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E-lections: Voting Behavior and the Internet†

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This paper analyzes the effects on voting behavior of information disseminated over the Internet. We address endogeneity in Internet availability by exploiting regional and technological peculiarities of the preexisting voice telephony network that hindered the roll-out of fixed-line infrastructure for high-speed Internet. We find negative effects of Internet availability on voter turnout, which we relate to a crowding-out of TV consumption and increased entertainment consumption. We find no evidence that the Internet systematically benefits specific parties, suggesting ideological self-segregation in online information consumption. Robustness tests, including placebo estimations from the pre-Internet period, support a causal interpretation of our results. (*JEL* D12, D72, L82, L86)

The emergence of the Internet as the new mass medium of the twenty-first century has changed the mass-media market substantially. Information can now be distributed at high speed, low cost, and more extensively, bringing more egalitarian access to the production and consumption of information. But even though the political economy literature on mass media emphasizes that additional information channels affect whether and how people vote (Strömberg 2004a; Besley and Prat 2006), we cannot draw immediate conclusions for the emergence of the Internet. The Internet may increase or decrease individual exposure to political information, depending on consumers' choice of usage. Despite this being a highly relevant topic, there is but

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little empirical evidence on how the Internet affects voter information and behavior. As a result, Putnam's (2000, p. 173) statement from the beginnings of the Internet era, that "Some of the allegedly greater democracy in cyberspace is based more on hope and hype than on careful research" is still valid today. We contribute to closing this research gap by providing a causal analysis of Internet effects on voting behavior, along with an assessment of the underlying mechanisms.

Without doubt, the Internet has led to a significant reduction in the cost of acquiring information. At the same time, it also provides new entertainment opportunities that might compete for consumers' time. Moreover, the Internet may crowd out other, potentially more informative media, as was the case with the introduction of television (Gentzkow 2006). Therefore, the time an individual spends acquiring information and overall political information might even decrease with the introduction of new media. The possibility of distributing information at a low cost further increased the number of available sources of information. This larger supply of specific information introduces the risk that consumers self-segregate ideologically and, "restrict themselves to their own points of view—liberals watching and reading mostly or only liberals; moderates, moderates; conservatives, conservatives; Neo-Nazis, Neo-Nazis" (Sunstein 2001, pp. 4–5).[1] As a result, people's viewpoints may become harder to change even after being exposed to more information from the Internet.

The identification of a causal effect of the Internet on political behavior is complicated by endogeneity concerns. A profit-maximizing telecommunication carrier will preferably roll out Internet infrastructure in areas where individuals are willing to pay for an Internet subscription. Typically, these areas have a higher average income, a large share of high-skilled workers, and a younger population. Given that these socioeconomic factors are correlated with voting behavior (Sondheimer and Green 2010), any Internet effect observed on voting behavior is potentially biased.

In this paper, we exploit historical peculiarities in the layout of the preexisting voice telephony network that cause exogenous variations in high-speed Internet access to identify its effect on voting behavior.[2] High-speed Internet subscriptions in Germany are almost exclusively based on digital subscriber line (DSL) technology. The first generation of DSL infrastructure was entirely built on the preexisting voice telephony access network, because this significantly reduced the cost of telecommunication infrastructure roll-out. However, since the voice telephony network was not specifically designed for the needs of high-speed Internet, almost one-third of the

[1] Demand-side explanations for ideological media biases may be more important in the context of the Internet, where consumers can filter news themselves, while traditional media with editorial departments may give rise to supply-side explanations for ideological media biases. For instance, Puglisi and Snyder (2011) suggestively ask how much negative news a newspaper will carry about ideologically close politicians, and show that papers with a Republican tendency tend to write more about scandals that involve Democrats, and vice versa. Gentzkow and Shapiro (2010) further show that biases in the US newspaper market are mostly driven by their audiences' ideological leanings. The difference between the Internet and traditional media may, however, disappear once we consider that it is costly to write good news stories online *and* offline. Consequently, editorial departments might still filter online news (Gentzkow and Shapiro 2011).

[2] This strategy is similar to other identification strategies that exploit exogenous variation related to dissemination technologies to identify the effects of new media on voting behavior and political participation. For instance, Strömberg (2004b) uses geological features that affect the quality of radio reception as instruments for the share of households with a radio receiver, and Olken (2009) exploits topographical differences that affect signal strength to identify the effect of exposure to television.

municipalities in West Germany could not readily employ DSL technology because the wires' conductivity limited the transmission of strong-enough signals. Beyond that, 11 percent of the East German population could not access DSL because of a technological error in the roll-out of telecommunication infrastructure in the 1990s. These technological peculiarities provide a unique opportunity to estimate local average treatment effects (LATEs) of access to high-speed Internet on voter mobilization and party support by comparing "unlucky" municipalities that could not readily be supplied with high-speed Internet to otherwise similar counterparts that were "lucky."

We combine administrative data on the outcomes of elections in Germany at different points in time with unique telecommunication data that document the availability of fixed-line broadband infrastructure as a precondition for high-speed Internet access across roughly 12,000 German municipalities. Our identification strategy reveals a negative Internet effect on voter turnout in West Germany but not in East Germany, with the negative Internet effect on voter turnout in West Germany mainly coming from nonlocal elections. To explore whether this pattern observed in voter turnout can be explained by the Internet substituting incumbent media and being primarily an entertainment medium, we employ detailed data on newspaper circulation within municipalities as well as survey information on TV consumption and time spent on entertainment. The Internet does not seem to crowd out newspapers, but we find evidence that the Internet crowds out TV consumption, with TV being the most prominent source of nonlocal political information in Germany, especially in the West. Moreover, we find indications that broadband Internet access indeed increases the amount of time individuals spend on entertainment. Intensified entertainment consumption may compete with the time spent on acquiring information online and offline, or simply distract individuals from voting. Finally, with respect to party support, we do not find that specific parties systematically benefit from the introduction of the Internet. If anything, small nonfringe parties tend to benefit from the Internet. The general pattern is however consistent with the prevalence of self-segregation in online information consumption.

Our findings contribute to a burgeoning empirical literature on the political effects of media that analyze the effect on voter turnout of the entry of newspapers (George and Waldfogel 2006; Snyder and Strömberg 2010; Gentzkow, Shapiro, and Sinkinson 2011), radio (Strömberg 2004b), and television (Gentzkow 2006). Closely related to our paper is Czernich (2012), which presents a first attempt to assess Internet effects on voting behavior. Recently, our findings on voter turnout have been confirmed by Campante, Durante, and Sobbrio (2013), who extend our analysis to the case of Italy. Our findings on specific parties' vote shares further relate to research by DellaVigna and Kaplan (2007); Durante and Knight (2012); Gerber, Karlan, and Bergan (2009); and Enikolopov, Petrova, and Zhuravskaya (2011), as well as to research on ideological segregation in online information consumption (Gentzkow and Shapiro 2011). Finally, our study is closely related to a large empirical literature examining the political economy of mass media, which we do not attempt to summarize here. We instead refer interested readers to DellaVigna and Gentzkow (2010) and Prat and Strömberg (2011) for an overview of this literature.

The remainder of the paper is organized as follows. Section I describes the data used in this study. Section II introduces our empirical estimation strategy and discusses the instruments. Section III presents our main results and discusses our findings in the context of the literature. Section IV presents robustness checks and discusses effect heterogeneity. Section V investigates potential mechanisms. Section VI concludes.

I. Data

A. *Broadband Internet Data for German Municipalities*

We employ data on high-speed Internet availability at the municipality level. Municipalities are the lowest level of territorial division in Germany. Their average size corresponds to a circle of 3.1 km (1.9-mile) radius. Data are taken from the broadband atlas (*Breitbandatlas Deutschland*) published by the Federal Ministry of Economics and Technology (2009), where telecommunication operators self-report the number of households that are covered by their networks at a minimum downstream data transfer rate of 384 kb/s. The data are available for the universe of German municipalities (in 2008 territorial boundaries) for the years 2005 through 2008, a period in which high-speed Internet usage spread rapidly throughout Germany and the number of broadband subscriptions doubled (Bundesnetzagentur 2012). We concentrate on DSL technology, since it is by far the dominant access technology in Germany.[3] It must be kept in mind, however, that our observations focus on the early days of the new technology. In this phase, the World Wide Web was entirely made up of web pages connected by hyperlinks. Web 2.0 applications, for instance, which played an important role in Barack Obama's 2008 presidential race in the United States, were not yet prominent in German election campaigns (Beckedahl, Lüke, and Zimmermann 2009). Accordingly, our analysis identifies the effects of the *introduction* of broadband Internet as a new mass medium and as an additional source of political information, but not the effects of the new social media.

Figure 1 shows the distribution of technical availability of DSL across municipalities for the period 2005 through 2008. While DSL was available for a sizable number of German households in this period, we observe an increase in DSL availability over time with considerable variation across municipalities. This regional variation does not only pick up differences between East and West Germany or urban and rural municipalities, but also technological peculiarities within fairly similar groups of municipalities. We are going to exploit this variation across municipalities when we identify the effects of the Internet on voting behavior. Since our data describe the technical possibility of accessing broadband Internet, we estimate the effects of DSL *availability* and not the effects of DSL *use* on voting behavior. However, in Section V we will show that availability is a good proxy for the actual use of DSL.

B. *Election Data*

We measure Internet effects on two aspects of voting behavior: voter turnout and election decisions. Regarding the second aspect, we distinguish four distinct

[3] Figure F1 in the online Appendix shows the development of DSL and other access technologies from 1999 to 2008. Deutsche Telekom first sold DSL to private households in July 1999. By the end of that year, Deutsche Telekom had 2,900 DSL subscribers (Kopf 2012).

