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Was Obama Elected by the Internet? Broadband Diffusion and Voters' Behavior in US Presidential Elections

Abstract

What are the political consequences of the diffusion of broadband internet? We address this question by studying the 2008 US presidential election, the first political campaign where the internet played a key role. Drawing on data from the FEC and the FCC, we provide robust evidence that internet penetration in US counties is associated with an increase in turnout, an increase in campaign contributions to the Democrats and an increase in the share of Democratic vote. We then propose an IV strategy to deal with potential endogeneity concerns: we exploit geographic discontinuities along state borders with different right-of-way laws, which constitute the main determinant of the cost of building new infrastructure. IV estimates confirm a positive impact of broadband diffusion on turnout, while the pro-Democratic Party effect of the internet appears to be less robust.

JEL-Codes: D720, L860.

Keywords: internet diffusion, political economy of the media, United States elections, turnout, campaign contributions.

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1 Introduction

According to a survey conducted in February 2016 by the PEW Research Center (Matsa and Lu 2016), the percentage of American adults who often get their news online is 38%. This is second only to TV (57%) and larger than Radio (27%) or print news (20%). Among younger age groups, 18-29 and 30-49, online news consumption actually exceeds TV news consumption. Thus, it is unsurprising that a common theme has emerged in the U.S. press, expressed succinctly by Huffington Post founder Arianna Huffington in the weeks after the 2008 presidential election: "were it not for the internet, Barack Obama would not be president" (Schiffman 2008). Similar claims were made in the wake of the 2016 election crediting the proliferation of online "fake news" for Donald Trump's upset win.[1] At the same time, scholars are debating whether the internet is responsible for the increased ideological polarization of American voters (see for example Sunstein 2001, and Gentzkow and Shapiro 2011).[2]

In this paper we provide evidence of the impact of internet broadband diffusion on US presidential elections, with a special focus on the 2008 election. In many ways this was a landmark election that transformed electoral campaigning strategies. As claimed in the New York Times the day before the election took place, the 2008 presidential campaign "has rewritten the rules on how to reach voters, raise money, organize supporters, manage the news media, track and mold public opinion, and wage — and withstand — political

[1] See for example Parkinson 2016.

[2] The presumption that the diffusion of the internet can have profound political implications is certainly not limited to US politics. The use of internet censorship in countries like China (King et al 2013), for example, must be based on the presumption that free access to some of the content available online can have political consequences. Alec Ross, Hillary Clinton's senior political adviser at the US state department, compared the internet to a "Che Guevara of the XXI century". Speaking about the upsurge of democratic movements in some Arab countries (the so-called "Arab spring"), he said that "dictatorships are now more vulnerable than ever as disaffected citizens organize influential protest movements on Facebook and Twitter" (Halliday 2011). Leading communication scholar Robert McChesney talks of a "communication revolution" providing yet unexplored opportunities for democratic development and social reform (McChesney 2007).

attacks, including many carried by blogs that did not exist four years ago" (Nagourney 2008). According to Mark McKinnon, a senior adviser to George W. Bush's presidential campaigns "we'll be analyzing this election for years as a seminal, transformative race" (Nagourney 2008).

Apart from the inherent importance of the 2008 election in US political history, there are several empirical advantages to focusing on the first decade of the 2000s for understanding the impact of internet on politics. Following similar studies that have focused on the availability of radio (Stromberg 2004 and Adena et al. 2015), television (Gentzkow 2006), or of specific cable channels like Fox News (DellaVigna and Kaplan 2007), it is clear that periods where a new medium is introduced offer empirical advantages in identifying causal relationships. As shown by these papers, in order to identify a treatment and a control group with aggregate observational data it is a necessary (although not sufficient) condition that only part of the population is reached by the new technology. The period between 2004 and 2008 saw the largest growth in broadband penetration in the US between any two presidential elections: hence comparing 2004 with 2008 creates a pre-post difference for a large number of observational units. At the same time, focusing on the 2004-08 period allows us to exploit a large geographic variation in internet access or uptake. In subsequent years the market has become saturated, leaving little variation to exploit for identification purposes (see Horrigan and Duncan 2015). In particular, the availability of internet on mobile phones means that there is now more widespread access opportunity and that therefore broadband uptake is more likely to reflect individual characteristics rather than external constraints. Exogenous constraints on supply (as opposed to variables affecting demand) are key in identifying a causal effect of broadband diffusion rather than simple correlations.

The 2008 race is also the first Presidential election that saw widespread usage of internet as a campaigning tool. The Obama campaign's online fundraising arm brought in a record \$500 million in small individual donations; and the campaign's heavy use of social media purportedly contributed to the highest rate of youth turnout since voting was extended to 18-year-olds.

We test the extent to which these assertions hold, looking at the effect of internet diffusion on campaign contributions, turnout and vote share in the 2008 election. Our analysis is conducted at the county level, for which we assemble a dataset of political outcomes, combining presidential vote data with turnout and a full record of campaign contributions.

A study of this sort poses two formidable obstacles: 1) how to reliably measure internet penetration at a sufficiently detailed level; 2) endogeneity of internet penetration could lead to correlations with the variables of interest, which do not accurately reflect causation.

Our first contribution is to propose a solution for the measurement problem by developing a proxy for internet usage over the 1999-2008 period. Unfortunately until December 2008 there was no publicly available data on internet usage in the U.S. at anything below the state level. We develop a county-level proxy, using data on the number of high speed Internet Service Providers (ISP) that were registered with the Federal Communication Commission (FCC) in a zip code. We test this proxy against state-level measures of the number of residential subscriptions and find a high correlation. We also show that the number of high speed ISPs and the number of broadband residential lines are highly correlated at the county level in 2008 (i.e. the first year when both are available at the county level) and 2012.

Armed with this proxy for internet penetration we report differences-in-differences results using county-level changes in our outcomes of interest between 2004 and 2008.[3] This methodology, however, can only partially address the problem of endogeneity in internet penetration. Ideally we would like to observe and compare counties which are

[3] We also report results for the period 2000-2004.

equal in all relevant characteristics, but that happen to have different internet uptake for reasons that do not depend on voters' characteristics. For this purpose it is necessary to separate demand and supply of internet services and find instances of exogenous shifts in the supply curve. To accomplish this task we exploit geographic discontinuities along state borders with different right-of-way laws. We argue that right-of-ways laws provide an exogenous source of variation in the cost of building internet infrastructure, hence exogenously affecting broadband penetration in the US counties.

We compare counties on either side of state borders, whose unobserved characteristics should be spatially correlated. By taking the difference between contiguous counties on either side of the border and instrumenting by right-of-way laws, our methodology controls for unobserved characteristics, providing us with estimates of the causal effect of broadband internet growth on the outcomes of interest.

Our findings are mixed. We find a strong effect of internet access on share of the vote going to the Democratic candidate in our OLS estimates but this effect vanishes when we use IV, suggesting the presence of a co-movement between broadband penetration and Democratic vote which is not causal. We find instead that the internet had a positive effect on turnout in 2008. This effect is robust in both OLS and IV estimates and holds across a number of specifications. We also find some evidence of an effect on campaign contributions for the Democratic candidate, but not robust to our IV strategy.

2 Related literature

This study makes a number of contributions and can be related to several streams of existing research. First, it provides some of the first quantitative evidence of the role of the internet in U.S. elections. There is only limited quantitative large-N research in this area and very few papers have explored the electoral impact of the internet while credibly addressing causal identification concerns. Falck et al 2014 find that the availability of the internet in Germany had a negative impact on electoral turnout. Campante et al. 2013 also find a sizeable negative impact of broadband diffusion on turnout in Italy. They also find, however, that the initial negative impact was reversed in 2013 when the anti-establishment Five Star Movement ran in the general election. Gavazza et al. 2017 find that internet penetration reduced turnout in the United Kingdom. They also show that broadband diffusion is correlated with various local government policies concerning taxation and expenditures. Miner 2015, on the other hand, finds that internet penetration in Malaysia increased turnout and had a general anti-incumbency effect. Positive effects on turnout have also been found by Poy and Shuller 2016 in their study of the Trento province in Italy. Other papers have related internet diffusion with an increase in social capital in Germany (Bauernschuster et al. 2014), reduced corruption in the US (Andersen et al. 2011), and have shown that the usage of social media can increase accountability when traditional media are not independent (Enikolopov et al. 2016).

In the case of the US, correlations between internet usage and political engagement are well documented, although with mixed results. Tolbert and Mcneal 2003 and Mossberger, Tolbert, and McNeal 2008 report a positive correlation between reading news online and turnout using, respectively, data from the American National Election Study and PEW's Internet and American Life Survey.[4] On the other hand, Bimber 2001, Baumgartner & Morris 2010 and Hargittai & Shaw 2013 find no correlation. Some research on US ideological polarization has been inspired by the seminal work of Sunstein 2001, claiming that the internet favors the creation of echo chambers where people are predominantly exposed to views similar to those they already have, increasing ideological segregation and the polarization of the American electorate. Nie et al. 2010 provide some support for Sunstein's echo chamber hypothesis by showing that people tend to use the internet in a selective fashion, i.e. exposing themselves to similar political views. However, this

[4] See also the meta-analysis in Boulianne 2009, which also finds a positive relationship.

is only a necessary condition for increasing polarization. Gentzkow and Shapiro 2011 use survey data to show that online news consumption is not associated with higher ideological segregation than offline news.

Our paper is the first to offer causal estimates of internet penetration in the US on various political outcomes. Moreover, in this paper we propose an identification strategy that can potentially be broadly applied to study the political and social impact of broadband diffusion.[5] From a methodological point of view our paper relates to a growing literature that employs spatial econometrics to achieve identification. Particularly related is Naidu 2012, which studies 19th century disenfranchisement of African Americans in the U.S. South, comparing adjacent county-pairs on state boundaries. Dube et al. 2010 use the same methodology in an earlier paper to estimate the effects of minimum wages on earnings. Our paper employs a different method in which the cross border comparison of adjacent county-pairs is supplemented by an instrumental variable approach.

3 Background

3.1 Personal Use of Internet

Internet use in the US has grown enormously from 16% in 1996 to 74% in 2008, with the latest percentage available (2015) at 75%.[6] This growth masks an even greater change in the method and sophistication of usage over this period of time. In 1996, AOL was still one of the main methods of accessing the internet, meaning relatively few people left its portal, which was effectively a walled garden of curated and licensed content. Moreover, most connections were dial-up and too slow to display video content. Modern

[5] See for example the paper by Lelkes et al 2017. Following our identification strategy, Lelkes et al. 2017 show that internet diffusion increases the consumption of partisan media and is, via this channel, a likely cause of increased polarization.

[6] Yearly data is available from the World Bank and from the International Telecommunication Union.

social networks did not yet exist. Bill Clinton did have his own web-page during the 1996 campaign, but it was small, with none of the fundraising and organizational tools that we see in modern campaigns.[7] Unsurprisingly, only a small fraction of the voting population used the internet for political ends, 4% according to a PEW study (Smith and Rainie 2008). In contrast, the same study found that, as of 2008, a full 44% of the adult population used the internet as a source of political information, and that it was the primary source of political information for 30% of the population.

Although the stereotype holds that a typical Obama supporter was more wired than a McCain supporter, in practice the opposite is the case: 83% of McCain supporters were internet consumers as opposed to 76% of Obama supporters (Smith and Rainie 2008). However, much of this is explainable by the gap in average income and education between the two groups. Moreover, whereas the average McCain voter might be more connected, the average Obama voter was more likely to use the internet to political ends. Nowhere is this more evident than in the case of young voters aged 18-29, who, at 72%, were the most politically active online of all age groups.[8] They also swung disproportionately to Obama: 66% of the youth vote went to Obama in 2008 as opposed to 54% to Kerry in 2004 (Keeter, Horowitz and Tyson 2008). Moreover, turnout among the youth was the highest on record since voting was extended to 18-year-olds in 1972 (Falcone 2008).

3.2 Campaign Use of the Internet

3.2.1 New Media

The Obama campaign exploited the internet in a number of ways. First, they made ample use of social media to further their cause both through Facebook and the campaign website my.barackobama.com (MyBO). These tools helped augment traditional campaign

[7]See www.4president.us/websites/1996/clintongore1996website.htm

[8]The number drops to 65% for people aged 30-49, 51% for 50-64, and 22% for 65+. For additional details see Smith and Rainie 2008.

tactics: detailed information on supporters helped improve mobilization, especially during caucuses; MyBO supplied tools allowing volunteers to make calls on the campaign's behalf from home; and MyBO and Facebook centered tools also helped volunteers organize their own fundraising events, connecting with friends they hadn't seen for years (Talbot 2008). The Obama campaign's aggressive action in the social media space played out in exit-survey data. According to PEW, 25% of Obama supporters used social networks for political ends as opposed to 16% of McCain supporters (Smith and Rainie 2008).

The Obama campaign also used online video to get their message to a large audience without having to pay the traditional advertising costs of television. For example, a video of Obama's speech on race relations got 6.7 million views by November 2008 (Miller 2008). Again this translated into a gap among supporters: 21% of Obama supporters shared political videos as opposed to 16% of McCain voters (Smith and Rainie 2008).

All of these trends were especially pronounced among young voters aged 18-29, the largest users of all types of social and online media: 67% of young voters reported watching online political videos; and 49% used social networks politically, with 40% posting original online content relating to the campaign (Smith and Rainie 2008).

3.2.2 Fundraising

The area of Obama's online campaign that has received the most attention is fundraising, where the campaign brought in an estimated 500 million dollars in online donations, eclipsing Howard Dean's previous record of 27 million. In fact the technology used for Obama's online fundraising was developed by veterans of the Dean campaign, a company called Blue State Digital. They built a number of tools to make the effort more "social": people could set their own personal targets; run fundraising campaigns; and watch personal thermometers rise, which gauged how well they met their targets.[9] The results in

[9] For more details, see Talbot (2008).

terms of exit polls is dramatic: PEW reports that 15% of Obama voters donated online in contrast to 6% of McCain voters (Smith and Rainie 2008). The results are equally striking in terms of type of donation: of the 6.5 million donations online, 6 million were in increments less than $100 and often from the same person (Vargas 2008).

3.3 Expected Outcomes

Online engagement with politics is only possible if access to internet at a sufficiently high speed is available. Our empirical investigation will therefore provide evidence on the causal effect of broadband diffusion in the US counties on outcomes that, for reasons outlined in the above sections, we expect to have been affected by voters' internet usage. In particular we expect that broadband diffusion should: 1) increase donations for the 2008 Democratic candidate; this increase should be particularly strong among small donations (less than $500 dollars), due to the Obama campaign's use of its online portal to collect a record amount of small-scale donations; 2) increase turnout, in particular among youth, since the Obama campaign employed extensive tools for interacting with and mobilizing voters, and usage of these tools was highest among people aged 18-29; 3) increase support for the Democratic candidate, due to the tools used by the Obama campaign both to convince swing voters (e.g. via video) and mobilize voters in general.

