

# Publicizing Scandal: Results from Five Field Experiments

Donald P. Green<sup>1</sup>, Adam Zelizer<sup>2</sup> and David Kirby<sup>3\*</sup>

<sup>1</sup>*Columbia University, New York, NY, USA; dpg2110@columbia.edu*

<sup>2</sup>*Columbia University, New York, NY, USA; apz2002@columbia.edu*

<sup>3</sup>*Cato Institute, Washington, DC, USA; davidrkirby@gmail.com*

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## ABSTRACT

Despite decades of research on the persuasive effects of propaganda, little is known about opinion change in the wake of journalistic accounts of scandal involving public officials. To what extent and under what conditions do opinions change in the wake of information conveyed through newspapers? We conducted five experiments to assess how publicizing scandal changes evaluations of the specific public officials involved and attitudes towards government in general. In each study, subjects drawn from voter files and lists of party activists were mailed “special edition” investigative newspapers that reported on scandals involving public officials. Feature stories depicted some public officials as villains and others as heroes. Treatment and control groups were interviewed approximately two weeks later. We find significant effects on both voters and activists. The most striking pattern is the change in net favorability of the public officials implicated in the scandals. Evaluations of the villains deteriorated and evaluations of the heroes improved. Changes in evaluations are especially large when scandals implicated public officials with whom respondents had little prior familiarity.

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**Keywords:** Political scandals; newspapers; voter persuasion; field experiments; public opinion

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\*The authors are grateful to the anonymous organization and its staff for implementing the interventions, conducting the surveys, and sharing the data with us. The organization bears no responsibility for the views expressed here. The group allowed the authors to

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Online Appendix available from:

[http://dx.doi.org/10.1561/100.00017094\\_app](http://dx.doi.org/10.1561/100.00017094_app)

Supplementary Material available from:

[http://dx.doi.org/10.1561/100.00017094\\_supp](http://dx.doi.org/10.1561/100.00017094_supp)

MS submitted on 22 June 2017; final version received 24 January 2018

ISSN 1554-0626; DOI 10.1561/100.00017094

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Propaganda's effects on public opinion have attracted scholarly attention for almost a century. Foundational works in social science by Lippmann (1922) and Lazarsfeld (1940), as well as some of the earliest field experiments (Hartmann, 1936–37), sought to assess whether messages conveyed via newspaper, radio, or leaflet had lasting effects on what people think about politics and political leaders. The influence of mass media has long been the subject of vigorous debate. Some scholars conclude that messages have minimal effects on attitudes and evaluations (Hovland *et al.*, 1949; Klapper, 1960), while others contend that effects are often profound and enduring (Iyengar and Kinder, 1987). Somewhere in between is the claim that persuasive political messages exert important short-term effects that die out after a few days (Gerber *et al.*, 2011; Hill *et al.*, 2013).

The present study hearkens back to early research gauging the effects of newspapers and propaganda. Our central research question is **whether the modern version of the muckraking newspaper has detectable effects on public opinion**. To what extent do such newspapers raise awareness of scandals? Do they change readers' evaluations of public officials connected with scandals? Do they change readers' assessments about the prevalence of corruption? These micro-level questions have direct implications for macro-level arguments about the effects of press freedom on corruption (Brunetti and Beatrice, 2003), the **extent to which incumbents suffer electoral consequences for corrupt behavior** (Basinger, 2012; Peters and Susan, 1980), and the extent to which news of wrongdoing by individual politicians affects voters' perceptions of government as a whole (Bowler and Karp, 2004; Citrin, 1974). A **related micro-level question focuses on which kind of voters punish politicians accused of corruption** (Klašnja, 2017). Drawing on the longstanding literature that stresses the distinction between the assimilation of news by ordinary citizens and by politically active citizens (Zaller, 1992), we also investigate whether a given piece of investigative journalism has detectable effects across the spectrum of political involvement.

To date, researchers have assessed the effects of news reports of scandals in three ways. First, experiments have informed survey respondents about allegations of criminal or corrupt behavior on the part of hypothetical candidates for office. These survey experiments were initially conducted among American undergraduates (Carlson *et al.*, 2000; Funk, 1996), but more recent works have **assessed the responses of general populations in Germany (Maier, 2011), Brazil (Winters and Rebecca, 2013), and India (Banerjee *et al.*, 2014)**. When voters are presented with allegations of scandal by the survey interviewer and queried immediately about their vote preferences, the apparent effects of news are quite

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publish findings regardless of the results.

Conflict of interest statement: The authors received no financial or in-kind compensation for conducting the analyses reported here, nor do they have formal ties to the organizations that composed or distributed the newspapers.

strong. For example, when an Indian candidate of the respondent's preferred party is merely "rumored" to have received kickbacks from a contractor, the chances that this candidate receives the respondent's vote drops by more than 40 percentage points (Banerjee *et al.*, 2014, p. 400).

Such allegations, however, seem to have much weaker effects in more naturalistic settings. Non-experimental studies of actual scandals implicating Brazilian (Ferraz and Frederico, 2008), Italian (Chang *et al.*, 2010), or Spanish local officials (Costas-Pérez *et al.*, 2012) suggest that the effects tend to be weak unless scandals receive news coverage, but even under conditions of considerable media attention, the effects are far weaker than suggested by survey experiments. For example, a scandal receiving maximal news coverage in the Spanish case resulted in a 14 percentage point decline in incumbent vote share. Still weaker effects are obtained in field experiments that bypass conventional media channels and convey messages about scandals directly to voters via leaflets in advance of an election. Randomized experiments in Mexico (Chong *et al.*, 2014) and Brazil (De Figueiredo *et al.*, 2011) show vote share effects of less than three percentage points. No experimental studies, to our knowledge, have tested the effects of conveying scandal news via traditional media channels such as newspapers.<sup>1</sup>

In order to assess the effects of media coverage of scandals, we conducted a series of five randomized field experiments and find treatment effects that fall somewhere between recent experimental studies of leafleting and observational studies of naturally occurring scandals that receive news coverage. Our studies break new ground in three ways: (1) by gauging the extent to which the effects of scandal vary depending on the prior visibility of the public figures receiving news attention, (2) by assessing the effects of news coverage on the opinions of both voters and party activists, and (3) by examining the extent to which news of individuals' wrongdoing changes voters' attitudes towards public institutions more generally.

