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# The Effect of Newspaper Entry and Exit on Electoral Politics†

*By* Matthew Gentzkow, Jesse M. Shapiro, and Michael Sinkinson*

*We use new data on entries and exits of US daily newspapers from 1869 to 2004 to estimate effects on political participation, party vote shares, and electoral competitiveness. Our identification strategy exploits the precise timing of these events and allows for the possibility of confounding trends. We focus our analysis on the years 1869–1928, and we use the remaining years of data to look at changes over time. We find that newspapers have a robust positive effect on political participation, with one additional newspaper increasing both presidential and congressional turnout by approximately 0.3 percentage points. Newspaper competition is not a key driver of turnout: our effect is driven mainly by the first newspaper in a market, and the effect of a second or third paper is significantly smaller. The effect on presidential turnout diminishes after the introduction of radio and television, while the estimated effect on congressional turnout remains similar up to recent years. We find no evidence that partisan newspapers affect party vote shares, with confidence intervals that rule out even moderate-sized effects. We find no clear evidence that newspapers systematically help or hurt incumbents.* (*JEL* D72, L11, L82, N41, N42, N81, N82)

The opening or closing of newspapers has long been linked to the health of democracy. Alexis de Tocqueville saw the large number of US newspapers in 1831 as key to the country's broad political participation (2003 [1831]). Contemporaries thought the growing number of newspapers in the late nineteenth century was strengthening democracy (Whitelaw Reid 1872; Charles W. Eliot 1897 [1894]), while both policy makers (*Miami Herald Publishing Co. v. Tornillo* 1973; Federal Communications Commission [FCC] 2003) and scholars (Ben Bagdikian 2000) have worried that

*Gentzkow: University of Chicago Booth School of Business, 5807 S. Woodlawn Avenue, Chicago, IL 60637, and NBER (e-mail: gentzkow@ChicagoBooth.edu); Shapiro: University of Chicago Booth School of Business, 5807 S. Woodlawn Avenue, Chicago, IL 60637, and NBER (e-mail: jesse.shapiro@chicagobooth.edu); Sinkinson: Harvard Business School, Wyss Hall, Soldiers Field Road, Boston, MA 02163 (e-mail: msinkins@fas.harvard.edu). We are grateful to numerous seminar participants for input on this project. Mike Abito, Patrick DeJarnette, Yao Lu, James Mahon, Nathan Petek, and Mike Thomas, assisted by Felicity Bloom, Yiwei Chen, David Cheng, Felipe Diaz-Arango, Ingrid Gonçalves, Hannah Melnicoe, Hugh Montag, Harish Raja, Rui Silva, and Margarida Soares, provided outstanding research assistance. James Snyder kindly provided his cleaned election data files for use in this project. This research was funded in part by the Initiative on Global Markets, the George J. Stigler Center for the Study of the Economy and the State, the Ewing Marion Kauffman Foundation, the Centel Foundation/Robert P. Reuss Faculty Research Fund, the Neubauer Family Foundation, and the Kathryn C. Gould Research Fund, all at the University of Chicago Booth School of Business, the Social Sciences and Humanities Research Council of Canada, and the National Science Foundation.

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the closing of competitive newspapers in the twentieth century has weakened it. In recent years, the possibility that the Internet may lead to further newspaper closures has provoked concerns about falling political participation (Sam Schulhofer-Wohl and Miguel Garrido 2009), increased ideological polarization (Cass R. Sunstein 2007), and the elimination of a check on government corruption (Paul Starr 2009).

In this paper, we use a new panel of US daily newspapers from 1869 to 2004 to look directly at how entries and exits of newspapers affect political participation, party vote shares, and incumbency advantage. Our data include every general-circulation English-language daily newspaper published in the United States over this period. We observe papers' location, circulation, and, in the early years of our sample, political affiliation. We supplement this data with search-based measures of newspaper content and county-level electoral data. We observe a total of 3,913 county-years with net newspaper entry and 3,303 county-years with net newspaper exit. Importantly, our data cover a period in which many markets have two or more competing newspapers, allowing us to study the way media effects vary with both competitiveness and ideological diversity.

We focus our analysis on the years 1869–1928, when newspapers were a uniquely important source of political information. We use the remaining years of the data to study how the effect of newspapers changes after the introduction of radio and television.

Our identification strategy exploits the fact that exits and entries cause large, discrete changes in newspaper readership. Trends in readership before or after such an event are small relative to the effect of the event itself. Our basic strategy is to look at changes in political outcomes in counties that experience an entry or exit relative to other counties in the same state and year that do not. To address the possibility of a spurious relationship, we first discuss theory and evidence on the determinants of newspaper profits and the extent to which they could be correlated with the political outcomes of interest. We then plot changes in the outcomes in years before or after entry and exit events to confirm that the effects we find are driven by sharp "on-impact" changes, that any associated trends match the predictions of theory, and that the pattern of leads and lags is inconsistent with unobserved shocks to economic or political variables driving our key findings.

We first study the effect of newspapers on political participation. Prior evidence suggests that any bias from omitted variables in this case is likely to work against finding a positive effect, because local area population and income growth tends to be associated with declines in turnout. Consistent with this evidence, we find that turnout tends to decline in the years before and after the entry of a newspaper, a trend that we can largely explain with observable covariates. In the period of an entry, on the other hand, we see sharp increases in turnout. We argue that the economics of the entry and exit decision make such a pattern highly unlikely in the absence of a true causal effect, and we present several pieces of evidence supporting a causal interpretation of our findings.

We conclude that newspapers have a robust positive effect on political participation. In the years 1869–1928, one additional newspaper increases presidential turnout by 0.3 percentage points. This effect is similar for congressional and presidential elections, and is robust to a range of alternative specifications. Turning to the role of competition, we find that the effect of the first entrant to a market on turnout is 1.0 percentage point, while the effect of later entrants is significantly smaller. The competition results are consistent with the hypothesis that all turnout effects are proportional to the effect on the share of eligible voters reading at least one paper, and imply that reading a newspaper increases the probability of voting by 4 percentage points. The effect of newspapers on presidential turnout diminishes after the introduction of radio and television, while the effect on congressional turnout remains similar up to recent years.

We next study the effect of partisan newspapers on Republican and Democratic vote shares. These specifications exploit the fact that a large fraction of newspapers in the early part of our sample declare explicit party affiliations, which we show predict large differences in newspaper content and endorsements. Here, the likely source of omitted variables bias pushes toward finding a spurious persuasive effect, though an analysis of pretrends suggests that, given our controls, any such bias is likely to be small.

We find that partisan newspapers do not have large effects on party vote shares. Our point estimate on the effect of a Republican newspaper's entry or exit on the Republican presidential vote share is very close to zero, with a confidence interval that rules out effects greater than 0.5 percentage points. We can reject the hypothesis that partisan newspapers convince 3 percent or more of their readers to change their votes. Our findings are similar (though less precise) for vote shares in congressional elections, do not vary significantly according to the extent of competition, are consistent over time, and survive a variety of robustness checks.

In the final section of the paper, we assess whether newspapers increase or decrease incumbency advantage. We find no clear evidence of an effect in either direction.

This paper contributes to a growing empirical literature on the political effects of media.[1] David Strömberg (2004), Gentzkow (2006), and James M. Snyder and Strömberg (2008) study effects of media on turnout, and find results consistent with our finding of a positive effect of newspapers. In a closely related paper, Schulhofer-Wohl and Garrido (2009) use within-city variation in circulation to study the effect of the closure of the *Cincinnati Post* on voter turnout and incumbent success.

Our findings on party vote shares relate to studies by Stefano DellaVigna and Ethan Kaplan (2007), Alan S. Gerber, Dean Karlan, and Daniel Bergan (2009), and Ruben Enikolopov, Maria Petrova, and Ekaterina V. Zhuravskaya (2011), who all estimate significant persuasive effects of partisan media using data from recent years. Based on Brian G. Knight and Chun-Fang Chiang (2008), a possible explanation for the difference between these findings and our own is that the partisan newspapers we study declared their political leanings explicitly, making it easy for consumers to filter bias, while most modern outlets claim to be neutral and unbiased.[2] That we can estimate the effect only of explicit party affiliations should be taken as an important limitation of our findings.

Our study differs from past empirical work on media effects in the large sample of media outlets we cover, our identification from sharp entry and exit events, our ability to study these effects over a long time period that includes years where

[1] In addition to the studies mentioned here, there is a large political science literature on media effects. See Doris A. Graber (2000) for a review and Markus Prior (2006) and Stephen Ansolabehere, Erik C. Snowberg, and James M. Snyder (2006) for closely related work on the effects of media on political competitiveness.

[2] Rational filtering does not explain the contrast between our findings and the larger persuasive effects estimated by Jamie L. Carson and M. V. Hood III (2008) for newspapers in the early nineteenth century.

newspapers were the primary source of political information, and our ability to study the effect of competition and ideological diversity.

Section I below describes the data used in our study. Section II provides background on the political content of newspapers and the determinants of newspaper entry and exit. Section III lays out our empirical strategy and discusses identification. Section IV presents results on political participation, Section V presents results on party vote shares, and Section VI presents results on political competitiveness. Section VII concludes.

## I. Data

### A. *US Newspaper Panel*

We collect data from annual directories of US newspapers from 1869 and from every presidential year from 1872 to 2004, inclusive. The data for 1869 through 1876 come from George P. Rowell and Company's (Rowell's) *American Newspaper Directory.* The data for 1880 through 1928 come from N. W. Ayer and Son's (Ayer's) *American Newspaper Annual.* The data for 1932 through 2004 come from the *Editor and Publisher Yearbook*. Although lists of newspapers were compiled in some earlier years, we are not aware of any regularly published directory of daily newspapers prior to 1869. Since our analysis will focus on presidential years, we treat the 1869 data as a measure of the newspapers that existed in 1868. (Dropping 1869 from the data does not affect our central conclusions.)

Newspaper directories are standard sources for historical research on US newspapers, but have not before been digitized on such a large scale. They originated as a guide to potential advertisers and were intended to be complete. Counts of daily newspapers from these sources are similar to independent tabulations performed by the US Census Bureau (Alfred McClung Lee 1937).

In each year, we extract the name, city, time of day, and circulation of every English-language daily newspaper. We match newspapers across years on the basis of their title, city, and time of day. We match cities to census place definitions and match each census place to the county containing the largest share of the place's population.

For each county-year, we compute the number of English-language daily newspapers, which serves as our first key independent variable.

From the data on circulation, we construct an estimate of the share of eligible voters reading at least one newspaper in each county in each year. The number of individuals reading diverges from the total number of copies circulated for two reasons: many individuals read more than one paper on a given day, and many copies of a given paper are read by more than one individual. Consistent with estimates of the ratio of reported readership to circulation in both historical readership surveys (Elmo Roper 1946) and recent data (Gentzkow 2007), we assume that each copy is read by two individuals. We then assume that for any two papers A and B, the share of A's readers who also read B is equal to the share reading B in the overall population. This is a highly stylized model of newspaper demand, but it approximates the patterns of readership overlap in several historical case studies, and it is a reasonable approximation to the demand structure estimated in Gentzkow (2007). We consider readership to be missing if any newspapers are missing data on circulation in a given county-year.

We also extract the political affiliation of each newspaper in our sample. We discuss the meaning of these affiliations in Section II below. The vast majority of affiliations are "Democratic," "Republican," or "Independent," with the share of Independent affiliations growing over time. We define a time-constant measure of affiliation for each newspaper, where papers are classified as Republican if they ever declare a Republican affiliation and Democratic if they ever declare a Democratic affiliation. In the handful of cases where a newspaper declares a Republican affiliation in one year and a Democratic affiliation in another, we use the majority affiliation. Our final sample includes 2,566 papers we classify as Republican, 2,431 we classify as Democratic, 1,714 we classify as Independent, and 1,063 which never report an affiliation.

For each county-year, we compute the difference in the number of Republican and Democratic newspapers, which serves as our second key independent variable.

A small number of newspapers identify a special emphasis in their content. We use these descriptors to classify 307 newspapers—such as commercial, financial, legal, or trade publications—as "nonpolitical" in the sense that they likely do not emphasize political news. Consistent with our classification, we find that 75 percent of nonpolitical newspapers never declare themselves as Republican, Democratic, or Independent, as against only 11 percent for other ("political") newspapers.

We discuss further details of the construction of our data in Appendix A, and present additional summary statistics in the online Appendix to this paper.

### B. *Measures of Newspaper Content*

We collect text-based data on the political content of newspapers in our sample from the website newspaperarchive.com. For each newspaper, for each presidential election from 1872 to 1928, and for each party, we search newspaperarchive.com for articles containing the last names of both the presidential and vice presidential candidates and at least one of the words "nominee," "candidate," "nomination," "race," "ticket," "election," or "campaign." We then compute the share of all candidate mentions that go to the Republican candidate. Our searches return hits for 137 unique newspapers, of which 52 are Democratic and 72 are Republican. The total number of hits over all years is 66,489.