4 Data

4.1 Internet Data

The Federal Communication Commission began collecting detailed information on broadband penetration in December 1999. Figure 1 shows the evolution over time of fixed-location connections in the period 1999-2012. If we take any two consecutive presidential elections in the period 1996-2016 (setting 1996 level at zero), 2004-08 was the period which saw the fastest increase in the number of residential connections, which rose from about 20 to around 75 million in just four years. By 2012 residential fixed connections had only grown by another 10 million.[10] Figure 2, from the PEW research center, uses survey data to show internet access by American adults and leads us to similar conclusions.

It is therefore not by chance that 2008 is the first election to witness a massive usage of internet for presidential electoral campaigning. This in itself, and irrespective of the anecdotal evidence we have reported, justifies our choice of basing our analysis of the political effects of the internet on a comparison between the 2004 and 2008 elections. Unfortunately, until December 2008 the U.S. lacked comprehensive, publicly available data on broadband usage and availability at anything finer than the state level. ISPs don't release private information on subscribers due to its proprietary nature, and survey data is scant. We therefore use an alternative proxy for broadband usage: the number of broadband providers operating in a zip code.

This data was collected by the FCC's Form 477 on a semi-annual basis for the 1999-2010 period from all high-speed providers with more than 250 high-speed lines in a state. A provider is counted as high speed if transfer speed is greater than 200 kilobits per second in at least one direction. The data does not differentiate between cable, DSL, satellite, residential, or business providers. A provider is counted if they have at least one subscriber in a zip code. Figure 3 illustrates on a map the number of ISPs operating

[10] Further details are available from the FCC web page. It should be noted that we are not claiming that broadband has not grown both in quantity and quality since 2008: while fixed-line connections have grown at a reduced pace, mobile connections have become much more diffuse and the connection speed, both upstream and downstream, has improved substantially, especially in some areas. An increased number of mobile connections, however, does not mean that as many people gained access to internet, as instead was the case when residential fixed lines were first installed. According to the International Telecommunication Union, for example, the number of people using internet has barely changed between 2008 and 2015. Moreover, focussing on fixed-location connections and on a period with limited mobile access to the internet gives us another empirical advantage, namely that it's easier to locate users and link observations to specific zip codes, a feature that will prove useful in our analysis.

in each zip code at the beginning of the period we study (June 2004). Our measure is obtained by averaging at the county level the number of ISPs operating in each zip code, where the average is weighted by the population of each zip code.

There are a number of reasons that this is a good proxy for internet take-up. In Table 1 we use available state-level data from FCC to regress the logarithm of high-speed lines per capita on the logarithm of providers for the 48 contiguous states for the period 1999-2008. As can be seen in specification (1), even without controlling for population or state-year fixed effects, the relationship between the log of providers and the log of high-speed lines is positive and highly statistically significant with an R-squared of .54. The strength of this relationship is unaffected by introducing a control for population in specification (2). In specifications (3) and (4) state-year fixed effects are introduced, barely affecting the coefficient on log providers and yielding markedly higher within R-squared values. Specification (3) can be seen graphically in Figure 4, which shows a strong positive relationship apart from three outliers in the bottom right corner. In specifications (5) and (6) we drop these outliers, yielding a far more significant relationship and, in the last case, a within R-squared of .96.

One concern is that this relationship could be far less significant at the county level. Fortunately, as of December 2008, the FCC reports broadband take-up at the county level, so that we can check how our measure correlates with the number of residential lines at the end of the period of study. Figure 5 reports fitted values of a fourth polynomial regression of subscribers on providers in December 2008, showing a robust positive correlation. This graph is based on a coarse description of broadband uptake, since the FCC only reported the data bracketed into 6 categories, from 0 (lowest) to 5 (highest). In 2012 the FCC conducted a more detailed investigation reporting the county-level number of individuals with no access to broadband, from which we can derive the number of individuals with access. Once again, a fourth polynomial regression displays an increasing relationship between the number of providers and the percentage of population with broadband access (Figure 6). The relationship is concave and becomes less precisely estimated at the extremes of the distributions of providers, where the number of observations is small.[11] A kernel density estimate of the distribution of providers in the US counties in 2008 is reported in Figure 7.[12] The evolution of our main explanatory variable between 2000 and 2008 is shown in Figure 8. Summary statistics are reported in Table A1.

Although the relationship between number of providers and internet take-up is significant, there are several factors that introduce measurement error. The main concern is that this measure may only be proxying for the competitiveness of ISP markets in a county. Although this will certainly introduce error, the strong correlation that we find between the number of ISPs and high-speed internet adoption reassures us that there will still be substantial variation related to internet usage. Also, high-speed internet does not include dial-up connections, although this should be only a minor concern in the period we study.[13]

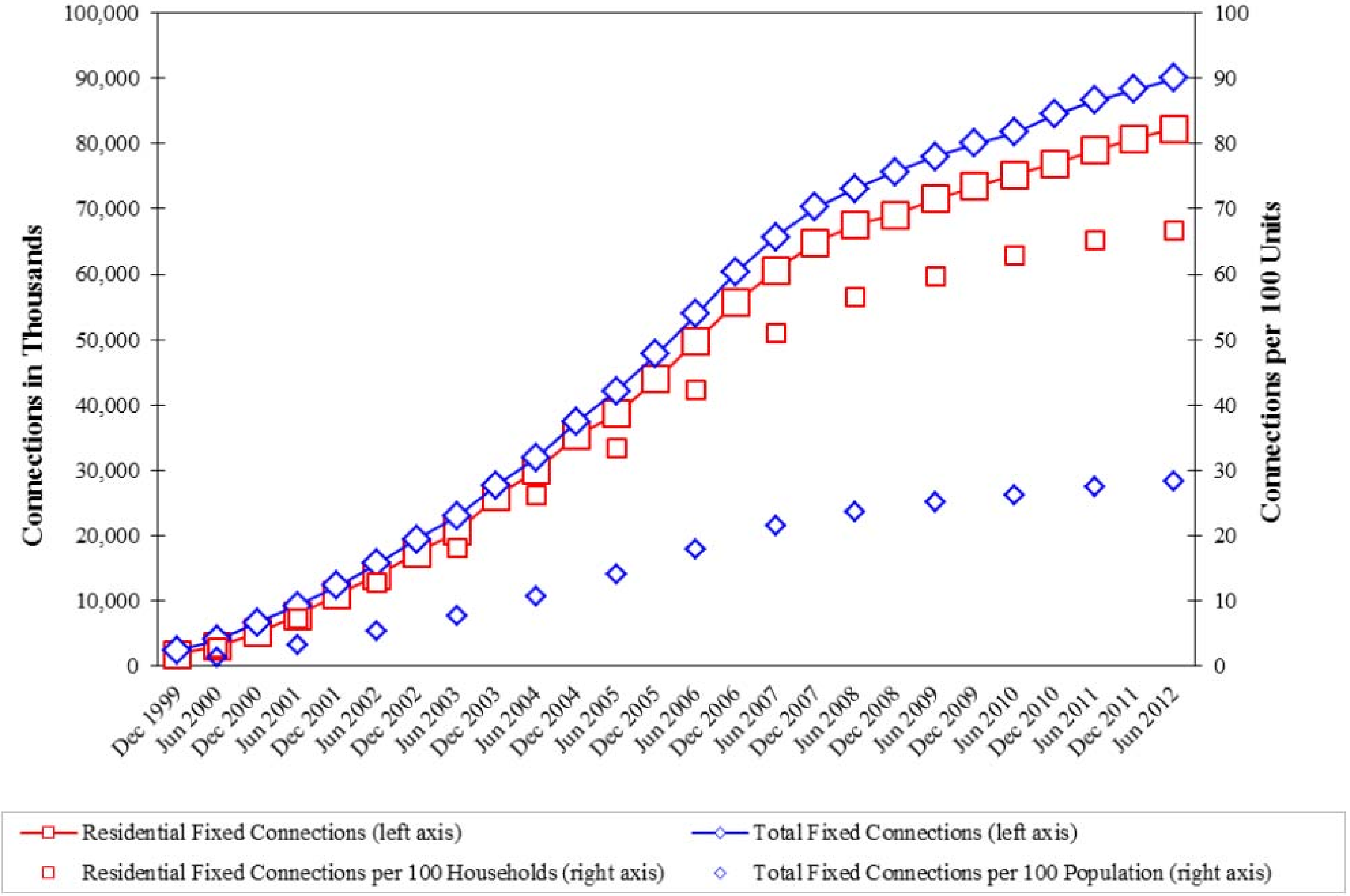
[11]Kolko 2010 analyzes the relationship between high-speed Internet adoption and the number of providers at the zip code level, using survey data from Forrester and also finds a strong positive, monotonic relationship between the number of providers and high-speed internet take-up. The only outliers are at the extremes of the distribution for which there are very few observations.

[12]For confidentiality reasons, the FCC does not report the number of providers in a zip code if this is between 1 and 3. We assume that all these zip codes have two providers. In Figure 7 it is possible o detect a small bump in the distribution which is due to this assumption. It is also clear that this assumption affects the distribution only marginally.

[13]According to survey data of the PEW Research Center, in 2004 about 30% of American adults accessed Internet using dial-up. This share declines to 10% in 2008. Assuming that most of this variation constitutes a switch to broadband, this upgrade is part of what our broadband intake proxy tries to capture. The fact that some 2008 broadband users used dial up in 2004 (rather than not using Internet at all) should, if anything, bias the effects of broadband adoption downwards. There remains a 10% of Internet users which is not captured by our indicator and this also should bias our results downwards. In any event, we don't think this will introduce much error into our results, mostly because, given the speed limitations inherent in dial-up, many of the modern techniques employed by campaigns such as video streaming are not available.

Figure 1

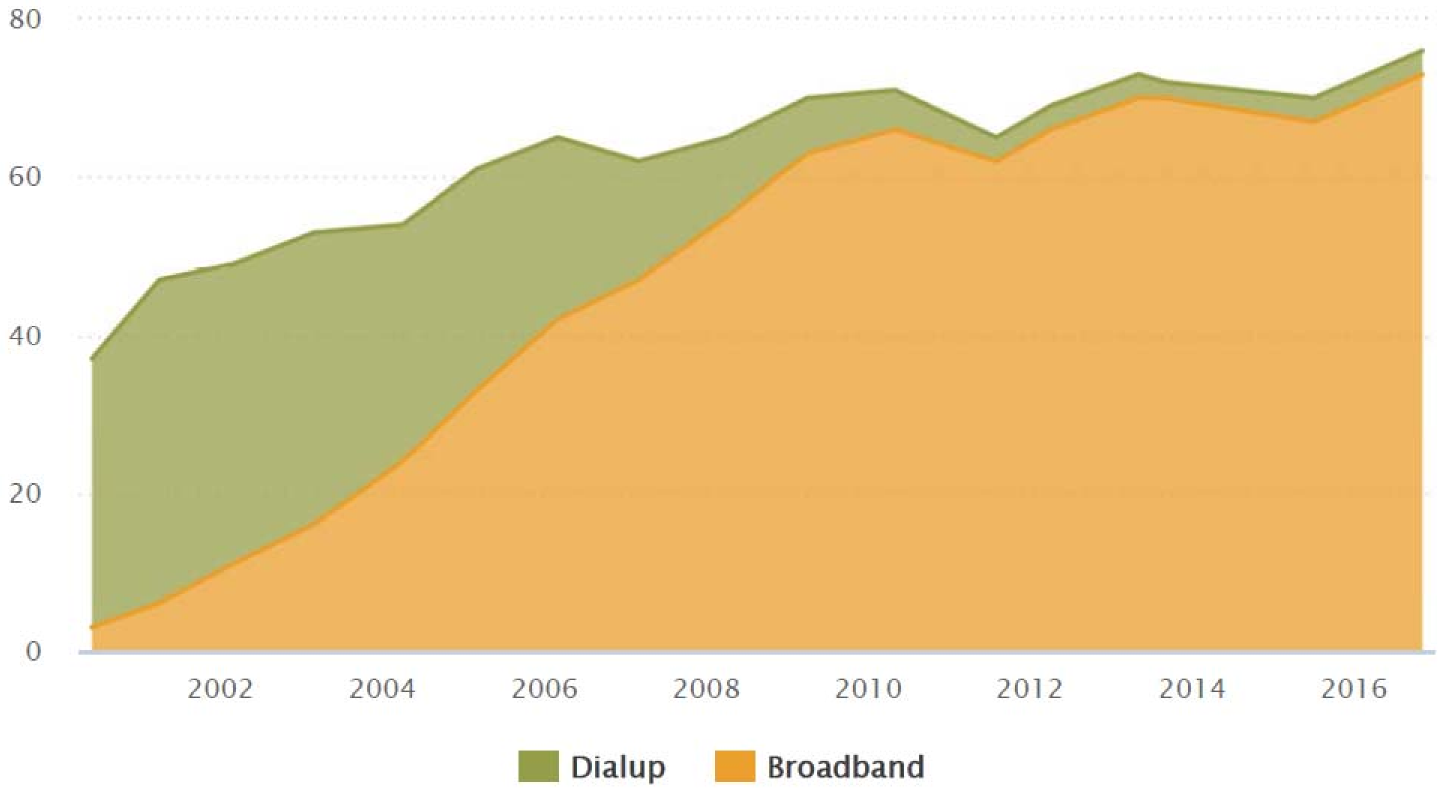
Fixed-Location Connections 1999-2012



Source: Federal Communication Commission (2013)

Figure 2

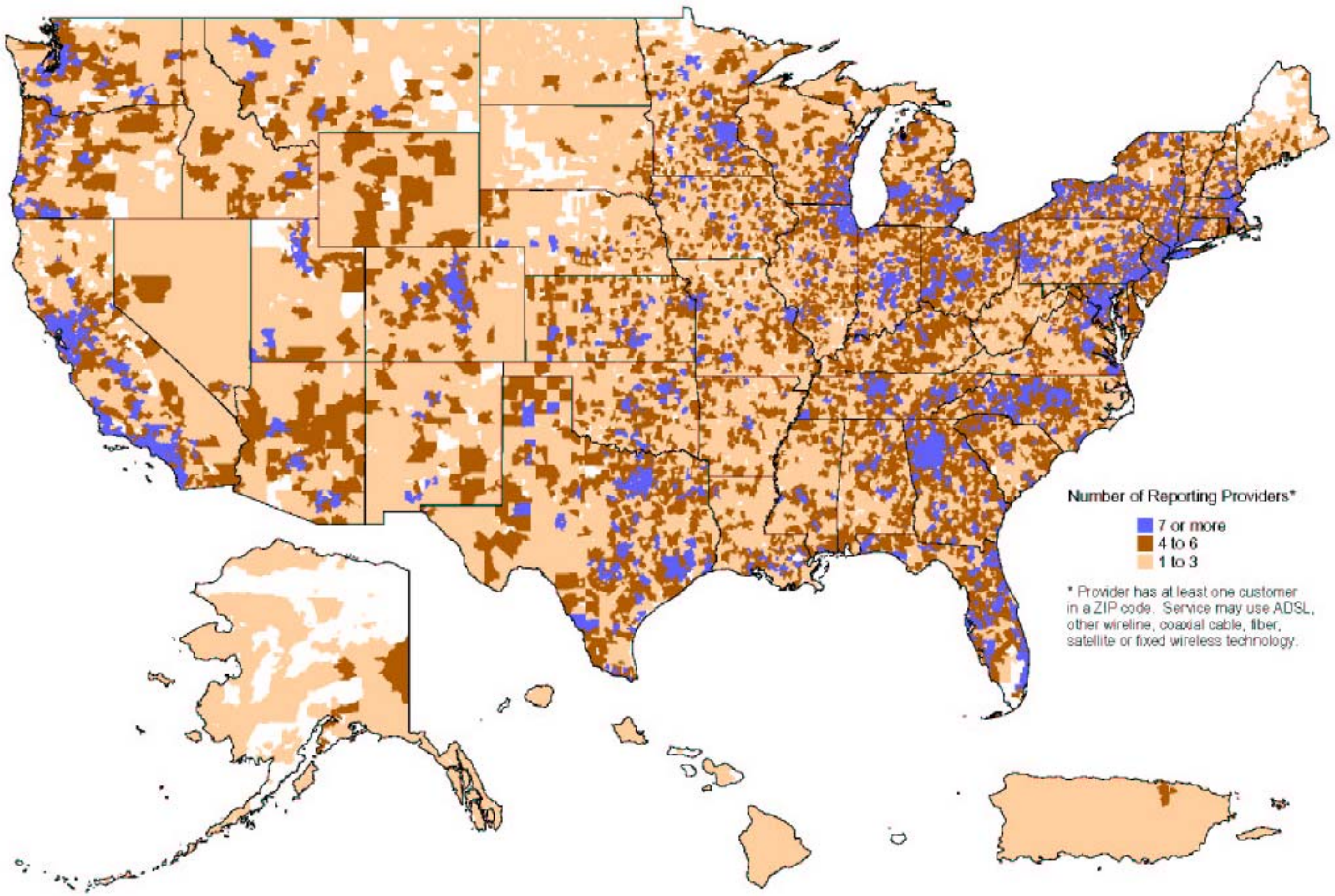
Percent of American adults who access the internet via dial-up or broadband



