

The Impact of Editorial Slant: Evidence from the Hearst Media Empire

Siona Listokin
School of Public Policy
George Mason University

Jason Snyder
Anderson School of Management
UCLA

Abstract

We examine whether editorial slant influences electoral outcomes in the context of one of the most powerful media conglomerates in US history. In the early 1900s, the Hearst newspaper empire was politically charged and considered influential. We test if the Hearst newspapers affected elections. Using a difference-in-differences and matching methodology, we find that the introduction of a Hearst newspaper into a county did not change electoral outcomes compared to similar counties - in contrast to other studies of media effects. We consider explanations for the results, and offer historical perspective to an issue that remains both salient and ambiguous.

Does the media affect our political opinions and behavior? Social scientists have struggled with this question since the rise of a “mass media” in the 19th century

that utilized new technologies to reach large audiences.¹ Conventional wisdom places newspapers, radio and television as critical determinants of certain historical events; thus, it has long been debated whether or not the Spanish-American War in 1898 was ignited by the journalism of William Randolph Hearst and Joseph Pulitzer, and that John F. Kennedy beat Richard Nixon in the 1960 presidential election in part because of his better appearance on television.² More recently, the rise of powerful media conglomerates has renewed the concern about media effects and the possibility that the political preferences of the owners of news companies influence popular perceptions of policies and politics.

Researchers have attempted to determine what influence, if any, the news media has on public opinion. In a series of studies in the 1940s and 50s, political scientists Lazarsfeld, Berelson and Gaudet (and later, Katz) investigated the influence of the mass media on presidential elections by examining survey answers and correlations to news availability, media habits and personal preferences (Lazarsfeld et al 1940; 1944; 1955). An entire field of political science research has since been devoted to the subject, and recently economists have contributed to the study of media effects.³ This literature is a reflection of both the importance of the research

¹For example, the Frankfurt School in the 1930s critiqued the emergence of a "mass media culture"; more formally, Lazarsfeld, Berelson and Gaudet (1940, 1944) studied survey data to determine the relationship between the media and public opinion.

² For example, Campbell (2003) opens his book on "yellow journalism" detailing the prevailing view of media's influence on the Spanish-American War. Druckman (2003) reviews the Kennedy-Nixon debates.

³ A literature review follows. See (McCubbins 1986) for a review of earlier literature; (Kull

questions on media effects and the difficulty of properly identifying them since media exposure can be both a cause and a consequence of political preferences. While the results are mixed, the empirical evidence does favor the possibility that editorial slant and media effects can wield critical influence on election outcomes.

We examine media effects in the context of the William Randolph Hearst newspaper empire in the early 20th century. Hearst owned one of the most powerful media conglomerates in United States history, and his newspapers had a distinct editorial slant. Did the Hearst newspapers affect presidential and congressional election outcomes between 1880 and 1938? We choose this historical context because the Hearst newspaper empire is unique in its scope and in the nature of its expansion. In the early 20th century, Hearst built and expanded a network of newspapers and magazines in cities across the United States; by 1930, his newspapers were read by almost 20% of the country's total population (Nasaw 2001). Hearst was also a political figure, having served two terms as a Democratic congressman, and remained powerful in the Democratic Party during the era. Furthermore, Hearst's editorial bias was reflected in both news articles of his newspapers and in the prominent placement of opinion pieces on the front page of the newspapers, following the practice of the era. These features allow us to examine the effects of editorial slant on electoral outcomes with limited concern of measurement error that plagues studies that focus on more recent elections, where

et al 2003; Zaller 1996; Gentzkow and Shapiro 2006; Druckman and Parkin 2005; George and Waldfogel 2005; Della Vigna and Kaplan 2007).

media outlets are diffuse and political bias in news reporting is relatively low.

The results of our study suggest that there is no Hearst effect on voting outcomes and voter turnout; this result is robust to a number of specifications. We use a difference-in-difference methodological approach that compares voter results and turnout in metropolitan areas where a Hearst newspaper was introduced to those that never received a Hearst paper. We infer from our results that the presence of a Hearst newspaper in an MSA was not associated with a change in election outcomes.⁴ Considering the unexpected nature of our results – especially given the recent empirical research suggesting strong media effects on election outcomes – we consider a number of possibilities that may have produced this result aside from an absence of media effects. Nevertheless, this is an empirical illustration of the limits of editorial slant influence on real voting outcomes.