As an additional measure of newspapers' political leanings, we collect data on newspapers' presidential endorsements for the years 1932–2004 from *Editor and Publisher Magazine*'s "Roll Call" survey. The survey, published annually, is based on questionnaires mailed to newspaper editors asking them which party their paper endorsed for president.

### C. *Market Definition*

Our analysis defines the news market to be a county. We do this because county is the smallest unit at which we can disaggregate presidential election data over such a long period. In fact, some counties contain multiple news markets (cities), and newspapers also circulate across nearby counties. Calculations from Gentzkow and Jesse

M. Shapiro's (2010) data show that, today, the median newspaper sells more than 80 percent of its copies in the county in which it is headquartered, and the median county in which at least one newspaper is headquartered gets more than 80 percent of its copies from in-county newspapers. If improvements in transportation technology mean that news distribution is at least as geographically dispersed today as in the past, these calculations indicate that counties will be a reasonable approximation of news markets in most cases. In Appendix B we repeat our analysis on the subsample of counties that contain only one news market each and are not in metropolitan areas, which we expect to eliminate most cases in which county is a poor approximation.

### D. *Voting and Demographic Data*

We match each county-year observation to data on voting from various sources through 2004. These data include the total number of votes cast by party and county in each election for president, representative, governor, and senator. We measure turnout as the ratio of total votes to eligible voters, where the number of eligible voters is interpolated using census demographic data. The data also include the party of the incumbent candidate in each election, if any. (Additional details on the construction of the voting data are available in the online Appendix to this paper.)

Our analysis focuses on presidential elections and congressional elections in presidential election years. We also perform some analysis of congressional turnout in off-year elections. We compute the change in off-year turnout as the change in turnout between the election two years after the current presidential year and the election two years prior to the previous presidential election year. This approach introduces some noise but guarantees that the newspaper entries and exits we measure occur strictly between the off-year elections. In the online Appendix, we present additional findings from gubernatorial and senate elections.

We obtain county-level demographic data from the US Census and County Data Books (ICPSR 2896 by Michael R. Haines 2006), supplemented with data from the National Historical Geographic Information System (NHGIS.org). We compute the share of the population that is white, the share of the white population that is foreign-born, the share of the population that is males 21 and older, the share of the population living in cities with 25,000+ residents, the share of the population living in towns with 2,500+ residents, and the population employed in manufacturing as a share of males 21 and older. For each measure, we interpolate both the numerator and denominator between census years using a natural cubic spline (John G. Herriot and Christian H. Reinsch 1973) and divide the two to obtain an estimate of the relevant share. We also use data from the census on the definition of Primary Metropolitan Statistical Areas as of 1990.

We use manufacturing output per capita as a proxy for income. This gives us a comparable county-level income proxy across our entire sample. To validate this measure, we have compared it at the state level to Richard A. Easterlin's (1960) reckoning of total income per capita in 1900. The correlation between the two measures is 0.49 for all states, and 0.76 when we remove three outlier states.

To estimate the effect of newspapers on journalist employment, we collect data on the number of journalists in selected cities from the printed manuscripts of the 1870 and 1880 US censuses.

County-level data on educational attainment do not exist for much of the period we study. In Appendix B we show that our results are robust to controlling for the literacy rate in the county.

### E. *Sample Selection*

We exclude outlier observations with changes in turnout per eligible voter greater than one in absolute value. These implausible values represent less than 0.1 percent of the cases in the data, and excluding them improves precision. Our results are also robust to excluding the most influential observations as measured by DFBETA influence statistics.

We restrict attention to counties that experience at least one four-year period in which the number of newspapers increases or decreases during our sample period. Doing so increases the homogeneity of the sample by, for example, excluding counties that never have newspapers. On average during our sample period, the voting-eligible population in excluded counties is one-quarter as large as in the included counties, and a formal likelihood-ratio test rejects the hypothesis that state-year effects are identical for included and excluded counties. Including the excluded counties in our sample reduces precision slightly, but leaves our central findings unchanged.

We divide the data into three time periods: the newspaper period (1872–1928), the radio period (1932–1952), and the television period (1956–2004). These years are chosen so that the radio and television periods each begin in the first presidential election in which the respective technology had a national penetration in excess of 50 percent (Christopher H. Sterling 1984). We conduct our main analysis on the newspaper period, and report results on how our estimates vary across these three time periods.

## II. Background

### A. *Political Content and Party Affiliation*

Newspapers were an important source of political information throughout our sample period. The historical record is clearest on their role in presidential elections. Newspapers were central players in the political process in the late nineteenth century (Richard L. Kaplan 2002). Newspapers in the 1890s devoted 20–40 percent of their coverage to politics (Gerald J. Baldasty 1992), with electoral coverage tilted significantly toward presidential contests (Samuel Kernell and Gary C. Jacobson 1987). Newspapers' relative importance was reduced substantially by the introduction of radio in the late 1920s. Radio coverage of presidential campaigns began in 1924, and expanded dramatically in the 1930s (Sterling and John Michael Kittross 2002). In 1944—still before the widespread diffusion of television—twice as many respondents chose radio as the most accurate source of information about the presidential campaign as chose newspapers (National Opinion Research Center 1944). Beginning in the late 1940s, television eclipsed both newspapers and radio as a source of information about presidential campaigns. Survey evidence from the 1950s–1970s shows that roughly twice as many people chose television as their most important source of information about presidential campaigns as chose newspapers, and evidence from the National Election Study shows that a similar pattern continued through the later twentieth century (Gentzkow 2006).

Congressional and state politics were an important part of day-to-day newspaper coverage in the nineteenth century (Kaplan 2002; Kernell and Jacobson 1987), and newspapers remained the most important source of information about state and local politics in the television era. Survey respondents in the 1950s to 1970s ranked newspapers as the most important source of information about local elections. Newspapers were also rated as more important than television for information about nonpresidential elections in the 1970s and 1980s. Jeffery J. Mondak (1995) showed that a 1992 newspaper strike in Pittsburgh was associated with substantial declines in knowledge of candidates and issues in the congressional campaign, but not in the presidential campaign. Snyder and Strömberg (2008) provide econometric evidence that newspapers significantly influence knowledge of congressional candidates.

During our main sample period, it was common practice for newspapers to declare an explicit affiliation with a political party (James T. Hamilton 2006; Gentzkow, Edward L. Glaeser, and Claudia Goldin 2006). Affiliated newspapers were explicit in representing their party's point of view. For example, in 1868, the Democratic *Detroit Free Press* announced, "The *Free Press* alone in this State is able to combine a Democratic point of view of our state politics and local issues with those of national importance" (Kaplan 2002, p. 23). Similarly, in 1872, the Republican *Detroit Post* declared as its mission "To meet the demands of the Republicans of Michigan and to advance their cause" (Kaplan 2002, p. 22). Gentzkow, Glaeser, and Goldin (2006) present case studies demonstrating that both the tone and the substance of coverage of nineteenth- and early-twentieth-century political scandals differed greatly by affiliation. Frank Luther Mott (1950), Mark W. Summers (1994), and Kaplan (2002) provide additional detail on the sharp content differences between Republican and Democratic papers.

Kaplan (2002) argues that partisan bias was often "latent," surfacing in practices such as devoting "a disproportionate attention to favored politicians' words and deeds" (Kaplan 2002, p. 27). Content measures from automated searches of news text support this claim. Column 1 of Table 1 shows that, on average, Republican newspapers devote 48 percent of mentions to the Republican ticket, as compared to 29 percent for Democratic newspapers. The difference is highly statistically significant. Although our searches do not distinguish between positive and negative mentions, in tandem with Kaplan's (2002) close reading of Detroit papers, it seems reasonable to conclude that partisan differences in candidate emphasis were widespread. Column 2 shows that the association between affiliation and content remains large after controlling for the Republican vote share in the market, suggesting that affiliation is capturing variation in content above and beyond reader views, at least as proxied by the vote share.

Content measures also support our approach to classifying newspaper affiliations, in which we consider a newspaper as partisan if it ever declares a partisan affiliation. Columns 3 and 4 of Table 1 show that, for papers that currently declare an Independent affiliation, the relationship between content and historical affiliation remains strong. Our data on newspaper endorsements also confirm that even in the later years of our sample (when most papers are nominally Independent), papers with a historical Republican affiliation have significantly different content

Table 1—Partisan Affiliation and Newspaper Content

| | Republican share of candidate mentions | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Republican affiliation (permanent) | 0.1850 (0.0283) | 0.1571 (0.0324) | 0.1815 (0.0298) | 0.1829 (0.1005) |
| Republican vote share | | 0.1858 (0.0850) | | |
| Constant | 0.2943 (0.0168) | 0.2135 (0.0359) | 0.2972 (0.0185) | 0.2900 (0.0635) |
| Sample | All | All | Currently Independent? No | Yes |
| Newspaper-years | 423 | 423 | 363 | 60 |
| $R^2$ | 0.2517 | 0.2591 | 0.2617 | 0.4482 |

*Notes:* Standard errors in parentheses are clustered by newspaper. Time period is 1872–1928. All specifications include year fixed effects. Each observation is a newspaper-year. Dependent variable is the number of search hits for the Republican presidential and vice presidential candidate divided by the total number of search hits for both Republican and Democratic candidates.

than papers with a historical Democratic affiliation. Over the years 1932–2004, historically Democratic papers endorsed Republican candidates 45 percent of the time, as compared to 90 percent of the time for historically Republican papers.

### B. *Newspaper Entries and Exits*

The central independent variable in our analysis is the change in the number of newspapers. We observe a total of 3,913 county-years with net newspaper entry and 3,303 county-years with net newspaper exit. (More detailed summary statistics are available in the online Appendix to this paper.)

These events represent large discrete changes in the availability of local newspapers. Newspapers enter large and remain large until the year before their exit. On average, circulation in the first year after entry is equal to 87 percent of a newspaper's lifetime average circulation; circulation in the last year before exit is equal to 115 percent of the lifetime average. We report below that a typical entry increases readership in a county by about 13 percentage points. Entries and exits are also associated with noticeable trends in the market for journalists. A regression of the change in the number of journalists on the change in the number of newspapers shows that between 1870 and 1880, each additional daily newspaper opened is associated with an additional increase of 17 journalists employed in the city, on a base of 50.

Entries and exits are also large events relative to the associated trends in circulation with which they coincide. As we would expect, newspaper exits are preceded by unusually slow growth in circulation, and newspaper entries are followed by unusually fast growth. These facts are consistent with stylized facts from life-cycle studies of firms in other industries (see, e.g., Kenneth R. Troske 1996). These trends tend to be on the order of a few percent, however, and are therefore dwarfed in size by the impact of the entry or exit event itself.

To justify our identification strategy, it is critical to understand the forces that cause entry and exit. Contemporary (Buford Otis Brown 1929) and historical (Baldasty 1992; Hamilton 2006) accounts of this period suggest two primary determinants of the number of newspapers in a market.

The first is population. Newspapers have nontrivial fixed costs (such as reporting and writing), so market size is a major determinant of the number of newspapers in a market (Timothy F. Bresnahan and Peter C. Reiss 1991; David Genesove 2003; Hamilton 2006). In our data, the average voting-eligible population over our sample period explains 61 percent of the variation across counties in the average number of daily newspapers. And, as we show below, the timing of entries and exits is strongly associated with trends in population growth.

The second is income. Advertisers care about dollars spent, and richer areas can command greater advertising revenue per reader. Across counties, per capita manufacturing output (our proxy for income) is significantly and positively correlated with the number of newspapers, and entries and exits of newspapers are associated with corresponding trends in output.

A number of idiosyncratic "supply-side" factors also affect the profitability of newspapers. Costs vary dramatically with the local price of paper and ink. Variation in advertising demand at both the local and national levels is a primary determinant of revenue. Early "how-to" guides to newspaper publishing (e.g., Brown 1929) identify a number of other local factors that make areas more or less attractive to potential entrants, such as the geographic location or administrative status of a town, the extent of retail competition, the interest of particular publishers, and the availability of financing.

In some cases, political considerations also affected entry and exit decisions directly. Affiliated papers sometimes received patronage from the parties they represented, typically in the form of government printing contracts and the like (Kaplan 2002). There are also anecdotal examples of newspaper entries that were motivated by political considerations (David Nasaw 2001). Our reading of the historical record suggests, however, that in most places and at most times during our sample period commercial considerations were paramount (Brown 1929; Leo Bogart 1981; Baldasty 1992; Baldasty 1999).

