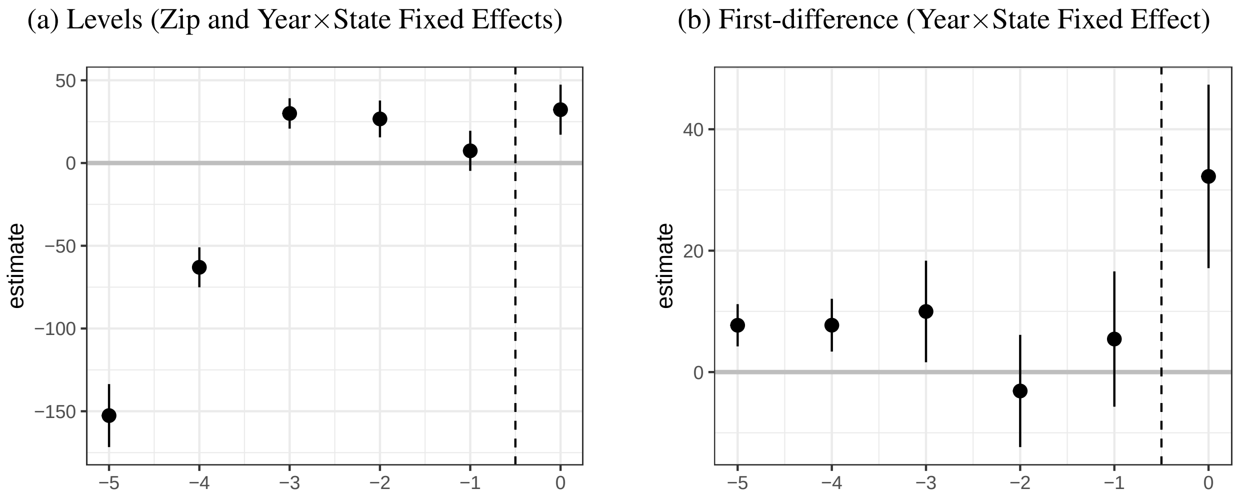


(d) 5th lag



Note: This figure plots the average outcome (contributions) along the 200 bins of the treatment (broadband). These averages are taken after residualizing fixed effects, following the specification of Table 2a, column 1.

Figure B2: Placebo Pre-trends Test Using Alternative Specifications



Note: This figure plots coefficients from regressions of lagged values of the dependent variable on contemporaneous growth in the number of broadband providers. Panel A follows the specification in Table B2, Column 1. Panel B follows the specification in Table B2, Column 3. Error bars represent 95% confidence intervals.

Table B3: Balance on Covariates

	Change in Providers	Change in Providers
Personal income	0.17***	
	(0.01)	
Baseline N providers	−0.10***	
	(0.00)	
Density	0.00***	0.00
	(0.00)	(0.00)
Population	0.00***	0.00***
	(0.00)	(0.00)
%White	−0.44***	−0.17
	(0.09)	(0.09)
%Black	−0.22*	−0.13
	(0.09)	(0.09)
%Chidren	−1.07***	0.83***
	(0.20)	(0.18)
%Senior	−0.44**	0.36*
	(0.16)	(0.15)
Agriculture land	0.39	−0.12
	(0.27)	(0.26)
Commercial land	0.34***	0.04
	(0.07)	(0.06)
Industrial land	0.05	−0.12
	(0.13)	(0.12)
Recreational land	0.34	0.29
	(0.88)	(0.84)
%Rented	0.00	−0.17
	(0.10)	(0.10)
%Homeowner	−0.08***	0.00
	(0.02)	(0.02)
After residualizing...		
Fixed effects	✓	✓
Personal Income		✓
Baseline N provider		✓

Note: The table presents balance tests from regressions of the change in the number of broadband providers on a set of covariates. Column 1 residualizes unit and time fixed effects prior to estimation. Column 2 additionally residualizes income and the baseline number of broadband providers. An observation is a ZIP code–year. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