## Theoretical Model

In abstract terms, the experiments described below are about learning. Residents are presented with a newspaper and either ignore it or update their prior views in light of what they have gleaned from its contents. Building on prior work that has used Bayes' Rule to characterize political learning (Gerber and Green, 1999; Gerber *et al.*, 2003), this section presents a model of learning in

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<sup>1</sup>The studies that come closest to doing so include those that send voters newspapers that contain report cards grading legislators' performance in office (Banerjee *et al.*, 2011) or that provide general election information and appeals for fair elections (Aker *et al.*, 2017). Both treatments show effects similar in magnitude to the Mexico and Brazil leafleting experiments.

which people update their prior beliefs after being exposed to a possibly biased newspaper that exposes a scandal. For concreteness, we consider the ways in which people update their beliefs about whether a given politician is prone to corrupt behavior in office, but the model applies as well to beliefs about other conjectures, such as the hypothesis that a given policy has harmful effects. The key feature of the model is that it acknowledges that readers may find the evidence presented in the newspaper to be ambiguous or slanted. The model provides both a set of testable propositions and a unified framework for interpreting experimental results.

We begin by briefly laying out the model's assumptions and notation. Let the parameter  $M$  represent a true underlying characteristic, such as the extent to which a given politician is corrupt. Before receiving an investigative newspaper, the recipient holds prior beliefs about the possible values of  $M$ . Suppose for analytic tractability that prior beliefs about  $M$  are distributed normally with mean  $\mu$  and variance  $\sigma_M^2$ . The larger  $\sigma_M^2$ , the more uncertain the reader is about the true parameter  $M$  in advance of seeing the information contained in the paper.

Some people who are sent a newspaper will read it, while others will ignore it. We assume that the share of readers in the population is  $\alpha$ . We further assume that non-readers are unaffected by receipt of the newspapers and do not update their prior beliefs about  $M$ . Updating occurs only among readers.

When encountering the newspaper, a reader harbors prior beliefs about its ideological bias. Let  $B$  be the random variable that denotes this bias. Suppose that prior beliefs about  $B$  are distributed normally with mean  $\beta$  and variance  $\sigma_B^2$ . Again, smaller values of  $\sigma_B^2$  indicate more precise prior knowledge about the newspaper's bias. Further, we assume that a reader's priors about  $M$  and  $B$  are independent; the intuition behind this assumption is that priors concerning a politician's proclivity for corrupt behavior in office are unrelated to whether the newspaper's slant is perceived to be liberal or conservative.

The newspaper conveys information about the politician's level of corruption, information that we denote  $X$ . Given that  $M = m$  (the truth about the politician equals  $m$ ) and  $B = b$  (the true bias equals  $b$ ), we assume that the distribution of  $X$  is normal with mean  $m + b$  and variance  $\sigma_X^2$ . The variance reflects ambiguity about what the evidence implies about the underlying value of  $M$ . The evidence itself may be unclear, or it may be conveyed in an ambiguous fashion by the newspaper.

Gerber *et al.* (2003) show that under these conditions, a reader's posterior distribution of  $M$  is normally distributed with  $E[M|X = x] = p\mu + (1-p)(x-\beta)$  and variance  $\sigma_{M|x}^2 = \frac{1}{\frac{1}{\sigma_M^2} + \frac{1}{\sigma_X^2 + \sigma_B^2}}$ , where  $p = \frac{\sigma_{M|x}^2}{\sigma_M^2}$ . In other words, the posterior mean is a weighted average of two terms: the prior expectation of the true

mean effect ( $\mu$ ) and the newspaper's report corrected by the prior expectation of the bias ( $x - \beta$ ). The weight attached to the newspaper's information depends on three factors. All else equal, the information receives more weight when readers (1) start off with more prior uncertainty, (2) regard the new information as credible, and (3) have little uncertainty about the newspaper's slant. Some limiting cases help illustrate these implications. Consider a case in which readers are completely uncertain about the newspaper's ideological bias; in this case,  $\sigma_B^2$  is infinite, and the newspaper has no effect on prior beliefs. Another interesting case arises when priors have infinite variance because readers have no prior information about the public official. In this case, the posterior entirely reflects whatever information is gleaned from the newspaper. Finally, as the newspaper's revelations more clearly demonstrate the public official's culpability, the smaller  $\sigma_X^2$  and the more weight the new information is accorded relative to prior beliefs.

This formalization suggests two hypotheses about the conditions under which information affects prior beliefs:

H1. All else being equal, readers who harbor more prior uncertainty about  $M$  will update their beliefs more sharply in the wake of new information.

This proposition implies that, *ceteris paribus*, experimental subjects who are less knowledgeable about politics prior to receiving the newspapers will be more influenced by reading them, since their  $\sigma_M^2$  is large relative to their  $\sigma_X^2$ . In practice, as Zaller (1992) points out, this prediction, which is conditional on exposure to the information, may be offset by less knowledgeable subjects' reluctance to read the newspaper. In the designs described below, this concern is partly addressed by targeting activists and voters with a strong record of turnout in low-salience primary elections, for whom  $\alpha$  is likely to be relatively high.

H2. All else being equal, prior beliefs that are held with more uncertainty will change more profoundly in the wake of new information.

This proposition implies that when updating beliefs about different public officials, readers will learn more when evaluating officials about whom they have little prior information. This pattern occurs because their  $\sigma_M^2$  is large relative to their  $\sigma_X^2$ . In the studies described below, this proposition leads to the prediction that opinions about little-known local figures will change more than opinions about prominent state officials, such as the governor.

In sum, this formalization provides a parsimonious framework that guides predictions about the persuasive effects of communication. Although the parameters of the updating model cannot be estimated directly, experiments shed light on their relative magnitudes by providing a sense of the extent and persistence of persuasive effects.

## Experimental Design

In this section, we describe the experimental designs for the five experiments, conducted between October 2014 and November 2015 in a Southern state. In sum, over 200,000 special edition newspapers covering state and local political scandals were mailed to subjects.