Some of our estimates exploit variation in entering and exiting newspapers' partisan affiliations. It is therefore important to understand the forces that affect newspapers' affiliation choices. An important factor is consumer ideology. Existing theory (Sendhil Mullainathan and Andrei Shleifer 2005; Gentzkow and Shapiro 2006) and evidence (Hamilton 2006; Gentzkow and Shapiro 2010) support the view that media content is tailored to the prior beliefs of consumers. In our data there is a highly statistically significant positive correlation between the affiliation of an entering newspaper and the voting behavior of the county before the newspaper's entry. Other factors that might affect newspapers' political affiliations include ownership (Ruben Durante and Brian Knight 2009), incentives to differentiate (Chiang 2009), and the opinions of journalists (Timothy Groseclose and Jeffrey Milyo 2005).

In each of the results sections below, we discuss the extent to which the various drivers of entry and exit are correlated with our political outcomes of interest, and the implications for the interpretation of our parameters.

## III. Empirical Framework

### A. *Specification*

Let $c$ index counties, $s$ index states, and $t \in \{1, \dots, T\}$ index presidential election years (with one time unit representing four calendar years). We model an outcome of interest $y_{ct}$, which could be voter turnout, Republican vote share, or incumbent vote share.

Our key independent variable of interest is $n_{ct}$, which for now we define as the number of newspapers in county $c$ at time $t$. We will also study the effect of other features of the news market, such as the degree of competition and the effect of newspapers' political affiliations. In such cases the change in notation is straightforward.

We assume that

$$y_{ct} = \rho_c + \beta n_{ct} + \gamma_{st} + \boldsymbol{\delta} \mathbf{x}_{ct} + \lambda z_{ct} + \varepsilon_{ct}, \tag{1}$$

where $\rho_c$ is a county effect, $\gamma_{st}$ is a state-year effect, $\mathbf{x}_{ct}$ is a vector of observable characteristics, $\boldsymbol{\delta}$ is a vector of parameters, and $\varepsilon_{ct}$ is a county-year shock. The parameter $\beta$ is the causal effect of $n_{ct}$ on $y_{ct}$.

The index $z_{ct}$ denotes newspaper profitability. The parameter $\lambda$ encodes the extent to which newspaper profitability is related to the political outcome $y_{ct}$ conditional on $n_{ct}$, $\mathbf{x}_{ct}$, $\rho_c$, and $\gamma_{st}$.

We estimate the model in first differences. We choose this specification over one with county fixed effects because the data display highly persistent shocks. (Jeffrey M. Wooldridge's (2002) recommended test for serial correlation in linear unobserved panel data models rejects the null hypothesis of no serial correlation at high levels of confidence for both turnout and Republican vote share.) We let $\Delta$ be a first-difference operator so that, for example, $\Delta y_{ct} = y_{ct} - y_{c(t-1)}$. Our estimating equation is, then,

$$\Delta y_{ct} = \beta \Delta n_{ct} + \Delta \gamma_{st} + \boldsymbol{\delta} \Delta \mathbf{x}_{ct} + \lambda \Delta z_{ct} + \Delta \varepsilon_{ct}, \tag{2}$$

where county fixed effects $\rho_c$ drop out due to the differencing.

We treat $\gamma_{st}$ as a state-year fixed effect in estimation. Because of the electoral college system, state-year–specific factors are likely to be important drivers of county-level political outcomes, and many of these factors (e.g., whether the state is a battleground) are likely unrelated to events in any given county's news market. Our estimates will thus be driven by the way political outcomes change in counties that experience changes in the newspaper market relative to counties in the same state and year that do not.

Unless otherwise noted, the vector of controls $\mathbf{x}_{ct}$ includes changes in the share of the population that is white, the share of the white population that is foreign-born, the share of the population that is 21+-year-old males, the share of the population living in cities with 25,000+ residents, the share of the population living in towns with 2,500+ residents, the population employed in manufacturing as a share of 21+-year-old males, and the log of manufacturing output per capita (a proxy for income).

We cluster our standard errors at the county level to allow for correlation over time within a county. Marianne Bertrand, Esther Duflo, and Mullainathan (2004) find that this approach yields tests of correct size when the number of units (counties) is large. A block-bootstrap at the county level yields similar results, as does clustering at the state-decade level to allow for spatial as well as serial correlation (C. Alan Bester, Timothy G. Conley, and Christian B. Hansen 2010). Experiments with three parametric models of within-county correlation—random effects, AR(1), and AR(2)—yield point estimates and standard errors comparable to those of our main estimates. (See Appendix B and the online Appendix to this paper for details.)

### B. *Identification*

We will think of $\varepsilon_{ct}$ as a county-year shock to the outcome of interest that is unrelated to newspapers' profits, and hence to newspapers' entry and exit decisions. Formally, we assume that

$$E(\Delta\varepsilon_{ct} \mid \Delta\gamma_{st}, \Delta\mathbf{x}_{ct}, \Delta z_{ct}, \Delta n_{ct}) = 0. \tag{3}$$

We assume that net newspaper entry is positively related to contemporaneous changes in newspaper profits: $\mathrm{Cov}(\Delta z_{ct}, \Delta n_{ct} \mid \Delta\gamma_{st}, \Delta\mathbf{x}_{ct}) > 0$.

Identification is straightforward if $\lambda = 0$, i.e., if variation in newspaper profitability is unrelated to political outcomes once we condition on observable market characteristics and state-year fixed effects. This is most plausible if remaining variation in newspaper profits comes from cost shocks and other idiosyncratic commercial considerations.

We address the possibility that this assumption could be violated in three ways.

First, in each section of results we draw on theory and prior evidence to evaluate the likely sign of $\lambda$ if it is nonzero. We argue in each case that any bias is likely to work against the results we find.

Second, we argue that identification from the fine timing of events limits bias even if $\lambda$ is nonzero. Formally, we argue that the discreteness and irreversibility of newspaper entries and exits means that variation in contemporaneous profits $\Delta z_{ct}$ will account for a small fraction of the conditional variance in $\Delta n_{ct}$.

Third, we use pretrends to test for remaining bias. We plot changes in the outcomes in years before and after entry and exit events to confirm that the effects we find are driven by sharp on-impact changes, that any other trends match our expectations regarding the sign of $\lambda$, and that such trends are largely eliminated by our controls.

These techniques are standard in "difference-in-differences" studies. We argue below that they are especially compelling in our context in light of the economics of newspaper entry and exit.

*Exploiting the Precise Timing of Entry and Exit*.—If $\lambda \neq 0$, the magnitude of the omitted variables bias will depend on the conditional covariance of $\Delta n_{ct}$ and $\Delta z_{ct}$ relative to the conditional variance of $\Delta n_{ct}$. If, after conditioning on $\Delta\mathbf{x}_{ct}$ and $\Delta\gamma_{st}$, most of the variation in net newspaper entry is a function only of $\Delta z_{ct}$, the bias is severe. If, on the other hand, the current profit innovations $\Delta z_{ct}$ explain only a small fraction of the variance in net entry, the bias is small. The economics of the entry and exit decision mean the latter is the more likely case.

To fix ideas, consider the following simple model. Suppose that $z_{ct}$ evolves as a random walk. In each county, a single newspaper that is out of the market at time zero decides in each period whether or not to enter. Once it enters, it remains in the market forever. It is easy to show that the optimal policy for the newspapers is to enter if and only if $z_{ct} \geq z^*$ for some cutoff $z^*$ (Avinash Dixit and Robert S. Pindyck 1994).

For a given value of $\Delta z_{ct}$, $\Delta n_{ct}$ depends on both the level of $z_{c(t-1)}$ and the value of the cutoff $z^*$. A given increase in profitability will prompt the newspaper to enter only if it happens to tip the level of profitability over the cutoff. There will be many periods where $\Delta z_{ct}$ is large and no entry occurs, and many other periods where $\Delta z_{ct}$ is small but still causes an entry. Even though entries depend only on $z_{ct}$, the current-period shock $\Delta z_{ct}$ will explain a small share of the variance in $\Delta n_{ct}$.

To see this another way, consider an analogy with regression-discontinuity analysis. In a regression-discontinuity design, assignment to treatment is determined by a score variable which is potentially correlated with the outcome of interest. Treatment does not vary smoothly with the score but, instead turns on when it crosses a threshold. In a small window around the threshold, the variation in treatment becomes arbitrarily large relative to the variation in the score that induces it. In our setting, time is the analogue of the score variable. If $\beta > 0$, we expect entry and exit to induce discontinuous changes in outcomes that are large relative to contemporaneous changes in profitability.

*Diagnosing Bias Using Pretrends*.—Pretrends are a standard diagnostic for bias in panel data models. If the relationship between $\Delta n_{ct}$ and $\Delta y_{ct}$ comes only from a causal effect, $\Delta n_{ct}$ cannot be correlated with past values of $\Delta y_{ct}$. If the observed relationship is driven by omitted components $\Delta z_{ct}$, $\Delta n_{ct}$ and past values of $\Delta y_{ct}$ may be correlated.

This diagnostic is especially powerful in our setting because the economics of the entry and exit decision implies that net entry in the current period should depend on both current and past values of $\Delta z_{ct}$. In the simple model of entry above, $\text{Cov}\big(\Delta z_{ct}, \Delta n_{c(t+k)} \,|\, n_{ct} = 0\big) > 0$ for $k > 0$. Conditional on the newspaper being out of the market at time $t$, the probability of entry is monotonically increasing in the level of $z_{ct}$, so a positive innovation in $\Delta z_{ct}$ increases the hazard rate of entry not only in the current period but also in every period in the future.[3] This in turn implies that if $\lambda > 0$, $\Delta n_{ct}$ and past values of $\Delta y_{ct}$ *must* be correlated.

Our data confirm this intuition for important drivers of entry and exit. In panel A of Figure 1 we plot coefficients $\alpha^k$ from the following specification:

$$\Delta z_{ct} = \sum_{k=-10}^{10} \alpha^k \Delta n_{c(t-k)} + \Delta \gamma_{st} + \boldsymbol{\delta} \Delta \mathbf{x}_{ct} + \Delta \varepsilon_{ct}, \tag{4}$$

[3] To confirm this theoretical intuition in a richer model, we have simulated a special case of the entry and exit model of Richard Ericson and Ariel Pakes (1995) in which we turn off the possibility of investment and in which profits depend on a single state variable $z$ which evolves as a random walk. As expected, the state variable increases significantly in expectation in periods before an entry (and decreases in periods before an exit). For many parameter values, the coefficient on the lagged value is almost as high as the coefficient on the on-impact effect.

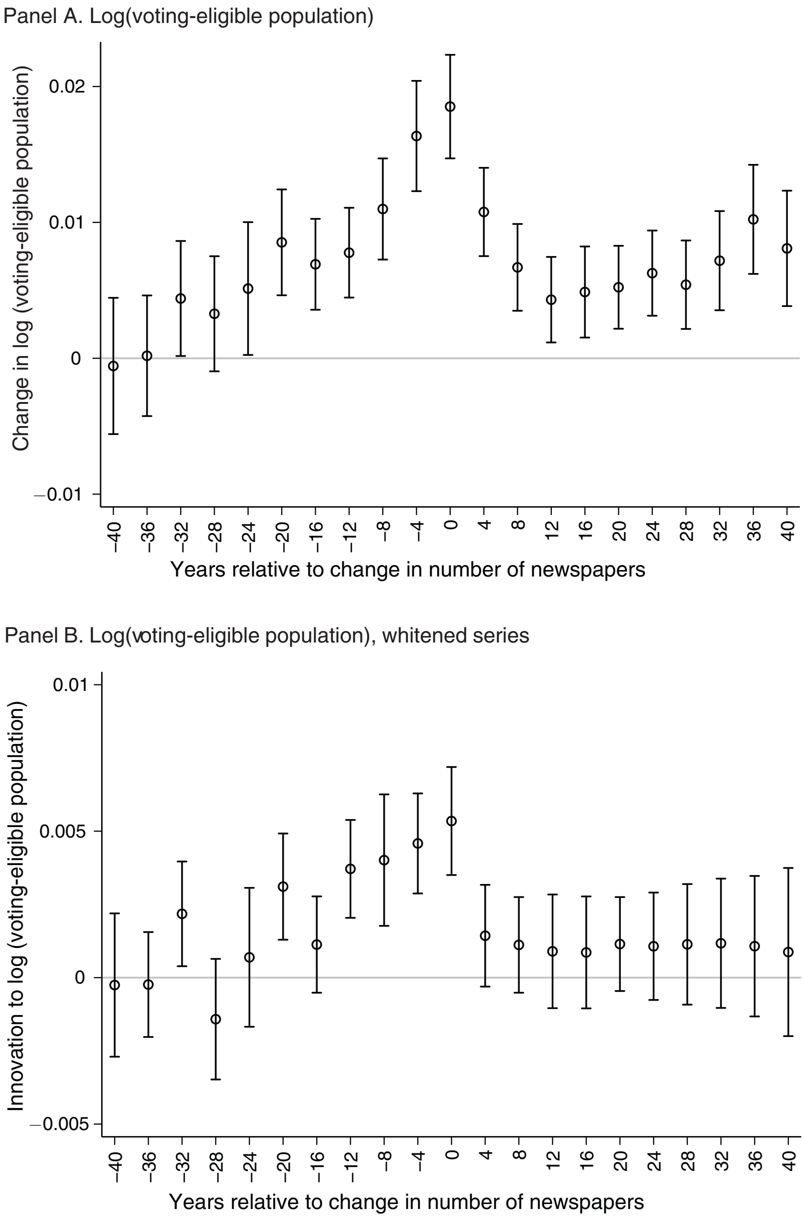


FIGURE 1. CHANGES IN POPULATION AROUND NEWSPAPER ENTRIES/EXITS

*Notes:* Panel A shows coefficients from a regression of change in log(voting-eligible population) on a vector of leads and lags of the change in the number of newspapers (see equation (4) for details). Panel B shows coefficients from a regression of innovation in log(voting-eligible population) on a vector of leads and lags of the change in the number of newspapers (see equation (4) for details). Innovation in a variable is its residual from a regression of the variable on ten lags of the variable and of the event indicator. Models include state-year fixed effects. Error bars are ±2 standard errors. Standard errors are clustered by county. Time period is 1868–1928.

where $z_{ct}$ is proxied with the log of the voting-eligible population and we abuse notation in defining the remaining terms as in equation (1). We plot coefficients $\alpha^k$ for $k < 0$ on the left-hand side of the plot, as these reflect the relationship between current changes in the number of newspapers and *past* changes in population. We plot coefficients $\alpha^k$ for $k > 0$ on the right-hand side of the plot, as these reflect the relationship between current changes in the number of newspapers and *future* changes in population.