Editorial Slant and Elections

Our focus is on the extent to which a powerful news media outlet influences Presidential and Congressional election outcomes. Political news media can influence voters by acting as the intermediary between voters and their elected representatives (Paletz, 1999). "Media effects" has thus been a concern for social

⁴ That is, the parameter estimate is tightly centered around zero, despite being statistically insignificant.

scientists because of the informational power it confers on a private industry. In their seminal analyses of the media and public opinion, Lazarsfeld, Berelson and Gaudet reported that media messages have a fairly small, but significant, effect on political opinions (Lazarsfeld et al 1945). These results were supported by much of the succeeding literature, with many studies conducted using National Election Survey data (Zaller 1996; Kull et al 2003).⁵

Inevitably, treatment of media effects leads to consideration of bias in the news media, as those editorial slants distort the media's informational function. Much of the research consideration has focused on first measuring media bias and then using survey data to determine its influence on voters (Dalton et al 1998; Gilens and Hertzman 2000; Kahn and Kenny 2002; Druckman and Parkin 2005; Gentzkow and Shapiro 2006). Some recent studies have shifted focus away from content analysis and polling data, exploiting plausibly exogenous variation in media expansion to test how the media affects voting outcomes against the counterfactual in the absence of the media.

Our study resembles a paper by DellaVigna and Kaplan that looks at the effect of the introduction of the Fox News channel on the 2000 presidential election (DellaVigna and Kaplan 2007). The authors are able to exploit the rapid expan-

⁵ The difficulty of isolating media effects on voter attitudes and behavior (and minor results) cast a certain degree of doubt on these results, leading one researcher to declare in 1993 that "The state of research on media effects is one of the most notable embarrassments of modern social science" (Bartels, 1993).

sion of the cable news channel in towns throughout the country, and they find that the introduction of the Fox News channel convinced 3 to 8 percent of voters to vote Republican. Gentzkow similarly studies the rapid expansion of television in the 1950s and tests its influence on voter turnout (Gentzkow 2006). The paper finds that turnout dropped, possibly because television featured less election coverage than other news sources that viewers substituted away from. George and Waldfogel test if the expansion of *The New York Times* in the 1990s changed local voting and newspaper readership patterns (George and Waldfogel 2006). They find that the expansion of the *Times* decreased local newspaper circulation and among certain populations, decreased voting in local elections.⁶

In studying the effects of the Hearst newspaper empire on electoral outcomes, we employ similar methodological tools as the papers above. Additionally, we consider the explicit media bias of the Hearst newspapers (though we do not quantitatively measure it). The Hearst newspaper empire remains unparalleled in its combination of market power and political bias.⁷ Informally, if the modern-day

⁶ (Oberholzer-Gee and Waldfogel 2001; Stomberg 2004)

⁷ An unscientific consensus elevates Hearst's media empire as paradigmatic of the media's ability to influence elections and policy. For (an unscientific) example, see the Wikipedia entry on "Media Effects" or the Encyclopedia Britannica's entry for William Randolph Hearst and/or Publishing (History, US). The most widely adopted elementary school (upper division) social studies textbooks (according to the American Textbook Council) at least partly ascribe the Spanish-American War to Hearst's reporting (Harcourt/Holt "Social Studies," Houghton Mifflin "Social Studies," Houghton Mifflin "We the People")

media effects measured in previous papers are applicable to Hearst's era – and we are able to isolate them – there would be a potentially substantive example of changes in election outcomes in the first quarter of the 1900s. Beyond historical curiosity, the Hearst papers' strong presence represents a unique opportunity to measure media influence on elections.

The Hearst Newspapers

William Randolph Hearst took control of his first newspaper in 1887, in his home town of San Francisco. Hearst started his practice of reporting national news in city papers and contracted with the *New York Herald* to cable articles to his *San Francisco Examiner*. This move “changed the face of the *Examiner* and journalism on the West Coast,” and the competition folded or followed suit in introducing national news coverage (Nasaw 2001).

During the next 25 years, Hearst started newspapers in more than seventeen cities, starting with the largest metropolitan areas (New York and Chicago), and then delving into smaller cities with geographic and demographic diversity. As the Hearst newspapers expanded its market scope, the number of readers and percent of market share also grew. Hearst papers held at least 10% market share in their markets, with an average of about 25% share of major newspapers after five years. At their height, 20 million Americans read a Hearst newspaper daily. For comparison, *The New York Times* had a total circulation of 1.1 million in 2006,

and an average of 1.5 million Americans watched the Fox News Channel during primetime in August 2006 (0.9 million watched CNN).