This study takes place in the context of economic upheaval within the newspaper industry. Faced with increasing competition from online news sources, newspapers have struggled to profit. Many local newspapers have closed or laid off relatively expensive investigative journalists, who in an earlier era may have worked months to break a story about scandal involving public officials. With many veteran journalists looking for work, nonprofit groups on the left and right have sought to fill this void. For instance, on the left Media Matters describes itself as a “progressive watchdog” to notify activists, journalists, pundits, and the general public about instances of “conservative misinformation” (Media Matters of America 2016).

The decline of paid newspapers masks the large market for free, advertising-based papers. Since the 1990s, large publishers including Metro International and Schibsted Media Group have offered free daily tabloids in densely populated urban areas such as subways, shopping centers, universities, and restaurants. These papers reach tens of millions of readers worldwide every day (Bakker, 2013). Even more common than these urban papers are free community newspapers. The National Newspaper Association reports that over 25 million free weekly newspapers are circulated in the United States (NNA, 2017). These newspapers provide community news packaged with advertisements and coupons for local businesses. The newspapers in our study most closely resemble these community newspapers with their local focus and free distribution. A redacted version of the first paper is displayed in Appendix H.

### *Experiment 1: University Scandal*

In Fall 2014, a local investigative journalism nonprofit prepared a “special edition” eight-page newspaper report on a scandal involving leaders at a local public university. University leaders allegedly granted admission to the under-qualified children of politicians, friends, and donors. The newspaper featured a detailed timeline of events, diagrams connecting various public officials to the scandal, and a sidebar with data on university operations.

Newspapers were mailed to two groups of voters: frequent voters, those who voted in at least two of the three preceding partisan primaries (for the same party),<sup>2</sup> in districts located near the university or represented by legislators

<sup>2</sup>Because the unit of assignment was the household and not the individual, household partisanship scores were calculated as the average turnout in party primaries over the past three primary elections for all registered voters in the household.

Table 1: Key features of the five experiments.

Topic	Subjects	Illustrative headline*	Outcome measures	Length	Date
Experiment 1: University Scandal	Frequent voters and activists. N: 118,191	“President forced to resign amid scandal”	— Familiarity — Corruption — Trust — Recall	Eight Pages	Fall 2014
Experiment 2: University Scandal	Frequent voters and activists. N: 89,131	“Cover-up escalates”	— Familiarity — Corruption — Favorability — Recall	Four Pages	Summer 2015
Experiment 3: Policy Reform Scandal	Frequent voters and activists. N: 55,324	“Legislator accused of undermining policy reform”	— Familiarity — Favorability — Recall	Four Pages	Summer 2015
Experiment 4: School Reform Scandal	Voters in select legislative districts and activists. N: 68,931	“Efforts to block school reform”	— Education reform — Donor influence — Favorability — Recall	Eight Pages	Fall 2015
Experiment 5: Local Government Scandal	Voters. N: 41,165	“Amid heated board debates, allegations of wrongdoing.”	— Familiarity — Corruption — Favorability — Recall	Eight Pages	Summer 2015

\* Illustrative headlines are similar to those that appeared in newspapers.

connected to the scandal; and “activists,” who were registered delegates to the state Republican Convention.

Outcomes were collected one to two weeks later by automated phone survey. The survey included questions about voters’ familiarity with the scandal, views

on whether “corruption is widespread” at the university, trust in government, and whether voters recalled receiving a “special edition newspaper” in the mail. Both the familiarity and recall questions serve as our manipulation checks, with the familiarity question appearing at the beginning and the recall question appearing at the end of the survey. Finally, the survey asked respondents for their age and gender. This allows us to match the person who responded to the survey with a voter in the household using corresponding information from the voter file.

### ***Experiment 2: University Scandal, an Update***

The Summer 2015 investigative newspaper featured a shorter, four-page update on the scandal at the local university under the headline “Cover-up escalates.” The story described the results of an outside investigation into the scandal.

The study included three groups of subjects: 1) the same frequent voters from the first experiment; 2) activists; and 3) frequent voters from districts not included in the first experiment. Standalone treatment effect estimates of the second newspaper focus on the third group, as this group had not previously been treated. This design also allows us to produce estimates on the decay of treatment effects for the first group, which was re-surveyed as part of Experiment 2. Subjects were mailed the newspapers in June 2015. Approximately two weeks later, subjects received an automated phone survey nearly identical to Experiment 1.

### ***Experiment 3: Policy Reform Scandal***

The third experiment again featured a shorter, four-page newspaper, this time containing one long-form article focused on a single legislator. The lead headline read: “Legislator Clark<sup>3</sup> accused of undermining policy reform.” The article describes a representative’s alleged abuse of political power in scuttling attempts to pass an ethics bill.

Subjects included both frequent voters residing in or near the legislator’s district and party activists.<sup>4</sup> Newspapers were mailed over a period of two weeks in August 2015. Once again, outcomes were measured through an automated phone survey conducted with treatment and control voters approximately one week after the newspapers were received. Survey questions included how familiar voters were with the scandal and whether they had a favorable or unfavorable opinion of elected officials named in the story. The survey included the standard questions on age, gender, and recall.

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<sup>3</sup>Pseudonyms are used for political figures throughout this paper.

<sup>4</sup>Activists were not broken down by type, but include a mix of convention delegates, past and present elected officials, and Tea Party leaders.



***Experiment 4: School Reform Scandal***

The fourth experiment involved a longer, eight-page newspaper with the lead story on “Efforts to block school reform.” The article featured a story of how House Speaker Davis and State House Representative Ian received donations from lobbyists and special interests to block school reform.

As with Experiment 3, subjects were of two types: voters who lived in districts represented by Speaker Davis, Rep. Ian, and other representatives connected to the scandal; and political activists. Newspapers were mailed over a period of two weeks in November 2015. Approximately one week later, an automated survey collected outcomes on attitudes toward both substantive policy questions and political figures. Voters were asked whether they supported school reform; whether “large campaign donations from public unions” influence legislators; and whether they had a favorable or unfavorable opinion of three political figures: Speaker Davis, Rep. Ian, and Mr. Henry, a wealthy businessman and major funder of special interest groups opposed to school reform. The survey asked the standard age, gender, and recall questions. However, in a change from previous surveys, the age and gender questions were moved to the end. This seemed to increase response rates, as discussed below.