Panel A of Figure 1 shows that population growth is above average in the period in which a newspaper enters ($\alpha^0 > 0$). The figure also shows that population growth is above average in the periods prior to entry ($\alpha^k > 0$ for $k < 0$) and that it is almost as large in the period before entry as in the entry period itself.

The figure also shows that population growth is above average in the periods immediately after entry ($\alpha^k > 0$ for $k > 0$). This is not a property of our simple model because we assume that profitability follows a random walk, so that entry in the present cannot predict future growth in population. In practice, population growth is highly serially correlated, so that newspaper entry in response to past population growth is predictive of future population growth. Panel B of Figure 1 illustrates this mechanism by plotting a "whitened" series of innovations to population constructed to have no serial correlation.[4] In this plot, $\alpha^k = 0$ for $k > 0$ by construction.

The fact that $\alpha^k > 0$ for $k < 0$ means that trends in outcomes of interest before an entry or exit can be used to estimate the sign and magnitude of the likely bias in our estimate of $\beta$. Consider estimating a model of the form of equation (4), but with an outcome of interest $\Delta y_{ct}$ as the dependent variable, producing coefficients $\tilde{\alpha}^k$. Because, by assumption, there can be no causal effect of entry in the period before it occurs, the terms $\tilde{\alpha}^k$ for $k < 0$ can be nonzero only if $\lambda \neq 0$, i.e., if newspaper profitability affects the outcome variable. Moreover, the sign of $\tilde{\alpha}^k$ for $k < 0$ will be identical to that of $\lambda$; and if, for example, $\alpha^1$ is close in magnitude to $\alpha^0$ in equation (4), then $\tilde{\alpha}^1$ will approximate the bias in our estimate of $\beta$.[5]

To summarize, we expect that if unobserved shocks to newspaper profits affect political outcomes (or are driven by the same factors) we will observe that current changes in the newspaper market are correlated with past changes in political outcomes. This will be true if the shocks in question are i.i.d. across time, and even more so if they are serially correlated.[6] By contrast, if newspapers exert a causal

[4] We do this by constructing a regression prediction of each variable using ten lags of the variable and ten lags of our main event indicator $\Delta n_{ct}$. We then construct a "whitened" series of innovations by extracting residuals from the predictive regression. By construction, these residuals are orthogonal to past realizations of the variable and of the event indicator.

[5] Consider our simple special case in which $z_{ct}$ is a random walk and there is at most one entry. Ignore state-year fixed effects and other controls, and assume that, absent entry, both profits and the outcome of interest have no drift, and that $\Delta\varepsilon_{ct}$ is i.i.d. Then, $\tilde{\alpha}^0 = E(\Delta y_{ct} | \Delta n_{ct} = 1) = \beta + \lambda E(\Delta z_{ct} | \Delta n_{ct} = 1) = \beta + \lambda\alpha^0$ and $\tilde{\alpha}^{-1} = E(\Delta y_{ct} | \Delta n_{c(t+1)} = 1) = \lambda E(\Delta z_{ct} | \Delta n_{c(t+1)} = 1) = \lambda\alpha^{-1}$. The naive estimate of the causal effect $\beta$ has a bias that approaches the lead effect $\tilde{\alpha}^{-1}$ as $\alpha^0$ approaches $\alpha^{-1}$. If we can use an observable proxy for profits such as population to estimate the ratio $\alpha^0/\alpha^{-1}$ (how much faster profits grow in the period of an entry relative to the period before), we can estimate the size of the bias directly from the pretrends in the outcome variable and adjust the naive estimate of $\beta$. We implement an approach in this spirit in Appendix B and show that our results are similar to our main specifications.

[6] Note that our random-walk model assumes that shocks to profits are permanent, an assumption motivated by the empirical time-series properties of key drivers of profitability such as population. Increases in profitability that dissipate within four years would not usually be enough to induce a forward-looking newspaper to enter, but if they were large they might. In such a case, absent a causal effect of newspapers (i.e., if $\beta = 0$ ), we would expect $\tilde{\alpha}^0$ and $\tilde{\alpha}^1$ to have opposite signs, because the trends that induce entry would reverse in the following period. This pattern would be a confound in models that estimate a temporary effect of newspapers on political outcomes, rather than the permanent effects we estimate, and in any case is not present in our data. Therefore, in order to mimic the patterns we will attribute to a causal effect of newspapers, transitory shocks to newspaper profitability would have to induce permanent changes in political outcomes. We consider such cases to be unlikely a priori. In addition, shocks to newspaper circulation exhibit a high degree of persistence, so it is unlikely that transitory shocks account for a large fraction of the events that we exploit in estimation.

effect on political outcomes but newspaper profitability does not, current changes in the newspaper market will be unrelated to past changes in political outcomes.

## IV. Effect of Newspapers on Political Participation

### A. *Specification, Mechanisms, and Potential Confounds*

In this section, we define $y_{ct}$ to be turnout (the ratio of votes cast to the number of eligible voters) in presidential and congressional elections.

In our main specifications, the independent variable is the number of newspapers $n_{ct}$. When we study competition in Section IVD, we define a vector of indicators for whether county $c$ (i) has $\geq 1$ newspaper, (ii) has $\geq 2$ newspapers, and (iii) has $\geq 3$ newspapers. When we look at ideological diversity in the final specification in Section IVD, we include these three components, plus an indicator for whether or not the county has at least one Democratic and one Republican newspaper. We estimate all models in first differences.

The most obvious mechanism linking newspapers and voter turnout is information. Newspapers may simply inform (or remind) people of the fact that an election is taking place. Newspapers also provide information about the issues at stake and the candidates' characteristics and platforms. Most theories of voting predict that individuals will be more likely to vote when they are better informed (Timothy Feddersen 2004), and existing evidence supports this prediction (John G. Matsusaka 1995; David Dryer Lassen 2005). Newspapers could also affect turnout as a by-product of increasing social capital and general civic engagement (Robert D. Putnam 2000), or because partisan papers intentionally mobilize their party's supporters to vote.

Several distinct intuitions suggest these effects may vary with the extent of newspaper competition. On the one hand, second and later entrants to a market may have smaller effects than first entrants because some of their readership comes from customers of the incumbent paper(s) and so they increase the share of people reading at least one newspaper by less. On the other hand, later entrants may have larger effects, or at least larger effects than a naive model would predict, because they expand the market by driving down prices and driving up quality, prevent capture by interests opposed to a broad franchise, or make government more responsive to constituent interests and so increase the motivation to vote.

There is also a variety of reasons to expect turnout effects to change over time. One of the most important is the availability of substitute news sources. As we argue in Section II, the introduction of radio and television reduced the importance of newspapers as a source of election news, especially in the case of presidential contests.

Turning to potential sources of bias in our estimates, recall from the discussion above that the most important drivers of newspaper entry and exit are likely to be population and income. If anything, these factors are likely to bias us against finding positive effects of newspapers on turnout.

Population growth typically decreases voter turnout. Benny Geys (2006), for example, draws this conclusion from a meta-analysis of aggregate studies of voter turnout. A natural explanation is that movers—both newcomers to an area and those who are most likely to leave—are less rooted in their community and consequently less likely to vote. Because newspapers enter growing markets, we would expect these forces to exert a downward bias on our estimates of the effect of newspapers on turnout. Indeed, as we show below, entries are associated with downward trends in turnout, which are largely attributable to demographic changes associated with population growth.

The effects of income growth on voter turnout are less clear. Theoretically, higher income could increase turnout (if the poor feel disenfranchised) or decrease turnout (if higher income increases the value of time). Empirical results vary, with many studies finding no consistent evidence of an effect in either direction. André Blais's (2006) review of cross-national/time-series evidence concludes that "there is no clear relationship between the economic conjuncture and turnout." Kerwin Kofi Charles and Melvin Stephens, Jr. (2009) find a statistically robust negative effect of local economic performance on turnout. We therefore expect the magnitude of any bias from unmeasured income shocks to be small and most likely negative. Consistent with this expectation, we find in unreported regressions that the growth rate of manufacturing output per capita is, if anything, slightly negatively related to changes in voter turnout, an effect that is largely eliminated when we control for population growth.

An important remaining question is to what extent political drivers of turnout, such as the closeness of elections, would also affect newspaper profits. Although such effects are possible, our reading of the historical literature is that political factors are likely to be small relative to commercial considerations. Consistent with this prior, we find that the competitiveness of the presidential election at the state level, which is plausibly exogenous to any given county's news market, has a large effect on county-level turnout, but no discernible effect on the timing of entries and exits. Similarly, using data from Grant Miller (2008), in state-level regressions we find no evidence that the timing of newspaper entries and exits is related to the timing of the granting of suffrage to women, despite the latter's large effect on the size of the electorate.

## B. *Main Results*

Figure A1 in the Appendix illustrates how readership evolves around the entry of a newspaper. The figure plots estimates of the coefficients $\tilde{\alpha}^k$ from a specification analogous to equation (4) where the dependent variable is readership per eligible voter. The figure illustrates three points. First, the entry of a newspaper is associated with a large increase in readership, around 13 percentage points. Second, most of the effect of the entry of a newspaper on readership occurs on impact: dynamics after the event are small relative to the contemporaneous effect of the event. Third, there is no evidence of positive trends in per capita demand for newspapers prior to the entry of a new paper. If anything, readership per eligible voter declines prior to entry. Note that, because of growth in the voting-eligible population, *total* readership—as opposed to readership per capita—does increase before the entry of a newspaper. The fact that the only significant trends in circulation prior to entry are attributable to market scale supports our earlier claim that population and income are the main drivers of entries and exits, and that other factors such as interest in politics likely play a much smaller role.

Figure 2 presents our core result on the effect of entries and exits on turnout visually.[7] The figure plots estimates of the coefficients $\tilde{\alpha}^k$ from a specification analogous to equation (4), where the dependent variable is the change in presidential turnout per eligible voter. The prediction that newspaper entry increases voter turnout (and that newspaper exit decreases it) corresponds to a positive spike in the plot at $k = 0$. Because the plots are in changes rather than levels, a single positive spike corresponds to a permanent positive effect on the level of turnout.

Panel A shows the estimated effects of entries and exits when we include no controls other than state-year fixed effects. The solid line in the figure plots the corresponding trends in turnout as predicted from demographics $\Delta \mathbf{x}_{ct}$. The figure clearly shows a positive on-impact effect of events, with an entry corresponding to an increase in turnout of roughly 0.2 percentage points. Consistent with our expectation that rising population will be associated with falling turnout, the plot shows a significant negative pretrend, with turnout declining on average in the five periods preceding an entry. This trend is reasonably well approximated by demographics, suggesting that the figure understates the true causal effect of entries.

Panel B shows the estimated effects when we control explicitly for demographics. With these controls, there are no significant trends immediately before or after events. There is a statistically significant trend 28 years before the event.[8] In panel B, the estimated on-impact coefficient increases to roughly 0.3 percentage points.