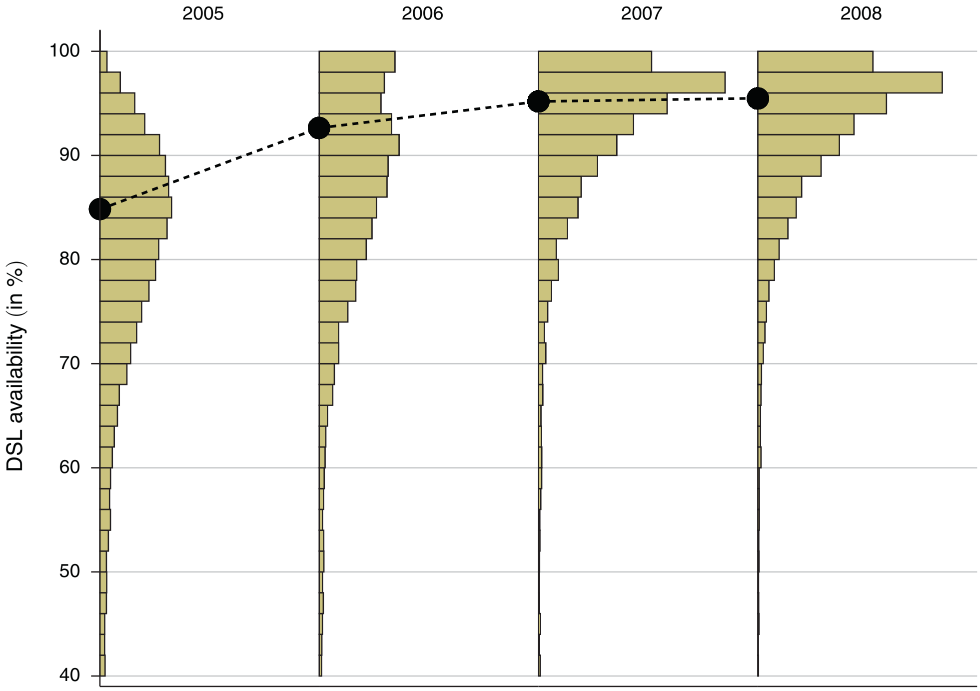

Figure 1. Development of Broadband Internet

Notes: Chart shows histograms of DSL availability (measured as a percentage of households for which DSL is technically available) in all German municipalities for the different years. The chart is truncated at 40 percent. The dotted line connects the population-weighted mean availabilities for all years.

outcomes: (i) the vote share of established parties; (ii) right-fringe parties; (iii) left-fringe parties; and (iv) a residual group of other small parties. Voter turnout allows us to estimate the mobilization effect of the Internet while vote shares indicate whether the Internet has any effect on political preferences. We use election data at the three main levels of governance in the Federal Republic of Germany, i.e., federal elections (*Bundestagswahlen*), state elections (*Landtagswahlen*), and local elections (*Kommunalwahlen*).[4] All election outcomes are observed at the municipality level.[5] We measure Internet effects on voting behavior for the election cycle 2004–2008, when DSL availability expanded rapidly. This election cycle widely overlaps with the availability of our Internet data.[6] For this election cycle, we use information on one federal election, one state election, and one local election per municipality. To assess the Internet effects, we extend our analysis to the 1995–1999 election cycle, where broadband Internet was not yet available. Again, we observe all three election types once per municipality. Finally, we add

[4] Data are compiled from the election statistics of the German states' statistical offices. A detailed description of the election data and our compilation is provided in online Appendix A.

[5] We observe all municipalities according to their 2008 territorial boundaries. Detailed information on how we take territorial reorganizations into account is provided in online Appendix A.

[6] Since we do not have information on DSL availability for the year 2004, we use the information for the year 2005 instead when merging election data with telecommunications data.

TABLE 1—DESCRIPTIVE STATISTICS

	Pre-Internet (1995–1999)		Internet (2004–2008)		Change	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (3) − (1) (5)	SD (6)
Outcome variables						
Voter turnout	73.34	9.30	64.95	11.27	−8.39	7.61
Vote share established parties	84.79	21.37	82.20	22.17	−2.59	8.94
Vote share small parties	11.76	22.57	13.59	23.58	1.83	8.28
Vote share right-fringe parties	3.24	3.42	2.30	2.95	−0.94	2.48
Vote share left-fringe parties	0.20	0.46	1.91	2.51	1.71	2.33
Broadband Internet						
DSL availability (share of households)	0.00	0.00	0.73	0.29	0.73	0.29
Control variables						
Female population share	50.20	1.80	50.10	1.82	−0.10	1.00
Share of population aged 18–65	67.03	3.37	64.38	5.75	−2.65	4.64
Share of population aged 65 and older	16.61	3.33	19.11	3.62	2.50	1.71
Real daily wage (in 2008 €)	88.49	13.15	84.70	12.89	−3.79	3.21
Share of net migration	0.21	1.97	−0.25	1.77	−0.46	2.52
Share of foreigners	2.77	3.86	2.74	3.83	−0.03	1.45
Unemployment rate	6.73	4.55	7.63	5.06	0.90	2.15
Share of unskilled workers	11.17	6.14	9.34	5.28	−1.83	2.83
Share of skilled workers	81.99	6.23	82.68	6.10	0.69	3.21
Share of high-skilled workers	6.84	4.13	7.97	4.61	1.13	2.04
Distance to next urban center (in km)	26.04	15.35	26.04	15.35	0.00	0.00
Municipalities	11,832		11,832		11,832	
Observations	35,391		35,391		35,391	

Notes: Table reports descriptive statistics for the full sample of German municipalities across three types of elections (federal, state, local). Values in percent, unless otherwise specified. The number of observations does not add up to three times the number of municipalities because we have 103 missing observations for local elections in Baden-Wuerttemberg and 2 missing observations for local elections in the city-states of Berlin and Hamburg where local and state elections coincide. Columns 1 and 2 report means and standard deviations from the period before broadband Internet introduction (1995–1999) and columns 3 and 4 report the analogous values for the period after the introduction of broadband Internet (2004–2008). DSL availability refers to the years 2005–2008. Control variables in the pre-Internet period refer to the years 2000 (foreigners, skills, unemployment, wages); or 2001 (remaining variables). Finally, columns 5 and 6 report changes between the two periods.

outcomes of all three election types for the election cycle 1990–1994 to run placebo estimations.

Table 1 shows summary statistics for the election outcomes in the two election cycles 1995–1999 (pre-Internet period) and 2004–2008 (broadband Internet period), where we identify the Internet effects. We see that voter turnout decreased by 8.4 percentage points between the two election cycles. In the period 2004–2008, voter turnout was on average 65 percent, ranging from about 61.4 percent to 74.4 percent depending on election type. Established parties reached over all three election types an average vote share of 82.2 percent in the Internet period, with more than 90 percent in nonlocal elections. In local elections, established parties only reached a vote share of about 66 percent. The vote share of right-fringe parties was on average 2.3 percent, with 3.3 percent in nonlocal elections and close to zero in local elections. Left-fringe parties reached on average 1.9 percent, with 2.6 percent in nonlocal elections and 0.5 percent in local elections.

C. *Socioeconomic Data*

We obtain additional socioeconomic information on the municipality level from two different sources. Information on wages, education, and the share of foreigners stems from the social security records in Germany.[7] Wages are measured as average daily wages of full-time workers in a municipality, expressed in 2008 prices. Based on the social security records, we additionally calculate the share of high-skilled workers (with a tertiary degree); skilled workers (complete secondary education); low-skilled workers (incomplete secondary education); and foreign workers per municipality. The remaining variables at the municipality level are provided by the German Federal Statistical Office. Beside the overall population, these variables include: the female population share; the population share of individuals of working age (aged 18 to 65); the population share of individuals older than 65; the net migration rate, which relates the difference between in- and out-migration to the overall population; and the unemployment rate, which is calculated by dividing the number of unemployed individuals by the working-age population. Descriptive statistics for the socioeconomic characteristics in the pre-Internet period and the Internet period, along with the respective changes, are provided in Table 1. The mean changes (column 5) reveal the aging of the German population as well as German unions' moderation in collective bargaining in the early 2000s.

II. Identification and Empirical Specification

Identifying Internet effects on voting behavior is not straightforward. A simple cross-municipality regression with the key right-hand-side variable being the share of households for which a DSL connection is technically available would suffer from potentially severe omitted-variable bias. Such a bias could most likely arise from differences in the composition of the population between municipalities. Telecommunication carriers face an incentive to roll out broadband infrastructure in areas where individuals have a high willingness to pay for a broadband subscription, typically individuals with high income or education. Simultaneously, these individuals may differ from the average individual in their voting behavior. As a result, cross-municipality estimates of DSL availability on voting behavior would be biased. Specifically, we would expect an upward bias on voter turnout given previous research that finds a positive correlation between turnout and socioeconomic characteristics like income and education (Sondheimer and Green 2010).[8]

The basic framework for our analysis is a first-difference model that compares election outcomes from the early broadband Internet period with election outcomes from the pre-Internet period. The model is of the form:

$$\Delta E_{ie} = \alpha + \beta_1 \Delta DSL_i + \Delta \mathbf{X}_i' \beta_2 + \alpha_t + \alpha_e + \varepsilon_{ie}, \tag{1}$$

[7] See Bender, Haas, and Klose (2000) for a detailed description of the data which are provided by the Institute for Employment Research (IAB). For an additional description of the regional distribution of wages across German municipalities, see Falck, Heblich, and Otto (2013).

