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From Couch to Poll: Media Content and The Value of Local Information†

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We document the importance of local information in mass media for the political engagement of citizens and accountability of politicians. We study this in the context of Canada, where until 1958, competition in television markets was suppressed—Canadians received either public or private television content, but never both. While public television provided national-level informational content, private television content was distinctly local and more politically relevant to voters. We find that the introduction of television reduced voter turnout, but that this effect is exclusive to public television districts. Our findings qualify existing knowledge about the political effects of the rollout of new media, by allowing the informational content to vary while holding the media type constant. We support our argument with evidence from parliamentary debates: politicians from districts with private television are more likely to speak and act on behalf of their constituents in Parliament. Our findings thus suggest that politicians are held accountable by relevant media content.

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1 Introduction

Mass media can provide valuable information about politics. An informed electorate is more engaged in the political process, more likely to vote and better able to hold politicians accountable (Ferraz and Finan, 2008; Snyder and Strömberg, 2010). But mass media also serves to entertain, and political disengagement can result if entertainment content crowds out relevant information for voters (Gentzkow, 2006). With any long-term change to the media landscape—e.g., the introduction of a new media—it is not clear whether political engagement will prevail. The effect new media has on the political process not only depends on its informational content, but also on who adopts the new media as a source of political information. Understanding *who* adopts *what* information with new media is thus necessary to reconcile the conflicting evidence around mass media's impact on political engagement.

In this paper, we study a long-term change to the media landscape—the introduction of television in Canada—and show that the extent of local information contained within new media explains the direction of change in political engagement. The Canadian context provides an ideal empirical setting because television developed as a dual system of public and private broadcasters, but a "one-station policy" prevented the expansion of both broadcasters into the same market. Voters had no choice, receiving either public or private television but never both. And yet content varied across these broadcasters: private stations aired a steady diet of local news and information because it was cheap to produce and offset the cost of importing American entertainment, whereas public stations aired news and information reflective of the nation as a whole. In this ideal setting, we know *who* adopts *what* type of television, so we can (i) compare the turnout of voters who receive the same new media type—television—but have no choice over available content—local or national information—and (ii) compare the response of politicians elected by these voters who possess different types of information.

While districts initially show similar levels of political engagement, we find that the introduction of television reduces voter turnout in all subsequent elections—an effect exclusive to public television districts. We interpret this finding as a consequence of the political value of local information aired on private stations, rather than being an effect specific to television, since media type is held constant in the analysis. We then consider how elected politicians speak and act in Parliament, and provide evidence that highlights the value of local information for political accountability.

Television marked a permanent change in how Canadians consumed information after its arrival in September 1952. In just two years, 75 percent of the population lived within range of a television signal (Peers, 1979), and by 1958 more than 90 percent of the population had access to television (Cole, 2002).[1] This rapid expansion was the intended purpose of the one-station policy, which facilitated a network expansion outward across space rather than concentrating in the country's most populated places.

[1] Over a similar time frame, television reached only 70 percent of the US population (Gentzkow, 2006), despite being a smaller country in terms of area.

Newspaper and radio consumption dropped significantly after 1952 due to this rapid expansion of television and its instant popularity. Past research suggests that a substitution towards television can reduce voter turnout because television brought with it an unprecedented amount of entertainment content (Campante et al., 2022). But this mechanism cannot explain a fall in voter turnout that is exclusive to public television districts: daily newspaper circulations decline in public television districts at a similar rate as private districts, and programming records for our sample period verify that entertainment content is balanced across public and private stations.

Instead, programming records indicate that private television stations aired more than twice the amount of local informational content compared to public stations at any time of day. This led individuals living in private television districts to substitute local newspapers and radio programming for local television news, and since the relevance of information did not change, neither did their level of political engagement. Whereas the national news broadcast to public television viewers implied a sharp drop in the local relevance of political information after the arrival of television, thus explaining why voter disengagement is exclusive to public television districts.[2]

We support this interpretation by linking the engagement of citizens to the accountability of politicians. Existing evidence suggests that if media content is informative and politically relevant, it will engage the electorate and hold politicians accountable (Strömberg, 2015). A canonical example of this is Snyder and Strömberg (2010), who find that locally relevant political information in newspaper markets improves both the selection and the incentives of politicians. In our context, if the local relevance of information is what links media to political engagement, we should expect an improvement in the selection and incentives of politicians in private television districts, relative to public television districts. For example, the positive accountability effects of an informed electorate will result in the selection of politicians who advocate and speak on behalf of the local communities they represent in Parliament. These positive accountability effects also create incentive for politicians to act on behalf of their constituents, including a willingness to vote across party lines in Parliament.

We test both of these propositions. In terms of selection, we develop various measures of speech localness, based on digitized debates among politicians in the House of Commons—the lower house of Parliament. We find that members of Parliament (MPs) speak more about the local communities they represent when their constituents receive private television but not public television. In terms of actions, we construct a measure of Parliamentary dissent—how often MPs vote against their own party on a House motion in each round of Parliament. We find that MPs are more willing to dissent when representing private television districts, unlike their counterparts representing public television districts. Altogether, this evidence indicates that the local relevance of media content evokes accountability in both the words and actions of elected politicians.

These findings cannot be explained by the potential bias of media. We find no evidence of shifts in vote shares across party lines or the left-right political spectrum after the arrival of public

[2]Political information is indeed more relevant when it highlights the candidates running for office in a local district and their policy preferences, especially in a parliamentary democracy like Canada, where, even in federal elections, constituents vote for a local politician to be their representative in Parliament.

or private television. We similarly find no evidence of television creating an advantage for the incumbent politician or party in a district. Moreover, theory suggests that biased news will reinforce prior beliefs and polarize the electorate (Bernhardt et al., 2008; Chan and Suen, 2008), but we document the opposite: a small but equal reduction in vote share polarization across both public and private television districts. The lack of a differential effect across these various outcomes cannot explain the pattern in political engagement that we document across public and private television districts.[3]

Our treatment variable—television signal strength—combines multiple sources of variation, including the non-random timing and location of television transmitter installations. This non-randomness creates an omitted variable bias problem: even if the timing of transmitter installations are conditionally random, districts located near the economic centers of the country are more likely to be treated than districts on the periphery. In this example, the *timing* of a district's exposure to treatment is likely non-random, and neither two-way fixed effects nor pre-treatment parallel trends are sufficient to guarantee an unbiased estimate of our treatment variable.

For our analysis, we introduce a novel measure of expected signal strength that enables a causal interpretation of our estimates. Based on the insights of Borusyak and Hull (2023), we develop a measure of expected signal strength by permuting the timing of transmitter installations in a given year, while conditioning on the correct number of active transmitters and their sampling probability. The result is a new distribution of signal strength that closely, but not exactly, matches the distribution of actual signal strength. We permute this counterfactual distribution 500 times and calculate the average signal strength for each electoral district. The average of these counterfactual distributions is a summary measure of a district's non-randomness in television exposure—it's *expected* signal strength. Including this measure in our baseline specification accounts for the aforementioned omitted variable bias and, conditional on district and election-year fixed effects, identifies the causal effect of *actual* signal strength on political engagement. The intuition of this solution is straightforward: the variation that we rely on in a regression is the difference between the observed and counterfactual network—signal strength above or below what is expected—and thus an outcome of chance.

We also show that our results are robust to adjustments for free-space signal strength—the signal loss across space in an environment free of any topographical variations (i.e., line-of-sight propagation). After controlling for free-space signal strength in a regression setting, an estimate of actual signal strength relies only on plausibly exogenous variation generated by topographical barriers between transmitters and receivers (Olken, 2009).

Our results shed new light on the value of local information in a democracy, and contribute to a large literature documenting mass media's impact on political engagement.[4] The closest paper

[3]The persuasive effects of politically biased news is well documented elsewhere. For example, DellaVigna and Kaplan (2007) is an early example of the persuasive effects of Fox News on voting behavior, with additional support from Martin and Yurukoglu (2017) and Ash et al. (2022). The language and slant of media also impacts political preferences (Djourelova, 2023) and the subsequent reporting of news (Widmer et al., 2023).

[4]For example, television tends to reduce voter turnout (Gentzkow, 2006; Althaus and Trautman, 2008; Campante and Hojman, 2013; Ellingsen and Hernæs, 2018; Angelucci et al., 2023) and shapes the political allegiance of voters

to ours is Gentzkow (2006), who studies the impact of television on voter turnout in US elections. He attributes the observed fall in turnout to a substitution away from newspapers and radio, where the predominance of entertainment content on television crowds out locally relevant political news consumption—an effect well-documented in other contexts (Ellingsen and Hernæs, 2018; Durante et al., 2019; Campante et al., 2022). This crowding out of locally relevant information is typically observed by comparing a new media type (e.g., television) to an alternative media type, where the decision of voters to adopt one source of information over another is non-random. In our empirical setting, we can observe differences in local content within a single media type, and the one-station policy solves the endogeneity problem of content choice, implying that a television viewer's exposure to local or national information is observable and as good as random.[5] And the fact that entertainment content is balanced between public and private television stations holds constant potential differences in crowding out between public and private stations, thus highlighting the salient role of local information.

Our findings also build on the work of Snyder and Strömberg (2010), who link local information in newspapers to increased political engagement among citizens, and in turn greater political accountability among politicians. The accountability-enhancing effects of local news takes many forms, including an improved selection of politicians (Ferraz and Finan, 2008; Larreguy et al., 2020) and better policy outcomes (Besley and Burgess, 2002; Strömberg, 2004b). Theoretical work supports this idea, where the issues media covers matters for voter information, and thus political engagement and accountability (Strömberg, 2004a; Chiang and Knight, 2011; Prat and Strömberg, 2013; Strömberg, 2015; Abramson and Montero, 2023). Local news can also temper support for ideologically extreme candidates, although competition with online platforms threatens the sustainability of this accountability effect (Djourelova et al., 2024). We contribute to this literature with evidence that locally relevant informational content elicits political engagement among the electorate, and that this engagement also elicits accountability among elected politicians who are more willing to speak and act on behalf of their constituents in Parliament.

We also contribute a novel solution to the non-randomness of television transmitter installations. In most studies where the treatment is a measure of signal strength across space, for either television or radio, a common solution is to control for free-space signal strength (Olken, 2009; Enikolopov et al., 2011; Yanagizawa-Drott, 2014; DellaVigna et al., 2014; Adena et al., 2015; Bursztyn and Cantoni, 2016; Wang, 2021, among others). Yet in free space, where line-of-sight propa-

(DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Martin and Yurukoglu, 2017; Durante et al., 2019; Ash et al., 2022). The Internet similarly has depressive effects on political engagement (Falck et al., 2014; Gavazza et al., 2019), although the evidence is mixed depending on the context (Miner, 2015; Campante et al., 2018; Donati, 2023), and in some instances can elicit political engagement in the form of protests (Fergusson and Molina, 2019; Enikolopov et al., 2020; Manacorda and Tesei, 2020; Amorim et al., 2022; Enikolopov et al., 2023). A large literature also documents the positive impact of media content on political engagement, particularly among newspapers and radio (Strömberg, 2004b; Snyder and Strömberg, 2010; Gentzkow et al., 2011; Drago et al., 2014; Wang, 2023).

[5] Bursztyn et al. (2023) and Durante et al. (2019) both rely on within-television content variation in settings somewhat similar to ours. Durante et al. (2019) compare differences in political outcomes across public television districts following the staggered introduction of private television. Bursztyn et al. (2023) compare adopted beliefs about the COVID-19 pandemic across viewers of different television programs, and develop an instrumental variable strategy to address the problem of a viewer's non-random choice over content.

gation implies no topographical variations, signals decay at the inverse rate of distance squared, which is unlikely to be the appropriate distance elasticity in our context, since television transmitters are often located on top of a mountain or hill—i.e., regions with a different topography than free space. The free-space approach also does not take the non-random timing of a transmitter's installation into consideration, which is a problem in a panel context like ours. Heeding the advice of Borusyak and Hull (2023), our approach takes the actual topography of the Canadian landscape into consideration, in addition to the timing of installation, giving us a control for the non-randomness of a television signal that is based on a more realistic counterfactual topography. Although we show that both estimation strategies yield similar results, we find that some key determinants of treatment are only balanced when adjusting our estimates for expected signal strength, which underscores the contribution of our identification strategy.

2 Historical Background

The empirical analysis draws on two key aspects of the regulatory environment in which Canadian television emerged: (i) a dual system of public and private broadcasters and (ii) a one-station policy that prevented the expansion of both public and private television into the same market. In this section, we provide context and background for this regulatory environment, describing the conditions that pre-date the arrival of television, and how those conditions shaped the arrival and expansion of the television network thereafter. We then highlight how the different objectives of the public and private broadcasters led the private broadcasters to adopt relatively more local informational content.

2.1 The Broadcasting Years Before Television (1936-1951)

The *Broadcasting Act* received royal assent in Parliament on 23 June 1936, making it the outline of radio broadcasting policy in Canada. Among other things, the *Act* cemented the dual system of public and private broadcasting with the creation of Canada's public broadcaster—the Canadian Broadcasting Corporation (CBC).[6] The *Act* also established the CBC as the regulator of the broadcasting system, with privately owned radio stations deemed affiliates of the public network.[7] This institutional arrangement remained in place until the 1958 revision of the *Broadcasting Act*.

In the years preceding the *Broadcasting Act*, radio was characterized by the forces of the free market, which ushered in American content at an alarming rate, at least in the eyes of the political elite (Filion, 1996). The creation of the CBC was the government's response to this commercialization of the airwaves and the dominance of American radio content in Canada. Still a young nation at the time, the adolescence of Canadian culture was a concern of policymakers who feared that

[6] Previous to the creation of the CBC, the Canadian Radio Broadcasting Commission (CRBC) was established as a public entity in radio broadcasting. However, the CRBC was short-lived because of Depression-era funding issues, and because the CRBC was criticized for not being at arm's length from the incumbent government (Armstrong, 2010).

[7] Despite the official status as a CBC-affiliate, private radio stations still maintained considerable autonomy over content provision. Indeed, the CBC "imposed little control on its affiliates." (Filion, 1996, p. 455)

the country's increasing economic integration with the US would inevitably lead to an adoption of American culture, rather than the maturation of a unique Canadian cultural identity (Armstrong, 2010).[8] This concern led the government to view the airwaves as public domain, and while private broadcasters may co-exist within this domain, the government's intent was to have the CBC—a public entity—oversee and enforce a system that reflects Canadian values and supports made-in-Canada content (Weir, 1965).

While the CBC was initially seen as a "triumph of culture over commerce," it soon fell short of its goal to weaken the nation's dependence on American radio content (Rutherford, 1990, p.18). Prerecorded American content remained extremely popular in Canada, airing on both public and private radio stations, all the while household radio ownership jumped to 75 percent by 1941 and over 90 percent by 1949 (Rutherford, 1990). The overall Americanization of the Canadian airwaves generated skepticism around mass media and its accompanying spread of American culture—would Canadian culture be defined by the likes of Hollywood? The political elite remained fearful that Canada was on a path of assimilation with its southern neighbour, and that Canada must break this cultural dependence and forge its own identity (Weir, 1965).

The Massey Commission (1949-1951) The Canadian cultural vacuum that stoked these fears of American influence put concern over cultural identity back into the government's spotlight. The Royal Commission on National Development in the Arts, Letters and Sciences—commonly known as the Massey Commission after its chair, Vincent Massey—was formed in 1949 to investigate the state of the country's cultural identity. The Massey Commission was tasked with nothing less than redefining the trajectory of Canadian cultural development.[9]

The Massey Commission released its report on 1 June 1951, where the issue of how Canada should regulate its television network occupied considerable space within the report. Two major narratives emerged from the report that defined the regulation of television in the years to come: a warning against permanent dependence on American culture, and the need for a state-sponsored approach to cultural development (Rutherford, 1990). Then Prime Minister Louis St. Laurent and his government embraced the recommendations of the Massey Commission, adopting the report as a blueprint for the inauguration and regulation of television in Canada.

2.2 The Arrival and Expansion of the Television Network (1952-1958)

Canadian television signed on in Montreal on 6 September 1952 and two days later in Toronto on 8 September 1952. On the recommendation of the Massey Commission, these two public stations

[8] The concern of American influence over Canadian culture was not new in 1936. The Royal Commission on Radio Broadcasting ("The Aird Commission") was previously tasked with assessing how radio broadcasting could best meet the needs of Canadian listeners while satisfying the interests of the nation. The first formal appeal for a public broadcaster was made in the Aird Commission's 1929 report.

[9] The scope of the Massey Commission report was unprecedented at the time, and is still considered a landmark document in Canadian history. To inform the report, the commissioners visited all 10 provinces, holding over 100 public hearings in 16 cities, hearing from over 1,200 citizens and receiving 462 briefs on different topics. It is estimated that the commissioners travelled more than 16,000 kilometers over the course of the two-year study (Canada Council).

were established to get a hold on television broadcasting before any private station was granted a broadcasting license. On 8 December 1952, member of Parliament and Minister of National Revenue, J. J. McCann, spoke to the House of Commons on the government's adopted policy, echoing the sentiment of the Massey Commission and the direction of broadcasting policy:

> *"The government believes [television] should be so developed in Canada that it is capable of providing a sensible pattern of programming for Canadian homes with at least a good portion of Canadian content reflecting Canadian ideas and creative abilities of our own people and life in all parts of Canada. [...] In addition the government will now be ready to receive applications for licences for private stations to serve areas not now served or to be served by publicly-owned facilities already announced. ... The objective will be to make national television service available to as many Canadians as possible through co-operation between private and public enterprise. [...] Since the objective will be to extend services as widely throughout Canada as is practicable, no two stations will be licensed at the present to serve the same area. [...] It is desirable to have one station in as many areas as possible before there are two in any one area."* (Bird, 1988, p. 241)

The One-Station Policy and Rapid Network Expansion The development of the one-station policy was a response to a set of problems defined by the size of Canada and its proximity to the US. The first problem was cost: the Canadian territory is vast in size and sparsely populated, implying that a significant number of television transmitters were needed to service the entire nation (Fowler and Smythe, 1957).[10] The high cost to service a small market put a limit on the profitability of expansion, and thus worked against the government's objective to extend the national television service to all Canadians. As a solution, the one-station policy facilitated a network expansion of public and private broadcasters across space, rather than a network concentration in large and profitable markets. The second problem was timing: regulators believed a rapid expansion of the television network was necessary to avoid any reliance on the extensive American programming that was already available for import at a lower cost than producing new Canadian entertainment content (Rutherford, 1990). To avoid permanent dependence on American programming, as the Massey Commission warned, an expeditious development of a nationwide network was believed to be essential for self-sufficiency.

The one-station policy served its intended purpose and led to a rapid and nationwide expansion: in just 2 years, television reached 75 percent of Canadian households (Peers, 1979), and by 1958, approximately 90 percent of Canadians had access to television (Cole, 2002). A full coast-to-coast network was established by 1958, at the time making it the largest television network in the world in terms of geographic coverage. In just 6 years, a nationwide service was established, made up of 55 television transmitter and rebroadcasting towers—12 public and 43 private. Not

[10] This feature of the Canadian landscape comes at a high cost. For example, the market that a single television station in Chicago could reach in 1957 was approximately the same size as the nationwide market that all 38 Canadian television stations serviced at the time (Fowler and Smythe, 1957).

only was this expansion rapid, but so was take up: by March 1958, more than 3 million television receivers were in use (DBS, 1959), and the average household watched 4 hours and 45 minutes of television daily, making it the dominant form of home entertainment and news consumption (DBS, 1961).

2.3 Broadcasting Objectives and Content Differences

Public and private broadcasting face different objectives. As a form of mass communication, public broadcasting is "designed to serve its audience as citizens rather than as consumers; it sees its viewers and listeners as a public, a *demos*, rather than a market." (Rowland, 2013, p. 8) Whereas an unregulated private broadcaster is motivated by profit; it sees its viewers and listeners as a market. These different objectives impact what gets covered as news and how it is presented to its viewers.

Public Television Content As Canada's public broadcaster, the CBC's objective was always cultural: to express and promote a Canadian consciousness and identity through entertainment and news that appeals to all Canadians. The CBC was also tasked with the costly production of the national television service. By 1958, the CBC produced 57 percent of content aired on its English-language stations and 72 percent on its French-language stations, with the remainder of content adopted from international or non-CBC domestic sources (Fowler and Smythe, 1957). National service content was produced in a few major centers and disseminated to all stations across the country. This centralized approach to content production made financial sense, given the significant costs imposed on the CBC as the producer of the entire national program service (Rutherford, 1990). But this centralized approach also limited the production of regional and local content. While this was not really an issue since the mandate of the CBC was to speak to all Canadians, it nevertheless tipped the scale of content production towards national issues. For example, the CBC's daily newscast, introduced in 1953, was a prime time news bulletin on national and international affairs aired by stations across the country. It was not until the end of the 1950s that public stations introduced regular news bulletins to serve the needs of local communities, such as CBC Toronto's *Metro News* and CBC Montreal's *Edition Métropolitain* (Rutherford, 1990).

Private Television Content The objective of private broadcasters in Canada is caught somewhere between public service and profit maximization. The airwaves are public domain, and the licensing of a private television station is predicated on its service of the public interest. Until 1958, in the interest of public service, private television stations were mandated to air at least 10 and a half hours of the CBC's national program service (Fowler and Smythe, 1957).[11] At the same time, private television stations were also given considerable autonomy in content provi-

[11] The 1958 *Broadcasting Act* eventually regulated minimum Canadian content requirements of approximately 50 percent, but in the early days of television the publicly produced national television service was the primary source of Canadian content across the airwaves.

sion. American television entertainment was popular in Canada, so it comes as no surprise that, motivated by profit, private stations added a significant amount of American content to their programming schedules (Rutherford, 1990). To offset the cost of importing this content, private television stations also delivered a steady diet of local news and local live programming because local informational content was cheap to produce and soon proved to be popular among the communities that private stations serviced (Fowler and Smythe, 1957). In this way, the profit motive of private broadcasters incentivized the production of local informational content and the adoption of American entertainment content beyond any public service requirements.

A Dual System of Public and Private Broadcasters Table 1 gives substance to this narrative with the quantification of local versus national programming content across broadcasters, for both entertainment and informational content. The first column reports the proportion of locally produced content relative to nationally produced content aired on private stations. For example, during prime time audience hours, the value 0.67 implies that private stations air 40 minutes of locally produced informational content for every hour of nationally produced informational content. The second column value (0.30) implies that, for every hour of nationally produced informational content, only 18 minutes of locally produced informational content is shown on public television. The third column reports the ratio of these two values—i.e., the proportion of locally produced content aired on private stations relative to public stations. The value of 2.22 indicates that, during prime time audience hours, private television stations air more than double the amount of locally produced informational content compared to public stations, on average. Private television viewers witnessed an even greater proportion of local content during adult audience hours, and during daytime hours, the relative increase in local content on private stations was exceptional—96.5 percent of informational content was locally produced for private stations, whereas only 5.1 percent of informational content was locally produced for public stations. Both these data and the historical record make clear that a defining difference between private and public television, in terms of content, is the relative localness of information aired on private television.

3 Data

In this section, we describe the main variables used throughout the empirical analysis. Section D in the Appendix provides additional details about data construction and sources.

Structure Throughout the analysis, our spatial unit of observation is a federal electoral district. We collect and digitize four sets of district maps for our extended sample period of 1935-1968, due to the redrawing of electoral districts in 1947, 1952 and 1966. We crosswalk these digitized maps using the procedure outlined in Eckert et al. (2020), giving us a consistent spatial unit of observation over the entire sample period.

We use the 1952 distribution of electoral districts as our reference map, the same year television arrives, and re-aggregate the other reporting maps to the 1952 distribution in the crosswalk. There are 265 electoral districts in the 1952 redistribution, to which we can match signal strength data for all but two districts. Our baseline sample runs from the establishment of the CBC to the end of the one-station policy (1935-1958), a time period that includes 7 general elections, implying a total of 1,841 observations for the 263 districts. Voter turnout data is occasionally missing for some districts in the historical record, so our final sample consists of 1,795 district-election-year observations (97.5 percent of possible observations). Missing district characteristics further reduces our sample to 1,764 observations when covariates are included in a specification (95.8 percent of possible observations). In the 1935-1968 extended sample, we add data for 4 additional general elections, implying a total of 2,893 observations for the 263 districts. We match voter turnout data for 2,832 observations (97.9 percent of possible observations), or 2,764 observations (95.5 percent of possible observations) for specifications that include the full set of covariates.

Television Signal Strength We gather archival records of television transmitter installations from the Library and Archives of Canada. The complete set of records come from three different collections. With these archival records, we piece together all of the needed information for the complete set of transmitter installations between 1952 and 1968—i.e., the station call sign, latitude-longitude coordinates and opening date; whether a station is publicly or privately owned; and transmitter features such as height and service power.[12]

To obtain an accurate estimate of television signal strength at the district level, we follow a two-step procedure of estimation and aggregation. First, we use the Irregular Terrain Model (ITM) to estimate the attenuation of signal strength across space, based on the location, height and power of television transmitters across the landscape of Canada.[13] ITM estimates take into account the elevation profile between sender and receiver to adjust each estimate for any topographic interruption of a signal. See Figure A.1 in the Appendix as an example of ITM output.

Second, we aggregate these signal strength estimates to our unit of observation. We start by aggregating our ITM estimates to the smallest available statistical area in Canada: the census subdivision (CSD). We use the 1951 CSD map, which is comprised of 4,987 non-overlapping units. We match 1951 census population data to these CSDs to use as weights when aggregating from CSDs to electoral districts. This procedure guarantees that even in large electoral districts we obtain accurate estimates of the signal strength received by the electorate, as densely populated CSDs are up-weighted in the aggregation, while sparsely populated CSDs are down-weighted. By design, this aggregation strategy overcomes the problem of aggregating by geographic unit, where such units tend to vary considerably in size and population, especially in a large country like Canada. Our final measure is an estimate of television signal strength at the electoral district

[12] The records also include stations with a 1969 opening date, but with federal elections occurring in 1968 and 1972, the 1969 records are irrelevant for our empirical analysis. For a detailed discussion of the different collections we use and we how piece them together, see section D in the Appendix.

[13] This approach to modeling signal strength across space is common in the literature, and well documented elsewhere (Olken, 2009; Yanagizawa-Drott, 2014; DellaVigna et al., 2014; Bursztyn and Cantoni, 2016; Wang, 2021, among others). We use CloudRF to make these estimates, a cloud-based service for modeling radio propagation.

level, which varies across time in accordance with the building of new television transmitters over our sample period.

The level of signal strength indicates whether the people residing in a district can watch television without noise. At baseline, we apply a minimum threshold for a district's average signal strength of 50 dbμV/m.[14] With this transformation, signal strength increases continuously for values greater than 50 dbμV/m and is set to zero otherwise.[15]

Voter Turnout Our measure of political engagement is voter turnout. We source data from Election Canada's Report of the Chief Electoral Officer for each general election between 1935 (18th general election) and 1968 (28th general election), although for most of the analysis we truncate our panel at 1958 (24th general election)—the last year the one-station policy was in effect. For every general election, Election Canada's Report summarizes results by electoral districts, including the total votes cast, the size of the electorate and population. We calculate voter turnout as the ratio of votes cast relative to the size of the electorate.

Vote Shares and Party Affiliation We construct various outcomes using data that we scraped from the Parliament of Canada's Parlinfo website. For every district in each general election, we collect a complete list of candidates running for office, their party affiliation and the number of votes cast. From these data, we calculate various outcomes: vote shares by political party, voter polarization (across party lines), vote shares across the left-right political spectrum, an indicator for incumbent re-election and an indicator for political party re-election.

Speech Localness To capture how political accountability changes the way politicians speak, we construct three different measures of an elected politician's speech localness, based on the universe of speeches given by MPs in the House of Commons. We obtain the digitized debates from Beelen et al. (2017). To calculate the localness of a speech, we use the Canadian Geographical Names Database, which lists all populated places in Canada and includes their latitude and longitude coordinates (CGNDB, 2021). We build an algorithm that identifies any populated place mentioned in a speech, and then calculate the distance between that place and the district of the politician who mentions the populated place. With this approach, we can determine if the populated place mentioned is within the boundaries of the district they represent, or not.

Our first measure, *speech locality*, is the fraction of total speeches given by a politician in an electoral cycle where they mention a populated place within their own district. Our second measure, *place-based speech locality*, is very similar but rather than using the count of all speeches given during an electoral cycle as the denominator, we instead use the subset of speeches where *any*

[14] This threshold is based on the Government of Canada's minimum requirement of 47 dbμV/m for a Grade B service contour, which by definition is a signal level the Government of Canada deems "to be adequate, in the absence of man-made noise or interference from other stations, to provide a picture which the median observer would classify as of satisfactory quality." (ISED, 2016, p. 12)

[15] In Section 5, we show that all conclusions are robust to a binary treatment design or different cut-off thresholds.

populated place is mentioned, thus conditioning this measure on place-based speeches only. Both of these measures are extensive margin measures of speech localness. Our third measure, *place locality*, is an intensive margin measure, where the numerator is the number of populated places mentioned within the politician's district, relative to the total number of populated places mentioned in that speech, and averaged across the electoral cycle. In all instances, these measures uniquely define an electoral district for a given electoral cycle, and thus exhibit the same variation as the rest of our data.

Parliamentary Dissent To capture how political accountability changes the way politicians act, we count how often an MP votes across party lines. We obtain roll-call voting records for every vote held in Parliament for our sample period, sourced from Godbout and Høyland (2017). We define a vote to be against the party line if an MP votes contrary to the majority of their party in a given round of voting. We then aggregate each dissent for each politician across electoral cycles, giving us a proxy measure of political accountability that varies by electoral district and election year. Because this outcome variable includes many zeros, we use the inverse hyperbolic sine transformation.

District Characteristics We collect various district-level characteristics as control variables for the empirical analysis. For each characteristic, we locate data observed as close to the 1935 start date of our sample and interact these "initial conditions" with year fixed effects. We obtain 1931 population data at the electoral district level from the Election Canada's 1935 Report of the Chief Electoral Officer, which comes from the 1931 decennial census. We calculate the area of each electoral district in ArcGIS, and construct a measure of population density as the ratio of population to area in squared kilometers. We obtain information on average earnings, age, literacy rates and urbanization rates from the 1911 census, which is available at the CSD level.[16]

Daily Newspaper Circulation Rates Per Capita We piece together daily newspaper circulation rates from various editions of the *Canada Year Book*.[17] Daily circulation rates are available as a time series of national averages and as a panel for 42 cities, although some cities are missing for a few of the reported years.[18] To convert the national circulation rates to per capita rates, we use annual population data from *Statistics Canada*. We collect city-level population data from the *Year Book*, which is available for the years 1941, 1951 and 1961. We interpolate these data to have a balanced city-level panel of population data for all years between 1947-1959, which we use to convert the city-level circulation rates of daily newspapers into per capita terms.

[16]The 1911 census is the last available digitized version of the decennial census. We use the CSD location of enumerated individuals in the 1911 census to aggregate up to the electoral district level.

[17]We do this using every edition of the *Year Book* from 1950 to 1960. The *Canada Year Book* was published every year from Confederation until 2013. The *Book* was presented in an almanac style, and includes detailed data on every major area of *Statistics Canada's* expertise.

[18]For 1947 and 1948, data is reported for cities with 20,000 inhabitants or more, whereas the threshold is increased to at least 30,000 inhabitants from 1949 and on; hence, some cities are not observable after 1948.

4 Empirical Design

We are interested in the causal effect of informational content in mass media on political engagement and accountability. An empirical challenge arises from the non-random expansion of the television network: factors that determine the location and timing of television transmitter installations (e.g., population density) may correlate with our political outcomes of interest. Moreover, even if the location of a transmitter is conditionally random, the timing of exposure to treatment may still be non-random.

In this section, we describe this bias and provide three solutions: (i) a baseline difference-in-differences specification that absorbs confounding variation with district and election-year fixed effects; (ii) adding a free-space signal control variable to the baseline specification that recenters our treatment around plausibly exogenous variation in topography (Olken, 2009, among others); and (iii) adding an expected signal strength control variable to the baseline specification that recenters our treatment around simulated counterfactual networks that might as well have been established (Borusyak and Hull, 2023).

We show that only the last solution—the inclusion of expected signal strength in our baseline specification—balances key determinants of treatment, since it is the only solution that accounts for the non-random timing of treatment exposure. In the absence of balance, a difference-in-differences specification requires parallel trends and homogeneous treatment effects to identify the causal effect of informational content on political engagement and accountability.

4.1 Difference-in-Differences Framework

We aim to identify the impact of content on political engagement using a difference-in-differences (DD) specification, where electoral districts receive a continuous treatment across different election years. This baseline model takes the following form:

$$Y_{d,t} = \alpha_d + \alpha_t + \beta\, signal_{d,t} + \Phi\left(\boldsymbol{X_d} \times t\right) + \epsilon_{d,t}. \tag{1}$$

Outcome $Y_{d,t}$ is voter turnout in electoral district d for election-year t, and our treatment variable, $signal_{d,t}$, is a measure of television signal strength in district d for election-year t. The vector $\boldsymbol{X_d}$ includes initial conditions of each district d that we interact with a year fixed effect, thereby allowing the impact of these conditions to vary over time.[19] We also include district fixed effects (α_d), which capture time-invariant characteristics of an electoral district, and election-year fixed effects (α_t), which capture any variation common to all districts for each election cycle. To account for serial dependence, we cluster our standard errors at the district level.

Our parameter of interest, β, captures the causal effect of television's arrival on voter turnout. The fixed effects included in equation (1) imply that we exploit variation in signal strength within each district over time, relative to other districts in the same election year. But these fixed effects

[19]These district-level characteristics include population density, earnings, age, literacy rates and urbanization rates.

alone will only identify β if the parallel trends assumption holds and treatment effects are homogeneous over time and across districts (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). Appendix C provides a series of tests that support our assumption of parallel trends and homogeneous treatment effects, which we also discuss further in Section 5.1.

Balance We begin by showing that treatment assignment is correlated with pre-treatment population density. If we can establish parallel trends for equation (1), we can capture this selection with the inclusion of district fixed effects. However, testing for balance at the timing of treatment provides a means to test whether the free-space and expected signal strength control variables sufficiently recenter and balance our treatment variable. Figure 1 plots coefficient estimates of our treatment variable from a series of cross-sectional regressions with two main determinants of treatment assignment as outcomes: population density and earnings.[20] The intuition of this test is that population density and earnings are key determinants of the timing and location of a transmitter's installation.[21] The OLS estimate of our treatment effect shown in the first panel is clearly correlated with population density, although in the second panel it appears that earnings are uncorrelated with treatment. Figure 1 implies that television signal strength is stronger in more densely populated districts at the time of the initial treatment, as we should expected based on the historical record—i.e., television is first introduced in large potential markets.

4.2 Free-Space Signal Strength

The non-random location of transmitter installations is rightfully acknowledged in similar studies to ours, where the typical solution is to control for a measure of free-space signal strength in a regression. A free-space signal is calculated by assuming no mountainous terrain in the region surrounding a television transmitter, meaning signal strength attenuates across space without any topographical interruptions. As a result, actual and free-space signal strength only differ due to variations in topography, since both signal strength estimates rely on the same transmitter features. A regression estimate of signal strength conditional on a free-space control variable thus yields an estimate of signal strength that relies only on variation in topography, since the endogenous component of any signal is held constant by the free-space control.

This approach is popular because it is simple, yet this simplicity comes at the cost of imprecision. In free space, signals decay at the inverse rate of distance squared, which is unlikely to be the appropriate distance elasticity in our context, since television towers are often located on top of a mountain or hill—i.e., regions with a different topography than free space. Nevertheless, we

[20] More precisely, we estimate the following cross-sectional regression: $Y_d = \beta\, signal_{d,1953} + \epsilon_d$, where Y_d is a pre-condition of treatment in district d, capturing either population density in 1931 or earnings in 1911.

[21] The sparse population of Canada, relative to its sheer size, creates a unique problem when deciding where to locate a transmitter, since the available audience is small outside of a few major centers (Fowler and Smythe, 1957). For this reason, population density was a key factor in the timing and location of a transmitter's installation. This is not a uniquely Canadian phenomenon either, as population and earnings are also two primary determinants of transmitter installations in the US (Gentzkow, 2006).

use this free-space approach to benchmark the performance of our new approach, including its relative performance in balancing population density and earnings at the time of initial treatment.

Balance We report these conditional estimates of actual signal strength in Figure 1. The coefficients labeled free-space signal strength come from a cross-sectional regression of population density or earnings on actual signal strength, conditional on the free-space control variable.[22] Similar to the unconditional estimate, the free-space approach does not balance population density across treatment and control districts. This is unsurprising in a panel context like ours, as free-space signal strength does not take the non-random timing of a transmitter's installation into consideration.

4.3 Expected Signal Strength

Our treatment variable—television signal strength—varies across electoral districts based on the location of a television transmitter and across election years based on the timing of a transmitter's installation. As shown in Figure 1, an unconditional estimate of the treatment effect will be biased by the fact that these sources of variation are non-random, and likely correlate with our political outcomes of interest. Yet even if the location of a transmitter's installation is conditionally random in the cross-section and satisfies the typical DD assumptions of our panel, the *timing* of exposure to television may still be non-random (Borusyak and Hull, 2023). For example, Montreal and Toronto are the first two cities to receive television because they are the largest cities in terms of population, and both are economic centers of the country. Nearby districts then receive television earlier than expected because of their proximity to Toronto and Montreal. This suggests that *any* district near an economic or population center of the country is more likely to be treated earlier than a district on the periphery. In this instance, a regression estimate of equation (1) will not to identify β unless we make the strong assumption that "central" districts do not differ from "peripheral" districts in any relevant, time-varying unobservable, such as political discontent, civic mindedness or non-electoral political activities.

To overcome this endogeneity problem and establish causality, we recenter our estimate of actual signal strength with a novel measure of expected signal strength (ESS), that in effect purges the non-random timing of television exposure. We construct ESS based on the insights of Borusyak and Hull (2023), who propose a general solution to a scenario like ours, where the treatment combines a non-random source of variation with an exogenous shock. We provide a detailed discussion of this approach in Appendix B, although the intuition is straightforward: the observed distribution of television signal strength is an outcome of the underlying data-generating process, which can be modeled and used to draw a counterfactual distribution in such a way that it might as well have occurred—i.e., each permutation is one realization of the underlying data-generating

[22] This specification is analogous to the cross-sectional estimate described in Section 4.1, only here we also control for the free-space signal strength of district d in 1953.

process. By permuting the television network many times, we can construct an average of the counterfactual realizations for each electoral district—i.e., a district's ESS.

By design, ESS is a summary measure that approximates a district's non-random exposure to television. For example, we model a data-generating process where transmitters nearby Toronto and Montreal receive a high probability of activation in each permutation of the network. The key insight of Borusyak and Hull (2023) is that we can construct an expected treatment based on these permutations, where in this example the expected treatment is a measure of any district's non-random exposure to the realized treatment of Toronto and Montreal. In a regression setting, *expected* signal strength can be included as a control variable to recenter an estimate of *actual* signal strength, effectively purging the non-random—and thus biased—component of our treatment effect. More intuitively, this approach works because the variation that we rely on in a regression is the difference between the observed and counterfactual network—signal strength above or below what is expected—and thus an outcome of chance.

Modeling Expected Signal Strength The set of transmitters that receive activation in a simulation are modeled as a function of (i) cross-sectional variation—i.e., each transmitter's sampling probability and a random shock to that probability—and (ii) temporal variation—i.e., how many transmitters are actually active in a given year.

We generate the cross-sectional probability of sampling transmitter s as $(\bar{t} - t_s)/(\bar{t} - \underline{t})$, where $\underline{t}$ and $\bar{t}$ respectively denote the first and last year of all observed transmitter installations, while t_s denotes the commencement year that the station transmitter s becomes operational.[23] This probability linearly decays as a function of a station's commencement year, where early transmitters receive a high probability of activation and late transmitters a low probability. For example, Toronto is one of two stations with a 1952 commencement date, and this early activation of the Toronto transmitter is as expected, since the city offers the most densely populated Canadian market. Our approach captures this expectation by assigning this transmitter a cross-sectional activation probability of 1 in any permutation of the network. Whereas the city of Timmins, approximately 700 kilometers north of Toronto, did not receive television until 1957 due to its remoteness and smaller size. We assign a cross-sectional probability of 0.69 to Timmins in any permutation of the network, which aligns with our expectation that television should be available in Toronto with a greater probability than Timmins in any given year.

To introduce temporal variation, we activate the correct number of transmitters for a given year, choosing transmitters with the highest cross-sectional probability in each permutation. Let $(1/a_t)$ be the probability any station is sampled in year t, where a_t be the correct number of activated transmitters for that year. This formulation implies that the probability of a station being activated is decreasing in years where many transmitters are activated. The combined cross-sectional and temporal probability implies that station s is sampled in each permutation of year t

[23] In our data, $\underline{t} = 1952$ and $\bar{t} = 1968$. We rely on the complete set of transmitter installations throughout this time period as our set of potential activation locations.

as follows:

$$\text{Pr} = \frac{\bar{t} - t_s}{\bar{t} - \underline{t}} + \frac{1}{a_t} + \epsilon_s. \tag{2}$$

In our simulation we permute the television network 500 times, drawing a distribution of ESS based on equation (2) each time. The inclusion of a normally distributed shock ϵ guarantees a non-deterministic distribution of active transmitters in each permutation of the network.[24] We derive our final measure of ESS by averaging across all permutations at the electoral district level.[25] With this measure of expected signal strength at our disposal, which effectively captures each district's non-randomness in television exposure, we can recenter our regression estimates to correct for the aforementioned omitted variable bias. In a regression setting, we can control for expected signal strength and estimate the effect of actual signal strength, which in effect means we our estimating the effect of television from variation in signal strength above or below what was expected.[26]

Balance For our final test of balance, we re-estimate the cross-sectional regression of a district's initial population density or earnings on actual signal strength, controlling for expected signal strength.[27] We report these conditional estimates in Figure 1. After making the necessary adjustments for cross-sectional and temporal sources of non-random variation, it is clear that population density and earnings are both balanced at the time of initial treatment.

4.4 Identification Strategy

Figure 1 demonstrates that an unbiased estimate of β can be realized by recentering our estimate of actual signal strength around expected signal strength. We thus draw inference from a two-way fixed effects DD estimator that controls for expected signal strength as follows:

$$Y_{d,t} = \alpha_d + \alpha_t + \beta\, signal_{d,t} + \gamma\, \mu_{d,t} + \Phi\,(\boldsymbol{X_d} \times t) + \epsilon_{d,t}. \tag{3}$$

Equation 3 is our preferred specification for the analysis, although we benchmark our estimates of this specification against equation (1) with and without a free-space control variable. All model variables are as described for equation (1), with the only addition being $\mu_{d,t}$, our measure

[24]For this measure of ESS, ϵ is drawn from a normal distribution with a mean of zero and a standard deviation equal to the standard deviation of the sampling probability in equation (2).

[25]More precisely, for each permutation, we aggregate the distribution of simulated signal strength to CSDs, exactly as described in Section 3 for actual signal strength, and use CSD-level population data as weights to aggregate from CSDs to electoral districts.

[26]We recognize that modeling the underlying data-generating process offers many degrees of freedom, so we explore 23 alternative ways to simulate the distribution of expected signal strength and report our findings in Appendix B.1.1. We test two necessary assumptions of our expected treatment approach, as outlined in Borusyak and Hull (2023), and report our findings in Appendix B.1.2. Both equation (2) and various alternatives to it satisfy these necessary assumptions, and controlling for any one of these alternative measures of ESS yield a remarkably stable and similar estimate of the treatment effect, as shown in Figure B.3.

[27]This specification is analogous to the cross-sectional estimate described in Section 4.1, only now we control for the expected signal strength of district d in 1953.

of expected signal strength in district d for election-year t. We continue to cluster standard errors at the district level to account for serial dependence.

The inclusion of $\mu_{d,t}$ as a control variable in equation (3) recenters our estimate of β, such that we estimate our parameter of interest from variation above or below the expected signal strength. By design, this variation is an outcome of chance—a testable and necessary assumption of this strategy that we bring to the data. In Appendix B, we show that $\mu_{d,t}$ satisfies the necessary assumptions outlined in Borusyak and Hull (2023), and in Appendix C we provide evidence in support of the parallel trends and homogeneous treatment effects assumptions. Thus, our parameter of interest, β, captures the causal effect of media content on voter turnout.

5 Political Engagement

In this section, we examine the link between media content and political engagement. We start by estimating the overall effect of television on voter turnout as a baseline. We then estimate the direct impact of information, based on the differential impact of public and private television content on voter turnout.

5.1 Television's Impact on Political Engagement

We begin our analysis with an investigation of the overall impact of television on voter turnout, our measure of political engagement. Our panel varies over time by election cycles, which for our sample period of 1935-1958 implies 4 pre-treatment election cycles and 3 post-treatment election cycles for the earliest adopters of television.

Results Figure 2 plots various estimates of β for each of the estimation strategies described in Section 4.[28] The DD estimates do not control for free-space signal strength nor expected signal strength, whereas the other sets of estimates adopt one of these controls as labeled. In all instances our negative estimate of β is significant at conventional levels, and the inclusion of covariates only slightly reduces the magnitude of the estimates. In line with previous research, we find a clear and unambiguous negative effect of television on voter turnout, and the robustness of this result to expected signal strength, or free-space signal strength, supports our assertion of a causal interpretation.

To get a sense of magnitude, we discretize our continuous treatment variable to equal one for all districts with actual signal strength greater than 50 dbμV/m. We report these estimates in Table A.2 in the Appendix. After controlling for expected signal strength, our preferred estimates imply a 2 to 3 percentage point drop in voter turnout following the arrival of television. The magnitude of this change in voter turnout is noteworthy: 8 to 12 percent of district elections are determined within this 2 to 3 percentage point range throughout our sample period.

[28]Table A.1 in the Appendix reports these estimates in table format.

We also present this baseline result as an event study to assess parallel trends. Figure 3 plots the event study for all three versions of our model as labeled, and documents an absence of any concerning pre-trends, with or without our full set of covariates. In all instances, the plots show that districts that receive television, compared to districts that do not, exhibit similar dynamics in the pre-treatment period, with a significant decline in voter turnout in the post-treatment period.

Robustness and Sensitivity Checks The identification of β relies on the assumption of parallel trends and treatment effect homogeneity. Although the timing of our treatment varies across districts, the adoption of television was rapid, so there is little reason to believe that late adopters show a significantly different treatment effect than early adopters, or that treatment effects grow over time. Nevertheless, we explore these assumptions formally in Appendix C. The parallel trends assumption is robust to including never-treated districts in an event study, and is similarly robust in an event study mimicking a classical DD—comparing only the initial treatment group to the never treated. We decompose our estimate into cohort treatment effects, and document that the majority of our estimated effect comes from a comparison of the treated to the never-treated, rather than early-versus-later adopter comparisons that could imply negative treatment weights (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). We also construct an interaction-weighted estimator that shows no evidence of treatment effect heterogeneity across time (Sun and Abraham, 2021).

We also test the robustness of our results to an alternative fixed effects strategy. Table A.3 replicates the findings in Figure 2, but we use province by election-year fixed effects for these estimates in place of election-year fixed effects. At baseline, we use election-year fixed effects because some provinces contain few electoral districts, meaning some within-province comparisons rely on few data points. All estimates maintain significance and are larger in magnitude than at baseline, suggesting that our choice of fixed effects in equation (3) yield conservative estimates.

Another concern relates to the Government of Canada's minimum signal strength requirements of a television station, as noted in Section 3 (ISED, 2016). For our baseline measure, we set a minimum threshold for television signal strength of 50 dbμV/m based on these requirements. While in practice a lower signal strength may result in service interruptions, our measure of signal strength is a district average, so even when the average may fall below 50, it is still possible that the populated regions of the district may receive a strong enough signal to watch television. For this reason, we show that our estimates are insensitive to the choice of threshold. Figure A.4 in the Appendix plots estimates of β from equation (3), where the minimum threshold is increased 1 dbμV/m at a time over a range of 25 to 75 dbμV/m. The results are insensitive to the choice of threshold. Combined with the results of Table A.2, where we discretize treatment, it is clear that our preferred transformation of our treatment variable has no impact on our findings.

The one-station policy was only in effect until 1958, after which private and public stations could compete in the same market. Repeal of the one-station policy makes it difficult to ascertain whether viewers watched public or private television, and thus reliably separate the impact of public television content from private content. However, we have not made that distinction yet in the analysis, so to obtain more precise estimates on the introduction of television, we test the robustness of these baseline results to an extended sample running from 1935-1968—increasing the number of post-treatment general elections from 3 at baseline to 7 in the extended sample. Table A.7 reports these extended sample estimates, where the results are similar to our baseline estimates, reflecting the robust negative impact of television on voter turnout.

We also show that our results are not sensitive to any influential electoral districts. To do this, we drop each observed district from our sample, one at a time, and re-estimate equation (3). In Figure A.5, we plot a histogram of the complete set of these estimates—each relative to the full sample estimate—to highlight the stability of our findings. The majority of the mass is centered around 1, where a value of 1 implies the same estimate in the full sample and subsample. Some estimates deviate from 1, but never by much more than a few percentage points. We conclude that no influential districts are driving our results.

5.2 The Impact of Information on Political Engagement

In Section 5.1, we document a negative impact of television on voter turnout. Yet we do not believe that this is necessarily an effect of television per se, but rather an outcome of the informational content aired on television. Public and private television content is distinct in terms of the national or local scope of information, as we make clear in Section 2.3. With this in mind, we continue our analysis with an investigation of the differential impact of public and private television content on voter turnout, based on the relative localness of information aired on private television. We do this by augmenting equation (3) to include an interaction term for electoral districts that receive private television, which allows us to test for significant differences in voter turnout across districts that receive public or private television.

$$\begin{aligned} Y_{d,t} = \alpha_d + \alpha_t + \beta^{pub} signal_{d,t} + \beta^{pvt} \left(signal_{d,t} \times I^{pvt}_{d,t} \right) \\ + \gamma^{pub} \mu_{d,t} + \gamma^{pvt} \left(\mu_{d,t} \times I^{pvt}_{d,t} \right) + \delta I^{pvt}_{d,t} + \Phi(\boldsymbol{X_d} \times t) + \epsilon_{d,t}. \end{aligned} \tag{4}$$

The only new variable here is $I^{pvt}_{d,t}$, an indicator variable that equals one if district d has private television in election year t.[29] By interacting our baseline measure $signal_{d,t}$ with $I^{pvt}_{d,t}$, we can separate the impact of public and private television on voter turnout, and test whether any difference in voter turnout across these broadcaster types is statistically significant or not. The coefficient β^{pub} captures the direct effect of public television on voter turnout relative to districts that do not

[29] We allow this variable to vary over time because of a peculiarity of our data. The one-station policy requires that no two stations service the same television market, but television markets do not necessarily correspond to electoral districts. An artifact of this mismatch is that some larger districts may receive both public and private television in non-overlapping space, but our measures of *average* public and private signal strength are both non-zero. At baseline, we assign these districts as public or private based on the strongest signal, which might change over the course of our sample period, but in a robustness check below, we show that results are unaffected by dropping these contaminated districts, where the clean separation of public and private television is not possible.

receive television over our sample period. By contrast, β^{pvt} represents the differential effect of private television on voter turnout relative to public television districts, and thus is a comparison of voters who receive the same new media type—television—but have no choice over available content—local or national information. We account for serial dependence by clustering standard errors at the district level.

Results Table 2 reports our estimates of equation (4). Column 1 reports the baseline DD estimates, while columns 2 and 3 report the free-space and expected signal strength estimates, respectively. Across all specifications, the estimate of β^{pub} is significant and negative, suggesting that the arrival of public television is a source of the overall drop in voter turnout that we document in Section 5.1. Whereas the estimate of β^{pvt} is similar in magnitude to β^{pub} but the opposite sign, suggesting that public television decreases political engagement relative to private television. Although our estimate of the interaction term in the free-space model is noisy and loses significance at conventional levels, the coefficient is similar in magnitude across all specifications and supportive of this interpretation. We also document evidence of the parallel trends assumption for public and private television in Appendix Figure A.3.

To assess the direct impact of private television on political engagement, we report the p-value for the null hypothesis $\beta^{pvt} + \beta^{pub} = 0$. Across all specifications, we fail to reject the null hypothesis, indicating that private television content does not impact voter turnout. Nevertheless, to be certain that the fall in voter turnout is exclusive to public television districts, we also present alternative evidence in the form of a horse race regression between public and private signal strength in Appendix Table A.4. We find that the negative impact of television on voter turnout is indeed driven exclusively by the national scope of public television content.

Robustness and Sensitivity Checks Table A.5 reports the regression estimates of equation (4), but in place of election-year fixed effects we use province by election-year fixed effects. The results again verify the insensitivity of our findings to this alternative fixed effects strategy.

Although the one-station policy guarantees that no two stations service the same market, electoral districts do not necessarily align with television markets. There are 15 districts in our sample where our measures of *average* public and private signal strength are both non-zero, even if these districts receive public and private television in non-overlapping space. These districts potentially contaminate the clean separation of public and private television that the one-station policy otherwise guarantees. We drop these contaminated districts from our sample to be certain our results do not hinge on their presence, and report our findings in Table A.6. The results are comparable to Table 2, albeit slightly larger in magnitude, suggesting that the presence of these 15 districts in our baseline sample, if anything, introduce measurement error and bias our estimates toward zero.

We replicate the estimates reported in Table 2 on a sample running from 1935-1968, and report the extended sample estimates in Table A.8 in the Appendix. These results should be interpreted with some caution, since the one-station policy was only in place until 1958, so the clean separation of public and private television content is no longer possible with this extended sample. Nevertheless, the extended sample results confirm the findings documented in Table 2, albeit with a little more noise, but that is to be expected since public and private stations can compete in the same market for the last 10 years of the sample. In Figure C.5, we also show that our results are remarkably similar to Table A.8 when we extend our observation of voter turnout to 1968, while holding treatment constant according to a district's treatment status in 1958—the last year of the one-station policy. Appendix C.2 describes this exercise in more detail.

6 Mechanism

In Section 5.1, we establish that the arrival of television marked a precipitous drop in political engagement, while the evidence in Section 5.2 indicates that this drop in engagement is driven exclusively by the national scope of public television content. We point to the relative focus of private broadcasters on local informational content as a consistent explanation of these findings. Yet this mechanism is not mutually exclusive to alternative explanations, and the extent to which local information shapes political outcomes is not directly observable. We thus consider other viable mechanisms that might overshadow our interpretation, but find no evidence in support of entertainment content crowding out information, or the political bias of media as alternative explanations. Although we document evidence of a substitution away from newspapers after the arrival of television, this cannot explain our findings because both public and private television districts exhibit similar substitution patterns. We conclude that local information plays a decisive role in engaging the electorate, particularly in a parliamentary democracy like Canada, where citizens vote for their local representative in Parliament.[30]

Local Informational Content In Section 2.3, we discuss how the localness of television content differs across public and private broadcasters due to the different objectives they face. We quantify these differences in Table 1 with television programming records, documenting that private television stations aired more than twice the amount of local informational content than public stations during prime time and adult audience hours. During day time hours this difference becomes exceptional: 96.5 percent of informational content aired on private stations is locally produced, with a mere 5.1 percent of local content aired on public stations. Overall, the evidence in Table 1 points to local information as a defining difference between public and private television content.

The local content of private broadcasters was held in high regard by local citizens, suggesting a meaningful impact of private television content on its viewers. The Royal Commission on Broadcasting's 1957 report includes a subsection dedicated to the local and community service activities of the private broadcasters, describing local content of private television as "valued and appreciated by local citizens and organizations" in many letters of support and public hearings.

[30] We also provide further indirect support of the local information mechanism in Section 7, with evidence of private television content evoking accountability among elected politicians—an outcome understood to be a consequence of political engagement (Strömberg, 2015).

The report authors contend that a fully public television system would be unlikely to service local communities to the same extent, and go on to write: "*Knowledge of local conditions* and *adaptability to local needs* can best be provided by having a number of independent [private] local units in the system. This is one of the principal reasons why we are strongly of the opinion that the continued presence of private elements in the system should be recognized and placed beyond uncertainty and doubt." (Fowler and Smythe, 1957, p. 147, emphasis added) In other words, the dual system of public and private broadcasting was maintained because private stations played a vital role in local content provision.

6.1 Alternative Explanations

Substitution Effect An alternative explanation for our findings is that the substitution from newspapers and radio to television may have been stronger in public television districts, since newspapers and radio tend to elicit more political engagement than television (Strömberg, 2004b; Gentzkow, 2006; Gentzkow et al., 2011). The rapid expansion and uptake of television speaks to its success at the expense of other legacy media. For example, hearings opened at the beginning of 1953 for private station licensing, after which the first 7 licenses were granted—all to private owners of local newspapers and radio stations (Peers, 1979).[31] The immediate response of these private owners to pivot into television, ultimately to maintain their local information monopolies, speaks to their concerns about the rapid success of television at the expense newspapers and radio.

As another example, the city of Halifax was due to receive public television in December 1954, so in the months leading up to television's arrival, the CBC surveyed Halifax residents on radio listening habits, preferences and more. A year later, after CBHT-TV Halifax was in operation, a follow-up survey was conducted to assess television viewing habits, and how radio listening habits might have changed. The study found that radio listening time dropped by about one-third, and during television prime time hours, the drop was upwards to two-thirds (Davies, 1998). *Our Miss Brooks*, an American radio program popular in Canada at the time, provides a stark example of this substitution effect: after CBHT-TV signed on, the listening base was only 15 percent of what it was in the days before television.

These anecdotes are suggestive of a substitution effect in both private and public districts. To get a better sense of the quantitative impact, we collect per capita circulation rates of daily newspapers, between 1947 and 1959. Figure 4 plots annual circulation rates of daily newspapers (per capita) for the entire country, showing a consistent drop in circulation rates after the arrival of television. Figure 5 plots annual circulation rates of daily newspapers per capita for 42 cities, but separately for cities that receive public and private television. Panel (a) plots a kernel-weighted linear polynomial smoothing of cities with public television, and panel (b) plots the same for pri-

[31] For example, the first private television station, CKSO-TV Sudbury, was initially owned by James Cooper, George Miller and Bill Plaunt, the owners of the Sudbury Star newspaper and the CKSO Sunday radio station (Peers, 1979). All of the other private television station owners, in the earliest years at least, held local information monopolies in the same city they purchased a television broadcast license.

vate television cities. Because cities receive television at different times, we also construct panels (c) and (d), which plot the percent of cities in each sample year with public or private television, respectively. With this variation in the timing of television's arrival, we can observe the substitution effect: daily newspaper circulation rates decrease as more cities receive television. It is clear from both figures that daily newspapers circulation rates fall after the arrival of television, and continue to do so thereafter.

The evidence presented in Figure 5 precludes a substitution effect as an alternative explanation, since we observe a similar pattern in cities with public or private television. As further evidence of this, we look at the change in daily newspaper circulate rates per capita for the same 42 cities, and test for mean differences before/after television's arrival, as well as mean differences between cities with public and private television. These difference-in-means tests are reported in Table A.9 in the Appendix. Because we consider the change in circulation rates at the city level, we are holding constant time-invariant city characteristics and testing for within-city changes over time. The first panel confirms that, on average, newspaper circulation rates were increasing in cities before television's arrival, and decreasing after television's arrival, with a mean difference that is significant at the 1-percent level. Whereas the second panel documents average changes in circulation rates between cities with public and private television across all years. The difference in means here is not significantly different from zero, which confirms the visual evidence of Figure 5—that changes in newspaper circulation rates are statistically no different in cities with public or private television. This evidence rules out the alternative explanation that different rates of substitution explain our main findings.

Entertainment Content Another competing explanation is that television is more entertainment focused than newspapers, so it is possible that the negative impact of television on political engagement is due to a crowding out effect (Gentzkow, 2006; Ellingsen and Hernæs, 2018; Durante et al., 2019; Campante et al., 2022). However, given the evidence that voter turnout falls in only public television districts, we should expect to see a disproportionate amount of entertainment content on public television, relative to private television, if our findings are due to a crowding out effect.

Table 3 documents the proportion of entertainment and informational content across broadcaster type, based on total airtime in 1956, as reported by the Royal Commission on Broadcasting (Fowler and Smythe, 1957). Overall, the evidence does not support the interpretation of a crowding out effect. Not only is the difference in entertainment content between public and private stations minimal, but private television stations actually air slightly more entertainment content than public stations—the opposite of what we need to reconcile our findings in Section 5.

Political Bias of Media Content An extensive literature underscores how biased media content can shape electoral outcomes. For example, the political bias of media impacts how people vote (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Chiang and Knight, 2011; Martin and

Yurukoglu, 2017; Durante et al., 2019; Ash et al., 2022), and polarizes the electorate through the reinforcement of prior beliefs (Bernhardt et al., 2008; Chan and Suen, 2008).

In this section, we consider outcomes related to political parties and how people vote, and we test for differential effects across public and private broadcasters to assess the potential impact of political bias. For example, the CBC has been criticized for taking a center-left position for as long as the public broadcaster has existed, yet this claim in untested and hotly debated (Rutherford, 1990).[32] If this were true, one explanation for our findings could be that the public broadcaster hollowed out support for the Conservative party. If the affected voters chose apathy over swing voting, then political bias could explain why public television content causes voter turnout to fall. With this in mind, we test for (i) differences in vote shares across party lines, (ii) differences in vote shares across the political spectrum, (iii) an incumbency advantage for politicians and parties, and (iv) vote share polarization. Throughout we use equation (4) to estimate the differential impact of public and private television on these outcomes.

Table 4 reports estimates for (i) vote shares across party lines. In columns 1 and 2, we consider vote shares for the Liberal party and Conservative party—Canada's two major political parties—and find no impact of public or private television on vote shares across these two major parties.[33] In column 3, we take the difference in Liberal and Conservative vote shares as an outcome and similarly document no significant effect. This evidence suggests that any perceived bias of the public broadcaster—whether true or not—has not shaped the way citizens vote. Nor are these results consistent with a media bias interpretation of our main findings. In columns 4 through 6, we construct a measure of vote shares for all other parties than the two major parties—the Other parties vote share. We find that the relative difference between these parties vote shares and that of the Liberals or Conservatives exhibit no significant difference after the arrival of television.

Table 5 reports estimates for outcomes (ii)-(iv), as noted above. Columns 1 through 3 show that both public and private television had no significant or differential impact in how people vote across the political spectrum.[34] The estimates in column 4 test for an incumbency advantage, but we find no evidence that television reinforces an incumbent's chance of re-election. Related to this, we find no evidence that the political party of the incumbent receives an advantage in the next election, even if the incumbent does not run again, as the estimates in column 5 make clear. Finally, we consider the polarization of vote shares across party lines, since politically biased news is known to increase polarization. To the contrary, we find that the arrival of public television reduced polarization, working against the political bias hypothesis. Because our estimate of β^{pvt} corresponds to an interaction term, its insignificance implies that the effect of private television on polarization is no different than public television. In other words, both public and private

[32] This view is still held today. On 27 June 2017, Peter Mansbridge, the chief correspondent for CBC News from 1988-2017, addresses this issue on air during an episode of *The National*, CBC's flagship nightly news program.

[33] We direct our focus to the Liberal and Conservative party because they are the only two parties to hold the Prime Minister's office in Canadian history. Other notable parties such as the Co-operative Commonwealth Federation (who later become the New Democratic Party) and the Social Credit exhibit similar patterns as the two major parties, so we abstract from the details of these parties vote shares for brevity's sake.

[34] Table A.10 in the Appendix documents our classification of left- and right-leaning parties.

television reduced the polarization of voters, with no discernible difference between the broadcaster types. Altogether, the null results and lack of differential effects do not support a media bias interpretation of our main findings in Section 5.

7 Political Accountability

In this section, we explore the impact of informational content on political accountability. We document consistent evidence that private television content evokes accountability of MPs in terms of what these politicians say and how they vote in Parliament. These results also support our interpretation of the mechanism: local informational content evokes accountability among elected politicians—an outcome well documented to be a consequence of political engagement (Strömberg, 2015).

Estimates of equation (4) are reported in Table 6, where we use different measures of political accountability as outcomes, as defined in Section 3. The first two columns of the table report findings for our extensive measures of speech localness. The outcome for the third column is an intensive measure of speech localness. All three measures allow us to test for accountability in terms of the selection of MPs who speak on behalf of the local communities they represent. Positive accountability effects should also create incentive for MPs to act on behalf of their constituents, which we test in column 4, where parliamentary dissent is a measure of an MPs willingness to vote against the party line.

For all outcomes, our estimate of β^{pub} is insignificant. This implies that we observe no change in accountability among MPs in public television districts, relative to districts without television. However, we do find that our estimate of β^{pvt} is positive and significant across all outcomes. The significance of this interaction term implies a differential effect of private television content, relative to public television content. The positive estimate tells us that MPs who represent private television districts speak more frequently and intensively about their constituents in Parliament, and are more willing to vote on their behalf, relative to MPs who represent public television districts. Altogether, this evidence supports our interpretation of the findings in Section 5 as a consequence of local informational content differences across public and private broadcasters.

8 Concluding Remarks

Understanding the role of media as an information technology is of the utmost importance in a democracy. Media content takes many forms, and not all content effectively links people to the political process. Highlighting the informational aspects of media content that facilitate—rather than hinder—political engagement is especially difficult in today's media landscape, where political content is viewed on television, read in newspapers, and is readily available across the Internet. Past research emphasizes some of the defining aspects of relevant media content for a well-functioning democracy, but the abundance of content choice both across and within these media types puts a limit on what we can learn in most context. The arrival of Canadian television provides a unique empirical setting to overcome this limitation. The endogeneity problem of content choice is solved by the one-station policy, which inhibited the expansion of public and private television into the same market, thus allowing us to parse out the effect of local content differences across these broadcasters.

We find that television reduces voter turnout, but the effect is exclusive to public television districts, where informational content is national in scope. By contrast, private television content is distinctly local and more politically relevant to voters, resulting in increased political engagement of the electorate and improved accountability of politicians. These findings are not an outcome of television per se, but a consequence of the political value of local information. We thus highlight the salient role of local informational content in an environment unaffected by the limitations typically imposed on a researcher.

We conclude that local information is an important driver of political engagement, and document its decisive role in evoking accountability among politicians. On the one hand, it is concerning that national news has come to dominant most media outlets at the expense of local news (Martin and McCrain, 2019). On the other hand, we believe that the positive accountability effects of local information provide insight for the contemporary world of social media, where there is an opportunity to push back against the growing presence of national news. With legacy media, accountability often comes in the form of an editorial, where an informed journalist may speak out against a politician on behalf of the people. While the low barrier to entry of social media gives a previously unheard voice to the people, and this voice can amplify the accountability effects of local information because a would-be voter can now speak directly to a politician on social media in a way that was never possible before.

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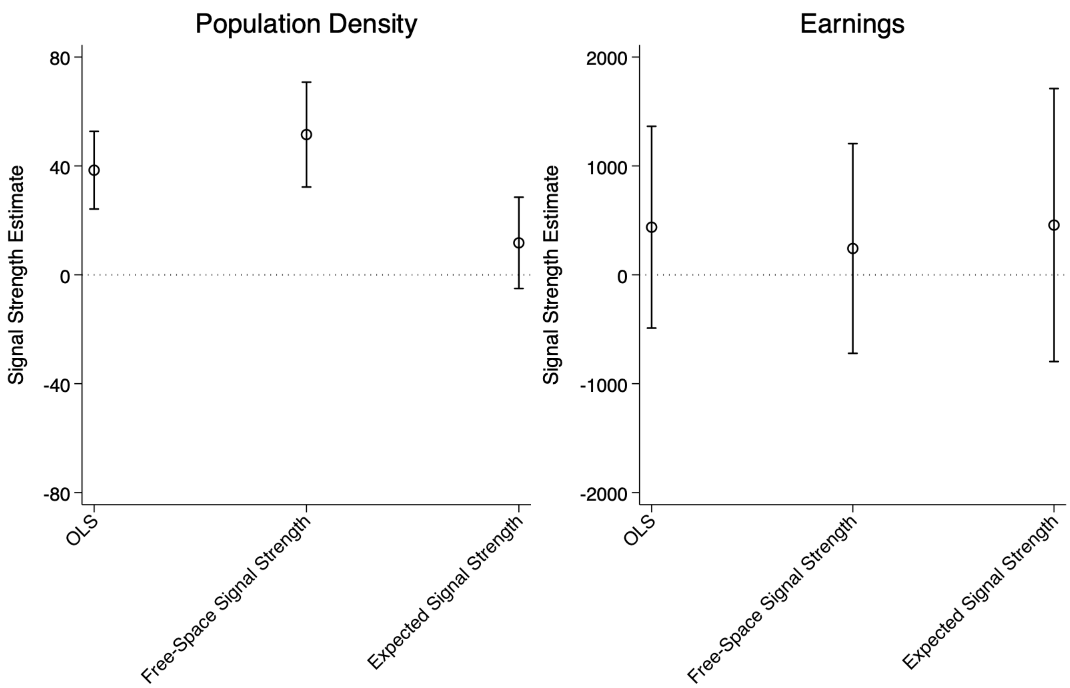
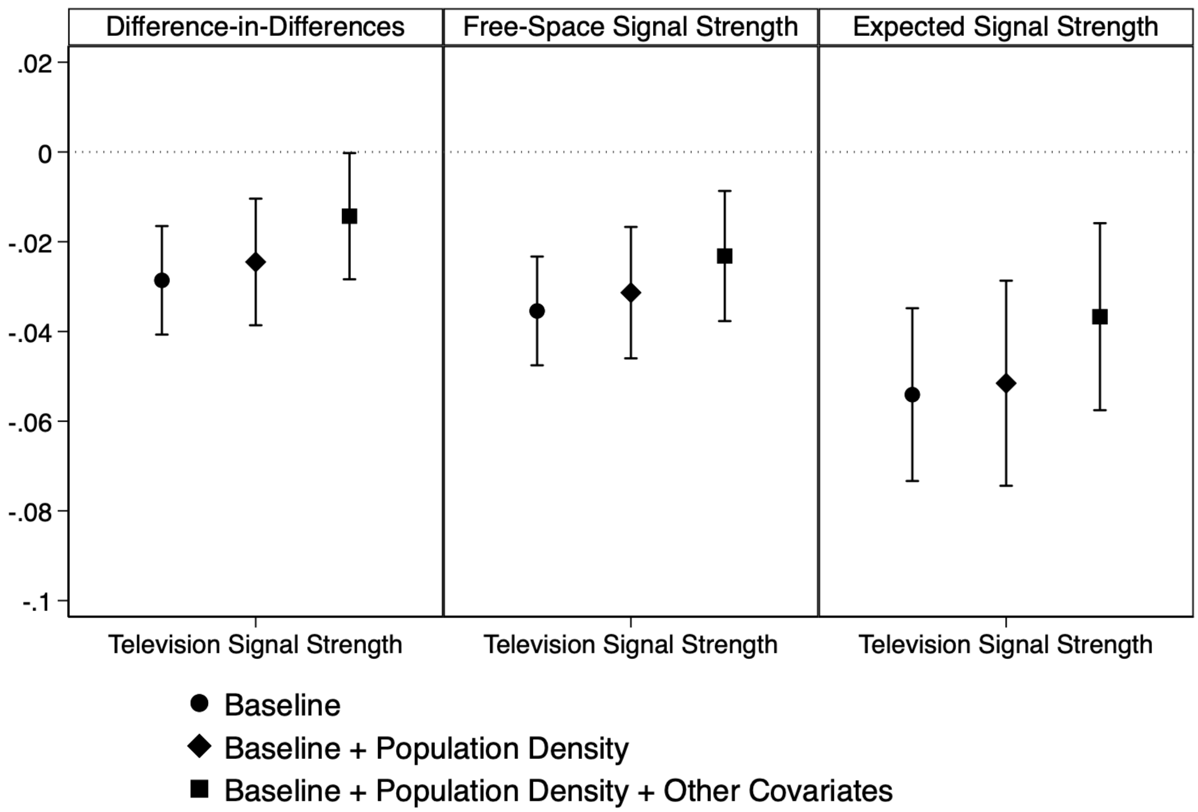
Figure 1: Balance Test – Population Density and Earnings



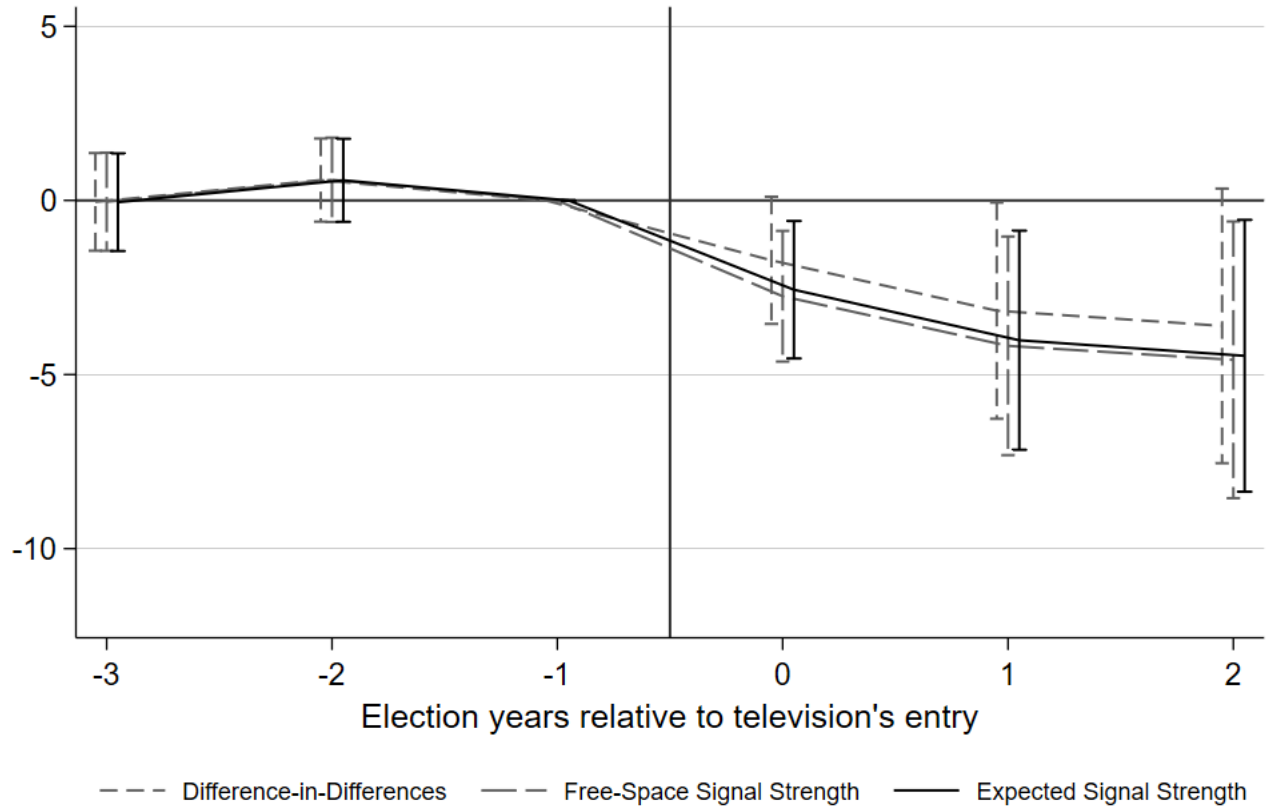
Notes: Observations are at the electoral district level. The outcome variables include the initial level of population density or earnings as labeled. This figure plots point estimates from a cross-sectional regression of each outcome on television signal strength in 1953—the initial treatment period. The free-space signal strength and expected signal strength estimates add the noted variable as a control to the basic OLS regression. Standard errors are clustered at the electoral district level and intervals reflect 95% confidence.

Figure 2: Regression Estimates – Television's Impact on Political Engagement



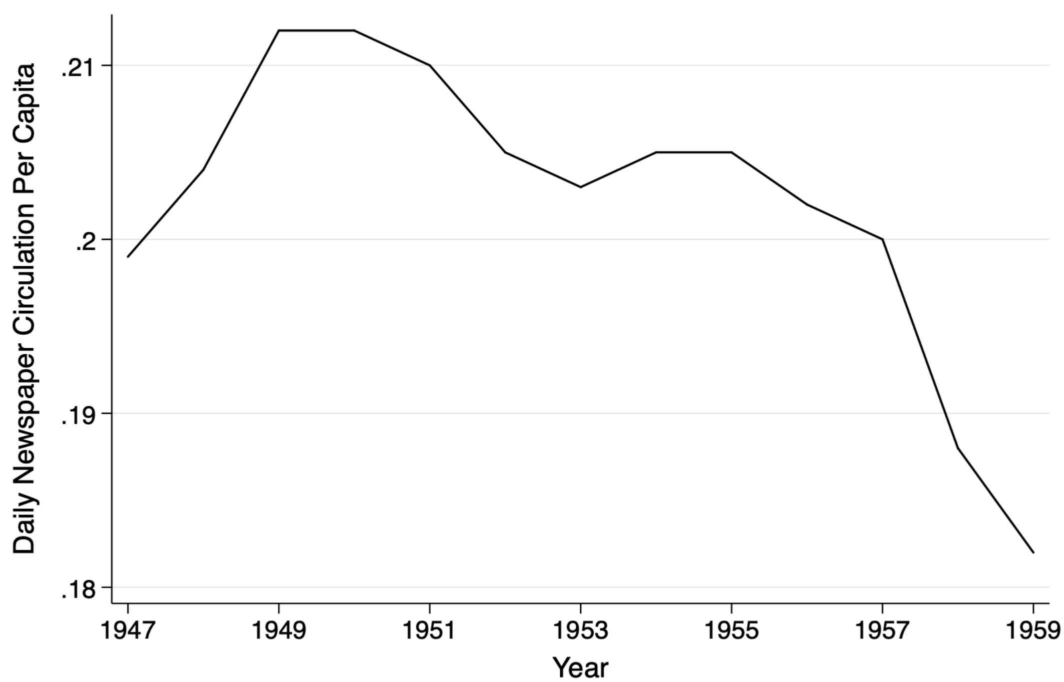
Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. The estimated treatment variable is a continuous measure of television signal strength. The left panel plots difference-in-differences estimates of β in equation (1), the middle panel plots these model estimates conditional on free-space signal strength, while the right panel plots estimates of equation (3) that are conditional on expected signal strength. All estimates include electoral district and election-year fixed effects. Other covariates include time-invariant measures of earnings, average age, literacy and urbanization rates. All covariates including population density are interacted with a year fixed effect, to allow the impact of each covariate to vary over time at the district level. Standard errors are clustered at the electoral district level and intervals reflect 95% confidence.

Figure 3: Event-Study Estimates – Television's Impact on Political Engagement



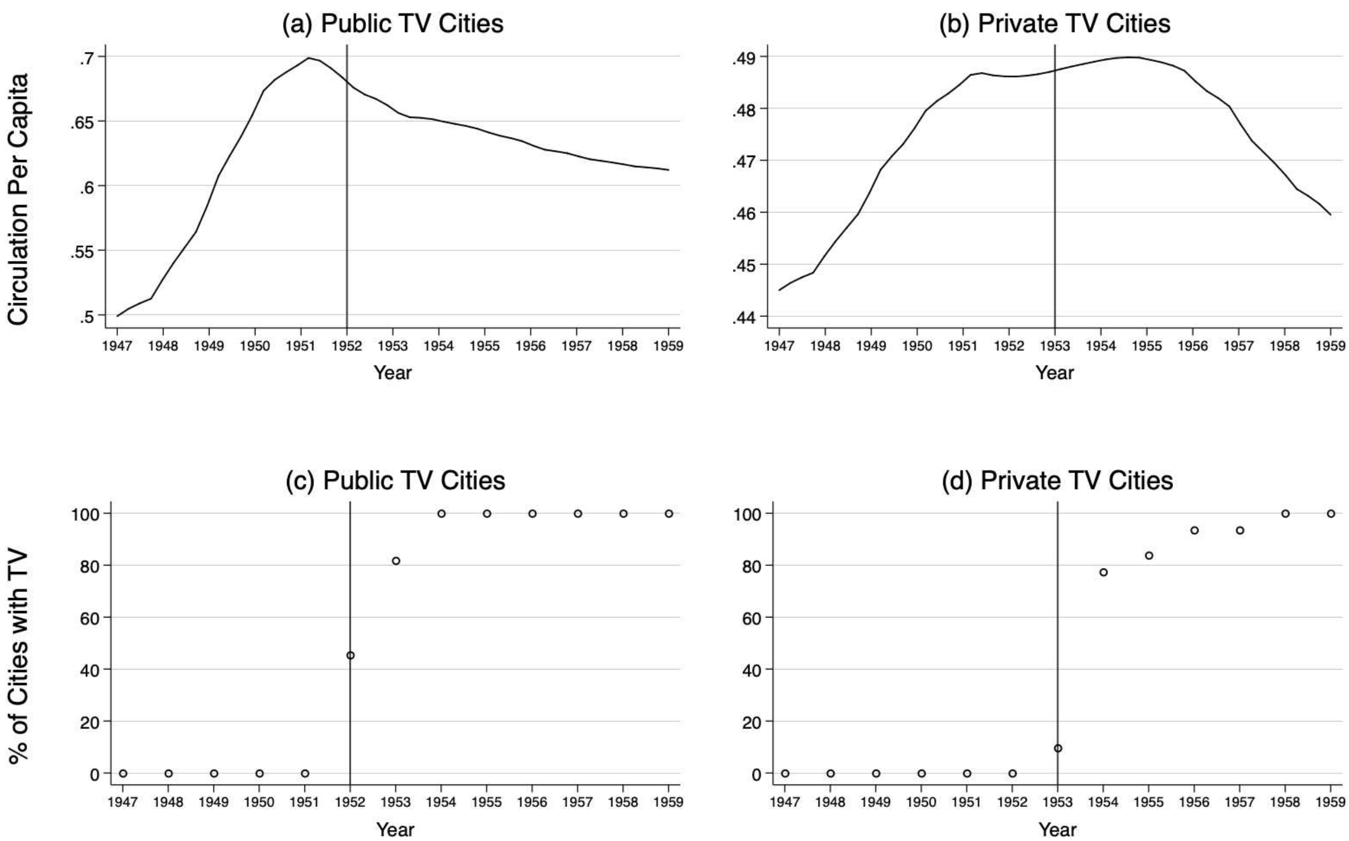
Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. The estimated treatment variable is a discrete measure of television signal strength equal to one if the district average is greater than 50 dbμV/m. The results of all three different estimation strategies are reported as labeled. All estimates include electoral district and election-year fixed effects, and the full set of covariates interacted a year fixed effect, including population density, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level and intervals reflect 95% confidence.

Figure 4: Daily Newspaper Circulation for Canada (1947-1959)



Notes: Observations are at the country level for this time series of daily newspaper circulation rates between 1947-1959. Circulation rates are adjusted for population and reported in per capita terms.

Figure 5: Daily Newspaper Circulation for Urban Centers (1947-1959)



Notes: Observations are at the city and year level. Average daily newspaper circulation rates are reported for our sample of 42 cities that receive public or private television—but not both—between 1947-1959, relative to the percent of cities with television. Circulation rates are adjusted for population and reported in per capita terms. Panel (a) is a kernel-weighted local polynomial smoothing of per capita circulation rates for the 11 cities that receive public television. Panel (b) is the same for the 31 cities that receive private television. Panels (c) and (d) report the percent of cities in our sample with public and private television, respectively.

Table 1: Television Content – Local Versus National Programming Ratios

	Prime Time Audience Segment		
	Private	Public	Private / Public
Informational content	0.67	0.30	2.22
Entertainment content	0.69	0.40	1.71
	Adult Audience Time Segment		
	Private	Public	Private / Public
Informational content	0.42	0.18	2.35
Entertainment content	0.67	0.66	1.01
	Daytime Audience Segment		
	Private	Public	Private / Public
Informational content	27.57	0.05	513.04
Entertainment content	29.30	7.62	3.85

Notes: In the first two columns, each number represents the ratio of locally produced content to nationally produced content aired on private or public television stations. The third column reports the ratio of the numbers in the first two columns, highlighting the predominance of locally produced content on private stations compared to public stations. Figures are based on total air time for the week January 15-21, 1956 for all English-language television stations in operation at the time (Fowler and Smythe, 1957). Prime time hours are 18:30 to 21:00 on weekdays and from sign-on to 21:00 on weekends. Adult audience hours include 21:00 to 6:00 every day. Daytime audience hours are 6:00 to 16:30 on weekdays.

Table 2: Public versus Private Television's Impact on Political Engagement

	Dependent Variable: Voter turnout		
	(1)	(2)	(3)
Signal Strength $[\beta^{pub}]$	-0.031***	-0.042***	-0.042***
	(0.008)	(0.009)	(0.013)
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	0.042**	0.048	0.058**
	(0.020)	(0.058)	(0.024)
Expected Signal Strength	No	No	Yes
Free-Space Signal Strength	No	Yes	No
Covariates	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes
Observations	1,764	1,764	1,764
Adjusted R^2	0.655	0.658	0.655
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.569	0.921	0.460

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Estimates are based on equation (4), where β^{pub} is the impact of public television on voter turnout, and β^{pvt} is the differential impact of private television. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Television Content – Percentage of Total Air Time by Content Type

	Private	Public	Private / Public
Informational content	25.00	29.20	0.86
Entertainment content	75.00	70.80	1.06

Notes: Figures are based on total air time for the week January 15-21, 1956 for all English-language television stations in operation at the time (Fowler and Smythe, 1957).

Table 4: Television's Impact on Vote Shares by Political Parties

	(1)	(2)	(3)	(4)	(5)	(6)
	Liberal Vote Share	Conservative Vote Share	Lib-Con Share Diff	Lib-Other Share Diff	Con-Other Share Diff	Major-Other Diff
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	-0.011	0.058	-0.070	0.051	0.121	0.109
	(0.045)	(0.053)	(0.082)	(0.085)	(0.098)	(0.109)
Signal Strength $[\beta^{pub}]$	-0.009	-0.029	0.019	-0.062	-0.082	-0.091
	(0.024)	(0.024)	(0.037)	(0.049)	(0.050)	(0.061)
Expected Signal Strength	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,764	1,764	1,764	1,764	1,764	1,764
Adjusted R^2	0.740	0.707	0.736	0.722	0.698	0.702
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.604	0.557	0.506	0.885	0.670	0.853

Notes: Observations are at the electoral district and election-year level. The outcome variables are for all election years between 1935-1958, and estimates are based on equation (4). Liberal and Conservative party vote shares are as labeled, and Lib-Con Share Diff denotes the difference in these vote shares. Other party vote shares are for all political parties excluding the Liberal and Conservative party, and the Major party vote shares denote the combined share of the Liberal and Conservative party. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Television's Impact on Political Competition

	(1)	(2)	(3)	(4)	(5)	(6)
	Left Vote Share	Right Vote Share	Left-Right Diff	Reelect Incumbent	Reelect Party	Polar-ization
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	0.031	-0.009	0.040	-0.003	-0.002	0.013
	(0.053)	(0.054)	(0.105)	(0.003)	(0.003)	(0.012)
Signal Strength $[\beta^{pub}]$	-0.023	0.028	-0.051	-0.000	0.001	-0.021***
	(0.024)	(0.028)	(0.050)	(0.002)	(0.001)	(0.007)
Expected Signal Strength	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,764	1,764	1,764	1,627	1,769	1,764
Adjusted R^2	0.718	0.722	0.722	0.003	0.115	0.277
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.856	0.685	0.910	0.175	0.587	0.442

Notes: Observations are at the electoral district and election-year level. The outcome variables are for all election years between 1935-1958, and estimates are based on equation (4). Left and Right vote shares are as labeled, and Left-Right Diff denotes the difference in these vote shares. Reelect Incumbent is an indicator equal to one if the incumbent politician of a district is reelected, whereas Reelect Party is equal to one if the same political party is reelected in a district. Polarization is a standard polarization measure based on vote shares across party lines. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Television's Impact on Political Accountability

	(1)	(2)	(3)	(4)
	Extensive		Intensive	
	Speech Locality	Place-Based Speech Locality	Place Locality	Parliamentary Vote Dissent
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	0.322**	0.388**	0.212***	0.008*
	(0.142)	(0.160)	(0.075)	(0.004)
Signal Strength $[\beta^{pub}]$	-0.004	-0.008	-0.025	-0.004
	(0.059)	(0.070)	(0.034)	(0.002)
Expected Signal Strength	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes
Observations	1,646	1,646	1,646	1,509
Adjusted R^2	0.293	0.330	0.284	0.188
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.010	0.008	0.004	0.321

Notes: Observations are at the electoral district and election-year level. The outcome variables are for all election years between 1935-1958, and estimates are based on equation (4). Speech locality and place-based speech locality are extensive margin measures of speech localness, and place locality is an intensive margin measure, as defined in Section 3. Parliamentary vote dissent is the inverse hyperbolic sine of the number of times an elected politician votes against party lines in Parliament. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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This appendix provides additional evidence in support of our main hypothesis that local information plays a crucial role in the political engagement of citizens and political accountability of elected politicians. In Appendix A we document several additional robustness checks. We then summarize the proposed method of Borusyak and Hull (2023) in Appendix B, and show that alternative ways of modeling expected signal strength lead to the same conclusions and point estimates as our baseline. In Appendix C, we provide evidence in favor of the parallel trends assumption and evidence against treatment effect heterogeneity biasing our results. Finally, Appendix D describes the data and sources.

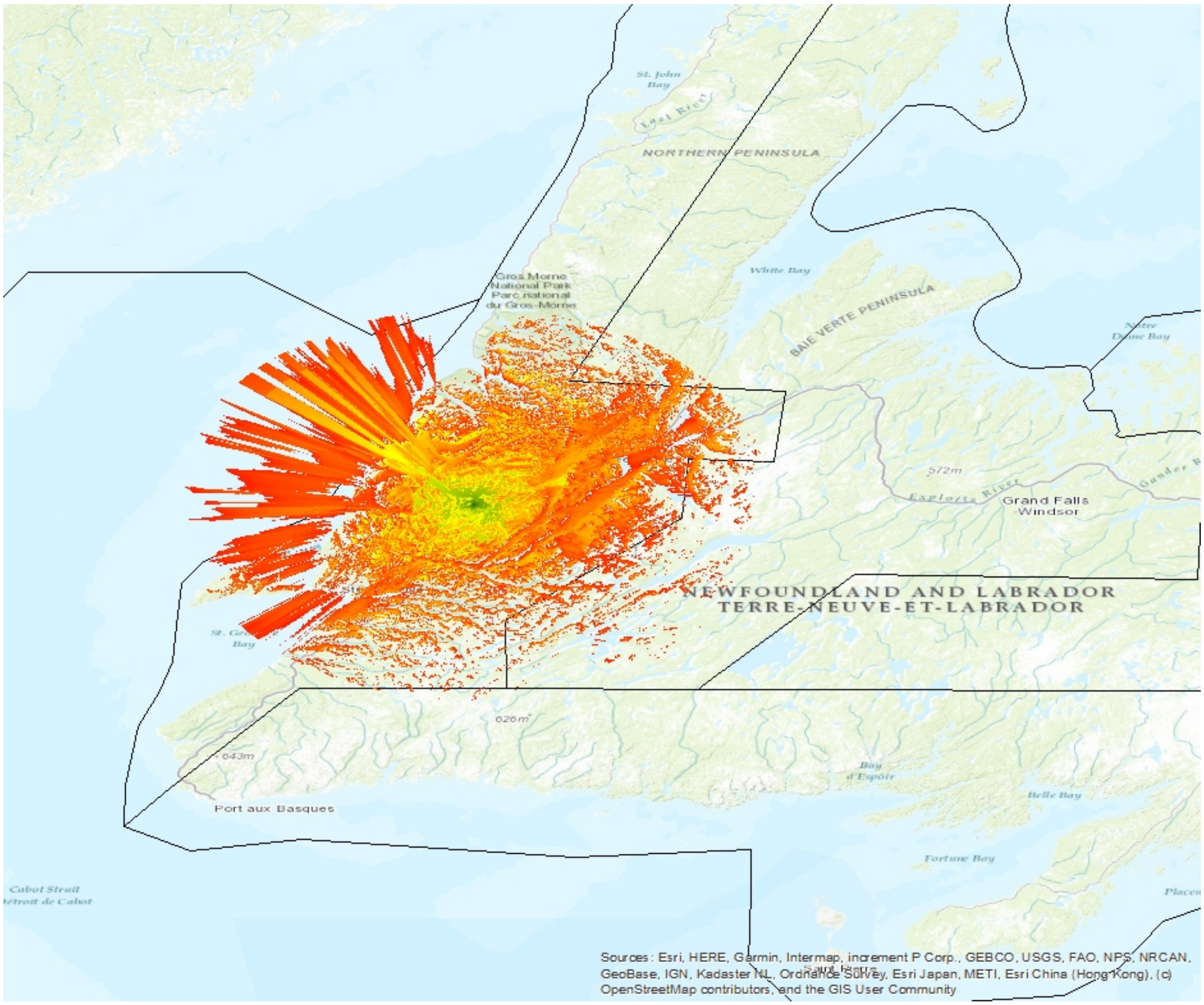
A Additional Empirical Evidence

In this section, we provide an example for the ITM signal strength Estimate (Figure A.1) and highlight the variation in actual and expected signal strength (Figure A.2). In addition, we provide evidence that our estimates are robust in the following figures and tables:

1. Event-Study for public v. private television (Figure A.3)
2. Sensitivity: Varying minimum threshold for signal strength (Figure A.4)
3. Sensitivity: Dropping districts one-at-a-time (Figure A.5)
4. Results with varying degrees of covariates and identification (Table A.1)
5. Magnitude with varying degrees of covariates and identification (Table A.2)
6. Results with varying degrees of covariates, identification, and fixed effects (Table A.3)
7. Horse race regressions between private and public signal strength (Table A.4)
8. Public v. private television, alternative fixed effects (Table A.5)
9. Public v. private television, dropping districts with dual exposure (Table A.6)
10. Results in the extended sample until 1968 (Table A.7)
11. Public v. private television in the extended sample until 1968 (Table A.8)
12. Differences in newspaper circulation per capita (Table A.9)
13. Left-Right assignment of political parties (Table A.10)

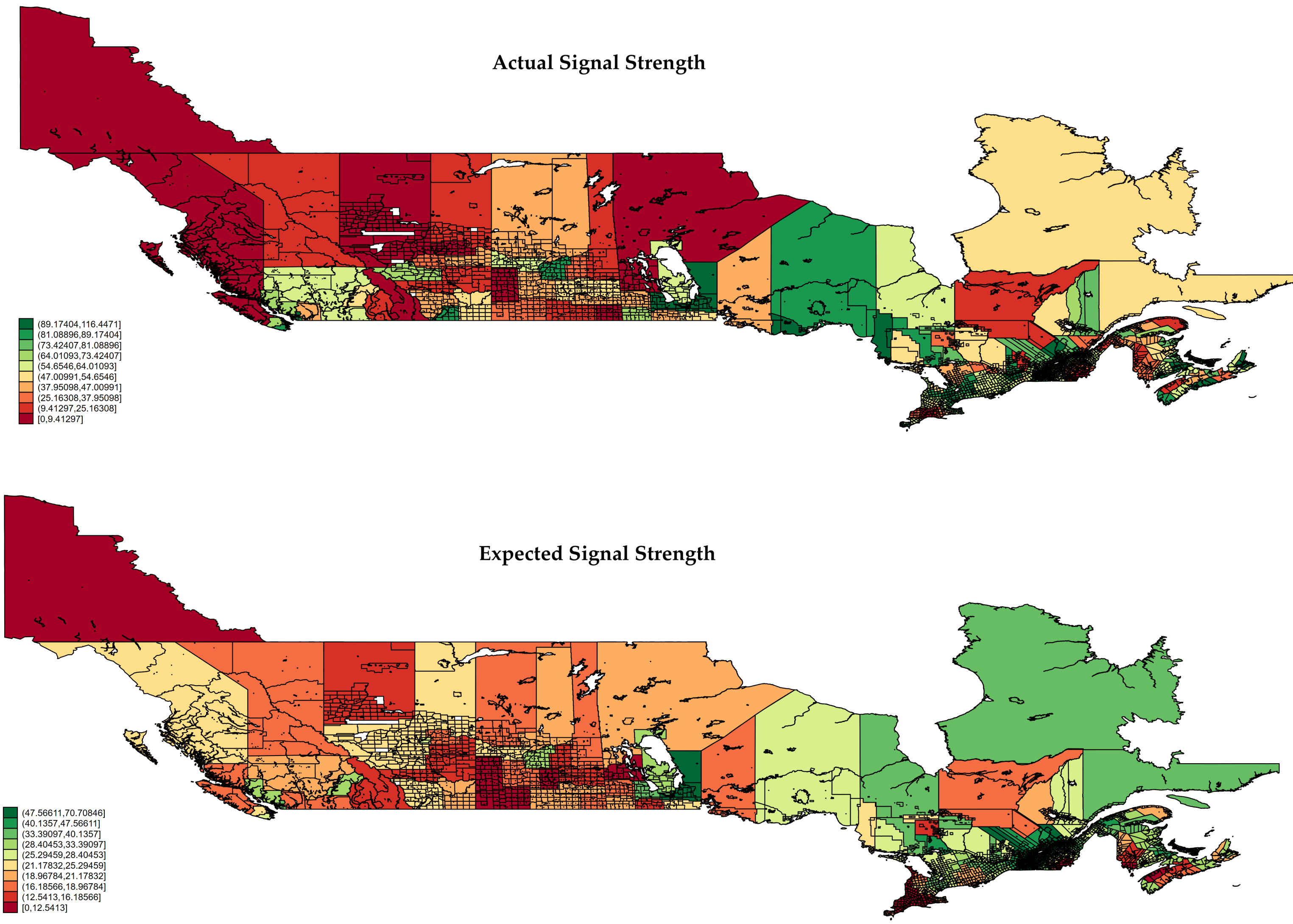
We conclude that our estimates are robust to various alternative specifications.

Figure A.1: Example ITM Signal Strength Estimate



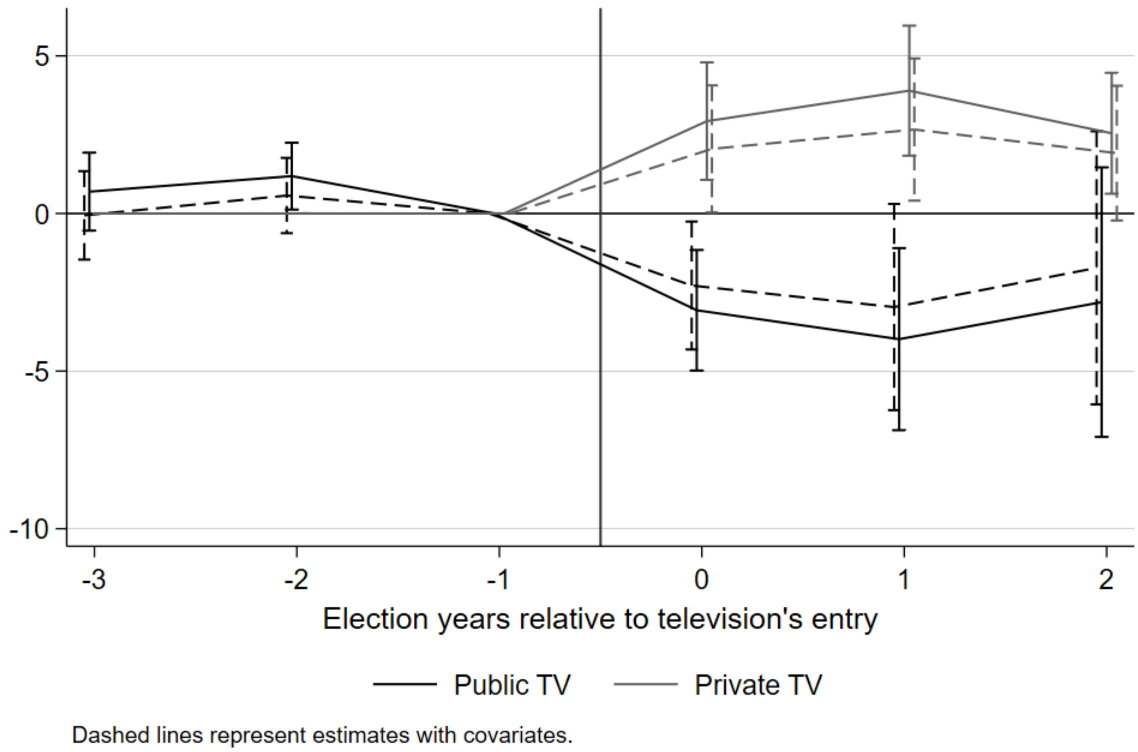
Notes: Irregular Terrain Model estimate for CBYT station, Corner Brook, Newfoundland. Green colors represent the strongest signal strength, with an attenuation is strength as the color gradient transitions to red. Black lines represent electoral district boundaries, with CBYT located in the Humber–St George electoral district.

Figure A.2: Example of Actual and Expected Signal Strength Estimates by Electoral Districts



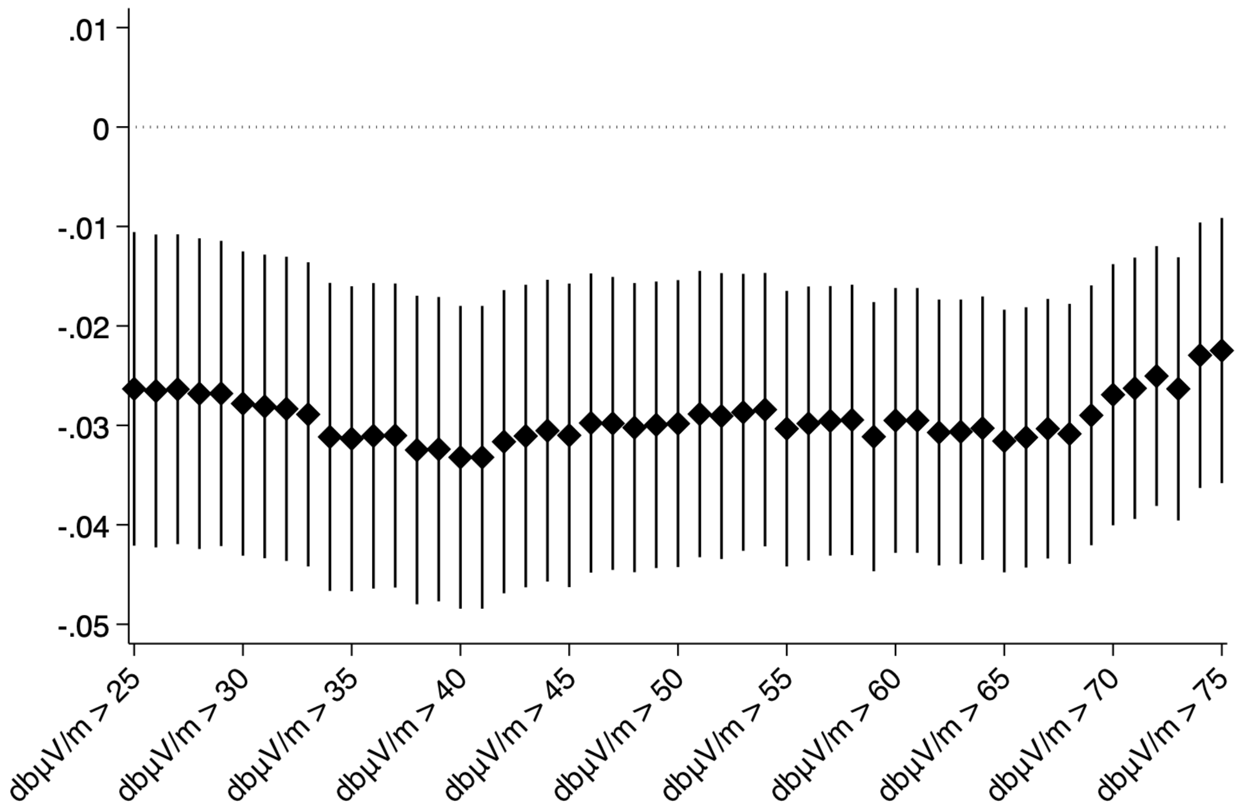
Notes: This figure visualizes actual and expected signal strength at the electoral district level for 1960 public television.

Figure A.3: Event Study – Public versus Private Television's Impact on Political Engagement



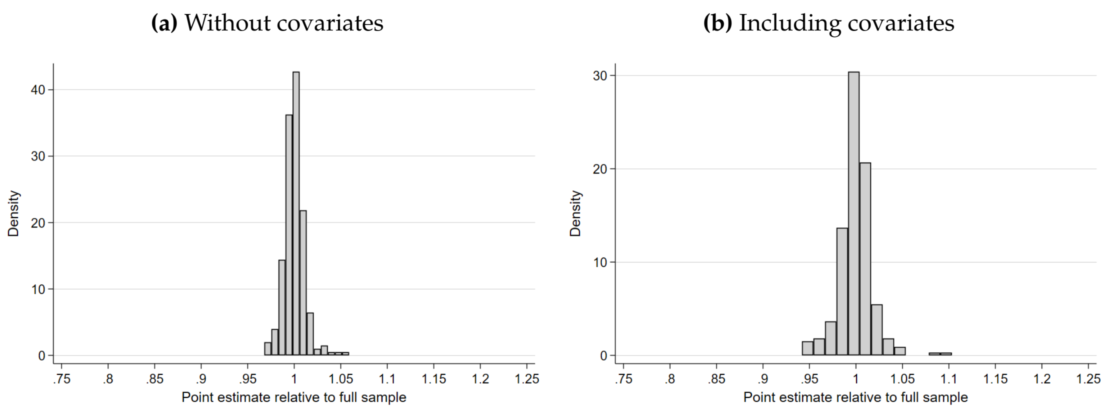
Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Estimates of the public and private television treatment effect are based on the interaction model described in equation (4) but presented as an event study, where television signal strength is equal to one if the district average is greater than 50 dbμV/m. All estimates include electoral district and election-year fixed effects. The dashed-line results include the full set of covariates interacted with a year fixed effect (i.e., population density, average age, literacy and urbanization rates). Standard errors are clustered at the electoral district level and intervals reflect 95% confidence.

Figure A.4: Sensitivity Check – Varying the Minimum Threshold for Television Signal Strength



Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Throughout the analysis, we use a measure of television signal strength set to 0 dbμV/m for all values of signal strength less than or equal to 50 dbμV/m. Here, we adjust this threshold to show that our main result is insensitive to the choice of threshold. We plot point estimates of β from equation (3), conditional on expected signal strength, for these different thresholds. All estimates include electoral district and election-year fixed effects, and include the full set of covariates interacted with a year fixed effect (i.e., population density, average age, literacy and urbanization rates). Standard errors are clustered at the electoral district level and intervals reflect 95% confidence.

Figure A.5: Sensitivity Check – Influential Observations



Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. This figure is based on estimates of β from equation (3), conditional on expected signal strength, excluding one electoral district d at a time. We plot these point estimates relative to the full sample estimate to highlight the stability of our estimates. For example, a value of 1 indicates that the estimate did not change across samples; i.e., the exclusion of district d had no impact on the estimated treatment effect.

Table A.1: Television's Impact on Political Engagement

	Difference-in-Differences			Free-Space Signal Strength			Expected Signal Strength		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Signal Strength	-0.029***	-0.025***	-0.014**	-0.035***	-0.031***	-0.023***	-0.054***	-0.052***	-0.037***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.010)	(0.012)	(0.011)
Population Density	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,795	1,764	1,764	1,795	1,764	1,764	1,795	1,764	1,764
Adjusted R^2	0.642	0.636	0.651	0.643	0.637	0.653	0.645	0.639	0.653

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Columns (1)-(3) report difference-in-differences estimates of equation (1), while columns (4)-(6) report estimates of this model conditional on free-space signal strength. Columns (7)-(9) report estimates of equation (3) that are conditional on expected signal strength. All estimates include electoral district and election-year fixed effects. Other covariates include time-invariant measures of earnings, average age, literacy and urbanization rates. All covariates including population density are interacted with a year fixed effect, to allow the impact of each covariate to vary over time at the district level. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Television's Impact on Political Engagement (Magnitude)

	Difference-in-Differences			Free-Space Signal Strength			Expected Signal Strength		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\mathbb{1}$[Signal Strength $>$ 50 dbμV/m]	-1.971***	-1.653***	-0.924	-2.402***	-2.033***	-1.507***	-3.014***	-2.961***	-2.260***
	(0.532)	(0.579)	(0.570)	(0.529)	(0.589)	(0.571)	(0.769)	(0.831)	(0.803)
Population Density	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,797	1,764	1,764	1,795	1,764	1,764	1,797	1,764	1,764
Adjusted R^2	0.640	0.634	0.650	0.640	0.634	0.652	0.640	0.635	0.651

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Across all estimates, signal strength has been discretized to equal one if greater than 50 dbμV/m for the purpose of assessing magnitude. Columns (1)-(3) report difference-in-differences estimates of equation (1), while columns (4)-(6) report estimates of this model conditional on free-space signal strength. Columns (7)-(9) report estimates of equation (3) that are conditional on expected signal strength. All estimates include electoral district and election-year fixed effects. Other covariates include time-invariant measures of earnings, average age, literacy and urbanization rates. All covariates including population density are interacted with a year fixed effect, to allow the impact of each covariate to vary over time at the district level. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Television's Impact on Political Engagement (Alternative Fixed Effects)

	Difference-in-Differences			Free-Space Signal Strength			Expected Signal Strength		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Signal Strength	-0.038***	-0.033***	-0.027***	-0.041***	-0.037***	-0.032***	-0.061***	-0.055***	-0.040***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.008)	(0.007)	(0.009)	(0.011)	(0.010)
Population Density	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year $\times$ Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,785	1,757	1,757	1,785	1,757	1,757	1,785	1,757	1,757
Adjusted R^2	0.756	0.744	0.762	0.757	0.744	0.763	0.758	0.745	0.762

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Columns (1)-(3) report difference-in-differences estimates of equation (1), while columns (4)-(6) report estimates of this model conditional on free-space signal strength. Columns (7)-(9) report estimates of equation (3) that are conditional on expected signal strength. All estimates include electoral district and election-year $\times$ province fixed effects. Other covariates include time-invariant measures of earnings, average age, literacy and urbanization rates. All covariates including population density are interacted with a year fixed effect, to allow the impact of each covariate to vary over time at the district level. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Public versus Private Television's Impact on Political Engagement

	(1)	(2)	(3)
Public Signal Strength	-0.029***	-0.045***	-0.044***
	(0.007)	(0.008)	(0.009)
Private Signal Strength	0.003	0.003	-0.009
	(0.006)	(0.006)	(0.008)
Expected Signal Strength	No	No	Yes
Free-Space Signal Strength	No	Yes	No
Covariates	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes
Observations	1,764	1,764	1,764
Adjusted R^2	0.655	0.658	0.655

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Estimates are based on horse race version of equation (3) between public and private television. All estimates include electoral district and election-year fixed effects, and include the full set of covariates interacted with a year fixed effect (i.e., population density, average age, literacy and urbanization rates). Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Public versus Private Television's Impact on Political Engagement (Alt. Fixed Effects)

	(1)	(2)	(3)
Signal Strength $[\beta^{pub}]$	-0.045***	-0.051***	-0.029**
	(0.008)	(0.008)	(0.012)
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	0.037**	0.056	0.038*
	(0.019)	(0.059)	(0.023)
Expected Signal Strength	No	No	Yes
Free-Space Signal Strength	No	Yes	No
Covariates	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes
Observations	1,757	1,757	1,757
Adjusted R^2	0.767	0.767	0.767
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.697	0.930	0.681

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Estimates are based on equation (4), where β^{pub} is the impact of public television on voter turnout, and β^{pvt} is the differential impact of private television. All estimates include electoral district and election-year $\times$ province fixed effects, and include the full set of covariates interacted with a year fixed effect (i.e., population density, average age, literacy and urbanization rates) Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Sensitivity Check – No Districts with Dual Exposure to Public and Private Television

	(1)	(2)	(3)
Signal Strength $[\beta^{pub}]$	-0.033***	-0.043***	-0.043***
	(0.009)	(0.009)	(0.013)
Signal Strength $\times I^{Pvt}$ $[\beta^{pvt}]$	0.052**	0.021	0.066***
	(0.021)	(0.049)	(0.024)
Expected Signal Strength	No	No	Yes
Free-Space Signal Strength	No	Yes	No
Covariates	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes
Observations	1,660	1,660	1,660
Adjusted R^2	0.659	0.662	0.659
p-value ($\beta^{pvt} + \beta^{pub} = 0$)	0.361	0.632	0.286

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1958. Estimates are based on equation (4), where all districts with dual exposure to both public and private television are dropped from the sample. Here, β^{pub} is the impact of public television on voter turnout, and β^{pvt} is the differential impact of private television. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01.

Table A.7: Television's Impact on Political Engagement (Extended Panel)

	Difference-in-Differences			Free-Space Signal Strength			Expected Signal Strength		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Signal Strength	-0.020***	-0.016**	-0.008	-0.026***	-0.022***	-0.017**	-0.041***	-0.044***	-0.031***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
Population Density	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,832	2,764	2,764	2,832	2,764	2,764	2,832	2,764	2,764
Adjusted R^2	0.666	0.657	0.670	0.667	0.658	0.673	0.668	0.660	0.672

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1968 (extended panel). Columns (1)-(3) report difference-in-differences estimates of equation (1), while columns (4)-(6) report estimates of this model conditional on free-space signal strength. Columns (7)-(9) report estimates of equation (3) that are conditional on expected signal strength. All estimates include electoral district and election-year fixed effects. Other covariates include time-invariant measures of earnings, average age, literacy and urbanization rates. All covariates including population density are interacted with a year fixed effect, to allow the impact of each covariate to vary over time at the district level. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Public versus Private Television's Impact on Political Engagement (Extended Panel)

	(1)	(2)	(3)
Signal Strength (dB) $[\beta^{pub}]$	-0.017**	-0.026***	-0.046***
	(0.008)	(0.008)	(0.010)
Signal Strength (dB) $\times I^{Pvt}$ $[\beta^{pvt}]$	0.024	0.008	0.055***
	(0.016)	(0.016)	(0.019)
Expected Signal Strength	No	No	Yes
Free-Space Signal Strength	No	Yes	No
Covariates	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Election-Year FE	Yes	Yes	Yes
Observations	2,764	2,764	2,764
Adjusted R^2	0.671	0.675	0.673

Notes: Observations are at the electoral district and election-year level. The outcome variable is voter turnout for all election years between 1935-1968 (extended panel). Estimates are based on equation (4), where β^{pub} is the impact of public television on voter turnout, and β^{pvt} is the differential impact of private television. All covariates are time-invariant initial conditions measured at the district level interacted with a year fixed effect, including population density, earnings, average age, literacy and urbanization rates. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Mean Differences in Newspaper Circulation Per Capita

	(1)	(2)	(3)
	After TV Mean (SE)	Before TV Mean (SE)	Difference in Means
Δ Circulation Per Capita	-0.005	0.009	-0.014***
	(0.002)	(0.004)	(0.005)
	$n = 207$	$n = 284$	$n = 491$
	Public TV Mean (SE)	Private TV Mean (SE)	Difference in Means
Δ Circulation Per Capita	0.004	0.002	0.002
	(0.008)	(0.002)	(0.006)
	$n = 128$	$n = 363$	$n = 491$

Notes: Observations are at the city and year level. This table reports difference-in-means t-tests. The reported means measure the change in daily newspaper circulation rates per capita for different subgroups of the data, based on a sample of 42 major urban centers for the years 1949-1959. In the first panel, columns (1) and (2) report the change in circulation rates (per capita) in cities after television arrives and before television arrives, respectively. In the second panel, columns (1) and (2) report the change in circulation rates (per capita) in cities with public television and cities with private television, respectively. In both panels, column (3) reports the difference in means between columns (1) and (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Political Parties in Parlinfo Database by Left-Right Assignment

Political Spectrum	**Political Party Name in Parlinfo**
Left-Leaning	Liberal Party of Canada, New Democratic Party, Green Party of Canada, Progressive, Co-operative Commonwealth Federation, Independent Liberal, Liberal Progressive, Labour, Liberal Labour Party, Unionist (Liberal), Independent Progressive, Labor-Progressive Party, Nationalist Liberal, Independent Co-operative Commonwealth Federation, Bloc Québécois, Patrons of Industry, Independent Labour, United Farmers of Alberta, United Farmers of Ontario, United Farmers of Ontario-Labour, United Farmers, United Reform Movement, Unity, Labor-Progressive Party, Bloc populaire canadien, New Party, Rhinoceros Party, Rhinoceros Party of Canada, Marxist-Leninist Party of Canada, Canadian Action Party, Marijuana Party, Natural Law Party of Canada, Party for the Commonwealth of Canada, Union Populaire, Animal Protection Party of Canada, Communist Party of Canada, Independent Liberal Progressive, Liberal Labour Progressive, Liberal Protectionist, National Labour, National Unity, Opposition-Labour, Progressive Canadian Party, United Farmers-Labour, Radical Chrétien, Socialist, United Reform, Opposition, Farmer, Farmer Labour, Labour Farmer, Non-Partisan League, National Liberal Progressive, National Party of Canada, Parti Ouvrier Canadien, Protectionist, Trades Union, United Progressive, Verdun, Socialist Labour, Canadian Labour, Christian Liberal, Farmer-United Labour, National Socialist, Ouvrier Indépendant, Strength in Democracy, Animal Alliance, Environment Voters Party of Canada, First Peoples National Party of Canada, Ouvrier indépendent, Progressive Workers Movement, Work Less Party
Right-Leaning	Progressive Conservative Party, Conservative (1867-1942), Conservative Party of Canada, Social Credit Party of Canada, Liberal-Conservative, Conservative Party of Canada, Social Credit Party of Canada, Reform Party of Canada, Canadian Reform Conservative Alliance, Independent Conservative, Nationalist Conservative, Independent Progressive Conservative, New Democracy, Ralliement des créditistes, Unionist, McCarthyite, Nationalist, Reconstruction Party, Independent Reconstruction Party, Confederation of Regions Western Party, Social Credit Party of Canada, Libertarian Party of Canada, Abolitionist Party of Canada, Canadian Party, Candidate of the Electors, Conservative-Labour, Independent Nationalist, People's Party of Canada, People's Party of Canada, Union of Electors, National Liberal and Conservative Party (1921), Reform, Protestant Protective Association, Unité nationale, National Government, Parti Nationaliste du Québec, Candidat libéral des électeurs, Christian Heritage Party of Canada, Independent Social Credit, Maverick Party, Social Credit-National Unity, Democratic Advancement Party of Canada, Liberal Conservative Coalition, New Capitalist Party, Newfoundland and Labrador First Party, Prohibitionist, Technocrat, Free Party Canada, Canada Party, Western Block Party, Canadian Nationalist Party, National Citizens Alliance of Canada, Parti pour l'Indépendance du Québec

Notes: In this table we document our assignment of political parties across the left-right political spectrum. Each party name noted in the table is the exact name as listed in the Parlinfo database, and is affiliated with at least one politician in our data.

B Modeling Expected Signal Strength

Our novel identification strategy is motivated by insights from Borusyak and Hull (2023). They derive a general solution to a treatment scenario similar to ours, noting that even when treatment is random, exposure to treatment may be non-random. In our context, this insight implies that even if television transmitter installations are *conditionally* random and satisfy the usual difference-in-differences assumptions, the timing of exposure to television may still be a source of omitted variable bias (OVB).

OVB Example Toronto and Montreal are the first two cities to receive television because they are the largest markets in the country. Nearby districts then receive television earlier than expected because of their proximity to either city. This suggests that *any* district near an economic or population center of the country is more likely to be treated earlier than a district on the periphery. In this instance, an estimate of our two-way fixed effects difference-in-differences model will fail to identify our parameter of interest, unless we make the strong assumption that "central" districts do not differ from "peripheral" districts in any relevant, time-varying way. Such an assumption is equivalent to assuming that districts are homogeneous with respect to political discontent, civic mindedness or really any non-electoral political activity.

Stylized Model Assume model $y_i = \beta x_i + \varepsilon_i$, where the realized treatment, $x_i = f_i(g_s, w_i)$, combines variation in shocks g_s due to installation of television transmitter s with pre-determined variables w_i according to $f(\cdot)$, a known formula to the researcher. Borusyak and Hull (2023) show that (i) if shocks to g are exogenous to ε, given predetermined variables w, and (ii) the conditional distribution $G(g|w)$ is known, then a candidate expected treatment variable can be defined to solve the OVB problem.

Result Expected treatment $\mu_i = E\left[f_i(g_s, w_i)|w_i\right]$ is the sole confounder of the realized treatment, x_i. With this stylized model, we can show this result by looking at the correlation of model residuals with the realized treatment:

$$E\left[\frac{1}{N}\sum_i x_i \varepsilon_i\right] = E\left[\frac{1}{N}\sum_i E\left[f_i(g_s, w_i)\varepsilon_i|w_i\right]\right] \tag{B.1}$$

$$= E\left[\frac{1}{N}\sum_i \mu_i E\left[\varepsilon_i|w_i\right]\right] \tag{B.2}$$

$$= E\left[\frac{1}{N}\sum_i \mu_i \varepsilon_i\right] \tag{B.3}$$

The OVB problem is solved by recentering the treatment variable around its expected treatment, $\tilde{x}_i = x_i - \mu_i$, which is uncorrelated with residuals ε_i by construction. In practice, this equates to adding expected treatment as a control variable to the stylized model, thus providing an unbiased and consistent estimate of β. For our purposes, the realized treatment (x_i) is television signal strength at the electoral district level ($signal_{d,t}$), and the expected treatment (μ_i) is expected signal strength ($\mu_{d,t}$)—i.e., the non-random exposure of a district to realized treatment.

B.1 Criteria for a Valid Measure of Expected Signal Strength

To compute expected treatment, it is necessary to know the underlying data-generating process of our realized treatment. In our setting, function $f(\cdot)$, which combines the pre-determined variables with the underlying distribution of shocks to g, is the Irregular Terrain Model—the radio propagation model we use to estimate the attenuation of signal strength across space (see Section 3 for more details). While the underlying distribution of shocks g is exogenous, it is also unknown and has to be modeled.

Modeling the distribution of these shocks amounts to modeling the timing of activation for the complete network of television transmitters. We do this by assigning every television transmitter a probability of being activated in year t, and construct an average value based on 500 permutations of this process for each year. As this procedure allows us many degrees of freedom, we restrict ourselves to the following criteria:

(1) In each simulation, transmitters with an early realized activation date receive a higher probability of activation than transmitters activated later on.

(2) The measure of expected signal strength that is constructed from the 500 permutations should be correlated with the model residuals, as in equation (B.3). In other words, the residuals from a simple OLS regression,

$$Y_{d,t} = \alpha_d + \alpha_t + \beta signal_{d,t} + \epsilon_{d,t},$$

should be correlated with $\mu_{d,t}$.

(3) Expected signal strength $\mu_{d,t}$ should capture all of the predictive variation of the initial treatment on any observable pre-determinant of treatment (e.g., a district's initial population density in 1931). In other words, the point estimate δ from this example equation,

$$Pop_{d,1931} = \delta signal_{d,1953} + \gamma \mu_{d,1953} + \epsilon_d,$$

should be statistically no different from zero.

B.1.1 Alternative Shock Distributions

In this subsection, we describe 23 shock distributions that satisfy criteria (1) in Section B.1, which we use as alternatives to the shock distribution described in Section 4.3 of the paper. Table B.1 summarizes this information. The first column of the table denotes an identifier for the alternative

Table B.1: Alternative Shock Distributions

Alt.	Step	Probability of Transmitter Activation	Activation Rank	Randomness	Selection Criteria
1		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)} + \frac{1}{\#\ towers\ activated_t}$	Months	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
2	i	LPM: $activated_t = f(Population,\ TransmittersActivated)_t$	-	$Active \sim Binomial(1, \widehat{activated}_t)$	Active = 1
	ii			$g \sim Normal(\widehat{activated}_t, sd(\widehat{activated}_t))$	Highest g
3		$\Pr = \frac{\#\ towers\ active_t}{\# towers}$	-	$Active \sim Binomial(1, \Pr)$	Active=1
4		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
5	i	$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$Active \sim Binomial(1, \Pr)$	Active = 1
	ii			$g \sim Normal(\Pr, sd(\Pr))$	Highest g
6		$\Pr = 1 - \frac{Year - min(Year)}{max(Year) - min(Year)}$	Years	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
7		$\Pr = \frac{1}{Duration}$	Year	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
8		Lasso: $activated_t = f(Population,\ TransmittersActivated,\ Lat,\ Lon)_t$	-	$g \sim Normal(\widehat{activated}_t, sd(\widehat{activated}_t))$	Highest g
9		Lasso: $activated_t = f(Population,\ TransmittersActivated,\ Lat,\ Lon)_t$	-	$activated_t \sim Binomial(1, \widehat{activated}_t)$	Active = 1
10		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, 2 * sd(\Pr))$	Highest g
11		$\Pr = \frac{\#\ towers\ active_t}{\# towers}$	-	$g \sim Normal(\Pr, 1)$	Highest g
12		$\Pr = 1 - \frac{Year - min(Year)}{max(Year) - min(Year)} + \frac{1}{\#\ towers\ activated_t}$	Years	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
13		$\Pr = 1 - \frac{Dist(Toronto) - min(Dist(Toronto))}{max(Dist(Toronto)) - min(Dist(Toronto))}$	Distance	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
14	i	$\Pr = 1 - \frac{Dist(Toronto) - min(Dist(Toronto))}{max(Dist(Toronto)) - min(Dist(Toronto))}$	Distance	$activated_t \sim Binomial(1, \Pr)$	Active = 1
	ii	$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
15	i	$\Pr = 1 - \frac{Dist(Toronto) - min(Dist(Toronto))}{max(Dist(Toronto)) - min(Dist(Toronto))}$	Distance	$g \sim Normal(\Pr, sd(\Pr))$	
	ii	$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, sd(\Pr))$	
	iii			randomly select step i) or ii)	Highest g
16		$\Pr = 1 - \frac{Pop. - min(Pop.)}{max(Pop.) - min(Pop.)}$	Population	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
17		$\Pr = 1 - \frac{Rank(Pop.) - min(Rank(Pop.))}{max(Rank(Pop.)) - min(Rank(Pop.))}$	Ranked Population	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
18		$\Pr = \frac{1}{Rank(Pop.)}$	Ranked Population	$g \sim Normal(\Pr, sd(\Pr))$	Highest g
19		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, sd(\sqrt{\Pr}))$	Highest g
20		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}$	Months	$g \sim Normal(\Pr, sd(\Pr^2))$	Highest g
21		$\Pr = \left(1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)}\right) \times \left(1 - \frac{1}{\#\ towers\ activated_t}\right)$	Months	$g \sim Normal(\Pr, sd(\Pr)$	Highest g
22		-	Years	$g \sim Normal\left(Year, 1 - \frac{1}{\#\ towers\ activated_t}\right)$	Highest g
23		$\Pr = 1 - \frac{Dur. - min(Dur.)}{max(Dur.) - min(Dur.)} + \frac{1}{\#\ towers\ activated_t}$	Months	$g \sim Normal(\Pr, sd(\Pr))$	Highest g w/ no overlap

shock distribution, ranging from 1-23. The activation process involves multiple steps for alternatives 2, 5, 14 and 15, so the second column outlines the order of these steps. The third column defines the modeled probability that transmitter j is activated in year t, with each being an alternative to the assumed probability in equation (2) from Section 4.3 of the paper. The fourth column describes how we rank the activation of the transmitters in our data—e.g., we take the approach of ranking transmitters by year of activation in Section 4.3. The fifth column describes how we transform each probability into a shock. Here, we either draw shocks from a normal or binomial distribution, using the probabilities calculated in the third column. The final column defines how we select the s number of transmitters active in year t. The "Highest g" criteria indicates that we sort across shock realizations g, selecting the s highest realizations until s is equal to the number of transmitters active in year t. "Active = 1" denotes that we simply select which ever transmitter has a shock realization of one. If there is a multi-step approach we first apply the selection method in step i), then step ii), and so forth.

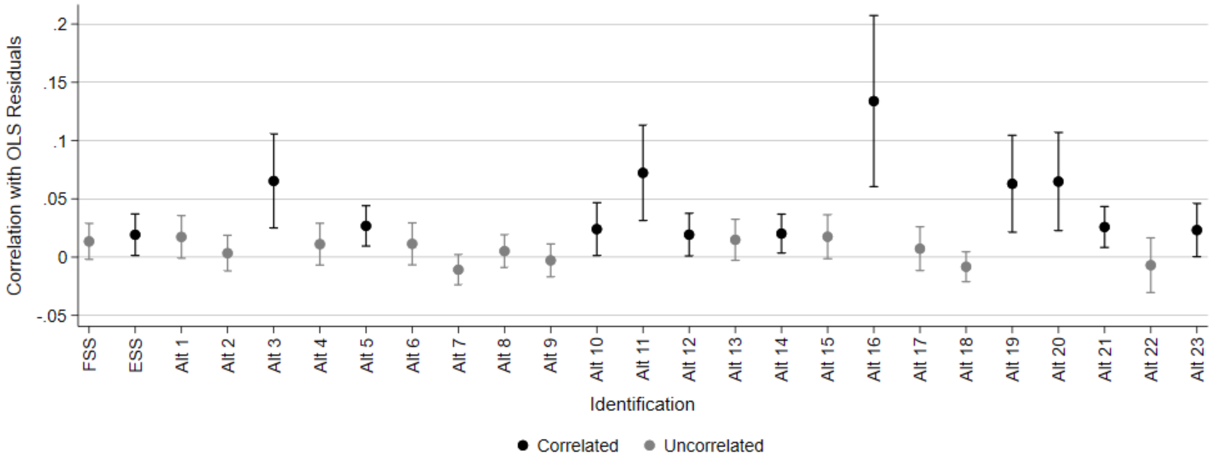
As an example, consider the sampling probability for the Toronto and Montreal transmitters, based on alternative 1. Here, we rank the activation of transmitters by months, for our complete set of installations that ranges over 204 months. The Toronto and Montreal transmitters were both installed in September 1952—the first two transmitters built. Based on the formula for alternative 1, we assign the probability of activating either transmitter as $\Pr = 1 - \frac{0-0}{204-0} + 1/2 = 1.5$, where both the duration and minimum duration are 0, while the maximum duration is 204 for our sample of transmitters. Only two transmitters were activated in 1952, so $s = 2$ here. We truncate probabilities to one for any value greater than one. Here, we assume a random shock according to the distribution $g \sim Normal(\Pr, sd(\Pr))$, which guarantees a non-deterministic distribution of active transmitters in each permutation. We repeat this randomization process 500 times for every post-treatment year of our sample—i.e., separately for 1952, 1953, 1954, 1955, 1956, 1957, and 1958. We derive our final measure of expected signal strength by averaging across all permutations at the census subdivision (CSD) level, exactly as described in Section 3, and use CSD-level population data as weights to aggregate from CSDs to electoral districts. The result is $\mu^A_{d,t}$, an average measure of expected signal strength for district d in year t, based on alternative A.

In most alternatives, we model the probability of a transmitter being activated as a concave function of duration (alt 1), years (alt 6), distance (alt 13), or population, although we depart from this functional form in some specifications. In alternative 3 and 11, we assign every transmitter the same probability of being activated, calculated as a fraction of transmitters activated in year t. In alternative 2, we use a linear probability model to obtain predicted values of the probability of activation. In alternatives 8 and 9, we use a lasso model selection algorithm. To increase concavity and assign transmitters activated late a low probability, we calculate the probability as $1/\text{Duration since first opened}$ in alternative 7, $1/\text{Population rank}$ in alternative 17, and take the squared probability as an input in alternative 20. We do the opposite and decrease concavity, and assign later opened transmitters a higher probability in alternative 19, where we take the square-root of the calculated probability. In alternative 23, we model the same underlying probability as in alternative 1, but require that no selected television transmitter serves the same market. That is, if a transmitter with the third highest shock value serves a market that is already served by the transmitter with the second highest shock value, we skip this tower and proceed to the next highest shock value until the number of selected transmitters equals the true number in year t.

B.1.2 Evaluating the Alternative Shock Distributions

In this subsection, we evaluate the 23 alternative shock methods based on criteria (2) and (3), as described in Section B.1. We also evaluate free-space signal strength and our main measure of expected signal strength used throughout the main body of the paper, based on these two criteria. Alternatives ($\mu^A_{d,t}$) that satisfy *both* criteria are then used in place of $\mu_{d,t}$ in our main equation (3) to obtain unbiased estimates of our treatment effect (β). Finally, we use the set of alternatives that satisfy both criteria to assess the stability of our estimated treatment effect, and obtain an upper and lower bound on television's impact on voter turnout based on these valid alternatives.

Figure B.1: Correlation of Expected Signal Strength with Residuals



Notes: This figure reports regression estimates of the residuals from the simple OLS on the free-space method (FSS), our expected signal strength in the paper (ESS), and the 23 alternatives from Table B.1. Colors denote whether the point estimate is significantly different from zero at the 5% level.

Criteria 2: Correlation of Expected Signal Strength with Residuals We begin by testing which alternative measures of expected signal strength correlate with the residuals from the following OLS regression:

$$Y_{d,t} = \alpha_d + \alpha_t + \beta signal_{d,t} + \epsilon_{d,t}.$$

For each alternative A, we separately regress the predicted residuals on expected signal strength:

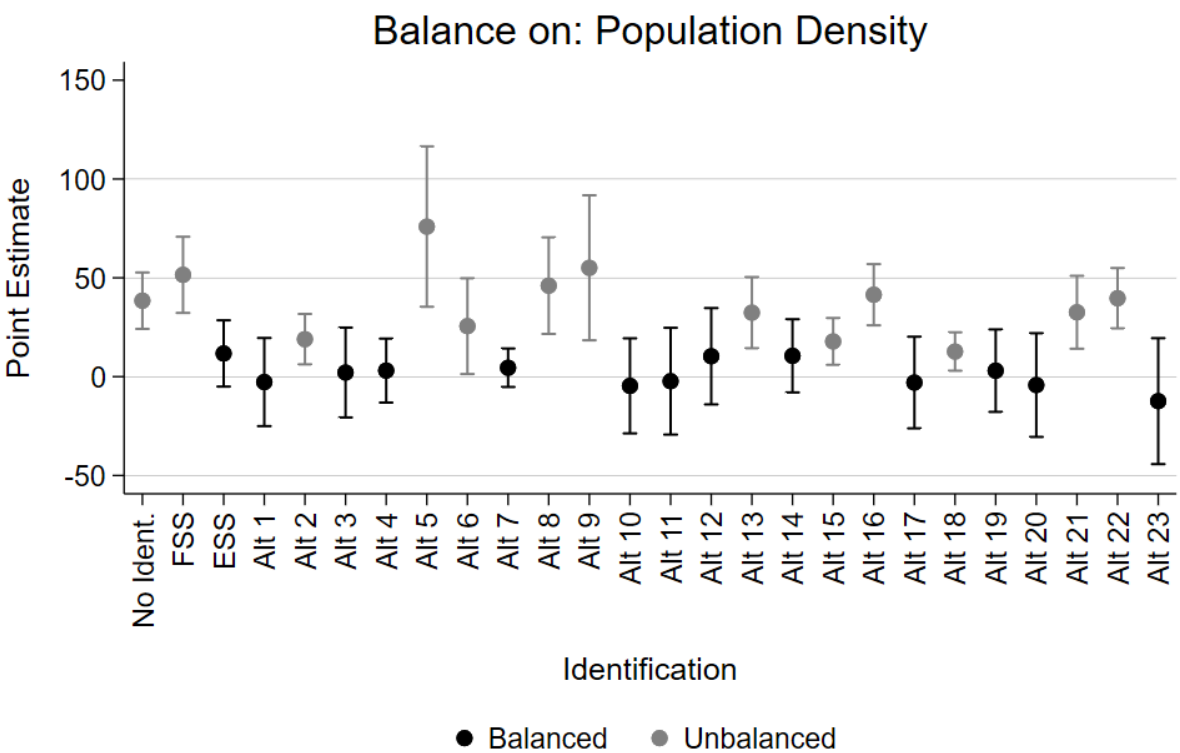
$$\hat{\epsilon}_{d,t} = \alpha_d + \alpha_t + \eta^A \mu^A_{d,t},$$

and plot the obtained coefficients $\hat{\eta}^A$ in Figure B.1.

We begin by showing that free-space signal strength (FSS) is uncorrelated with the OLS residuals, and thus does not satisfy criteria (2). To the contrary, our main measure of expected signal strength (ESS) is correlated with the OLS residuals and satisfies the necessary criteria. Moreover, 11 of the proposed alternatives satisfy criteria (2) at the 5 percent level, as indicated by the point estimates color-coded black in Figure B.1. The estimated correlation is especially large for alternative 16, where the probability of a tower being activated is based solely on population. Here the channel is clear: actual signal strength is highlight correlated with population density, since transmitters were strategically built in the vicinity of large population centers. Hence, we can create an expected network that explicitly models population density that is, almost by construction, correlated with the residuals.

Criteria 3: Correlation of Signal Strength with Population Density Next, we test which alternative measures of expected signal strength satisfy the conditional exogeneity of actual signal

Figure B.2: Correlation of Signal Strength with Population Density in 1931



Notes: This figure reports regression estimates from the regression $Pop_{d,1931} = \beta^A signal_{d,1953} + \eta^A \mu^A_{d,1953} + \epsilon_d$, using the 23 alternatives from Table B.1. Colors denote whether the point estimate is not statistically different from zero at the 5% level.

strength with respect to initial population density—a primary determinant of the timing of treatment (Figure 1). In Figure B.2, we plot the point estimates β^A from the regression:

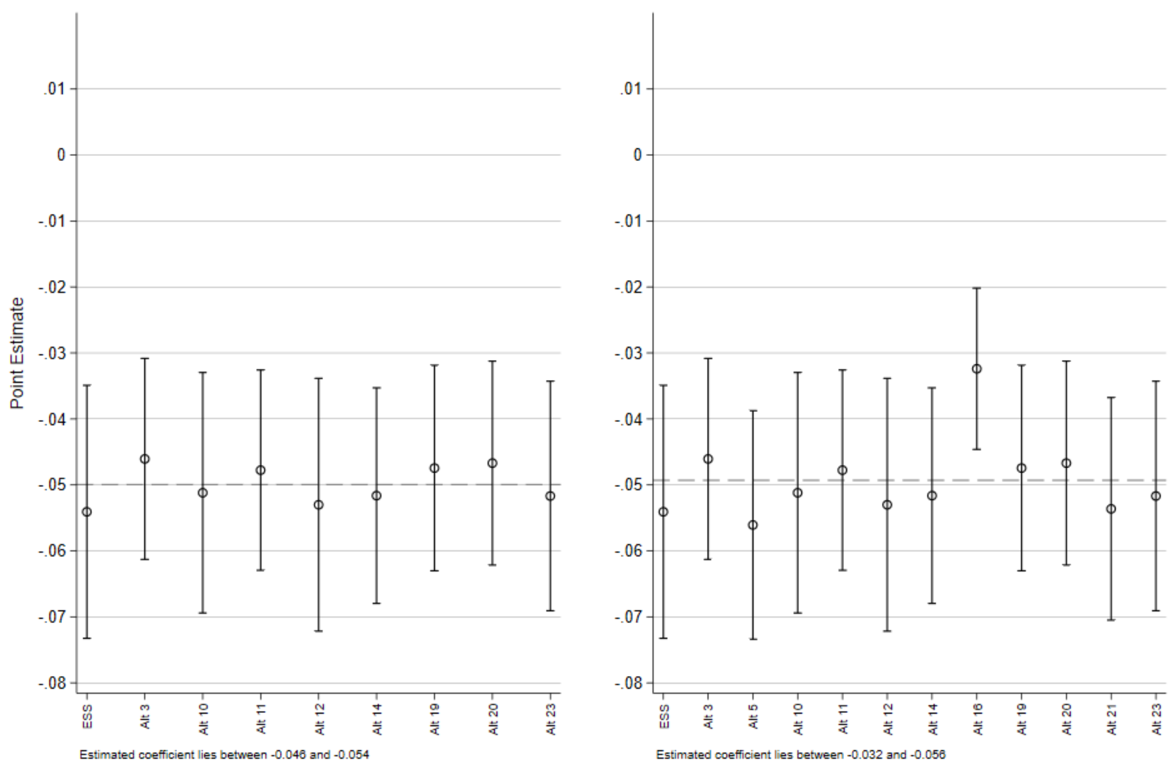
$$Pop_{d,1931} = \beta^A signal_{d,1953} + \eta^A \mu^A_{d,1953} + \epsilon_d.$$

By design, this specification is a test of whether recenetering a regression estimate of *actual* signal strength around an expected signal strength alternative A balances our sample in the cross-section.

We begin by noting that the first three reported estimates are analogous to what we report in Figure 1 of the paper—neither the simple OLS nor the free-space control variable balance our treatment design, yet our main measure of expected signal strength does. Moreover, 12 of the proposed alternatives balance population density at the 5 percent level and thus satisfy criteria (3), as indicated by the point estimates color-coded black in Figure B.2.

Stability of the Treatment Effect Here, we use the set of alternatives that satisfy both criteria to assess the stability of our estimated treatment effect, and obtain an upper and lower bound on

Figure B.3: Identified β^A on Signal Strength



Notes: This figure reports regression estimates from the regression $Y_{d,t} = \alpha_d + \alpha_t + \beta^A signal_{d,t} + \eta^A \mu^A_{d,t} + \epsilon_{d,t}$, based on the alternatives shock distributions described in Table B.1. We benchmark these estimates against ESS, our main measure of expected signal strength. The left panel plots the 8 alternatives that satisfy the necessary criteria (2) and (3), as described in Section B.1. The right panel adds alternatives 5, 16 and 21, which only pass the criteria (2) but not (3). The dashed line indicates the average point estimate across these specified alternatives.

television's impact on voter turnout based on these valid alternatives. In particular, alternatives 3, 10, 11, 12, 14, 19, 20, 23 satisfy criteria (2) and (3) from Section B.1, and thus yield unbiased and consistent estimates of our treatment effect.

The point estimates in the left panel of Figure B.3 are remarkably stable, despite the differences in the modeled shock distribution. For example, we model the probability as a concave function of a transmitter's duration for the ESS estimate, as well alternatives 12, 19 and 20. Instead, alternative 14 is modeled as a function of distance to Toronto, and alternatives 3 and 11 are simply random. Plugging these probabilities into a normal or binomial distribution does not alter the point estimate. The estimated coefficient for β is consistently estimated to lie within the interval $[-0.046, -0.054]$ and significantly different from zero in each instance.

Next, we relax the assumption that an alternative shock distribution must balance our treatment in the cross-section, since our main estimating equation (3) includes district fixed effects that will balance any cross-sectional differences between treatment and control districts. Relaxing this assumption means that we also consider alternatives that only satisfy criteria (2) but not (3), thus extending consideration to 11 different alternatives. The right panel of Figure B.3 plots the point estimates for this less stringent set of alternatives. Once again, the point estimates are remarkably stable, although the bounds do change slightly to $[-0.032, -0.056]$, since the activation of transmitters based on population density alone (alt 16) yields a smaller point estimate. Nevertheless, this point estimate is tightly estimated and significantly different from zero.

B.2 Concluding Remarks

We believe that the evidence in this section provides (i) strong support for our research design and (ii) a clear indication that our estimates are not sensitive to changes in modeling signal strength. We adopt the approach of Borusyak and Hull (2023) in our own context, laying out a set of criteria that a candidate measure of expected signal strength must satisfy for a valid research design. We show that our preferred measure of expected signal strength not only satisfies these criteria, but that many of the alternatives do as well. Importantly, we benchmark our preferred measure against all the alternative measures that satisfy these criteria, and show that the estimated treatment effect that each alternative yields is remarkably similar to our preferred measure. This body of evidence, and the absence of pre-trends that we document in Appendix Section C, provide strong support for our research design.

C Alternative Event-Study Designs and Parallel Trends

Our main estimating equation (3) is a two-way fixed effects model. In Appendix B, we document how the estimated treatment effect is conditionally random, after adjusting our estimate for the expected treatment effect. Yet the timing of this treatment varies across electoral districts, which might affect the interpretation of our parameter of interest—notably in terms of treatment effect heterogeneity and whether the parallel trends assumption holds.[35]

In this section, we document the robustness of our findings to alternative event-study methodologies proposed by Goodman-Bacon (2021) and Sun and Abraham (2021), which both account for treatment effect heterogeneity. Both methods showcase almost perfect parallel trends and are robust negative impact of television on voter turnout. The empirical findings also conform with our expectations: the expansion and adoption of television was almost immediate in Canada, as discussed in Section 2, and so there is little reason to believe that our treatment effect should be growing over time. Somewhere between 75 percent Peers (1979) and 90 percent (Cole, 2002) of Canadians had access to television by our second observed treatment period—the 1957 federal election. Nevertheless, we proceed to show that the assumptions of parallel trends and homogeneous treatment effects hold in our context.[36]

Motivating Evidence of Parallel Trends We plot our main event study in Panel (a) of Figure C.1.[37] Here, we rely on a standard trimmed event-study plot, without and with covariates. In Panel (b), we reproduce this plot but add the never-treated from the trimmed sample as a comparison group in the last period before treatment.

The inclusion of the never-treated serves two purposes. First, it enables more efficient estimation of the leads and lags, thus reducing standard errors. Second, it changes the control groups and thus allow us to gauge the effect of different treatment-control comparisons. While panel (a) only compares early- to late-treated districts, the right panel adds the comparison of early-control and late-control. If the inclusion of the never-treated significantly changes either the leads or lags, then it would provide evidence of treatment effect heterogeneity across groups (Sun and Abraham, 2021). The absence of a change in estimates across panel (a) and (b) provides evidence in favor our empirical design.

C.1 Decomposition of Treatment Cohorts

Our panel of electoral districts includes three potential treatment cohorts, based on the timing of Canadian federal elections: 1953, 1957 or 1958. This implies districts receive treatment at different times and that the same district may serve as a control district and a treated district in

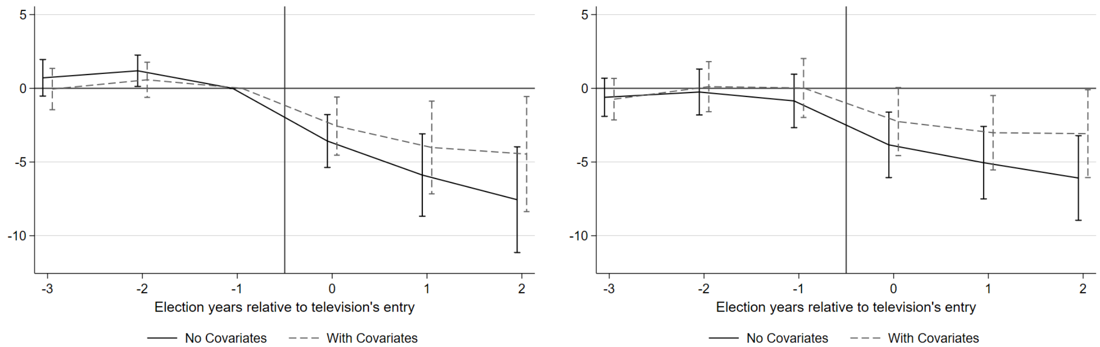
[35] These concerns reflect the burgeoning literature on the interpretability of difference-in-differences and event-study designs. The main insights that we have in mind here come from de Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Sun and Abraham (2021); Borusyak et al. (2024) and more.

[36] Throughout this section, we discretize our treatment variable to equal one for signal strength values greater than 50 dbμV/m, as we have done elsewhere in the main body of the paper for event study analyses.

[37] This plot is the same as Figure 3 in the main body of the paper.

Figure C.1: Event-Study Plots

(a) Without never-treated **(b)** Including never-treated



Notes: This figure reports event-study estimates from excluding federal election districts that never get TV in our sample (left) and including them at period '-1' (right). Both figures provide evidence in favor of parallel trends and support our research design. Intervals reflect 95 percent confidence.

our difference-in-differences framework, depending on the timing of treatment. Recent research shows that, in a similar framework to ours, the staggered introduction of treatment can put negative weights on cohort treatment effects (de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024). In principle, this could switch the sign of the estimate if the treatment effect grows over time (Goodman-Bacon, 2021).

We thus proceed by decomposing our estimate into cohort treatment effects, as suggested by Goodman-Bacon (2021), to assess the weights associated with each treatment comparison. Panel (a) of Figure C.2 plots our findings from this decomposition, where we color-code cohort treatment effects relative to never-treated districts in grey, and early-treated versus late-treated cohort comparisons in black. The dashed horizontal line represents the average treatment effect on the treated (-2.046, s.e. 0.537), which we estimate using the Goodman-Bacon (2021) decomposition.

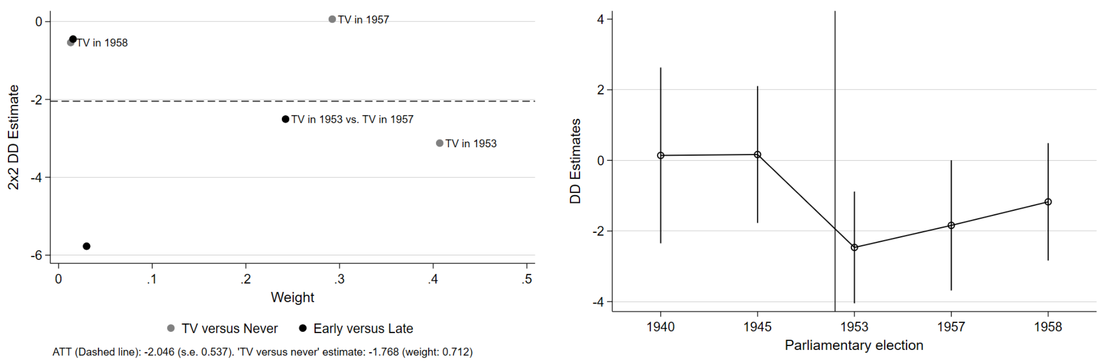
The largest weights are for the 1953 cohort—the earliest treatment cohort. We estimate a treatment effect of -3.122 for the 1953 cohort relative to the never-treated, and -2.510 relative to the 1957 cohort—the second treatment cohort. In total, these two treatment effects make up 65 percent of the variation in the average treatment on the treated. If we abstract from early-late comparisons and focus only on comparison of the treated versus never-treated, we find a slightly smaller coefficient of -1.768. In this instance, the 1953 cohort weights make up 71 percent of estimated treatment effect alone, indicating a valid research design.

In Panel (b) of Figure C.2, we make a similar point in a different way. We abstract from multiple treatment periods—focusing on the 1953 treatment cohort—in a simple difference-in-differences setting, where we document near-perfect evidence of parallel trends prior to television's arrival, and a dynamic effect similar to our event-study estimates in Figure C.1.

Next, we compare Sun and Abraham's (2021) interaction weighted (IW) estimator to our traditional trimming estimator without and with our baseline set of covariates. The IW Estimator

Figure C.2: Goodman-Bacon Decomposition

(a) Decomposition　　**(b)** Pre-trends for 1953 treatment cohort



Notes: The left figure reports the weights and point estimates associated to the three treatment groups 1953, 1957, and 1958. The right figure plots the leads and lags for the 1953 treatment cohort compared to the never treated units. Intervals reflect 95 percent confidence.

accounts for treatment effect heterogeneity by weighting treatment cohort effects by their sample shares. Thus, comparing these estimates to the traditional trimming estimator, allows us to draw inference about treatment effect homogeneity (Sun and Abraham, 2021, Proposition 4, Equation 19). Figure C.3 plots an event study, and includes estimates from (i) Sun and Abraham's (2021) IW estimator, (ii) a trimming estimator without covariates and (iii) a trimming estimator with covariates. All three estimators produce no evidence of a concerning pre-trend, suggesting that in terms of our outcome—voter turnout—electoral districts exhibit parallel trends prior to treatment. All estimators also estimate the same immediate treatment effect when television is introduced (period 0). This is especially true when comparing the IW estimator to the trimming estimator with controls, where we observe almost no difference in treatment effects over time.

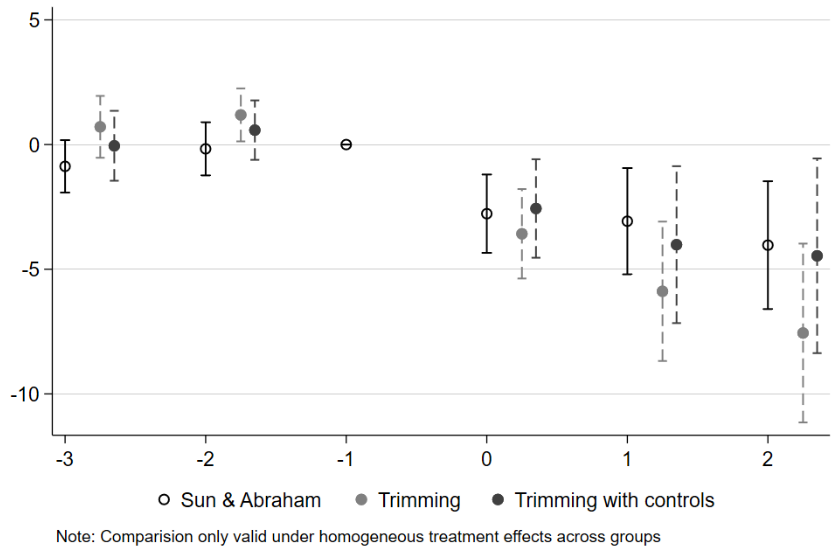
In summary, we believe that our research design is valid. The IW estimator provides no evidence of treatment effect heterogeneity across time, and the weighted average of cohort treatment effects that we estimate at baseline is largely derived from the early adopters of television. Negative weights are also unlikely to affect our estimator for two reasons: the rapid expansion and uptake of television suggests there is little reason to believe that our treatment effect should be growing over time, and this intuition is validated by the decomposition of our estimate into weights, where our treatment effect is mainly derived from a clean comparison of the treated to the never-treated cohort (Goodman-Bacon, 2021).

C.2 Extended Panel Event-Study Design

Our sample period is defined by the one-station policy that remained in effect between 1952-1958, with federal elections—and thus treatment cohorts—occurring in 1953, 1957 and 1958.[38] At the

[38]Refer to Section 2 for details of the policy.

Figure C.3: Event Study with Sun and Abraham's (2021) IW Estimator



Notes: This figure reports Sun and Abraham's (2021) interaction weighted (IW) estimator and includes a comparison to the trimming estimator without and with our baseline set of covariates. The outcome is voter turnout, while treatment is a discrete variable equal to one if television signal strength is greater than 50 dbμV/m. These estimates come from the 1935-1958 panel of electoral districts. Intervals reflect 95 percent confidence.

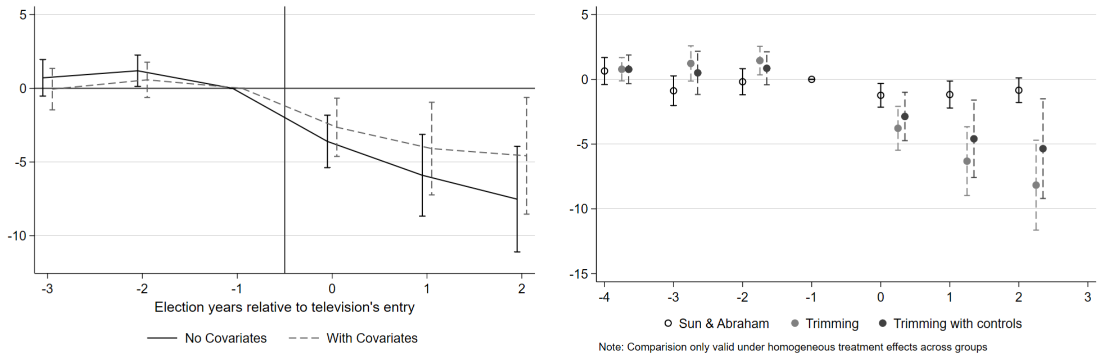
time of the 1953 election, 98 out of 263 electoral districts receive treatment (i.e., television). By the 1957 election, 83 additional districts are treated and finally 6 more in 1958. Yet, the need to truncate the data in 1958 implies that the last lag of our event-study specification is only estimated from the first 98 electoral districts.

In this section, we extend our panel to 1968, which includes four additional election years: 1962, 1963, 1965 and 1968. We fix each district to its 1958 treatment status, thus allowing us to observe *all* cohorts for three lags after treatment—something that is not possible for the 1957 and 1958 treatment cohorts in our baseline sample. We can thus increase the precision of our estimates with this sample extension. Panel (a) of Figure C.4 plots an event study for this extended panel with added precision in the pre- and post-period estimates, relative to Figure C.1. In panel (b), we again observe no differential pre-trends prior to treatment and a significant post-treatment fall in voter turnout after the arrival of television.

The extended panel also allows us to trace the effects of the one-station policy to later elections. Here, we consider two alternative but related estimation strategies: (i) hold the 1958 treatment status fixed ("one-station policy treatment") and (ii) use the complete set of treatments after 1958, despite the fact that any subsequent transmitter installations were the by-product of endogenous market forces between 1959-1968. In Panel (a) of Figure C.5, we compare strategies (i) to (ii), noting that when including the full set of treatments in (ii), the point estimates remain remarkably stable over time within a range of -0.037 and -0.030, and are precisely estimated. This result suggests

Figure C.4: Extended Panel Analysis

(a) Baseline Event-Study Design   **(b)** Sun and Abraham's (2021) IW-Estimator Design



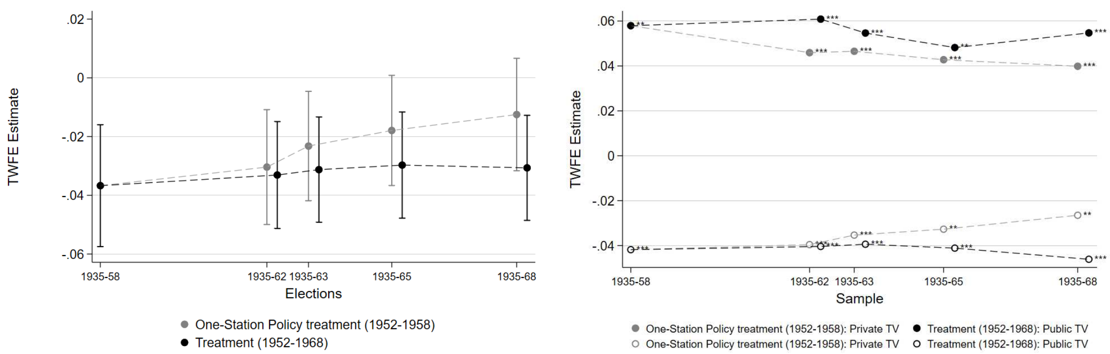
Notes: This figure plots Sun and Abraham's (2021) interaction weighted (IW) estimator and includes a comparison to the trimming estimator without and with our baseline set of covariates. The outcome is voter turnout, while treatment is a discrete variable equal to one if television signal strength is greater than 50 dbμV/m. Importantly, we fix treatment according to the 1958 treatment status, but estimate the reported effects from the extended 1935-1968 panel, thus allowing us to observe three periods before and after treatment for the later treatment cohorts. Intervals reflect 95 percent confidence.

that the one-station policy continued to affect turnout in later elections in a similar way. Moreover, the fraction of never-treated observations decrease to only 12 percent in (ii), as a significant fraction from of the never-treated in (i) become not-yet-treated observations instead. Altogether, this provides evidence against heterogeneous treatment effects across cohorts.

Finally, in panel (b) of Figure C.5, we report the point estimates separately by public and private television to test whether a crowding out is indeed affecting the estimates in Figure C.5. Again, the point estimates of the one-station policy is lower, yet remain significant over time. Especially the impact of private TV remains stable in a range 0.040-0.046.

Figure C.5: Coefficients in the extended panel

(a) Television Treatment **(b)** Public v Private Television Treatment



Notes: Figure (a) reports estimates from our main specification with covariates, based on the two alternative estimation strategies. Figure (b) plots the equivalent estimates for public and private television. Intervals reflect 95 percent confidence. Standard errors are clustered at the electoral district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Data Description and Sources

Federal electoral district maps: We digitized federal electoral district (FED) maps for all years relevant to our extended sample period, 1935-1968. Over this sample period, FED boundaries were redrawn three times, so we collected and digitized the following set of FED boundaries:

- The Representation Order of 1933-1947
- The Representation Order of 1947-1952
- The Representation Order of 1952-1966
- The Representation Order of 1966-1976

For each Representation Order, we digitize FED boundaries from scanned maps using ArcGIS. We crosswalk these maps using the procedure outlined in Eckert et al. (2020), thus giving us a consistent spatial unit of observation for the sample period. We use the 1952 distribution as our "reference map" because this is the same year television arrives, and all other distributions as our "reporting maps," which we re-aggregate to the reference map.

Source: Scanned maps of the 1933 Representation Order were acquired from two sources, the University of Toronto's *Map and Data Library* and the University of Alberta's *Digital Repository and Data Services*. Scanned maps of the 1947 Representation Order are from Western University's *Map and Data Centre* as well as University of Alberta's *Digital Repository and Data Services*. Scanned maps for both the 1952 and 1966 Representation Orders were acquired directly from the *Library and Archives of Canada*.

Television Signal Strength: We require a variety of information on television transmitters before we can proceed to estimating signal strength. In addition to the installation date of a transmitter, we require information on the height and service power of each transmitter, as well as their latitude and longitude coordinates. We piece these data together from three archival documents located at the *Library and Archives Canada*.

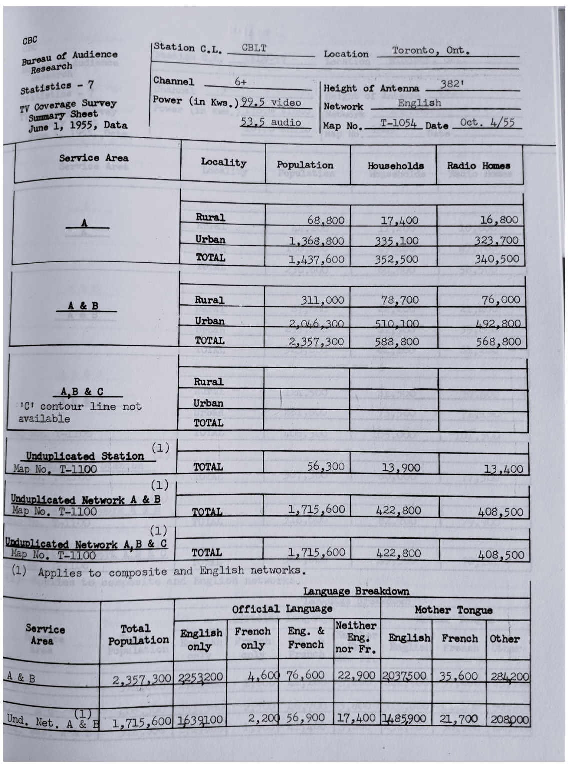
1. *List of Television Stations in Canada*: This set of records was produced by the Stations Relations division of the CBC and is the starting point of our data. It documents basic information about every television transmitter installed between 1952-1969, including station call signs, public or private ownership, service power of the transmitter and its opening date, among other details. This gives us information on all 323 transmitters and rebroadcasters installed by 1969.

2. *Television Coverage in Canada*: This set of records was produced by the Statistics Division of the Bureau of Audience Research at the CBC, and includes additional details for television transmitters, including antenna height, channel number and network information. Panel (a) of Figure D.1 provides an example from this set of records for CBLT Toronto.

3. *List of Broadcasting Stations in Canada*: This set of records was produced by the Canadian Radio-Television Commission, and includes information on latitude and longitude coordinates for radio broadcast stations, among other details. Especially in the early days of television, radio and television signals were broadcast from the same transmitters, allowing us to deduce the location coordinates for each television station. Panel (b) of Figure D.1 provides an example from this set of records, where CBC station CBL (radio) was broadcast from the same transmitter as CBC station CBLT (television).

Figure D.1: Example Archival Records

(a) CBLT Transmitter Details



CBC
Bureau of Audience Research
Statistics - 7
TV Coverage Survey
Summary Sheet
June 1, 1955, Data

Station C.L. CBLT Location Toronto, Ont.
Channel 6+ Height of Antenna 382'
Power (in Kws.) 99.5 video Network English
53.5 audio Map No. T-1054 Date Oct. 4/55

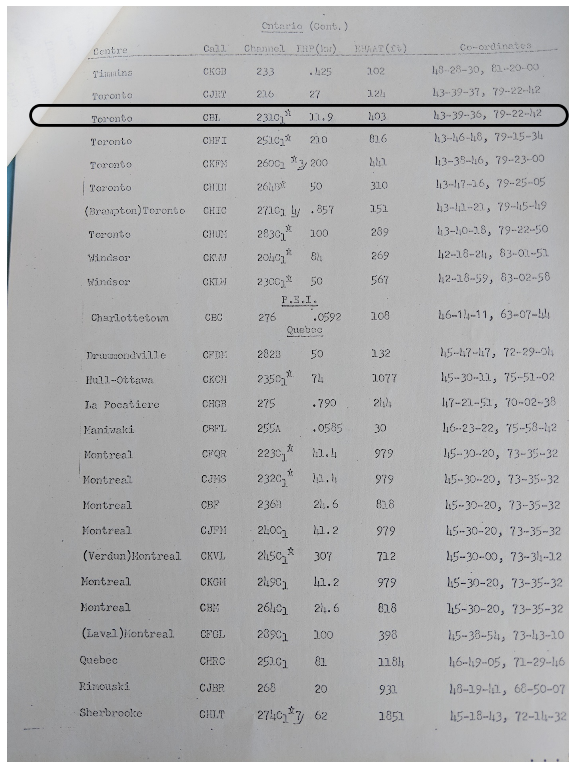
Service Area	Locality	Population	Households	Radio Homes
A	Rural	68,800	17,400	16,800
	Urban	1,368,800	335,100	323,700
	TOTAL	1,437,600	352,500	340,500
A & B	Rural	311,000	78,700	76,000
	Urban	2,046,300	510,100	492,800
	TOTAL	2,357,300	588,800	568,800
A,B & C 'C' contour line not available	Rural			
	Urban			
	TOTAL			
Unduplicated Station (1) Map No. T-1100	TOTAL	56,300	13,900	13,400
Unduplicated Network A & B (1) Map No. T-1100	TOTAL	1,715,600	422,800	408,500
Unduplicated Network A,B & C (1) Map No. T-1100	TOTAL	1,715,600	422,800	408,500

(1) Applies to composite and English networks.

Language Breakdown

Service Area	Total Population	Official Language				Mother Tongue		
		English only	French only	Eng. & French	Neither Eng. nor Fr.	English	French	Other
A & B	2,357,300	2253200	4,600	76,600	22,900	2037500	35,600	284200
Und. Net. A & B (1)	1,715,600	1639100	2,200	56,900	17,400	1485900	21,700	208000

(b) CBLT Transmitter Location



Ontario (Cont.)

Centre	Call	Channel	ERP(kw)	EHAAT(ft)	Co-ordinates
Timmins	CKGB	233	.425	102	48-28-30, 81-20-00
Toronto	CJRT	216	27	124	43-39-37, 79-22-42
Toronto	CBL	$231C_1$*	11.9	403	43-39-36, 79-22-42
Toronto	CHFI	$251C_1$*	210	816	43-46-48, 79-15-34
Toronto	CKFM	$260C_1$ *3/	200	441	43-38-46, 79-23-00
Toronto	CHIN	264B*	50	310	43-47-16, 79-25-05
(Brampton)Toronto	CHIC	$271C_1$ 4/	.857	151	43-41-21, 79-45-49
Toronto	CHUM	$283C_1$*	100	289	43-40-18, 79-22-50
Windsor	CKWW	$204C_1$*	84	269	42-18-24, 83-01-51
Windsor	CKLW	$230C_1$*	50	567	42-18-59, 83-02-58
P.E.I.					
Charlottetown	CBC	276	.0592	108	46-14-11, 63-07-44
Quebec					
Drummondville	CFDM	282B	50	132	45-47-47, 72-29-04
Hull-Ottawa	CKCH	$235C_1$*	74	1077	45-30-11, 75-51-02
La Pocatiere	CHGB	275	.790	244	47-21-51, 70-02-38
Maniwaki	CBFL	255A	.0585	30	46-23-22, 75-58-42
Montreal	CFQR	$223C_1$*	41.4	979	45-30-20, 73-35-32
Montreal	CJMS	$232C_1$*	41.4	979	45-30-20, 73-35-32
Montreal	CBF	236B	24.6	818	45-30-20, 73-35-32
Montreal	CJFM	$240C_1$	41.2	979	45-30-20, 73-35-32
(Verdun)Montreal	CKVL	$245C_1$*	307	712	45-30-00, 73-34-12
Montreal	CKGM	$249C_1$	41.2	979	45-30-20, 73-35-32
Montreal	CBM	$264C_1$	24.6	818	45-30-20, 73-35-32
(Laval)Montreal	CFGL	$289C_1$	100	398	45-38-54, 73-43-10
Quebec	CHRC	$251C_1$	81	1184	46-49-05, 71-29-46
Rimouski	CJBR	268	20	931	48-19-41, 68-50-07
Sherbrooke	CHLT	$274C_1$*7/	62	1851	45-18-43, 72-14-32

Notes: These example documents provide insight into how we piece together the necessary information for each television station in our data. Panel (a) includes information for CBLT Toronto, including the service power, antenna height and more. Panel (b) includes information about CBL—the radio station affiliate of CBLT—with latitude and longitude coordinates for the transmitter that broadcast CBL's radio signal and CBLT's television signal. Together with the *List of Television Stations in Canada* document, these two documents provide the information needed to estimate signal strength with the Irregular Terrain Model.

We use the Irregular Terrain Model (ITM) to estimate the attenuation of signal strength across space, based on the timing and location of television transmitter installation. The ITM approach takes into account the elevation profile between a transmitter and its surrounding region, adjusting estimates for any topographic interruption of a signal. Television signal strength is thus an outcome of a transmitter's features, net of topographic interruptions. We use CloudRF to make these ITM estimates, a cloud-based service for modeling signal propagation across space.

Our extended sample runs until 1968, which includes the following election years after television's arrival: 1953, 1957, 1958, 1962, 1963, 1965 and 1968. Based on our ITM estimates and the timing of a transmitter's installation, we can determine the spatial coverage of television signal strength in any given election year. We map these data onto electoral districts using the digitized maps described above to construct an average measure of district signal strength.

Source: The *List of Television Stations in Canada* is available from the Library and Archives Canada as a standalone file, with reference number RG41-B-II-2, Volume number: 590, File number: 236, File part: 1. Whereas both *Television Coverage in Canada* and *List of Broadcasting Stations in Canada* come from a series of textual records titled *Canadian Broadcasting Corporation (C.B.C.)*, with reference number R2551-1-6-E, MG30-E273.

Aggregating Signal Strength to Electoral Districts: An average measure of signal strength can introduce measurement error for large districts with few people living across large swathes of land. Districts of this type are common in a large country like Canada, where outside of major cities the size of a district is quite large. We overcome this aggregation problem using a population-weighted method, where we first aggregate our ITM estimates to the smallest available statistical area in Canada: the census subdivision (CSD). We match 1951 census population data to these CSDs to use as weights when aggregating from CSDs to electoral districts. We successfully match 1951 population data to 92.4 percent of CSDs, and supplant missing values with the last available year, starting with 1941, then 1931 if 1941 is not available, and so on. This procedure guarantees that even in large electoral districts we obtain accurate estimates of the signal strength received by the electorate, as densely populated CSDs are up-weighted in the aggregation, while sparsely populated CSDs are down-weighted. This procedure gives us a measure of television signal strength at the electoral district level, which varies across election years in accordance with the building of new television transmitters over our sample period.

Because television markets do not necessarily overlap with electoral districts, some districts are assigned a value of signal strength well below what any viewer would deem of satisfactory quality, and in some instances simply unwatchable. At baseline, we apply a minimum threshold for a district's average signal strength of 50 dbμV/m. This threshold is based on the Government of Canada's minimum requirement of 47 dbμV/m for a Grade B service contour, which by definition is a signal level the Government of Canada deems "to be adequate, in the absence of man-made noise or interference from other stations, to provide a picture which the median observer would classify as of satisfactory quality." (ISED, 2016, p. 12) With this transformation, signal strength increases continuously for values greater than 50 dbμV/m and is set to zero otherwise.

Source: The harmonized decennial census population data we match to CSDs are at https://borealisdata.ca/file.xhtml?fileId=277432&version=2.10.

Voter Turnout: We calculate voter turnout as the ratio of total votes cast in electoral district relative to the size of the electorate, for every district d in election year t. We do not include by-elections.

Source: Election Canada's Report of the Chief Electoral Officer, Table 5, Summary of General Election Results by Electoral District.

Political Party Vote Shares: We calculate party vote shares as the votes cast for a given party divided by all votes cast, for each electoral district d in election year t. In our analysis, we report results based on four different shares:

1. Liberal party vote share.
2. Conservative party vote share.
3. Major party vote share; i.e., the aggregate vote share for both the Liberal and Conservative party.
4. Others party vote shares; i.e., the aggregate vote share for all parties excluding the Major parties.

Source: Information on candidates, party affiliation and total votes was scraped from Parliament of Canada's Parlinfo website, https://lop.parl.ca/sites/ParlInfo/default/en_CA/.

Voter Polarization: We calculate voter polarization using a standard polarization index:

$$\sum_p s_p^2(1 - s_p),$$

where s_p is the share of votes for party p. We include the Liberal and Conservative party separately, and otherwise group the remaining share of votes into an Others party. In other words, the polarization index captures the distribution of votes across the only two major parties to ever hold office in Canada—the Liberals and Conservatives—relative to all other contending parties.

Source: Information on candidates, party affiliation and total votes was scraped from Parliament of Canada's Parlinfo website, https://lop.parl.ca/sites/ParlInfo/default/en_CA/.

Votes for Politically Left and Right Parties: We assign political parties as "left" or "right" through a variety of methods. Institutional knowledge of the Canadian political system makes the assignment of some parties non-controversial, particularly the parties that receive most of the votes; e.g., the Liberal party is left-leaning and the Conservative party is right-leaning. For lesser-known fringe parties, we use party websites, and various online sources such as Wikipedia to deduce the political alignment. Any possible measurement error introduced here is assumed to be minimal, since these fringe parties make up a tiny portion of the total votes cast, and typically only have a candidate on the ballot in only a few districts in a given election year.

Source: Information on candidates, party affiliation and total votes was scraped from Parliament of Canada's Parlinfo website, https://lop.parl.ca/sites/ParlInfo/default/en_CA/.

Incumbent Re-election: For every district d, we create an indicator equal to one in election year t if the elected politician also held office in election year $t-1$, and zero otherwise.

Source: Information on candidates, party affiliation and total votes was scraped from Parliament of Canada's Parlinfo website, https://lop.parl.ca/sites/ParlInfo/default/en_CA/.

Political Party Re-election: For every district d, we create an indicator equal to one in election year t if the winning political party also held office in election year $t-1$, and zero otherwise.

Source: Information on candidates, party affiliation and total votes was scraped from Parliament of Canada's Parlinfo website, https://lop.parl.ca/sites/ParlInfo/default/en_CA/.

Speech Localness: We construct three different measures of how members of Parliament (MP) speak in the House of Commons—Canada's lower house of Parliament. All measures are based on the universe of speeches given by MPs over our sample period, and designed to capture the frequency and intensity of how often politicians speak about the local communities they represent.

Our starting point is the Canadian Geographical Names Database, which includes latitude and longitude coordinates for the populated places, administrative areas, as well as various geographic features. Based on the database variable "Generic Category," we keep only names and coordinates associated with populated places; i.e., we collect the name and location coordinates of all places with permanent human settlements in Canada. We create a "dictionary" of these places, and build an algorithm that identifies any populated place mentioned in a speech. The algorithm then calculates the distance between each mentioned place and the district of the politician who mentions the populated place. By design, this gives us the following information that varies by electoral cycle:

(i) The total number of speeches given by an MP;

(ii) The total number of speeches given by an MP where any populated place is mentioned;

(iii) The total number of speeches given by an MP where a populated place within the district they represent is mentioned.

With this information, we construct two extensive margin measures of speech localness:

(1) *Speech locality* is the fraction of speeches given by an MP in an electoral cycle where they mention a populated place within their own district. In other words, the ratio of (iii) over (i).

(2) *Place-based speech locality* is the fraction of place-based speeches given by a politician in an electoral cycle where they mention a populated place within their own district. In other words, the ratio of (iii) over (ii).

With our algorithm, we can also determine the following information that varies at the speech level:

(iv) The number of populated places mentioned in a speech;

(v) The number of populated places within the MPs district mentioned in a speech.

With this information, we construct an intensive margin measure of speech localness:

(3) *Place locality* is the fraction of populated places mentioned in a speech that fall within a given MP's district, and averaged over all of their speeches mentioning a populated place within an electoral cycle. In other words, the ratio of (v) over (iv) for a given MP, and averaged over all of their speeches mentioning a populated place within an electoral cycle.

Source: We obtain the digitized version of House of Common debates from Beelen et al. (2017). The Canadian Geographical Names Database list of populated places is available at https://natural-resources.canada.ca/earth-sciences/geography/download-geographical-names-data/9245.

Parliamentary Vote Dissent: We construct a measure of an MP's willing to vote against their own party, based on roll-call voting records for every vote held in Parliament over our sample period. We define a vote to be against the party line if an MP votes contrary to the majority of their party in a given round of voting. We then aggregate each dissent for each politician across electoral cycles, giving us a proxy measure of political accountability that varies by electoral district and election year. Because this outcome variable includes many zeros, we use the inverse hyperbolic sine transformation.

Source: Roll-call voting records are from Godbout and Høyland (2017).

Population Density: We measure population density as the ratio of an electoral districts total population divided by area. Electoral district population is based on 1931 data from the decennial census.

Source: Election Canada's 1935 Report of the Chief Electoral Officer, Table 5, Summary of General Election Results by Electoral District.

Earnings: We construct a measure of district-level earnings, based on a five percent sample of the 1911 decennial census. The census contains two variables, EARNINGS_AT_CHIEF_OCC and EARNINGS_AT_OTHER_OCC, which are measures of the total amount of money earned by the person being enumerated at their chief occupation and other occupation, respectively. From these, we construct an aggregate measure of total earnings of each enumerated individual in the census, and then calculate the average total earnings at the CSD level, based on census ID variable CCRIUID_CSD_1911. We then intersect our digitized CSD and electoral district maps in ArcGIS to determine the share of area for a CSD that corresponds to an FED, and aggregate our earnings measure up to the electoral district level using the shares as weights. This gives us our final measure of average district-level earnings.

Source: Census of Population, 1911, https://doi.org/10.5683/SP3/MDTWGJ, Borealis, V2.

Average Age: We construct a measure of district-level average age, based on a five percent sample of the 1911 decennial census. The census contains a variable, DERIVED_AGE, which measures the age of the enumerated individual. We calculate the average age at the CSD level, based on census ID variable CCRIUID_CSD_1911. We then intersect our digitized CSD and electoral district maps in ArcGIS to determine the share of area for a CSD that corresponds to an FED, and aggregate our age measure up to the electoral district level using the shares as weights. This gives us our final measure of the average age in a district.

Source: Census of Population, 1911, https://doi.org/10.5683/SP3/MDTWGJ, Borealis, V2.

Literacy Rates: We construct a measure of district-level literacy rates, based on a five percent sample of the 1911 decennial census. The census contains two variables, CAN_READ and CAN_WRITE, which are both indicators equal to 1 if the enumerated individual can read or write, respectively. From these, we construct a literacy variable equal to one if an individual can read or write, and zero otherwise, and then calculate average literacy at the CSD level, based on census ID variable CCRIUID_CSD_1911. This approach yields the percent of literate enumerated individuals. We then intersect our digitized CSD and electoral district maps in ArcGIS to determine the share of area for a CSD that corresponds to an FED, and aggregate our literacy measure up to the electoral district level using the shares as weights. This gives us our final measure of a district's literacy rate.

Source: Census of Population, 1911, https://doi.org/10.5683/SP3/MDTWGJ, Borealis, V2.

Urbanization Rates: We construct a measure of district-level literacy rates, based on a five percent sample of the 1911 decennial census. The census contains a variable, CCRI_URBAN_RURAL_1911, which indicates if the geographic location where an enumerated individual lives is classified as urban or rural. We assign a new variable equal to one if urban, and zero otherwise, and then calculate average urbanization at the CSD level, based on census ID variable CCRIUID_CSD_1911. This approach yields the percent of enumerated individuals living in an urban area. We then intersect our digitized CSD and electoral district maps in ArcGIS to determine the share of area for a CSD that corresponds to an FED, and aggregate our urbanization measure up to the electoral district level using the shares as weights. This gives us our final measure of a district's urbanization rate.

Source: Census of Population, 1911, https://doi.org/10.5683/SP3/MDTWGJ, Borealis, V2.

National Daily Newspapers Circulation Per Capita: We use information on daily newspaper circulations from every edition of the *Canada Year Book* from 1950 to 1960. The Canada-wide circulation of daily English-language newspapers are reported for different years across these editions, which we piece together into a time series that runs from 1947-1959. We also collect annual population data from *Statistics Canada*, and calculate the circulation of daily newspapers per capita as our final measure.

Source: The *Canada Year Books* for the years 1950-1960, and *Statistics Canada's* "Population of Canada and the provinces, annual, 1926-1960" (Table: 36-10-0280-01).

Major Cities Newspaper Circulation Per Capita: We use information on daily newspaper circulations from every edition of the *Canada Year Book* from 1950 to 1960. The total circulation of daily English-language newspapers for 42 major urban centers are reported for different years across these editions, which we piece together into a panel that runs from 1947-1959. A few of the urban centers do not appear in all years, although the majority do. The *Canada Year Books* also report population estimates for the urban centers, based on the most recent decennial census. We collect these population estimates for the census years 1941, 1951 and 1961, and interpolate the data to fill in the gaps for the years between census. Altogether, these data allow us to calculate the circulation of daily newspapers per capita for our panel of 42 major urban centers between 1947-1959.

Source: The *Canada Year Books* for the years 1950-1960.

Local and National Content Records: Content information for our sample period comes from the Royal Commission on Broadcasting's 1957 report. We aggregate the "recorded local" and "local live" values into a single measure of local content, whereas national content corresponds to the values labeled "network." This gives us the percent of air time that is local content and national content for four subcategories: private informational content, private entertainment content, public informational content and public entertainment content. The percent of airtime totals 100 percent for each subcategory. Then, for each subcategory, we take the ratio of local to national content to derive the values reported in the first two columns of the table, for each audience time segment. Finally, the third column is the ratio of the numbers in the first two columns—i.e., the proportion of locally produced content aired on private stations relative to public stations.

Source: Canadian Television and Sound Radio Programmes, Appendix XIV, Royal Commission on Broadcasting, 1957, page 122, "Percent of entertainment-type and information-orientation-type programming originated as network, recorded local and live local by class of station and audience time segment."

Information and Entertainment Content Records: Content information for our sample period comes from the Royal Commission on Broadcasting's 1957 report. Public television values correspond to the table row labeled "Total CBC Stations" and private television values correspond to the table row labeled "Total Private Stations."

Source: Canadian Television and Sound Radio Programmes, Appendix XIV, Royal Commission on Broadcasting, 1957, page 46, "Canadian television and sound radio stations: entertainment-type and information-orientation-type programmes as percentage of total time on the air, January 15-21, 1956".