Source: PEW Research Center (2017)

Figure 3

High-Speed Providers by ZIP Code (as of June 2004)



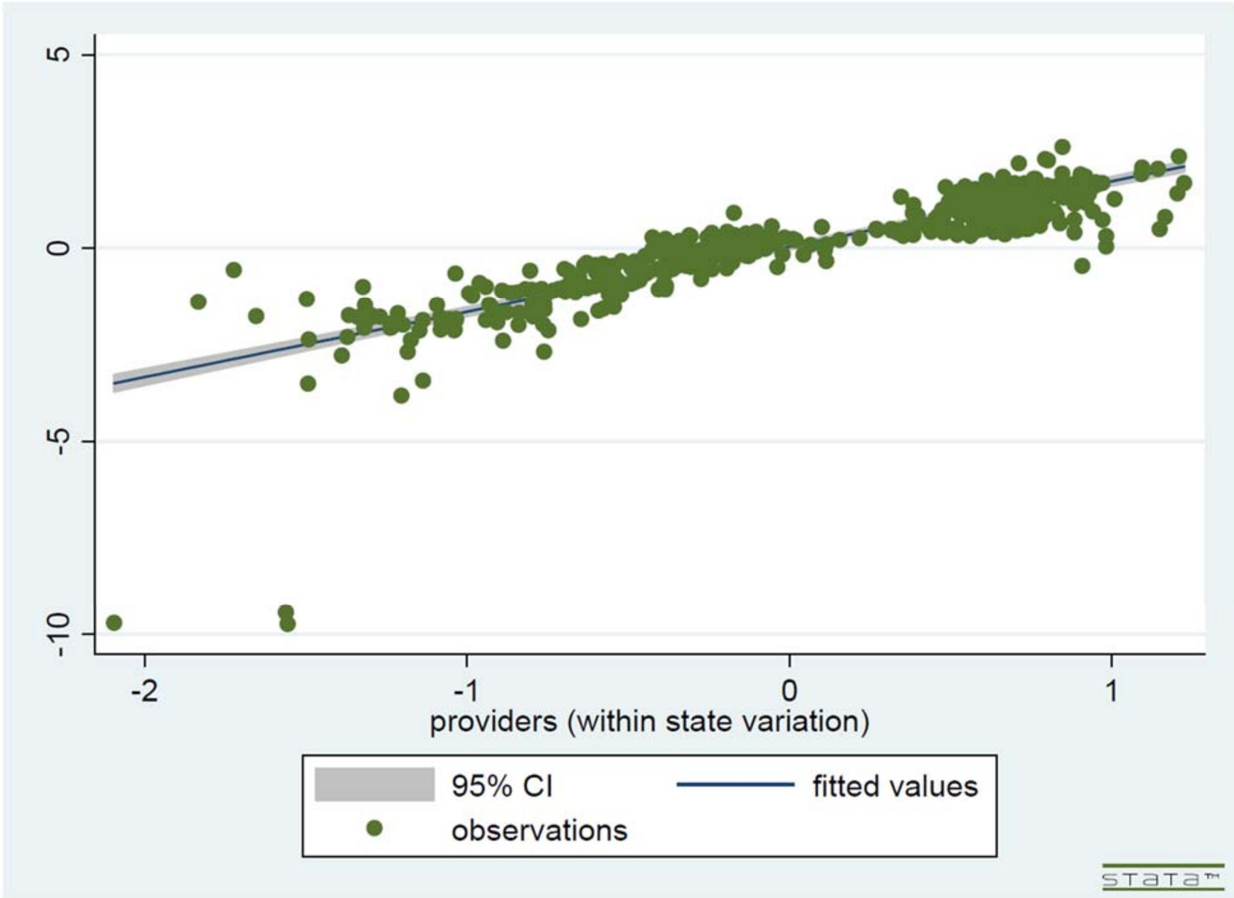
Source: Federal Communication Commission (2004)

Table 1. Correlations between number of providers and number of high-speed lines per capita

dep. variable	log of number of high-speed lines per 1000 residents					
	(1)	(2)	(3)	(4)	(5)	(6)
log of providers	1.2668***	1.3984***	1.3100***	1.2630***	1.0866***	0.2167***
	(0.109)	(0.116)	(0.163)	(0.166)	(0.045)	(0.067)
population		-0.0448***		-0.2207		
		(0.007)		(0.144)		
Constant	-1.9937***	-2.1597***	-3.0215***	-1.6887*	-1.3596***	-0.3973***
	(0.384)	(0.387)	(0.353)	(0.939)	(0.153)	(0.148)
Observations	452	452	452	452	449	449
R-squared	0.5414	0.5754	0.7993(w)	0.8005(w)	0.5613	0.9558(w)
State and year fixed effects	no	no	yes	yes	no	yes

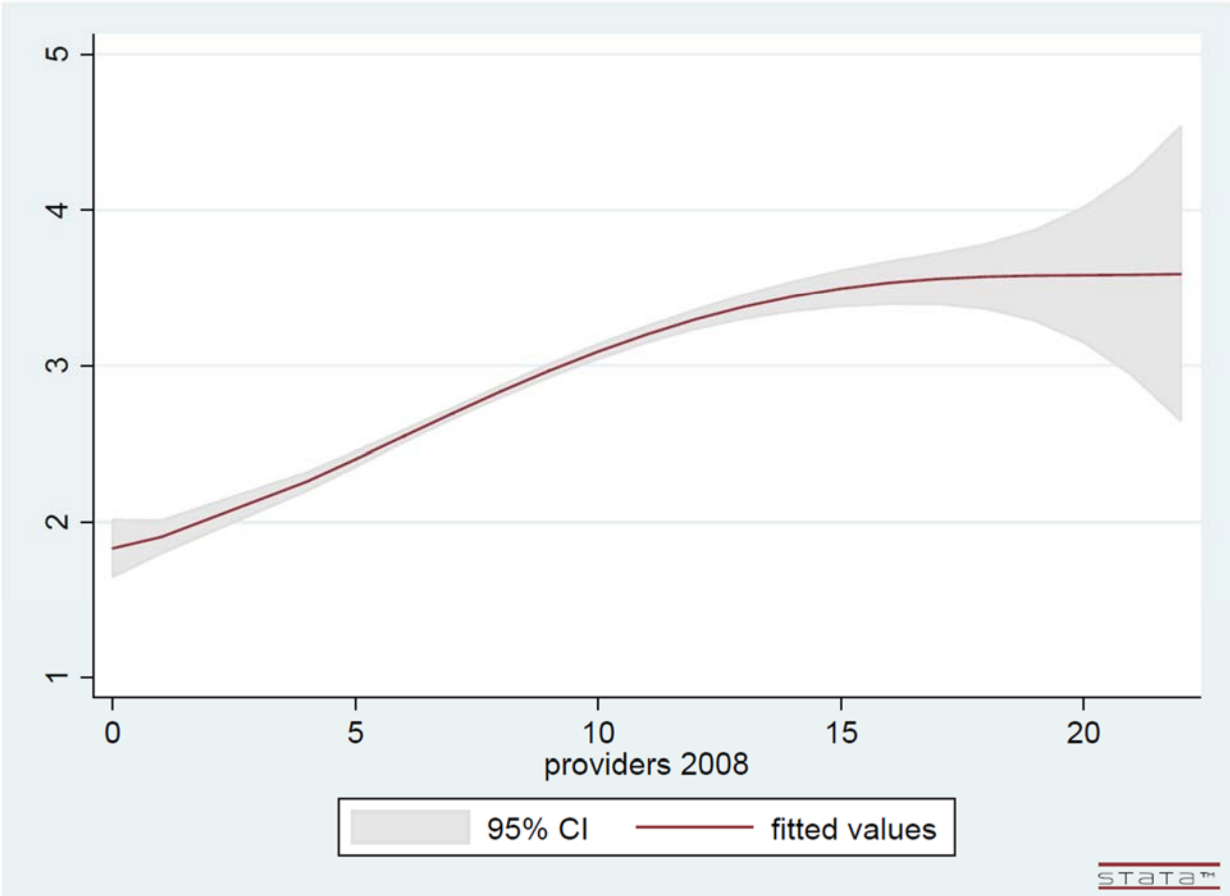
Robust standard errors in parentheses. Columns 3, 4 and 6 report within R-squared indicated with (w).
*** p<0.01, ** p<0.05, * p<0.1

Figure 4
Within-states correlation between broadband adoption and number of providers



The regression uses state-level data. For each state-year there is one observation given by the deviation from the 1999-2008 state mean (observed in June each year). Values are expressed in natural logarithms.

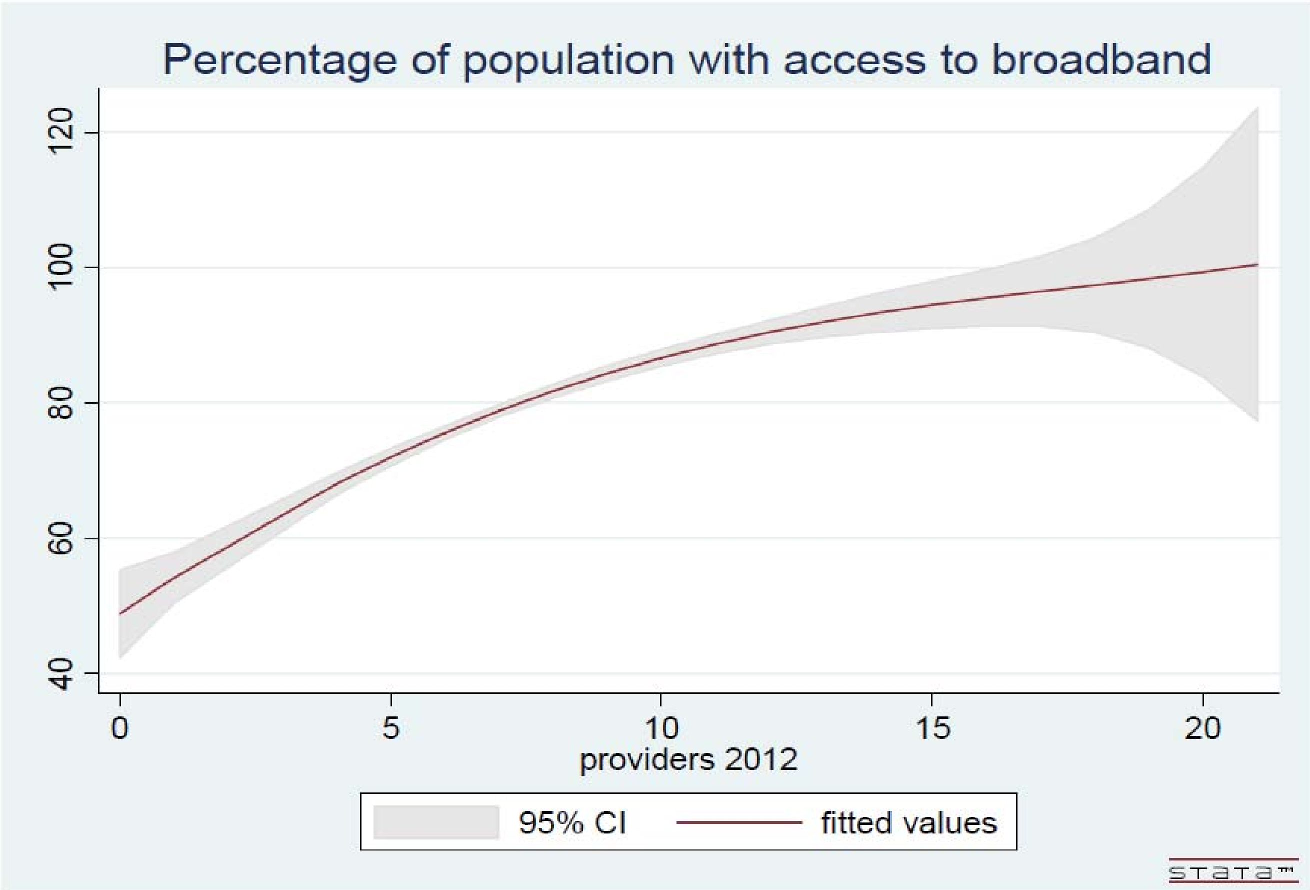
Figure 5
County level correlations between providers and subscribers (December 2008)



The graph reports fitted values of a fourth order polynomial regression of residential subscribers on the number of providers, where the former is provided by the FCC as an aggregate indicator taking values from 0 (low number of subscribers) to 5 (high number of subscribers).

Figure 6

Number of providers and access to broadband (2012)



The graph reports fitted values of a fourth order polynomial regression of the percentage of the population with access to broadband on the number of providers (in June 2012).