[8] Online Appendix Table F1 illustrates the differences in socioeconomic characteristics between municipalities with below-median and above-median DSL availability. In line with our endogeneity concerns, the table reveals that, for example, wages are significantly higher and the unemployment rate is significantly lower in above-median-DSL-availability municipalities than in below-median-DSL-availability municipalities.

where i indexes municipalities and e election types (that is, federal, state, and local elections). ΔE_{ie} is the change of the respective election outcome from the pre-Internet period to the Internet period. The first difference is calculated from the election cycle 1995–1999, when broadband Internet was not yet available, to the election cycle 2004–2008, where it was available. This first-difference model is equivalent to a standard fixed-effects regression with two repeated observations per municipality and election type. First differences are calculated over different time spans, since election years (t) for state and local elections vary across states within a single election cycle. We account for this fact by including dummies for the election years (α_t). α_e are dummies for the election types that control for election-type-specific trends in election outcomes. DSL availability is obviously zero in the pre-Internet period. We thus measure the diffusion of DSL technology, ΔDSL_i, as the share of households in a municipality for which a DSL connection is technically available in the year of election during the Internet-period. $\Delta \mathbf{X}_i$ is a vector that captures changes in the municipalities' socioeconomic characteristics. ε_{ie} is an error term.

While the first-difference model allows us to overcome estimation biases that result from unobserved time-persistent factors at the municipality level, there might still be unobserved time-variant factors that are correlated with both changes in election outcomes and DSL availability across municipalities. We thus instrument DSL availability with regional and technological peculiarities of the traditional public switched telephone network ($PSTN_i$), which affect the possibility to provide DSL in certain municipalities. The first-stage equation is

$$\Delta DSL_i = \alpha + \gamma_1 PSTN_i + \Delta \mathbf{X}_i' \gamma_2 + \alpha_t + \alpha_e + \nu_{ie}. \tag{2}$$

In the following, we introduce three technological features of the traditional PSTN that affect DSL availability in certain municipalities. The early generations of DSL technology in Germany completely relied on the copper wires between the household and the main distribution frame (MDF)—the so-called "last mile" of the PSTN (see Figure F2 in the online Appendix for a graphical illustration of the DSL network). Employing the existing wires offers a significant cost advantage, because wires are usually rolled out subsurface in Germany. The basic structure of the West German PSTN was widely determined in the 1960s, at a time when the provision of telephone services was a state monopoly with the declared goal of providing universal telephone service to German households. At that time, lots and buildings were acquired to host the MDFs while routes for the cable ducts were fixed (Steinmetz and Elias 1979). The latter determine the catchment areas served by one MDF. While dense municipalities always have at least one own MDF, less agglomerated municipalities typically share an MDF. The length of the copper wires was irrelevant for the quality of the telephone services and, accordingly, the choice of MDF locations in less agglomerated areas was determined by such restrictions as the availability of lots and buildings to host one of the more than 6,000 West German MDFs (cf. Figure F3 in the online Appendix). However, in a DSL access network distance does play a crucial role, because the maximum bandwidth depends on the length of the copper wire between the household and the MDF. When a threshold of about 4,200 meters (2.6 miles)[9] is surpassed, DSL technology is no longer feasible and parts of the copper wire must be replaced with fiber wire, which involves costly construction that increases with the length of the bypass.[10]

Our first instrument exploits this technical threshold in the municipalities' distance to the MDF they are connected to. It is a dummy variable that equals unity for municipalities with distances above the threshold of 4,200 meters.[11] The distance is calculated from the geographic centroid of each municipality to the MDF that serves the municipality.[12] We use this instrument in a sample of 3,333 less-agglomerated West German municipalities without an own MDF (i.e., municipalities that are connected to an MDF located in another municipality).[13] By focusing on municipalities without an own MDF, we homogenize the sample of municipalities with respect to their socioeconomic characteristics. The municipalities in our sample account for 40.9 percent of all West German municipalities and 53.1 percent of all less-agglomerated municipalities in West Germany.[14] Thirty-five percent of the municipalities in this sample are above the 4,200-meter threshold. Within our sample, we compare only adjacent municipalities that are connected to the same MDF by including MDF-by-election-type fixed effects in our regressions.[15] Concretely, in our first-difference model, the MDF-by-election-type fixed effects control for catchment-area-specific trends in election outcomes for each type of election. Panel A in Figure 2 graphically illustrates this identification strategy.

A second source of technical variation comes from West German municipalities where the distance to their actual MDF exceeds 4,200 meters. For some of these municipalities, the threshold is less binding because they can be connected to a closer MDF at comparatively low cost. This peculiarity arises from the geographic layout of the MDFs' catchment areas that were designed when distance

[9] Deutsche Telekom only markets DSL subscriptions at the lowest downstream data transfer rate of 384 kb/s if the line loss is less than 55dB. The copper cables used for the "last mile" typically come with a diameter of 0.4 mm. A line loss of 55dB is therefore typically reached at a length of about 4,200 meters along the "last mile." As the actual line loss depends on other factors as well, the 4,200-meter threshold as technological limitation of DSL availability is only a fuzzy threshold.

[10] Rolling out one kilometer of fiber wire subsurface incurs a cost of 80,000 euros, plus an additional 10,000 euros to install a new node where the remaining part of the copper wires is connected to the fiber wire.

[11] Czernich (2012) uses linear distance to the MDF as instrument for DSL availability in both East and West Germany to estimate the Internet effect on voter turnout in the German federal election in 2005. From a technical perspective it is, however, only the 4,200-meter threshold that determines DSL availability, while distance determines the available bandwidth (Prieger and Hu 2008). From an empirical perspective, a potential concern with Czernich's identification strategy is the violation of the exclusion restriction, since pure distance may be correlated with unobserved municipalities' socioeconomic characteristics that are correlated with turnout. This might be especially relevant in East Germany, where location decisions for the MDFs were made in the 1990s (i.e., after reunification), and not in the 1960s as in West Germany.

[12] In an alternative specification, we calculate the distance between the MDF and a municipality's population center instead of its geographic centroid. Doing so accounts for the possibility of an uneven population distribution across the municipality, which would violate the assumption that the geographic centroid approximates a household's average distance to the next MDF. We used Google Earth and the Corine Land Cover database to determine each municipality's population center. We also report results for this alternative specification in Section IIIA.

[13] In this sample, an MDF serves on average four municipalities, excluding the municipality the MDF is located in.

[14] We define less-agglomerated municipalities as municipalities of type 9–17 according to the classification scheme provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR 2007).

[15] We further exclude municipalities that are actually connected to an MDF above the threshold but where another closer MDF is available below the threshold of 4.2 km. For this subset of municipalities, the 4,200-meter threshold does not necessarily impose a restriction on broadband Internet access, since municipalities could be redirected to the closer MDF. We exploit this peculiarity in a second instrumental variable specification.

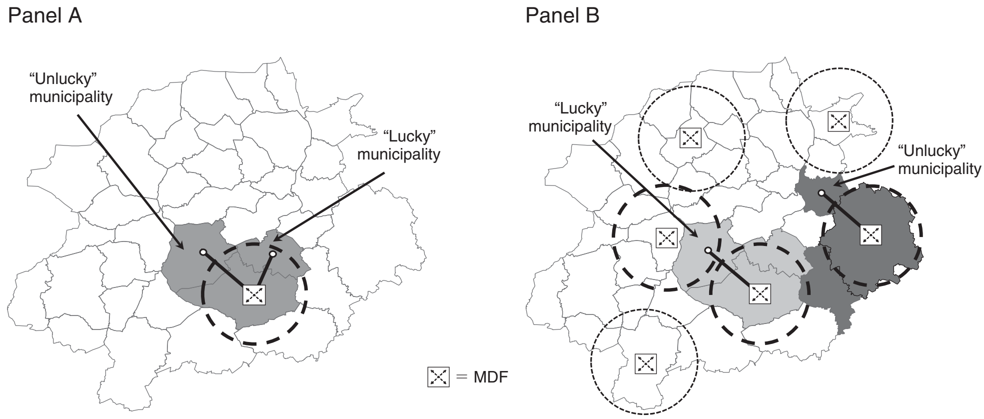

FIGURE 2. GRAPHICAL ILLUSTRATION OF THE DISTANCE INSTRUMENTS

Notes: In panel A, the three gray-shaded West German municipalities are served by the same MDF. The circle represents the 4,200 m threshold around the MDF. While large parts of the northwestern municipality without an own MDF lies above the threshold, large parts the northeastern municipality without an own MDF lies below the threshold. As a result, technical DSL availability is higher in the northeastern municipality than in the northwestern municipality. In panel B, the map illustrates the catchment areas (light gray and dark gray shaded) of two different West German MDFs. The circles represent the 4,200 m threshold around an MDF. In both catchment areas, large parts of the northwestern municipalities lie above the threshold. However, while for the light gray shaded northwestern municipality a significantly closer MDF is available, the actual MDF is also the closest MDF for the dark gray shaded northwestern municipality. As a result, technical DSL availability is higher in the light gray shaded northwestern municipality that can be reconnected to a closer MDF than in the dark gray shaded northwestern municipality lacking this option.

was irrelevant. At that time, organizational considerations determined the allocation of municipalities to a particular MDF. As a result, we observe some municipalities that are currently served by an MDF located more than 4,200 meters distant while a neighboring MDF is available below the 4,200-meter threshold. In these cases, it is often cheaper for the telecommunication carrier to provide DSL via the closer MDF than via the "wrong" MDF that actually serves the municipality. Still, both procedures imply costly construction to roll out new wires. As a result, the advantage of one procedure over the other depends on geological and geographic features as well as on the distance to the actual MDF. We construct a dummy variable that equals unity for municipalities above the threshold which could not be connected to another MDF at a distance below 4,200 meters. Conditional on distance to the actual MDF, we use this dummy variable as an instrument for technical DSL availability in a sample of 1,800 less-agglomerated West German municipalities without an own MDF and with a distance to their actual MDF greater than 4,200 meters. This sample accounts for 22.1 percent of all West German municipalities and 28.7 percent of the less-agglomerated municipalities in West Germany. Within this sample, 84.3 percent of the municipalities do not have a closer MDF that could be used for broadband deployment. We expect the "No Closer MDF" dummy variable to have a significantly negative impact on DSL availability. But a closer MDF could also be closer to an urban center. To assure that the effects of the availability of a closer MDF are not confounded with this proximity, we further control for the municipalities' distance to the next urban center in this setup. Panel B in Figure 2 graphically illustrates this second identification strategy. While we exploit within MDF-catchment-area variation in our first identification strategy (cf. panel A in Figure 2), this second identification strategy exploits variation across municipalities being located in different MDF catchment areas.