Table B4: Regressions with Covariates Interacted with Time Trends

	(1)	(2)	(3)	(4)
N new providers	29.91***	63.97***	24.43**	55.94***
	(8.39)	(9.95)	(8.60)	(10.08)
Personal income	642.83***	680.98***	691.62***	601.04***
	(59.41)	(58.62)	(59.23)	(61.20)
Baseline N providers	36.50***	132.61***	31.06***	112.94***
	(6.52)	(10.23)	(6.94)	(10.27)
N	136,381	136,388	136,381	136,374
ZIP FE	✓	✓	✓	✓
Year × State FE	✓	✓	✓	✓
Year × Baseline children level FE	✓			✓
Year × Baseline population level FE		✓		✓
Year × Baseline senior level FE			✓	✓

Note: The table presents regression results where the dependent variables is the change in the number of donations per 100,000 population. The basic specification follows that in Table 2a, Column 1, but allows for separate time trends based on binned baseline levels of total population, percentage of children, and percentage of seniors. An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

Table B5: Regressions with Lagged Dependent Variables

	(1)	(2)	(3)
N new providers	32.88***	32.71***	31.51***
	(7.69)	(7.71)	(8.15)
Baseline N providers	63.19***	71.08***	85.75***
	(7.79)	(8.89)	(9.97)
Personal income	866.13***	966.02***	962.40***
	(70.04)	(80.71)	(97.43)
Lagged DV (1)	-0.36***	-0.39***	-0.45***
	(0.03)	(0.03)	(0.03)
Lagged DV (2)		-0.10***	-0.20***
		(0.03)	(0.03)
Lagged DV (3)			-0.22***
			(0.03)
N	136,388	136,388	116,904
ZIP FE	X	X	X
Year X State FE	X	X	X

Note: The table presents regression results where the dependent variables is the change in the number of donations per 100,000 population. The basic specification follows that in Table 2a, Column 1, but includes controls for the lagged dependent variable. Each column adds an additional lag, up to three lags. An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

C Effect of broadband on small donations

An important concern of this project is the issue of missing data problem. The Federal Election Commission (FEC) does not require candidates and PACs to itemize contributions from individuals whose total donations over the course of an election cycle fall below $200. Nevertheless, researchers can still glean insights into these unitemized donations in two key ways.

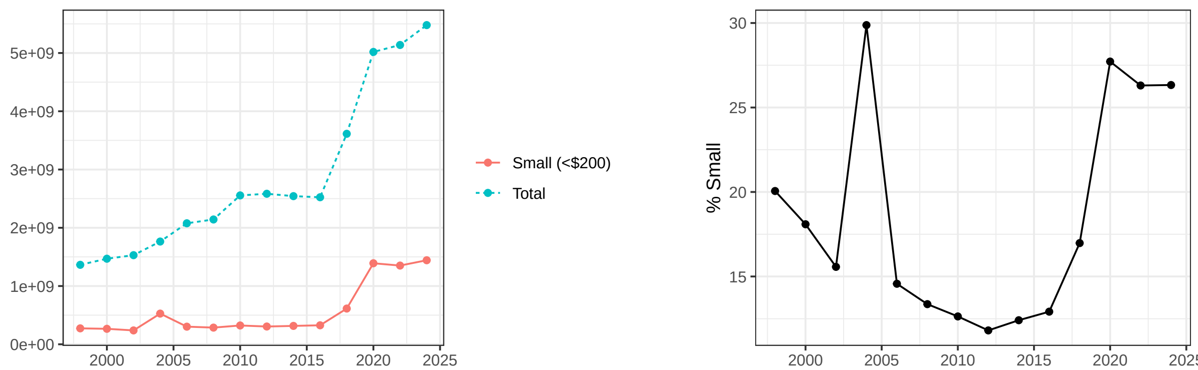
First, while political entities are not obligated to report individual small donations, they are required to disclose total fundraising amounts. As a result, we can infer the aggregate volume of unitemized contributions by subtracting the reported itemized donations from the total raised. Figure C3 visualizes these trends for all congressional candidates from 1998 to 2024, using data from OpenSecrets. The left panel plots total donation amounts over time, while the right panel shows the average share of funds that are unitemized. While there is a marked increase in overall donations in the 2020 election, during the time period covered by this study (1996–2008), the share of unitemized contributions was generally lower and was declining over time.