Figure 7

Distribution of providers by counties (2008)

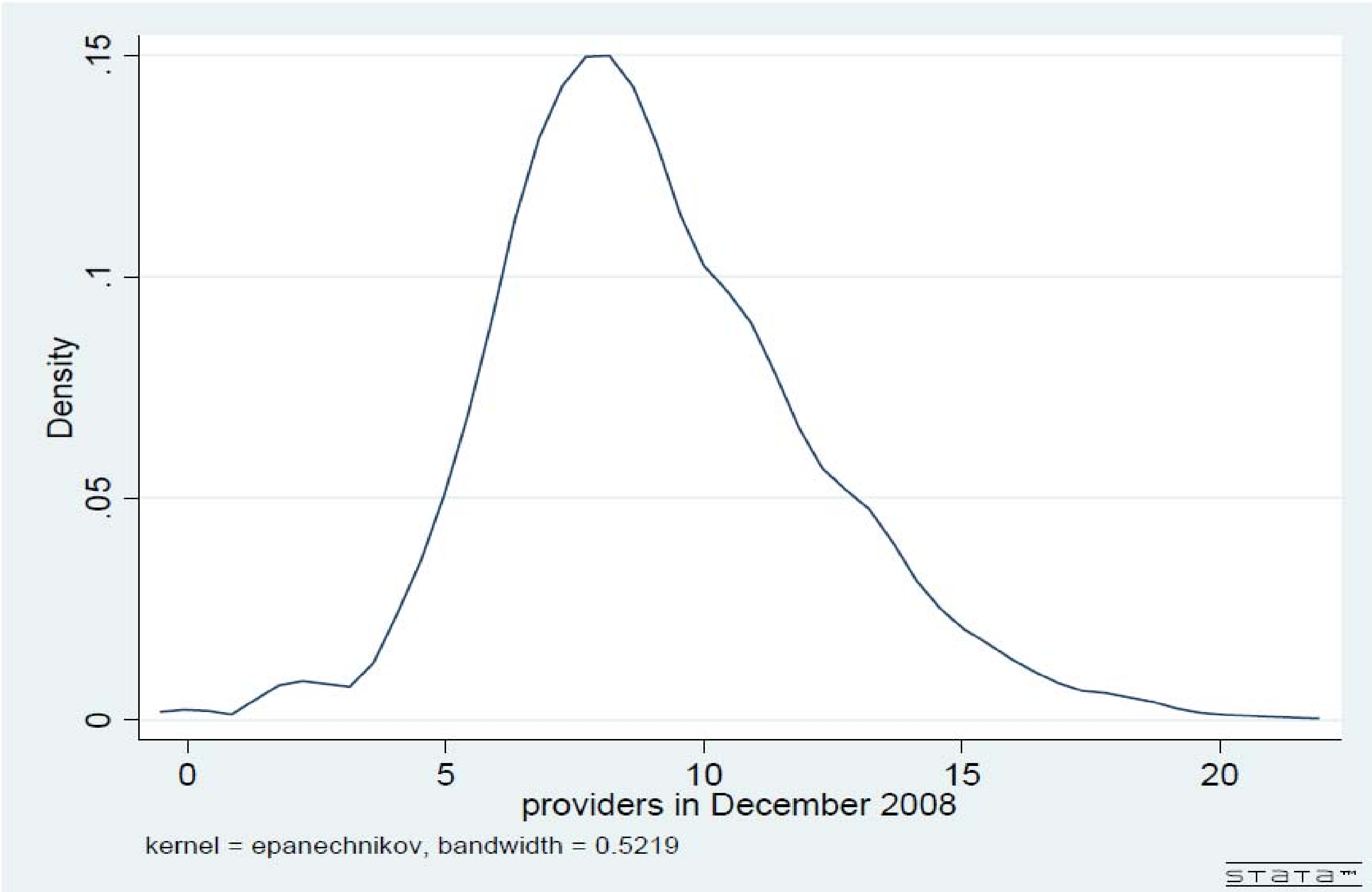
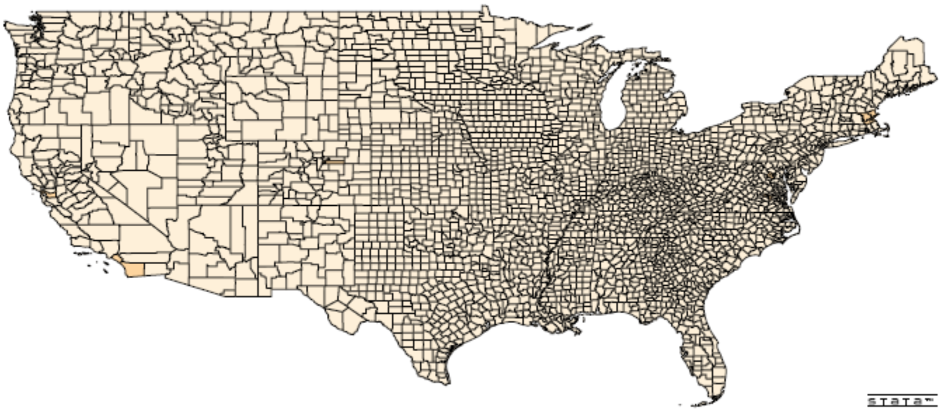


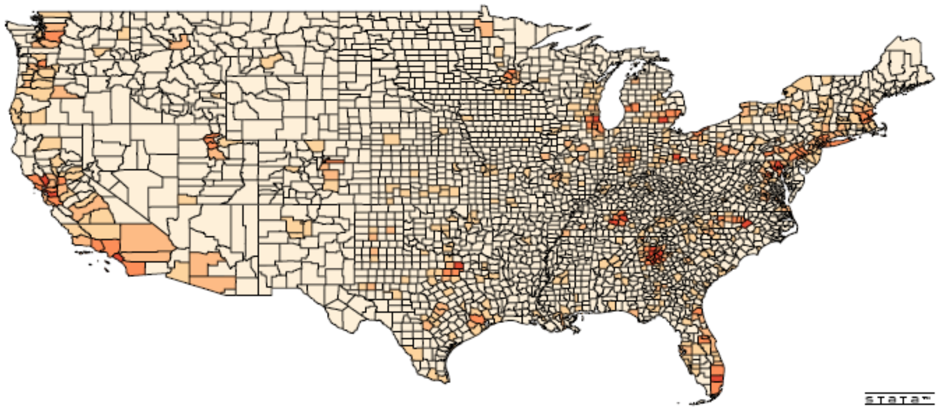
Figure 8

ISPs as a proxy of broadband penetration 2000-08

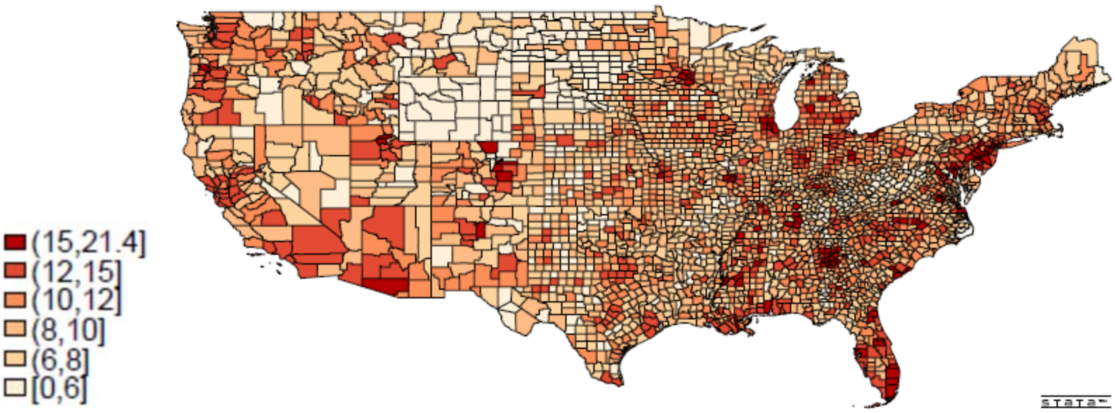
2000



2004



2008



4.2 Political Data

All political variables are likewise available at the county level, covering presidential elections for the period of interest. Variables on absolute number of votes are derived from FEC data. This allows us to derive a measure of Democratic and Republican vote share for each presidential election.

Turnout is also calculated at the county level. It is the ratio of the number of votes cast divided by the estimated voting age population per county from the U.S. Census Bureau.

Campaign contribution data is also available from the FEC. However it does not include donations less than $200 unless these donations are from the same individual and add up to more than $200 in an electoral cycle. As a result, the sample misses a significant amount of smaller donations. The unavailability of donations inferior to $200 clearly introduces measurement error in our campaign contributions data. This measurement error can be particularly severe when we consider small contributions (that we define as less than $500). However, since the Obama campaign made the greatest inroads among small donors, raising half a billion dollars, this should work against our results by decreasing the precision of our estimates. In the regression analysis, we use contributions in per capita terms for the total amount donated to each of the two major parties and the total amount donated to Democrats (Republicans) in small contributions (less than $500).[14] Summary statistics are reported in Table A1.

4.3 County-level control variables

We assemble a dataset of social, economic and demographic characteristics of the US counties by using a number of sources to generate controls for presidential elections during

[14]These variables are calculated for the period July-December of presidential election years.

the 2000-2008 period. Data on ethnicity, age, gender composition, population, income and poverty rates are taken from the Census. Employment data is available from the Bureau of Labor Statistics. We also control for characteristics of the terrain (elevation and slope).

Alaska, Hawaii, and outlying U.S. territories are excluded from the sample because their geographic placement makes them outliers and they cannot be used as part of our main identification strategy. D.C. and counties which changed boundaries in the period 1996-2008 have also been excluded from our sample.[15]

5 Differences-in-differences models

5.1 Method

The first step in our empirical investigation consists of estimating the relationship between changes in outcomes of interest and changes in number of ISPs between two presidential elections. We focus on the 2004-08 period but also report estimates for 2000-04. We estimate equations of the form:

$$\Delta y_{is} = \alpha_s + \beta \Delta I_i + \gamma \Delta \mathbf{Z}_i + \varepsilon_{is} \tag{1}$$

where y_{is} is an outcome variable in county i and state s (voting shares of Democrats or Republicans, turnout and campaign contributions), I_i is our proxy for internet take-up (number of providers) in county i, $\mathbf{Z}$ is a vector of control variables and ε_{is} is an error term. As customary, we indicate with Δ the difference between the 2008 and 2004 (2004 and 2000) values of a variable. Since our dependent variable is expressed as county-level difference, a simple regression controls for all county-specific time invariant confounding factors and corresponds to a standard fixed effects specification. Including state fixed

[15]Summary statistics of all control variables at their "pre-treatment" 2004 level can be found in Table 6.

effects in this specification accounts for state-specific shocks, i.e. anything that might have changed at the state level between the two elections, which is important since states are the relevant battleground in presidential elections. We include the following control variables: demographic characteristics (population density, percentage of voters aged 18-29, percentage of population above 65, percentage male, percentage of black, hispanic, asian residents), economic indicators (poverty, average individual income, unemployment), education (percentage with a college degree). In our most demanding specification

$$\Delta y_{is} = \alpha_s + \beta \Delta I_i + \gamma \Delta \mathbf{Z}_i + \delta \mathbf{Z}_{it} + \varepsilon_{is} \tag{2}$$

we also include our control variables at their first-period level (i.e. 2004 level if the Δ refers to 2004-08). It should be noted that including these level-variables together with their Δs serves a more demanding purpose than simply controlling for those omitted factors. Since the dependent variable is a difference, by including control variables expressed in levels we are accounting for the possibility that different pre-existing characteristics were affecting the trends in the outcome of interest across the counties. In this specification we also include characteristics of the terrain, namely average slope and elevation and their respective standard deviations. Standard errors are clustered at the state level.

5.2 Results

Table 2 reports the results of the effects on turnout and vote share for the Democratic party. Results for Republican vote share, not reported, are almost exactly the opposite of what we find for the Democrats. For the period 2004-08 (panel A) there is a robust correlation between the increase in number of providers and both the vote share of the Democratic candidate and voter turnout. This correlation also holds in the more demanding specifications of columns 4 and 8 where we introduce control variables at their 2004 levels, although the magnitude is substantially reduced. In our preferred specification (columns 3 and 7), which corresponds to a county-fixed effect regression with state-specific shocks, an increase in the number of providers by one standard deviation above the average (1.89) corresponds to about 0.67% increase in the percentage of Democratic vote and 0.31% increase in turnout. In the most conservative specifications (columns 4 and 8) these effects drop respectively to 0.21% and 0.13%.

The bottom panel of the table reports results for the 2000-2004 period, i.e. when both the dependent and independent variables refer to the previous electoral cycle. The pattern that emerges from the 2000-04 period is similar to that of 2004-08 but less robust, particularly for the share of the Democratic vote. For voting returns, the results with state specific shocks are comparable to those of the top panel but vanish when we include control variables. For turnout the results are similar to the 2004-08 case, although statistically insignificant when control variables are included. If these coefficients could, at least in part, capture a causal effect, then we should conclude that the mobilizing effect of the internet on turnout was, to a certain extent, already present before the Obama campaign but that the partisan pro-Democrat effect was probably not. To better understand the difference between panel A and panel B it is important to stress that the main explanatory variable in the two periods has a correlation coefficient of -0.015. In other words the counties with fastest broadband diffusion in 2004-08 are not the same that experienced broadband diffusion in 2000-04. The fact that in both periods we find a positive association between broadband diffusion and our electoral outcomes is therefore unlikely to be due to specific county characteristics.

In Table 3 we report diff-in-diff estimates for campaign contributions.[16] Again we uncover a pro-Democrat effect of internet penetration in the form of increased campaign contributions, while no effect is detected for contributions to Republicans. The effect this

[16] For space reasons simple regressions of outcomes over providers are not reported.

time appears to be stronger in 2000-04 but the magnitudes for the two periods are more similar when all controls (both in differences and in levels) are included. Magnitudes are also similar both for overall contributions and for contributions below 500 dollars.[17]

Our diff-in-diff estimates show a robust correlation between broadband diffusion and increase in turnout in presidential elections. They also suggest that the internet might have had a pro-Democrat effect in its early days, both in terms of vote shares and campaign contributions. This is not surprising since both in 2004 and in 2008 the Democrats were ahead of the Republicans in exploiting the new medium, as we documented in Section 3.

These estimates take us a long way in identification terms since they take into account fixed county-specific characteristics, state-specific shocks (particularly important in presidential elections) and a number of observable county variables that change over time. Nevertheless, these estimates would still be biased if there were unobservable county characteristics that changed between two elections and that could be related to both broadband diffusion and our variables of interest. For example, Stephens-Davidowitz 2014 provides evidence that racism played a large role in the outcome of the 2008 election. If racism was on the decline in areas with more internet penetration, our results would be overestimated. For this reason in the next section we propose an identification strategy based on a source of exogenous variation in internet penetration.

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[17] The dependent variable in these regressions is $log\left[\frac{1+donations}{population}\right]_t - log\left[\frac{1+donations}{population}\right]_{t-1}$ where t is 2008 (2004) and $t-1$ is 2004 (2000). This can be transformed into $log\left[\frac{(1+donations)_t}{(1+donations)_{t-1}} \times \frac{(population)_{t-1}}{(population)_t}\right]$. This explanatory variable takes into account the changes that occour in the population of a county. One of the advantages of this log-transformation is that it varies with changes in population even when total donations are zero both in t and $t-1$. This log-linear model implies that, for β sufficiently small (i.e. when $e^{\beta} \approx 1+\beta$), coefficients can be interpreted as percentage changes in the dependent variable. Take for example column 2 in panel A: a coefficient of 0.067 implies that one more ISP in a county is associated, if its population has not changed between the two periods, with a 6.7% increase in campaign contributions to the Democratic party.