The locations and timing of the Hearst expansion were not random. Although Hearst was ostensibly a multimillionaire, his money was tightly controlled by his mother, Phoebe Hearst, and shifts in their relationship – most radically, Phoebe’s death – often precluded newspaper “buying sprees.” Thus, the periods between 1911-12 and 1919-22 saw the acquisition of four and ten more newspapers, respectively, as Phoebe granted more assets to Hearst in 1911 and following her death in 1919.

Hearst chose the cities in which to expand for different reasons.⁸ Appendix 1 lists the cities that Hearst expanded into during this period. The *San Francisco Examiner* was a gift from his father. The choice to move into New York was strategic: Hearst meant to build a publishing empire while establishing himself in the Democratic Party leadership, and the New York newspaper market was necessary for both objectives (Nasaw 2001). In 1900, the Democratic National Committee explicitly asked Hearst to start a paper in Chicago in order to increase their exposure through sympathetic reporting in the mid-West. After his early expansion into the large population centers (New York, Chicago, Boston, Detroit, Baltimore), Hearst later expanded into some smaller cities, such as Atlanta and Syracuse, to extend his geographic scope into new regions. His deliberate growth

⁸ In our analysis, we consider Hearst’s initial entry into a metropolitan newspaper market. In some instances, there were multiple Hearst publications.

in upstate New York in the early 1920s was also attributed to his desire to run for the governorship of New York.⁹

As mentioned, there are two aspects unique to the Hearst newspaper empire and this time period that make the Hearst papers an interesting and highly relevant empirical study of the effects of media bias: the overt subjectivity injected into the Hearst papers and their overwhelming market penetration. It may be hard for today's newspaper readers to appreciate the level of bias injected into newspaper reporting during this time. Gentzkow et al describe newspapers during the 19th century as "often public relations tools funded by politicians," though they emphasize that bias declined during the early 20th century (Gentzkow et al 2004).

Hearst generally bought small or ailing newspapers, and upon purchase Hearst and his editors changed the news content, most notably by tapping into the Hearst editorials, cartoons and articles that were wired through private cables and syndicated across the country (Nasaw 2001). The content of these Hearst features were generally urban, worker oriented and geared to readers with Democratic Party leanings. This did not mean, however, that Hearst monolithically towed the Democratic Party line during the almost forty years under study in this paper; to the contrary, Hearst had frequent "lapses" away from the Democratic mainstream in favor of western progressive politics and the lower taxes offered by Republicans in the post-World War I era (Nasaw 2001).

Nonetheless, Hearst was comfortable using his newspapers as publicity tools

⁹ <http://timesunion.com/specialreports/tu150/>

for his own campaigns, to support other candidates, to ingratiate himself to the Democratic Party and even to promote his other commercial endeavors. According to a Hearst biographer, "Hearst employed the power of the media to set the national political agenda, [as] a muckraking progressive trustbuster (Nasaw, xiv)," and ran his papers as "pro-labor, pro-immigrant and anti-Republican. (106)" Hearst's first newspaper, the San Francisco Examiner was "defiantly pro-labor, anti-capital and anti-railroad," positions well-aligned with the Democratic Party. Hearst himself viewed editorial campaigning as a necessary function of his newspapers. Hearst wrote in 1936, "The word crusade must not be considered as an attack... It is essential for the papers to conduct constructive campaigns for the benefit of the community with which they are associated. And it is of vital importance for the papers to identify themselves with the aims and ambitions of the community and to make themselves recognized forces in the accomplishment of these aims and ambitions (The Newspaper Credo of William Randolph Hearst)."¹⁰

This involvement in political campaigns is most evident in presidential and congressional election periods. In the 1896 presidential election, "The *Journal* office became headquarters for the [William Jennings Bryan] campaign... His

¹⁰ Hearst was less pugnacious in official communications with his editors and reporters. In 1933, Hearst ordered that the following bulletin be posted in his newsrooms: "Be fair and impartial. Don't make a paper for Democrats and Republicans, or Independent Leaguers. Make a paper for all the people and give unbiased news of all creeds and parties. Try to do this in such a conspicuous manner that it will be noticed and commented upon." (from The Newspaper Credo of William Randolph Hearst)

reporters followed Bryan's every step along the campaign trail; interviews and articles were published daily (Nasaw 2001)."¹¹ In the summer of 1931, Hearst gave explicit orders to his editors to use news reporting and cartoons to paint President Herbert Hoover as treasonous and a presidential failure (Carlisle 1965). Hearst's newspapers were important elements in local congressional elections, too. Between 1928 to 1932, a number of congressman received editorial support and favorable coverage in Hearst newspapers in their respective congressional districts (or themselves wrote articles and editorials for Hearst newspapers), including William Borah of Idaho, Hiram Johnson of California, Thomas Walsh of Montana and George Norris of Nebraska.¹² While this support was notable even for its time, it is unclear whether the presence of a Hearst paper systematically affected election results.