The second strategy involves examining conduit organizations like ActBlue (and WinRed on the Republican side), which are classified as "conduit PACs" by the FEC. These organizations process earmarked contributions to specific recipients and must disclose all donations, regardless of the individual amount. This means we can observe donations routed through ActBlue, even if they fall below the $200 itemization threshold.

I use these contributions to estimate the effect of broadband using the same specification as in Table 2a. Column 1 reports the results for all donations under $200 that appear in the FEC database. Columns 2 through 4 focus on ActBlue donations specifically. While the count of ActBlue donations is statistically indistinguishable from zero, the total dollar amount increases by $69.50—equivalent to roughly 1.5% of the overall broadband effect. It's worth emphasizing that ActBlue was still in its early stages during the study period. For perspective, in 2004, ActBlue raised roughly $900,000; by 2024, that figure had grown to $3.8 billion—a nearly 5,000-fold increase over two decades (OpenSecrets, 2025).

Figure C3: Trends in Small Donations

(a) Aggregate Contributions from Individuals (b) Average Proportion of Small Donations



Note: Trends in small-dollar fundraising among U.S. House and Senate candidates. The figure includes total unitemized contributions under $200. Data from OpenSecrets.

Table C6: Effect of Broadband on Small Donations

	N donations ($\leq$ \$200)	N donations (ActBlue)	Sum dollars (ActBlue)	N unique donors (ActBlue)
Growth provider	9.48***	−0.81	82.35***	0.24***
	(1.56)	(0.58)	(14.51)	(0.02)
Baseline N provider	31.38***	2.71***	216.37***	1.01***
	(0.90)	(0.56)	(12.23)	(0.03)
Personal income	80.91***	77.83***	2657.83***	6.44***
	(10.92)	(6.99)	(372.83)	(0.62)
N	136,388	136,388	136,388	136,388
ZIP FE	✓	✓	✓	✓
Year × State FE	✓	✓	✓	✓

Note: The table presents regression results where the dependent variables are the change in the number of donations per 100,000 population (Columns 1, 2, and 4) and the change in the total dollar amount of donations (Column 3) for a specific subgroup. Column 1 includes all donations under $200 recorded in FEC data, while Columns 2–4 are restricted to donations made via ActBlue. An observation is a ZIP code–year. Standard errors are clustered at the ZIP code level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

D Effect of broadband subscription on alternative outcomes

Table D7: The Effect of Subscription of Broadband

	N access (Campaign websites + ActBlue)	Minutes (Total time online)
Broadband	−0.08	17394.03***
	(0.12)	(1547.13)
Household size	0.00	961.28
	(0.02)	(1006.58)
Children indicator	0.01	90.04
	(0.07)	(2445.53)
N	270.892	72.625
Household FE	✓	✓
Year × City FE	✓	✓

* p <0.05, ** p <0.01, *** p <0.001

Note: "Broadband" is an indicator for whether the respondent reports subscribing to broadband. The table presents regression results where the dependent variables are: (1) the number of times a machine accessed a presidential campaign website or `actblue.com` (2) the total number of minutes spent online. All outcomes are measured on a yearly basis. An observation is a household–year. Standard errors are clustered at the household level. * p < 0.05, ** p < 0.01, *** p < 0.001 (two-tailed test)

E Robustness of TV ads regression

I present alternative specifications for estimating the effect of television campaign advertisements on political donations in ZIP codes located near DMA (Designated Market Area) borders. Table E8 retain the baseline specification but change the dependent variable to the number of donations per 100,000 residents to facilitate a population-adjusted interpretation. Table E9 modifies the main specification by interacting the integer-valued broadband provider variable with a categorical variable indicating the ZIP code's broadband provision tercile. These terciles are re-computed separately for each year to account for changes in the distribution of broadband availability over time. The estimates in Columns 2 and 3 are used to generate Figure 4.