The third source of technical variation is relevant for East German municipalities only. Here, the idea of as-good-as-random MDF locations is potentially not justifiable, since the public switched telephone network was only modernized after German reunification in 1990. Consequently, our first two instruments are not applicable to East Germany. Instead, we introduce a third instrument. After German reunification, the German Postal Ministry, together with *Deutsche Bundespost* (the state-owned telecommunication monopolist at that time), decided to roll out telephone infrastructure on the basis of a special type of fiber wires, the so-called OPAL technology, instead of the traditional copper wires. In the early 1990s, this technology was considered state-of-the-art and was expected to dominate the future of communications technology. It is suited for voice-telephony services and a limited amount of data transmission especially in denser areas. As part of a subsidy scheme to support the reconstruction of East Germany (*Aufbau Ost*), the European Investment Bank (EIB) granted a sponsored loan of more than 500 million euros to Deutsche Bundespost in 1994 to roll out OPAL infrastructure (EIB 2013). OPAL was eventually rolled out in 213 East German catchment areas, covering about 11 percent of the East German population. But then things changed dramatically. With the Internet becoming a mass phenomenon, services demanded higher and higher bandwidths. Eventually, DSL became the technical standard for broadband Internet in Germany.

This development was the misfortune for the thought-to-be-high-tech OPAL areas, because OPAL technology is not compatible with DSL technologies. To nonetheless provide broadband Internet access in OPAL areas, two very costly alternatives were feasible. One was to replace the OPAL wires in the access network with copper wires; the other, install new hardware and software at the networks' nodes (with the latter alternative being even more expensive). We exploit this accident by constructing a dummy variable as an instrument that equals unity for East German municipalities situated in an original OPAL area. We expect this dummy to have a significantly negative impact on DSL availability. We further restrict the sample to the 1,249 municipalities whose distance to the actual MDF is below 4,200 meters. In these municipalities, DSL would be easily available if they had not been located in an OPAL area.

The sample accounts for 34 percent of all East German municipalities, out of which 13.4 percent are connected to an OPAL node. Given that OPAL technology is especially suited for denser areas, our subsample of East German municipalities also includes more urban municipalities, leading to a right-tailed size distribution in this sample. Figure F3 in the online Appendix maps the original OPAL nodes in East Germany. The figure reveals that OPAL nodes are distributed across the entire East German territory, with some concentration in the northern parts of Saxony (around Leipzig), western parts of Brandenburg, and Berlin. To account for the fact that unobserved regional factors might explain this uneven spatial distribution of OPAL nodes, we only compare municipalities within the same county by including county-fixed effects in all East German regressions. More precisely, the county fixed effects control for county-specific trends in election outcomes in our first-difference model.[16]

Since our three instruments are dummy variables, we identify local average treatment effects for the "compliant municipalities" in all instrumental variable specifications, whereby the compliant municipalities are expected to have a lower DSL availability because of the technical features predetermined by the traditional voice telephony network. The interpretation of our results as local average treatment effects leads us in the following sections to label our three samples LATE 1 with the 4,200-meter-threshold dummy as instrument, LATE 2 with the "No Closer MDF" dummy as instrument, and LATE 3 with the OPAL dummy instrument.

III. Internet Availability and Voting

A. *Basic Results*

We analyze the effects of Internet availability on election outcomes using the LATE 1, LATE 2, and LATE 3 samples described above. As detailed in Section II, each specification is built around one instrument and involves a subsample of municipalities where the instrument applies.[17] Since all instruments are measured on the municipality level, we cluster standard errors on the municipality level. For each sample, we report results for our first-difference model. Since we use samples of quite homogeneous and (at least in the West German samples) less-agglomerated municipalities, we chose not to weight observations by population size. We thus identify broadband Internet effects for the average treated municipality. The upper parts of Tables 2 to 4 show the coefficients of DSL availability. Each cell shows the results from a separate regression. The lower parts of the tables report the first-stage results, if applicable.

Table 2 reports the results for the LATE 1 sample. Throughout the specifications, we report estimation results with and without control variables. For a better understanding of potential biases, columns 1 and 2 show the OLS results for the first-difference model.[18] IV results are shown in columns 3 to 8, where we instrument DSL availability with the 4,200-meter-threshold dummy. Columns 3 and 4 report instrumental variable estimates, where distance calculations are based on municipalities' geographic centroid. In columns 5 and 6, we further restrict the sample to municipalities within a band of $+/-2{,}000$ meters around the threshold. In columns 7 and 8, we employ an alternative distance measure based on municipalities' population center. The lower part of the table shows the first-stage results from regressions of DSL availability on our threshold instrument. Conditional on MDF-by-election-type fixed effects and control variables, municipalities above the threshold have on average a 10.3 to 15.0 percentage point lower DSL availability. F-statistics of excluded instruments ranging from 140.22 to 262.86 underline the

[16] One concern may be that the German Postal Ministry prioritized municipalities with specific socioeconomic characteristics and higher growth potential for OPAL roll-out. However, Table D1 in the online Appendix shows that, conditional on county fixed effects, treatment and control areas do not show any statistically significant differences in a broad range of socioeconomic characteristics. This clearly supports our instrument's validity.

[17] Detailed descriptive statistics by subsample are reported in Tables F2 to F4 in the online Appendix.

[18] Additional reduced-form estimates are reported in Table F5 in the online Appendix.

Table 2—Basic Estimation Results for the LATE 1 Sample

	OLS First-difference model		IV Geographic centroid		IV 2,000m around threshold		IV Population center	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ voter turnout	−0.401	−0.407	−3.347***	−3.558***	−4.380***	−4.772***	−3.219***	−3.423***
	(0.334)	(0.332)	(0.984)	(0.989)	(1.488)	(1.501)	(0.949)	(0.953)
Δ vote share established parties	−0.304	−0.329	−1.257	−1.368	−0.283	−0.440	−1.054	−1.081
	(0.285)	(0.284)	(0.971)	(0.973)	(1.448)	(1.456)	(0.930)	(0.929)
Δ vote share small parties	0.333	0.342	1.962**	2.009**	1.617	1.630	1.682**	1.658**
	(0.234)	(0.234)	(0.868)	(0.870)	(1.292)	(1.300)	(0.821)	(0.821)
Δ vote share right-fringe parties	0.045	0.055	−0.312	−0.277	−0.477	−0.369	−0.063	−0.033
	(0.107)	(0.106)	(0.263)	(0.268)	(0.394)	(0.402)	(0.259)	(0.263)
Δ vote share left-fringe parties	−0.073	−0.067	−0.392	−0.364	−0.857*	−0.820*	−0.565*	−0.544*
	(0.106)	(0.106)	(0.304)	(0.308)	(0.462)	(0.465)	(0.299)	(0.301)
First stage								
Threshold	—	—	−0.142***	−0.141***	−0.104***	−0.103***	−0.150***	−0.149***
			(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
F-test of excluded instruments	—	—	255.11	252.88	141.98	140.22	262.86	262.22
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Number of MDFs	869	869	869	869	742	742	869	869
Municipalities	3,333	3,333	3,333	3,333	2,649	2,649	3,339	3,339
Observations	9,923	9,923	9,923	9,923	7,882	7,882	9,941	9,941

Notes: Table reports municipality-level pooled regressions for elections at three main levels of governance in Germany: federal elections, state elections, and local elections. The sample consists of West German municipalities without an own MDF which are actually connected to the closest MDF. Each cell in the upper part of the table shows the coefficient from a separate regression of an election outcome on DSL availability. Columns refer to different models and rows refer to different outcome variables and test statistics. Columns 1 and 2 show first-difference estimates; columns 3 and 4 show instrumental-variable estimates where distance calculations are based on municipalities' geographic centroid; and columns 5 and 6 show the same regressions on a sample that is restricted to a band of 2,000 m around the threshold of 4,200 m, i.e., between 2,200 and 6,200 m from the MDF. Columns 7 and 8 show instrumental-variable estimates with an alternative distance measure based on municipalities' population center. Since we exclude municipalities that could be connected to a closer MDF from the LATE 1 sample, the sample size changes when we employ another distance measure. The instrument in columns 3–8 is a threshold dummy indicating whether a municipality is more than 4,200m away from its MDF (1 = lower probability of DSL availability), and 0 otherwise. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-statistic. The five election outcomes are the changes in voter turnout, vote share of established parties, vote share of small parties, vote share of right-fringe parties, and vote share of left-fringe parties between the pre-Internet period (1995–1999) and the Internet period (2004–2008). All specifications include election type dummies, MDF-by-election-type dummies, and year dummies. Specifications in columns 2, 4, 6, and 8 additionally control for changes in the following municipality characteristics: female population share, share of population aged 18–65, share of population older than 65, average wage, net migration rate, share of foreigners, unemployment rate, and share of unskilled, skilled, and high-skilled workers. Descriptive statistics for the LATE 1 sample are provided in online Appendix Table F2. All standard errors are clustered on the municipality level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

high relevance of the instrument. In all IV specifications, the effect of DSL availability on voter turnout is significantly negative and lower than the OLS estimate, which implies an upward bias in the OLS estimations. This result is in line with our concern that individuals with higher probability of voting are overrepresented in areas with broadband Internet access. The effects of DSL availability on party vote shares are in most specifications not significantly different from zero. Only in some specifications do we find indications for a negative effect of DSL availability on the vote share of left-fringe parties and a positive Internet effect on the vote share of small parties.