***Experiment 5: Scandal in Local Government***

The lead story of the fifth investigative newspaper featured a scandal related to officials on a local government board. An eight-page newspaper discussed allegations of insider dealing by board members to benefit their friends and relatives. An interior sidebar connected members’ “cozy relationships” to a company earmarked for a “nearly \$1 billion development plan.”

Unlike other experiments, the randomization for this experiment occurred not at the household level, but by geography. Half of 250 geographic areas were randomly assigned to treatment. Subjects in treated and control areas received an automated survey by phone approximately 7–10 days after the newspapers were mailed. Survey questions included familiarity, favorability, and perceptions of corruption in local government.

**Treatment Assignment and Attrition**

In the first four studies, the unit of treatment assignment was the household. Voters in the subject population were grouped into households by shared last name and address so as to avoid sending multiple newspapers to the same location. One voter within each household was randomly selected to be the addressee of the newspaper (in treated households), and households were randomly selected to be treated or untreated. The fifth study was randomized by geography. Registered voters in areas assigned to treatment were all sent the

newspaper. Results for this study take into account the clustered assignment of subjects to treatment.

Attrition, or missing outcome data, occurred for several reasons. First, subjects in Experiment 1 who had been assigned to treatment and control were dropped from the study if their addresses or phone numbers were flagged as invalid by the survey vendor. Rates of attrition on account of bad addresses or phone numbers were between 1% and 3%. For the last four experiments, addresses and phone numbers were scrubbed prior to treatment assignment.<sup>5</sup>

Attrition primarily reflects low response rates to the end-line survey. Outcome measures were collected through automated telephone surveys, which exhibited response rates of 2.3–5.8%. Low response rates do not necessarily pose a threat to causal inference. Assuming that attrition occurs symmetrically between treatment and control groups, the estimand becomes the average treatment effect among the subset of the subject pool who would respond to the end-line survey regardless of their treatment assignment. The question, however, is whether attrition in fact occurs symmetrically in both experimental groups. In order to address this question, Table A1 presents t-tests of covariate balance for all subjects in the study population and for those subjects who responded to the survey. We find no statistically significant indications of imbalance in demographic characteristics among either the subject pool as a whole or among survey respondents. This finding is confirmed in a separate analysis that shows that rates of attrition are similar across experimental conditions. We conclude that attrition does not pose a threat to inference, although it does reduce power and limit generalizability.<sup>6</sup> Survey respondents are, on average, five years older than the subject population as a whole. If we reweight respondents by age and gender to match the population, results do not change.<sup>7</sup>

## Results

### *Manipulation Checks*

In order to assess the extent to which the newspapers were read and remembered by recipients, each survey included the question, “Over the past week or so, do

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<sup>5</sup>Budget considerations necessitated calling only a portion of treated voters for Study 1. Voters were randomly selected for the survey.

<sup>6</sup>The study remains quite powerful. For the politician favorability questions which are the focus of the analysis, there are 10,150 respondents who provided either a “favorable” or “unfavorable” evaluation for the 11 instances of a figure covered negatively in Studies 2–5. Across all 11 comparisons, the minimum detectable effect in average favorability ratings is approximately 2.1 percentage points, which is considerably lower than the 3.2 percentage point average change that we actually find.

<sup>7</sup>Appendix G displays full results with respondents reweighted by age and gender.

you recall seeing in the mail a special edition newspaper covering the [Name of Controversy], or not?" Respondents were given the choices "yes" or "no." Importantly, this recall question appeared as the last question in the survey, so as not to prime the treatment group to think of the newspaper they recently received.

One way to assess recall would be to tabulate the proportions of each treatment group that recalls seeing the newspaper. By this metric, the treatment remained memorable to between 20 and 50% of those who received it. A more cautious approach is to focus on the difference in recall rates between treatment and control conditions to offset the problem of faulty recall or demand effects. As Table 2 shows, the difference between those who said "yes" in treatment and control ranges from 12 percentage points in Experiment 1 to 23 percentage points in Experiment 3. These estimates are all strongly significant.

As an additional manipulation check, we asked respondents how familiar they were with the scandal. For Experiments 1, 2, and 5, we offered respondents four answer choices that ranged from "not too familiar" to "very familiar." In Experiment 3, response options were consolidated to "familiar" or "unfamiliar" to conserve survey time. In Experiment 4, the familiarity question was dropped altogether. As Table 2 shows, the difference between those who were somewhat or very familiar in treatment and control ranges from 5 percentage points to 10 percentage points in Experiments 1, 2, and 5. In Experiment 3, the difference between the percentage familiar in treatment and control was 6 percentage points, but this difference falls short of statistical significance.

Taken together, these manipulation checks leave little doubt that treatment groups received the newspapers, recalled receiving them, and were more familiar with the scandals covered in the newspapers.<sup>8</sup> All of these results suggest a reasonable value of  $\alpha$ , the share of readers in our experimental groups. In order to shed light on the parameter  $\sigma_B^2$ , the extent of uncertainty about source bias, a live survey call was placed to 200 treated subjects after the final study concluded. Sixty-four percent of respondents reported "a great deal" or "a fair amount" of "trust and confidence" in the papers' reporting about state and local politics, while only 26% trusted the paper "not very much" or "not at all."<sup>9</sup> For comparison, only 53% of respondents gave such positive evaluations to other media sources, while 46% gave negative evaluations. Over 70% of respondents rated the newspapers' performance "in exposing political leaders' wrongdoing" as "Excellent," "Very good," or "Good." According to our model, readers' high marks on the trustworthiness of the news coverage should enhance the persuasiveness of the newspapers.

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<sup>8</sup>It should be stressed that individual respondents' answers to the manipulation check cannot be used to assess treatment effects, as they are post-treatment, and those who recall the newspapers in the assigned treatment group are not comparable to those who recall the newspapers in the assigned control group.

<sup>9</sup>The remainder answered "Don't Know."