Table 2. Diff-in-diff estimates: broadband diffusion, voting returns and turnout

A. 2004-2008

	percentage Democrat				Turnout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of providers	0.353***	0.357***	0.232***	0.111**	0.472***	0.194***	0.163***	0.068*
	(0.066)	(0.067)	(0.067)	(0.043)	(0.128)	(0.058)	(0.06)	(0.040)
State specific shocks	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Control variables (Δ)	No	No	Yes	Yes	No	No	Yes	Yes
Control variables (2004)	No	No	No	Yes	No	No	No	Yes
N	3086	3086	3086	3086	3086	3086	3086	3086
R2 (w=within)	.0269	.0269 (w)	.1136 (w)	.3869 (w)	.0453	.0116 (w)	.1054 (w)	.2732 (w)

B. 2000-2004 (voting outcomes, turnout, number of providers and controls are referred to changes between 2000 and 2004)

	percentage Democrat				Turnout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of providers	0.025	0.263***	0.107*	0.039	0.201*	0.219***	0.138***	0.072
	(0.102)	(0.065)	(0.054)	(0.049)	(0.102)	(0.040)	(0.038)	(0.045)
State specific shocks	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Control variables (Δ)	No	No	Yes	Yes	No	No	Yes	Yes
Control variables (2000)	No	No	No	Yes	No	No	No	Yes
N	3086	3086	3086	3086	3086	3086	3086	3086
R2 (w=within)	.0001	.0201(w)	.0754 (w)	.3546 (w)	.0111	.0165 (w)	0.064 (w)	.1090

All regressions include an intercept. The control variables are: population density, share young voters (aged 18-29), share aged above 65, share black, share hispanic, share asian, poverty rate, per capita income, unemployment rate, percentage with college degree, percentage male. We also include average elevation, average terrain slope and their respective standard deviations in columns 4 and 6. In panel A voting outcomes, turnout, number of providers and controls are referred to changes between 2004 and 2008, in panel B changes are referred to the 2000-04 period. Standard errors are clustered at the state level. *** significant at 1% level, ** at 5%, * at 10%.

Table 3. Diff-in-diff estimates: broadband penetration and campaign contributions by individuals

A. 2004-2008

	Total Democrats			Democrats (small)			Total Republican			Republican (small)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of providers	0.056*	0.067**	0.052*	0.078**	0.073**	0.052*	-0.002	0.020	0.035	0.005	0.025	0.026
	(0.032)	(0.029)	(0.029)	(0.032)	(0.031)	(0.03)	(0.037)	(0.039)	(0.04)	(0.040)	(0.041)	(0.045)
State specific shocks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (Δ)	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables (2004)	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
N	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086
R2 within	.0008	0.0098	.0286	.0018	.0110	.0282	.0000	0.0063	.0147	.0000	0.0063	.0139

B. 2000-2004

	Total Democrats			Democrats (small)			Total Republican			Republican (small)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of providers	0.372***	0.194***	0.097**	0.366***	0.183***	0.074**	0.086**	0.064	0.052	0.127***	0.060	0.030
	(0.041)	(0.042)	(0.042)	(0.039)	(0.036)	(0.033)	(0.036)	(0.042)	(0.048)	(0.036)	(0.35)	(0.036)
State specific shocks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (Δ)	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables (2004)	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
N	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086	3086
R2	.0359	.0752	.1020	.0388	.0825	.1119	.00165	.0122	.0226	.00419	.0190	.0315

All regressions include an intercept. The control variables are the same used in Table 2. In panel A campaign contributions, number of providers and controls are referred to changes between 2004 and 2008, in panel B changes are referred to the 2000-04 period. Standard errors are clustered at the state level. *** significant at 1% level, ** at 5%, * at 10%.

6 Instrumental variable: Right-of-Way Regulations

6.1 The ROW Data

Our diff-in-diff estimates take us a long way in removing unobservable confounders. However, while we control for a large set of county characteristics, we should still be concerned about possible bias generated by unobservable omitted variables. The most important identification concern come from the fact that political behavior and attitudes could be correlated to the demand for internet services. Correlations between broadband adoption and political variables could then simply be the consequence of the fact that different type of individuals live in different counties. Although diff-in-diff estimates take this problem into account if these variables are fixed or moving slowly (for example people more interested in politics and more likely to vote could also be more interested in surfing the net), this remains a concern if these same characteristics changed sufficiently rapidly, i.e. in the space of four years between two presidential elections. An exogenous source of variation in broadband diffusion must then be found in the non-demand related part of the supply of internet, i.e. when broadband availability is constrained by variables which are not related to the demand side.

When taking decisions about the roll out of infrastructure, internet providers consider the profitability of alternative investments, comparing the costs with the potential revenue. Although the most important variables are likely to be the determinants of the demand for internet services (for example population density, education, wealth, age etc.), different levels of broadband penetration can be attributed in part to different regulatory regimes concerning Right-of-Ways (ROW) laws (Day 2002). These can be broken down into a number of different items, which influence the cost for ISPs to lay infrastructure:[18]

1. Jurisdiction: In some states, an ISP has to get permission to build on the public

[18]See National Association of Regulatory Utility Commissioners (NARUC) 2002 for more details.

ROW from every single municipality that the project crosses, whereas in other states this is handled by a centralized authority.

2. Compensation: Compensation demanded by municipalities in return for granting ROW permission ranges from cost recovery (the cost to the municipality of administering the ROW) to a rental fee (e.g. percentage of gross revenue) to a flat tax.

3. Timeliness: Some states have a maximum time for processing permit applications, significantly speeding up the process.

4. Mediation and Condemnation: States also vary in how they deal with conflicts between municipalities and ISPs, and private landowners and ISPs. For example, in Vermont, landowner complaints can be heard on a wide range of issues including aesthetics, and decisions are appealable to the Vermont Supreme Court. On the other extreme is Texas, where most factors are not appealable and landowners must pay the ISPs legal expenses if they lose in court.

5. Remediation and Maintenance: These laws dictate issues such as in what state of repair ISPs must maintain their facilities. For example, if a sidewalk is torn in order to lay cabling, these laws determine to what extent the sidewalk would need to be restored to its original state and under what time frame.

Historically, municipal ROW laws can be dated back to a different era, when they served the purpose of regulating the roll out of electricity and telephone infrastructure. The current ROW regime has been re-defined by the Telecommunications Act of 1996, which allowed municipalities to regulate the public ROW, hence leaving many pre-existing laws in place.[19]

States also passed their own laws concurrent with the Telecommunications Act that either limited or reinforced this municipal right, leading to the significant variation in

[19] Section 253(c) of the Act states: "Nothing in this section affects the authority of a State or local government to manage the rights-of-way or to require fair and reasonable compensation from telecommunication providers, on a competitively neutral and non-discriminatory basis".

ROW regulations across states that we see today. For example, the time frame within which a local government must respond to a request to use its rights-of-way has been fixed to 30 days in Ohio, 90 days in Michigan and 120 days in Washington. Most states, however, decided to impose no limit. When states opt for a light-touch regulation the consequence is often that *"each municipality may have its own policies, impose separate requirement or fees, and take different lengths of time to grant permits. This inconsistency across a state makes it very difficult to plan a coherent network rollout"* (Kende and Analysys 2002). Some municipalities regulate the hours that a telecom provider must be available to take customer complaints and the time-frame in which new customer orders must be filed. Some municipalities require having a customer care office in the municipality. According to Day 2002 *"A number of municipalities continue to block ROW access as a means of extracting additional compensation from telecommunication providers"*.[20]

According to the National Telecommunication and Information Administration (NTIA) the consequence of the current regulatory regime is that *"rights-of-way management has arisen as a key issue in broadband deployment at the federal, state, and local levels. The steps required for a telephone company to lay new lines on a public street, a cable company to start providing internet service, or a cell phone company to place antennas on public poles, can have real consequences in the decision to deploy broadband service to a community."*[21]

In 2002 TechNet, an industry lobbying organization that counts almost every major technology company among its members, released a report on the state regulations influencing supply and demand of broadband (Kende and Analysis 2002). As part of this report, the authors compiled an index, ranking the regulatory regime in terms of ROW

[20] An extreme example is Memphis (TE), which requires new providers to give the city four optical fibers in any new fiber-optic cable installation and 5% of all gross revenues.
[21] See the NTIA report "State and Local Rights of Way Success Stories": https://www.ntia.doc.gov/legacy/ntiahome/staterow/ROWstatestories.htm (accessed September 14, 2017).

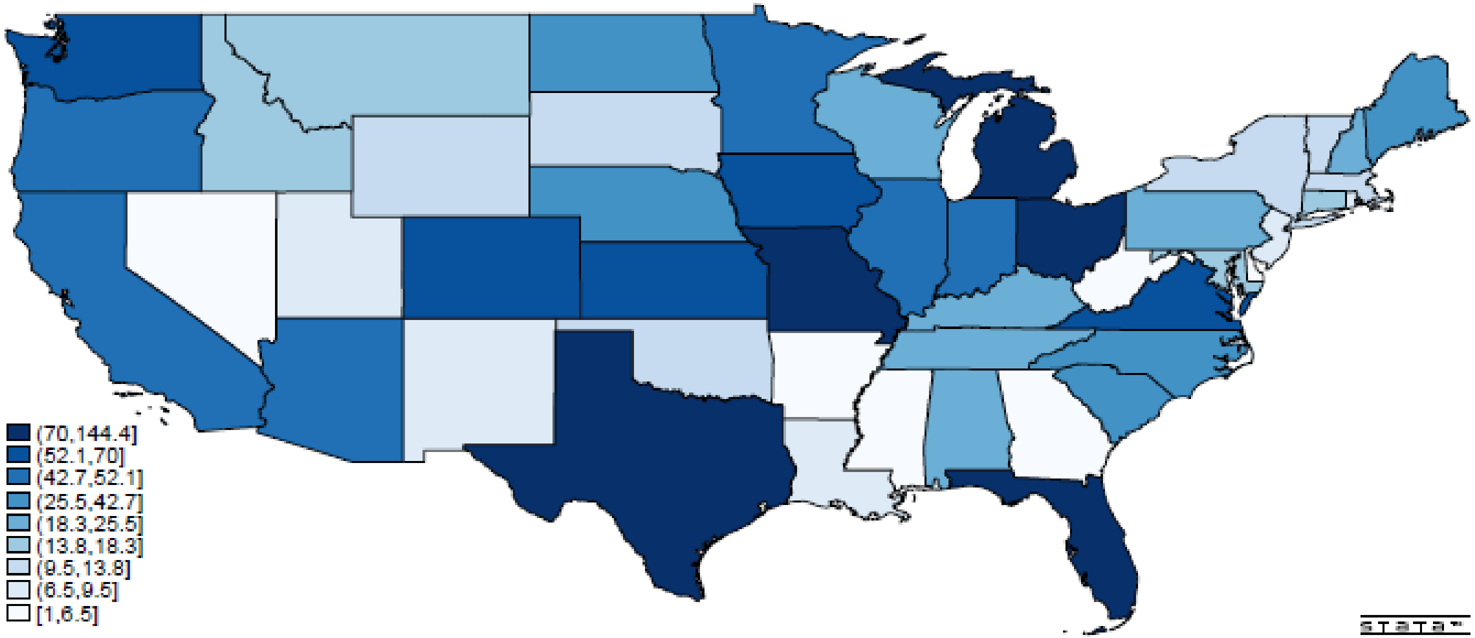
laws across states. The "state broadband index" aggregates 52 underlying variables, tapping in all relevant broadband policy dimensions which can be affected by State policy. The result is a score given to each state "based on the extent to which their public policies spur or impede broadband deployment"[22] The index captures the regulatory difference in ROW laws across states two years before the 2004 election and can therefore be used to explain the rollout of broadband infrastructure during the 2004-08 period independently of other variables affecting broadband uptake. Figure 9 shows the variation in ROW index levels across US states. Because the ROW index varies by state, rather than county, we combine our proposed IV with a spatial discontinuity approach.

Since ROW regulations are endogenous policy decisions, one concern is that regulatory regimes could be determined along partisan lines. Republicans, for example, could take a more pro-business approach than Democrats and therefore be more prone to limit the regulatory discretion of municipalities. On the other side, anticipating a potential pro-Democrat effect of broadband penetration, Democratic politicians could take a policy stance which is more favorable to the roll out of internet infrastructure. This concern leads us to check if the ROW index is correlated with any relevant political covariate at the state level. We run this check for the years 1996 and 2000, taking into account that the TechNet index was released in 2002. We find no robust correlation between ROW laws and state-level political variables (Table 4). Variables related to state partisanship (the share of Democratic vote in presidential elections, having a Democratic governor and the share of Democratic members of the State House) are far from statistical significance. We find statistically significant simple correlations with turnout (in 1996 only) and with the closeness of presidential elections (both in 1996 and 2000), but these become insignificant once some standard control variables are included. Our conclusion is that there is no relationship between the state regulation of ROW and the state-level political situation.

[22] See Kende and Analysis 2002 for details on the construction of this index.

Figure 9

The TechNet State Broadband ROW Index



Details of the value of the index by state can be found in Table A3.

Table 4. Correlations between the ROW index and political covariates

	dep variable: TechNet ROW state broadband index									
year 2000										
share of democratic vote	-4.411	1.888								
	(5.226)	(7.426)								
democratic governor			-0.234	-1.564						
			(0.995)	(1.096)						
Democratic share of state house					-1.799	-2.086				
					(2.875)	(3.127)				
turnout %							-0.135	0.036		
							(0.083)	(0.080)		
closeness in presidential election									10.400**	1.348
									(4.049)	(4.833)
control variables	no	yes	no	yes	no	yes	no	yes	no	yes
N	48	48	48	48	47	47	48	48	48	48
R2	.00848	.451	.000985	.482	.0063	.456	.0444	.452	.074	.451
year 1996										
share of democratic vote	-3.555	-5.384								
	(6.792)	(8.847)								
democratic governor			-0.435	-1.295						
			(1.049)	(1.256)						
Democratic share of state house					-1.029	-2.815				
					(2.589)	(3.085)				
turnout %							-0.182**	0.004		
							(0.082)	(0.102)		
closeness in presidential election									12.824**	6.943
									(6.264)	(5.774)
control variables	no	yes	no	yes	no	yes	no	yes	no	yes
	48	48	48	48	47	47	48	48	48	48
	.00371	.393	.0035	.411	.00237	.397	.0894	.389	.0636	.404

Control variables include income per capita, population density, percentages of poor, unemployed, black, old (above 65), kids (between 5 and 17), per capita federal spending, per capita defense spending.