Table 3—Basic Estimation Results for the LATE 2 Sample

	OLS First-difference model		IV Geographic centroid		IV Population center	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ voter turnout	0.525	0.387	−3.684**	−3.367*	−2.845*	−3.096*
	(0.352)	(0.366)	(1.795)	(1.835)	(1.570)	(1.640)
Δ vote share established parties	0.755*	0.741*	−1.805	−2.099	−0.267	−0.286
	(0.402)	(0.420)	(2.286)	(2.293)	(2.021)	(2.069)
Δ vote share small parties	0.111	0.053	1.142	1.468	1.209	1.224
	(0.346)	(0.360)	(2.037)	(2.045)	(1.835)	(1.878)
Δ vote share right-fringe parties	−0.370***	−0.447***	−0.075	0.124	−0.734	−0.750
	(0.125)	(0.130)	(0.687)	(0.689)	(0.580)	(0.597)
Δ vote share left-fringe parties	−0.497***	−0.347***	0.738	0.508	−0.208	−0.189
	(0.130)	(0.132)	(0.555)	(0.540)	(0.509)	(0.535)
First stage						
"No Closer MDF"	—	—	−0.138***	−0.136***	−0.139***	−0.134***
	—	—	(0.010)	(0.010)	(0.011)	(0.011)
F-test of excluded instruments	—	—	191.98	180.40	163.31	150.38
Control variables	No	Yes	No	Yes	No	Yes
Municipalities	1,800	1,800	1,800	1,800	1,742	1,742
Observations	5,362	5,362	5,362	5,362	5,191	5,191

Notes: Table reports municipality-level pooled regressions for elections at three main levels of governance in Germany: federal elections, state elections, and local elections. The sample consists of West German municipalities without an own MDF which are connected to an MDF above the 4,200 m threshold. Each cell in the upper part of the table shows the coefficient from a separate regression of an election outcome on DSL availability. Columns refer to different models and rows refer to different outcome variables and test statistics. Columns 1 and 2 show first-difference estimates; columns 3 and 4 show instrumental-variable estimates where distance calculations are based on municipalities' geographic centroid; and columns 5 and 6 show instrumental-variable estimates with an alternative distance measure based on municipalities' population center. The instrument in columns 3–6 is a dummy variable that equals unity if a municipality is connected to an MDF at more than 4,200 m distance and cannot be connected to another MDF that is closer than 4,200 m (1 = lower probability of DSL availability), and zero otherwise. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-statistic. The five election outcomes are the changes in voter turnout, vote share for established parties, vote share for small parties, vote share for right-fringe parties, and vote share for left-fringe parties between the pre-Internet period (1995–1999) and the Internet period (2004–2008). All specifications include election type dummies, year dummies, and controls for the distance to the actual MDF and the distance to the next urban center. Specifications in columns 2, 4, and 6 additionally control for changes in the following municipality characteristics: female population share, share of population aged 18–65, share of population older than 65, average wage, net migration rate, share of foreigners, unemployment rate, and share of unskilled, skilled, and high-skilled workers. Descriptive statistics for the LATE 2 sample are provided in the online Appendix's Table F3. All standard errors are clustered on the municipality level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 3 reports the results for our LATE 2 sample. DSL availability is now instrumented with a dummy that equals unity if the municipality is connected to an MDF more than 4,200 meters distant and could not be connected to an MDF located less than 4,200 meters away. Again, the first stage results show the relevance of our instrument. Conditional on control variables, municipalities above the 4,200-meter threshold that are connected to an MDF where no closer MDF is available have on average a 13.4 to 13.9 percentage point lower DSL availability than municipalities above the threshold that could be reconnected to a closer MDF. *F*-statistics of excluded instruments range between 150.38 and 191.98. The results for the effects of DSL availability on voter turnout in this IV specification are remarkably similar

Table 4—Basic Estimation Results for the LATE 3 Sample

	OLS First-difference model		IV Geographic centroid		IV Population center	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ voter turnout	0.180	0.153	1.696	0.730	−0.160	−0.922
	(0.395)	(0.394)	(4.261)	(4.172)	(3.774)	(3.784)
Δ vote share established parties	1.230*	1.227*	1.242	0.850	−0.350	−0.607
	(0.671)	(0.672)	(7.621)	(7.513)	(6.597)	(6.607)
Δ vote share small parties	−0.951	−0.967	0.128	0.515	2.105	2.410
	(0.660)	(0.659)	(7.440)	(7.318)	(6.471)	(6.466)
Δ vote share right-fringe parties	−0.277**	−0.261*	−1.071	−1.076	−1.400	−1.456
	(0.133)	(0.134)	(1.673)	(1.613)	(1.498)	(1.485)
Δ vote share left-fringe parties	−0.002	0.000	−0.299	−0.289	−0.355*	−0.348*
	(0.016)	(0.016)	(0.195)	(0.191)	(0.182)	(0.182)
First stage						
OPAL	—	—	−0.079***	−0.080***	−0.088***	−0.088***
	—	—	(0.026)	(0.026)	(0.026)	(0.026)
F-test of excluded instruments	—	—	9.05	9.54	11.50	11.61
Control variables	No	Yes	No	Yes	No	Yes
Number of counties	63	63	63	63	63	63
Municipalities	1,249	1,249	1,249	1,249	1,279	1,279
Observations	3,747	3,747	3,747	3,747	3,837	3,837

Notes: Table reports municipality-level pooled regressions for elections at three main levels of governance in Germany: federal elections, state elections, and local elections. The sample consists of East German municipalities with distance to the actual MDF below 4,200 m. Each cell in the upper part of the table shows the coefficient from a separate regression of an election outcome on DSL availability. Columns refer to different models and rows refer to different outcome variables and test statistics. Columns 1 and 2 show first-difference estimates; columns 3 and 4 show instrumental-variable estimates where distance calculations are based on municipalities' geographic centroid; and columns 5 and 6 show instrumental-variable estimates with an alternative distance measure based on municipalities' population center. The instrument in columns 3–6 is a dummy variable that equals unity if a municipality was initially supplied with OPAL technology (1 = lower probability of DSL availability), and zero otherwise. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-statistic. The five election outcomes are the changes in voter turnout, vote share of established parties, vote share of small parties, vote share of right-fringe parties, and vote share of left-fringe parties between the pre-Internet period (1995–1999) and the Internet period (2004–2008). All specifications include election type dummies, county-by-election-type dummies, and year dummies. Specifications in columns 2, 4, and 6 additionally control for changes in the following municipality characteristics: female population share, share of population aged 18–65, share of population older than 65; average wage, net migration rate, share of foreigners, unemployment rate, and share of unskilled, skilled, and high-skilled workers. Descriptive statistics for the LATE 3 sample are provided in online Appendix Table F4. All standard errors are clustered on the municipality level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

to the results for the LATE 1 sample. Only the standard errors are somewhat higher. In contrast to the LATE 1 sample, we do not find any evidence of DSL availability systematically benefiting any type of parties. The results are not affected by the inclusion of control variables or the way distances are calculated.

Finally, Table 4 reports the results for our East German LATE 3 sample. Here, our instrument is a dummy variable indicating OPAL areas. Conditional on control variables, DSL availability turns out to be 7.9 to 8.8 percentage points lower in OPAL areas. The *F*-statistics of excluded instruments range between 9.05 and 11.61 and are thus lower than in the LATE 1 and LATE 2 samples. We do not find any evidence that DSL availability systematically affects election outcomes in this sample.

All coefficients of DSL availability are—except for the vote share of left-fringe parties in the specification with distance calculation based on municipalities' population center—not significantly different from zero. Moreover, we often observe comparatively large standard errors relative to the coefficients' size.

B. *Discussion of the Results in Context of the Literature*

Internet effects on vote shares of any type of parties are in general not significantly different from zero. This suggests that self-segregation may be an issue in online information consumption, while the Internet does not seem to foster ideological polarization.[19] If partisans consume information according to their political preferences, it will not affect party support.[20] However, in some specifications, we find significantly positive Internet effects on small parties' vote share and significant negative Internet effects on the vote share of left-fringe parties. Gentzkow and Shapiro's (2011) work on online news consumption provides a possible explanation for these results. They find that a significant share of online consumers receive their information from multiple outlets. This might increase the voters' probability of being confronted with information on small parties and thus affect the election decision, even if voters used to support other parties and selected their information accordingly. Gentzkow and Shapiro (2011) also find that websites with ideologically extreme contents are of second-order importance, which may explain why parties on the fringes cannot benefit from the Internet.

Even though we do not find a systematic pattern in the Internet effect on *who* people vote for, we find that the Internet demobilizes potential voters in West Germany (LATE 1 and LATE 2 sample).[21] The negative Internet effect on voter turnout is consistent with Gentzkow's (2006) argument that the emergence of a new medium may crowd out overall political information that had previously been provided by the incumbent media. The extent to which incumbent media had previously been a source for political information might however differ across potential voters and may thus offer an explanation for the differences between East and West Germany. We will explore the potential crowding-out mechanism behind the negative Internet effect on voter turnout in more detail in Section V. Interestingly, none of the effects mentioned above are affected by whether preelection polls predicted the elections to come out close or not.[22]

One likely explanation for the large standard errors in the LATE 3 sample relates to East Germany's history. In the pre-Internet period—which is the basis for our first-difference model—East Germany had experienced less than 10 years of democracy after 40 years of socialist rule. Even in reunified Germany, the Socialist experience continues to affect East Germans' beliefs and economic behavior (Alesina and

[19] Campante and Hojman (2013) find that the introduction of radio and TV led to a reduction in ideological polarization in the United States in the mid-twentieth century.