### **Estimation**

Estimated effects and accompanying standard errors are obtained using regression. We compare survey responses in the treatment groups with the responses in the control group, controlling for the background attributes of the respondents:

$$Y_i = a + b_1 d_i + g_1 X_{1i} + g_2 X_{2i} + \cdots + g_K X_{Ki} + u_i, \quad (1)$$

where  $Y_i$  indicates the survey response,  $d_i$  is an indicator variable for whether voter  $i$  was assigned to receive the newspaper, and  $u_i$  represents unmeasured determinants of turnout. The key parameter of interest is  $b_1$ , the average intent-to-treat effect (i.e., the effect of assignment to treatment). In order to improve the precision with which the parameter  $b_1$  is estimated, we control for covariates available in the voter file ( $X_{ki}$ ). These include indicator variables reflecting voter turnout in the 2014 Republican and Democratic Primary elections, indicator variables for the subject's age in years, gender, and an indicator variable that marks subjects for whom age is unknown. We report robust standard errors and  $p$ -values. See the supplementary materials for corresponding estimates obtained without covariate adjustment, which are quite similar.

### **Persuasion Effects on Frequent Voters**

We begin by examining the newspapers' effects on the views expressed by subjects drawn from the voter file (Table 3). By far the most consistent treatment effects are found in the domain of personal evaluations. The scandal investigations are akin to feature stories that depict protagonists and antagonists, and it seems clear that readers catch on to who the stories' villains and heroes are. Across four of the studies, surveys assessed evaluations of 11 figures who were negatively implicated in the newspaper stories. In all 11 instances, respondents in the treatment group gave more unfavorable average ratings than their counterparts in the control group. The proportion of respondents who provided a favorable evaluation of the political figure declined by 1–17 percentage points, with an average drop across figures of over 5 percentage points. The precision-weighted average estimated treatment effect across all instances is 3.2 percentage points (estimated standard error 0.8 percentage points; one-tailed  $p$ -value  $< 0.0001$ ). When newspaper coverage was positive in tone, evaluations moved in the predicted direction. Three figures received positive coverage, and their evaluations were more positive in the treatment group than the control group by 0.5–5.5 percentage points. Taken together, the survey ratings in all 14 instances change in a manner consistent with the tone of newspaper coverage.

Table 3: Estimated treatment effects on favorability of political figures for Studies 2–5 (voters).

<b>Negative coverage (predicting unfavorable ratings)</b>				
Political figure	Study 2	Study 3	Study 4	Study 5
Gov. Clark (SE)	0.009 (0.012)			
Speaker Davis	0.034 (0.025)	0.018 (0.089)	0.087 (0.031)	
President Evans	0.020 (0.029)			
President Fisher	0.022 (0.035)			
Rep. Green		0.044 (0.082)		
Mr. Henry			0.165 (0.039)	
Rep. Ian			0.135 (0.049)	
Allen				0.050 (0.035)
Baker				0.008 (0.037)
<b>Positive coverage (predicting favorable ratings)</b>				
Sen. Lane		0.055 (0.128)		
James				0.022 (0.042)
Knight				0.005 (0.042)
<b>Neutral coverage (predicting favorable ratings)</b>				
Gov. Clark		−0.080 (0.055)		
Martin				0.011 (0.050)

Regression includes demographic and partisan covariates. “Don’t Know” answers excluded. Robust standard errors reported except for Study 5, for which standard errors are clustered by geography.

### *Treatment Effect Heterogeneity*

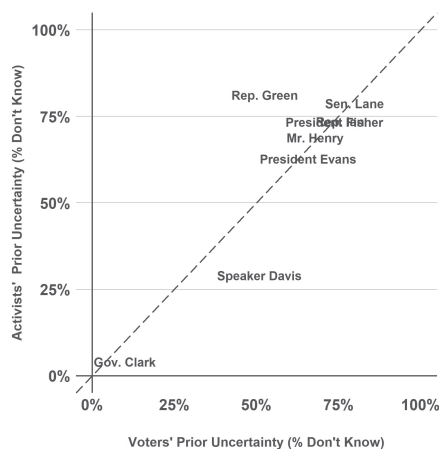
Heterogeneity is predicted across subjects and across topics. The Bayesian model predicts that newspapers' persuasive effects vary with subjects' prior uncertainty and prior beliefs about political figures. In this section, we investigate treatment effect heterogeneity among different subjects and across different topics.

The uncertainty that each question elicits may be measured by the percentage of "Don't Know" responses to a question in the control group. Figure 1 displays uncertainty across the political figure evaluations for separate groups of subjects: activists versus voters; Democratic versus Republican voters; Republican primary voters versus primary non-voters; and male versus female voters. Points along the 45-degree line are political figures who are equally well-known among both complementary groups of respondents; those off the 45-degree line are better known among one group than another.

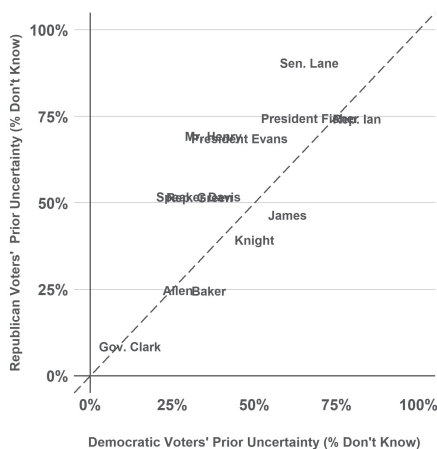
The four scatter plots indicate that awareness of the personae mentioned in the newspapers varied widely. All subgroups were well-acquainted with Governor Clark, but largely unfamiliar with the university presidents. The Speaker of the state house was the second best-known official, but he was unknown to half of subjects. State representatives were even less well-known. Figure 2 adds some additional depth to this description by depicting how favorably each figure was judged among respondents in the control group. Although our Bayesian model implies that everyone, regardless of their prior opinions, will update in the direction of the evidence gleaned from the newspaper, other models of motivated reasoning (Kunda, 1990; Redlawsk, 2002; Taber and Lodge, 2006) or cognitive priming (Lodge and Taber, 2005) predict that political information accentuates readers' prior beliefs. In effect, readers become more entrenched in their prior views when reading political feature stories. As Figure 2 make clear, Democrats and Republicans have sharply divergent prior opinions about prominent politicians. Governor Clark is widely supported by Republicans but opposed by Democrats. On the other hand, the two non-partisan government officials, the university presidents, are evaluated similarly by Democrats and Republicans.