6.2 The ROW index in contiguous county-pairs

Our identification strategy is based on a combination of a spatial discontinuity approach with instrumental variables. The spatial discontinuity strategy is described in detail in Holmes 1998, and has been used and extended by Dube et al. 2009 and by Duranton et al. 2011. Each county on a state border is matched with a contiguous county across the border and a county-pair fixed effect is then included in the specification so that coefficients are estimated from within-pair variation. Figure 10 shows in blue the counties included in this restricted sample. A county which is adjacent to more than one county of a bordering state will therefore enter multiple times into the sample, each time with a separate pair fixed effect. Standard errors must therefore be corrected to take into account the resulting correlations across county pairs. This is done following Cameron at al. 2011 by two-way clustering the standard errors, with non-nested clusters given by the state (as in our OLS regressions) and by each border (i.e. including all counties along a border between any two states). This amounts to estimating an equation of the form

$$\Delta y_{isp} = \alpha_p + \beta \Delta I_{is} + \gamma \Delta \mathbf{Z}_{is} + \delta \mathbf{Z}_{is} + \varepsilon_{isp} \tag{3}$$

where p refers to a pair of counties from two different states sharing a border. Note that we are still using the 2004-08 differences both for the dependent variables and internet penetration, which means we still retain the identification advantages of our diff-in-diff strategy.

By using this strategy, we are able to also take into account unobservable confounding factors which can't be accounted for by using control variables since it is unlikely that, on average, economic and social conditions vary discontinuously along state borders: this renders comparisons between county pairs more reliable than comparisons between any two random counties from the full sample. We combine this empirical strategy with an instrumental variable, exploiting exogenous variation across state borders in internet supply due to different ROW laws as previously outlined. Our first stage equation therefore is the following:

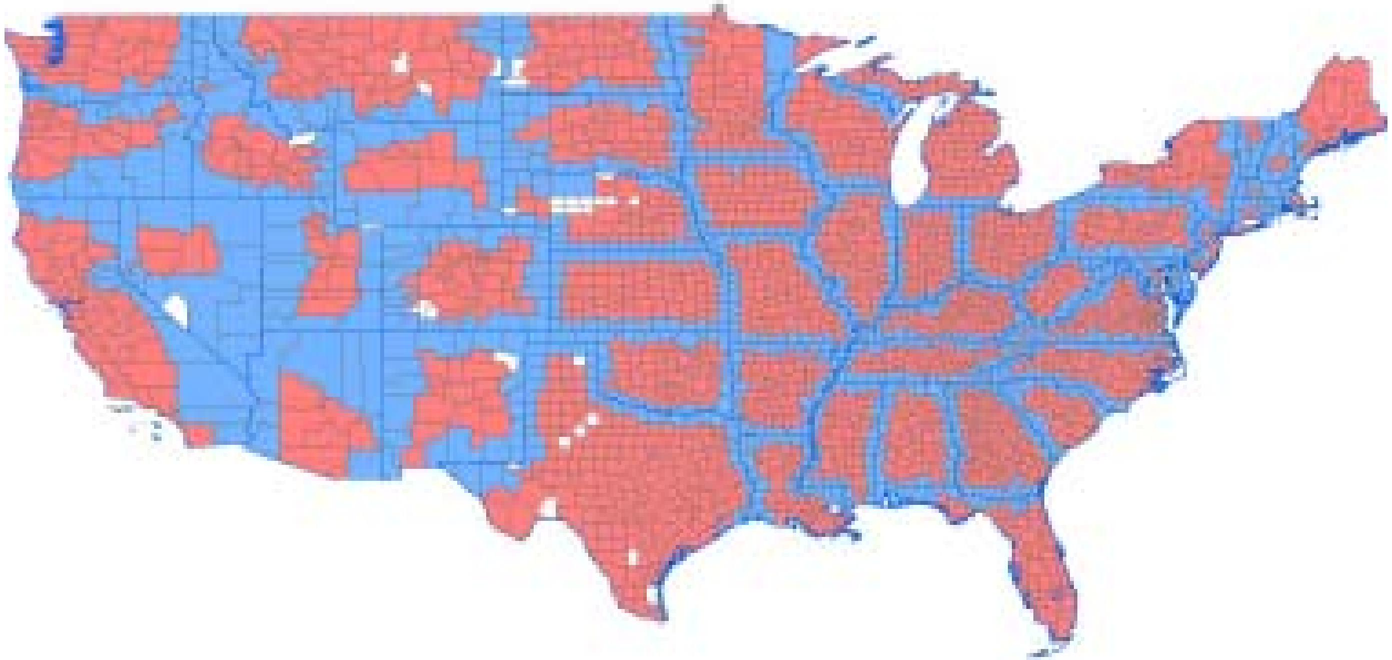
$$\Delta x_{is} = aROW_s + b\Delta \mathbf{Z}_{is} + c\mathbf{Z}_{is} + \omega_{is} \tag{4}$$

The identification assumption is that, conditional on the baseline county characteristics - income, poverty, education, ethnicity, population density, age, slope and elevation - the ROW index does not affect the change in the dependent variables independently of growth in internet access. A more general condition is that, conditional on observables, no state-time variables should explain both broadband penetration and the outcomes of interest. As shown in Table 4 we can rule out that conditions for broadband deployment followed partisan lines or other political characteristics of the states.

We report a few selected examples to illustrate visually our IV strategy. Figure 11 (a) shows the border between Kansas and Oklahoma: counties in Kansas appear to have a substantially higher broadband penetration (darker), with a clear discontinuity at the border. It is also the case that Kansas is ranked 7th according in the ROW indicator while Oklahoma is ranked 34th. We can observe similar patterns at the border of Iowa with Wisconsin and Illinois (Figure 11 b) or at the border between Georgia and South Carolina (Figure 11 c).

Figure 10

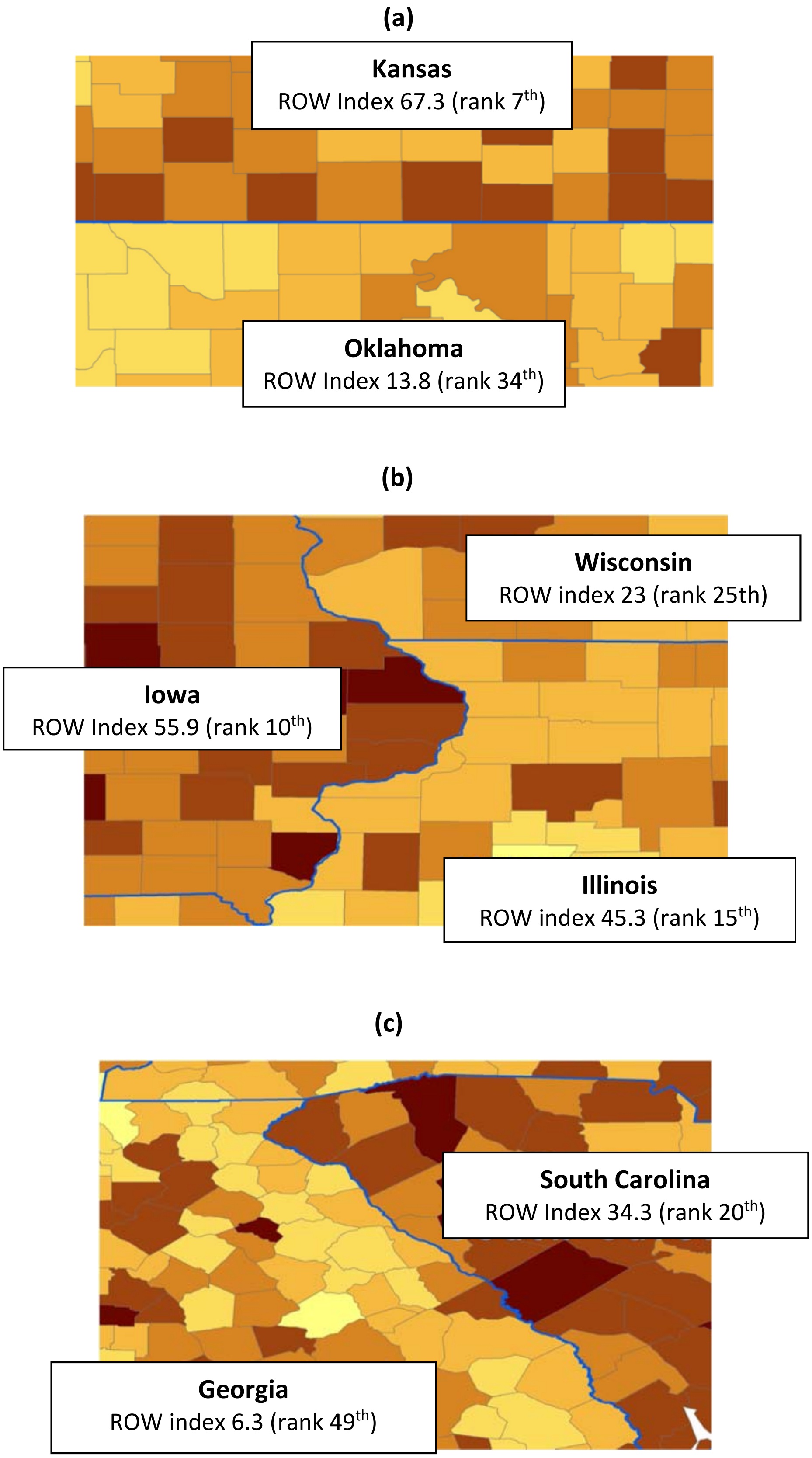
Restricted sample of counties on state borders



Counties in white are those which changed boundaries during the period 1996-2008 and are therefore excluded from our sample

Figure 11

ROW Index and broadband penetration: examples



Not all cases are so stark or reflect so closely our prior expectations, but what matters for the relevance condition to be satisfied is that the pattern we illustrate in these maps holds on average and controlling for other covariates. We show in the first stage regressions reported in Table 5 that this is the case. In the first three columns of Table 5 we report regressions on the full sample of US counties: from these we note that a simple regression of change in providers on the ROW Index gives a statistically insignificant coefficient. This coefficient becomes larger and significant at the 10% level when differences in control variables are included. However, it again becomes insignificant when control variables at their 2004 levels are introduced. The coefficient remains positive in all cases but the lack of significance could suggest, perhaps unsurprisingly, that broadband deployment follows first and foremost the demand for broadband services, which is mostly determined by population characteristics.

Columns 4-6 of Table 5 use our restricted sample of contiguous counties and constitute the proper first stage of our IV estimates. We obtain positive and statistically significant coefficients in all specifications. The coefficient of specification (6) implies that an increase in the ROW index by one standard deviation (0.26) should lead to an increase in the number of providers operating in a county of about 0.28 above the average increase. That would be about 15% of a standard deviation in the change in providers between 2004 and 2008. The difference with the results obtained using the full sample are large both in magnitude and in statistical significance. This suggests that, when first order considerations related to population characteristics are controlled for (i.e. when counties are more comparable along both observable and unobservable dimensions), ROW laws become important predictors of broadband diffusion.

Table 5. First stage regressions

Dependent variable: ∆providers (2004-08)	full sample			contiguous counties (first stage)		
	(1)	(2)	(3)	(4)	(5)	(6)
ROW Index	0.5797	0.953*	0.796	0.941**	1.222***	1.077**
	(0.4098)	(0.5550)	(0.5844)	(0.455)	(0.430)	(0.444)
County-pair fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (Δ)	No	Yes	Yes	No	Yes	Yes
Control variables (2004)	No	No	Yes	No	No	Yes
N	3086	3086	3086	2302	2302	2302
R2 within	0.0086	0.0532	0.1119	.016	.0647	.1099

For the list of control variables see the note to Table 2. Standard errors are clustered at the state level in columns 1-3 and at the state and border level in columns 4-6.

Before we proceed to the second stage regressions we address two important concerns arising from our choice to restrict the sample of counties. The first concern is whether the restricted sample is in fact representative of the entirety of US counties. This in itself is not a problem for our identification strategy (and more generally for the internal validity of our study): restricting attention to comparable units of observation is standard practice in many estimation methods.[23] If, however, the selected counties were substantially different from the rest in important demographic, economic and political dimensions, then we would be concerned about the generalizability of our results to the US population as a whole. Table 6 reports t-tests of the difference in means between the two samples. Overall, the differences in the means of most variables in the two samples are small, but we can't reject that these small differences are statistically significant for population density and the distribution of several demographic groups (young voters, Hispanic voters, college educated and above 65s). In all other demographic and economic variables, the two samples have statistically indistinguishable means.[24] We also checked the differences in the two samples in terms of presidential voting patterns and reassuringly found that there is no systematic difference in turnout and no difference in partisan leaning, as shown by the share of Republican vote (over the sum of Republican and Democratic vote) in both the 2000 and 2004 presidential elections. Perhaps not suprisingly, the two samples are instead substantially different in average elevation and slope.

The second concern is that our empirical strategy constitutes an improvement over the diff-in-diff estimates only if the contiguous counties are indeed more similar (and therefore comparable) than any two randomly chosen counties. If the opposite was the case then the advantages of IV could be more than compensated by the disadvantages of having less comparable counties. We are not aware of the existence of any formal test in the literature

[23] For example matching estimates.

[24] These variables are considered at their 2004 levels, i.e. in what in an experimentalist terminology would be their "pre-treatment" values.

to check the validity of this assumption, but we can provide evidence that it is indeed the case that, looking at observables, contiguous counties sharing a state border are more similar than non-contiguous counties. This should in turn reinforce the assumption that they are more similar across unobservables as well.

For this purpose we generate randomly assigned pairs. For each pair and each variable of interest we then calculate the mean (within a pair) and the distance of each observation from this mean. The distribution of distances from the mean are therefore symmetric around zero. We can then compare the distribution of within distances from the mean of the true contiguous pairs with the distribution of within distances from the mean of the randomly assigned pairs. The validity of restricting the sample to contiguous counties (and using contiguous counties fixed effects) then relies on the distribution of distances being denser around zero (i.e. each element of a pair is closer to the mean and therefore to the other element of the pair) for the adjacent relative to the random pairs. Figures 12a and 12b report results for economic (unemployment, poverty, income) and demographic (population density, percentage aged 18-29 and percentage of college educated) variables, which should be particularly important in determining broadband uptake. In all cases, the contiguous counties appear "more similar" than randomly paired counties. In Fig 12b we also add average county elevation and slope which show an even stronger pattern of similarity between bordering counties.

One concern of paramount importance is that, although there is a clear spatial correlation crossing state boundaries for economic and demographic variables, the same might not be true for political variables. In particular, it could be that each state has a political context which differs in fundamental ways from that of neighboring states. Figure 12c shows that the pattern of spatial correlation also applies to a large degree to political variables. We apply our check to two variables: the share of Republican vote (over the total of Republican and Democratic votes) and turnout rates. In both 2000 and 2004 these variables are substantially more correlated across contiguous counties than across the entirety of US counties.

These graphs help to visually illustrate our method, which consists of a very simple test. First, note that for each variable both distributions (that referred to the contiguous counties and that of the random pairs) are centered at zero and symmetric by construction. Our hypothesis is that the bordering counties are more similar and this can be tested by using the standard deviations of the two distributions. Hence, Table 7 reports the results of a test of equality of the standard deviations for all our control variables (both at their 2004 level and in 2004-08 differences). Since our assumption is that the random pairs are more distant from their averages than the bordering counties, we use a one-sided F-test. The results show that in all cases (except for the 2004-08 change in the share of elderly) we can accept with a high degree of confidence the hypothesis that those variables have closer values within a pair of bordering counties than in our randomly generated pairs. This applies not only to baseline 2004 values but also to 2004-08 changes, suggesting that the trends in these counties also tend to be more correlated. From this exercise, we conclude that restricting our sample to pairs of contiguous counties generates more comparable observations. Having established greater similarity on observables, it is then likely that exploiting the within-pairs variation of contiguous counties also helps remove unobservable confounders.

