Does High-speed Internet Erode Voter Turnout?*

Evidence from a Large-Scale Broadband Reform

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Abstract: The arrival of high-speed internet has led observers to worry that it erodes voter turnout. Central is broadband's purported substitution effect, where individuals substitute traditional media for less news-intensive online sources. Less exposure to political information means less turnout. We study this explanation in the light of two alternative mechanisms. First, online media consumption may complement rather than substitute traditional news sources—and thus increase turnout. Second, broadband internet may decrease individuals' sense of political efficacy by increasing income inequality. Empirically, we evaluate the effect of broadband on turnout using a large-scale broadband reform that was rolled out in a staggered fashion across Norwegian municipalities between 2000 and 2008. We find that expanding broadband coverage caused increased turnout. The findings suggest that the reform increased online media use without replacing traditional media. We find no evidence that inequality mediates the effect of broadband on turnout. The study shoes that the link between high-speed internet and political participation is contingent on the news content of both online and traditional news sources.

Keywords: Elections; Mass Media; Internet; Voter Turnout

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Does the arrival of high-speed internet erode voter turnout? Electoral participation is in decline across democracies (Kostelka and Blais 2021). Simultaneously, access to high-speed internet has risen substantially. Between 2000 and 2022, the percentage of the world's population with a broadband subscription rose from 0.3 to 17.7. Among high-income countries, the rise was from 1.4 to 37.8 percent (World Bank 2023). A burgeoning literature debates whether the arrival of high-speed internet sways citizens away from voting, thus contributing to the observed turnout decline.

However, the impact of high-speed internet on electoral participation remains contested, and the mechanisms are not well understood. Similar to seminal studies of how the introductions of radio and TV affect political participation (e.g., Strömberg 2004; Gentzkow 2006; DellaVigna and Kaplan 2007; Prior 2007), most existing work emphasizes how high-speed internet affects turnout through *changing exposure to news* about politics (Campante, Durante, and Sobbrio 2018; Czernich 2012; Falck, Gold, and Heblich 2014; Gavazza, Nardotto, and Valletti 2019; Poy and Schüller 2020; see also Putnam 2000; Prior 2005, 2007). A critical debate is whether the arrival of high-speed internet makes people substitute (less news-heavy) online media for traditional media (e.g., Falck, Gold, and Heblich 2014; Gavazza, Nardotto, and Valletti 2019). Or, alternatively, whether the arrival of broadband increases exposure to politics by both lowering the barrier to news consumption (e.g., DiMaggio et al. 2001; Tolbert and McNeal 2003; Lelkes, Sood, and Iyengar 2017; Flaxman, Goel, and Rao 2016) and increasing parties', organizations', and protest groups' possibilities to reach voters with campaigning (Gibson 2004; Campante, Durante, and Sobbrio 2018).[1] Whether the *substitution* or *supplementation* mechanism dominates, and under what conditions, thus remains unresolved in the literature.

In contrast to the political impact of radio and TV, the effects of high-speed internet may additionally run through channels other than citizens' changing media consumption and political campaign strategies, most notably the labor market. Such channels have yet to be explored in the literature. However, recent work on the economic effects of ICT adoption documents substantial effects of broadband internet on labor markets (see Gallego and Kurer 2022, for a recent review). First, the arrival of broadband internet may exacerbate *economic inequality* by increasing the productivity and relative wages of high-skilled workers.[2] Studies suggest that higher inequality may lower turnout by reducing low-skilled citizens' sense of political efficacy (Jensen and Kersbergen 2016, ch. 9). Second, high-speed internet may lower *unemployment* by reducing search

[1]The availability of high-speed internet may particularly benefit the mobilization efforts of smaller (protest) parties and movements, due to lower costs, more user-created content, and less dominance of mainstream media organizations (Gibson and McAllister 2015; Campante, Durante, and Sobbrio 2018).

[2]Akerman, Gaarder, and Mogstad (2015) provide causal evidence from Norway, whereas Hope and Martelli (2019) give cross-national evidence on the association between ICT and income inequality.

frictions (Bhuller, Kostøl, and Vigtel 2021). Still, evidence on the effect of unemployment on turnout is more mixed, as unemployment may either lower the opportunity cost of voting (Charles and Stephens Jr. 2013) or increase political alienation (Emmenegger, Marx, and Schraff 2017). Both the inequality and unemployment mechanisms suggest that to understand the *overall* effect of broadband internet, we must consider media *and* labor market effects, as these may push in different directions.

To this end, our study makes both theoretical and empirical contributions. First, we formulate and provide predictions based on alternative theoretical mechanisms by accounting for channels working through both labor and media markets. Second, we estimate the causal effect of broadband expansion on voter turnout and explore the potential underlying media and labor market mechanisms behind the effect. To do so, we use a unique Norwegian broadband reform following (Bhuller et al. 2013). The reform led to a staggered and plausibly exogenous expansion of high-speed internet across the approximately 430 Norwegian municipalities between 2000 and 2008.[3] To estimate the causal effect of broadband on turnout, we exploit the fact that the most important determinants of coverage remained fixed within each municipality over the reform

[3]Some studies have investigated the reform's economic effects—including labor productivity (Akerman, Gaarder, and Mogstad 2015), unemployment (Bhuller, Kostøl, and Vigtel 2021), and bilateral trade (Akerman, Leuven, and Mogstad 2022)—but the reform's political consequences remain understudied.

period. By exploiting differences in yearly changes in the broadband coverage rate across municipalities, we can thus net out key unobserved time-invariant, municipality-specific factors that may confound the effect of broadband internet on political participation. We additionally show that the temporal changes in broadband coverage are unrelated to municipality trends in crucial potential confounders, such as educational attainment and population growth. Our research design thus improves on previous studies of broadband, which have foremost relied on cross-sectional variation to estimate the impact of broadband on turnout.

We find that the overall effect of broadband expansion on turnout is *positive*. A two-standard deviations increase in broadband coverage between municipal elections (72 percent) heightens voter turnout in a municipality by 0.96 percentage points in the preferred specification. Using data on the share of households with broadband subscriptions, we adjust this estimate for the take-up of broadband in response to increased coverage. We find that a two-standard deviations increase in broadband subscriptions between municipal elections (41 percent) heightens voter turnout in a municipality by 3.77 percentage points in the preferred specification. The effect is robust to the inclusion of time-varying covariates and differential time trends, and sensitivity analyses suggest that the effect is unlikely to be driven by omitted unobserved confounders.

To explore the potential mechanisms behind the effect, we find evidence suggesting that the internet foremost complemented other media sources of political information, especially among the young, who also increased their internet usage faster than older generations. Although other studies have documented positive effects of broadband coverage on wage inequality (Akerman, Gaarder, and Mogstad 2015) and employment (Bhuller, Kostøl, and Vigtel 2021), we do not find evidence that the effect on turnout is mediated through the labor market.

Whereas previous studies have documented both positive and negative effects of broadband on voter turnout (see, e.g., Czernich 2012; Falck, Gold, and Heblich 2014; Campante, Durante, and Sobbrio 2018; Poy and Schüller 2020), we document a positive effect using plausibly exogenous changes in broadband coverage. Our findings highlight the importance of media effects, suggesting that increased broadband coverage can increase turnout by facilitating news consumption in contexts with a strong traditional media sectors. Taken together, these results highlight that the impact of broadband on political participation is likely to be more contingent on the pre-existing media environment than previously recognized.

High-speed Internet and Voter Turnout

Whether and how the arrival of high-speed internet affects voter turnout *through changing exposure to political news* is contested in the existing literature. One set of works studies how high-speed internet reduces voter turnout by providing both less and more biased political news than traditional media (Gavazza, Nardotto, and Valletti 2019; Falck, Gold, and Heblich 2014; see also Putnam 2000; Prior 2007).[4] In this argument, the effect is due to a *substitution mechanism*, where high-speed internet crowds out political news consumption with entertainment. Citizens' *total* news consumption from all media sources thus drops. With less knowledge about elections and politics generally, citizens are accordingly less likely to attend the polling station.[5] The substitution mechanism thus posits an adverse effect of broadband introduction on voter turnout, driven by reduced consumption and exposure to political news.

The substitution mechanism, however, assumes that citizens' media consumption through TV, radio, and newspapers gives higher exposure to political news than online media consumption (Falck, Gold, and Heblich 2014, 2262; Gavazza, Nardotto, and Valletti 2019, 2120–2). Whereas this assumption might be plausible if there is a substitution from radio and newspaper to online activity, it is less clear-cut regarding the substitution from TV to the internet (Campante, Durante, and Sobbrio 2018). Falck, Gold, and Heblich (2014), for instance, find

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[4]Other studies also suggest that high-speed internet may drive those who do vote toward the extremes of the political spectrum (see, e.g., Lelkes, Sood, and Iyengar 2017; Azzimonti and Fernandes 2018; Guriev, Melnikov, and Zhuravskaya 2021; Schaub and Morisi 2020).

[5]For the well-established theoretical and empirical link between political information and turnout, see, e.g., Matsusaka (1995); Feddersen and Pesendorfer (1999); Lassen (2005).

that broadband expansion reduces TV consumption but not newspaper circulation. While Bhuller et al. (2020) document an adverse effect of broadband internet on the circulation of national newspapers, they also show that *local* newspapers did not suffer and used the move online to shift from tabloid to more news-heavy content.[6] Thus, at best, there is mixed evidence of outright substitution away from traditional news sources. Studies showing a negative effect of broadband on turnout also fail to consider whether traditional sources moved online and whether overall news exposure dropped, which the substitution mechanism implies (Gavazza, Nardotto, and Valletti 2019; Falck, Gold, and Heblich 2014).

Indeed, focusing solely on substitution effects overlooks how broadband internet may lower the barrier to news exposure and consumption (Campante, Durante, and Tesei 2022). Due to its superior speed and reliability compared to dial-up connections, broadband significantly boosts internet and online media usage (Hitt and Tambe 2007; Lelkes, Sood, and Iyengar 2017). Online media consumption may thus foremost *add* to traditional news sources—especially among the young (DiMaggio et al. 2001; Tolbert and McNeal 2003; Lelkes, Sood, and Iyengar 2017; Flaxman, Goel, and Rao 2016). This *supplementation mechanism*, in other words, implies that the arrival of high-speed internet increases

[6] Gavazza, Nardotto, and Valletti (2019), on the other hand, do find some evidence of high-speed internet on local newspaper circulation, but they do not take into account whether local newspapers moved online.

overall news consumption. Citizens' increased exposure to political information again forsters their electoral participation. Consequently, whereas the *substitution mechanism* predicts a negative effect of broadband on turnout driven by less overall news consumption, the *supplementation mechanism* predicts a positive effect on turnout driven by more overall news consumption.

Although less discussed in the existing literature on high-speed internet and turnout, broadband may also affect parties' effectiveness in mobilizing voters (see Campante, Durante, and Sobbrio 2018). The availability of high-speed internet may particularly benefit the mobilization efforts of smaller (protest) parties and movements due to lower costs, more user-created content, the possibility of ads targeting, and less dominance of mainstream media organizations (Gibson and McAllister 2015; Campante, Durante, and Sobbrio 2018). In short, if high-speed internet lowers mobilization costs, smaller parties may reach potential voters more efficiently (Cox 2015). Supplementation mechanisms for the positive effect of broadband on voter turnout may thus include both voters' heightened media consumption, as discussed above, and parties' ease of voter mobilization.

Existing studies of the relationship between high-speed internet and turnout almost exclusively study how the arrival of broadband changes media consumption. Unlike the arrival of TV and cable, however, broadband's turnout effects may not be confined to its impacts on media consumption. Recent evidence suggests that broadband expansion drives *economic inequality* by altering labor markets. Increased access to broadband internet heightens economic inequality because high-speed internet complements the non-routine tasks often performed by high-skilled workers—such as problem-solving, knowledge gathering, and communication—but at the same time substitutes the routine tasks often performed by low-skilled workers (Akerman, Gaarder, and Mogstad 2015, 1783).[7] Thus, broadband complements the high-skilled more than the low-skilled, which can exacerbate inequality by increasing the relative wages of the former (for empirical evidence, see Akerman, Gaarder, and Mogstad 2015; Falk and Biagi 2017).

A voluminous literature, in turn, links inequality and turnout, where higher levels of economic inequality make low-income individuals less likely to participate in elections (e.g., Goodin and Dryzek 1980; Jensen and Kersbergen 2016, ch. 9; Schäfer and Schwander 2019; Polacko 2022). This body of research highlights how growing inequality makes low-income voters perceive less influence on politics, reducing their sense of political efficacy and discouraging them from voting. Low-income voters may also prioritize immediate financial stability over political engagement if growing inequality leads to a decline in their real wages.[8] In line with these arguments, several studies find higher income

[7]See Autor, Levy, and Murnane (2003); Acemoglu and Autor (2011); Diessner, Durazzi, and Hope (2022); Hope and Martelli (2019) for the impact of the more general ICT revolution on inequality.

[8]On the demands economic hardship puts on individuals' cognitive capacity, see Mullainathan and Shafir (2014).

inequality to reduce turnout, especially among low-income citizens (Schäfer and Schwander 2019, 406; Polacko 2022).[9] Taken together, the literatures on the broadband-inequality and inequality-turnout links suggest a *wage-inequality mechanism*, where high-speed internet may attenuate turnout by driving up inequality.

In addition to inequality, the arrival of high-speed internet may also influence employment rates. Research from the Norwegian context, suggests that broadband expansion lowers job search frictions: firms post more vacancies online, and these fill quicker and more often. The result is lower unemployment, both among low- and high-skilled (Bhuller, Kostøl, and Vigtel 2021; see also Hjort and Poulsen 2019).[10] However, there is mixed evidence on whether unemployment again affects turnout. Some research indicates that unemployment boosts turnout by decreasing the opportunity cost of participation (Charles and Stephens Jr. 2013). Others find that unemployment—especially among the young—may have scarring effects on individuals' political involvement (Emmenegger, Marx, and Schraff 2017). Thus, even if broadband boosts employment rates, the indirect effect on turnout may still be positive or negative. An *employment mechanism* could go both ways.

[9]Still, we are unaware of any studies isolating the causal impact of inequality on turnout.

[10]Thus, broadband introduction affects both inequality (driven by the wages of skilled workers) and employment (driven by skilled and unskilled workers).

Table 1. Predictions for the effect of broadband introduction on voter turnout via different media and labor market mechanisms.

Type	*Mechanism*	*Description*	*Effect*
1. Media	(a) Substitution	Decreasing consumption of political news	-
	(b) Supplementation: news consumption	Increasing consumption of political news	+
	(c) Supplementation: party mobilization	Lowering parties' costs of mobilizing voters	+
2. Labor market	(a) Wage inequality	Decreasing low-skilled's relative economic position	-
	(b) Unemployment	Decreasing unemployment	-/+

Table 1 summarizes the mechanisms discussed in this section and shows the diverging predictions that the different mechanisms stipulate. The last column "Effect" gives the hypothesized direction of how a given broadband mechanism affects voter turnout. Spelling out the various mechanisms enables us to more clearly see that the overall (average) effect of broadband introduction on turnout may be the sum of several potentially countervailing mechanisms. For instance, if the introduction of broadband causes both increasing consumption of political news through internet usage (1b) and wage inequality (2b) the overall effect depends on their relative strength and may be positive, negative or zero in different cases. This may help account for the fact that some studies find a negative average effect of broadband internet on turnout (Falck, Gold, and Heblich 2014; Gavazza, Nardotto, and Valletti 2019), whereas others find positive (Czernich 2012) or mixed (Campante, Durante, and Sobbrio 2018) effects. Yet none of the existing studies systematically investigate both media and labor market mechanisms or seek to rule out alternative mechanisms. A further reason for the inconclusive findings may be methodological, as most studies rely on an instrumental variable strategy that exploits cross-sectional variations in broadband quality caused by technological deficiencies or disruptions. Such deficiencies may be correlated with a host of time-invariant unobserved factors (for instance geographical sorting of citizens and businesses) which can complicate the interpretation of the estimates. In the next section, we instead leverage the difference between municipalities in *temporal changes* in the expansion of broadband coverage, which allows us to account for a range of time-invariant and time-varying confounding factors. In the subsequent section, we explore the mechanisms behind the positive effect we find of broadband expansion on voter turnout.

Research design

To test the effect of high-speed internet on turnout, our research design relies on the staggered nature of broadband expansion in Norway, induced by a public program promoting the construction of access points between 2002 and 2008.[11] The Norwegian Broadband Reform was enacted by the Norwegian Parliament

[11]Several papers have documented the effect of the reform on a range of outcomes. Bhuller et al. (2013) use the reform to look at the effect of the internet on sex crime, Akerman, Gaarder, and Mogstad (2015) study its effect on labor productivity, Bhuller, Kostøl, and Vigtel (2021) explore the impact on unemployment, and Akerman, Leuven, and Mogstad (2022) studies the impact on bilateral trade.

in May 1998 (n. 3. (.-9. St.meld. 1997). The aim was to replace dial-up internet (> 56 kbit/s), with uniformly and reasonably priced high-speed internet (> 256 kbit/s) access for households and businesses throughout the country (see also Bhuller et al. 2013, 1246–9). Municipal governments were required to ensure broadband internet access for local public institutions by 2005. To meet these goals, the national government invested heavily in the required infrastructure and established a special funding scheme (*Høykom*), where municipalities could apply for state funding to cover the large fixed costs of providing access.

Figure 1 shows the broadband reform achieved its aim: by 2008, almost all households in the country were covered and thus had the opportunity to acquire broadband internet (the next subsection provides details on the coverage and subscription variables). Still, as local access points were unevenly rolled out during the 2000-2008 period, the exact *timing* of broadband access varied considerably across municipalities. First, in 28.1 percent of municipalities, less than half of the households had broadband access by 2004. Second, Figure 2—which maps the development of broadband coverage across municipalities in the Greater Oslo area—shows that the timing varies considerably even among neighboring municipalities. Figure D.1 displays that the spatial and temporal variation is substantial across the entire country. Third, few municipalities experience a transition between no to full coverage between two years, as a result there is substantial heterogeneity in access *within* municipalities as well. Conditional on a municipality experiencing an increase in coverage between two years,

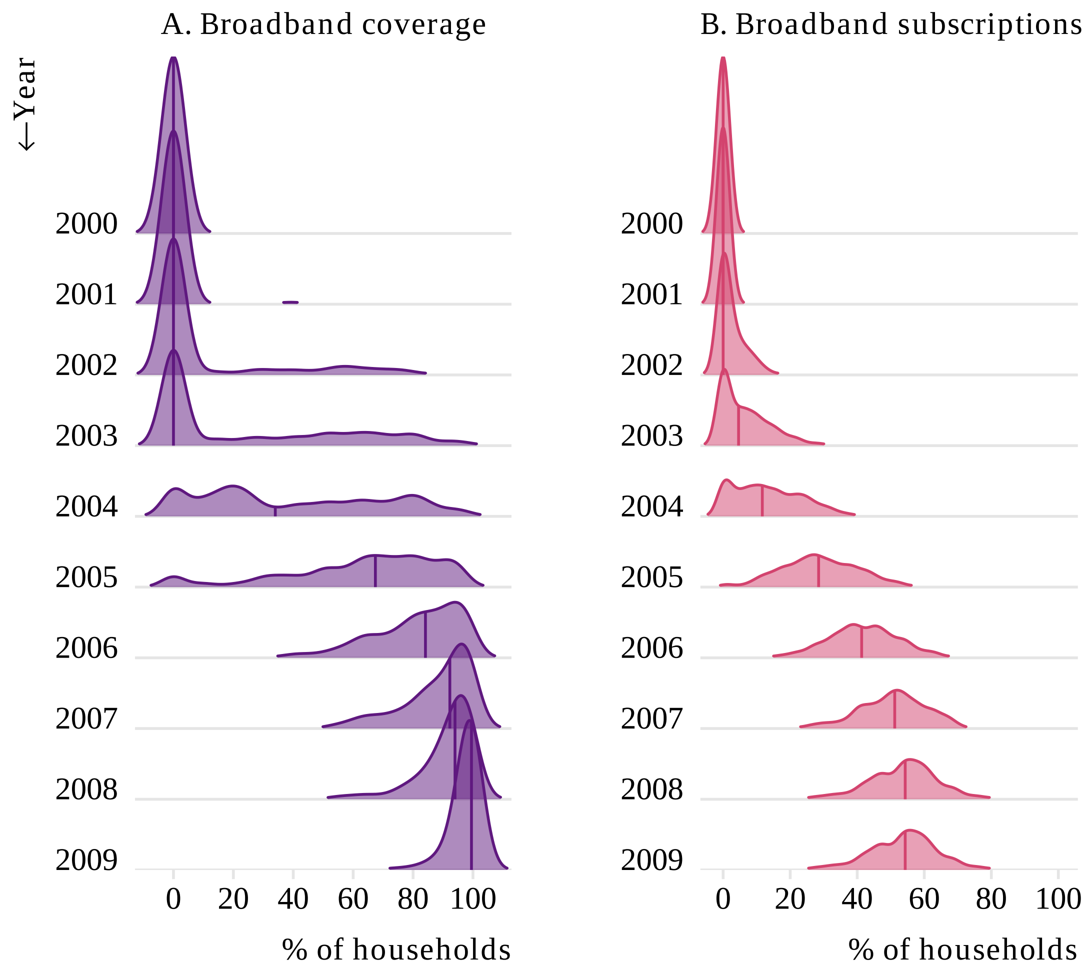

Figure 1. Panel A depicts the median and distribution of broadband coverage rates across municipalities. Panel B depicts the median and distribution of broadband subscription rates across municipalities. Source: Norwegian Ministry of Government Administration.

the average increase in coverage is 18.6 percent.

Still, as one might worry that the timing of the rollout is related to unobserved determinants of voter turnout, we examine the correlates of expanded broadband coverage. Due to Norway's large latitudinal range, rugged coastline, and scattered population, a government report concluded that the differences in rollout timing was mainly related to topographical variation but also funding and pre-existing transportation infrastructure (n. 3. (.-9. St.meld. 1997; n. 4. (.-2. St.meld. 2003; Bhuller et al. 2013). Among the ten municipalities with the lowest coverage in 2007, for instance, all have a population size below the median. This suggests that realizing economies of scale appear to be important in explaining the changes in coverage. Bhuller et al. (2013, 1250) notes that "86% of the variation in broadband coverage can be attributed to time-invariant municipality characteristics and common time effects, whereas less than 1% of the variation in broadband coverage can be attributed to time-varying supply and demand factors." As a result, the most important determinants of both supply and demand for coverage remain fixed or change little during the reform period. The stated goals of the reform and these features of the rollout therefore also suggests it is unlikely that access is determined at the level of the household. Thus, by using *changes* in broadband *coverage* over time a municipality, the variation in coverage is plausibly unrelated to unobserved determinants of political behavior at the municipality level.

To assess whether the *timing* of the coverage expansion between elections is unrelated to unobserved determinants of turnout, we regress the annual change in coverage on a large set of observable pre-reform municipality characteristics interacted with year indicator variables.[12] Here we follow Bhuller et al. 2013, but

[12]We consider average income, population, terrain ruggedness, share with higher education, share with high school education, the share of the population between 16 and 40, total municipality spending, the share of the population living

focus restrict to election years. The regression model takes the following form: $\Delta c_{mt} = \theta_t \times x_{m1999}\lambda_t + \gamma_t \times \omega_f + \varepsilon_{mt}$, where Δc_{it} is the change in broadband coverage, θ_t are year indicators, x_{m1999} is a vector of municipality characteristics in 1999 (the last election before the rollout), and λ_t are the coefficients of interest. The vector λ_t therefore captures the associations between each element of x_{m1999} and changes in coverage between election years t and $t-1$.

We find that many characteristics are unrelated to the timing of coverage expansion, with the exception that more densely populated and urbanized municipalities were more likely to expand broadband coverage early on (see Figure D.2). We also find an association between the timing of the rollout, the population size, and the age structure. This is largely in line with the government report (n. 3. (.-9. St.meld. 1997; n. 4. (.-2. St.meld. 2003) and Bhuller et al. (2013). Note, however, that we do not find that changes in urbanization is related to changes in voter turnout in the pre-reform period. It is therefore unlikely that trends in urbanization confound a potential relationship between broadband coverage and voter turnout. We nevertheless control for these characteristics in the baseline specification below.

in an urban area, share of population that are immigrants, total municipal government spending, turnout in local and national elections, and the unemployment rate in 1999.

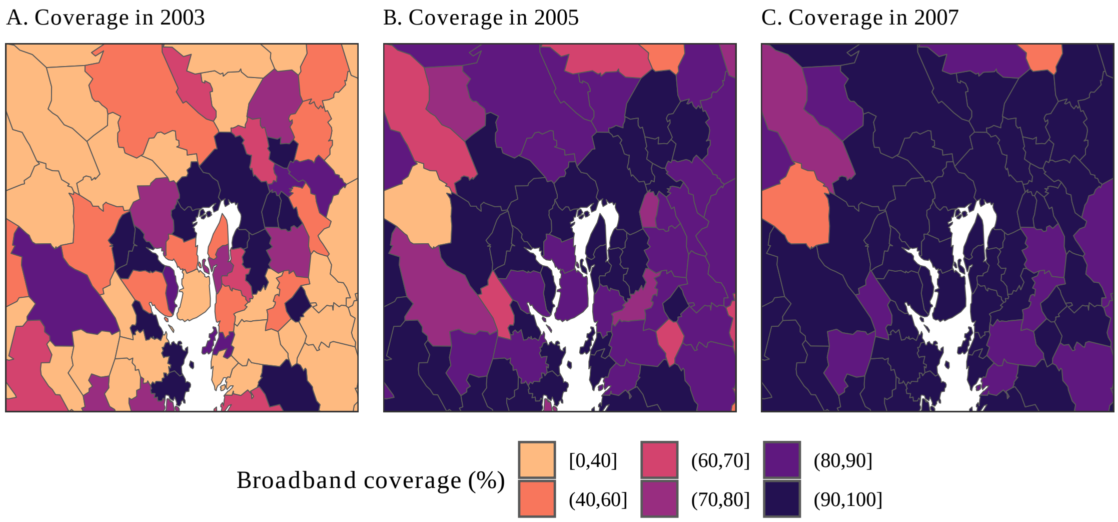

Figure 2. The maps show the cross-sectional distribution of coverage rates across municipalities in the greater Oslo area for the election years 2003, 2005, and 2007. Source: Norwegian Ministry of Government Administration.

In light of these features of the rollout, we estimate the following model

$$T_{mt} = \lambda_m + \gamma_t \times \theta_f + \beta c_{mt-1} + \rho x'_{mt} + u_{mt}, \tag{1}$$

where T_{mt} is the turnout rate in municipality m in election year t and c_{mt-1} the coverage rate in municipality m in year $t-1$. c_{mt-1} is measured as the share of households in a municipality with *access* to broadband internet. The parameter of interest β, therefore denotes how many percentage points the turnout rate changes for a one percentage point change in the broadband coverage. λ_m denotes municipality fixed-effects that account for time-invariant municipality-specific demand and supply factors. Crucially, since the county and municipal elections occur at the same time throughout the sample period, we control for time-varying election-specific factors. These are captured by county times election year fixed effects denoted by $\gamma_t \times \theta_f$. We also include a vector x'_{it} containing the observable municipality characteristics found above to predict the timing of the rollout. In a more stringent specification, we also include interaction terms between these pre-reform characteristics and linear time trends.[13] The regression is weighted by the size of the voting age population (above 18 years of age). u_{mt} captures unobserved time-varying factors that affect the turnout rate. Since u_{mt} is potentially correlated over time within municipalities, we cluster standard errors at the municipality level. The key assumption underlying the causal interpretation of β is that u_{mt} is the same on average across municipalities that experience different changes in broadband coverage between two elections. We provide evidence supporting this assumption below.

To estimate the model, we combine several datasets. The broadband internet data contains information on the fraction of households in each municipality that has access and subscription to broadband internet between 2002 and 2008. The data on broadband coverage is from the Norwegian Communication Authority and the Ministry of Transport. In some cases, we correct for measurement error

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[13]In particular we control for variables found to predict the timing of the rollout: the urban population, the total population size, and the share of the population between 16 and 45 years of age in 1999.

in the coverage data using the data from Bhuller et al. 2020. Data on subscription rates are from Statistics Norway and measures the share of households with internet subscriptions above 384 kbit/s. We match the coverage and subscription data with various sources. Data on municipal elections is from Fiva, Halse, and Natvik (2023), which covers the entire study period. Our main outcome is *turnout in municipal elections*. Voter turnout is measured as the percentage of eligible voters who turn out to vote.

While there were 428 municipalities in Norway at the end of the study period, this varies somewhat over the sample period due to mergers and border changes. We restrict the sample to include only municipalities without border changes that have existed for the entire sample period 1995-2017. This leaves us with 399 municipalities.[14] Local elections are held every fourth year and we include the years 1995, 1999, 2003, and 2007. The data set used in the main analysis is therefore a balanced panel of four elections and 399 units in the cross-section leaving us with 1,596 observations (4 elections $\times$ 399 municipalities gives 1,596 observations).[15] We further supplement this data set with information on socioe-

[14]We find that the 29 municipalities omitted from the sample are similar in terms of basic socioeconomic and demographic characteristics during the period of study.

[15]We also consider national elections. We consider the national elections in 1997, 2001, 2005, and 2009 which gives the same number of observations (4 elections $\times$ 399 municipalities gives 1,596 observations).

conomic and demographic characteristics provided by Statistics Norway and the Norwegian Social Science Data Service. Summary statistics for all the variables used in the main analysis are provided in Table B.1. We provide a detailed description of the variables in the Appendix.

Results

The Effect of Broadband Internet on Voter Turnout

Table 2 shows the effect of increased broadband coverage on turnout in local elections under different specifications. Column (1) shows the result from a minimal specification only including election and municipality fixed effects. To relax the assumption that unobserved determinants of turnout are the same on average across municipalities that experienced different changes in the coverage rate between elections, we condition a series of covariates in Column (2). Column (3) interacts pre-reform municipality characteristics with a linear trend, thus allowing trends at the municipality level to depend on pre-reform characteristics of the municipality (urbanization, population size, and the share of the population aged 16 to 45 in 1999). Column (4) includes county times election fixed effects, while Column (5) includes region times election fixed effects.[16]

All columns in Table 2 show that increases in broadband coverage have a *pos-*

[16]Regions denote labor market regions. These are constructed by Statistics Norway based on commuting flows (analogous to European Statistical Office NUTS-4 level) (Fiva, Halse, and Natvik 2023).

Table 2. Broadband coverage and turnout in local elections

Dependent variable:	Turnout in local elections				
	(1)	(2)	(3)	(4)	(5)
Coverage %	0.0020	0.0090	0.0013	0.0134	0.0170
	(0.0059)	(0.0040)	(0.0083)	(0.0046)	(0.0055)
Controls		✓	✓	✓	✓
Year × $m_{i,1999}$			✓		
County × Year FE				✓	
Region × Year FE					✓
Mean turnout	63.06	63.06	63.06	63.06	63.06
Municipalities	399	399	399	399	399
N	1,596	1,596	1,596	1,596	1,596
R^2	0.7934	0.8234	0.8269	0.8824	0.9175

Notes: The table reports OLS estimates. Regressions are based on data for four elections, (1995, 1999, 2003, 2007) × 399 municipalities = 1,596 observations. *Coverage* is the fraction of households with access to broadband in the year prior to the election measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population size. Standard errors are clustered at the municipality level.

itive effect on voter turnout in local elections. A one percentage point increase in broadband coverage in the year prior to the election, increases the turnout rate by 0.013 percentage points in the preferred specification. This implies that a municipality that goes from zero to complete coverage between two local elections will increase its turnout rate by 1.3 percentage points. For a municipality with an average turnout (63.06) this implies an increase in turnout of approximately 2 percent. The effect is precisely estimated in the baseline specification. As can be seen in Table C.1, the turnout in national elections also increases, although the effect is more muted.

To put the magnitudes in perspective, we use the estimated model in Column (4) to calculate the counterfactual evolution of turnout during the period.[17] Figure D.3 compares the average actual turnout at the country level with the counterfactual scenario in which the broadband coverage remains zero throughout the period. The estimates of the baseline model imply a lower turnout in both national and local elections in the absence of the rollout. Furthermore, the difference between the scenarios is increasing as the rollout proceeds. For local elections, the average turnout is 0.57 percentage points lower for the 2003 election in the absence of the rollout. This increases to 1.82 percentage points for the 2007 election. As a result, without the rollout, the average turnout in local

[17]This exercise presupposes that the regression model is correctly specified and that there is no treatment effect heterogeneity across municipalities.

elections would be 3 percent lower in 2007. The national-level turnout mirrors these findings, although the impact is attenuated and less precisely estimated.

We continue by looking more closely at the *timing* of the effect from Table 2. The assumption underlying our causal interpretation, is that time-varying unobserved determinants of turnout are similar in municipalities that experienced different changes in coverage. We assess this assumption by estimating the coefficients of the lead and lagged levels of coverage in the Equation 1. We expect the lead term to capture potential differences in determinants of turnout that vary across municipalities which experienced different changes in coverage. We estimate these coefficients using the following modified version of Equation 1,

$$T_{mt} = \alpha_m + \gamma_t \times \theta_f + \beta_k c_{mk} + \phi x'_{mt} + \epsilon_{mt}. \quad (2)$$

T_{mt} again denotes turnout and c_{mt-k} is the coverage rate of the municipality in year $t-k$. β_k is the coefficient of interest and captures the association between turnout and the coverage prior to the previous election β_{t-5}, the contemporaneous election (β_{t-1}) and next election (β_{t+3}). The other terms follow the notation of Equation 1. Standard errors are again clustered at the municipality level.

Figure 3 depicts the estimated coefficients. Panel (A) depicts the estimated coefficients on the lead and lagged terms for the local elections. As can be seen from the figure, the coverage before the next election ($t+3$) does not predict turnout in the election in year t. This suggests that the main effect is not driven by differential trends in turnout. We find a similar pattern for turnout in national elections which is depicted in Panel B. While the coverage rate in the year prior to the election is strongly related to changes in turnout, this is not the case for the lagged impact (the coverage prior to the previous election in $t-5$). This could point towards the effect being driven by more short-term factors, such as changes in media consumption or exposure to information, rather than structural changes in the economy or social change more broadly that has also been documented in the literature (see e.g. Bauernschuster, Falck, and Woessmann 2014; Campante, Durante, and Sobbrio 2018).

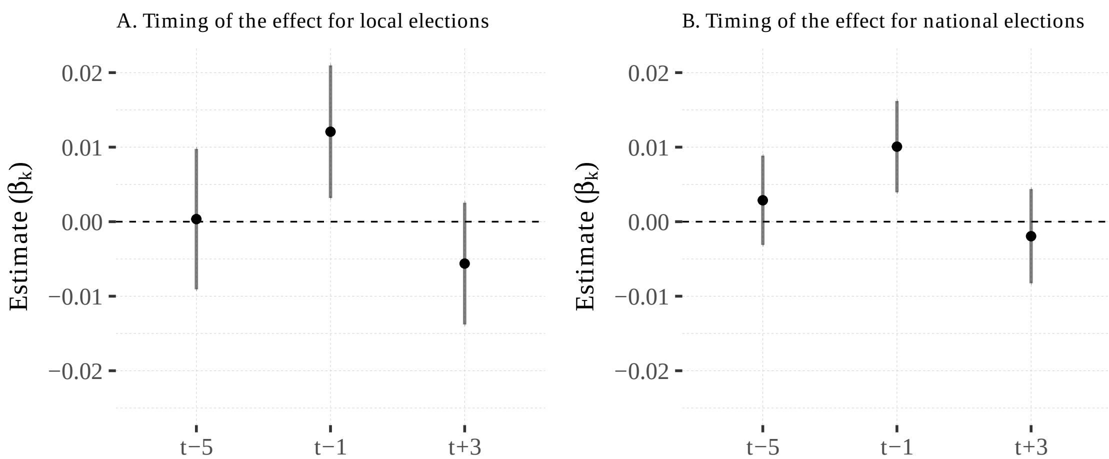

Figure 3. The figure depicts the estimated coefficients of the lead (β_{t+3}), lagged (β_{t-5}), and contemporaneous (β_{t-1}) coverage rate from Equation 2. The specifications include the set of baseline controls, municipality fixed-effects, as well as county× election fixed effects. Standard errors are clustered at the municipality level. Panel A. depicts the coefficients from the local elections while Panel B. depicts the coefficients from the local elections.

Sensitivity Analysis

We continue by exploring further the robustness of our findings using alternative specifications that challenge the assumptions underlying the causal interpretation of the estimates. For brevity, we only discuss the robustness checks using turnout in local elections as the outcome. The results are reported in the Appendix.

Throughout we have used municipalities as the unit of analysis. While municipalities coincide with electoral districts and approximate local labor markets, the low level of aggregation raises the possibility of spillovers across nearby municipalities. Municipalities might then be affected by changes in broadband coverage in nearby municipalities through for example commuting flows. As this would complicate the causal interpretation of the estimates in Table 2, we conduct several robustness checks to explore this issue. Since commuting is likely to be most common in municipalities bordering larger cities, we estimate the model after removing the three largest cities in the sample (Oslo, Trondheim, and Bergen). We also aggregate the data to larger administrative units such as regions and counties. We find similar estimates using these alternative approaches. Relatedly, we examine if the results are robust to alternative assumptions about the variance-covariance matrix of the error term. We find that the conclusions are robust to correcting for spatial and time correlation by clustering standard errors at the region level as well as clustering standard errors following Colella et al. 2019.

The causal interpretation rests on an assumption of parallel trends between groups with different treatment intensities. We assess the validity of this assumption by estimating several specifications where we allow for differences in trends across municipalities. If the estimates are sensitive to the inclusion of these controls, it could suggest that underlying trends in turnout differ across municipalities that experienced different changes in broadband coverage. We therefore estimated versions of Equation 1 where we allow control for differences in trends across municipalities, counties, and regions. These estimates are presented in Table C.4. Across most of these specifications, we find a positive and precisely estimated impact of changes in coverage on turnout. Furthermore, the magnitude is similar to the estimates in Table 2. Table C.4 also provides the baseline specification without weighting.

An important threat to the research design is unobserved demand or supply shocks for broadband that are correlated with the turnout but that are not captured by the observed covariates. Since these demand or supply factors are likely to be related to the observed time-varying covariates, it is reassuring that the estimated effect of broadband coverage on turnout is stable to the inclusion of these covariates. A more systematic approach to analyzing coefficient stability is developed in Cinelli and Hazlett 2020. We follow this approach and calculate the robustness value, which quantifies the explanatory power of unobserved confounders needed to account for the effect size we find. We find that a robustness value of 8.83 is needed for the point estimate of broadband coverage to be zero, and 3.36 for it to be statistically insignificant. In other words, if there is a confounder that explains 8.83 of the outcome (after conditioning on municipality and year fixed effects), then this would be consistent with a true effect size of zero. How plausible is it that we are overlooking such a confounder? This is substantially larger than the effect of key demand factors such as income or education. We therefore think it is unlikely that the estimated effect is driven solely by unobserved confounders.

Controlling for municipality and election fixed effects is an important part of our identification strategy. Although our preferred specification also includes county $\times$ election fixed effects, our empirical model is thus related to the so-called two-way fixed effects model (TWFE), in which one controls for both group and time fixed effects. A rapidly growing recent literature has scrutinized the application of the TWFE model to situations with staggered adoption of a treatment, revealing potential pitfalls when treatment effects are heterogeneous.[18] While much of this discourse has centered on binary treatments, it's important to note that similar issues can arise with continuous treatments, as is in our study. With binary or multi-valued treatments, the solutions for estima-

[18]See Imai and Kim 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Gardner 2022; Borusyak, Jaravel, and Spiess 2023 for some important contributions and Chaisemartin and D'Haultfœuille 2023 and Roth et al. 2023 for overviews.

tion over a given time interval typically involve comparing units with a given level of treatment with non-treated units. This becomes more complicated with continuous treatments, as treatment units will always differ in their treatment intensity. To investigate dynamic effects, we follow Chaisemartin et al. 2023. In our sample, we have four election years – 1995, 1999, 2003 and 2007. In the first two election years, coverage (the year before) is 0 for all municipalities. From 1999 to 2003, coverage increases in 189 municipalities. In the terminology of Chaisemartin et al. 2023, these municipalities become switchers, while the remaining 210 municipalities become stayers. This situation enables estimation of one placebo treatment, in 1999, and one first-period treatment, in 2003. Unfortunately, no municipality remain a stayer also from 2003 to 2007. Thus, there is no group of municipalities that can serve as controls to estimate a second-period treatment. We therefore focus on one pre-treatment placebo estimate and one post-treatment estimate. In practice, we then follow Chaisemartin and D'Haultfœuille 2020. The average effect for all switchers in 2003, disregarding information about the intensity of the treatment, is 1.69 percentage points. Normalizing this by magnitude of the treatment, i.e. the increase in coverage, yields an effect of 0.023 percentage points higher turnout per percentage point coverage.

Mechanisms

What drives the positive effect of broadband coverage on turnout? In this section, we explore several potential mechanisms. First, we examine the role of changes in media consumption patterns. Then we explore potential mechanisms working through labor markets. Finally, we discuss the role of broadband internet in changing parties' effectiveness in mobilizing voters.

Media Mechanisms

To examine the media mechanisms, we use individual-level data on media consumption and exposure to political information through media from the nationally representative Media Use Surveys as well as the Local Election Surveys. Data on media consumption are from the annual Norwegian media use surveys for the years 1991–2007. Unfortunately, the media use data do not come with a municipality identifier, preventing us from employing the identification strategy including municipality fixed effects as elsewhere in the paper. The survey data are all gathered and compiled by Statistics Norway, and distributed and made publicly available by the Norwegian Social Science Data Service. The Local Election Surveys are conducted every fourth year and cover each municipal election from 1999 to 2007. Summary statistics can be found in Table B.2.

In Figure 4, we use waves of the Media Use Survey and plot minutes spent per day on reading newspapers, listening to the radio, watching TV, and going on the internet over the 1991-2007 period. Since the onset of the broadband reform, average time spent online has increased considerably: from 19 minutes in 2000 to 60 in 2007. Whereas the existing literature on broadband and politics finds substitution effects between the internet and traditional media sources (see, e.g., Gentzkow 2006; Falck, Gold, and Heblich 2014; Lelkes, Sood, and Iyengar 2017; Gavazza, Nardotto, and Valletti 2019), this appears not to be the case in Norway. Since 2000, time spent on newspapers and radio has declined very gradually, whereas there is no trend in time spent watching television. At the aggregate level, this suggests that the arrival of the internet has thus foremost led individuals to spend *additional* time on media. These findings are summarized in Table C.2 which shows there is a weak association between time spent online and time spent using other media sources. These findings are similar to evidence from Swedish data which also suggests weak substitution effects between online and traditional media (Liang and Nordin 2013).[19] However, it is important to note that since we lack municipality identifiers, it is not possible to separate the degree of substitutability between the media sources from differences in preferences (Gentzkow 2007).

Did the increase in broadband coverage affect the voters' exposure to information about local politics? The Local Election Surveys ask respondents about their exposure to news about politics through different media sources, (the inter-

[19]This is contrary to Liebowitz and Zentner 2012 who find that internet use reduces television viewing in the United States.

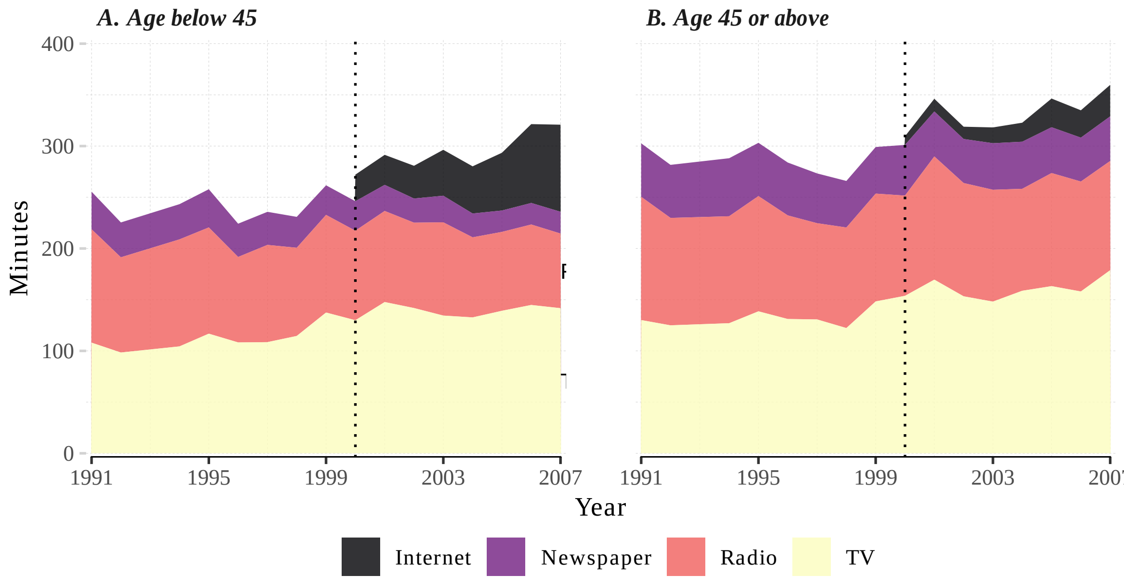

Figure 4. The figure depicts the number of minutes spent on the internet, reading the newspaper, listening to the radio, and watching television the day before for different age groups during the years 1991-2011. Source: Media Use Survey, Statistics Norway.

net, newspapers, radio, and television).[20] We explore this question by modifying the baseline model to accommodate individual-level outcomes as follows,

$$y_{imt} = \alpha_m + \gamma_t \times \theta_f + \lambda c_{mt-1} + \psi x'_{imt} + \xi_{imt}, \tag{3}$$

where i, m, t denotes individual, municipality, and year, respectively. c_{mt-1} again denotes the coverage rate in the municipality the year before the survey. α_m denotes a municipality fixed effect and $\gamma_t \times \theta_f$ denotes county times year fixed effects. x'_{imt} contain individual level controls (income, age, education, and

[20]The wording is as follows: *Have you read statements made by political candidates in the newspaper/radio/television/the internet?*

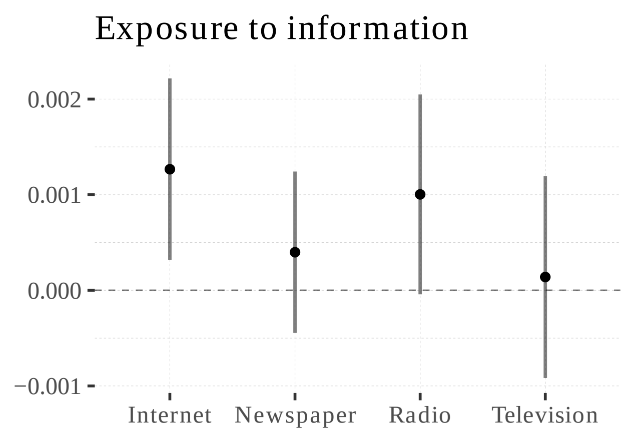

Figure 5. The figure shows the estimated coefficients of the interaction term between coverage and an indicator for the respondent being below 44 years of age in Equation 3 for different information sources. The estimate capture the impact of increased coverage on exposure to information about local politics through the internet, newspapers, radio, and television.

gender) as well as the same time-varying municipality level controls as Equation 1 (the share of the population residing in urban areas, the population size, and the share of the population aged 16-45). Standard errors are again clustered at the municipality level. The results are presented in Table C.6. As can be seen in the table, we find no evidence of a direct effect of coverage on self-reported exposure to political information on average.

If there is an effect of increased coverage, then the effect is driven by the individuals who increase their broadband consumption in response to increased coverage, i.e. the compliers. Bhuller et al. (2013) document that particularly the young (age $<$ 44) were most likely to increase their time online as a result of the reform. In Figure 4, we plot the minutes spent consuming various media by age group. For radio, there might be indications of a substitution effect from

2000 and onward but for newspapers and TV, the trends are similar for both age groups, which does not lend support to a substitution argument. Figure D.5—which displays the fraction of respondents who have used the internet for news, fact-checking, and entertainment—shows that during this period it was more common to have used the internet for news and facts than for entertainment. Furthermore, Bhuller et al. (2020) show that increased broadband coverage did *not* reduce local newspaper circulation. Rather, the local newspaper shifted their content towards more serious news content as a result of increased broadband coverage. It is therefore not clear that even with substitution effects internet consumption reduces exposure to information about current affairs in this context.

To examine if these patterns are mirrored in the self-reported exposure to information, we re-estimate Equation 3 after interacting the coverage rate with an indicator for whether the individual is below 44 years of age. The results are displayed in Figure 5 which shows the estimated interaction term for all four media sources. There is a positive effect of coverage on self-reported exposure to information about local politics.[21] While there is no differential effect between young and old groups for newspapers, radio, or television, there is a precisely estimated and sizable effect for exposure through the internet: a one percentage point increase in coverage induces the average of the group of young individ-

[21]Unfortunately, the estimated coefficient for voting is imprecisely estimated and inconclusive.

uals to increase with 0.0013 relative to the baseline group. Consistent with the hypothesis that high-speed internet leads to more news consumption, the estimates consequently suggest that if anything the broadband reform led to more, and not less, media exposure to local politics. The estimates are also displayed in Table C.6.

Labor Market Mechanisms

We continue by exploring whether the increased turnout is mediated through adjustments in the labor market. There is mounting evidence that computer-based technologies complement high-skilled labor and substitute for low-skilled workers performing routine tasks (Autor, Levy, and Murnane 2003). As increased broadband coverage reduces the price of adopting computer-based technologies for firms, we expect increased coverage to increase earnings inequality. Akerman, Gaarder, and Mogstad 2015 document that the rollout of broadband increased the productivity of high-skilled workers and reduced that of low-skilled workers in Norway. As a result, the effect of broadband coverage on turnout could have been mediated through increased earnings inequality.

We first calculate the Gini coefficient and 90/10 ratio for each municipality using data on individual incomes for the universe of the Norwegian population using administrative data from Statistics Norway. Since the employment share is also affected by increased broadband coverage (Bhuller, Kostøl, and Vigtel 2021), we calculate the Gini coefficient for the entire population (not just employed) in each municipality. We then estimate the baseline model with measures of inequality as the dependent variable. Consistent with the findings in Akerman, Gaarder, and Mogstad 2015, we find a positive effect of increased broadband coverage on inequality. We report these findings in the Appendix.[22]

Does income inequality mediate the effect of broadband coverage on turnout? We explore this by estimating the average controlled direct effect (ACDE) using the sequential g-estimator (Acharya, Blackwell, and Sen 2016). The ACDE captures the effect of broadband coverage, for a fixed value for the level of income inequality. We set this fixed value to the sample mean (32.6). The estimation is conducted in three steps. First, the turnout is regressed on the covariates, fixed effects, and inequality which is the mediator. Second, the product of the coefficient on the Gini coefficient and the observed level of inequality is subtracted from the observed level of turnout. This creates a *demediated* version of turnout or an estimate of turnout in the counterfactual case that inequality remains fixed at the mean for all municipalities. Finally, the ACDE can be recovered by regressing the demediated level of turnout on the baseline model.

To recover the ACDE using the procedure above, two assumptions need to be satisfied. We assume that (i) there is no omitted variables between the turnout, inequality, and broadband coverage, conditional on the set of controls, and (ii)

[22]As we estimate the effect for the whole population in each municipality, the estimates are naturally less precise than in Akerman, Gaarder, and Mogstad 2015 who study the impact on employed workers.

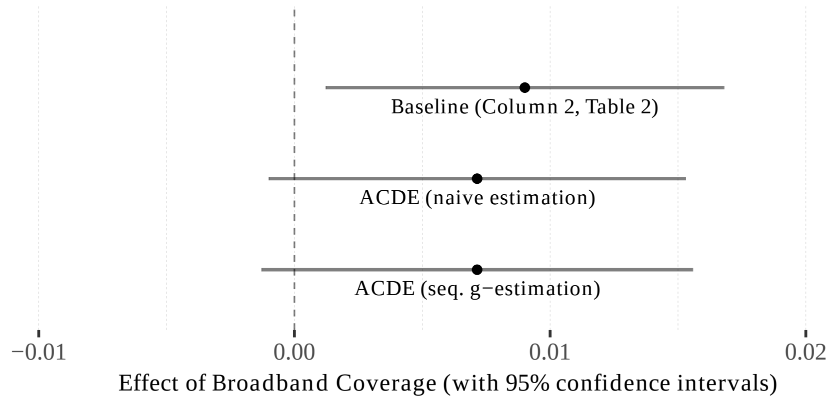

Figure 6. The figure depicts the baseline estimate, the naive controlled direct effect, and the controlled direct effect using the sequential-g estimator with inequality as the mediator. Regressions are weighted by the voting age population. The 95% CIs for the sequential-estimator are based on the asymptotic variance derived in Acharya, Blackwell, and Sen 2016.

that there are no omitted variables between inequality and turnout once we condition on the main controls and broadband coverage (Acharya, Blackwell, and Sen 2016). We elaborate on the assumptions and evaluate the sensitivity of the sequential-g estimate of the ACDE to the violation of these assumptions in the Appendix.

The results are reported in Figure 6. For comparison, the first row shows the baseline specification from Column (2) in Table 2. The second row depicts the effect of coverage on turnout when controlling for inequality in the same specification. Finally, the third row shows the effect of increased coverage on turnout when keeping inequality fixed and accounting for post-treatment bias using the sequential-g estimator. As can be seen, there remains a large effect of coverage after keeping inequality fixed. This suggests that the effect is not primarily mediated through this channel.

Bhuller, Kostøl, and Vigtel 2021 document that increased broadband coverage lowered unemployment by facilitating labor market matching. Lower unemployment can affect voter turnout by changing the opportunity cost of participating in politics (see e.g. Charles and Stephens Jr. 2013). We explore this by conducting the same analysis as above but now treating the unemployment rate as the mediator. These results are displayed in D.6. Again, the estimated coefficients are stable when keeping the mediator fixed at the average. In summary, these findings suggest the labor market mechanisms considered here are not important in mediating the impact of broadband coverage in this context.

Other Mechanisms

So far, we have analyzed the impact of broadband coverage on voter behavior. It is a possible that the broadband expansion increased turnout by changing how parties campaign and to communicate with the electorate. Internet as a tool for campaigning has been increasing in importance since the 1990s in many countries (Gibson 2004). While this is likely to also be the case in Norway, Karlsen 2009 highlights several reasons for why the internet is less important for campaigning in Norway. First, in party-centered campaigns such as Norwegian local and national elections incentives for targeting are weaker. Second, there is a high reliance on state funding of parties and therefore less need for fundraising that could be facilitated by the internet. Finally, strong privacy laws limited the ability of parties to micro-target their campaign. These assertions are supported by the lower importance assigned to the internet for campaigning as late as 2005 when traditional media sources were considered more important than before by party strategists (Karlsen 2009). This does not square with the large effect we find on turnout already in the early 2000s. In addition, in the context of Norway, most political parties have websites predating the reform, starting in the mid-1990s. Finally, we also do not find any clear relationship of the effect depending on party size, as one would expect if it changed campaigning strategies (Gibson and McAllister 2015). So while part of the positive impact on turnout might be attributed to changing forms of mobilization, we think it is unlikely to explain the full effect.

Internet and social media use has been linked to increased polarization (Lelkes, Sood, and Iyengar 2017) which in turn can affect turnout in numerous ways (Rogowski 2014). However, during the reform period social media was still in its infancy. For example, Facebook was only widely adopted in Norway towards the end of the reform period. Furthermore, there is little evidence of increased political polarization in the context of Norway (Boxell, Gentzkow, and Shapiro 2021). To explore this in more detail, we consider the impact of increased broadband coverage on election results for the main parties. We do find a positive relationship between increases in coverage and voting for the Socialist Left Party (*SV*) and the Progress Party (*FRP*) both as measured as the share of the electorate and cast votes. When looking at how this aggregates into seat shares we only find an impact on the seat share for the Socialist Left Party

(SV). These results are reported in the Appendix. In light of these magnitudes and the features of the context highlighted above, increased political polarization is unlikely to explain the full increase in turnout although we cannot rule out that it does not play any role in this context.

Magnitudes

The above findings suggest that the effect of coverage on turnout is mediated through changes in media consumption patterns. If this is the main mechanism through which increased coverage affects turnout, we can leverage this to study the effect of increases in household subscription rates. This is of interest because changes in subscriptions capture a location's consumption of broadband internet which maps more closely into the mechanisms considered in this paper. As can be seen in Table C.3, we find a strong and precisely estimated relationship between changes in coverage and subscription rates. A one percentage point increase in the coverage rate increases the subscription rate by between 0.12 and 0.15 percentage points. Scatter plots are depicted in Figure D.7.

Assuming that the effect of coverage on turnout works only through the effect of increased subscription rates among households, we can use changes in coverage as an instrument for changes in the subscription rate. In practice, this corresponds to rescaling the effect of increased coverage on turnout with the induced increase in subscriptions. Using the estimates in Columns (4) of Tables 2 and C.3, this shows that a one percentage point increase in the subscription

Table 3. Broadband subscriptions and turnout in local elections

Dependent variable:	Turnout in local elections				
	(1)	(2)	(3)	(4)	(5)
Subscriptions %	0.0096	0.0458	0.0092	0.0912	0.1111
	(0.0291)	(0.0210)	(0.0584)	(0.0321)	(0.0385)
Controls		✓	✓	✓	✓
Year × $m_{i,1999}$			✓		
County × Year FE				✓	
Region × Year FE					✓
Mean turnout	63.06	63.06	63.06	63.06	63.06
Municipalities	399	399	399	399	399
First stage	0.21	0.2	0.14	0.15	0.15
F-value (instr.)	114.82	129.96	154.64	169.12	166.83
N	1,596	1,596	1,596	1,596	1,596
R^2	0.7935	0.8256	0.8273	0.8819	0.9171

Notes: The table reports IV estimates. The instrument is the fraction of households with access to broadband in the year prior. Regressions are based on data for four elections, (1995, 1999, 2003, 2007) × 399 municipalities = 1,596 observations. *Subscriptions* is the fraction of households with subscription to broadband measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population. Standard errors are clustered at the municipality level.

rate results in a 0.0134/0.1465 ≈ 0.091 percentage point increase in turnout. A ten percent increase in the subscription rate of a municipality therefore increases voter turnout by approximately 1 percentage point. Again considering the effect for a municipality with the average turnout, this results in a 1.5 percent increase in turnout. To estimate the standard errors, we use the instrumental variable estimator which we report in Table 3. As can be seen in the table the instrumental variable estimates are stable and precisely estimated several of the different specifications. Moreover, the F-statistic ranges from 115 to 169 which suggests that the instrument is unlikely to suffer from bias induced by weak instruments.

In the context of Norway, turnout in local elections is relatively high to begin with (about 63 percent in the main sample). Among people who don't already turn out to vote, how large is the share of people that turn out to vote as a result of increased broadband exposure? This is akin to a persuasion rate which gives the share of people that turn out to vote as a fraction of people that could potentially do so (DellaVigna and Kaplan 2007). Consider a municipality experiencing an increase in subscription rates equal the sample average increase between 2000 and 2007 (50.54). Assume also that the turnout in local elections equaled the average in 1999 (62.98). The persuasion rate can then be expressed as,

$$f = 100 \times \frac{y_T - y_C}{100 - y_C} \qquad (4)$$

where y_T is the turnout after the 50.54 percentage point increase in broadband subscriptions, y_C is the turnout rate without the increase, and $100 - y_C$ is the share of voters not turning out. With the coefficient from the baseline specification in Column (4) of Table C.3, the implied persuasion rate is given by $f = 12.5$ percent. This magnitude is consistent with other information treatments in the literature on voter turnout (see e.g. DellaVigna and Gentzkow 2010).

Conclusion

To what extent the surge of high-speed internet has contributed to democratic erosion is contested. On the one hand, it may crowd out voters' consumption of traditional media with higher and more unbiased knowledge about politics, which is expected to reduce turnout. On the other hand, it may add to individuals' existing news consumption and increase political knowledge, which is expected to increase turnout. Furthermore, broadband internet may decrease individuals' sense of political efficacy by increasing income inequality. We evaluate these competing hypotheses, by exploiting a large-scale broadband reform that was rolled out in a staggered fashion across Norwegian municipalities during the 2000-2008 period. We find that increased coverage of broadband is associated with *larger* turnout. Our findings also suggest that the arrival of high-speed internet increased time online without replacing the consumption of traditional media. We find little evidence that labor markets are important in mediating the impact of increased broadband use on voter turnout. Our analysis therefore suggests that high-speed internet has more nuanced effects on electoral participation than what is commonly asserted.

Our study highlights that the effect of broadband on political participation is likely to be more dependent on pre-existing media institutions and patterns of inequality than considered in previous studies. Indeed, depending on the relative strength of media and inequality mechanisms, the overall effect of broadband internet expansion on turnout may be positive, negative, or zero. Because Norway has strong wage compression and an influential state broadcaster with high news content across all media platforms, the broadband effect on (i) inequality is likely to form a *lower* bound and (ii) on media complementary an *upper* bound. In countries with fewer constraints on wage formation and/or weak or absent public broadcasters, the total effect of broadband internet on turnout may instead be negative.

The reform provides a unique opportunity to study the impact of broadband on voter turnout but also has important limitations. First, we document an average effect across a range of municipalities that differ among potentially important dimensions. As a result, of regional differences in economic specialization, municipalities presumably differ in how the labor market mediates the impact of broadband coverage. Moreover, since the research design relies on changes in coverage between elections, we cannot explore effects after 2009 when the roll-out was completed. This would be of interest since the advent of social media has potentially changed the impact of broadband coverage in important ways (see e.g. Larson et al. 2019; Enikolopov, Makarin, and Petrova 2020). For the same reason, the research design does not enable us to identify the impact of long-term exposure to broadband. We believe these are both interesting and important avenues for future research.

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Online Appendix

Does High-speed Internet Erode Voter Turnout?
Evidence from a Large-scale Broadband Reform

Material intended for online publication only

Table of Contents

A Data Sources and Definitions

Turnout in local elections. Ratio of votes to eligible voters in the 1995, 1999, 2003, and 2007 local elections. *Source:* Fiva, Halse, and Natvik 2023.

Turnout in national elections. Ratio of votes to eligible voters in the 1997, 2001, 2005, and 2009 national elections. *Source:* data for 1997, 2001, and 2005 is from Fiva, Halse, and Natvik 2023. Data for 2009 is provided by the Norwegian Center for Research Data (NSD).

Turnout in national elections. Ratio of votes to eligible voters in the 1997, 2001, 2005, and 2009 national elections. *Source:* data for 1997, 2001, and 2005 is from Fiva, Halse, and Natvik 2023. Data for 2009 is provided by the Norwegian Center for Research Data (NSD).

Vote share left parties. Share of votes received by DNA, RV, SV, as well as joint lists of left-wing parties. *Source:* Fiva, Halse, and Natvik 2023.

Vote share right parties. Share of votes received by FRP, KRF, V, H, V, as well as joint lists of right-wing parties. *Source:* Fiva, Halse, and Natvik 2023

Vote share other parties. Share of votes received by lists not classified as left or right. *Source:* Fiva, Halse, and Natvik 2023.

Broadband coverage. Share of households with access to broadband services by municipality and year between 2000 and 2015. For years before 2000, we set coverage rates to 0. *Source:* Norwegian Ministry of Government Administration.

Broadband subscriptions. Share of households with subscription to broadband services by municipality and year between 2002 and 2009. For years before 2002, we set subscription rates to 0. *Source:* Norwegian Ministry of Government Administration.

Income. Average net income for all individuals in a municipality who are 17 years or older in a given year. Data are for the years 1993-2009. *Source:* Statistics Norway and provided by Norwegian Center for Research Data (NSD).

Education. Number of individuals by municipality and year by level of education (*grunnskole*, *videregående skole*, *fagskole*, *universitet kort*, *universitet lang*, *ingen eller uoppgitt*). *Source:* Statistics Norway and provided by Norwegian Center for Research Data (NSD).

Urban population. Share of the population in a municipality living in a contiguously built-up area by year. *Source:* Statistics Norway and provided by Norwegian Center for Research Data (NSD).

Children. Fraction of population aged 7 to 15 years for the period 1972-1996 and 6 to 15 years for the period 1997-2011 (pre-school age). *Source:* Fiva, Halse, and Natvik 2023.

Young. Fraction of population aged 7 to 15 years (school age). *Source:* Fiva, Halse, and Natvik 2023.

Elderly. Fraction of population aged 66 years and higher. *Source:* Fiva, Halse, and Natvik 2023.

Unemployment. Number of registered unemployed individuals on average per year as a share of the total number of inhabitants aged 16-66. *Source:* Fiva, Halse, and Natvik 2023.

Gini coefficient. The Gini coefficient is calculated for each municipality using data on individual incomes for the universe of the Norwegian population using administrative data. *Source:* Statistics Norway.

90/10 ratio. The 90/10 ratio is calculated for each municipality using data on individual incomes for the universe of the Norwegian population using administrative data. *Source:* Statistics Norway.

Longitude. The longitude of a municipality is defined by the administrative center. We supplement Fiva, Halse, and Natvik 2023 by including the longitude for 30 municipalities in our sample for which this information is missing using Wikipedia. *Source:* Fiva, Halse, and Natvik 2023 and Wikipedia.

Latitude. The latitude of a municipality is defined by the administrative center. We supplement Fiva, Halse, and Natvik 2023 by including the longitude for 30 municipalities in our sample for which this information is missing using Wikipedia. *Source:* Fiva, Halse, and Natvik 2023 and Wikipedia.

B Summary statistics

Table B.1. Summary statistics for the main dataset

Statistic	N	Mean	St. Dev.	Min	Median	Max
Broadband						
Coverage %	1,596	32.83	40.68	0.00	0.00	100.00
Subscriptions %	1,596	14.23	22.11	0.00	0.00	95.84
Election variables						
Turnout	1,596	63.06	5.84	43.25	62.54	86.69
Vote share left	1,596	36.88	13.80	0.00	36.19	100.00
Vote share right	1,596	54.75	17.64	0.00	56.48	94.10
Vote share other	1,596	8.36	15.04	0.00	0.24	100.00
Incumbent support	1,596	61.04	15.51	16.41	62.07	100.00
#Parties	1,596	6.09	1.60	0	6	11
Control variables						
Income	1,596	221,009.70	58,221.60	118,100.00	215,900.00	453,700.00
Education	1,596	13.09	3.65	5.95	12.46	28.31
Urban pop. share	1,596	48.29	27.99	0.00	48.70	99.80
Children	1,596	7.87	1.47	3.68	7.77	14.67
Young	1,596	13.14	1.73	7.86	13.14	19.24
Elderly	1,596	16.38	3.60	6.81	16.48	29.77
Unemployment	1,596	2.44	1.32	0.38	2.22	10.08
Gini coefficient	1,596	32.53	3.64	24.92	32.05	70.62
90/10 ratio	1,588	17.99	421.18	3.31	5.06	16,459.19

Notes: The dataset is a balanced panel based on three elections, (1995, 1999, 2003, 2007) $\times$ 399 municipalities = 1,596 observations. *Coverage* is the fraction of households with access to broadband measured from 0-100. *Subscriptions* is the fraction of households subscribing to broadband measured from 0-100.

Table B.2. Summary statistics for the local election surveys

Statistic	N	Mean	St. Dev.	Min	Median	Max
Control variables						
Age group	4,960	2.57	0.91	1	3	5
Gender	4,960	0.49	0.50	0	0	1
Years of schooling	4,918	13.31	2.60	0	13	20
Income	4,325	4.96	2.04	1	5	8
Exposure to information						
Radio	3,121	0.43	0.50	0	0	1
Television	3,235	0.46	0.50	0	0	1
Newspaper	3,248	0.85	0.35	0	1	1
Internet	3,168	0.17	0.37	0	0	1

Notes: Data is from the Local Election Surveys. Age is dichotomized in discrete bins ranging from 1-5: $age_g = 1$ if age $\in [16, 25)$, $age_g = 2$ if age$\in [25, 45)$, $age_g = 3$ if age$\in [45, 67)$, $age_g = 4$ if age$\in [67, 80)$, $age_g = 5$ if age≥ 80. Income is dichotomized in discrete bins ranging from 1-8. Years of schooling is based on the recorded highest level of education. Radio, television, newspaper, and internet are responses to the questions: *Have you read statements made by political candidates in the newspaper/radio/television/the internet?* Data are from the Local Elections Surveys 1995-2007.

C Tables

Table C.1. Broadband coverage and turnout in national elections

Dependent variable:	Turnout in national elections				
	(1)	(2)	(3)	(4)	(5)
Coverage %	−0.0055	−0.0021	−0.0088	0.0074	0.0088
	(0.0038)	(0.0029)	(0.0039)	(0.0029)	(0.0028)
Controls		✓	✓	✓	✓
Year × $m_{i,1999}$			✓		
County × Year FE				✓	
Region × Year FE					✓
Mean turnout	76.06	76.06	76.06	76.06	76.06
Municipalities	399	399	399	399	399
N	1,596	1,596	1,596	1,596	1,596
R^2	0.9192	0.9251	0.9291	0.9488	0.9652

Notes: The table reports OLS estimates. Regressions are based on data for four elections, (1997, 2001, 2005, 2009) × 399 municipalities = 1,596 observations. *Coverage* is the fraction of households with access to broadband in the year prior to the election measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population. Standard errors are clustered at the municipality level.

Table C.2. Internet usage and media consumption

Dependent variable:	Mintues spent on media source (day prior)					
	Television		Radio		Newspaper	
	(1)	(2)	(3)	(4)	(5)	(6)
Internet use (minutes day prior)	−0.0481	0.0060	−0.0299	−0.0032	0.0023	0.0550
	(0.0212)	(0.0211)	(0.0051)	(0.0048)	(0.0254)	(0.0265)
Year FE	✓	✓	✓	✓	✓	✓
Soc. + dem. controls		✓		✓		✓
N	8,836	8,836	8,833	8,833	8,836	8,836
R^2	0.0026	0.0261	0.0079	0.1296	0.0016	0.0175

Notes: The table reports OLS estimates. Regressions are based on data for the years 2000 to 2007. *Internet use* measures the number of minutes spent on the internet the day prior to the survey. Socioeconomic and demographic controls contain age, education, household income, urban/rural, and gender. Data are from the Norwegian Media Use Survey. Robust standard errors in parenthesis.

Table C.3. Broadband coverage and subscriptions

Dependent variable:	Share of households with broadband subscription				
	(1)	(2)	(3)	(4)	(5)
Coverage %	0.2067	0.1966	0.1426	0.1465	0.1533
	(0.0193)	(0.0172)	(0.0115)	(0.0111)	(0.0108)
Controls		✓	✓	✓	✓
Year × $m_{i,1999}$			✓		
County × Year FE				✓	
Region × Year FE					✓
Mean turnout	63.06	63.06	63.06	63.06	63.06
Municipalities	399	399	399	399	399
N	1,596	1,596	1,596	1,596	1,596
R^2	0.9730	0.9743	0.9793	0.9832	0.9884

Notes: The table reports OLS estimates. Regressions are based on data for four elections, (1995, 1999, 2003, 2007) × 399 municipalities = 1,596 observations. *Subscription* is the fraction of households connected to broadband measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population. Standard errors are clustered at the municipality level.

Table C.4. Broadband coverage and turnout (robustness checks)

Dependent variable:	Turnout in local elections				
	(1)	(2)	(3)	(4)	(5)
Panel (a): Dropping large cities					
Coverage %	0.0070	0.0096	0.0093	0.0144	0.0188
	(0.0054)	(0.0049)	(0.0050)	(0.0048)	(0.0055)
Panel (b): Unweighted model					
Coverage %	0.0147	0.0126	0.0104	0.0200	0.0227
	(0.0056)	(0.0046)	(0.0047)	(0.0047)	(0.0052)
Panel (c): Municipality trends					
Coverage %	0.0158	0.0128	0.0128	0.0109	0.0137
	(0.0048)	(0.0067)	(0.0067)	(0.0052)	(0.0066)
Panel (d): County trends					
Coverage %	0.0159	0.0156	0.0122	0.0134	0.0170
	(0.0043)	(0.0046)	(0.0056)	(0.0046)	(0.0055)
Panel (e): Region trends					
Coverage %	0.0159	0.0146	0.0122	0.0170	0.0170
	(0.0042)	(0.0046)	(0.0056)	(0.0055)	(0.0055)
Controls		✓	✓	✓	✓
Year × $m_{i,2000}$			✓		
County × Year FE				✓	
Region × Year FE					✓

Notes: The table reports OLS estimates. Regressions are based on data for four elections, (1995, 1999, 2003, 2007) × 399 municipalities = 1,596 observations. *Coverage* is the fraction of households with access to broadband measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Standard errors are clustered at the municipality level.

Table C.5. Broadband coverage and turnout (aggregating to region and county)

Dependent variable:	Turnout in local elections			
	(1)	(2)	(3)	(4)
Panel (a): Aggregating by region				
Coverage %	−0.0086	0.0105	0.0214	0.0194
	(0.0035)	(0.0055)	(0.0041)	(0.0106)
Region & year FE		✓	✓	✓
Controls		✓		✓
Year × $m_{i,1999}$			✓	✓
Mean turnout	62.8	62.8	62.8	62.8
N	360	360	360	360
R^2	0.7821	0.9017	0.9038	0.9078
Panel (b): Aggregating by county				
Coverage %	−0.0080	0.0155	0.0268	0.0555
	(0.0039)	(0.0074)	(0.0026)	(0.0225)
County & year FE		✓	✓	✓
Controls		✓		✓
Year × $m_{i,1999}$			✓	✓
Mean turnout	63.19	63.19	63.19	63.19
N	76	76	76	76
R^2	0.8546	0.9676	0.9745	0.9773

Notes: The table reports OLS estimates. *Coverage* is the fraction of households with broadband coverage measured from 0-100. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population size. Regressions in Panel (a) are based on data for four elections, (1995, 1999, 2003, 2007) × 90 regions = 360 observations. Standard errors in Panel (a) are clustered at the region level. Regressions in Panel (b) are based on data for three elections, (1995, 1999, 2003, 2007) × 19 counties = 76 observations. Standard errors in Panel (b) are clustered at the county level.

Table C.6. Broadband coverage and exposure to information

Dependent variable:	Exposure to information about local politics			
	Television	Newspapers	Radio	Internet
	(1)	(2)	(3)	(4)
Panel A: Interacted				
Coverage %	−0.00072	0.00051	0.00009	−0.00048
	(0.00080)	(0.00068)	(0.00089)	(0.00068)
$age_j \leq 44 \times$ Coverage %	0.00014	0.00040	0.00100	0.00127
	(0.00054)	(0.00043)	(0.00053)	(0.00048)
Ind. + muni. controls	✓	✓	✓	✓
N	2,859	2,872	2,768	2,802
R^2	0.31093	0.14423	0.21268	0.26657
Panel B: Uninteracted				
Coverage %	−0.00067	−0.00001	−0.00010	−0.00043
	(0.00081)	(0.00071)	(0.00078)	(0.00075)
Ind. + muni. controls	✓	✓	✓	✓
N	2,859	2,872	2,768	2,802
R^2	0.31092	0.13841	0.20344	0.25655

Notes: The table reports OLS estimates. All specifications include year and municipality fixed effects, county times year fixed effects, and controls for the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are based on data for four elections (1995, 1999, 2003, 2007). The estimates are weighted by the sampling probability. Standard errors are clustered at the municipality level.

Table C.7. Broadband coverage and inequality

Dependent variable:	Inequality				
	(1)	(2)	(3)	(4)	(5)
Panel (a): Gini coefficient					
Coverage %	0.0244	0.0140	0.0025	0.0097	0.0155
	(0.0102)	(0.0111)	(0.0153)	(0.0052)	(0.0046)
Mean Gini	32.53	32.53	32.53	32.53	32.53
Panel (b): 90/10 ratio					
Coverage %	1.9620	3.6093	−1.7079	−5.6252	−4.6608
	(4.0299)	(2.6136)	(1.7638)	(1.4867)	(1.1739)
Mean 90/10 ratio	17.99	17.99	17.99	17.99	17.99
Controls		✓	✓	✓	✓
Year × $m_{i,1999}$			✓		
County × Year FE				✓	
Region × Year FE					✓
Municipalities	396	396	396	396	396
N	1,588	1,588	1,588	1,588	1,588

Notes: The table reports OLS estimates. *Coverage* is the fraction of households with access to broadband measured from 0-100. The baseline controls contain share with higher education, population size, share of urban population, share children, share young, and share elderly. Regressions are weighted by the voting age population size. Standard errors are clustered at the municipality level.

Table C.8. Broadband coverage and turnout in local elections

Dependent variable:	Vote shares							
	RV	SV	AP	V	SP	KRF	H	FRP
Panel (a): Adjusted vote share								
Coverage %	−0.0037	0.0242	−0.0018	0.0114	−0.0015	−0.0006	−0.0182	0.0058
	(0.0009)	(0.0045)	(0.0050)	(0.0036)	(0.0022)	(0.0023)	(0.0045)	(0.0041)
Panel (b): Vote share								
Coverage %	−0.0057	0.0432	−0.0120	0.0051	−0.0052	−0.0024	−0.0280	0.0131
	(0.0016)	(0.0082)	(0.0087)	(0.0060)	(0.0037)	(0.0037)	(0.0075)	(0.0066)
Panel (c): Seat share								
Coverage %	−0.0055	0.0410	−0.0097	0.0176	−0.0044	−0.0025	−0.0360	0.0001
	(0.0019)	(0.0080)	(0.0095)	(0.0062)	(0.0042)	(0.0041)	(0.0078)	(0.0001)
Panel (d): Vote share (national)								
Coverage %	−0.0042	0.0039	−0.0028	0.0051	0.0108	0.0020	0.0038	−0.0133
	(0.0012)	(0.0046)	(0.0052)	(0.0028)	(0.0031)	(0.0022)	(0.0048)	(0.0044)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,596	1,596	1,596	1,596	1,596	1,596	1,596	1,596

Notes: The table reports OLS estimates. Regressions are based on data for four elections, (1995, 1999, 2003, 2007) × 399 municipalities = 1,596 observations. *Coverage* is the fraction of households with access to broadband in the year prior to the election measured from 0-100. The adjusted vote share is the share of the voting eligible population that cast votes for a given party. The baseline controls contain the share of the population residing in urban areas, the population size, and the share of the population aged 16-45. Regressions are weighted by the voting age population size. Standard errors are clustered at the municipality level.

D Figures

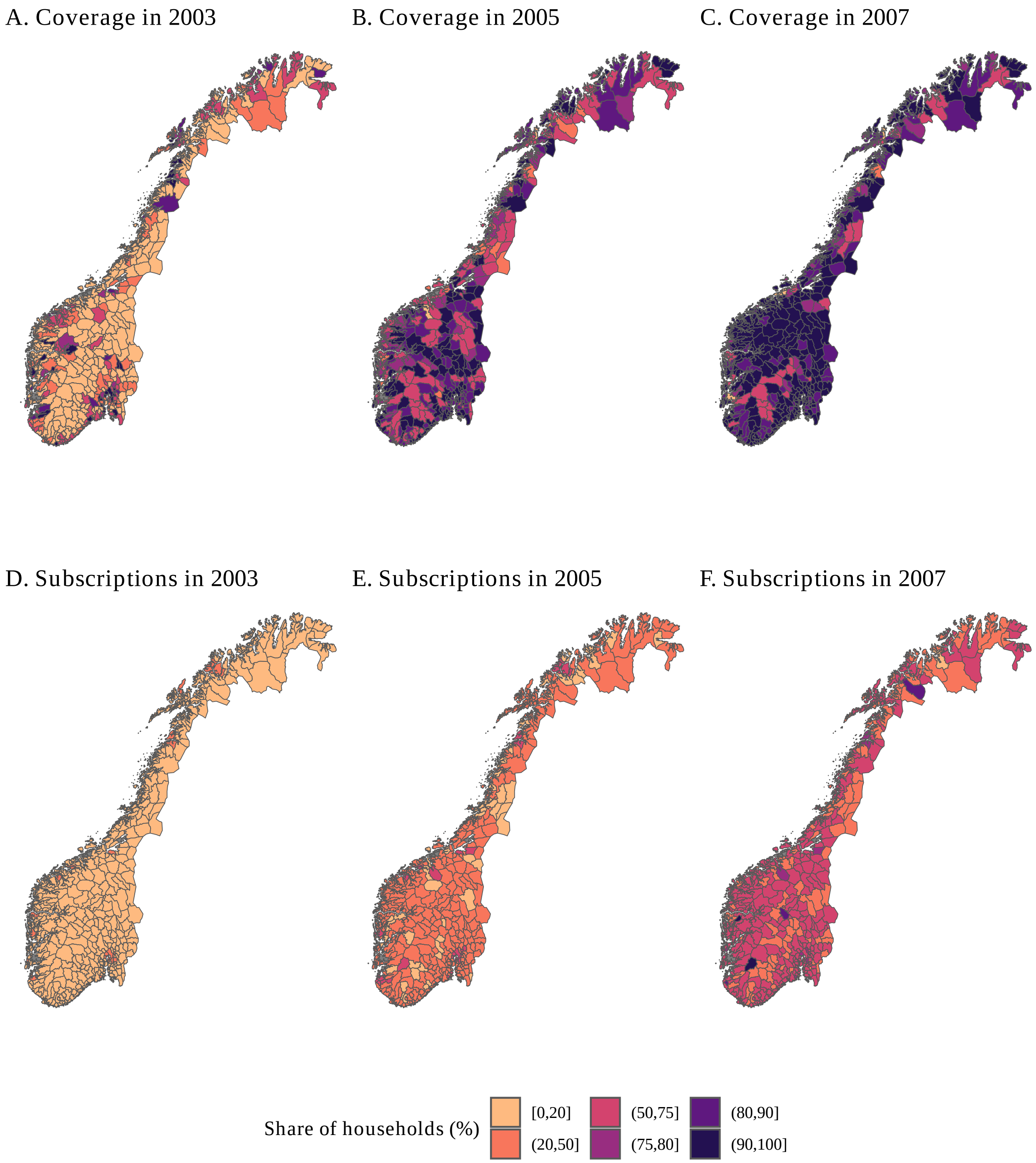

Figure D.1. The maps show the distribution of coverage/subscription rates across municipalities for the years 2003, 2005, and 2007. Source: Norwegian Ministry of Government Administration.

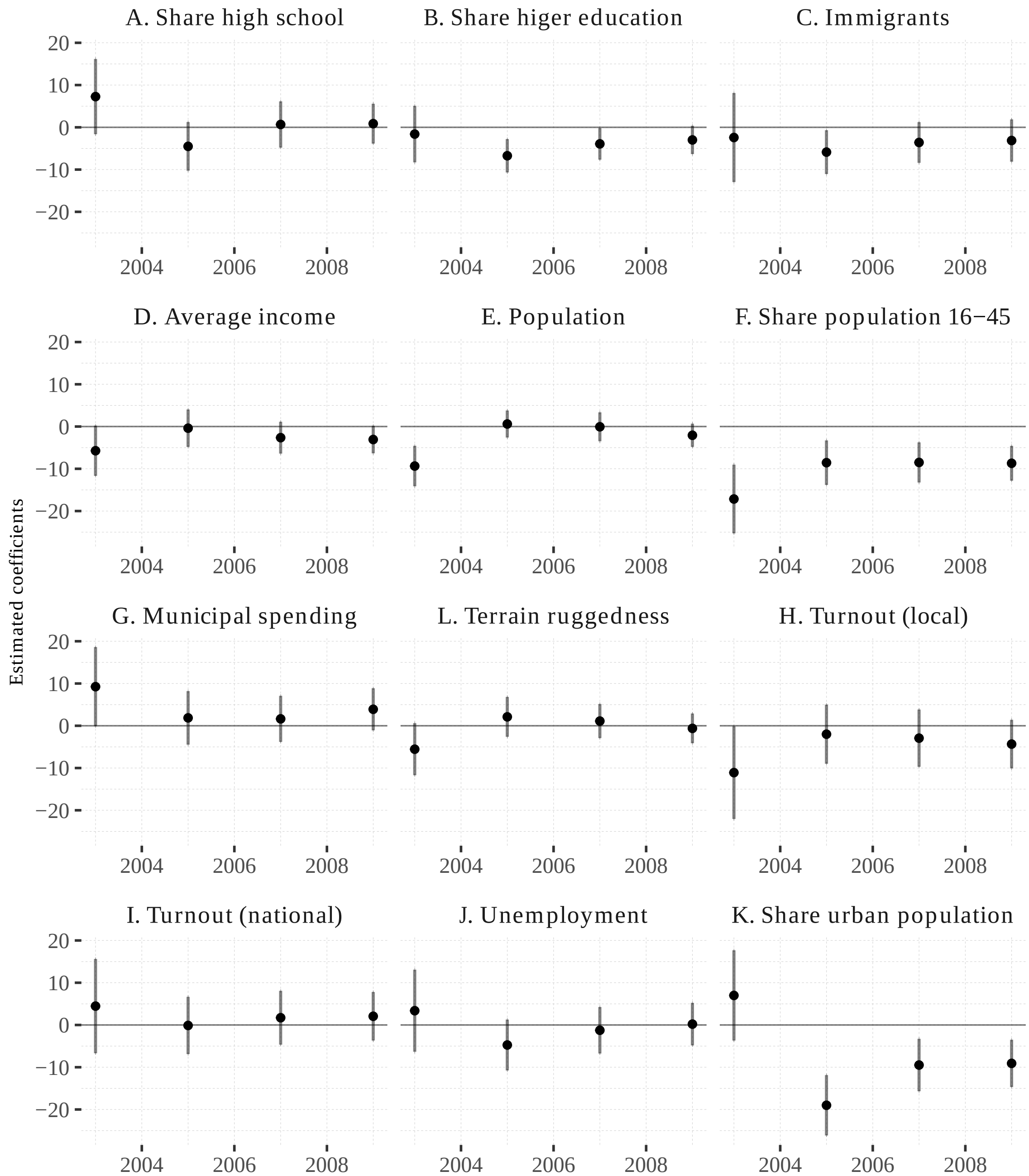

Figure D.2. The figure shows coefficients from changes in coverage rates regressed on baseline municipality characteristics in 1999 (the last local election prior to the reform). The specification is $\Delta c_{mt} = \theta_t \times x_m \lambda_t + \gamma_t + \epsilon_{mt}$ where Δc_{it} is the change in broadband coverage, θ_t are year indicators, and λ_t are the coefficients of interest. The figure plots the coefficients of the interaction terms (λ_t) together with the 95 percent confidence intervals. Standard errors are clustered at the municipality level.

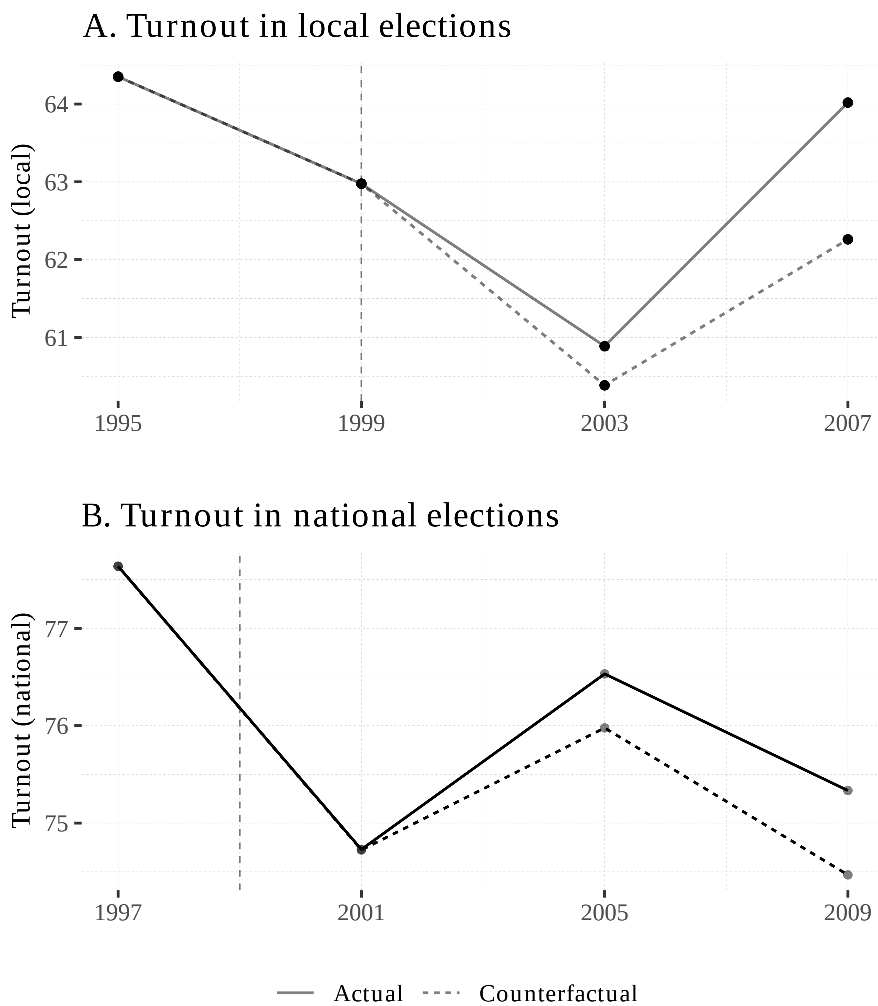

Figure D.3. The figure depicts the counterfactual turnout rates for national and local elections using the baseline model estimates presented in Columns (4) of Tables 2 and C.1. The model contains country times election fixed effects and the baseline controls (the share of the population residing in urban areas, the population size, and the share of the population aged 16-45). *Actual* denotes the evolution of turnout observed in the data. *Counterfactual* denotes the evolution in the absence of the rollout of broadband coverage.

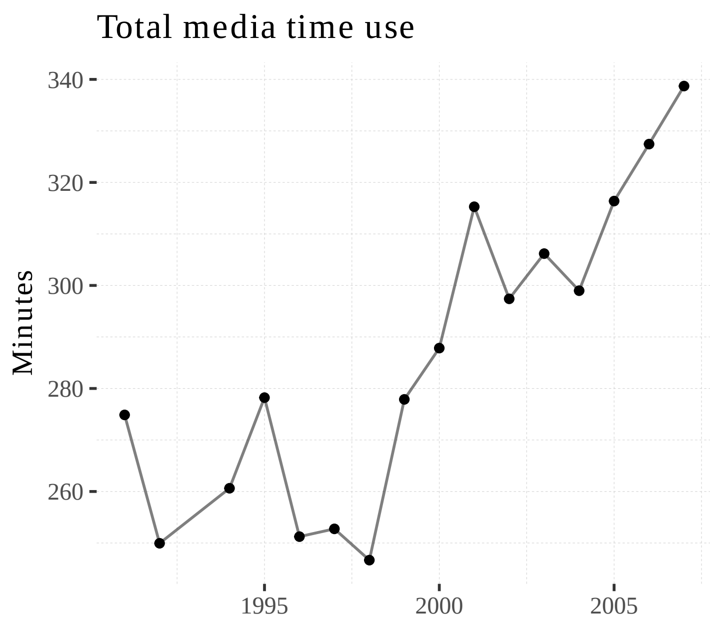

Figure D.4. The figure depicts the total amount of minutes spent on mass media for respondents over the reform period. Source: Media Use Survey, Statistics Norway.

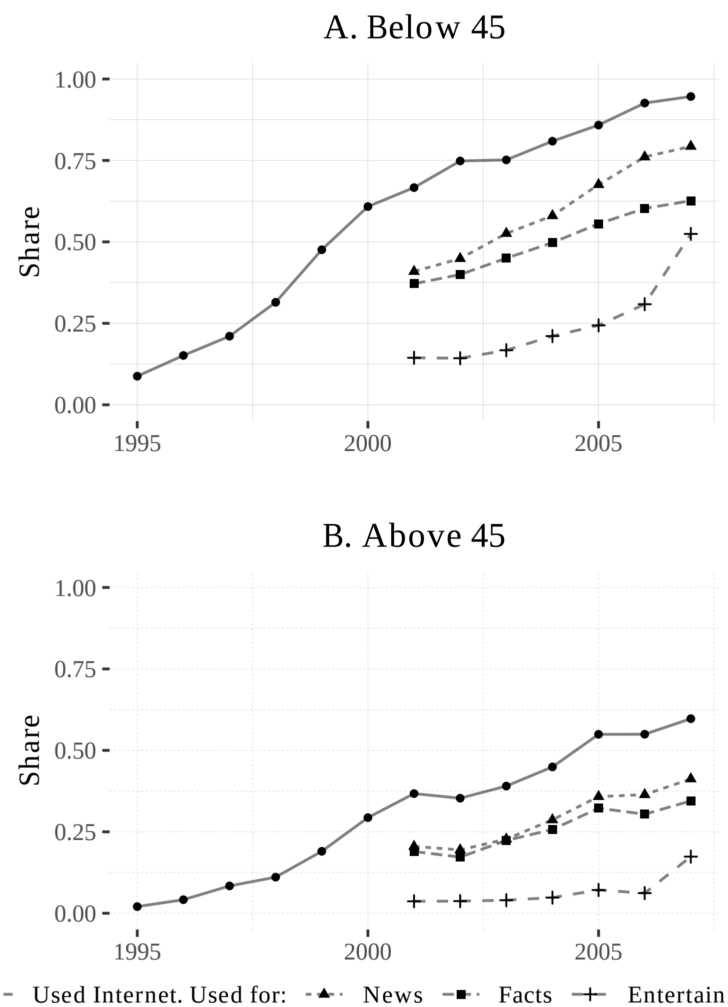

Figure D.5. The figure shows the fraction of respondents who have used the internet, and used the internet for news, fact checking, and entertainment for different age groups. Source: Media Use Survey, Statistics Norway.

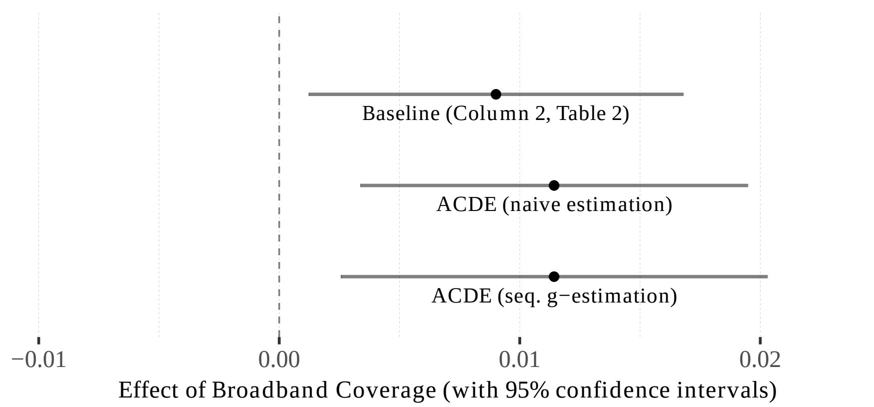

Figure D.6. The figure depicts the baseline estimate, the naive controlled direct effect, and the controlled direct effect using the sequential-g estimator with unemployment as the mediator. Regressions are weighted by the voting age population. The 95% CIs for the sequential-estimator are based on the asymptotic variance derived in Acharya, Blackwell, and Sen 2016.

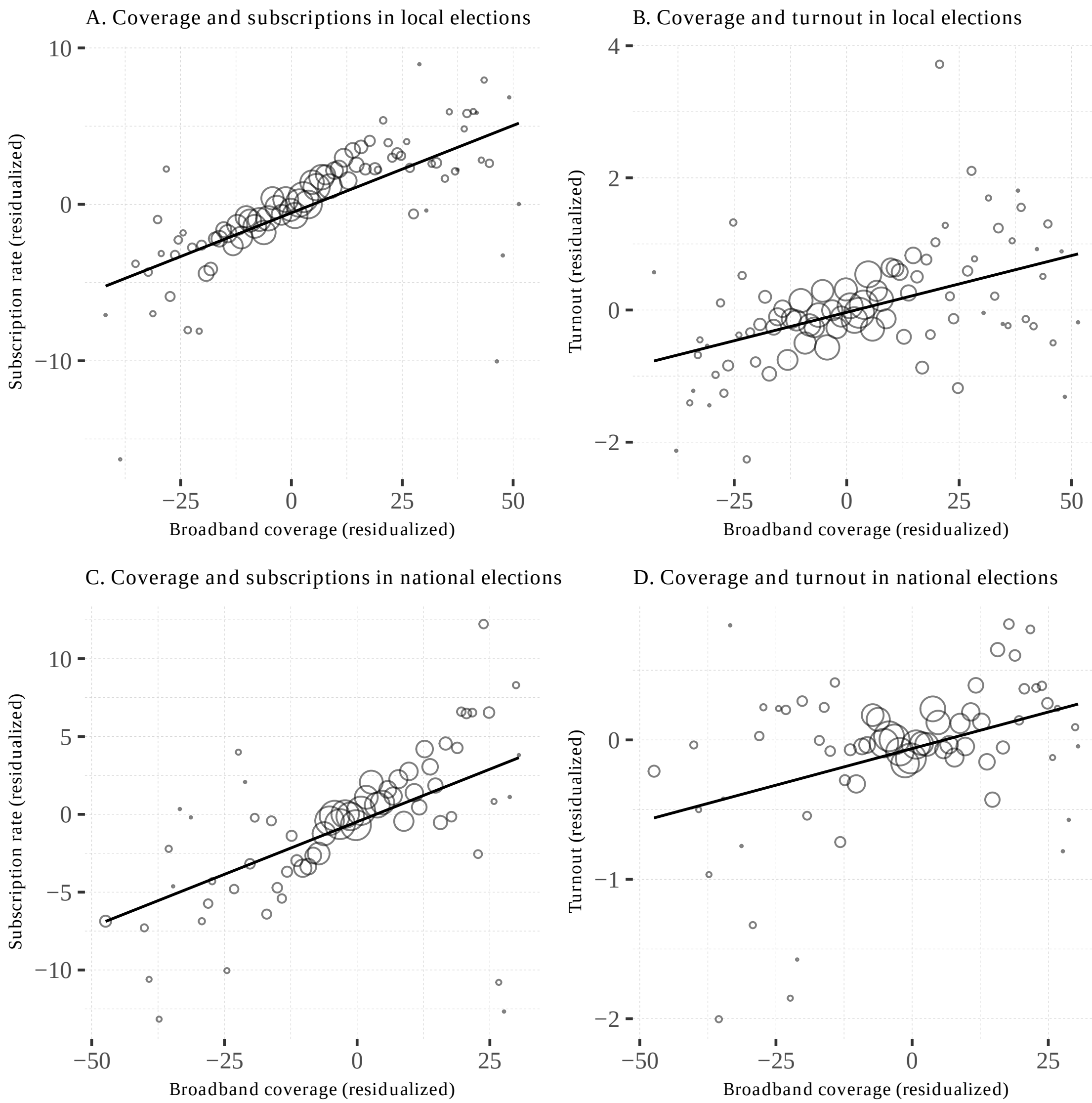

Figure D.7. The figure depicts the relation between broadband coverage, subscription rates, and turnout rates in national and local elections. All figures are residualized using the baseline specification in Equation 1. Regressions for local (national) elections are based on the 1995, 1999, 2003, and 2007 elections (1997, 2001, 2005, and 2009 elections). Panel A depicts the relation between subscription rates in a local election year and the coverage rate the year prior. Panel B depicts the relation between the coverage and turnout in local elections. Panel C depicts the relation between subscription rates in a national election year and the coverage rate the year prior. Panel D depicts the relation between the coverage and national in local elections.

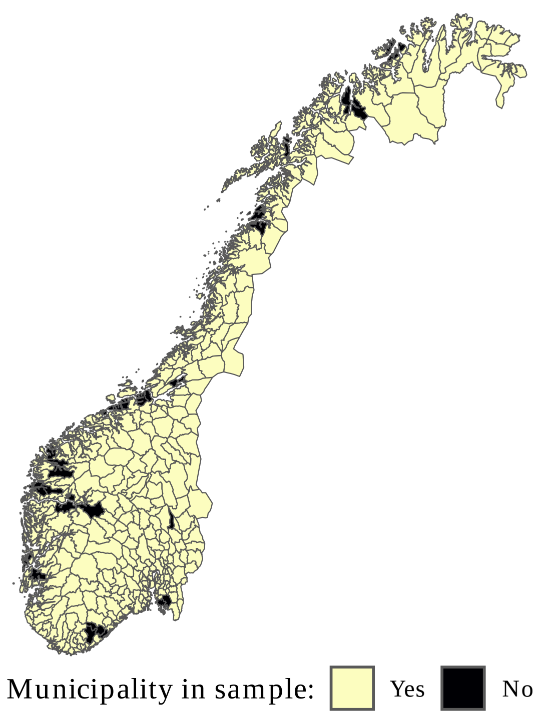

Figure D.8. The figure depicts the 399 municipalities in the sample. A municipality is in the sample if it exists and has the same municipality number throughout the 1990-2015 period and does not experience any border changes. The map displays 2000 municipality borders, the first year for which the sample contains data on broadband coverage.

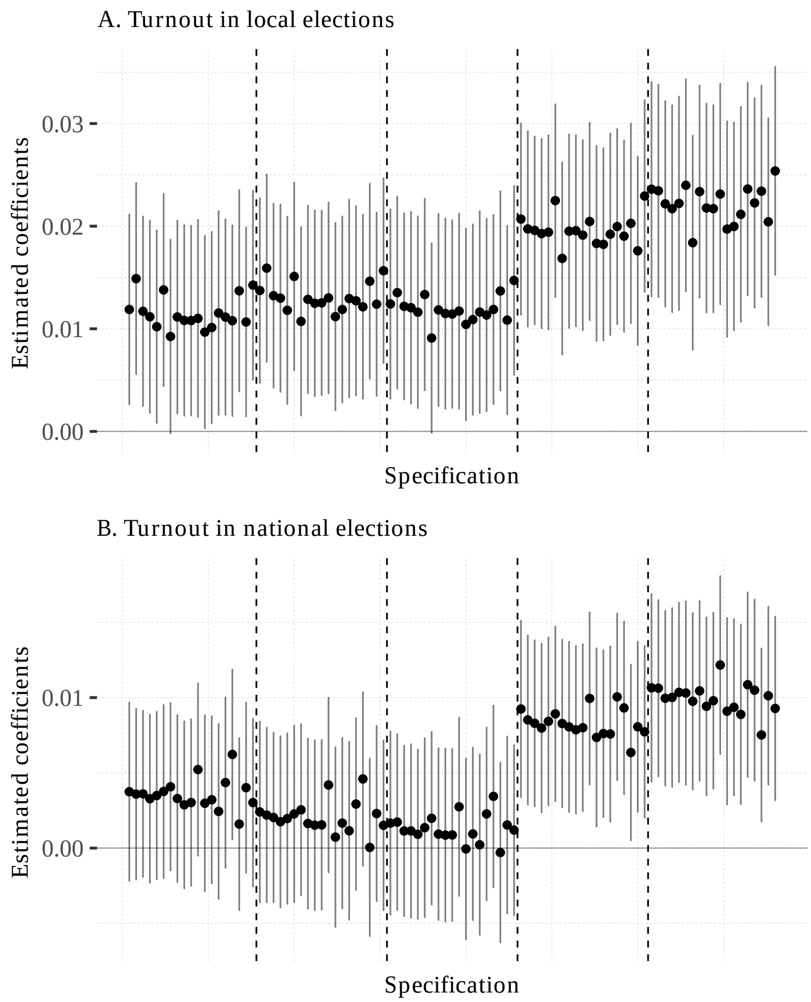

Figure D.9. The figure depicts the coefficients from the five regression models in Table 2 and Table 6. Each of the 95 coefficients is based on a regression where one of the 19 counties are dropped from the sample. Panel A depicts the estimates on turnout in local elections. Panel B denotes the estimates on turnout in national elections.

E ACDE Estimation Details

In this section we elaborate on the assumptions underlying the estimation of the average controlled direct effect (ACDE). To this end, we reproduce several insights from Acharya, Blackwell, and Sen 2016 applied to our context. The treatment of interest is the level of broadband coverage in municipality m denoted C_m. Next, let L_m denote a labor market mediator, e.g. unemployment. Let X_m denote the set of pre-treatment confounders (for example time invariant demand and supply factors) and Z_m the set of intermediate confounders. Finally, $T_m(c)$ denotes the potential outcome for municipality m under broadband coverage c. Acharya, Blackwell, and Sen 2016 show that the ACDE is identified by the sequential g-estimator under the following assumptions:

Assumption 1 *(Sequential Unconfoundedness)* $T_m \perp\!\!\!\perp C_m \mid X_m = x$ and $T_m \perp\!\!\!\perp L_m \mid C_m = c, X_m = x, Z_m = z$ for all values x, c, z, and mediator values l. Moreover, for the above values $P(C_m = c \mid X_i = x) > 0$ and $P(L_m = l \mid C_m = c, X_m = x, Z_m = z) > 0$.

Assumption 2 *(No intermediate Interactions)* $E[T_m(c,l) - T_m(c,l') \mid X_m = x, C_m = c, Z_m = z] = E[T_m(c,l) - T_m(c,l') \mid X_m = x, C_m = c]$.

Assumption 1 is the absence of U_1 and U_2 in Figure E.1. While we find evidence consistent with the absence of U_1, the absence of U_2 is unlikely to hold in practice. To assess the extent to which this leads the sequential-g estimator to be inconsistent, we consider a sensitivity analysis developed in the aforementioned paper.

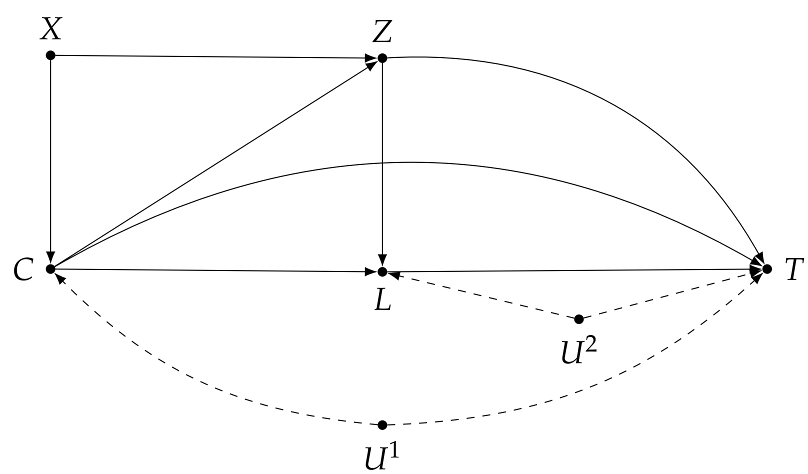

Figure E.1. A directed acyclical graph showing the relationship between broadband coverage and turnout. Dashed lines denote unobserved variables.

Consider the parametric model estimated in the paper. The turnout rate T_{mt} and labor market outcome L_{mt} in municipality m during election t are given by,

$$T_{mt} = \phi L_{mt} + \alpha c_{mt-1} + \gamma x'_{mt} + \varepsilon^T_{mt}, \tag{5}$$

$$L_{mt} = \theta c_{mt-1} + \lambda x'_{mt} + \varepsilon^L_{mt}, \tag{6}$$

where ε_{mt}^{k} are error terms. x'_{mt} now contains both the baseline controls as well as municipality and year fixed effects. From Assumption 1 it follows that there are no omitted variables which is the case when $E[\varepsilon_{mt}^{T} \mid L_{mt}, c_{mt-1}, x'_{mt}] = E[\varepsilon_{mt}^{L} \mid c_{mt-1}, x'_{mt}] = 0$. It follows from Assumption 1 that $\text{Cov}(\varepsilon_{mt}^{L}, \varepsilon_{mt}^{T}) = 0$. Moreover, let $\tilde{\varepsilon}_{m}^{T} = T_{mt} - E\left[T_{mt} \mid c_{mt-1}, x'_{m}\right]$. It then follows that the probability limit of the sequential g-estimate as a function of $Cov(\varepsilon_{mt}^{L}, \varepsilon_{mt}^{T})$ is given by

$$\text{plim } \widehat{ACDE}_{sg} = ACDE - \theta\rho \frac{\text{Var}\left(\tilde{\varepsilon}_{m}^{T}\right)}{\text{Var}\left(\varepsilon_{m}^{L}\right)} \sqrt{\frac{(1-\tilde{\rho}^{2})}{(1-\rho^{2})}}, \tag{7}$$

where $\tilde{\rho} = \text{Cov}\left(\tilde{\varepsilon}_{m}^{T}, \varepsilon_{m}^{L}\right)$. Given ρ, we can therefore measure the sensitivity of the inconsistency of the sequential-g estimator of the ACDE as a function of ρ. Figure E.2 presents the results. From the figure, we can see that for most values of ρ there is an effect of coverage on broadband that goes over and beyond its effect through the labor market. However, for larger negative values of ρ, we cannot rule out that the effect of broadband on turnout is mediated through the labor market.

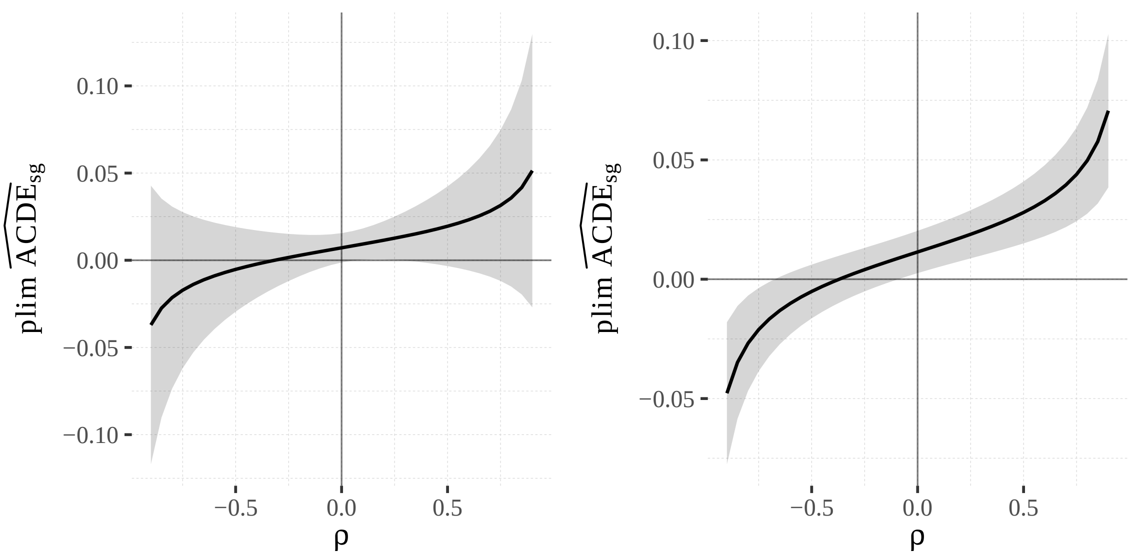

Figure E.2. The figure depicts the relationship between the sequential-g estimate and the correlation between the mediator and outcome errors. The mediator in the left panel is the Gini coefficient. The mediator in the right panel is the unemployment rate.

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