Table 2 presents regression estimates of $\beta$ from equation (1). All models include state-year fixed effects and are estimated on our main sample period of 1868–1928. (The models do not include the leads and lags that identify the coefficients in Figure 2.) In column 1, the dependent variable $y_{ct}$ is newspaper readership per eligible voter. The positive sign of this coefficient is essentially mechanical, but its magnitude is informative about the average size of our events. The results show that the average event changes the share of eligible voters reading at least one newspaper by approximately 13 percentage points. In columns 2 and 3, the dependent variable is presidential turnout. These estimates represent the same information presented in Figure 2. With no controls, we estimate that the average event increases turnout by 0.26 percentage points. As we would expect based on Figure 2, including controls increases the coefficient to 0.34 percentage points.

Column 4 presents results for congressional turnout in presidential election years. Column 5 presents results for congressional turnout in off-year elections. In both cases the results are similar to results for presidential elections.

### C. *Robustness*

We have argued that mismeasurement of the trends in population and income that drive most entries and exits would likely lead us to underestimate the effect

[7] Because we cannot identify events prior to the 1868–1872 period, some lag terms $\Delta n_{c(t+k)}$ for $k < 0$ are unknown in years prior to 1912. We code these as having a value of zero, although the value at which we impute them does not matter because our models include state-year fixed effects. The pattern of coefficients $\beta^k$ for $k \geq 0$ is essentially unchanged when we exclude these unknown terms, and is extremely similar when we include data on changes in turnout prior to 1868 in the model.

[8] We do not have a strong explanation for this anomalous trend, but a referee suggests it may be related to the length of a generation.

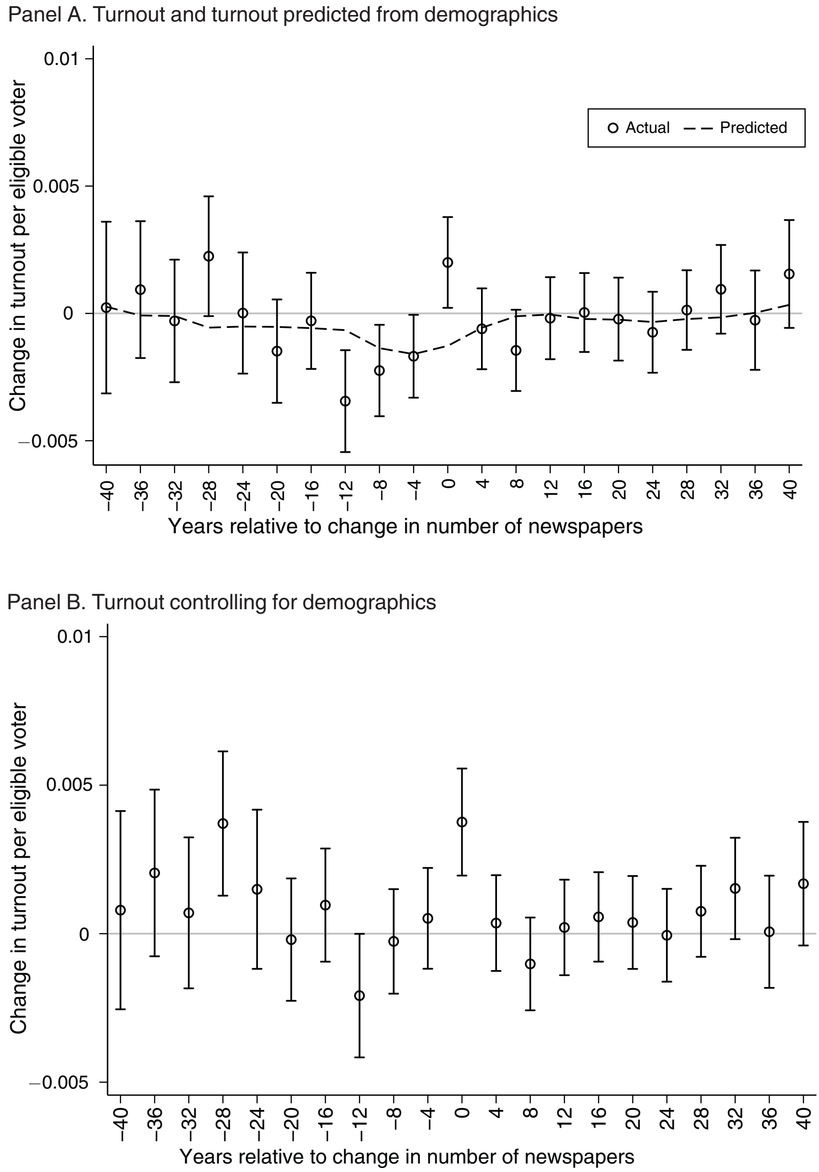


Figure 2. Presidential Turnout and Newspaper Entries/Exits

*Notes:* Panel A shows coefficients from a regression of change in turnout per eligible voter on a vector of leads and lags of the change in the number of newspapers (see equation (4) for details). "Actual" refers to estimated coefficients. "Predicted" refers to estimated coefficients using as a dependent measure the change in turnout predicted from demographics as defined in Section ID. Panel B shows coefficients from a regression of change in turnout per eligible voter, controlling for demographics, on a vector of leads and lags of the change in the number of newspapers. Models include state-year fixed effects. Error bars are ±2 standard errors. Standard errors are clustered by county. Time period is 1868–1928.

of newspapers on turnout. Table 3 provides a check on this claim by separately estimating the effect of political and nonpolitical newspapers. Nonpolitical newspapers likely respond to the same economic forces as political newspapers, but are far less likely to affect turnout. Column 1 shows that, as expected, entries of both

Table 2—The Effect of Newspaper Entry/Exit on Voter Turnout

| | Readership | Presidential turnout | | Congressional turnout | |
|---|---|---|---|---|---|
| | | | | (Pres. years) | (Off years) |
| | (1) | (2) | (3) | (4) | (5) |
| Number of newspapers | 0.1314 | 0.0026 | 0.0034 | 0.0031 | 0.0032 |
| | (0.0044) | (0.0009) | (0.0009) | (0.0011) | (0.0012) |
| Demographic controls? | Yes | No | Yes | Yes | Yes |
| $R^2$ | 0.435 | 0.569 | 0.579 | 0.521 | 0.531 |
| Number of counties | 1,181 | 1,195 | 1,195 | 1,195 | 1,192 |
| Number of county-years | 11,281 | 15,627 | 15,627 | 14,634 | 13,869 |

*Notes:* Standard errors in parentheses are clustered by county. Time period is 1868–1928. Models are estimated in first differences. All specifications include state-year fixed effects. Demographic controls are changes in county demographics as defined in Section ID, with dummies included for missing data. The dependent variable in column 4 is the change in congressional turnout between the current presidential year ($t$) and the previous presidential year ($t-4$). The dependent variable in column 5 is the "long difference": congressional turnout in the first off-year election following the current presidential election year ($t+2$) minus congressional turnout in the last off-year election prior to the previous presidential election year ($t-6$).

types of newspapers are associated with positive (and statistically indistinguishable) trends in population. Column 2 shows that only political newspapers affect turnout: the effect of nonpolitical newspapers on turnout is negative, statistically insignificant, and statistically distinguishable from that of political newspapers at the 10 percent level.

We have also argued that our estimates are not likely to be biased by trends in interest in politics correlated with the entries of newspapers. As a test of this claim we have estimated the effect of an entry on the turnout in counties elsewhere in the same state. Counties in the same state exhibit highly correlated trends in turnout, likely due to institutional factors such as the Electoral College. These counties have relatively distinct news markets, however, although we caution that some spillover in circulation is likely present in most cases. Nevertheless, we find a small and statistically insignificant positive relationship between entry of newspapers in a county and changes in turnout in other counties in the same state.

In Appendix B, we show that our estimates of turnout effects are robust across a range of alternative specifications. The estimates are larger for events involving large papers than for events involving small papers, larger when we focus on the subsample of isolated counties that contain only a single city (where our market definition is likely to be most accurate), and larger when we truncate $\Delta n_{ct}$ to vary between $-1$ and 1. They are similar when we estimate the cumulative effect over the period of the event and one subsequent period, suggesting that most of the effect we measure happens on impact. Estimates are robust to allowing flexible polynomial trends in outcome variables around events, as well as allowing restricted trends whose time path is based on the relationship between observed state variables and entry and exit.

We have also explored the heterogeneity in the effect of newspapers on turnout. First, we have estimated specifications that allow increases and decreases in the number of newspapers to affect turnout differently, but find no evidence of such an asymmetry (see online Appendix). Second, we have estimated models allowing affiliated and unaffiliated newspapers to affect turnout differently, and again find no

Table 3— Turnout Effects of Political and Nonpolitical Newspapers

| | Log(voting-eligible population) (1) | Presidential turnout (2) |
|---|---|---|
| Number of political newspapers | 0.0072 | 0.0037 |
| | (0.0013) | (0.0010) |
| Number of nonpolitical newspapers | 0.0137 | −0.0022 |
| | (0.0046) | (0.0028) |
| *F*-test of equality of coefficients | 2.122 | 3.639 |
| *p*-value | 0.1454 | 0.0567 |
| $R^2$ | 0.816 | 0.579 |
| Number of counties | 1,195 | 1,195 |
| Number of county-years | 15,627 | 15,627 |

*Notes:* Standard errors in parentheses are clustered by county. Time period is 1868–1928. Models are estimated in first differences. All specifications include state-year fixed effects and demographic controls as defined in Section ID, with dummies included for missing data. Nonpolitical newspapers are those that identify themselves as commercial, financial, legal, trade, or other types of publications unlikely to emphasize political news. All other newspapers are classified as political. See Section I for details.

evidence of heterogeneous effects. Third, consistent with Strömberg's (2004) finding that the turnout effects of radio are greater in more competitive elections, we find that the estimated effect of an additional newspaper on presidential turnout is greater when the two-term convention prevents the incumbent president from seeking reelection, although the difference in coefficients is not statistically significant. Fourth, following Strömberg (2004), we have estimated the effect of a newspaper separately for counties with and without exclusively rural populations, and find no statistically distinguishable difference between the two (although we note the test has limited power).

We have confirmed that our use of turnout per eligible voter as a dependent measure is not driving our results. We have estimated a specification with log(total votes) as the dependent measure and log(voting-eligible population) as a control. The estimated effect of newspapers on turnout is highly statistically significant and comparable in magnitude to our main estimate.

### D. *Interaction with Market Structure*

Table 4 shows how our estimated effects vary with the extent of market competition and ideological diversity. The model in columns 1 and 2 is identical to the one in the previous table, except that the independent variables of interest are a set of indicators for the number of newspapers in the county. These interactions are identified by variation in the effect of entries/exits on turnout according to the number of newspapers in the county at time $t - 1$. If there are no other sources of heterogeneity in the effect of newspaper entries/exits that are correlated with the number of newspapers prior to the event, then (under our other maintained assumptions) these parameters can be taken as causal estimates of the effect of the number of competing newspapers on voter turnout.

Column 1 shows the effect on readership. The entry or exit of a county's first newspaper changes the share of eligible voters reading at least one newspaper by

Table 4—Turnout Effects by Number of Newspapers and Ideological Diversity

| | Readership | Presidential turnout | |
|---|---|---|---|
| | (1) | (2) | (3) |
| County has: | | | |
| $\geq$ 1 newspaper | 0.2470 | 0.0098 | 0.0121 |
| | (0.0082) | (0.0027) | (0.0033) |
| $\geq$ 2 newspapers | 0.1030 | −0.0018 | −0.0050 |
| | (0.0068) | (0.0022) | (0.0032) |
| $\geq$ 3 newspapers | 0.0710 | 0.0052 | 0.0030 |
| | (0.0067) | (0.0024) | (0.0034) |
| At least one Republican and one Democratic paper | | | −0.0023 |
| | | | (0.0040) |
| $F$-test of equality of coefficients | 146.8 | 5.949 | — |
| $p$-value | 0.0000 | 0.0027 | — |
| $R^2$ | 0.500 | 0.579 | 0.578 |
| Number of counties | 1,181 | 1,195 | 1,168 |
| Number of county-years | 11,281 | 15,627 | 12,515 |

*Notes:* Standard errors in parentheses are clustered by county. Time period is 1868–1928. Models are estimated in first differences. All specifications include state-year fixed effects and demographic controls as defined in Section ID, with dummies included for missing data.

25 percentage points on average. The marginal effect of second and third newspapers is significantly smaller, both because the circulation of these papers is typically lower and because some individuals who read a second or third paper already read the first.

Column 2 shows the effect on turnout. The entry or exit of the first paper has a significant positive effect of 1.0 percentage point. Subsequent entries have smaller marginal effects that are not consistently significantly different from zero. We can reject the null hypothesis that the marginal effect of second and subsequent newspapers are as large as the marginal effect of the first. The relative magnitudes of the $\geq 3$ and $\geq 1$ newspaper coefficients are reasonably close to what we would expect based on column 1 if the effects were proportional to the increase in readership, while the $\geq 2$ newspaper coefficient is smaller than we would expect.