Measuring the Hearst Effect

We measure the effect of the Hearst newspapers by examining the conglom-

¹¹ Hearst's control over tone and bias became notorious just before World War II broke out, when his papers were highly sympathetic to Hitler and Mussolini. Both men wrote "reports" from their home countries for Hearst publications.

¹² For example, the San Francisco Examiner expressed support for Hiram Johnson and his policies on June 19, 1928, pg 26; June 21, 1928, pg 30. Johnson wrote a public thank you in the paper on August 29, 1928, pg 2.

erate's expansion over time and across different cities. We examine the electoral outcomes before and after a Hearst newspaper enters a new county and compare this to electoral outcomes in similar counties that did not receive a Hearst newspaper during the relevant era. We first use the conventional difference in differences OLS estimator:

$$(1) Y_{i,t} = \beta \text{Hearst}_{i,t} + \sum_i \text{County}_i + \sum_t \text{Year}_t + \varepsilon_{i,t}$$

This specification examines how the outcome $Y_{i,t}$ (for instance the percentage of the electorate in county i , in year t , that voted for the Democratic Congressional candidate) changed when Hearst entered the county ($\text{Hearst}_{i,t}$ is a dummy variable equal to one if a Hearst newspaper has entered the market) conditioning on county fixed effects, County_i , and year fixed effects, Year_t . This approach eliminates any fixed differences across counties and over time. Throughout all of our specifications the error terms are clustered at the level of the metropolitan statistical area¹³ to account for auto-correlation in the data across counties and over time. This clustering relaxes the assumption of independence of the error terms of counties that are in close proximity to one another. Panel applications that do not account for autocorrelation in the data can dramatically underestimate the standard error for the parameters of interest (Bertrand, et al 2004).

Though difference in differences is a widely used methodology for the analysis of panel data, it often relies on serious assumptions about the trends prior to an intervention for causal inference to be valid. For example, if prior to the intro-

¹³ A metropolitan statistical area covers multiple counties.

duction of a Hearst newspaper the trends in voting outcomes differ significantly between a treatment county (the county where Hearst enters) and control counties (counties where Hearst didn't enter), then using a conventional difference in differences estimator could lead to a spurious inference that Hearst ownership had an influence of voting outcomes. For example in Table 2A we observe that the trends in the percentage of votes for Democrats over the four elections prior to the introduction of a Hearst paper differ from the potential control counties. There is a pronounced decline in the average votes received by Democratic congressional candidates in the areas that Hearst expanded into (for example, in column 1 of Table 2A, Democratic congressional vote share declines from 0.50 to 0.45). To partially address these concerns we include a yearly linear trend at the county level to account for differences in the pre-intervention trends. This specification is given by:

$$(2) Y_{i,t} = \beta \text{Hearst}_{i,t} + \sum_i \text{County}_i + \sum_t \text{Year}_t + \sum_i \text{County}_i * \text{Time}_t + \varepsilon_{i,t}$$

Here the time trend Time_t is a counter that starts at zero in the year 1880.¹⁴ The main effect of the linear time trend is absorbed by the year dummies, so the interaction term controls for county specific time trends that are not picked up by the main year effects. This technique is standard in the difference in differences literature and has been employed in a variety of settings (Wolfers 2006).

We further address these concerns by create control groups that match the treatment group to a set of controls that have similar pre-trends and are of similar

¹⁴ $\text{Time}_t = 0$ in year 1880, $\text{Time}_t = 1$ in year 1881, etc.

size.¹⁵ We construct a series of sets $W(1) \dots W(\mathbf{K})$, each of which consist of a single treated county and 4 control counties for all treated counties \mathbf{K} .¹⁶ The objective in constructing $W(k)$ is that they find the "best" set of control counties for the treatment county k . To construct a set of controls we proceed in a two step process. First we require that a potential control match the treatment exactly on the voting population bin.¹⁷ We then employ a standard distance metric¹⁸ that allows for us to find a set of potential control counties based on observable pre-intervention voting outcomes. For a potential control observation x , a treatment observation z , and a diagonal matrix V consisting of the inverse of the variances of the observations we use the following distance metric:

$$(3) \|z - x\| = ((z - x)' V (z - x))^{1/2}$$

From here we can construct the set $W(i)$ by taking the 4 closest control counties. All of methods were implemented in Stata (Abadie, et al 2004).