Our model of Bayesian updating is best assessed using instance where prior uncertainty varies substantially across subgroups. From Figure 1, we see that prior uncertainty differed substantially between voters and activists for two figures. Rep. Green was relatively well-known among voters drawn from his district, but not activists drawn from across the state. Speaker Davis was well-known among activists, but not voters. The model suggests that the newspapers should be more persuasive among activists for Rep. Green and voters for Speaker Davis.

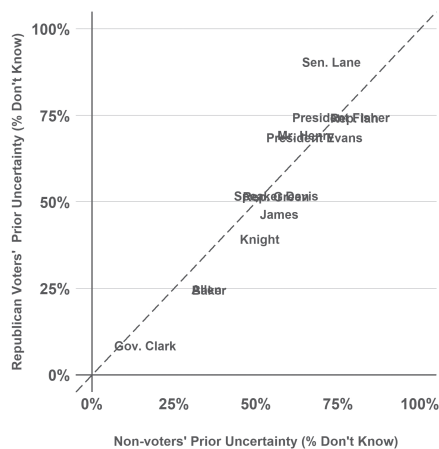
We observe the predicted pattern of treatment effects. For Rep. Green,  $\widehat{ITT} = 4.4$  percentage points among voters and 13.2 percentage points among



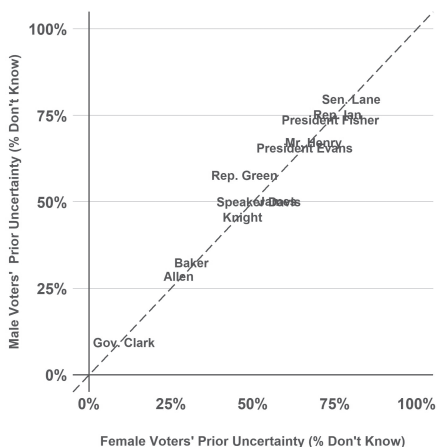
(a) Voters and Activists.



(b) Republican and Democratic Voters.



(c) Republican Primary Voters and Primary Non-voters.



(d) Male and Female Voters.

Figure 1: Estimated prior uncertainty across population subgroups. Uncertainty is defined as the percentage of “Don’t Know” responses among control group subjects.

activists. For Speaker Davis,  $\widehat{ITT} = 5.3$  percentage points among voters and 2.9 percentage points among activists. Both groups moved in the same direction, but the magnitude of the effects is larger for subgroups with greater prior uncertainty.



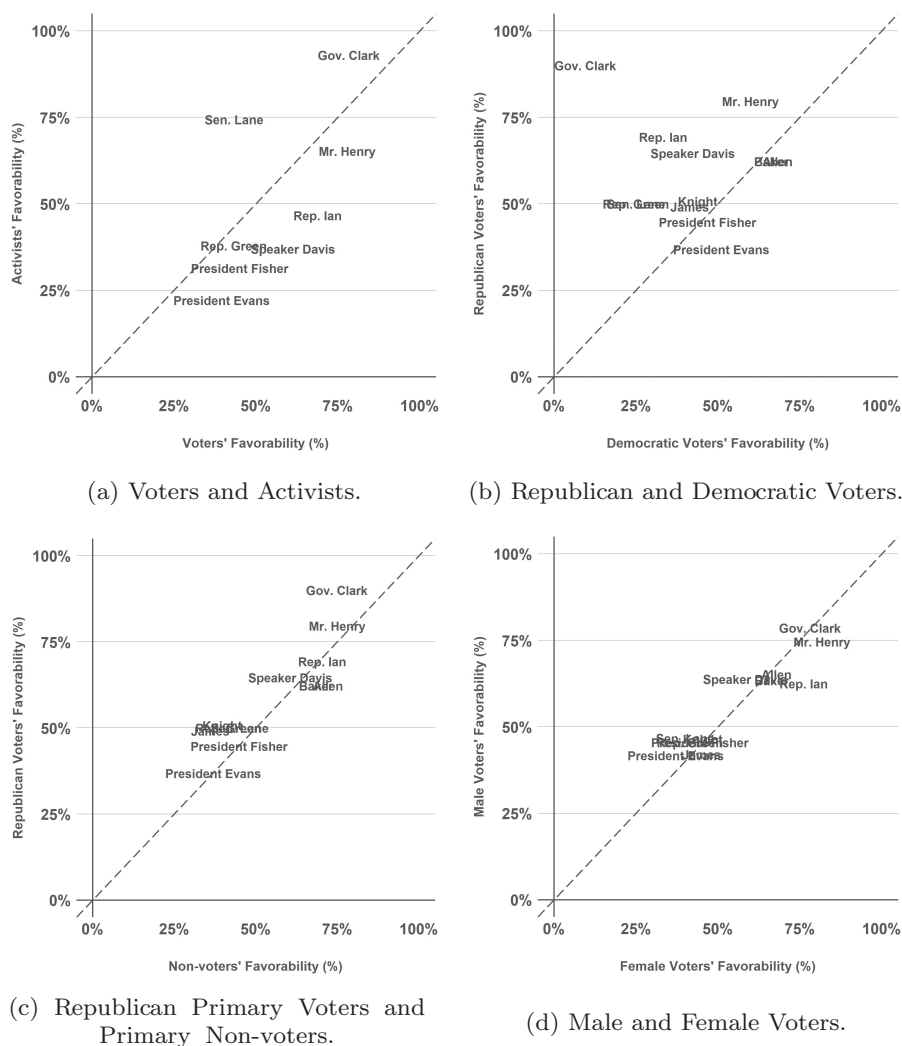


Figure 2: Estimated favorability across population subgroups. Favorability is defined as the percentage of “Favorable” responses among control group subjects who expressed a “Favorable” or “Unfavorable” opinion.