Column 3 tests the hypothesis that ideological diversity promotes political participation. In addition to the interactions with number of newspapers in the market, we include an indicator for whether the county has at least one Republican and one Democratic newspaper. To facilitate interpretation, we restrict analysis to counties with no unaffiliated newspapers. We cannot reject the null hypothesis that there is no additional effect of ideological diversity.

## E. *Changes over Time*

Table 5 shows how our estimated effects change over time. We estimate a single regression with separate effects for events in each of our three time periods. Of course, many things are changing over time, and we cannot attribute differences across these periods definitively to the effects of radio and television per se.

Table 5—Turnout Effects Over Time

| | Readership (1) | Presidential turnout (2) | Congressional turnout (pres. years) (3) | Congressional turnout (off years) (4) |
|---|---|---|---|---|
| County has $\geq$ 1 newspaper: | | | | |
| Newspaper period | 0.2591 | 0.0084 | 0.0070 | 0.0110 |
| (1868–1928) | (0.0084) | (0.0027) | (0.0030) | (0.0031) |
| Radio period | 0.4512 | 0.0054 | 0.0042 | 0.0051 |
| (1932–1952) | (0.0191) | (0.0032) | (0.0042) | (0.0043) |
| Television period | 0.4681 | 0.0010 | 0.0064 | 0.0070 |
| (1956–2004) | (0.0161) | (0.0021) | (0.0054) | (0.0042) |
| $F$-test of equality of coefficients | 90.87 | 2.466 | 0.151 | 0.711 |
| $p$-value | 0.0000 | 0.0852 | 0.860 | 0.491 |
| $R^2$ | 0.492 | 0.602 | 0.497 | 0.602 |
| Number of counties | 1,489 | 1,489 | 1,486 | 1,486 |
| Number of county-years | 41,840 | 46,899 | 38,130 | 37,339 |

*Notes:* Standard errors in parentheses are clustered by county. Time period is 1868–2004 for presidential turnout and 1868–1990 for congressional turnout. Models are estimated in first differences. All specifications include state-year fixed effects and demographic controls as defined in Section ID, with dummies included for missing data. The dependent variable in column 3 is the change in congressional turnout between the current presidential year ($t$) and the previous presidential year ($t - 4$). The dependent variable in column 4 is the "long difference": congressional turnout in the first off-year election following the current presidential election year ($t + 2$) minus congressional turnout in the last off-year election prior to the previous presidential election year ($t - 6$).

Column 1 of Table 5 shows that the effect of entries and exits on readership grows larger over time, reflecting the larger circulation of entering and exiting papers in later years.

Column 2 presents results on presidential turnout. As predicted, the results are strongest in the period before radio or television, with the average event increasing turnout by 0.8 percentage points.[9] The point estimate for the radio period is smaller and marginally statistically significant. The coefficient in the television period is almost exactly zero. We can reject the equality of the coefficients across the three periods at the 10 percent level.

Columns 3 and 4 show how the changes over time differ for congressional turnout. The precision of these estimates does not permit us to make strong statements about the relative magnitudes, but the results are consistent with newspapers in the television era being more important for congressional elections than for presidential elections. The point estimate for the effect of entries and exits on congressional turnout in the television period is larger than for presidential turnout (and marginally significantly different from zero in off-year elections, though not in presidential years). We cannot rule out that the effect of newspapers on congressional turnout is constant over these three periods.

[9] The estimate for the newspaper period in Table 5 is slightly smaller than the analogous estimate from Table 4. Following the criteria in Section IE, the sample in Table 4 includes the set of counties that experience at least one four-year period in which the number of newspapers increases or decreases during the newspaper period (1868–1928). The sample in Table 5 includes the set of counties that experience at least one such change during the entire period (1868–2004). The latter is by construction a broader set of counties. Defining the sample in this way allows us to estimate our model for a consistent set of counties over time, but results in a slightly smaller estimate of the effect of newspapers on turnout during the newspaper period.

### F. *Discussion of Magnitudes*

Our estimates suggest that introducing a newspaper to a county without one raises presidential turnout per eligible voter by about 1.0 percentage point. It raises the share of individuals reading at least one newspaper by 25 percentage points. Following the logic of Gerber and Donald P. Green's (2000) intent-to-treat calculation, our point estimate corresponds to a $(1.0/0.25) = 4.0$ percentage point effect of reading the newspaper on the probability of voting.

We can also translate our estimates into a "persuasion rate" (DellaVigna and Kaplan 2007)—the number of eligible voters who changed their voting behavior as a result of the introduction of the newspaper, as a fraction of all those who could have changed their behavior. To do this, we assume: (i) the effect of the introduction of the newspaper on the likelihood of voting is nonnegative for all eligible voters; and (ii) voters and nonvoters are equally likely to read the newspaper. In the average county-year for counties with no newspaper, 69 percent of eligible voters vote, so, by assumption: (i) only 31 percent of eligible voters could have their behavior altered by the entry of a newspaper; then, by assumption, (ii) among those who would not otherwise vote, 25 percent read the newspaper, representing 7.7 percent of eligible voters. The 1.0 percent of eligible voters who vote as a result of the introduction of the newspaper therefore implies a persuasion rate of $(1.0/0.077) = 12.8$ percent.

Both the intent-to-treat and persuasion rate estimates would increase if fewer than two eligible voters read each newspaper circulated, as seems plausible given the documented overreporting of news consumption in survey data (Prior 2009). These estimates would also increase if we accounted for error in market definition. If, for example, a newspaper sends only 80 percent of its copies to its home county (as is true for the median newspaper today), these estimates would increase by a factor of $(1/0.8) = 1.25$.

For comparison, DellaVigna and Gentzkow (2010) summarize effect sizes and persuasion rates for a number of recent randomized get-out-the-vote experiments. The persuasion rates range from a low of 1 percent ($ITT = 0.6$ percentage points) for direct-mail solicitations in Gerber and Green (2000), to highs of 16 percent ($ITT = 8.6$ percentage points) and 21 percent ($ITT = 6.9$ percentage points) for face-to-face and by-phone solicitations in Gerber and Green (2000) and Gerber and Green (2001), respectively.

Our estimates lie toward the low end of the spectrum of recent estimates by economists of the effect of media on voter turnout. Strömberg (2004) finds that increasing the share of households with radios from 0 to 1 increased turnout in gubernatorial races by 7 percentage points in the 1920–1940 period. Felix Oberholzer-Gee and Joel Waldfogel (2009) find that the introduction of Spanish-language local television increases turnout among Hispanics in a metro area by 5 to 10 percentage points.

Gentzkow (2006) finds that the introduction of television in the 1940s and 1950s reduced turnout, and hypothesizes crowding out of newspapers and radio as a possible mechanism. The estimated cumulative effect of television on turnout over ten years is equal to 2 percentage points for congressional elections and 0.7 percentage points for presidential elections (with the latter effect not significantly different from zero). Gentzkow (2006) estimates that the introduction of television reduced the likelihood of getting campaign information from a newspaper by 11.4 percentage points. Applying our intent-to-treat estimates, this would imply a reduction in voter turnout of about $(11.4)(0.04) = 0.5$ percentage points, or one-quarter of the estimated effect of television, with the rest attributable to crowd-out of radio or other mechanisms.

As another benchmark, Brad T. Gomez, Thomas G. Hansford, and George A. Krause (2007) find that rain on election day reduces turnout by about 0.12 percentage points on average. (The average rainy election day has 0.14 inches greater than the normal amount of rain for the county, and the effect of rain on turnout is 0.89 percentage points per inch.) Thus, the effect of a monopoly newspaper closing is about eight times larger than an average rainy election day.

Finally, we note that the effect of introducing a newspaper to a county without one is 8 percent of the standard deviation of the change in voter turnout between presidential elections, and 12 percent of the root mean squared error of our model.

## V. Effect of Newspapers on Party Vote Shares

### A. *Specification, Mechanisms, and Potential Confounds*

In this section, we define $y_{ct}$ to be the share of the presidential or congressional two-party vote won by Republicans.

We define the main independent variable to be the change in the difference $(\#Rep - \#Dem)$, where $\#Rep$ is the number of Republican papers in the market and $\#Dem$ is the number of Democratic papers.[10] We estimate all models in first differences.

The literature suggests several mechanisms by which partisan newspapers may shift vote shares in favor of their preferred candidates. Even if voters are rational, papers may filter or slant information so as to systematically persuade voters to shift their allegiances (Valentino Larcinese, Riccardo Puglisi, and Snyder 2007; Puglisi and Snyder 2008; Knight and Chiang 2008; Emir Kamenica and Gentzkow 2011). If voters underestimate the bias of outlets (Erik Eyster and Matthew Rabin 2009), make errors in updating their beliefs (Mullainathan, Joshua Schwartzstein, and Shleifer 2008), or are subject to other nonrational forms of persuasion (Richard E. Petty and John T. Cacioppo 1996), the scope for media influence will be even greater. A substantial literature in political science adds the further observation that even if media do not change voters' views on particular issues, they may still influence their votes by determining which issues are most salient—i.e., by determining the political "agenda" (Maxwell E. McCombs and Donald L. Shaw 1972). Finally, partisan media may influence vote shares as a by-product of the turnout effects demonstrated in the last section. Since partisan papers are read most by those who start out sharing the papers' views (Gentzkow and Shapiro 2010; Durante and Knight 2009), turnout effects on readers could also shift party vote shares.

[10] This specification restricts the effect of Republican and Democratic newspapers to be equal and opposite. We have tested that restriction and find that we cannot reject it. The specification also ignores any effect of the total number of newspapers (irrespective of affiliation) on the vote share. In unreported tests, we find no evidence for such an effect.

Although the basic intuition that Republican papers should increase Republican vote shares seems transparent, theory suggests an important subtlety in a world where newspapers choose their party affiliations endogenously. Consider a simple model where voters are distributed on a continuum from conservative to liberal, each newspaper also has a location on this continuum, and the effect of newspapers is to pull voters closer to their own location. If newspaper locations were assigned randomly, we would expect the entry of a right-wing monopolist to shift voters to the right on average. If newspapers choose their locations to maximize profits, however, a right-wing monopolist is probably coming into a market where the median voter is already right-wing. Such an entry might have little or no effect on vote shares. In this view, we might predict significantly larger effects for duopoly papers, since they are more likely to be located away from the median voter.

The endogenous positioning of newspapers is also the most obvious confound to our estimation strategy. All else equal, a market where a Republican paper enters (or a Democratic paper exits) is likely to be a place where voters are becoming more Republican for reasons that have nothing to do with newspapers. This suggests that, if anything, $\lambda > 0$, resulting in a positive bias in our estimate of $\beta$. Thus, our estimates will overstate the persuasive effects of newspapers.

### B. *Main Results*

Figure 3 shows the effect of entry and exit of partisan papers on party vote shares visually for our main sample period of 1868–1928. The figure plots estimates of the coefficients $\tilde{\alpha}^k$ from a specification analogous to equation (4), where the dependent variable is the change in the share of votes going to Republicans. The prediction that having a Republican newspaper shifts votes to Republicans corresponds to a positive spike in the plot at $k = 0$.

Panel A shows the estimated effects when we include no controls. The solid line in the figure plots the corresponding trends in turnout predicted from state-year fixed effects $\Delta\gamma_{st}$. The figure shows no evidence of an on-impact effect of events. Consistent with what we would expect, there is a broad positive trend associated with net entry of Republican papers. The majority of estimated coefficients is above the line, suggesting that Republican papers tend to enter markets where the Republican vote share is growing. The estimated effect in the year of the event is not significantly different from zero, however, and is no larger in magnitude than the estimated effect two periods prior to, or after, the event.

Panel B shows that when we control for state-year fixed effects, the positive trends in Republican vote share around the net entry of a Republican paper essentially vanish. There is no distinguishable on-impact effect of entry on vote shares.