[20] This interpretation finds additional support in separate regressions for the incumbent parties and for potential coalitions between these parties. The results are reported in Table F6 in the online Appendix. Internet effects on any vote share are not significantly different from zero.

[21] By contrast, Czernich (2012) measures a positive effect of Internet availability on voter turnout in the federal elections in Germany 2005. However, for the reasons previously discussed, her distance-based IV approach is likely to result in upward-biased estimates.

[22] Polls are available for nonlocal elections only. See online Appendix B for details.

Fuchs-Schündeln 2007). Specifically, a decade was not enough for East Germans to develop strong partisanship, leading to rather volatile voting behavior (Kopstein and Ziblatt 2006; Pacek, Pop-Eleches, and Tucker 2009). Also, East German voters had far less experience with free media. Consequently, patterns of media consumption may not have been fully established when the Internet era began.

Given that we find remarkably similar Internet effects on voter turnout in our LATE 1 and LATE 2 samples, we are confident that we can generalize the identified local average treatment effects to all less-agglomerated municipalities in West Germany that characterize our LATE 1 and LATE 2 samples. Accordingly, we can evaluate the magnitude of the negative effects of DSL availability on voter turnout from a perspective over time. Between the period before and after the introduction of broadband Internet, voter turnout decreased on average by 7.5 (7.2) percentage points in the LATE 1 (LATE 2) sample. During the same period, DSL availability increased by 77.2 (72.7) percentage points. Multiplying the change in DSL availability with the negative effect of DSL availability on voter turnout of -3.6 (-3.4) percentage points in the LATE 1 (LATE 2) sample (IV specifications on the basis of the geographic centroid) and relating the result to the overall decrease in voter turnout in the two samples implies that DSL expansion explains more than one-third of the observed decrease in voter turnout.

The magnitude of our effects is comparable to the effect size in Gentzkow (2006). He studies the effect of the introduction of television in the United States on voter turnout in an attempt to explain the observed decrease in voter participation during the period from 1940 to 1970. Using variation across regions in the timing of the introduction of this mass medium, he shows that the introduction of television had a negative effect on voter participation.[23] The estimations imply that the introduction of television in the 1940s and 1950s reduced the turnout in congressional races (without simultaneous presidential elections) by two percentage points. The overall trend in turnout since the 1950s was -3.4 percent per decade, implying that television accounted for nearly 60 percent of the decline.

Our results further imply that the decrease in turnout among those individuals who are exposed to the Internet must be even larger than the average effect on voter turnout. This is because overall turnout in the Internet period (T_1) is a weighted sum of turnout among individuals (I) exposed to the Internet and individuals (NI) not exposed to the Internet, i.e., $T_1 = \gamma T_1^I + (1-\gamma) T_1^{NI}$. Weights are the population shares of the two groups of individuals exposed (γ) and not exposed ($1-\gamma$) to the Internet. Survey evidence presented in Section V suggests that we can safely equate the share of the population exposed to the Internet with the share of households for which DSL is technically available ($\gamma = DSL$). To calculate the change in turnout among those individuals who are exposed to the Internet, i.e., the mobilization effect ($T_1^I - T_0$), we finally need an estimate for T_1^{NI}. We derive this estimate from the turnout estimations (cf. equation (1)). In the complete absence of high-speed Internet, our estimates imply that overall turnout would differ from its actual value by

[23] By contrast, Oberholzer-Gee and Waldfogel (2009) as well as Prat and Strömberg (2005) find positive effects of TV consumption on voter turnout; Strömberg (2004b) finds positive effects of the introduction of the radio on voter turnout; and Gentzkow, Shapiro, and Sinkinson (2011) find positive effects of increased newspaper coverage on voter turnout.

$\beta_1 DSL$, and we could fully ascribe this value to individuals not exposed to the Internet, i.e., $\hat{T}_1^{NI} = T_1 - \beta_1 DSL$, since the group of individuals exposed to the Internet is nonexistent in this counterfactual situation. Our estimations then imply that the demobilization effect among individuals exposed to the Internet is a decrease of 8.3 (8.1) percentage points in the LATE 1 (LATE 2) sample. Relating this decrease to the initial turnout of 75.8 (76.1) percent in the pre-Internet period, this implies a negative persuasion rate of 10.9 (10.7) percent. This magnitude is in line with the persuasion rates on voting behavior that DellaVigna and Gentzkow (2010) report for different media. Moreover, Campante, Durante, and Sobbrio (2013) find the same negative Internet effect on turnout for Italy for the period we observe. In their case, the effect translates into a demobilization effect of about 8 percent.

IV. Validity of the Identification Strategy and Effect Heterogeneity

A. *Placebo Estimation and Alternative Outcomes*

Throughout our regressions, controlling for relevant municipalities' socioeconomic characteristics hardly affects the DSL coefficient in our instrumental variable estimations. This provides confidence that the exclusion restriction of our instruments is not violated. We now go one step further and explore systematically whether our instruments are correlated with election trends in the pre-Internet period or with changes in the municipalities' socioeconomic characteristics. First, we run reduced-form regressions of our IV specifications, where the left-hand-side variable is the change of one of the respective election outcomes in the pre-Internet period, i.e., between the election cycles 1990–1994 and 1995–1999. The right-hand-side variable of interest is one of our instruments that determines future DSL availability in the respective municipality. This is a placebo specification, since the roll-out of broadband infrastructure after the year 2000 cannot affect changes in election outcomes in the 1990s. Due to data limitations, we cannot control for other municipalities' socioeconomic characteristics than the change in the overall population and the female population share in the placebo specification.[24]

Second, we run reduced-form estimations where we stepwise regress the change in one socioeconomic characteristic (used as a control variable in our baseline regressions) on one of our instruments, while all the other control variables remain unchanged. This allows us to test whether our instruments are systematically correlated with changes in municipalities' socioeconomic characteristics. A correlation between our instruments and the changes in socioeconomic municipality characteristics would indicate a potential violation of the exclusion restriction. Put differently, if the observables are correlated with our instruments, the same could apply to unobservable variables.

The latter reduced-form estimations do not just underline the validity of our instruments. They also shed light on another potential mechanism through which the

[24] The previous estimations with and without controls (Tables 2–4) as well as the robustness checks in the next subsection (panel B of Table 5) show that (i) changes in the controls have almost no effect on the estimations, and that (ii) changes in the controls are uncorrelated with the instruments. Accordingly, the lack of controls for the pre-Internet period should not affect the placebo estimations.

Internet might causally affect voter turnout. The opportunity cost of time use implies that activities that provide political information are substitutive to labor market activity (Charles and Stephens Jr. 2013). As a consequence, a positive Internet effect on labor market activity (i.e., an increase in wages and a reduction in unemployment) would go hand-in-hand with a crowding-out of the time individuals spend on acquiring political information, which then lowers voter turnout.

The results of these two exercises are shown in Table 5. Each cell shows the DSL coefficient from a separate reduced-form regression. None of the coefficients in the placebo specifications in panel A is significantly different from zero. The point estimates are also close to zero and well below the significant reduced-form coefficients of our basic results (cf. Table F5 in the online Appendix).[25]

Panel B of Table 5 presents results of the regressions on the control variables. As it turns out, our instruments are not systematically correlated with changes in municipalities' socioeconomic characteristics.[26] Out of 30 coefficients, 2 coefficients are significant at the 10 percent level and only 1 at the 5 percent level. The probability that 2 or more out of 30 coefficients will be significant at a 10 percent level by chance is 81.6 percent. The probability that 1 or more out of 30 coefficients will be significant at the 5 percent level by chance is 78.5 percent. If the coefficients are significantly different from zero, the coefficient size is small in economic terms, e.g., in the LATE 1 sample, the share of population aged 18–65 changes in the treated municipalities by 0.18 percentage points more than in the control municipalities. The regressions on the control variables support evidence from the United States of limited economic benefits of broadband expansion for single locations (Forman, Goldfarb, and Greenstein 2012; Kolko 2012). The regressions on the control variables additionally support the conclusion that the Internet does not affect voter turnout through a labor-market-induced increase in the opportunity cost of time.

B. *Effect Heterogeneity across Election Types*

We now turn to the heterogeneity of the effects of DSL availability on voting outcomes between nonlocal and local elections. Table 6 shows the effects of DSL availability on the voting outcomes of interest across the two election types. As in the case of pooled regressions, we find a systematic negative effect on voter turnout in nonlocal elections in the two West German subsamples.[27] However, for local elections, we find a much smaller and imprecisely measured coefficient.

[25] In online Appendix C, we confirm these findings in an "event study" version of our reduced-form regressions.

[26] In online Appendix D, we explore differences in the pre-Internet *levels* of the socioeconomic characteristics between treatment and control municipalities. We do not find that these differences drive our results when controlling for pre-Internet levels of the socioeconomic characteristics in our first-difference model.

[27] Note that our OPAL instrument turns out weak in the local election estimation for the East German LATE 3 sample. This is because four out of five East German local elections in the Internet period are observed in the year 2004. At this time, average DSL availability was still low in East Germany, even in the non-OPAL areas (on average 65 percent of East German households in non-OPAL municipalities could technically access DSL at that time, compared to 85 percent of all German households (cf. Figure 1)). Thus, most East German households could only access narrow-band Internet (e.g., ISDN) and living in an OPAL area was not too much of a limiting factor. Investigating elections in the LATE 3 sample that took place later in the Internet period consequently results in a much stronger first stage with an F-test of excluded instruments of roughly 30. However, in all regressions, the DSL coefficients are still not significantly different from zero. Specifically, we find positive but insignificant effects on voter turnout compared to the negative and significant effects in West German samples. This finding also holds for nonlocal elections in the LATE 3 sample that took place in the late Internet period.

Table 5—Validity Tests

	LATE 1 Threshold (1)	LATE 2 No closer MDF (2)	LATE 3 OPAL (3)
Panel A. Pre-Internet period			
Δ voter turnout	−0.105	−0.012	0.081
	(0.113)	(0.167)	(0.360)
Δ vote share established parties	−0.060	0.195	0.011
	(0.114)	(0.197)	(0.662)
Δ vote share small parties	0.085	−0.134	−0.208
	(0.100)	(0.182)	(0.643)
Δ vote share right-fringe parties	−0.043	−0.007	0.180
	(0.036)	(0.077)	(0.144)
Δ vote share left-fringe parties	0.001	−0.009	0.003
	(0.010)	(0.016)	(0.004)
Panel B. Broadband-Internet period			
Δ female population share	0.002	−0.052	−0.137
	(0.046)	(0.073)	(0.088)
Δ share of population aged 18–65	0.178**	0.117	−0.137
	(0.070)	(0.100)	(0.121)
Δ share of population aged 65 and older	−0.058	−0.052	0.027
	(0.052)	(0.073)	(0.097)
Δ daily wage (2008 = 1)	−0.193	−0.103	0.029
	(0.122)	(0.215)	(0.214)
Δ share of net migration	0.042	0.143	−0.098
	(0.112)	(0.150)	(0.214)
Δ share of foreigners	0.056	0.199*	0.071
	(0.070)	(0.106)	(0.045)
Δ unemployment rate	−0.039	0.010	0.330
	(0.053)	(0.080)	(0.230)
Δ share of unskilled workers	0.012	−0.106	0.155
	(0.131)	(0.183)	(0.201)
Δ share of skilled workers	0.085	0.299	−0.141
	(0.150)	(0.208)	(0.251)
Δ share of high skilled workers	−0.097	−0.193*	−0.014
	(0.085)	(0.115)	(0.161)
Municipalities	3,333	1,800	1,249
Observations	9,923	5,362	3,747

Notes: Table reports municipality-level pooled regressions for two types of validity tests. Values in percent. Panel A reports reduced-form placebo regressions of changes in election outcomes in the pre-Internet period, i.e., between the period 1990–1994 and 1995–1999, on the three instruments. Each cell shows the coefficient from a separate regression. Columns refer to different models and rows refer to different outcome variables. In LATE 1, the variable of interest is the threshold dummy at 4,200 m; in LATE 2, the variable of interest is the "No Closer MDF" dummy; and in LATE 3, the variable of interest is the OPAL dummy. All estimations control for changes in the municipalities' population and share of females along with election type dummies, and year dummies. Column 1 estimations additionally include MDF-by-election-type dummies; column 2 estimations include controls for the distance to the actual MDF and the distance to the next urban center; and column 3 estimations include county dummies. Panel B reports reduced-form estimations of the first differences of municipalities' socioeconomic characteristics on the three instruments. The differences are calculated between the pre-Internet period (1995–1999) and the Internet period (2004–2008). All regressions include the same controls as those in the basic regressions except the one that is the dependent variable. In the skill-level regressions, we do not include the shares of unskilled, skilled, and high-skilled workers as controls, since they add up to one. Standard errors are clustered on the municipality level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 6—Estimation Results for Subsamples

	LATE 1 sample		LATE 2 sample		LATE 3 sample	
	Nonlocal	Local	Nonlocal	Local	Nonlocal	Local
	(1)	(2)	(3)	(4)	(5)	(6)
Δ voter turnout	−4.262***	−1.999	−5.026**	−0.817	−1.962	10.208
	(0.991)	(1.753)	(2.059)	(2.774)	(3.578)	(13.572)
Δ vote share established parties	0.577	−5.529**	−1.447	−4.582	−1.023	6.673
	(0.690)	(2.589)	(1.603)	(6.210)	(2.498)	(33.282)
Δ vote share small parties	0.339	5.583**	0.753	4.385	2.816*	−7.017
	(0.405)	(2.574)	(0.855)	(6.175)	(1.646)	(33.373)
Δ vote share right-fringe parties	−0.600	0.410**	0.095	0.052	−1.436	0.344
	(0.382)	(0.173)	(0.930)	(0.625)	(1.886)	(1.905)
Δ vote share left-fringe parties	−0.316	−0.464*	0.599	0.144	−0.357	—
	(0.422)	(0.280)	(0.749)	(0.591)	(0.236)	—
First stage						
Instrument	−0.143***	−0.137***	−0.137***	−0.129***	−0.095***	−0.052*
	(0.009)	(0.009)	(0.010)	(0.011)	(0.027)	(0.028)
F-test of excluded instruments	220.82	249.88	180.43	129.03	12.71	3.53
Number of MDFs/counties	869	853	—	—	63	63
Municipalities	3,333	3,257	1,800	1,762	1,249	1,249
Observations	6,666	3,257	3,600	1,762	2,498	1,249

Notes: Table reports municipality-level instrumental-variable regressions for nonlocal (federal and state) and local elections. Each cell in the upper part of the table shows the coefficient from a separate regression of an election outcome on DSL availability. Columns refer to different models and rows refer to different outcome variables and test statistics. Columns 1 and 2 show estimates for the LATE 1 sample with the threshold at 4,200 m as instrument; columns 3 and 4 show estimates for the LATE 2 sample with "no closer MDF" as instrument; and columns 5 and 6 show estimates for the LATE 3 sample with OPAL as instrument. The control variables and sample definitions are detailed in the notes to Tables 2–4. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-statistic. Standard errors are clustered on the municipality level in columns 1, 3, and 5, and robust in columns 2, 4, and 6, since we observe only one local election per municipality.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

This finding is consistent with the hypothesis that the Internet crowds out incumbent media that are primarily sources of nonlocal political information. We will explore this crowding-out mechanism in Section V. Regarding vote shares, we find that some of the DSL coefficients in the vote share regressions for types of parties are significantly different from zero, but there is still no indication of a clear and systematic pattern.

V. Mechanisms

Similar to our results, Gentzkow (2006) finds a negative effect of the introduction of TV on voter turnout in US elections. He relates this effect to a crowding-out of traditional media with more extensive coverage of political issues, particularly newspapers. Similarly, the emergence of the Internet may crowd out traditional media like newspapers and TV. Both these media have a higher probability of what Prior (2007) describes as by-product learning: Newspapers and broadcasting media present a compilation of diverse issues that expose consumers to opinions and topics they did not deliberately look for. When "googling" specific news and information, we would expect the probability of such chance encounters to decrease. As a result, consumers might be less well-informed when the Internet crowds out more informative media, and that could lead to a lower voter turnout.

We first investigate whether the Internet has crowded out daily newspapers. With more than 1,600 single titles, German newspapers are an important source of political information. Local newspapers in particular are the main source of information on local issues and local politicians' efficacy (Bruns and Himmler 2011). To analyze the substitution between Internet and newspapers, we use municipality-level information about the circulation of daily newspapers in Germany. Data are provided by the German Audit Bureau of Circulations IVW (*Informationsgemeinschaft zur Feststellung der Verbreitung von Werbeträgern e.V.,* 2011) for every other year from 2000 onwards. Based on a survey from one representative week, IVW provides municipality-level information on the circulation of nearly all daily newspapers. These data provide the most detailed picture of the German newspaper market. We group the newspapers into the following five mutually exclusive categories: local newspapers; leading national newspapers; yellow-press newspapers; other newspapers; and Sunday newspapers. Among the daily newspapers, local newspapers have the highest circulation, with an average of 32 newspapers per 100 households in 2008. Additionally, there are 12 other daily newspapers, 6 yellow-press newspapers, and 1 national newspaper per 100 households. For Sunday newspapers, we observe 6 papers per 1,000 households. More information and descriptive statistics on the newspaper data can be found in online Appendix E.

We run the same IV specifications as described in Section II. Our outcome variables are now changes in daily newspaper circulation in a municipality between the years 2000 and 2008. Newspaper circulation is standardized by the number of households in a municipality in the respective year. The estimation results for the five categories of newspapers are provided in Table 7. Overall, we do not find strong evidence for Internet effects on the newspaper market. The only significant effect is a positive DSL effect on yellow-press newspapers in our LATE 1 sample of West German municipalities. As a result, we conclude that there is no evidence for the Internet crowding out the consumption of daily newspapers. Of course, we can only identify an overall substitution effect at the extensive margin. We cannot rule out an effect of the Internet on newspaper circulation for certain population groups (George 2008). We further cannot rule out that people continue to subscribe to their newspaper but spend significantly less time reading it.[28]

We next investigate whether the Internet has crowded out TV consumption. To assess this issue, we now employ survey data from the German Socio-Economic Panel (SOEP). The SOEP is an annual household survey covering roughly 20,000 adults living in more than 11,000 households (Wagner, Frick, and Schupp 2007). The 2008 SOEP wave provides household information on whether Internet access is available and, for the first time, whether this Internet access is based on DSL technology. We employ this information and build a dummy variable that

[28] Note that our results do not imply that the organization of the newspaper industry as a whole was not affected by the emergence of the Internet. Over the last decade newspapers have lost important sources of revenue. For example, classified advertising (real estate, job market, used cars, etc.) has nearly completely moved to the Internet. Lower revenues in turn affect newspapers' financial scope and possibly the size and quality of their editorial departments.