¹⁵(Card 1990) uses a similar approach

¹⁶ Throughout this paper the estimation procedures will be done with replacement. That is being a control for one treated county does not preclude said county from being a control for another treated county.

¹⁷ We construct three bins: counties with less than 25,000 voters, counties with 25,000 to 100,000 voters, and counties with over 100,000 voters.

¹⁸ This is standard in many matching applications. We could also use the Mahalanobis metric as a means of computing the distance (Abadie et al 2004)

In order to evaluate the final outcomes, the conventional differences in differences estimator can be used on each group $W(k)$. For each sub-group $W(k)$, specification (1) is run on the treatment and its best controls, no longer using the less desirable controls for the estimation. To recover the average treatment effect on the treated across all counties we run the following model using dummies $Group_k$ for each k group from $W(k)$ (i.e. each for each Hearst county and its matches):

$$(4) Y_{i,t} = \beta Hearst_{i,t} + \sum_i County_i + \sum_t Year_t + \sum_k Group_k * Year_t + \epsilon_{i,t}$$

Here $Hearst_{i,t}$ is the average treatment effect on the treated. Notice how the variable $Hearst_{i,t}$ is the only variable that is not interacted. One could potentially interact this with $Group_k$ to recover the estimates for each treatment and its m controls. This would be the exact same result as running each regression separately within each group. The usefulness of using specification (4) is that it allows for the easy inclusion of matching methods within a standard regression framework.

Historical Data & Sample Selection

Hearst Data. We obtained data on Hearst expansion cities from historical and biographical studies. Occasionally, years of the newspaper expansions differed slightly (one year off), in which case we relied on the David Nasaw biography. Any county that is in a Metropolitan Statistical Area where a Hearst paper is introduced is considered to be a treated observation.

Voting Data. The voting data comes from the ICPSR "Electoral Data for

Counties in the US: Presidential and Congressional Races, 1840-1972." This is our source for the dependent variables, Democratic vote shares from the congressional elections over this time period, Democratic vote share from the presidential elections over this time period, and turnout for congressional elections during this time period. We use the ICPSR Democratic Party vote share "equivalent," which accounts for party variations in the 18th and 19th century. This data also includes population numbers that are computed to represent the number of eligible voters.

Unfortunately since this is historical data, there are often numerous missing observations. We find that on average missing observations come from smaller counties¹⁹ and are less frequent over time.²⁰ We also find that the number of missing observations is approximately the same between counties where a Hearst paper was present and ones where Hearst did not enter.

Our sample covers the years 1880 – 1938. We decided on this sample since it is a natural historical era, the time between the end of the reconstruction and the start of World War II. We only study counties that are in large Metropolitan areas.²¹

¹⁹ In 1900 the average county population where the observations were missing was 7,434 while the average county size without missing observations was 16,394.

²⁰ Approximately 22% of the observations were missing in 1900, while in 1932 only 12% were missing.

²¹ Summary statistics can be found in an online appendix, available upon request.

Empirical Results

The difference in differences OLS estimates from equation (1) & (2) are shown in Table 1. The dependent variables are Democratic Party vote share and voter turnout for both presidential and congressional elections. We regress each dependent variable on the explanatory variable of the Hearst entry in the MSA, a population control and year and county fixed effects. Each regression is also run with an additional control for the linear year trend and county interaction from equation (2), which are in the even numbered columns. The results show that Hearst entry has a very small and statistically insignificant effect on voting outcomes and turnout. Despite the statistical insignificance, the results are informative. The confidence intervals for each parameter estimate are tightly centered around zero (\pm less than 0.10), indicating a precisely estimated zero.²²

Turning to the matching results, we first examine the effectiveness of the matching process in Tables 2A-2D. While pre-trends and levels of the unmatched sample are in general close to the treated counties, after matching we are able to obtain matches that are extremely close to the pre-trends of the treated counties. Additionally the matching process enables us to obtain much closer matches in terms of county population.

Tables 3-6 show the results using the matched samples, with the congressional

²² In the online appendix we show that the results are robust to including interpolated data and cubic linear time trends.

and presidential vote share, and congressional and presidential voter turnout in the four tables. In the matching estimates we restrict the estimates to looking for the Hearst effect 10 years after Hearst entered into a city. Furthermore we eliminate counties where over 20% of the sample is missing. The matching results show a very small and statistically insignificant "Hearst effect" on all of the four vote outcome variables. As with the difference in difference regression, most of the matching estimates are closely centered around zero.