Table E8: Alternative Specifications of Ad Effect

(a) Effect of Advertisements

	Count per capita (All donations)	Count per capita (New donations)	Count per capita (ActBlue)
Battleground DMA Ads	12.06*	6.29*	0.82
	(5.85)	(3.08)	(37.13)
N	7.410	7.410	4.573
Pair × Year FE	✓	✓	✓

(b) Effect of Advertisements Conditional on Internet Penetration

	Count per capita (All donations)	Count per capita (New donations)	Count per capita (ActBlue)
Battleground DMA Ads	−38.12*	−18.29*	−92.3**
	(17.54)	(8.88)	(32.68)
N broadband provider	9.37***	5.53***	−14.65**
	(1.92)	(1.02)	(5.41)
Battleground DMA Ads ×	8.18*	3.96*	14.86***
N broadband provider	(3.17)	(1.61)	(3.83)
N	7.410	7.410	6.174
Pair × Year FE	✓	✓	✓

Note: "Battleground DMA Ads" is an indicator for whether the ZIP code is located within a Designated Media Area (DMA) in a battleground state where presidential television advertisements were aired. The table presents regression results where the dependent variables are the number of donations per 100,000 population. Across both tables, Column 1 includes all donations made in border-county ZIP codes from 2000 to 2008, Column 2 includes all donations made by first-time contributors from 2000 to 2008, and Column 3 is restricted to donations made via ActBlue in 2004 and 2008. An observation is a ZIP code–border pair-year. Standard errors are clustered at the DMA level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test)

Table E9: Heterogeneity of Presidential Advertisement Effect

	Sum dollars (All donations)	Sum dollars (All donations)	Sum dollars (All donations)
Battleground DMA Ads	517.48	−3075.72	−2344.76**
	(661.60)	(1652.34)	(702.83)
N broadband: Medium	1695.22**		−5948.89*
	(600.48)		(2316.60)
N broadband: High	5424.50*		−17642.76*
	(2305.00)		(7864.86)
Battleground DMA Ads × N broadband: Medium	−1115.15		197.81
	(954.18)		(1184.43)
Battleground DMA Ads × N broadband: High	17401.13***		326.35
	(4275.47)		(4607.80)
Battleground DMA Ads × Year 2004		7837.23**	2973.53*
		(2512.14)	(1185.17)
Battleground DMA Ads × Year 2008		8794.12**	2375.23*
		(3340.53)	(1193.43)
Year 2004 × N broadband: Medium			1059.94
			(1486.81)
Year 2008 × N broadband: Medium			4246.65**
			(1557.52)
Year 2004 × N broadband: High			8786.63**
			(2753.27)
Year 2008 × N broadband: High			12258.81***
			(2714.65)
Battleground DMA Ads × Year 2004 ×			−1278.81
N broadband: Medium			(1776.99)
Battleground DMA Ads × Year 2008 ×			−498.85
N broadband: Medium			(1843.97)
Battleground DMA Ads × Year 2004 ×			13233.69***
N broadband: High			(3408.16)
Battleground DMA Ads × Year 2008 ×			18486.39**
N broadband: High			(6236.97)
N	7.410	7.410	7.410
Demographic controls	✓	✓	✓
Pair × Year FE	✓	✓	✓

Note: "N broadband: Medium" and "N broadband: High" denote the middle and top terciles of the number of broadband providers, calculated by year within ZIP codes in border counties. The dependent variable is the total dollar amount of donations from individuals during the presidential general election cycle. Column (1) interacts the treatment indicator with broadband levels, Column (2) with year dummies, and Column (3) with both broadband and year categories. All regressions control for income, total population, percentage white, and percentage children. An observation is a ZIP code–border pair-year. Standard errors are clustered at the DMA level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed).