TELEVISION AND VOTER TURNOUT*

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I use variation across markets in the timing of television's introduction to identify its impact on voter turnout. The estimated effect is significantly negative, accounting for between a quarter and a half of the total decline in turnout since the 1950s. I argue that substitution away from other media with more political coverage provides a plausible mechanism linking television to voting. As evidence for this, I show that the entry of television in a market coincided with sharp drops in consumption of newspapers and radio, and in political knowledge as measured by election surveys. I also show that both the information and turnout effects were largest in off-year congressional elections, which receive extensive coverage in newspapers but little or no coverage on television.

I. INTRODUCTION

Television was first licensed for commercial broadcasting on July 1, 1941. By 1960, 87 percent of American households had television sets, and they were watching an average of five and a half hours per day [Television Bureau of Advertising 2003]. Contemporary observers anticipated a "revolution" in politics, pointing to an "infinite broadening of the democratic process . . . giving all Americans a clearer understanding of trends and issues" [Mickelson 1960], a "new direct and sensitive link between Washington and the people" [Stanton 1962], and "a better medium for truth" [Taft 1951].

What took place in the years after television's introduction was not a broadening of the democratic process, but rather a sharp decline in political participation. Average presidential turnout in both the 1980s and 1990s was lower than in any decade since the 1920s, and outside the South (where a substantial remobilization of Black voters muted the decline) it was lower than in any decade since the 1820s.[1] The decline in turnout is

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1. These turnout figures are based on Rusk [2001]. McDonald and Popkin [2001] offer a more precise calculation of the number of eligible voters beginning in 1948, adding corrections for ineligible felons and eligible voters living overseas. They find that these corrections eliminate the slight negative trend in turnout after 1972 that earlier data showed. The corrections do not eliminate the sharp

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especially striking since many legal barriers to voter registration were dismantled during the same period, and education and income—both positively correlated with the propensity to vote—increased substantially.[2] Numerous books have been written about this decline.[3] It has been indicted as a threat to American democracy and a symptom of broader disengagement of Americans from the lives of their communities [Teixeira 1992; Putnam 2000]. It is, according to one source, "the most important, most familiar, most analyzed, and most conjectured trend in recent American political history" [Rosenstone and Hansen 1993, 57].

In this paper I use plausibly exogenous variation in the timing of television's introduction to show that it significantly reduced voter turnout, accounting for between a quarter and a half of the aggregate decline since midcentury. I also show that television caused sharp drops in consumption of newspapers and radio, that it reduced citizens' knowledge of politics as measured in election surveys, and the effects on both turnout and information were largest in relatively local elections.

These latter facts point toward crowding out of local political information as a possible mechanism linking television and turnout. George and Waldfogel [2005] argue that growth of national media can cause substitution away from local news sources, and document this by showing that in localities where the *New York Times* has expanded in recent years, readership of local newspapers fell among educated readers. They speculate that one result of this crowding out may have been reduced participation in local elections.[4] Television in the 1950s and 1960s was similar to the *New York Times* in that its political coverage was primarily national, and we would expect it to cause similar substitution away from local news. Furthermore, since television was a dramatic improvement in the quality of entertainment available to most households, it may also have reduced the *total* time devoted

drop in turnout between 1960 and 1972, however, and do not change aggregate voting during the period I study.

2. The combination of falling turnout and changing demographics was the basis of an article by Brody [1979]. The legal changes over this period are summarized by Kleppner [1982, pp. 122–123] and Teixeira [1992, pp. 29–30]. Teixeira estimates that based on changing demographics alone, voter turnout should have been 3.9 percent *higher* in 1988 than in 1960.

3. See Kleppner [1982], Teixeira [1992], Piven and Cloward [2000], and Patterson [2003].

4. In an earlier version of the same paper, George and Waldfogel [2002] present evidence that in cities where the *New York Times* grew rapidly, turnout among educated voters fell in off-year but not presidential year elections.

to news consumption. This would reduce turnout in both local and national elections.[5]

To identify the effect of television, I take advantage of three historical facts. The first is that television was introduced to different markets at different times, with the earliest and latest cities separated by more than ten years. Although the timing was far from random, two exogenous events—World War II and the imposition of a licensing freeze between 1948 and 1953 (caused by technical problems with spectrum allocation)—added an important element of idiosyncratic variation. The second is that when television was introduced, it grew rapidly. In many markets, penetration went from 0 to 70 percent in roughly five years, and even in the earliest years the average television household was watching more than four hours per day. The third is that television stations from a given city broadcast over a large area. Even though the earliest television cities were also the largest and wealthiest, their signals reached a heterogeneous group of counties including many that were small and rural. This allows me to eliminate spurious correlation between the timing of television and shocks to voting by controlling for flexible functions of time interacted with county demographics, and by looking at changes in voting within demographically similar groups of counties.

The first set of results quantifies the effect of television on turnout. In panel regressions with county and time-region fixed effects, I find that television reduced voter turnout, and that the effect was significantly larger in off-year congressional elections (when the only races are at the state, district, or local level) than in presidential election years. The result is robust to including fourth-order polynomials in time interacted with observable county characteristics, as well as controls for changes in observable characteristics over time. It also remains when the analysis is performed on subsets of counties with similar characteristics. Overall, I estimate that television reduced off-year turnout in an average county by 2 percent per decade after it was introduced, and that television explains half of the total off-year decline in turnout since the 1950s. The effect on presidential-year turnout is smaller—accounting for roughly a quarter of the total decline—and is not significantly different from zero.

5. For evidence on the link between information and turnout more generally, see the theoretical literature reviewed by Feddersen [2004] and empirical analyses by Gant [1983], Gerber and Green [2000], and Lassen [2005].

The second set of results shows that crowding out of information provides a plausible mechanism linking television and voting. One approach is to document the extent to which television caused substitution away from newspapers and radio. I confirm that television news provided less political information than either of these other media, and that the gap was larger for information about congressional races. I then document the dramatic decline in newspaper and radio use which television caused. A second approach is to look directly at measures of political knowledge. I use data from the 1952 National Election Study to show that respondents in counties with television were less likely to be able to name candidates running in the election, with the strongest and most significant effects for congressional elections. A third approach is to look for an exogenous variable that shifts the amount of information about local congressional races provided by television and ask whether this also changes the intensity of the turnout effect. The variable I consider is the number of congressional districts within a television market. The more districts a market is divided into, the more congressional races are taking place in a given election year, and the less local stations should be able to cover any one race. Although the results for the effect of districts on survey measures of information show no significant effects, the effect on turnout is exactly as predicted: having more districts increases the magnitude of television's effect in off-year congressional elections but does not change the effect in presidential years.

These findings contribute to a growing economics literature on the link between media and voter behavior.[6] DellaVigna and Kaplan [2006] study the impact of introducing the Fox News cable channel and find a positive effect on the vote shares of Republican candidates, as well as evidence of a positive effect on voter turnout. Oberholzer-Gee and Waldfogel [2001] show that minority voter turnout is higher in counties with larger minority populations, and attribute the difference to the ability of minority-targeted media to deliver campaign messages. Stromberg [2004] presents evidence that penetration of radio in the New

6. I will not attempt to review the large political science literature on media and voter behavior. See Graber [2000] for a survey. One study of particular relevance is Prior [2004], who uses data on television in the 1950s to argue that it increased the incumbency advantage. A second is Althaus and Trautman [2004], who use recent cross-sectional data to show that large television markets fragmented over many congressional districts tend to have lower turnout, especially in off-year elections.

Deal period *increased* turnout in gubernatorial elections, an interesting finding that contrasts with the effect of television documented here. One possible explanation is that technological features of radio (for example, the fact that it is possible to read and listen at the same time) meant that it caused less substitution away from newspapers. This is consistent with aggregate trends in newspaper circulation per capita which show a sharp downward trend beginning when television was introduced but no similar trend around radio's introduction. Radio also had more political coverage than television, and may have represented a greater improvement in the availability of information, especially in rural areas.

The remainder of the paper proceeds as follows. The next section discusses the construction of the data. Section III presents background on the history of television's introduction. Section IV discusses the paper's identification strategy and the empirical specification, Section V analyzes the effect of television on turnout, and Section VI looks at informational crowding out as a possible mechanism. Section VII concludes.

II. Data

Data on the availability of television in each U. S. city between 1940 and 1970 were compiled from various issues of *Television Factbook* [Television Digest 1948–1970], a yearly data book on the television industry used by advertisers, equipment manufacturers, station managers, and others. The books profile each station operating in each year, giving its location, signal strength, network affiliation, ownership, and starting date.

To define the geographic region reached by stations in a particular city, I use Designated Market Areas (DMAs), which are the current industry standard. Every county in the United States is assigned to one DMA such that all counties in a given DMA have a majority of their measured viewing hours on stations broadcasting from that market.[7] These definitions are based on viewership as of 2003, rather than in the historical period I am analyzing. However, since the broadcasting strength of stations is

7. In a handful of cases, a county is split across multiple DMAs. (An example is El Dorado county in California. The eastern half of the county is assigned to the Reno DMA, and the western half to the Sacramento DMA.) In these cases, I assign the entire county to the larger of the two DMAs. Dropping these counties does not meaningfully alter the results (see Gentzkow [2005]).

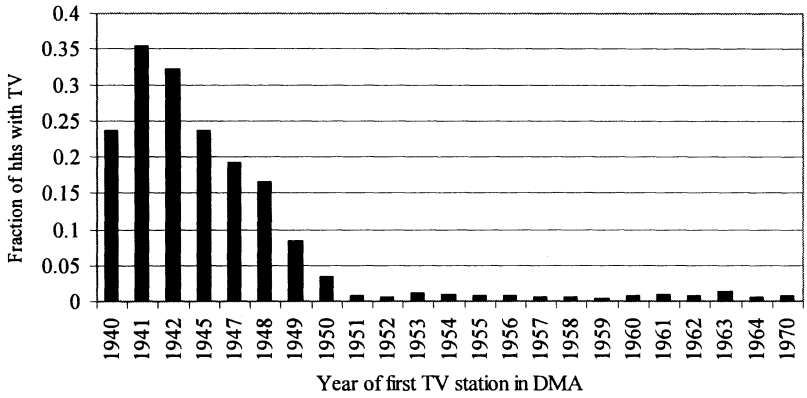

FIGURE I
1950 Television Penetration by First Television Year
Note: Years in which no county received their first station are omitted from the figure.

regulated by the FCC to avoid interference with neighboring markets, the area reached by particular stations has not changed significantly.[8] I take the DMA definitions as a reasonable approximation of the viewing area of stations in the 1950s and 1960s, and calculate the first year each county received television as the first year in which a station in the DMA broadcast for at least four months.[9]

One way to check the validity of the television data is to compare the recorded entry dates to county-level television penetration from the 1950 and 1960 censuses. Figure I shows 1950 penetration by the measured first year of television. The average penetration in DMAs whose first station began broadcasting before 1950 ranges from 8 percent in the 1949 group to over 35 percent in the 1941 group, while the average for post-1950 DMAs never exceeds 1 percent. A similar comparison using 1960 data shows that the handful of DMAs that got television in 1960 or later already had extensive television ownership. These DMAs

8. I have verified this by spot-checking the DMA definitions against coverage maps from the 1960s.

9. In most cases, I use the date that a station began commercial broadcasts, as regulated by the FCC. The exceptions are two stations—KTLA in Los Angeles and WTTG in Washington, DC—that began large-scale experimental broadcasts and subsequently converted to become commercial stations. In these cases, I use the stations' experimental start dates.

are small—often consisting of only one or two counties—and could all receive some television broadcasts from neighboring markets. I therefore omit all counties in DMAs whose first television station began broadcasting in 1960 or later. This applies to 13 out of 205 DMAs, accounting for .7 percent of the U. S. population as of 1950. If these counties are included, the measured effects of television are attenuated somewhat, but remain statistically significant.

I combine the television data with county-level election returns compiled by the Inter-university Consortium for Political and Social Research (ICPSR).[10] The data give total votes as a percentage of the legally eligible electorate, as defined by contemporary citizenship, race, sex, and age criteria.[11] This does not take account of other eligibility criteria based on residency, or status as a convicted felon. Not all counties have data for all years, and I include counties if they have turnout data available for a majority of the years from 1940–1972, leaving a total of 3081 counties.[12] I use as my key dependent measure the number of people casting votes for Congress as a fraction of the eligible electorate.

Figures II and III plot turnout in House elections for the years 1910–1998. Figure II shows turnout in presidential years, and Figure III in nonpresidential years. Although turnout has been volatile over the century, the graphs show a marked decline after 1960 which is steepest in the years 1960–1974 and flattens out significantly thereafter. Because there was substantial remobilization of Black voters in the South during this period, the decline outside the South was even more pronounced.[13]

10. The study is titled "Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840–1972" (ICPSR study number 8611).

11. This is the standard measure of turnout, and is considered superior to the measure with number of *registered* voters in the denominator. According to Prysby [1987, p. 113]: "Calculating turnout as a proportion of registered voters . . . is generally inappropriate in the American context, given the large numbers of people who are eligible to register but fail to do so. In fact, the available research indicates that most nonvoters are not registered."

12. Rusk [2001] provides a detailed discussion of the construction of historical turnout data, including possible errors in the ICPSR data. The results are unchanged if I use a balanced panel only including counties that have data for all years.

13. Three major events that affected turnout are visible in Figures II and III. First, the Nineteenth Amendment was ratified in 1920, extending the franchise to women. This has frequently been cited as a reason for the drop in presidential turnout in the 1920 and 1924 elections as it took several years for voting rates among women to reach near-parity with those of men [Rusk 2001]. Second, there was a sharp drop in turnout during and immediately following World War II. I

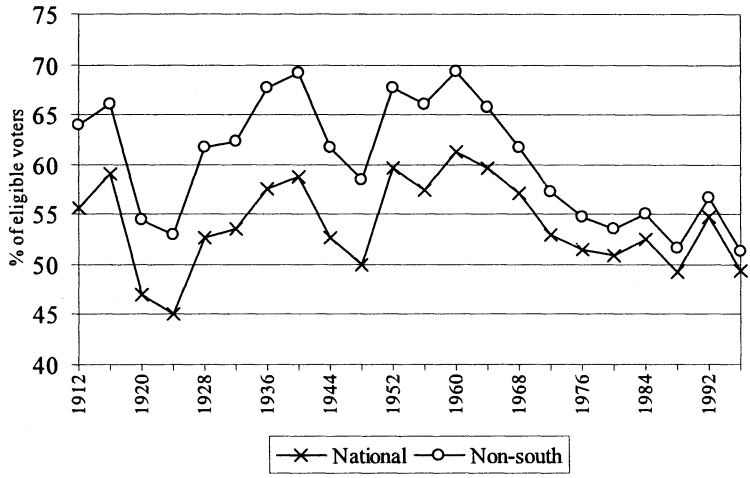

FIGURE II
Congressional Turnout in Presidential Election Years

To assess political knowledge and the use of media for political information, I match the television data to the 1952 National Election Study.[14] This is a nationally representative sample of 1899 voting-age citizens interviewed both before and after the 1952 election. The timing of this election is ideal, because it was the last year of the FCC freeze and so maximizes the idiosyncratic variation in television access. The survey included detailed questions on political attitudes, voting behavior, and demographics, as well as limited information on media use. It also identifies the county of residence of each respondent.

For newspaper circulation, I compile a state-level data set using various issues of *Editor and Publisher Yearbook* [Editor and Publisher 1946–1971], which give the total circulation of daily newspapers for each state in each year. I translate these

have not seen a convincing analysis of what caused this drop, but I would speculate that it could result in part from sharply reduced participation among military personnel stationed overseas. Third, the Twenty-Sixth Amendment passed in 1971 required that all states extend the franchise to 18–21-year-olds, a group that historically has significantly lower turnout rates than older voters. Kleppner [1982, p. 123] estimates that approximately 25 percent of the drop in turnout between 1968 and 1972 can be attributed to the expanding electorate.

14. The study is titled "American National Election Study, 1952" (ICPSR study number 7213).

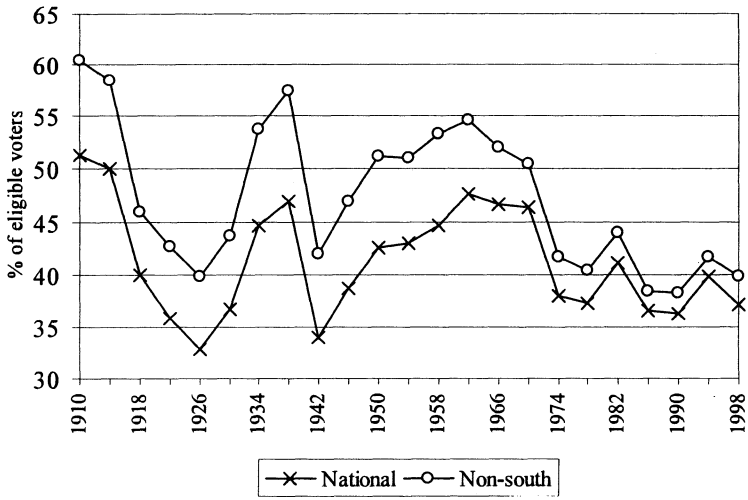

FIGURE III
Congressional Turnout in Nonpresidential Years

into circulation per capita using census population data. County and city-level demographics are obtained from the decennial census, with intercensus years estimated by linear interpolation.

III. The Introduction and Growth of Television

Television technology was already well developed by the late 1930s. The first workable prototypes for television receivers were made in the early 1920s, the first public demonstration took place in 1923, and numerous experimental broadcasts were made in the late 1920s. By 1931, eighteen experimental stations were operating in four cities. The first television sets went on sale in 1938, and by 1939 fourteen companies were offering sets for sale. After several delays, the Federal Communications Commission (FCC) finally licensed television for full-scale commercial broadcasting on July 1, 1941.[15]

Although television seemed poised for rapid growth at this point, two events intervened to delay the process. The first was World War II: less than a year after the FCC authorization, the

15. This section draws primarily on Sterling and Kittross [2001] and Barnouw [1990]. For details on the regulatory process, see also Slotten [2000].

government issued a ban on new television station construction to preserve materials for the war effort. Although existing stations continued to broadcast, the total number of sets in use during the war was less than 20,000. After the war, television grew rapidly. Over 100 new licenses were issued between 1946 and 1948, so that by 1950 half of the country's population was reached by television signals. This growth was again halted, however, by an FCC-imposed freeze on new television licenses in September 1948. The FCC had determined that spectrum allocations did not leave sufficient space between adjacent markets, causing excessive interference. The process of redesigning the spectrum allocation took several years, and it was not until April 1952 that the FCC lifted the freeze and began issuing new licenses.

To justify the empirical analysis that follows, it is important to establish that the diffusion of television was rapid enough that its effects could be felt within a relatively short window of time. One reason is that many aspects of the television industry—from the format of newscasts, to the structure of the networks—had been developed and perfected by the radio industry. Also, the technical and logistical aspects of television had been largely worked out well before the end of the war. A number of technological innovations during the war years, including better cameras and a technology for rebroadcasting cinema film, further improved quality. As Barnouw [1990] writes, the prospects for television's growth after the war were "explosive":

> Electronic assembly lines, freed from production of electronic war materiel, were ready to turn out picture tubes and television sets. Consumers, long confronted by wartime shortages and rationing, had accumulated savings and were ready to buy. Manufacturers of many kinds, ready to switch from armaments back to consumer goods, were eager to advertise [p. 99].

We can look at the pattern of television's growth in a number of different ways. Some evidence was presented in Figure I, which showed substantial diffusion of television ownership by 1950 (13 percent on average in markets that had television). Figure IV shows the time path of diffusion, drawing on data from Nielsen surveys. In the largest counties, 20 percent had televisions by 1950, and 80 percent had televisions by 1955. Data from Nielsen surveys also show that in those households with television, viewership had already surpassed four and a half hours per day by 1950 [Television Bureau of Advertising 2003].

Though the most dramatic period of growth in stations and

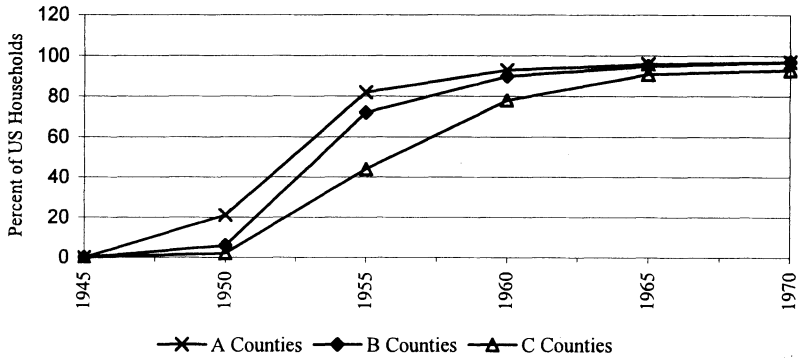

FIGURE IV
Percent of U. S. Households with Television by County Size
Note: Data are from Nielsen Television Index as quoted in Sterling [1984]. "A Counties" are all counties in the 25 largest metropolitan areas. "B Counties" are all counties not in A with populations of over 150,000 or in metropolitan areas.

ownership occurred between 1950 and 1960, growth continued after 1960 on other dimensions. Although the number of households with televisions had plateaued, the number with multiple sets more than tripled between 1960 and 1970 [Sterling 1984, p. 236]. Color television was introduced at the end of the 1950s, and the fraction of television households with color sets increased from less than 1 percent in 1960 to more than 50 percent in 1972 [Television Digest 2001]. The combined effect of these changes is reflected in the number of viewing hours per household, which increased from four and a half hours in 1950, to just over five hours in 1960, to more than six hours by 1975.

While the analysis in this paper does not extend beyond 1970, it is worth examining briefly how television has changed since then. If the scale of television continued to increase, the long-run effects could be even greater than those documented here. On the other hand, if television began providing substantially more political information, some of the effects could actually be reversed. In reality, the former seems more likely than the latter. The time devoted to television viewing continued to increase steadily until the mid-1980s, with daily viewership in 1995 50 percent higher than in 1965 [Robinson and Godbey 1999]. The number of sets per household increased from 1.4 to 2.3 between 1970 and 1995 [Putnam 2000], and both color television and cable saw the majority of their growth after 1970 [Television Digest

2001]. Furthermore, although the supply of news on television has clearly grown, entertainment options have proliferated as well, and there is little evidence that people were getting more political information from television in the late 1990s than they were in 1970.[16] Looking at local news in particular, the evidence suggests that viewership remained constant or even fell.[17]

IV. Identification and Empirical Specification

Although television is widely believed to have large effects on outcomes from politics to crime to education, identifying its causal effect has proved to be difficult. The most common research design uses cross-sectional data with the key right-hand-side variable being hours of television viewing or number of television stations. Such analyses suffer from potentially severe bias caused by correlation of the television measures with unobserved characteristics of individuals or localities. Panel-data designs using variation over time in the number of stations (or more ambitiously the availability or price of cable) could potentially address some of this bias. However, the impact of the marginal station or cable subscriber on aggregate viewing patterns is likely to be very small, making it difficult to detect effects in small samples.

The key innovation of this study is to use panel data from a period in which the variation over time in television's availability was far from marginal. As already described, many counties went from having essentially no television viewing to having 80 percent of households watching an average of 4.5 hours per day in as

16. The fraction of respondents in the National Election Study who said they had watched programs about the presidential campaign on television was lower in all but one year of 1980–2000 than in *any* year between 1960 and 1972. Network evening news viewership was flat from 1970–1980 and then declined by more than 50 percent between 1980 and 2000 [Nielsen Media Research 2003]. Some of the decline in the 1980s may have been offset by growing cable news viewership, but this does not appear to be true of the 1990s, as the fraction of people saying they watch CNN "regularly" or "sometimes" was not substantially higher in 2000 than in 1990 [Pew Research Center 2002].

17. In 1972 and 1996, National Election Study respondents were asked how often they watched local newscasts. The questions were different in the two years, so are not directly comparable, but they do not show evidence of a large increase: 10 percent said they "never" watched local news in 1972, and another 8 percent said they watched "rarely"; in 1996, 15 percent said they had not watched at all in the last week, and 5 percent said they had watched only one day. Furthermore, the fraction of people who say they "frequently" watch the local news fell from 76 percent in 1993 to 56 percent in 2000 [Pew Research Center 2002].

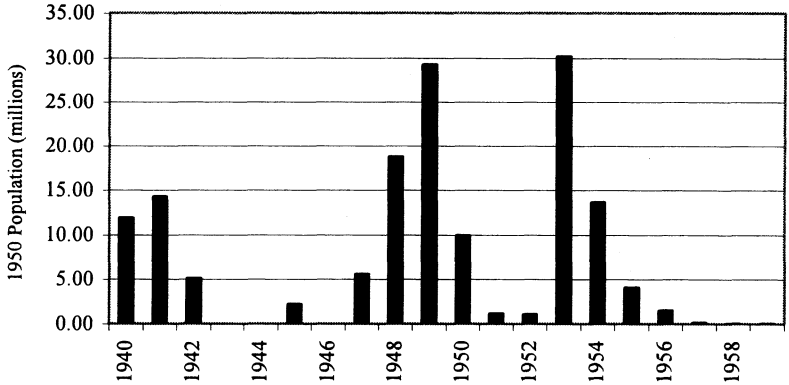

FIGURE V
1950 Population of Counties by Year Television Was Introduced

little as five years. Furthermore, the last significant group of counties to receive television got it a full ten years after the postwar growth of television began in the largest cities. This suggests a differences-in-differences research design that asks whether changes in political information or voter turnout were correlated with the timing of television's introduction.

Two key features of the data allow me to strengthen this basic identification strategy. The first is the fact that both World War II and the FCC freeze were exogenous shocks to the pattern of television's introduction across markets. The second is the fact that individual television stations broadcast over a large area, reaching a heterogeneous group of counties.

To examine the first of these, Figure V shows the fraction of the U. S. population in counties receiving television for the first time in each year. Three distinct groupings are clearly visible: counties that had television before the war, starting in 1940; counties that got television between 1945 and 1951, with the bulk in 1948 and 1949; and counties that got their first station after the freeze, beginning in 1952. The gaps between these three groups are much greater than they would have been if exogenous events had not intervened, and this added variation adds identification power.

While the war and the freeze changed the *timing* of television's introduction across markets, they did not necessarily change the *ordering*. Predictably, it was in the largest and wealthiest television markets that potential entrants found it

TABLE I
SUMMARY STATISTICS

			Mean by year of first TV station		
	Mean	St. dev.	1940–1944	1945–1951	1952 and later
Percent urban (1950)	29.0	27.1	60.4	31.6	26.0
Population (1960 '000)	59,428	208,549	466,960	76,057	31,278
Income per capita (1959)	$1,352	405	$2,044	$1,391	$1,298
% high school (1950)	27.2	10.9	35.9	27.0	26.8
Median age (1950)	28.5	3.92	32.8	28.7	28.1
Percent non-White (1950)	10.9	17.0	4.48	10.1	11.7
Persons per square mile (1950)	209	2,077	3,006	235	67.5
Base year turnout (1940)	56.5	23.6	73.1	57.4	55.2
N	2958		83	1052	1823

Percent high school is the fraction of the population aged 25 and older who completed high school. Income per capita is measured in 1959 dollars.

profitable to apply for licenses and begin broadcasting in the earliest years. Table I shows county-level demographics for the three major groupings of counties: counties that got television earlier were larger, more urban, with higher income, median age, and schooling; the earliest group also had substantially higher turnout in the 1940 election. Although the panel design controls for both cross-sectional differences correlated with the timing of television's introduction and common changes over time, the results could still be biased if there were negative shocks to information or voting that hit the largest and richest cities in the mid-1940s, medium-sized cities in the late-1940s, and small cities and rural areas in the early-1950s.

One way to address this issue is to make use of observable county characteristics. Note, for example, that the largest differences in observables shown in Table I are between the first two groups; the only large differences between the 1945–1951 and postfreeze groups are in population, population density, and percent urban. This suggests that spurious correlation is likely to be

less of a problem if attention is restricted to the latter groups.[18] Furthermore, a simple regression analysis confirms that market size and income were the key determinants of the timing of television's introduction. A linear regression at the DMA level of the year television was introduced on log population and log total income yields an R^2 of .69. When the residuals from this regression are regressed on percent urban, percent high school, median age, percent non-White, population density, median income, and 1940 turnout, the coefficients are neither individually nor jointly significant at the 10 percent level. The largest individual t-statistics are on percent urban, percent non-White, and population density (-1.46, -1.17, and -1.04, respectively). This suggests that controls for population and income may eliminate much of the spurious correlation.

An even more powerful way to address this issue is to exploit the large and heterogeneous broadcast area of individual television stations. Consider Figure VI which shows a map of the Chicago DMA and the characteristics of the counties that compose it. Although Chicago was the second largest television market in 1950 and the DMA as a whole is highly urban, dense, and wealthy, the counties that compose it are heterogeneous. They range from Newton County, IN, which has no urban population, a density of 27 people per square mile, and a median income (in 1950 dollars) of $2,778, to Cook County, IL, which has 99 percent urban population, a density of 4726 people per square mile, and a median income of $4,085. If there were bias in the basic panel specification caused by shocks to voting in dense urban areas in the mid-1940s, this should show up in the voting patterns of Cook County but not Newton County. A robust way to identify television's effect would therefore be to compare counties that are similar on dimensions like density and percent urban (taking only rural counties like Newton, for example) and ask whether they saw changes in voting around the time television was introduced. The variation in the timing of television would then be driven by whether a county happened to fall within the roughly 100-mile radius of television broadcasts from a large city, with the identifying assumption being that proximity at this distance is uncorrelated with unobserved shocks that changed the level of turnout over time.

18. Gentzkow [2005] verifies that the results are robust to excluding the earliest television counties.

County	State	Percent urban	Population per square mile	Median income ($1950)
Kendall	IL	0%	38	$3,229
Newton	IN	0%	27	$2,778
Jasper	IN	24%	30	$2,557
McHenry	IL	36%	83	$3,574
Grundy	IL	36%	44	$3,351
Porter	IN	40%	94	$3,574
Kankakee	IL	50%	108	$3,421
DeKalb	IL	51%	64	$3,299
La Porte	IN	60%	126	$3,409
Lake	IL	61%	392	$4,021
La Salle	IL	67%	87	$3,447
Will	IL	67%	159	$3,698
Kane	IL	76%	291	$3,923
DuPage	IL	76%	467	$4,531
Lake	IN	93%	716	$3,890
Cook	IL	99%	4726	$4,085
Entire Chicago DMA		92%	635	$4,085

FIGURE VI
The Chicago DMA

The basic framework for the analysis is a fixed effects regression of the form,

$$Y_{it} = \alpha_i + \delta_{rt} + \gamma TV_{it} + \beta X_{it} + \epsilon_{it}, \quad (1)$$

where i indexes counties, t indexes years, and r indexes census regions. Y_{it} is an outcome measure of interest, α_i and δ_{rt} are location and region-time fixed effects, respectively, and ϵ_{it} is a random shock. TV_{it} is some measure of the scale of television in

county i and time t. X_{it} includes observable county characteristics (that change over time) and other functions of these characteristics described below. It also includes the absolute difference of the Republican and Democratic vote percentages, a competitiveness measure that has been shown to have a strong effect on turnout (see Stromberg 2004). Note that in specifications using the National Election Study data, the variation is only cross-sectional, and so I cannot include the α_i and δ_{rt} terms in equation (1).

I use two approaches to exploit within-DMA heterogeneity. The first is to include in X_{it} interactions between key county-level observables in a base year and a fourth-order polynomial in time. This controls flexibly for differences in the time path of the dependent variables whose correlation with television is driven by the endogenous pattern of television's introduction. Based on the analysis of this timing described above, I include interactions with 1960 log population, 1959 log total income, and turnout in the 1940 election.[19] I also check that the results are robust to including interactions with 1950 percent urban, 1950 population density, and 1950 percent non-White, the remaining characteristics with t-statistics greater than one in the timing regression. The second approach is to partition the counties into thirds along observable dimensions and run the analysis separately for each group. This more flexible specification allows the region-time dummies to be estimated separately for each group. It is also a literal implementation of the experiment described above—comparing similar counties that differ in the timing of television because of their proximity to large cities.[20]

The remaining issue is how to model the effect of television.

19. I also include an interaction of the time polynomial with a dummy variable indicating a missing value for 1940 turnout. Turnout in 1940 is not significantly related to the year television was introduced once population and income are controlled for, but since any residual correlation could lead to severe bias it seems safest to include it.

20. A third way to exploit within-DMA heterogeneity would be to use a matching algorithm. As a robustness check, I have estimated the effect of television using the nearest-neighbor matching algorithm of Abadie and Imbens [2004]. I define a binary treatment variable equal to one if a county received television before the end of the FCC freeze (1952), and two dependent variables: the change in turnout between the presidential elections of 1944 and 1952; and the change in turnout between the off-year elections of 1946 and 1954. I match counties based on 1960 log population and log per capita income and 1950 median age, population density, percent urban, percent non-White, percent with high-school diploma, and median income. I consider specifications with and without bias correction, with between one and four matched observations, and with heteroskedasticity-corrected standard errors. The sample average treatment effect of television is

There are several reasons to expect that its impact would not be a one-time discontinuous change, but would rather grow gradually over time. The quantity and quality of programming increased steadily following television's initial introduction, television ownership diffused slowly across households, and the process by which television is linked to voting—substitution among media leading to a depreciating stock of information and ultimately to reduced participation in elections—would be expected to take effect gradually over time.

A natural starting point, then, would be to allow TV_{it} to grow linearly over time, beginning in the year when the first station starts broadcasting in county i. That is, $TV_{it} = I(t > \tau_i)(t - \tau_i)$, where τ_i is the year television is introduced and $I(\ )$ is the indicator function. Because penetration of television was negligible before the end of World War II, I assign a first television year or 1946 to all counties that had stations before that date. I also include separate trends for presidential and off-year elections. In some specifications I also include the squared value of TV_{it} to allow the effect to grow weaker over time.

To further check the validity of the identification strategy, Table II presents results from a series of placebo regressions. The goal is to test whether once the effect of the key observables is partialed out, variation in the timing of television's introduction is orthogonal to the remaining observables. The timing of television is measured by a dummy equal to one if a county received their first station after the FCC freeze.[21] Column 1 of the table displays the F-statistic for the null hypothesis that the coefficients on the remaining observables are all equal to zero. Column 2 shows the number of the coefficients that are individually significant at the 5 percent level.

The first set of tests looks at whether the timing of television is orthogonal to the full vector of observables once we restrict attention to subsets of counties. The F-statistics here are from a simple regression of the postfreeze dummy on the eight demo-

significant at the .1 percent level in all specifications, and roughly 50 percent larger for off-year turnout than for presidential-year turnout.

21. An alternative is to use the year television was introduced as the left-hand variable. In this case, the F-statistic for the lowest third by population density is no longer significant, while the F-statistic for the whole sample regression becomes significant at the 1 percent level.

TABLE II

PLACEBO REGRESSIONS WITH POSTFREEZE DUMMY AS DEPENDENT VARIABLE
(F-test for joint significance of demographics)

	F	# Significant coefficients
Lowest third of counties by:		
Population (1960 '000)	1.32	0 of 8
Population per square mile (1950)	2.23*	1 of 8
Percent urban (1950)	4.38**	1 of 8
Income per capita (1959)	1.50	0 of 8
% high school (1950)	2.29*	1 of 8
1940 turnout	1.92	1 of 8
Full sample after controlling for log population, log income, and 1940 turnout	1.04	0 of 5
National Election Study sample	1.31	1 of 28

* Significant at the 5 percent level.
** Significant at the 1 percent level.
The two columns show F-statistics for the joint significance of the demographic variables in each regression and the number of coefficients significant at the 5 percent level, respectively. The "lowest third of counties" regressions use only counties falling into the lowest third of the indicated demographics; the dependent variable is a dummy equal to one if television was introduced in 1952 or later and the right-hand-side variables are log total income, log population, percent urban, percent non-White, percent high school, median age, population density, and 1940 turnout; region dummies are also included but not reported in the F-test. In the "full sample" regression, the dependent variable is the residual from a regression of the postfreeze dummy on log population, log total income, 1940 turnout, and region dummies; the right-hand variables are percent urban, percent non-White, percent high school, median age, and population density. In the "National Election Study" regression, the dependent variable is the residual from a regression of a dummy for having TV in 1952 on DMA log population and log total income, county percent non-White, and region dummies; the right-hand side variables are individual-level dummies for male, White, age categories, education categories, occupation categories, income in the highest category, and missing values, as well as number of children and log income.

graphics shown in Table I and region dummies.[22] For the lowest third of counties by population, income per capita, and 1940 turnout, the hypothesis that the demographic coefficients are jointly equal to zero cannot be rejected. It is rejected at the 5 percent level for the lowest third by density and percent with high school education, and at the 1 percent level for the lowest third by percent urban. In no case is more than one demographic coefficient individually significant. This suggests that focusing on subsets of counties removes much, but possibly not all, correlation between the timing of television and county characteristics.

The next test looks at whether the timing of television is

22. Because the subset regressions below allow a different set of time dummies by region, the region dummies are treated as incidental controls here and not included in the F-test.

orthogonal to observables in the full sample once we control for population, income, 1940 turnout, and region dummies (the variables that are interacted with a time polynomial in the main specification). In a regression of these residuals on the remaining demographics, the demographics are neither jointly nor individually significant, confirming that the remaining variation is largely idiosyncratic.

The final test looks at the orthogonality of television in the National Election Study sample. Here, the data are at the individual level, and the dependent variable is a dummy equal to one if the respondent's county had television in 1952. The specification tests whether individual respondent characteristics predict having television once region dummies and county-level population, income, and percent non-White are controlled for. The 28 right-hand variables are neither jointly nor individually significant, again confirming that the remaining variation in timing is idiosyncratic.

V. Did Television Affect Turnout?

V.A. Relative Trends

As a first step in analyzing the effect of television on turnout, I present direct comparisons of turnout in counties divided into three groups by the year their first television station began broadcasting. This is a coarser approach than will be possible with the fixed effects model of equation (1), but has the advantage of allowing one to look at the data directly.

As was clear from Figure V, the natural groups are pre-1945, 1945–1951, and 1952 and after. If television reduced voter turnout, we would expect the first two groups to show a negative trend relative to the third group in the years up to 1952. The relative decline should begin in 1946 for the first group (recall that television did not begin to diffuse widely until after the war), and sometime between 1946 and 1948 for the second group.

Figure VII shows turnout in the first and second groups, respectively, measured relative to the third. To construct the figure, I regress county-level turnout on year-region dummies (I take out separate time effects by region to control for the exogenous changes affecting turnout in the South). I then calculate the mean of the residuals for each group in each year. The first panel plots the mean of the first group residuals minus the mean of the

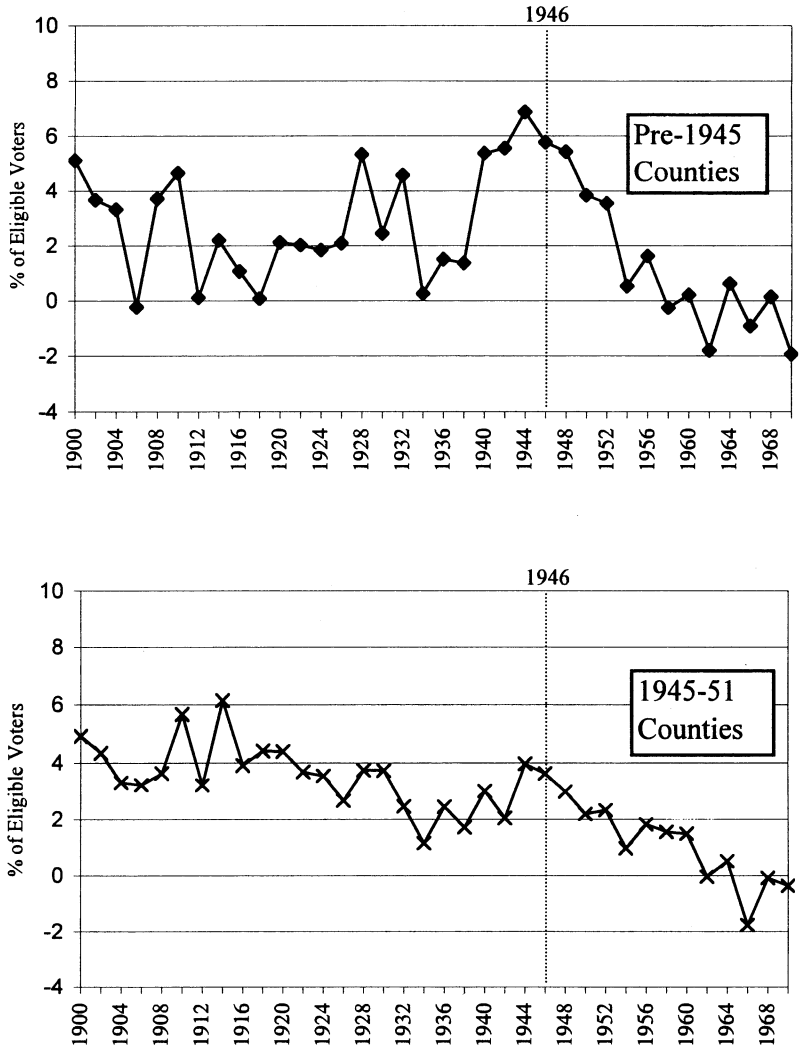

FIGURE VII
Voter Turnout by Year of Television's Introduction
(Relative to Post-1951 Counties)

third. The second panel plots the mean of the second group residuals minus the mean of the third.

A clear relative decline is apparent for both groups, beginning in 1946. That the negative trend is slightly larger for the

TABLE III
DIFFERENTIAL TRENDS IN VOTER TURNOUT BY YEAR OF TELEVISION'S INTRODUCTION
(Relative to counties where television was introduced in 1952 or later)

	(1)	(2)	(3)
First year of TV before 1945			
Relative trend, 1944–1950	−.513	−.414	−.399
	(.0775)	(.0777)	(.0750)
Relative trend, 1952+	−.306	−.006	.026
	(.0618)	(.0542)	(.0535)
First year of TV 1945–1951			
Relative trend, 1946–1950	−.125	−.101	−.106
	(.0391)	(.0396)	(.0386)
Relative trend, 1952+	−.139	−.030	−.0219
	(.0241)	(.0192)	(.0194)
Demographics			X
Time polynomial interactions		X	X
County, year-region dummies:	X	X	X
R^2	.914	.925	.927
N	46003	46003	46003

Standard errors are clustered by county. Dependent variable is the percentage of legally eligible voters casting a vote for Congress. All trends are relative to counties getting television in 1952 or later. When years are given for a relative trend (e.g., 1944–1950), the coefficient is on a variable equal to zero in the first year (1944), one in the second year (1945), and so forth. All regressions include county fixed effects, separate year fixed effects by census region, the absolute difference between the Democrat and Republican vote percentages, and a dummy for a missing value for the absolute difference. Demographics are log population, population density, percent urban, percent non-White, median income, median age, and percent with high-school education. Time polynomial interactions are a fourth-order polynomial in time interacted with 1960 log population, 1959 log total income, 1940 congressional turnout, and an indicator for a missing value of 1940 congressional turnout. Note that the levels of these variables are absorbed by the county fixed effects.

first group is consistent with the early areas having relatively high income and density and thus being the places where television ownership diffused the fastest (a point which is examined more rigorously when I add interaction terms to the regression analysis). For the first group, the relative trend also clearly flattens out beginning in 1954, the year that most counties in the reference group first had television. No clear flattening is visible for the second group.

Table III provides a more detailed look at these trends in a regression framework. I regress county-level congressional turnout on differential trends for the first two television groups relative to the third for both the pre-1952 and post-1952 periods.[23]

23. More precisely, TV_{it} is specified as four separate terms: an interaction of a dummy for membership in the first group and a time trend for the years 1944–1950; an interaction of the first group dummy and a trend for 1952+; an

Observe that in the simple model where all counties have an identical linear television effect, the coefficients would be negative for both groups in the earlier period and zero thereafter. The first column duplicates what was shown in Figure VII, including only county and year-region fixed effects. The second adds interactions of county 1960 log population, 1959 log total income, and 1940 turnout with a fourth-order polynomial in time. Note that the levels of these variables are absorbed in the county fixed effects. The third adds levels of log population, population per square mile, percent urban, percent non-White, median income, median age, and percent with high-school education. The results show that the negative relative trend in early television counties is robust to the inclusion of controls, and that it flattens out significantly once the later counties also have television (post-1952).

V.B. Years of Television Regressions

Table IV presents the core set of regressions in this section. These are again based on equation (1) but differ from the relative trends regression in that rather than using a coarse division of counties into three groups and looking at the first two groups relative to the third, the TV_{it} variable is defined as a separate trend for each county beginning in the year it first received television. That is, $TV_{it} = I(t \geq \tau_i)(t - \tau_i)$, where τ_i is the year television was introduced in county i and $I(\ )$ is the indicator function. The first two columns show the coefficients on this variable in specifications with county and region-year dummies alone, and then adding time polynomial interactions and demographics. The results show that television caused a significant decline in voter turnout. The magnitude of this effect drops with the added controls but remains strongly significant. In the complete specification, introducing television causes turnout to fall by .136 percentage points per year.

The next two columns repeat these specifications allowing different trends for presidential and off-year congressional elections. The off-year coefficient is significantly negative and suggests that television reduced turnout by .196 percentage points per year in the specification with controls. For presidential years,

interaction of a second group dummy and a trend for 1946–1950; and an interaction of a second group dummy and a trend for 1952+.

TABLE IV
REGRESSIONS OF TURNOUT ON YEARS OF TELEVISION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years of TV	−.416	−.136					
	(.0486)	(.0412)					
Years of TV * nonpresidential year			−.489	−.196	−.193	−.187	−.188
			(.0577)	(.0478)	(.0483)	(.0478)	(.0477)
Years of TV * presidential year			−.332	−.067	−.067	−.059	−.056
			(.0468)	(.0438)	(.0443)	(.0437)	(.0434)
Additional time-polynomial interactions:							
*Time * percent urban*					X		
*Time * population density*						X	
*Time * percent non-White*							X
Demographics		X		X	X	X	X
Time polynomial interactions		X		X	X	X	X
County, year-region dummies:	X	X	X	X	X	X	X
R^2	.913	.927	.913	.927	.927	.927	.927
N	46003	46003	46003	46003	46003	46003	46003

Standard errors are clustered by county. Years of TV is the number of years since the first year in which a commercial station was broadcasting in the county for at least three months. The dependent variable is the percentage of legally eligible voters casting votes for congress. Fixed effects, demographics, and time polynomial interactions are as in Table III. All regressions include the absolute difference between the Democratic and Republican vote percentage. Additional time-polynomial interactions are interactions of a fourth-degree polynomial in time with the indicated demographics.

on the other hand, the coefficient is significantly negative in the simplest specification, but it is much smaller and insignificant with the full set of controls. In the final specification, the point estimate is a negative effect of .067 percentage points per year. The final three columns add additional interactions between the time polynomial and percent urban, population density, and percent non-White, respectively. These were the three characteristics, after those already included in the interactions, that had the highest t-statistics in the DMA-level regression of television's timing on demographics (though none were significant). The results do not change substantially when these interactions are included, suggesting that the remaining variation identifying the television coefficient is largely idiosyncratic.

The results presented so far cast doubt on the hypothesis that the apparent effect of television is driven by spurious correlation with some unrelated shock to voting. The effect remains strong and significant in a variety of specifications even after allowing interactions between a flexible function of time and all of the major correlates of the timing of television's introduction. This means in particular that any factor influencing voting whose

TABLE V
REGRESSIONS OF TURNOUT ON YEARS OF TELEVISION FOR SUBSETS OF COUNTIES
(Coefficient on years of television)

	Lowest third	Second third	Highest third
Counties partitioned by:			
Population	−.332**	−.228**	−.443**
	(.1045)	(.0798)	(.0819)
Population density	−.447**	−.239**	−.289**
	(.1041)	(.0798)	(.0827)
Percent urban	−.380**	−.296**	−.440**
	(.0940)	(.0885)	(.0733)
Per capita income	−.127	−.228**	−.578**
	(.0984)	(.0803)	(.0817)
% high school	−.275**	−.282**	−.706**
	(.0940)	(.0798)	(.0752)
1940 turnout	−.160*	−.365**	−.518**
	(.0778)	(.0788)	(.0781)

* Significant at the 5 percent level.
** Significant at the 1 percent level.
Standard errors are clustered by county. The table shows the coefficient on years of TV from regressions of turnout on the absolute difference between the Democrat and Republican vote percentages, county dummies, and year-region dummies. The years of TV variable is as in Table IV. Each column gives the coefficient from regressions using only counties that fell into the given third of the data, and each row specifies the demographic characteristic on which counties were divided.

correlation with television was driven by city size or income—for example, a social change that began in the largest cities in the 1940s and then diffused outward—would not bias the coefficients.

It is possible to perform an even stronger check on the validity of the results. As discussed earlier, a more direct way to exploit the fact that television stations broadcast to a heterogeneous group of counties is to perform the analysis using only those that are demographically similar—for example, comparing turnout patterns in rural counties that happened to get television early because of proximity to a large city and those that got television late. In Table V, I divide counties into thirds by different observable characteristics and then perform the analysis separately on each third. The table reports the coefficient on years of television from a regression of turnout on this variable, county dummies, and separate year dummies by region (as well as the usual competitiveness measure). The first column of the first row, for example, reports this coefficient using only the smallest counties.

The results provide further evidence that the estimates rep-

resent a causal effect of television. The television coefficient remains significant in all but one of the eighteen specifications, and has the correct sign in all of them. The magnitude does not vary in a systematic way across thirds by population or percent urban, and is actually largest in the least dense counties, providing strong evidence against the hypothesis that the results are driven by changes taking place in the largest cities. (Note that even if there were shocks to large cities that reverberated throughout the broader television markets, we would expect the coefficients to be smaller for the less urban counties.) On the other hand, the magnitude is higher in quartiles with higher levels of income and education, consistent with the expectation that these would be the counties where television diffused the fastest. Finally, the effect is larger for counties whose turnout at the beginning of the period was highest. One interpretation is that the voting population in these counties in 1940 included a large number of relatively marginal voters whose turnout was more affected by the introduction of television.

V.C. Interactions and Nonlinear Effects

The next step in the analysis is to look in more detail at the way the magnitude of the television effect varies with county characteristics. The most obvious reason that the effect might differ is because television ownership (and in later years multiple set and color ownership) diffused faster and more broadly in some places than others. The picture is more complicated, however, because we would also expect the sensitivity of turnout to a given change in information consumption to vary with demographics. For example, if wealthier or more educated individuals have high levels of information regardless of media consumption, they would tend to be farther away from the turnout margin, and less affected by the introduction of television.

To structure the analysis of the interactions, I construct two vectors of fitted values: predicted television diffusion and predicted turnout sensitivity. The first is based on a regression of 1960 television penetration on the census year demographics from Table I and dummy variables for the year in which television was introduced. The fitted values are from the demographics alone. The second is derived by asking how sensitivity to the closeness of elections varies across counties. Although there is no reason to expect the reaction to variation in election closeness to

be identical to the reaction to information, we would expect that counties that had a large number of marginal voters would respond more to both. I regress turnout on county fixed effects, time-region fixed effects, the absolute difference in Republican and Democratic vote shares, and interactions of the absolute difference with the census-year demographics from Table I and 1940 turnout.[24] The fitted values are the predicted marginal effect of the absolute difference in vote shares.[25]

Table VI presents turnout regressions that allow a heterogeneous effect of television. The first column includes interactions with the full set of county demographics. The negative trend introduced by television is larger in counties with low population, high income, low education, high percent non-White, high density, and high 1940 turnout. These results are hard to interpret because they may conflate differences in the diffusion of television and the fraction of marginal voters. Column (2) includes an interaction with predicted television penetration in 1960 and shows that the television effect is largest in those counties where ownership diffused the fastest. The effect is large, with a one standard deviation increase in diffusion more than doubling the television effect. Column (3) includes an interaction with the predicted sensitivity of turnout and shows that the television effect is largest where the fraction of marginal voters is highest. A one standard deviation increase in this variable increases the television effect by approximately 60 percent. Column (4) includes both the diffusion and sensitivity measures. The magnitude of the former falls slightly, and the magnitude of the latter increases slightly. Both remain highly significant. Column (5) adds an interaction with the year television was introduced, which is small and insignificant.

The last three columns of Table VI address the question of how the effect of television changes over time by including both years of television and years of television squared. One weakness of a research design that relies on relative changes between counties over time is that such nonlinearities will be difficult to separate from heterogeneity across counties. Recall, for example,

24. County turnout in 1940 is allowed to affect sensitivity but not diffusion—this is based on the assumption that unusually high turnout may reflect the participation of a large block of marginal voters but should not directly affect television set purchases.

25. See Gentzkow [2005] for more detailed discussion of the construction of these predicted values.

TABLE VI
INTERACTIONS AND NONLINEAR EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of TV	−.128	−.123	−.138	−.114	−.199	−.186	−.160	−.192
	(.0422)	(.0413)	(.0421)	(.0425)	(.0528)	(.0489)	(.0522)	(.0543)
Years of TV squared						.00235	.0255	.0104
						(.00205)	(.0125)	(.0126)
Interaction with:								
Predicted diffusion		−.0184		−.0168	−.0171			−.0167
(mean = 0, stdev = 8.76)		(.00367)		(.00365)	(.00367)			(.00374)
Predicted sensitivity			−.0828	−.108	−.111			−.110
(mean = 0, stdev = 1)			(.0138)	(.0147)	(.0149)			(.0150)
First TV year					−.00761		.0466	.01281
(mean = 0, stdev = 2.58)					(.00419)		(.0252)	(.0256)
Percent urban (1950)	−.000337							
	(.000476)							
Log population (1960)	.131							
	(.0394)							
Log income per capita (1959)	−.360							
	(.192)							
% high school (1950)	.00372							
	(.00145)							
Median age (1950)	−.0236							
	(.00286)							
Percent non-White (1950)	−.00264							
	(.000968)							
Population density (1950)	−.0123							
	(.00278)							
1940 turnout	−.00380							
	(.00194)							
Demographics	X	X	X	X	X	X	X	X
Time polynomial interactions	X	X	X	X	X	X	X	X
County, year-region dummies:	X	X	X	X	X	X	X	X
R^2	.9273	.9270	.9269	.9270	.9271	.9268	.9268	.9271
N	42832	46003	42832	42832	42832	46003	46003	42832

Standard errors are clustered by county. The dependent variable, fixed effects, demographics, and time polynomial interactions are as in Table III. The Years of TV variable is as in Table IV. All regressions include the absolute difference between the Democrat and Republican vote percentage. All interactions are the Years of TV variable times the interaction variable minus its mean. Predicted diffusion and predicted sensitivity are fitted values from regressions of diffusion and turnout sensitivity on demographics as described in the text.

that the relative trend regressions in Table III show that turnout in the early television counties neither rises or falls relative to the later counties after 1952 (when all counties have television). This could reflect a constant linear television effect whose magnitude is the same across counties. Alternatively, it could reflect an effect that gets smaller over time combined with a smaller effect overall for the later counties. The results in columns (6) through (8) show that the estimated quadratic term is highly sensitive to which interaction terms are included. It is positive in all specifications (suggesting that the effect is getting smaller over time), but it is only significant in the regression with only the first TV year interaction. The magnitude ranges from .002 (implying that it would take 93 years for the effect to die out entirely) to .0255 (implying that this would take only 6 years). This suggests that there is some nonlinearity, but that the exact functional form is beyond the power of these data to identify convincingly. It also suggests that estimates of the total impact of television over a long period of time are unlikely to be very precise.

V.D. Magnitudes

We can evaluate the magnitude of the effects documented above in a number of ways. The main specifications in Table IV imply that television decreased off-year turnout by approximately 2 percent each decade after it was introduced. They imply that it decreased presidential turnout by .7 percent, although this estimate is not significantly different from zero. As context for these numbers, the overall negative trend in non-South turnout since the 1950s was 3.4 percent per decade, and the negative trend in presidential turnout was 3.2 percent per decade, implying that television accounted for 60 percent of the off-year decline and (possibly) 22 percent of the presidential decline. Although these effects assume that the per-year decline in voting remains constant over time, and so might overstate how much of the decline television explains, they probably do not do so by much because most of the aggregate decline took place before 1970.

To more accurately assess the effect on overall turnout, it is necessary to take account of the fact that television was introduced to different counties at different times and that the effect was larger in some counties than others. To do so, I estimate counterfactual aggregate turnout levels based on the estimated parameters. The preferred specification is the nonlinear model in column (8) of Table VI to which I add separate television effects

for presidential and off years—this allows both a squared term in years of television and a full set of interactions. I also check the results for the basic specification in column (3) of Table IV and the specification with interaction terms only in column (5) of Table VI, again allowing for a separate presidential year effect. For each model, I ask how much higher turnout would have been if television had never been introduced assuming that television caused no further effect after 1970. As a point of reference for these results, the difference between the average non-South turnout level in the 1950s and in the 1980s–1990s was 11 percentage points for off-year elections and 13 points for presidential elections. The difference between the highest and lowest turnout years between 1940 and 2000 is 16 points for off-year elections and 18 points for presidential elections.

The results of this counterfactual experiment are as follows. For non-South turnout, the total impact of television by 1970 is estimated to be a reduction of 5.6 percent in off-year elections and 3.1 percent in presidential elections. Television therefore accounts for 50 percent of the off-year decline and 24 percent of the presidential year decline since 1950.

As stressed above, however, the difficulty of separately identifying a nonlinear effect within counties and heterogeneity across counties means that these estimates can vary significantly depending on the specification. Thus, the specification with interactions but no nonlinear term gives effects of 9.5 percent for off years and 6.2 percent for presidential years, while the basic specification with neither nonlinear terms nor heterogeneity gives effects of 4.1 percent and 1.3 percent, respectively. The differences across these models are intuitive: we would expect the model without nonlinear terms to overstate television's effect when we extrapolate far beyond the introduction date, since the per year impact is not allowed to fall over time; omitting interactions should cause us to understate the aggregate effect because the largest counties were also the ones that had the largest effects. Nevertheless, the variability of the estimated magnitudes suggest that they should be interpreted with a great deal of caution.

VI. How Did Television Affect Turnout?

In the Introduction I argued that crowding out of political information provides a plausible mechanism linking television

and voting. The goal of this section is to present a range of evidence consistent with this prediction. I will not be able to identify precisely how much of the effect on turnout worked through this channel, but the results build a strong case that information played a critical role.

VI.A. Substitution among Media

The first kind of evidence comes from examining the extent to which television caused substitution away from other news sources—specifically newspapers and radio. This is relevant because the amount of political information provided by television in its early years was substantially less than that provided by either newspapers or radio, making it unlikely that a dramatic shift away from the latter media would lead the public to become better informed. Predictably given the large economies of scale in television broadcasting relative to newspaper publishing, the difference was especially large for more local elections.

A variety of evidence documents the difference in political coverage across media. Until the mid-1960s, television news in general was extremely limited. Until 1963, NBC and CBS evening news programs were only fifteen minutes in length, and ABC did not switch to a thirty-minute format until 1967. Local stations prior to 1963 usually scheduled 30 minutes of news programming, of which 20 minutes was taken up by sports and weather [Sterling and Kittross 2001]. Nielsen [1975] points out that the entire text of a national newscast from the 1950s would fill only three columns on the front page of the *New York Times*. The comparison to radio is also stark: in 1950 and 1955, network radio had about seven times as much regularly scheduled news as network television [Sterling 1984]. Moreover, the difference between television and other media did not disappear after the 1960s. Morgan and Shanahan [1992] summarize a number of studies in the political science literature showing that those who turn to television news as their main source of information have lower levels of political knowledge, trust government less, and are less likely to participate in the political process in ways other than voting than those who rely on other media. This evidence is primarily correlational, but it is suggestive of the way television differs from other media.

Evidence for the greater difference in local coverage comes from a series of Roper Surveys conducted in 1952, 1964, 1968, and 1972. The percentage of respondents saying television was their

most important source of information about national elections was about twice as large as the fraction saying newspapers. For local elections, on the other hand, the percentage saying television was the most important source was 25–35 percent *lower* than the percentage saying newspapers [Sterling 1984, p. 165]. Similar evidence for later years comes from the National Election Studies which show that for the presidential years from 1970 to 1980, more respondents heard about the election on television than in newspapers, with the reverse pattern holding for off-year elections. Perhaps the most compelling piece of evidence is Mondak [1995]. This study is based on a survey of Pittsburgh residents during an eight-month newspaper strike in 1992. Comparing Pittsburgh to demographically matched residents in a nearby county not affected by the strike, Mondak finds that those deprived of a local newspaper but with continuing access to television report significantly less knowledge of candidates and issues in the House campaign, but no difference in knowledge of the presidential race.

What remains is to show that television indeed reduced consumption of newspapers and radio. For radio, even casual evidence of this substitution is very strong. The average number of radio-listening hours per household per day fell from four hours in 1950 to just more than two in 1955 (prior to 1950, the number of listening hours had been roughly constant since 1930 [Sterling 1984, p. 220]). Ratings for evening radio programs in New York fell by 60–80 percent between 1948 and 1951 [Gould 1951].

For newspapers, it is possible to analyze changes in state-level circulation using the variation in the timing of television's introduction discussed above. I divide states into three groups based on when their first station began broadcasting: before 1945, 1945–1951, and 1952 and after. Figure VIII shows circulation of daily newspapers per thousand people in the first two groups of states relative to the third (for example, the first panel shows circulation per thousand in the pre-1945 states minus circulation per thousand in the third group). Although the clarity of the picture is limited by the fact that data do not extend prior to 1945, the graphs are consistent with a negative effect of television. Both series show relative declines over the 1946–1952 period. In the second group of states, most of which got television in 1948 or 1949, the decline becomes significantly steeper after these years. Also, both trends flatten out after 1953 when most states in the

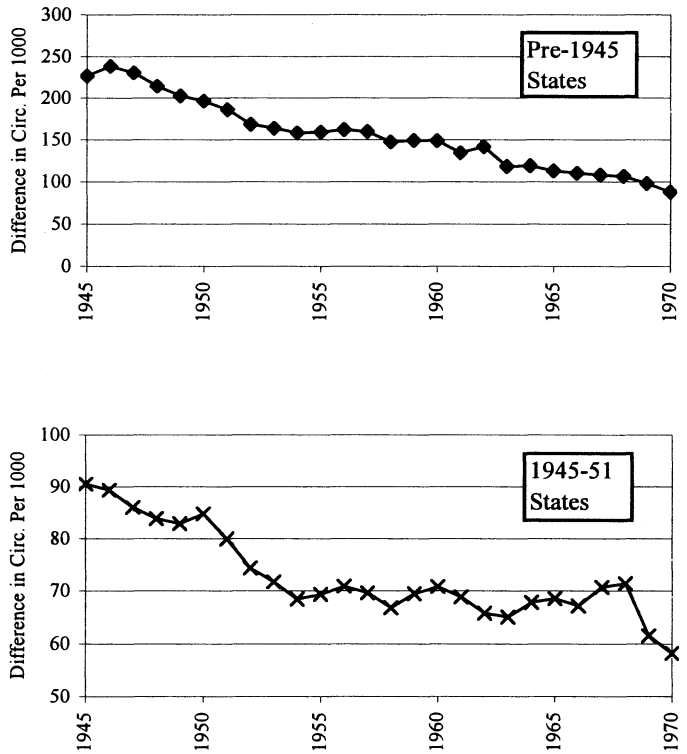

FIGURE VIII
Circulation of Daily Newspapers by Year of Television's Introduction
(Relative to Post-1951 States)

reference group had television, though this is much more pronounced in the second panel than in the first.

A more direct way to look at substitution among media for political information specifically is to use data from the 1952 National Election Study. The primary limitation of this data is that it is a single cross section, and so does not allow me to control for unobserved time-constant heterogeneity among counties. However, 1952 was the end of the FCC freeze and so was the year in which idiosyncratic variation in the availability of television was greatest. This is confirmed by the placebo regression in Table II: after controlling for DMA log income and log population, county percent non-White, and region dummies, the dummy for TV availability is uncorrelated with observable individual char-

TABLE VII
MEDIA USE IN THE 1952 ELECTION

	Any campaign info from:		
Dependent variable	Newspaper	Radio	Television
TV dummy	−0.114	−0.283	0.445
	(.0402)	(.0423)	(.0745)
Log income	0.0752	0.000037	0.1136
	(.0211)	(.0172)	(.0339)
Ed: high school	0.119	0.0815	0.1133
	(.0207)	(.0297)	(.0312)
Ed: college	0.165	0.0936	0.1224
	(.0163)	(.0515)	(.0677)
Mean of dep. var.	.791	.699	.514
R^2	.185	.089	.309
N	1693	1705	1653

Standard errors are clustered by county. Coefficients are marginal effects from probit regressions. In addition to controls shown, all regressions include individual-level dummies for age, occupation, sex, White race, highest income category, political party identification, and missing values of each control, as well as a continuous control for number of children. Each also includes controls for DMA-level log income and population; county-level population, percent urban, population density, percent non-White, median age, median income, and median schooling; and dummies for census regions. The TV dummy is one if the respondent's county had television prior to 1952. The "any campaign info" variables are dummies equal to one if the respondent reported obtaining information from the given source.

acteristics. All regressions in this section include DMA log income and log population; region dummies; county percent non-White, population, percent urban, population density, median age, median income, median schooling; and individual level dummies for age, education occupation, sex, White race, highest income category, political party identification, and missing values of each control, as well as continuous controls for number of children and log income. The independent variable of interest is a dummy for whether or not the respondent's county had television.

Table VII shows results from three specifications in which the dependent variables are responses to the following questions:

Did you read about the campaign in any newspaper?

Did you listen to speeches or discussions about the campaign on the radio?

Did you watch any programs about the campaign on television?

The results show a large and highly significant substitution effect. Respondents in television counties were 11.6 percent less likely to have read about the campaign in the newspaper and 28.5 percent less likely to have heard about the campaign on the radio.

Each coefficient is significant at the 1 percent level, and their magnitudes are large relative to the 79 percent and 70 percent of the overall sample getting campaign information from newspapers and radio, respectively. Note, too, that both the newspaper and radio variables are positively correlated with education and income. Since counties with higher education and income were more likely to have television, this argues against the radio and newspaper coefficients being driven by unobservable variation in the overall level of economic development. The final column verifies that substantially more people watched programs about the campaign on television in the counties coded as having television in 1952. This is primarily a check on the validity of the data. The percentage watching is also large, at 45 percent, suggesting that television was already an important political outlet.[26]

VI.B. Direct Measures of Political Information

The second kind of evidence that I present on television and information also comes from the 1952 National Election Study, but concerns direct measures of political knowledge. The first set of questions comes from the preelection survey. Respondents were asked:

> Who do you plan to vote for as United States senator?
>
> How about congressman? Who do you plan to vote for there?
>
> Who do you think you will vote for as governor here in [state]?

These were free response questions, and the data indicate whether the respondent mentioned a candidate actually running in the race, an incorrect candidate, or a party with no mention of a specific candidate.[27] The question was only asked of respondents who said they intended to vote in the election (I drop other respondents from this analysis). For each question, I create a dummy variable equal to one if the respondent correctly named a candidate running in the race and equal to zero otherwise. Because races for senate and governor are statewide while congressional races are more local (California currently has 54 separate races, for example), we would expect television stations to devote

26. This coefficient is slightly lower than the percentage of individuals in the overall sample watching on television, partly reflecting the fact that some individuals not in television counties reported having watched campaign programs. This could result both from travel to other counties during the campaign season and measurement error in the television variable.

27. A question was also asked about presidential voting intentions, but it was coded differently and did not distinguish correct and incorrect responses.

TABLE VIII
POLITICAL INFORMATION IN THE 1952 ELECTION

	Able to name candidate preelection:			Able to name candidate postelection:		
Dep. var:	House	Senate	Governor	House	Senate	Governor
TV dummy	−0.248	0.08916	0.0392	−0.340	−0.236	−0.114
	(.1155)	(.1353)	(.0811)	(.1054)	(.0700)	(.0700)
Log income	0.0192	0.0344	0.0360	0.0904	0.0742	0.0410
	(.0373)	(.0481)	(.0416)	(.0371)	(.0344)	(.0312)
Ed: high school	0.120	0.181	−0.003	0.098	0.0156	0.052
	(.0553)	(.0507)	(.0291)	(.0492)	(.0409)	(.0383)
Ed: college	0.198	0.224	0.016	0.227	0.069	0.135
	(.0987)	(.0690)	(.0676)	(.0654)	(.0814)	(.0435)
Mean of dep. var.	.261	.280	.272	.489	.662	.755
R^2	.181	.166	.201	.203	.180	.199
N	653	525	612	1011	795	682

Standard errors are clustered by county. Coefficients are marginal effects from probit regressions. TV dummy is equal to one if the respondent's county had a television station in November 1952. Controls are as in Table VII. "Able to name candidate preelection" variables are dummies equal to one if the respondent gave the name of a candidate that they intended to vote for the specified office in the preelection interview, and equal to zero if they simply named a party or mentioned a candidate not running; respondents who said they did not intend to vote for the specified office are omitted. "Able to name candidate postelection" variables are defined analogously for the postelection survey.

substantially less coverage to any given congressional race. This suggests that the negative effect of television should be largest for the congressional question.

The second set of questions took a similar form, but was asked retrospectively after the election. Respondents were asked whether or not they voted for each office, and if they answered affirmatively, were asked "who did you vote for?" This was again a free response question and was coded as above. In the analysis of this question, I use only those who voted for the relevant office. I create a dummy variable equal to one if the respondent correctly named a candidate.

The results of this analysis are presented in Table VIII. The regressions all include the same controls as those in Table VII, and differ only in the dependent variables and the number of observations. The first thing to note about the table is that all but one of the education and income coefficients are positive, and the majority are significant. This provides some confirmation that the dependent variables are picking up what they are intended to. It also suggests that omitted variables positively correlated with the overall level of economic development (the main predictor of early

television entry) would bias the results against finding a negative effect on information.

The coefficients on the preelection variables are negative and significant for knowledge of congressional candidates, and positive and insignificant for knowledge of senatorial and gubernatorial candidates. This is consistent with the prediction that the negative effect should be larger for the more local race. One might have expected some negative effects on the senatorial and gubernatorial variables, however, since these races still receive limited coverage on the national network programs that make up a large portion of viewership. The magnitude of the congressional coefficient is very large—having television predicts being 24.8 percent less likely to provide the name of a congressional candidate, while only 26.1 percent of the overall sample was able to do so. Note, however, that since television areas have higher education and income, the regression does not predict that the fraction answering in areas with television would be zero.

The postelection coefficients are all negative, with the largest effect on knowledge of congressional candidates. Both the Senate and House coefficients are significant. Having television predicts being 34 percent less likely to name a candidate for congress, 24 percent less likely to name a candidate for Senate, and 11 percent less likely to name a candidate for governor. This compares with 49 percent, 66 percent, and 76 percent, respectively, naming such candidates in the sample as a whole. Why the postelection effects are larger than the preelection effects is not immediately obvious, although it may simply reflect the fact that this is a more highly selected group (it only includes those who actually voted) and so had higher baseline levels of knowledge overall.

Taken together these results provide strong evidence that television caused substitution away from newspapers and radio, and that this in turn caused a large drop in levels of information about candidates in the 1952 election, with the strongest and most significant effects on information about congressional candidates.

VI.C. Interaction with Number of Congressional Districts

The final piece of evidence I report is the interaction between the television effect and the number of congressional districts in a television market. Recall that the contrast between presidential and off-year congressional elections is the key piece of evidence tying together the information and turnout results. Congres-

sional elections are less covered on television and extensively covered in newspapers, and these are the races for which the effect of television on both information and turnout is largest. The number of districts provides an additional "difference" to test: within congressional elections, television coverage should be limited in television markets that are fragmented into many districts. This should make the television effect on both information and turnout in congressional races largest in such markets. Importantly, the number of districts should not change the television effect in presidential years.

Of course, the number of districts in a market is far from exogenous. Because representation in the House of Representatives is proportional, the number of districts in a state is a mechanical function of population. The number of districts in a television market will also depend on population, but the relationship will be less perfect, since district boundaries are often highly irregular and do not follow county boundaries. Once population is controlled for, the remaining variation should be relatively idiosyncratic.

I define the number of districts in a DMA based on the county-level ICPSR voting data. These data report the congressional district of each county, or the number of different districts in the county if there is more than one. Unfortunately, since they do not identify the individual districts in a multiple-district county, there are some ambiguous cases. For example, if county A and county B have two districts each, they might together account for anywhere from two to four. I use the upper-bound measure (in this case four), assuming that all such districts are unique. This will likely overstate the variance in districts, biasing the magnitude of the estimated effect downward, but the ordering of counties in terms of number of districts should remain roughly the same.

I first ask whether the effect of television on information about congressional candidates is more negative in markets with more congressional districts. I repeat the National Election Study regressions of Table VIII where the dependent variable is being able to name a House candidate both before and after the election. I add interactions between television and both population and the number of districts, as well as levels of the latter variables. I do not present these results, but the interaction between television and number of districts is not significant in either specification. It is small and positive for the preelection question and small and

TABLE IX
REGRESSIONS OF TURNOUT ON YEARS OF TELEVISION INTERACTED WITH NUMBER OF DISTRICTS

	(1)	(2)
(number of districts: mean = 0 stdev = 5.78)		
Off-year elections		
Years of TV	−.374	−.144
	(.0584)	(.0486)
Years of TV * number of districts	−.00829	−.00462
	(.00130)	(.00126)
Presidential elections		
Years of TV	−.268	−.0597
	(.0479)	(.0454)
Years of TV * number of districts	−.0048	−.00119
	(.00139)	(.00133)
Demographics		X
Time polynomial interactions		X
*County, year * region dummies:*	X	X
R^2	.914	.927
N	46003	46003

Standard errors are clustered by county. The dependent variable is the percentage of legally eligible voters casting votes for congress. Number of districts is measured at the DMA-level. All regressions include an interaction between years of television and 1950 DMA population as a control. Years of TV, dependent variable, fixed effects, demographics, and time polynomial interactions are as in Table III. All regressions include the absolute difference between the Democratic and Republican vote percentage.

negative for the postelection question, with the t-statistic in both cases less than one.

Table IX presents turnout regressions which include interactions with the number of districts. In contrast with the analysis of the National Election Study, these results are significant and align closely with what the information mechanism would predict. The regressions are the same as the main specification (Table IV) except that they include separate interactions between number of districts and years of television for presidential and off-year elections. They also include an interaction between years of television and population as a control. The first column shows the regression with only county and region-year fixed effects, and the second column adds time-polynomial interactions and levels of demographics. The effect of television on off-year turnout is significantly more negative in markets with more congressional districts, with a one standard deviation increase in the number of districts increasing the magnitude by 18 percent in the final specification. In contrast, the number of districts does not change

the presidential year effect once the full set of controls is added, and the difference between the off-year and presidential interactions is significant at the 1 percent level. These results are not conclusive given the failure to find first-stage effects on the National Election Study measure. But they do provide some additional evidence that crowding out of information may link television and turnout.

VII. Conclusions

Taken together, the results of this paper tell a consistent story. Although television did provide a new medium for delivering political information, it also offered consumers a wide array of new ways to use their leisure time. The fact that even in 1950 the average television household was watching for four and a half hours per day makes clear what a dramatic improvement television was over previous entertainment technologies. Faced with both a reduction in a price of information and a much larger drop in the price of entertainment, consumers responded by substituting away from the former and toward the latter. This substitution was largest where the information provided by television was most limited: local elections.

The evidence also shows that television also caused fewer voters to go to the polls. This effect was particularly strong in exactly those elections where the drop in information was shown to be the largest. The evidence linking television to a drop in voting is robust to partitioning counties into groups by demographic characteristics, controlling explicitly for demographics, and allowing nonlinear functions of time interacted with demographics. The magnitude of the effect interacts in an intuitive way with observed characteristics of counties. Although this analysis cannot identify conclusively the mechanism by which television affected turnout, it strongly supports the hypothesis that crowding out of information played an important role.

Among the motivations for this study discussed in the introduction was a paradox: the coincidence of dramatic improvements in media and education with a sharp decline in turnout. These results provide a partial resolution to this paradox by showing that not only did television fail to increase information and turnout, it was an important cause of the decline, explaining half of the drop in off-year turnout since 1950, and possibly a quarter of the drop in presidential years. Furthermore, the logic that under-

lies these results may apply more broadly. The improvements in media and education that are at the root of the apparent paradox have been accompanied by a proliferation of new ways to spend leisure time, from cable, to video games, to DVDs, to the Internet. While a conclusive answer will require detailed study of these broader trends, it would not be surprising to find that this expansion of choices led to further crowding out of political engagement.

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