We examine the robustness of the matching results in column (2) of Tables 3-6 by only looking at counties that are near the center of the MSA. We find the results are unchanged. In column (3) we make the sample more restrictive by only admitting counties with less than 10% of the data missing. Again the results are unchanged. Finally in column (4) we linearly impute the missing data in these counties and find the results unchanged.

Discussion

The results of our empirical test are strongly suggestive that the Hearst newspaper empire did not change election vote shares or turnout in the early 20th century, and provide a thorough empirical illustration of the limitations of media bias. The coefficient estimates for the "Hearst" dummy variable are consistently insignificant, through a host of specifications intended to address potential problems in the identification. This stability mitigates our concerns that our failure to reject the null hypothesis is a false finding. We also note that the data set used in

this paper have produced reasonable, significant results in past research (for example, Ansolabehere et al 2001; Carson and Roberts 2005). The coefficients in our results are small in scale, especially considering the baseline of the MSA's in the years before Hearst's entry. We conclude that while we cannot reject the null hypothesis of no effect, we can infer that the Hearst newspapers had a "zero" effect on election outcomes on congressional races, presidential races, and turnout.

The consistency of these results, which suggest no Hearst influence on voting outcomes, prompt the subsequent question: Given Hearst's market penetration and editorial message, why wasn't there a "Hearst effect?" We consider three types of plausible explanations that are consistent with the historical record and our results: (1) The treatment effect of Hearst's entry into a media market was in fact very small, (2) There was a substantial influence upon the electorate, however the effect washed out, or (3) that Hearst did not influence electoral voting, but he did influence government along other dimensions such as policy making or the selection of politicians.

The first possibility, that Hearst did not influence election results because his newspapers did not change voter opinions, is most obviously consistent with our baseline results. This possibility was addressed indirectly in a 1936 poll in Fortune Magazine. The poll asked "Do you think the influence of the Hearst papers upon national politics is good or bad?" The results are shown in Table 7.

[Insert Table 7 about here]

The consistency of the favorable response suggests that a large portion of Hearst newspaper readers were members of a stable population that generally

agreed with Hearst, in which case the presence of a Hearst publication would not have an influence on their opinions and voting outcomes.

It is also important to note that at the height of Hearst's circulation (and market share), Hearst was somewhat more inconsistent in his support for the Democratic Party. The Hearst newspapers were favorable to Hoover when he ran against Democrat Al Smith. In the congressional elections of 1930, the papers did not follow a national policy, and the California papers abstained from political support of particular congressmen. Since our empirical test includes a number of periods in the mid and late thirties, the lack of effect may reflect this inconsistency in political campaigning (Carlisle, 1965).

The second possibility, that the Hearst effect exists but is not observable, is conceivable for the same reasons we might have expected the Hearst papers to have a discernible influence on elections. The Hearst papers may have changed underlying features of treatment cities that negate Hearst's influence. It is possible that Hearst changed the distribution of voters' preferences without changing the actual electoral outcomes. For example, suppose that following the introduction of a Hearst newspaper in a metropolitan area, local newspaper competitors responded with similarly powerful pro-Republican articles. If voters were uniformly distributed along a unidimensional continuum of policy preferences and after the entry of Hearst the distribution of voter preferences was transformed into a bimodal distribution with modes at both the left and the right end of the spectrum, the position of the median voter might still remain unchanged. Many simple models of political polarization would be consistent with this finding of no change

in average voting outcomes.

In order to determine the competitive effect of a Hearst newspaper entry, we examined the circulation records for US cities collected in 1910, 1919 and 1929. These records provide market information in Hearst cities over time; given the varied dates of Hearst entry, we can observe competitive influences both immediately after a Hearst purchase and decades later. The circulation numbers suggest that the smaller newspaper markets like Pittsburgh and Milwaukee turned into “two paper” markets, which would support the possibility that other newspaper competitors strategically placed themselves in political opposition. In most of the mid-sized and large markets, however, the competitive landscape was not altered so radically; Hearst newspapers gained market share by cutting into smaller circulation papers. While the circulation figures cannot reveal the content of the competing newspapers, it does not appear that local newspaper competitors strategically responded to Hearst with right-leaning content that would be reflected in polarized market share figures.

The final possibility that we consider is that the Hearst newspaper empire did not influence electoral voting, but influenced other electoral and policy outcomes. This would be consistent with our results if, for example, the Hearst papers influenced policies whose absence would have been discernable in the electoral outcomes. There is no question that political campaigns and presidential administrations considered the reaction of the Hearst newspapers when choosing policies. For example, both the Hoover and FDR administration crafted parts of their labor policies with Hearst in mind (Carlisle, 1965). Communications between Hearst,

FDR, Joseph Kennedy (SEC Chairman) and Ed Coblentz (manager of multiple Hearst papers) reveal that Hearst relayed detailed opinions on domestic policy and suggestions for cabinet secretaries. Thus, the Hearst newspaper may have influenced politics by virtue of their communication power, in manners that would not be reflected in presidential and congressional elections.