Table 7—Newspapers

	LATE 1 Threshold (1)	LATE 2 No closer MDF (2)	LATE 3 OPAL (3)
Difference 2000–2008			
Δ All daily newspapers	1.648	2.805	−0.812
	(3.398)	(15.288)	(2.959)
Δ Local daily newspapers	−2.917	0.269	2.440
	(1.987)	(5.919)	(2.314)
Δ Other daily newspapers	0.843	−3.920	−1.534
	(1.753)	(12.740)	(1.769)
Δ National daily newspapers	0.398	0.990	0.242
	(0.818)	(0.602)	(0.234)
Δ Yellow press daily newspapers	3.243**	5.970	−1.961
	(1.588)	(4.564)	(1.617)
Δ Sunday newspapers	−0.045	1.281	0.115*
	(0.187)	(1.588)	(0.064)
First stage			
Instrument	−0.129***	−0.110***	−0.242***
	(0.009)	(0.009)	(0.030)
F-test of excluded instruments	197.04	145.55	64.26
Number of observations/municipalities	3,319	1,792	1,247

Notes: Table reports municipality-level instrumental-variable regressions for changes in the newspaper circulation between 2000 and 2008. Each cell in the upper part of the table shows the coefficient from a separate regression of a newspaper type on DSL availability. The outcome variables are percentage point changes in the circulation of all daily newspapers, local daily newspapers, other nonlocal newspapers, seven leading national newspapers, yellow-press daily newspapers, and Sunday newspapers. All outcomes are standardized by the number of households in a municipality. Column 1 refers to instrumental-variable estimations for the LATE 1 sample with the threshold at 4,200 m as instrument; column 2 shows instrumental-variable results for the LATE 2 sample with "no closer MDF" as instrument; and column 2 shows instrumental-variable results for the LATE 3 sample with OPAL as instrument. The F-test of excluded instruments refers to the Kleibergen-Paap F-statistic. All estimations control for changes in the following municipality characteristics: female population share, share of population aged 18–65, share of population older than 65; average wage, net migration rate, share of foreigners, unemployment rate, and share of unskilled, skilled, and high-skilled workers. LATE 1 estimations additionally include MDF dummies; LATE 2 estimations control for distance to the actual MDF and distance to the next urban center; and LATE 3 estimations include county dummies. Standard errors are robust.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

equals unity if a household has a DSL connection. The SOEP also provides information on TV consumption. The variable is measured as a five-scale categorical variable ranging from "never," "less than once a month," "at least once a month," "at least once a week," to "every day."

To account for the endogeneity of Internet access, we rely on the same empirical strategy as before. The only difference is that we can use the exact geo-coordinates of the SOEP households[29] to calculate each household's distance to the actual MDF. Based on this information, we create the same three instruments. The precision

[29] The geo-coordinates of the SOEP households are confidential and only available on-site at the DIW in Berlin.

TABLE 8—SURVEY EVIDENCE

	TV watching (standardized) (1)	Entertainment (hours) Workday (2)	Saturday (3)	Sunday (4)
DSL subscription	−0.340*	0.585	0.858	1.303*
	(0.212)	(0.422)	(0.564)	(0.727)
First stage				
Threshold	−0.136***	−0.141***	−0.143***	−0.142***
	(0.024)	(0.024)	(0.025)	(0.025)
"No closer MDF"	−0.095*	−0.109**	−0.114**	−0.110**
	(0.048)	(0.049)	(−0.05)	(0.049)
OPAL	−0.124***	−0.125***	−0.125***	−0.128***
	(0.029)	(0.029)	(0.029)	(0.029)
F-test of excluded instruments	17.91	17.77	17.92	17.97
Overidentification test	2.004	1.163	0.057	0.594
χ^2 p-value	0.367	0.559	0.972	0.743
Observations	16,043	14,932	14,858	14,851

Notes: Table reports individual-level instrumental-variable regressions with the threshold at 4,200 m, "No closer MDF," and OPAL as instruments. Dependent variable reported on top of each column. Sample includes all adults in SOEP (in 2008) with available information on the respective outcome variable. Additional controls included in each model: the individual's gender, age (and its square), marital status, number of children living in the household, secondary and university education, occupational status, migration background, a dummy indicating house/flat ownership, and log of net household income. All regressions further include county dummies. The F-test for excluded instruments refers to the Kleibergen-Paap F-statistic. Standard errors are clustered on the household level. The overidentification test is based on the Huber-White robust variance-covariance matrix without clustering.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

in our distance calculations comes at a cost: While the previous sections employ administrative voting data which aggregate information from millions of individuals residing in about 12,000 municipalities, these estimations are based on a survey sample of less than 20,000 individuals. It is thus harder to obtain the same statistical power in our IV specifications. Consequently, we only run analyses for Germany as a whole and use all three instruments simultaneously.

Column 1 in Table 8 reports the instrumental variable results for TV consumption. TV consumption is z-standardized so that we can estimate linear models. The regression includes county fixed effects and individual background characteristics that are comparable to the control variables in our municipality-level analyses. Concretely, we control for gender; age; marital status; number of children in the household; secondary and university education; occupational status; migration background; ownership of a house or flat; and net household income.[30] Since our instruments are calculated at the household level, we cluster standard errors accordingly.

In the first stage we regress our instruments on a dummy variable that indicates whether a household has a DSL subscription. The results not only show that our instruments are also relevant at the household level but have almost the same

[30] Table F7 in the online Appendix presents descriptive statistics on these variables.

magnitude that we find at the municipality level, where the first-stage outcome is DSL availability. This is reassuring, since it strongly suggests that broadband Internet availability is indeed the limiting factor for broadband Internet use. As we use all instruments jointly we can run an overidentification test. The test statistics reported at the bottom of Table 8 show that we comfortably fail to reject the hypothesis that the three technical peculiarities of the voice telephony network are jointly valid instruments.

The second-stage result suggests that Internet crowds out TV consumption. Individuals with a DSL subscription watch about one-third of a standard deviation less TV than individuals without a DSL subscription. In line with this result, Wallsten (2013), using US time use data, reports evidence that leisure time spent online is crowding out TV consumption. Given that the original TV consumption variable is measured on a categorical scale, we can also explore the Internet effect at different margins of the distribution. We find that the negative effect of the Internet on TV consumption is mainly driven by a negative effect in the "every day" category. Similarly, Liebowitz and Zentner (2012) find moderate displacement effects of the Internet, with the strongest effects on the group of young TV consumers, which were also the most intensive TV consumers.

Under the assumption that the Internet is a less important source for political information than TV, the crowding out of TV might explain a negative effect of the Internet on voter turnout. The extent to which TV is a source for political information might however differ across election types and potential voters. In fact, Mende, Oehmichen, and Schröter (2012) document that TV is primarily a source for nonlocal news in Germany. Moreover, TV is the primary medium for political advertising during national election campaigns, since parties in Germany have a legal right to broadcast time in public television during national election campaigns.[31] Consequently, we would expect to find an Internet effect first and foremost in nonlocal elections. We would also expect the Internet effect to be stronger in West Germany than in East Germany, since media analyses reveal that West Germans watch more news programs on TV than East Germans and that, in contrast to East Germans, West Germans prefer news from public television over private television—with private-TV news programs carrying significantly less political information (Darschin and Gerhard 2004; Media Control 2010; Krüger and Zapf-Schramm 2012). Specifically, Krüger, Müller-Sachse, and Zapf-Schramm (2005) report that about 83 percent of the political information disseminated by TV stations during the federal election of 2005 was provided by public TV, compared to 17 percent by private TV. In the light of these results, the crowding out of TV consumption is consistent with our finding of heterogeneous turnout effects that vary between local and nonlocal elections as well as between East and West Germany.

The SOEP data also allow us to shed light on the question of whether individuals primarily use the Internet to enjoy more entertainment at the expense of time spent on other endeavors, including the acquisition of political information. Given that individuals may feel more affected by local politics (either because it is more relevant or because they are more likely pivotal), one may assume that information

[31] Cf. Paragraph 11(1) ZDF-StV (*ZDF-Staatsvertrag*).

on nonlocal politics is crowded out first.[32] The SOEP provides information on time spent on online and offline entertainment on working days, Saturdays, and Sundays. Columns 2 to 4 show the results. Having a DSL subscription at home increases entertainment consumption on Sundays by about 1.3 hours (about 80 minutes), and it is significantly different from zero; for entertainment consumption on Saturdays, the Internet effect is about 0.8 hours (about 50 minutes) and it is close to being significant on the 10 percent level; for entertainment consumption on working days, the Internet effect is again smaller and far from being significantly different from zero. One interpretation of this finding is that individuals consume more entertainment at the expense of political information. Another explanation might be that the Internet simply distracts people from voting, since time spent on entertainment increases most on Sundays, the common election day in Germany.

VI. Conclusions

In summary, the results of this paper reveal a consistent pattern. Even though the Internet is a new medium for delivering political information at low cost, it also offers consumers various entertainment possibilities. Indeed, our results reveal that having access to high-speed Internet increases entertainment consumption. This potentially crowds out news consumption, reducing voters' political information and thus voter turnout. Moreover, if the increase of time spent online comes at the expense of time spent for incumbent media consumption, voters' overall political information might decrease under the assumption that incumbent media have been used as a source of political information before the Internet era. Our results show that the Internet crowds out TV consumption and that this crowding out leads to a reduction in turnout precisely among those voters (West German voters) and elections (nonlocal elections) where TV is a primary source for political information.

In our instrumental variable approaches, we identify local average treatment effects that are naturally limited in their generalizability. However, the logic underlying these results may very well apply more broadly, at least to elections in established democracies. To fully understand the effects of a new medium on voting behavior it is important to understand the information role of the medium as well as its relation to the incumbent media. This brings us also to a caveat to our study: Our paper only analyzes the *introduction* of the Internet. Further research is needed to assess whether the estimated effects persist when considering the information and mobilization role of Web 2.0 applications such as blogs, Twitter, or Facebook. Web 2.0 applications might be particularly relevant for people who are interested in topics off the main policy issues. Before the social-media era, voting for small fringe parties without a chance of winning a significant vote share was one way to make a political statement. In the social-media era, blogging and twittering may be more effective ways to express political opinions on specific topics. However, these recent developments are not yet covered in our election data and thus remain a subject for future research.

[32] Given the comparably low turnout in local elections, it could also be that voters who participate in local elections are already well informed and thus less likely to be influenced or distracted by new media.

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