Only Experiment 2 includes a sufficient number of Democratic voters to examine heterogeneity in treatment effects across subjects with very different prior beliefs about the politicians. We compare the opinions of Democrats and Republicans with Gov. Clark and Speaker Davis, both Republicans

covered negatively in the paper. There were large partisan differences in prior evaluations of Gov. Clark, but there was little uncertainty in either party. Since subjects had low prior uncertainty  $\sigma_M^2$ , they updated little when presented with new information in the newspapers even though their prior beliefs  $M$  were very different. Both partisans' estimated treatment effects are smaller than 1 percentage point. Republicans and Democrats were much less certain about the Speaker. This uncertainty leads subjects to place greater weight on the evidence in the newspaper. Both updated unfavorably against Speaker Davis ( $\widehat{\text{ITT}}_{Rep} = 2.4$ ,  $\widehat{\text{ITT}}_{Dem} = 4.7$ ). The shifts in favorability among treated subjects are consistent with a relatively high  $\sigma_M^2$  for Speaker Davis among voters in both parties. The fact that subjects also shifted opinions in the same direction when assigned to the newspaper is consistent with voters having similar priors over the newspaper's bias ( $\beta$ ). They all interpreted the evidence as negative and updated in the same direction, contrary to the motivated reasoning hypothesis.

Because prior uncertainty seldom varied markedly across subgroups, our primary test of the updating model examines variation in persuasion effects across political figures. Our core hypothesis is that, all things being equal, voters update less when receiving information about people about whom they have prior information. Admittedly, our collection of political figures is not perfectly homogeneous in terms of background attributes (as would be the case for hypothetical candidates used in survey experiments), and so we cannot rule out the possibility that our results are driven by unmeasured attributes that are correlated with prior uncertainty. It should be noted, however, that these political figures are to all appearances quite homogeneous: they are all white males from the same state and the same political party.

Figure 3 plots estimated treatment effects against prior uncertainty for the 12 distinct survey questions about political figures with a "Don't Know" or "Not Sure" response option.<sup>10</sup> Results are plotted for both voters and activists.<sup>11</sup> Estimated treatment effects and prior uncertainty are both estimated quantities subject to sampling variability. Figure 3 indicates one standard error above and below the observed values with horizontal and vertical line segments. The thick, upward-sloping line shows the predicted value of persuasion at each value of prior uncertainty based on a weighted-least squares regression of treatment effects on prior uncertainty. Weights are the inverse of the standard error of the estimated treatment effects. We indicate 90% confidence intervals around the regression line by the shaded regions.

<sup>10</sup>For questions repeated across surveys, a weighted average is displayed. Evaluations of four figures, those in Study 5, were only obtained from voters.

<sup>11</sup>See Table C7 for estimated treatment effects for activists.

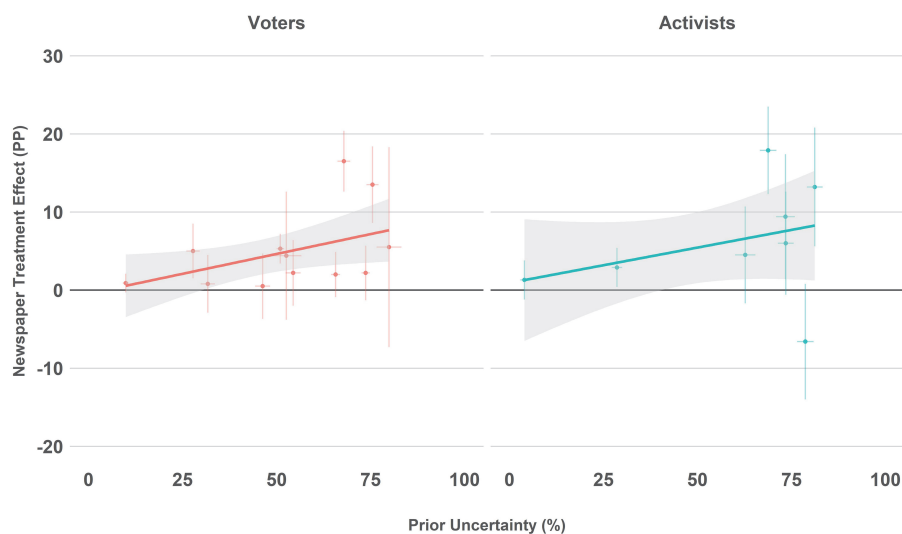


Figure 3: Estimated treatment effects and prior uncertainty, defined as percentage of “Don’t Know” responses among control group subjects. One standard error range indicated by horizontal and vertical segments for each observation. Weighted linear regression and 90% confidence interval also plotted. Estimated slope from WLS regression for voters is 0.10 ( $\widehat{se} = 0.05$ ); for activists is 0.09 ( $\widehat{se} = 0.08$ ); and for the pooled regression is 0.10 ( $\widehat{se} = 0.04$ ).

At low levels of prior uncertainty  $\widehat{\sigma}_M^2$ , treatment effects are indistinguishable from zero for both groups. Treatment effects rise with prior uncertainty. When four out of five respondents provide a “Don’t Know” response, predicted treatment effects are 7.7 percentage points for voters (with an estimated standard error of 2.2 percentage points) and 8.0 percentage points for activists (3.5 percentage points). The slopes from the two regressions are statistically indistinguishable from one another. If we pool the data for voters and activists, WLS regression with separate intercepts for the two groups yields an estimated slope coefficient of 0.10 ( $\widehat{SE} = 0.05$ ) and one-tailed  $p$ -value of 0.03.

These results suggest a possible explanation for why public opinion does not shift markedly or continually in response to media messages. Persuasion effects are strong when readers have little prior information. Among readers who previously were familiar with these public figures, persuasion effects are more muted. Conversely, these results help explain the enormous treatment effects often observed in survey experiments. Given weak priors about hypothetical candidates, respondents understandably find scandals informative and influential.

Table 4: Estimated treatment effects on trust in government institutions and policy preferences for all studies (voters).