Hearst's papers may also have influenced candidate choice. Hearst was pivotal in getting FDR the Democratic Party's nomination at the 1932 Democratic National Convention. The delegates were deadlocked between FDR (Governor, NY) and Alfred E. Smith (1928 Democratic Presidential candidate) for several days, while a third candidate, John Garner (Speaker of the House), controlled the deciding votes. Hearst supported Garner. Eventually, Joseph Kennedy called Hearst in the middle of the night and convinced Hearst to have Garner drop out of the race and throw his delegates to FDR. Hearst agreed and FDR won in the next ballot (Nasaw, 2001). Hearst may have greatly influenced voters' opinions without a discernible effect on election results, by virtue of a change in candidates or policies. We cannot reject this possibility, and certainly these historical facts are supportive of a non-electoral Hearst effect.

In future research it would be of considerable interest to learn whether Hearst and other media effects influence behavior along other political dimensions. For instance it might be possible that the newspaper ended up being more issue oriented, forcing politicians that had an electorate in it's domain to support certain issues that Hearst favored. One could imagine studying how Hearst's entry (or any other media "message") may have influenced roll call votes, public spending,

or the spread of corruption.²³

The Hearst newspapers' overwhelming circulation percentages alarmed politicians and journalists in the early 20th century; nonetheless, at the time their influence was unclear. One paper actually noted, "More interesting [than circulation] would be some test of the actual influence of Hearst's ideas upon his more than five million daily readers, but no means of investigation seems to uncover this sort of information (Doan, 1932)." This paper uncovers Hearst's influence on elections, and opens the door to the possibility that his papers had other means of influence. If nothing else, the Hearst newspapers at their height were a means of political communication that has not been duplicated in the United States since; the lack of evidence for a strong media effect in this case suggests that "media effects" may have their most substantial influence on the competitive and political environment leading up to elections. As the study of media bias and electoral influence continues, our study is indicative of the necessity – and limitations – of quantitative research to address long-held assumptions.

²³This would be in line with Stromberg (2004)

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Table 1: Effect of Hearst Entry on Voting Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Congressional Democratic Vote Share	Congressional Democratic Vote Share	Presidential Democratic Vote Share	Presidential Democratic Vote Share	Congressional Election Turnout	Congressional Election Turnout	Presidential Election Turnout	Presidential Election Turnout
Hearst Entry	-.026 (.023)	-.009 (.016)	-.019 (.025)	.006 (.029)	.015 (.031)	-.002 (.013)	.011 (.033)	-.010 (.014)
County * Time Trend	N	Y	N	Y	N	Y	N	Y
Population Quadratic	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	19466	19466	11826	11826	22397	22397	11681	11681

Note: These estimates are obtained through ordinary least squares regressions. This sample follows years 1880 through 1938. Robust standard errors in brackets clustered at the MSA level.

Table 2 C & D: Turnout Variables Match Quality

Table 2C: Match Quality for Congression Voter Turnout

	Treatment	Unmatched Control Sample	Matched Control Sample
Congressional Voter Turnout (t-8)	.561 (.015)	.623 (.004)	.572 (.008)
Congressional Voter Turnout (t-6)	.456 (.017)	.529 (.004)	.463 (.008)
Congressional Voter Turnout (t-4)	.532 (.016)	.595 (.004)	.537 (.008)
Congressional Voter Turnout (t-2)	.491 (.016)	.505 (.004)	.473 (.008)
County Population in Year Adoption	56895 (9206)	22848 (779)	49571 (3799)

Table 2D: Match Quality for Presidential Voter Turnout

	Treatment	Unmatched Control Sample	Matched Control Sample
Presidential Voter Turnout (t-12)	.595 (.016)	.651 (.004)	.605 (.008)
Presidential Voter Turnout (t-8)	.601 (.015)	.652 (.004)	.615 (.008)
Presidential Voter Turnout (t-4)	.579 (.015)	.625 (.004)	.582 (.008)
County Population in Year Adoption	56895 (9206)	22848 (779)	47297 (3521)

Note: Standard errors in brackets clustered at the MSA level.