Table 6 presents our main results, for the period 1868–1928. Column 1 presents the coefficient from a specification where the dependent variable is the difference between the share of circulation going to Republican papers and the share going to Democratic papers. (A graphical representation of this specification is available in the online Appendix to this paper.) This is one measure of the magnitude of the effect of an average event on the distribution of content that voters are actually reading. If all events were monopoly entries or exits and all newspapers were affiliated with a party, the value of this coefficient would be one mechanically. The

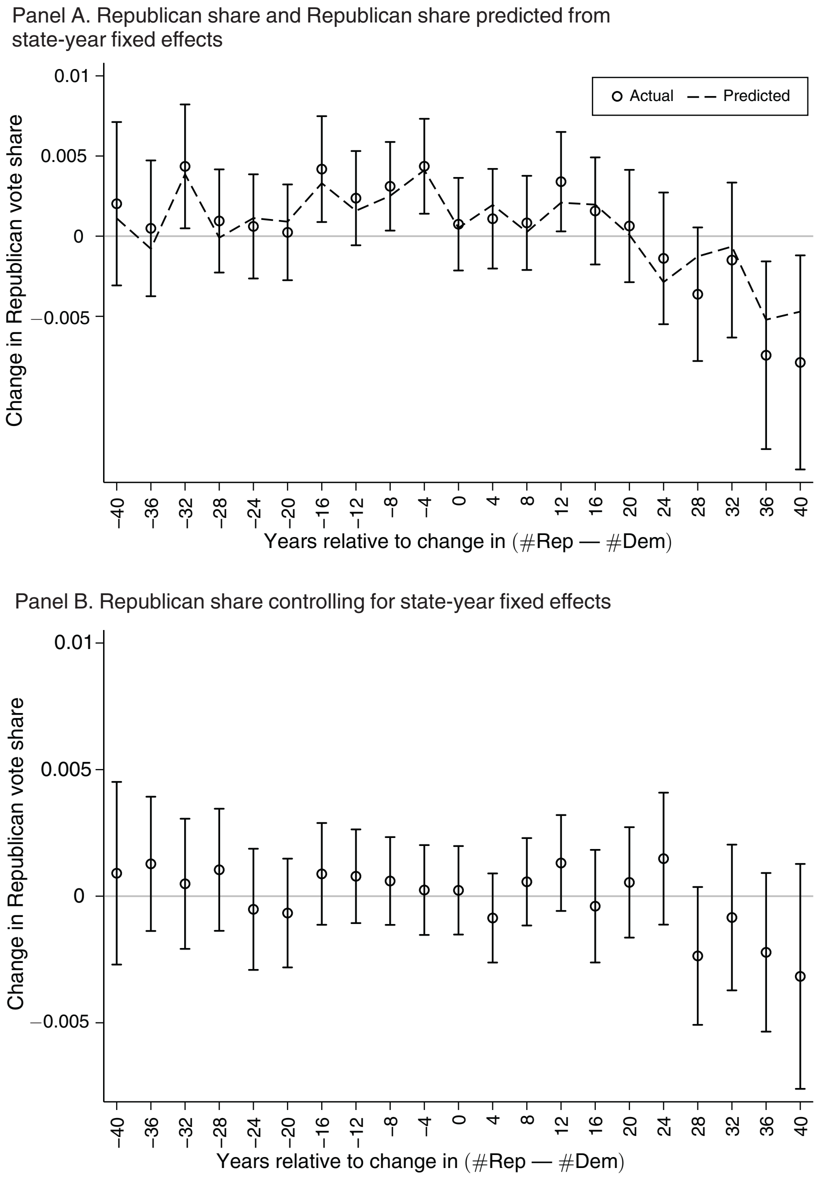


Figure 3. Republican Vote Share and Newspaper Entries/Exits

*Notes:* Panel A shows coefficients from a regression of change in Republican share of two-party vote on a vector of leads and lags of changes in the difference in the number of Republican and Democratic newspapers (see equation (4) for details). "Actual" refers to estimated coefficients. "Predicted" refers to estimated coefficients using as a dependent measure the change in Republican share of two-party vote predicted from state-year fixed effects alone. Panel B shows coefficients from a regression of change in Republican share of two-party vote, controlling for state-year fixed effects, on a vector of leads and lags of changes in the difference in the number of Republican and Democratic newspapers. Error bars are ±2 standard errors. Standard errors are clustered by county. Time period is 1868–1928.

estimated coefficient shows that the average event shifts the balance of readership by 51 percentage points.

Columns 2 and 3 show the effects of partisan papers on presidential vote shares. The coefficients are almost exactly zero, with confidence intervals that rule out

Table 6—The Effect of Partisan Newspaper Entry/Exit on Republican Vote Share

| | Circulation Rep share – Dem share | Presidential vote share | | Congressional vote share |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (# Rep – # Dem) newspapers | 0.5096 | 0.0002 | 0.0002 | 0.0021 |
| | (0.0135) | (0.0010) | (0.0010) | (0.0018) |
| Demographic controls? | yes | no | yes | yes |
| $R^2$ | 0.586 | 0.735 | 0.736 | 0.351 |
| Number of counties | 1,181 | 1,195 | 1,195 | 1,191 |
| Number of county-years | 11,281 | 15,401 | 15,401 | 14,295 |

*Notes:* Standard errors in parentheses are clustered by county. Time period is 1868–1928. Models are estimated in first differences. All specifications include state-year fixed effects. Demographic controls are changes in county demographics as defined in Section ID, with dummies included for missing data.

positive effects greater than about 0.2 percentage points. The coefficient is unaffected by the inclusion of demographic controls.

Column 4 shows the estimated effect on congressional vote share. Here, the point estimate is slightly positive, but still far from statistical significance. The standard errors are larger here, and we are able to rule out positive effects greater than 0.6 percentage points.

In Appendix B, we show that our finding of no statistically significant vote share effects is robust across a range of alternative specifications. In the online Appendix, we find no statistically significant vote share effects when we strengthen our definition of partisanship to include only newspapers that report their partisan affiliation in all years of their existence.

In the online Appendix, we also show how the effect of newspapers on party vote shares varies with market competition. In no case do we detect a significant effect of entry and exit events. If anything, the coefficient on duopoly papers tends to be larger than the coefficient on monopoly papers, possibly reflecting the role of endogenous positioning. We cannot reject, however, the hypothesis that all the coefficients are equal to zero, either individually or jointly.

Our ability to study changes over time in persuasive effects is more limited than for turnout effects, because partisan affiliation is less common in later years of our sample. That said, we find no significant effects in the newspaper, radio, or television periods. We omit these results for brevity.

## C. *Discussion of Magnitudes*

To get a sense of the size of the persuasive effects our estimates rule out, consider a county with a single Democratic newspaper. Changing the local newspaper to be Republican (which would increase the share of mentions devoted to Republican candidates by 18 percentage points) represents a change of two units (from $-1$ to 1) in the independent variable. We can therefore rule out an effect of this change on the Republican share of the two-party vote of about 0.4 percentage points. Following our calculations in Section IV, this corresponds to an intent-to-treat effect of $(0.4/0.25) = 1.7$ percentage points. In an otherwise evenly split county, this corresponds to a persuasion rate of about 3.4 percent provided that the newspaper is equally likely to be read by voters of both parties. (The persuasion rate would be smaller under the more realistic assumption that Republicans are more likely to read the Republican paper.)

A few caveats are worth noting. First, the calculations above assume an evenly split county. In reality the average entry of a Republican paper is into a county that leans Republican; adjusting for this factor increases the persuasion rate to 3.5 percent. Second, the estimated persuasion rate is greater the fewer households in the county that read the newspaper. Therefore measurement error, and in particular error in defining the newspaper's market, would bias this calculation downward. If, for example, only 80 percent of a newspaper's circulation is in its home county (as is true for the median newspaper today), the persuasion rate should be scaled up by a factor of $(1/0.8) = 1.25$.

For comparison, DellaVigna and Kaplan's (2007) estimate of the effect of Fox News on the Republican presidential vote share implies a persuasion rate of 11.6 percent. Enikolopov, Petrova, and Zhuravskaya (2009) estimate effects of an independent anti-Putin broadcaster in Russia on Putin's party's vote, finding a persuasion rate of 10.2 percent. Gerber, Karlan, and Bergan (2009) find that randomly assigned subscriptions to the *Washington Post* decreased the Republican gubernatorial vote share in Virginia by 11.2 percentage points; this implies a persuasion rate of about 20 percent if we assume all individuals who were given subscriptions read the paper. Our confidence interval thus easily rules out the hypothesis that the entry or exit of an average monopoly newspaper has effects of the same size as any of these experiments.

As another benchmark, Gomez, Hansford, and Krause (2007) estimate that a typical rainy day increases the Republican vote share by 0.33 percentage points. (They find that an inch of rain above normal in a county raises the Republican share of the vote by 2.4 percentage points and that a typical rainy day has about 0.14 inches of rain more than normal.) The upper end of our confidence interval thus corresponds to an effect 1.3 times as large as an average day of rain.

As a final comparison, we find that the upper bound of our confidence interval corresponds to 2 percent of the standard deviation of the change in the Republican share of the two-party vote, and 3 percent of the root mean squared error of our model.

A separate question is whether our null estimates in this section are consistent with the significant turnout effects we estimate in Section IV. As discussed above, if readers differentially prefer newspapers whose affiliation matches their own ideology, a positive turnout effect by itself implies we should see some effect on vote shares. To get a sense of the magnitude of the vote share effect we would expect based on our turnout estimates, consider the entry of a monopoly Republican paper to a 50 percent Republican market and make the following assumptions: (i) newspapers cannot change the share of Republicans in the population; (ii) in the absence of newspapers, turnout is identical for Republicans and Democrats; (iii) newspapers increase turnout of all readers by the same amount. The change in the Republican vote share $y$ caused by the newspaper's entry is

$$\Delta y = \frac{\gamma(c_R - c_D)}{4t},$$

where $\gamma$ is the effect of reading a newspaper on turnout, $c_R$ and $c_D$ are the readership rates of Republicans and Democrats, respectively, and $t$ is the postentry turnout rate for the population as a whole. Given our intent-to-treat estimate of $\gamma = 0.040$ (Section IVF) on turnout, overall average turnout in markets with no newspaper of $t = 0.69$, and average readership share of 25 percent (Table 4, column 1), Republicans would need to be four times more likely to read the paper than Democrats to generate a vote share effect of 0.4 percentage points. Our vote share and turnout estimates are, therefore, consistent for even large differences in the propensity to read.

## VI. Effect of Newspapers on Incumbency Advantage

In this section, we define $y_{ct}$ to be either the share of the congressional two-party vote won by the incumbent candidate or an indicator of whether the congressional incumbent is running unopposed. We exclude cases in which the incumbent in the current or previous election is either not running or is not from one of the two main parties, or in which the state was redistricted between the current and previous election. We conduct the analysis at the level of the congressional district. Our independent variables are defined as in Section IV, and we estimate all models in first differences.

A particular media outlet could either increase or decrease the vote share of incumbents, depending on how it compares to alternative communication channels. If voters are rational, introducing more accurate information about the performance of incumbents will tend to help good incumbents and harm bad incumbents, and need have no clear effect on average (Sanford C. Gordon and Dimitri Landa 2009).

Newspapers could increase incumbency advantage if they make winning an earlier election a more informative signal of quality (Scott Ashworth and Ethan Bueno de Mesquita 2008) or if they increase noninformational advantages of incumbency such as name recognition (Ansolabehere, Snowberg, and Snyder 2006; Prior 2006).

Newspapers could decrease incumbency advantage if incumbents control alternative means of reaching voters. Moreover, if incumbents have a differential ability to bribe or otherwise "capture" information outlets, making media markets more competitive will tend to limit their influence (Timothy Besley and Andrea Prat 2006). Such theories are ambiguous about the influence of a first newspaper (at least in a world with no television or radio), but predict strong effects of second and later entrants.

The link between media and incumbency is made more complicated by the possibility that both challengers and incumbents respond endogenously to changes in the media market. If the media make communication by challengers easier, for example, challengers may be more likely to contest races in the first place. The quality of candidates who choose to enter may also be higher. These responses would serve as a "multiplier" to the baseline effect of media. On the other hand, incumbents might respond to media scrutiny by reducing corruption or exerting more effort in satisfying constituent interests. This would be an important positive effect of the media that we would not measure. It would also tend to reduce the size of the effects we would see on incumbent vote shares.

Table 7— Incumbency Effects by Number of Newspapers

| | Congressional incumbent vote share (1) | Uncontested incumbent (2) |
|---|---|---|
| Effect of having: | | |
| ≥ 1 newspaper | −0.0170 | −0.0432 |
| | (0.0183) | (0.0597) |
| ≥ 2 newspapers | 0.0019 | −0.0139 |
| | (0.0112) | (0.0339) |
| ≥ 3 newspapers | −0.0136 | −0.0193 |
| | (0.0112) | (0.0285) |
| $R^2$ | 0.583 | 0.388 |
| Number of districts | 319 | 355 |
| Number of district-years | 901 | 1206 |

*Notes:* Standard errors in parentheses are clustered by district. Time period is 1868–1928. Models are estimated in first differences. All specifications include state-year fixed effects and demographic controls as defined in Section ID, with dummies included for missing data. Congressional incumbent vote share is the share of the two-party vote received by the incumbent candidate. Uncontested incumbent is a dummy for whether the incumbent candidate received more than 95 percent of the total vote. Sample excludes district-years in which there is no incumbent in the current or previous presidential election year, the incumbent in the current or previous presidential election year was neither Republican nor Democratic, or there was a redistricting in the state between the current and previous presidential election year.