Table 6. Comparing the sample of bordering counties to the full sample of US counties

	Full Sample (mean)	Full Sample (sd)	Bordering Counties (mean)	Bordering Counties (sd)	Difference	(p-value)
unemployment %	5.66	(1.78)	5.65	(1.88)	0	0.95
young (18-29) voters %	15.68	(3.38)	15.17	(2.72)	0.51	0
population density (log)	2.83	(1.68)	2.66	(1.7)	0.17	0
black %	9	(14.36)	8.13	(13.67)	0.87	0.08
hispanic %	6.9	(12.32)	5.65	(8.86)	1.24	0
asian %	0.91	(1.79)	0.82	(1.67)	0.09	0.13
poverty %	13.77	(5.13)	13.67	(5.17)	0.1	0.58
mean income (log)	10.52	(0.23)	10.51	(0.24)	0.01	0.12
college educated %	17.69	(8.09)	17.15	(7.32)	0.54	0.05
male %	49.64	(2.02)	49.55	(1.7)	0.08	0.21
elderly (above 65) %	15.05	(4.11)	15.52	(4.12)	-0.48	0
mean elevation	436.87	(498.03)	525.87	(564.27)	-89	0
mean slope	2.12	(2.48)	2.67	(2.84)	-0.54	0
elevation (sd)	74.56	(114.85)	95.49	(128.22)	-20.92	0
Republican vote 2000	58.89	(12.13)	58.63	(12.76)	0.26	0.54
Republican vote 2004	60.97	(12.55)	60.82	(12.8)	0.15	0.73
turnout 2000	53.27	(9.01)	53.71	(8.87)	-0.44	0.16
turnout 2004	58.52	(9.29)	58.69	(8.67)	-0.16	0.6

The table reports the means and standard deviations of, respectively, the full sample of US counties used in the regressions and the sample of contiguous counties. When not otherwise specified, the value refers to the pre-treatment situation, i.e. 2004.

Fig. 12a. Comparing bordering and randomly paired counties

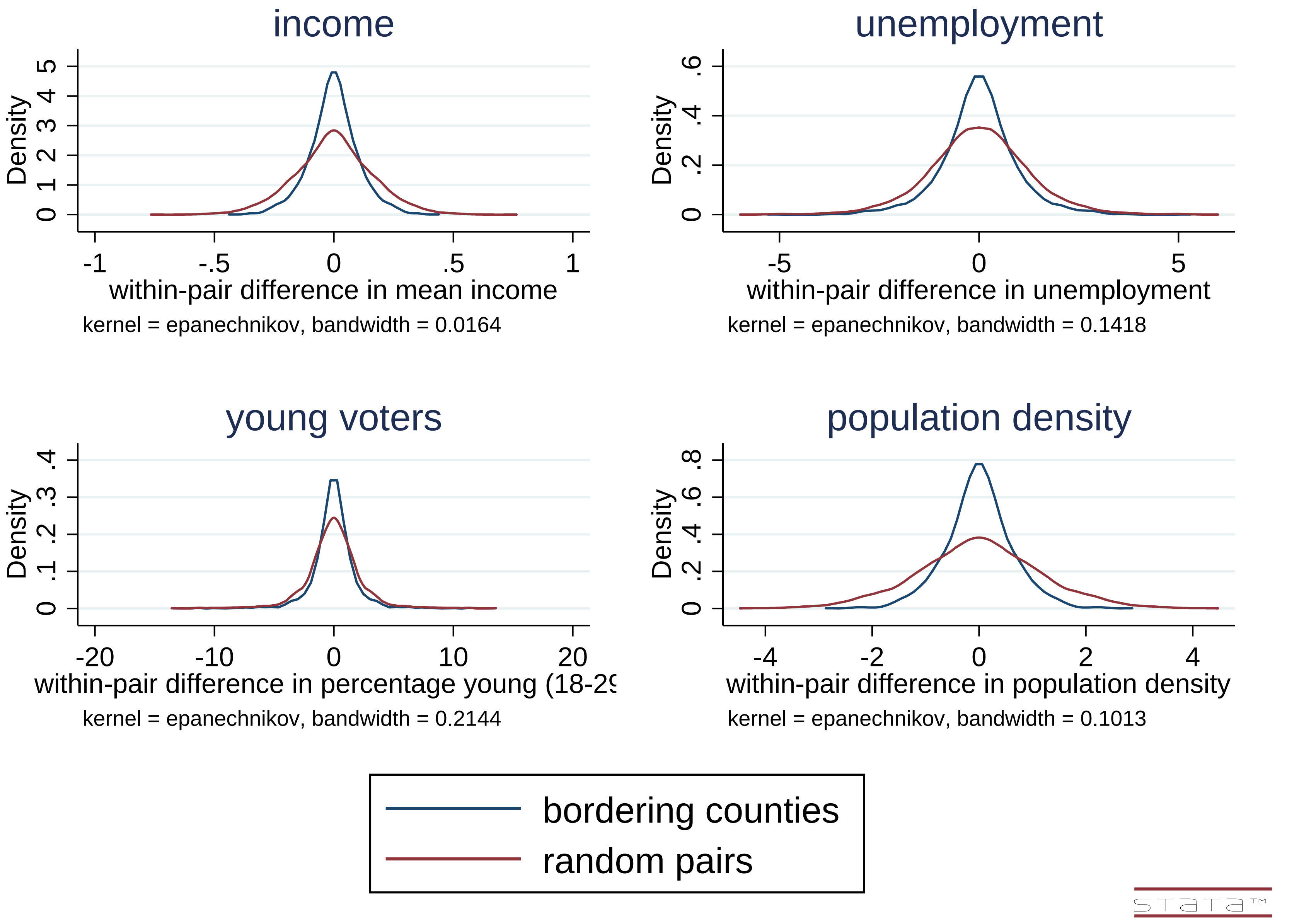


Fig. 12b. Comparing bordering and randomly paired counties

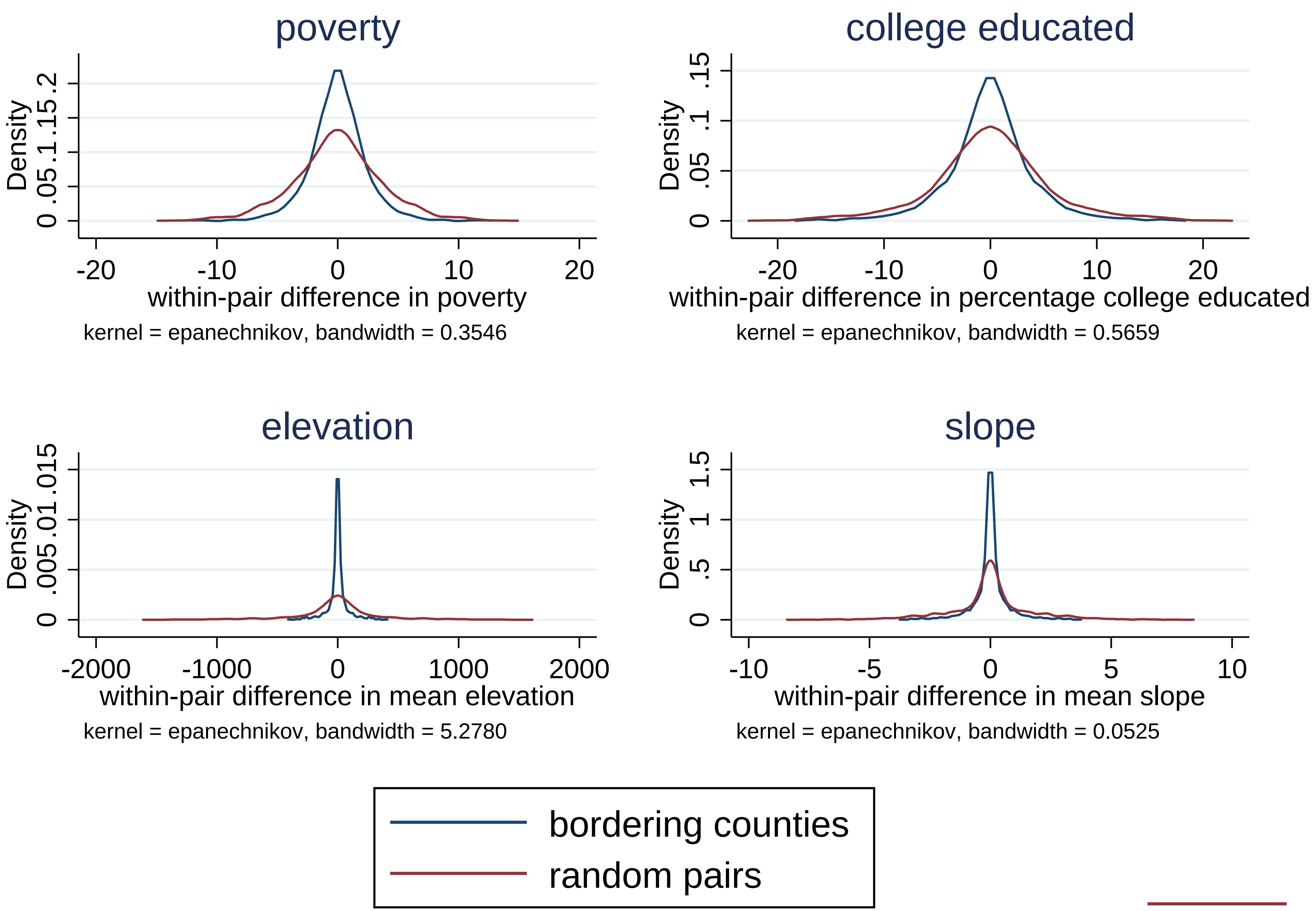


Fig. 12c. Comparing bordering and randomly paired counties

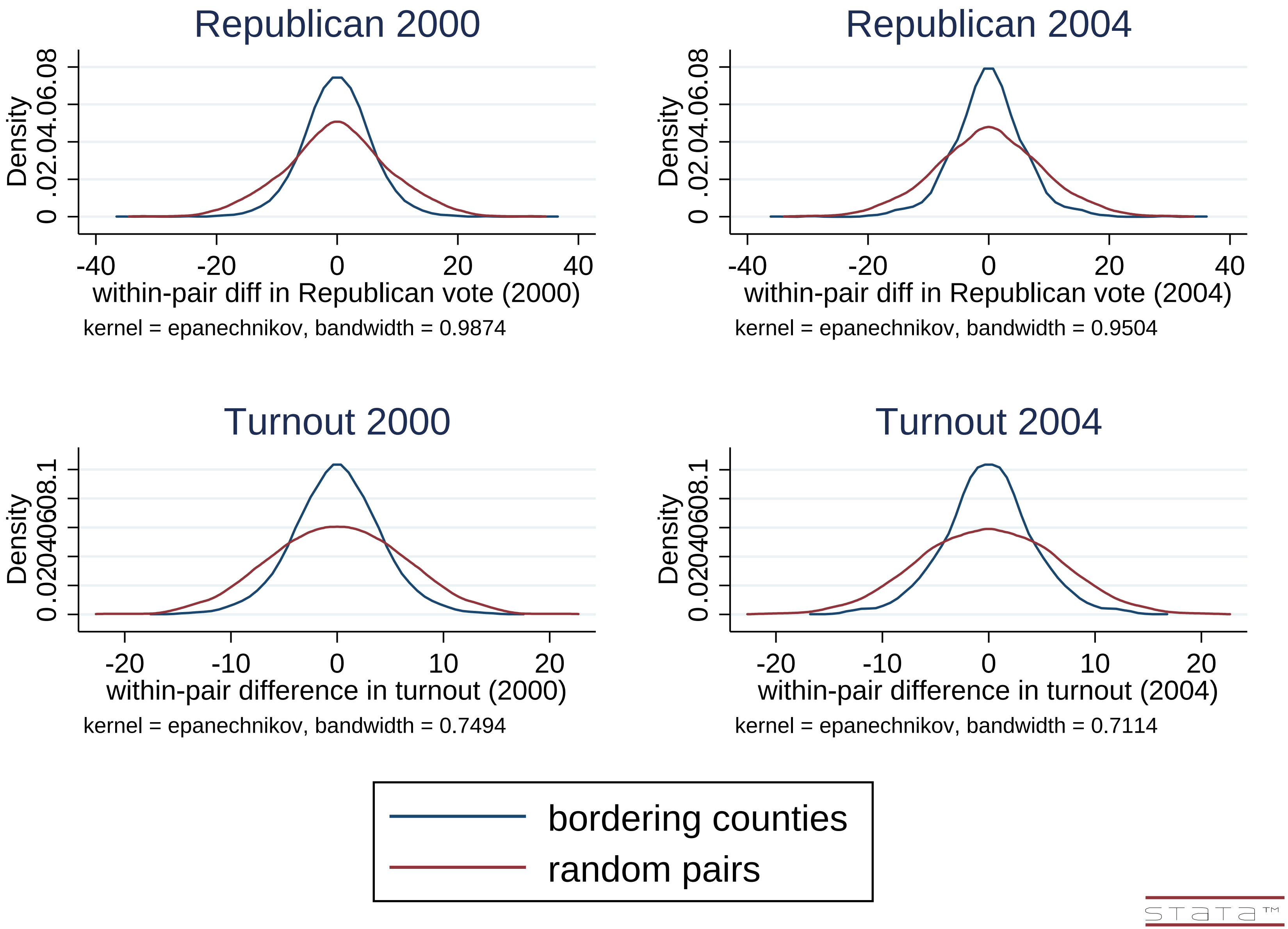


Table 7. Testing the similarity of bordering county pairs vs randomly paired counties

	Full Sample (sd)	Bordering Counties (sd)	Ratio	p-value (one sided)
unemployment %	0.927	1.263	0.73	0
young voters %	1.897	2.375	0.8	0
population density	0.622	1.172	0.53	0
black %	4.21	10.323	0.41	0
hispanic %	3.292	8.725	0.38	0
asian %	0.752	1.272	0.59	0
poverty %	2.331	3.706	0.63	0
mean income	0.102	0.165	0.62	0
college educated %	4.008	5.603	0.72	0
male %	1.153	1.435	0.8	0
elderly %	2.016	2.858	0.71	0
mean elevation	80.999	359.881	0.23	0
mean slope	0.731	1.77	0.41	0
elevation (sd)	33.593	81.293	0.41	0
Rep vote 2000	5.764	8.672	0.66	0
Rep vote 2004	5.866	8.984	0.65	0
turnout 2000	4.146	6.254	0.66	0
turnout 2004	4.224	6.459	0.65	0
Δ unemployment	0.654	0.889	0.74	0
Δ young voters	0.007	0.009	0.83	0
Δ population density	0.027	0.04	0.68	0
Δ black	0.003	0.005	0.72	0
Δ hispanic	0.005	0.006	0.73	0
Δ asian	0.001	0.001	0.91	0
Δ poverty	0.011	0.014	0.8	0
Δ income	0.033	0.042	0.78	0
Δ college educated	0.007	0.008	0.92	0
Δ male	0.002	0.003	0.81	0
Δ elderly	0.672	0.497	1.35	1

The p-value refers to a one-sided F-test where the nul hypothesis is that the standard deviaton of the distribution of within-pair differences is smaller for bordering than for randomly paired counties. Baseline values refer to 2004, the Δ refers to changes between 2004 and 2008. Population density and mean county income are expressed in logs.