Policy and government attitudes						
Question	Response	Study 1	Study 2	Study 3	Study 4	Study 5
Corruption (SE)	Yes	0.062 (0.034)	0.039 (0.025)			0.010 (0.042)
Trust	“None of the time” or “Only some of the time”	0.039 (0.023)				
School Reform	“Favor”				−0.008 (0.021)	
Influence from Donors	Yes				−0.001 (0.024)	

Regression includes demographic and partisan covariates. “Don’t Know” responses excluded. Robust standard errors reported except for Study 5 for which standard errors are clustered by geography.

Scope of Opinion Changes

Did respondents’ updating about individual politicians affect their assessments of public institutions more broadly? Table 4 shows that the newspapers significantly increased the share of respondents who believe that corruption or wrongdoing is widespread in public universities and local government. Respondents in Studies 1 and 2, for example, were asked “From what you know or have heard, is corruption widespread throughout the university administration, or not?” In Study 5, subjects were asked “From what you know or have heard, do you believe that the [local government board] has engaged in wrongdoing?” In the three experiments in which opinion toward government institutions was assessed, we find increases in the proportion of respondents who answer in the affirmative. This pattern was statistically significant in Studies 1 and 2, but not in Study 5. In the first study, we also asked about trust in government: “How much of the time do you think you can trust the [state government] to do what is right?” Respondents in the treatment group were more likely to say “None of the time” or “Only some of the time” than those in the control group, again suggesting that the newspapers’ coverage of the admissions scandal imparted a more cynical outlook. The estimated precision-weighted average treatment effect is 4.0 percentage points with an estimated standard error of 1.4 percentage points ( $p < 0.01$ ).

On the other hand, Experiment 4 suggests the limited extent to which the newspapers influenced specific policy views. Coverage of interest group maneuvers that were said to derail school reform legislation neither affected

respondents' support for school reform nor increased their likelihood of agreeing that "large campaign donations from public unions influence legislators to prevent the adoption of school [reform]..." Taken together, the results suggest that these newspapers affect global perceptions of corruption but do not leave an enduring imprint on policy positions or convey a detailed sense of how the legislative process operates. One explanation for these results is that voters have clearer preferences over policy and the role of unions compared to evaluations of government as a whole. More than one-half of subjects were "Not Sure" about corruption in government compared to fewer than one-fourth who were "Not Sure" about school policy or union influence. In the language of the Bayesian learning model, their prior uncertainty  $\sigma_M^2$  is smaller on matters of whether public funds should be used for private education and whether unions influence policy than on more abstract and general questions like trust in government or corruption.

## Conclusion

The experiments presented in this paper are unusual in at least four respects. First, they test the effects of media messages deployed in a naturalistic manner. The delivery of the investigative newspapers at subjects' homes was part of an organization's ongoing effort to spread the word about local and statewide scandals. Second, outcomes were assessed in a manner that did not alert respondents to the connection between the newspaper intervention and the survey questions that were posed to respondents. It was only at the end of the survey, after outcomes were assessed, that subjects were asked the manipulation check items. Third, in contrast to most lab and survey experiments, weeks elapsed between the intervention and the measurement of outcomes, and so our assessment represents a hard test for the hypothesis that newspapers change opinions. Finally, the study was replicated repeatedly using somewhat different interventions and sampling strategies. The net result is a well-powered suite of studies that features an array of outcome measures.

At the most basic level, these studies establish that newspapers are indeed read by high-propensity voters and activists. Manipulation checks consistently show that those in the treatment group were substantially more likely to recall having received the newspaper than their counterparts in the control group. Perhaps, the success with which the newspapers imparted their message reflects their splashy, accessible format.<sup>12</sup>

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<sup>12</sup>Another possibility is that the format works well, given the age of the respondents: those who answered the automated phone surveys were on average 69 years old, and newspaper readership remains frequent in this age cohort. However, a closer look at treatment effect heterogeneity within our subject pool reveals that respondents under 65 show strong manipulation check effects and persuasion effects similar to those found

The strongest and most robust pattern of effects concerned evaluations of the people who were the *dramatis personae* of the scandal stories. Here we find that evaluations of the villains deteriorated, and evaluations of the heroes improved. Overall, opinion change in the voter samples moved in the predicted direction 14 out of 14 times; among activists, opinion changed in the predicted direction 8 of 10 times.<sup>13</sup> The special resonance of the “feature article” content is consistent with prior research suggesting that such stories attract larger and more interested audiences than so-called “hard news” (Bell, 1991; Gans, 1979). Interestingly, these stories seem to engage and persuade both frequent voters and party activists.

The newspapers were less successful in shaping broader political attitudes. The papers had little effect on policy opinions, and in that sense our results are reminiscent of the null findings obtained by Gerber *et al.* (2009) in their study of exposure to mainstream newspapers. For example, we found little movement on the issue of school reform despite the fact that the newspaper coverage clearly implied that it was an attractive policy. On the other hand, the newspapers’ coverage of scandal was more successful at conveying the impression that corruption is widespread. This pattern of results is consistent with the idea that the media is less effective at effecting change on particular issues but more effective at enforcing broader social norms and focusing attention on particular events and persons (Lazarsfeld and Merton, 1948).

A closer look at the pattern of persuasive effects offers some insight into why opinion change occurs. Although these respondents are substantially more politically engaged than the general public, the people they are asked to rate are often relatively unknown. The persuasive effects of the newspaper climb steadily as the focus shifts from well-known public officials, such as the governor, to little-known people, such as local businessmen. This is not to say that well-known public figures are impervious to scandals. There is little doubt that scandals adversely affect presidential approval ratings (Brace and Barbara, 1991; Norpoth, 1996), even though the president is a well-known figure. Our evidence concerns the effectiveness of relatively meager news coverage — a single newsletter — not the avalanche of coverage that typically ensues when presidents or other high-ranking figures are embroiled in controversy. It is noteworthy that personal narratives can substantially shape public opinion in domains where it remains amorphous.

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among older respondents. Reweighting the survey sample by age and gender to match the composition of the broader voter population yields identical results.

<sup>13</sup>These figures exclude Gov. Clark in Experiment 3 and Mr. Martin in Experiment 5, as they were covered in a neutral manner by the newspaper.

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