Potential bias in our incumbency estimates would require that newspaper entries and exits be correlated with drivers of party vote shares in a way that differs depending on the party affiliation of the incumbent. As long as newspapers respond primarily to economic forces, the scope for such bias seems limited. Bias could be introduced if incumbents themselves fund newspaper entries and are systematically more likely to do so when their support is either growing or shrinking. At least some contemporary evidence (Gentzkow and Shapiro 2010) casts doubt on incumbents' influence on news content. While we cannot rule out such confounds, we expect their effect on our estimates to be limited.

Table 7 presents our main results in this section, showing the effect of newspaper competition on our measures of incumbency advantage in our main sample period of 1868–1928. Our point estimates generally indicate that additional newspapers reduce the incumbent's vote share and the likelihood that the incumbent runs unopposed. The estimates are statistically insignificant. (The online Appendix presents a graphical analysis.)

At the point estimates, having a newspaper reduces the incumbency advantage by about 1.7 percentage points. For comparison, the much-discussed increase in the incumbency advantage in the post-WWII United States took the incumbency advantage from 2 percentage points in the 1940s to 8 percentage points in the 1990s (Ansolabehere and Snyder 2002). Snyder and Strömberg (2008) find that greater press coverage increases the incumbency advantage by about 1 percentage point. Prior (2006) argues that the diffusion of television increased the incumbency advantage by about 2–3 percentage points, although Ansolabehere, Snowberg, and Snyder (2006) use a different methodology and find that television had no effect.

In unreported specifications, we find no statistically significant evidence of incumbency effects when we aggregate all changes in the number of newspapers (rather than breaking these out according to the degree of competition in the market), and no evidence of variation in the incumbency effects of newspapers over time. The absence of a clear incumbency effect is robust across a range of alternative specifications, analogous to those in Appendix B.

## VII. Conclusions

Policy has long been built on assumptions about the way media markets influence politics. We introduce a new dataset on the history of US newspapers and use it to test the influence of media in three channels. Focusing on the period from 1869 to 1928, we show that newspapers have a robust positive effect on political participation. We estimate that one additional newspaper increases both presidential and congressional turnout by approximately 0.3 percentage points. The effect on presidential turnout diminishes after the introduction of radio and television, while the estimated effect on congressional turnout remains similar up to recent years. Newspaper competition is not a key driver of turnout: our effect is explained mainly by the first newspaper in a market, and the effect of a second or third paper is significantly smaller. Second, we show that the persuasive impact of partisan newspapers is limited. We find no evidence that these papers sway large numbers of voters to support one party or the other, with confidence intervals that rule out even moderate-sized effects. Finally, we find no clear evidence that newspapers systematically help or hurt incumbents.

These results are consistent with a model in which newspapers affect the political process mainly by providing information. In a market with no newspapers and no alternative media sources, turnout is depressed because voters have limited information about issues, candidates, and elections. The first newspaper has a large effect on turnout because it has a large effect on information. Second and third newspapers have smaller effects, and newspapers in the television and radio periods are relatively less important for national elections that these alternative media cover heavily. Our finding of limited persuasive effects is consistent with consumers filtering partisan information when it is clearly labeled as such. That newspapers do not systematically help or hurt incumbents is consistent with informational theories of incumbency advantage.

All of our results concern average effects over a large sample of years, markets, and events. They do not rule out the possibility that particular papers had effects that were either larger or smaller than what we estimate. This is an important caveat to keep in mind when comparing our estimates to those in the literature, and when applying lessons from this study to policy.

A second important caveat is that none of our electoral outcomes can be interpreted as unambiguously increasing or decreasing welfare. Measures such as turnout and incumbency advantage are extensively studied empirically and are often taken as measures of the performance of political markets. But theoretically they need not be positively related to welfare in all cases. A useful next step would therefore be to connect the changes in media markets that we exploit to more concrete measures of public policy and of the performance of public officials.

A third important caveat is that our measure of partisanship is the explicit political affiliation of a newspaper. Theory and evidence (DellaVigna and Kaplan 2007; Knight and Chiang 2008) show that implicit and explicit bias may have different effects on voting outcomes, and this contrast may be especially important in generalizing our results on partisan effects to present-day media that do not declare their biases explicitly.

## Appendix A: Construction of US Newspaper Panel

Data for 1869 through 1876 are from G. Rowell and Co.'s *American Newspaper Directory* ("Rowell's"). Data for 1880 through 1928 are from N. W. Ayer and Son's *American Newspaper Annual* ("Ayer's"). Data for 1932 through 2004 are from *Editor and Publisher Yearbook* ("E&P"). We scanned the volumes for 1869 and each presidential election year thereafter to PDF, and had them converted to machine-readable text by a data entry firm. For Rowell's and Ayer's (where the text is of poorer quality) the firm keyed each record twice independently and reconciled discrepancies. We used audits and automated scripts to detect and correct remaining errors.

These sources are considered authoritative and are often used in historical research. To evaluate their coverage, we measured mutual consistency in 1932, where we have data from both E&P and Ayer's. Only 1.3 percent of E&P papers could not be found in the Ayer's directory, and only 1.6 percent of the Ayer's papers could not be found in E&P.

A data entry firm entered data from the directories on all daily newspapers. For the Rowell's and Ayer's directories, some judgment is required in deciding which newspapers are dailies. To check the quality of these judgments, we examine data for the years 1908–1928, in which the Ayer's directories are supplemented with a separate (and separately keyed) listing of all daily newspapers (the "Daily Lists"). Of newspapers listed in the Daily Lists, 0.09 percent were not found in the data entered from the Ayer's directories. Of newspapers listed in the Ayer's directories, 1.76 percent were not found in the Daily Lists. (Most of these were daily student newspapers which were excluded from the Daily Lists because they do not publish year-round.)

We conducted random audits of the quality of the data keyed by the data entry firm for the Rowell's and Ayer's directories. We estimate that the error rate in keying is less than 1 in 5,000 characters. When possible we identified and corrected typos in the course of producing data for analysis.

Our dataset includes English-language daily newspapers. We consider a newspaper to be a daily if it circulates at least four or more weekdays each week. In cases where newspapers published multiple editions, typically both a morning and evening edition, we treat those as separate records. We exclude foreign-language newspapers, national newspapers (the *Christian Science Monitor*, *Wall Street Journal*, and *USA Today*), and some clearly nonnews publications (e.g., real estate listings, live stock listings, etc.) from the sample.

We extract data on daily (weekday) circulation of newspapers. Some records report circulation values from multiple sources. Whenever possible, we use figures provided by the Audit Bureau of Circulations. We employ a consistent hierarchy of sources when such a figure is not available. We identified cases with unusual changes in circulation as likely errors, and corrected any incorrect values.

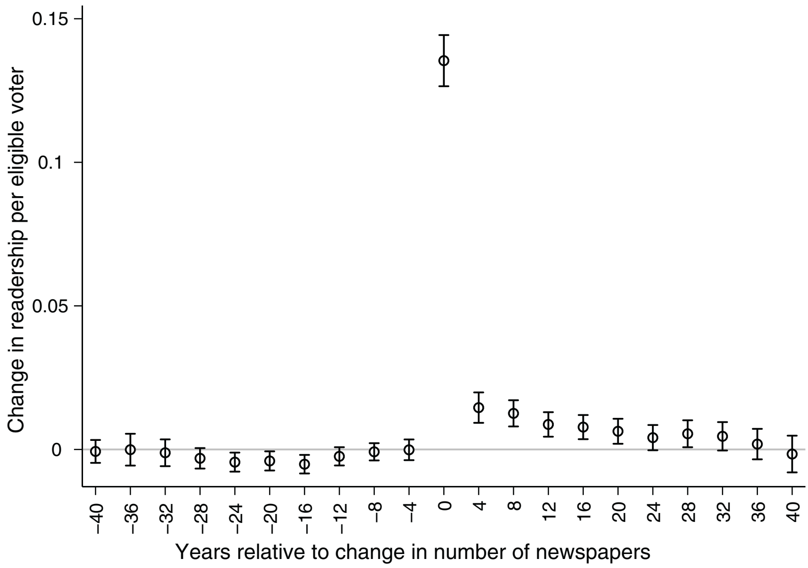


Figure A1. Readership per Eligible Voter and Newspaper Entries/Exits

*Notes:* Figure shows coefficients from a regression of change in readership per eligible voter on a vector of leads and lags of the change in the number of newspapers (see equation (4) for details). Models include state-year fixed effects. Error bars are $\pm 2$ standard errors. Standard errors are clustered by county. Time period is 1868–1928.

We use an automated script (supplemented with manual corrections) to match newspapers across years on the basis of their title, city, and time of day. The script allows for some inexact matches and tries to identify cases in which multiple newspapers merge to form a new paper. We use this matching to construct a time-constant classification of political affiliation for the newspapers in our sample, as described in Section I. We do not attempt to account for newspapers that move across cities; random-case audits suggest that such moves are rare.

The directories associate each newspaper with a city. We use an automated script (supplemented with manual corrections) to match listed city names to census-defined places. We use the 1990 Geographic Identification Code Scheme (GICS) data from the US Census Bureau to match census places to counties. In cases where there were multiple counties assigned to a place, we chose the county that was home to the highest proportion of the place's population.

This approach assumes that county boundaries are constant over time, when in fact they do sometimes change. Data from Interuniversity Consortium for Political and Social Research (ICPSR) Study 6576 show that 21 percent of the counties in our data experienced a border change between 1870 and 1960. In an audit of city-county matches in 1900, using a 1900 atlas of US counties, we find that cities were incorrectly assigned in 0.98 percent of cases.

## Appendix B: Robustness Checks

In Table B1, we show how our key results vary with alternative definitions of our key independent variables, dependent variables, sample, and controls. The columns

Table B1—Robustness Checks

| | | Turnout | Vote share |
|---|---|---|---|
| (1) | Baseline | 0.0034 | 0.0002 |
| | | (0.0009) | (0.0010) |
| (2) | Truncated event variable | 0.0047 | 0.0002 |
| | | (0.0012) | (0.0011) |
| (3) | Isolated markets | 0.0043 | 0.0028 |
| | | (0.0019) | (0.0019) |
| (4) | Eight-year cumulative effect | 0.0037 | −0.0011 |
| | | (0.0012) | (0.0013) |
| (5) | Allowing for smooth trends | 0.0033 | 0.0000 |
| | | (0.0010) | (0.0010) |
| (6) | Allowing for restricted trends | 0.0039 | 0.0002 |
| | | (0.0011) | (0.0010) |
| (7) | Small events | 0.0030 | 0.0007 |
| | | (0.0024) | (0.0024) |
| (8) | Large events | 0.0041 | 0.0008 |
| | | (0.0025) | (0.0019) |
| (9) | Excluding counties with border changes | 0.0038 | 0.0005 |
| | | (0.0012) | (0.0010) |
| (10) | Excluding short-lived newspapers | 0.0031 | −0.0018 |
| | | (0.0010) | (0.0011) |
| (11) | Controlling for literacy rate | 0.0034 | 0.0002 |
| | | (0.0009) | (0.0010) |
| (12) | Block bootstrap over counties | 0.0034 | 0.0002 |
| | | (0.0008) | (0.0010) |
| (13) | AR(1) within counties | 0.0033 | 0.0001 |
| | | (0.0009) | (0.0010) |

*Note:* See Appendix B for details.

of the table show (i) estimated effects of newspaper entries and exits on presidential turnout (corresponding to Table 2, column 3), and (ii) estimated effects of partisan newspaper entry and exits on Republican vote share (corresponding to Table 6, column 3).

The first row of the table repeats the results from our main specifications for reference.

The second row of the table truncates changes in our independent variables at 1 and −1.

The third row of the table restricts the sample to counties that contain only one city that ever has a newspaper entry or exit in our sample, and that are not part of a Primary Metropolitan Statistical Area as of 1990.

The fourth row of the table presents the cumulative effect of events over the event period, plus one period following.

The fifth row of the table adds, as a control, a third-order polynomial interacted with time in a ten-year window around events.

The sixth row of the table adds, as a control, time trends in a ten-period window around events that are restricted to have the same lead and lag pattern as the estimated "effect" of events on population. This specification implements a bias correction motivated by the discussion in Section IIIB.

The seventh and eighth rows of the table allow separate effects for "small" events and "large" events. In column 1, small and large events are distinguished by whether the absolute change in circulation per eligible voter is less than or greater than 10 percent. In column 2, small and large events are distinguished by whether the absolute change in the difference between the Republican and Democratic share of circulation is less than or greater than 20 percent.

The ninth row of the table excludes counties with border changes in the decades 1870–1960.

The tenth row of the table excludes newspapers that are observed in only one presidential election year, i.e., whose "life spans" are fewer than four years according to our data.

The eleventh row of the table adds a control for the literacy rate in the county, interpolated following the methods used for other demographic controls as described in Section ID.

The twelfth row of the table computes standard errors using a block bootstrap at the county level with 100 replications.

The thirteenth row of the table allows the errors to follow an AR(1) process within counties.

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