Table 8. Estimates using contiguous county pairs: Democratic party vote share, turnout and campaign contributions

	Vote share D		Turnout		Contributions D		Small contr. D		Contributions R		Small contr. R	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Providers	0.185**	0.197**	0.153**	0.107*	0.132**	0.144**	0.160***	0.182***	0.024	0.029	0.056	0.046
	(0.083)	(0.081)	(0.70)	(0.064)	(0.061)	(0.062)	(0.061)	(0.057)	(0.055)	(0.058)	(0.056)	(0.057)
Control variables (Δ)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (2004)	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302
R2	.070	.256	.114	.259	.019	.042	.0.24	.052	.014	.024	.020	.041

For the list of control variables see the note to Table 2. Standard errors are double clustered at the state and border level. *** significant at 1% level, ** at 5%, * at 10%

Table 9. IV estimates using contiguous county pairs: Democratic party vote share, turnout and campaign contributions

	Vote share D		Turnout		Contributions D		Small contr D		Contributions R		Small contr R	
Providers	0.516	0.718	1.644**	1.648*	-0.326	-0.240	-0.055	0.026	0.233	0.233	0.128	0.262
	(0.914)	(1.032)	(0.820)	(0.905)	(0.482)	(0.487)	(0.464)	(0.484)	(0.365)	(0.443)	(0.324)	(0.381)
Control variables (Δ)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (2004)	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Cragg-Donald F-statistic	30.67	22.84	30.67	22.84	30.67	22.84	30.67	22.84	30.67	22.84	30.67	22.84
N	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302	2302

For a list of control variables see the note to Table 2. The Stock and Yogo (2005) 10% critical value is 16.38. Standard errors are double clustered at the state and border level. *** significant at 1% level, ** at 5%, * at 10%

6.3 Results

Having established the usefulness of restricting the sample independently of our IV strategy, we first use our dataset of contiguous counties to estimate OLS coefficients. The results (reported in Table 8) are similar to those obtained with the full sample of counties and confirm positive effects of broadband penetration on turnout, share of Democratic vote and campaign contributions to the Democratic candidate. The magnitudes are also not very different from those we found in the full sample. It is worth noting that in the restricted sample the inclusion of control variables at their 2004 level has a substantially smaller impact on the estimated coefficients if compared with what happens in the full sample. This might suggest that restricting the sample does indeed remove unobserved confounders, hence reducing the impact of observed controls (presumably correlated with the unobservables) on the estimates.

Table 9 reports our results when we instrument broadband diffusion by the ROW index. Results for both the vote share of and the contributions to the Democratic candidate are not robust to IV estimation, which suggests that the positive correlation found in diff-in-diff estimates (both in the full sample of US counties and in the restricted sample of contiguous counties) likely captures a co-movement due to omitted unobservables.

However, the correlations we found for turnout in our previous regressions hold in our IV estimates as well. The magnitude of the coefficient is substantially larger, indicating a possible attenuation bias in simple OLS estimates. As recounted in Section 4.1, the number of ISP providers in a county is a noisy proxy for internet access and our IV strategy may have reduced the bias caused by measurement error. A within-pair standard deviation increase in the number of providers (1.34) leads to an increase of about 2.2% in turnout. The corresponding magnitude in OLS estimates with contiguous county pairs was about 0.21% (a similar magnitude was implied by the coefficients found in the full sample). The increase in turnout implied by our IV estimates is therefore substantial, although still within reasonable limits.

In conclusion, our findings on turnout appear to be robust to all methods and samples. We are instead less sanguine about the pro-Democrat effect of broadband penetration in the 2008 election.

6.4 Heterogeneous effects

Average effects can hide important heterogeneities. Socioeconomic characteristics may have an influence on the likelihood of internet use and on the responsiveness to online political campaigns. In particular, young Americans are substantially more likely than older individuals to use the internet and have also been reported to have had a record high turnout rate in the 2008 election. Hence, in Table 10 we report IV estimates when we include interaction terms with county-level socioeconomic variables.

In contrast to anecdotal evidence, we find no statistically significant coefficient when we include an interaction between the change in the number of providers and the share of voters aged 18-29. The coefficient for turnout is actually negative, suggesting that, if anything, young voters might have been influenced negatively. We find instead that broadband penetration had a positive impact on Democratic vote share in counties with a high percentage of African American residents. That the participation of black voters in the first election with a black candidate was at unprecedented rates is a well-established fact. Here we show that broadband diffusion contributed in shifting African American votes to the Democratic candidate. Our exercise also shows that counties with a higher population density, hence more urban on average, responded to broadband diffusion by disproportionately supporting the Democratic candidate.

We find instead no particular pattern for turnout. This suggests that the positive impact of broadband internet on political participation does not concern specific groups but is rather homogeneous across the population, at least for observables. Interactions with campaign contributions also do not deliver any significant coefficient. In the interest of space we only report estimates for small donations.

In Table 11 we ask whether our results are driven by the specific campaign dynamics of states with a close race. Hence, we remove from the sample all counties belonging to a state where the 2008 presidential race ended with less than a 5% margin. These states are Florida, Indiana, Missouri, Montana, North Carolina and Ohio. Our results on turnout are robust to this check, with magnitudes that are close to those of Table 9. Null findings for Democratic vote share and campaign contributions are also similar to those of Table 9. Thus, also this check confirms that the positive effect of broadband diffusion on turnout is the only robust finding of our study.

Table 10. IV Estimates: Heterogeneous Effects

Dep variable	Turnout					Small Contributions D					Democratic candidate vote share				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Providers x Young	-2.205					.859					2.179				
	(1.747)					(1.248)					(1.712)				
Providers x Black		-1.156					.219					1.743*			
		(1.257)					(.581)					(1.005)			
Providers x College			-.497					-.031					.41884		
			(1.146)					(.595)					(1.408)		
Providers x Income				-.964					-.559					-1.335	
				(.814)					(.454)					(1.203)	
Providers x Density					-.044					.015					.173*
					(.107)					(.072)					(.098)
Control variables (Δ)	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes		Yes
N	2302	2302	2302		2302	2302	2302	2302		2302	2302	2302	2302		2302

For the list of control variables see the note to Table 2. Standard errors are double clustered at the state and border level. *** significant at 1% level,

Table 11. IV estimates using contiguous county pairs and excluding close races (margin < 5%)

	Vote share D		Turnout		Contributions D		Small contr D		Contributions R		Small contr R	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Providers	0.033	-0.119	1.730**	1.470*	-0.732	-0.489	-0.517	-0.412	0.422	0.287	0.320	0.446
	(0.950)	(0.869)	(0.812)	(0.823)	(0.730)	(0.601)	(0.701)	(0.598)	(0.483)	(0.497)	(0.478)	(0.533)
Control variables (Δ)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (2004)	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Cragg-Donald F-stat	18.025	16.431	18.025	16.431	18.025	16.431	18.025	16.431	18.025	16.431	18.025	16.431
N	1612	1612	1612	1612	1612	1612	1612	1612	1612	1612	1612	1612

For a list of control variables see the note to Table 2. The Stock and Yogo (2005) 10% critical value is 16.38. Standard errors are double clustered at the state and border level. *** significant at 1% level, ** at 5%, * at 10%

7 Conclusion

This paper provides some of the first quantitative empirical evidence on the effect of the rapid rise in internet usage in the U.S. on the basic functioning of the electoral process. We focus on the first decade of the 2000s and in particular on the difference between the 2004 and the 2008 presidential elections, when residential broadband subscriptions saw their largest increase.

The evidence we present in this paper is in "reduced-form": we do not attempt to disentangle the exact behavioural mechanisms leading to any of the aggregate effects we uncover. Instead, our primary purpose here is to provide as much detail as possible on a novel identification strategy that should help solving the endogeneity problem that plagues most of the literature on the political effects of the internet. To this end we have preferred to focus strictly on measurement and identification issues. We hope in this way to provide an identification strategy which can be broadly applied to study in more detail the role of the internet in politics.

The evidence we provide, though mixed, shows that the diffusion of broadband internet across US counties had clear political effects. Going back to the expected outcomes we had outlined in Section 3.3, our most robust finding concerns turnout. The 2008 presidential election saw a marked increase in political participation, especially among young voters. Our findings show that the internet was partially responsible for the increase in turnout of 2008. We also find that this effect was homogeneous across observable characteristics of the US counties and that it did not boost the turnout of young voters more than it did the turnout of older voters. To test robustness, we employ a wide array of empirical strategies and specifications, all showing a positive impact of the internet on turnout. The magnitude is large, implying that an increase by one standard deviation in internet penetration increased turnout by more than 2%.

Internet diffusion is also likely to have provided an advantage to Barack Obama in

2008, as we expected, both in terms of campaign contributions and in terms of vote shares. However, these results are less robust than the turnout case if we focus on average effects only. Our diff-in-diff estimates, which clearly suggest a pro-Democrat effect of broadband diffusion, do not survive our IV strategy based on Right of Way regulations. We show, however, that these nil average effects hide relevant heterogeneities. Interaction terms reveal that, even in IV estimates, there is a pro-Obama impact of the internet in the 2008 election in areas with a high share of African American voters as well as in more densely populated (urban) counties.

Putting our result in perspective, while we do find a robust positive effect of internet diffusion on turnout, with the exception of one study on Malaysia (Miner 2015), all other related studies have reported negative effects of the internet on turnout (Falck et al 2014, in Germany, Campante et al. 2013, in Italy and Gavazza et al. 2017, in the UK). This suggests that the political impact of the internet is likely to vary from country to country and from election to election. Similar considerations should be applied to our other findings. Any positive effect for the Democrats is likely limited to the specific circumstances of the 2004 and 2008 elections, with Barack Obama effectively building on some of the early initiatives of the Democrats in 2004. Although this goes beyond the purposes of this work, it is quite possible that this Democratic advantage has been compensated and possibly reverted by the Republicans in subsequent elections.

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Appendix. Summary statistics of main dependent and explanatory variables

Table A1. Summary statistics of political variables and broadband diffusion (full sample)

Variable	Mean	Std. Dev.	Min	Max
broadband 2000	1.70	0.97	0.00	7.31
broadband 2004	4.30	2.32	0.00	14.46
broadband 2008	9.04	3.06	0.00	21.39
turnout 2000	53.27	9.01	11.76	88.38
turnout 2004	58.52	9.29	13.89	90.42
turnout 2008	58.78	9.00	16.76	93.25
vote Democratic party 2000	39.78	11.72	6.83	86.80
vote Democratic party 2004	38.64	12.40	7.10	84.62
vote Democratic party 2008	41.41	13.71	7.84	88.71
vote Republican party 2000	57.02	11.87	11.77	92.47
vote Republican party 2004	60.36	12.45	12.48	91.97
vote Republican party 2008	57.05	13.67	9.87	92.08
contribution per 1000p 2000 (D)	4.43	17.98	0.00	350.26
contribution per 1000p 2004 (D)	36.61	137.00	0.00	3221.78
contribution per 1000p 2008 (D)	159.03	419.52	0.00	6432.72
contribution per 1000p 2000 (R)	14.17	45.33	0.00	1167.68
contribution per 1000p 2004 (R)	59.28	129.21	0.00	2853.52
contribution per 1000p 2008 (R)	121.21	295.28	0.00	7221.35
small contributions 1000p 2000 (D)	2.35	10.36	0.00	309.02
small contributions 1000p 2004 (D)	15.04	48.50	0.00	1323.95
small contributions 1000p 2008 (D)	83.21	184.07	0.00	3352.76
small contributions 1000p 2000 (R)	4.85	14.41	0.00	317.36
small contributions 1000p 2004 (R)	25.07	65.66	0.00	2646.03
small contributions 1000p 2008 (R)	46.25	64.49	0.00	789.89

The number of observations in the full sample is 3086. Campaign contributions are reported in amounts per 1000 residents while the variable used in the regression is described in footnote 17 in the text.

Table A2. Summary statistics of political variables and broadband diffusion (restricted sample)

Variable	Mean	Std. Dev.	Min	Max
broadband 2004	3.91	2.00	0.00	12.34
broadband 2008	8.60	2.91	0.00	19.64
turnout 2004	58.57	8.73	13.89	87.41
turnout 2008	58.57	8.44	16.76	84.27
vote Democratic party 2004	38.69	12.91	7.90	84.62
vote Democratic party 2008	41.03	14.04	7.84	88.70
vote Republican party 2004	60.27	12.91	12.48	91.97
vote Republican party 2008	57.36	13.98	9.87	91.97
contribution per 1000p 2004 (D)	35.87	118.41	0.00	1647.74
contribution per 1000p 2008 (D)	154.82	420.93	0.00	6325.87
contribution per 1000p 2004 (R)	57.60	113.81	0.00	1671.97
contribution per 1000p 2008 (R)	112.62	214.81	0.00	3617.05
small contributions 1000p 2004 (D)	14.03	38.28	0.00	516.95
small contributions 1000p 2008 (D)	80.14	171.97	0.00	2623.45
small contributions 1000p 2004 (R)	22.91	39.58	0.00	464.65
small contributions 1000p 2008 (R)	43.60	58.17	0.00	789.89

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The restricted sample consists of 2302 observations on 1124 counties (since some counties enter the sample multiple times as explained in Section 6.2). Campaign contributions are reported in amounts per 1000 residents, while the variable used in the regressions is described in detail in footnote 17.

Table A3. TechNet Broadband Index by state

Position	State	Score
1	Michigan	144.35
2	Florida	80.6
3	Missouri	75.65
4	Texas	73
5	Ohio	71.1
6	Washington	70
7	Kansas	67.25
8	Virginia	65.6
9	Colorado	58.45
10	Iowa	55.85
11	Oregon	52.05
12	Arizona	51.2
13	Indiana	47.45
14	California	46.55
15	Illinois	45.3
16	Minnesota	44.95
17	Nebraska	42.7
18	North_Dakota	37.7
19	North_Carolina	35.75
20	South_Carolina	34.3
21	Maine	26
22	Pennsylvania	25.5
23	Alaska	25.15
24	Kentucky	24.25
25	Wisconsin	23
26	New_Hampshire	19.35
27	Tennessee	18.75
28	Alabama	18.4
29	Idaho	18.25
30	Connecticut	17.9
31	Montana	17.25
32	Maryland	16.5
33	District of Columbia	14.65
34	Oklahoma	13.75
35	New_York	11.75
36	Vermont	11.5
37=	South_Dakota	11.25
37=	Wyoming	11.25
39	Massachusetts	10.75
40	Louisiana	9.5
41	New_Mexico	8.5
42=	Hawaii	7.5
42=	New_Jersey	7.5
42=	Rhode_Island	7.5
45	Utah	6.75
46=	Delaware	6.5
46=	Mississippi	6.5
46=	West_Virginia	6.5
49	Georgia	6.25
50=	Arkansas	1
50=	Nevada	1