Table 3: Influence of Hearst Entry on Congressional Democratic Vote Share

	(1) Congressional Democratic Vote Share	(2) Congressional Democratic Vote Share	(3) Congressional Democratic Vote Share	(4) Congressional Democratic Vote Share
Hearst Entry	.007 (.016)	.013 (.015)	.007 (.019)	.012 (.017)
Exclude Exterior Areas	N	Y	N	N
More Restrictive Sample	N	N	Y	N
Use Imputed Data	N	N	N	Y
Year Fixed Effects	Y	Y	Y	Y
County Fixed Effects	Y	Y	Y	Y
Observations	6346	4385	5383	7150

Table 4: Influence of Hearst Entry on Presidential Democratic Vote Share

	(1) Presidential Democratic Vote Share	(2) Presidential Democratic Vote Share	(3) Presidential Democratic Vote Share	(4) Presidential Democratic Vote Share
Hearst Entry	.004 (.020)	.004 (.021)	.007 (.020)	.002 (.019)
Exclude Exterior Areas	N	Y	N	N
More Restrictive Sample	N	N	Y	N
Use Imputed Data	N	N	N	Y
Year Fixed Effects	Y	Y	Y	Y
County Fixed Effects	Y	Y	Y	Y
Observations	4143	2751	3410	4150

Note: These estimates are obtained through ordinary least squares regressions on the matched sample. This sample comes from four election cycles prior to the entry and six elections following entry. Robust standard errors in brackets clustered at the MSA level.

Table 5: Influence of Hearst Entry on Congressional Voter Turnout

	(1) Congressional Voter Turnout	(2) Congressional Voter Turnout	(3) Congressional Voter Turnout	(4) Congressional Voter Turnout
Hearst Entry	.008 (.010)	.006 (.012)	.007 (.009)	.009 (.010)
Exclude Exterior Areas	N	Y	N	N
More Restrictive Sample	N	N	Y	N
Use Imputed Data	N	N	N	Y
Year Fixed Effects	Y	Y	Y	Y
County Fixed Effects	Y	Y	Y	Y
Observations	6975	4682	6544	7150

Table 6: Influence of Hearst Entry on Presidential Voter Turnout

	(1) Presidential Voter Turnout	(2) Presidential Voter Turnout	(3) Presidential Voter Turnout	(4) Presidential Voter Turnout
Hearst Entry	-.008 (.018)	-.002 (.021)	-.009 (.024)	-.007 (.018)
Exclude Exterior Areas	N	Y	N	N
More Restrictive Sample	N	N	Y	N
Use Imputed Data	N	N	N	Y
Year Fixed Effects	Y	Y	Y	Y
County Fixed Effects	Y	Y	Y	Y
Observations	4099	2718	3379	4120

Note: These estimates are obtained through ordinary least squares regressions on the matched sample. This sample comes from four election cycles prior to the entry and six elections following entry. Robust standard errors in brackets clustered at the MSA level.

Table 7: Fortune Magazine Poll, 1936

Question: Do you think te influence of the Hearst newspapers upon national politics is good or bad?

	Good	Bad	Don't Know
Areas without Hearst Newspaper	10.7%	27.6%	61.7%
Areas with at least one Hearst Newspaper	10.5%	43.3%	46.2%

Appendix Table 1: Hearst Newspaper Expansion

Newspaper	City	Year
San Francisco Examiner	San Francisco	1887
New York Journal	New York	1895
New York Morning Advertiser	New York	1897
Chicago American	Chicago	1900
Examiner	Chicago	1902
Motor	National	1903
LA Examiner	Los Angeles	1904
The American	Boston	1904
Cosmopolitan	National	1905
Farm and Home	National	1906
Good Housekeeping	National	1911
World Today	National	1911
Harper's Bazaar	National	1912
Atlanta Georgian	Atlanta	1912
Boston Daily Advertiser	Boston	1917
Chicago Herald	Chicago	1918
Washington Times	Washington	1919
Wisconsin News	Milwaukee	1919
The Times	Detroit	1921
The Record	Boston	1921
The Post-Intelligencer	Seattle	1921
The Herald	Los Angeles	1922
The Herald	Washington	1922
Oakland Post-Enquirer	Oakland	1922
Syracuse Telegram	Syracuse	1922
Rochester Journal	Rochester	1922
Baltimore American	Baltimore	1923
Baltimore News	Baltimore	1923
Fort Worth Record	Fort Worth	1923
Albany Times-Union	Albany	1924
San Antonio Light	San Antonio	1924
Milwaukee Sentinel	Milwaukee	1924
New York Daily Mirror	New York	1924
Pittsburgh Sun	Pittsburgh	1927
Omaha News	Omaha	1928