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Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions[†]

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We conducted a field experiment to measure the effect of exposure to newspapers on political behavior and opinion. Before the 2005 Virginia gubernatorial election, we randomly assigned individuals to a Washington Post *free subscription treatment, a* Washington Times *free subscription treatment, or a control treatment. We find no effect of either paper on political knowledge, stated opinions, or turnout in post-election survey and voter data. However, receiving either paper led to more support for the Democratic candidate, suggesting that media slant mattered less in this case than media exposure. Some evidence from voting records also suggests that receiving either paper led to increased 2006 voter turnout.* (*JEL* D72, L82)

Citizens learn about politics and government from the news they watch on television and read in newspapers. Recent work has examined how the media shapes the public's political knowledge, attitudes, and behavior (Timothy J. Besley and Robin Burgess 2002, Alexander Dyck and Luigi Zingales 2002, and James T. Hamilton 2003). Media sources may influence the public not only through the slant of a particular report (Stefano DellaVigna and Ethan Kaplan 2007) but also by choosing what to cover (Lisa M. George and Joel Waldfogel 2006).

This paper reports the results of a natural field experiment[1] to measure the effect of political news content on political behavior and opinions. The Washington, DC area is served by two major newspapers, the conservative *Washington Times* and the more liberal *Washington Post* (Tim Groseclose and Jeffrey Milyo 2005). The presence of a liberal and conservative paper serving the same region creates an outstanding opportunity to study the effect of media slant in a naturalistic setting within a single population.[2] Approximately one month prior to the Virginia gubernatorial election in

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[†] To comment on this article in the online discussion forum visit the articles page at: http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.35.

[1] As per the taxonomy put forth in Glenn W. Harrison and John A. List (2004).

[2] The *Washington Post* is also a more nationally prominent newspaper than the *Washington Times*. Thus, readers may perceive a difference in quality and trustworthiness, not just slant, between these two newspapers.

November 2005, we administered a short survey to a random selection of households in Prince William County, a northeastern Virginia county. From the 3,347 households reporting that they received neither the *Post* nor the *Times*, we randomly assigned households to get subscriptions to either the *Post* or the *Times* for approximately ten weeks, or to a control group that was not sent either paper. A week after the election, we conducted a follow-up survey in which we asked individuals whether they voted in the November 2005 election; which candidate they selected (or preferred, if they said they did not vote); their attitudes toward the president, the political parties, and national political issues; their attitudes toward news events of the previous weeks; and their knowledge about recent news events. We also obtained voter turnout data for the November 2005 and 2006 elections from state administrative records.

Our research contributes to a large and growing literature on the effect of news media on political attitudes and behavior. The earliest media studies discounted the impact of mass communications, but most recent studies conclude that media exposure can have a sizable effect. The most common method of measuring the effect of media content is to use data from surveys to measure the association between a respondent's reported media exposure and his or her political views. Among the findings adduced to suggest media effects, researchers frequently document strong associations between exposure to media with a distinctive slant and the viewers' political attitudes and information (Peter Clarke and Eric Fredin 1978; Arthur Miller, Edie N. Goldenberg, and Lutz Ebring 1979; Carl R. Bybee et al. 1981; Gina M. Garramone and Charles K. Atkin 1986; Joel Lieske 1989; Craig Leonard Brians and Martin P. Wattenberg 1996; Russell J. Dalton, Paul A. Beck and Robert Huckfeldt 1998; and John R. Hibbing and Elizabeth Theiss-Morse 1998). While suggestive, this research design may produce upwardly biased estimates of media influence due to biased reports of media exposure and to selection bias from the tendency for individuals to seek out information that agrees with their pre-existing views (Timothy C. Brock 1965, Paul D. Sweeney and Kathy L. Gruber 1984, Matthew Gentzkow and James M. Shaprio 2005, and Sendhil Mullainathan and Andrei Shleifer 2005).

Several recent papers employ natural experiments to measure media effects on voter turnout. Gentzkow (2006) studies the introduction of television and shows that the introduction of television was associated with a decline in voter turnout, a sharp drop in the number of newspapers, and a drop in newspaper readership. Noting the political science literature linking newspaper readership to political participation (Michael Morgan and James Shanahan 1992), he argues that the rise in television is responsible for between one-quarter and one-half of the total decline in voter turnout from the 1950s to the 1970s. George and Waldfogel (2002) study the expansion of the national edition of the *New York Times*. They find that, as the *New York Times* displaces local newspaper readership, turnout in nonpresidential elections falls relative to turnout in presidential elections.

A recent study of the persuasive effect of political news (DellaVigna and Kaplan 2007) uses variation in the availability of the Fox News Channel across cable systems to measure the effects of the channel's news coverage on the Republican Party vote share in presidential elections (as well as the Republican share in Senate races and voter turnout). They estimate that Fox News caused about one-half of a percentage point shift toward George W. Bush in the 2000 presidential election. Since

Fox News is viewed by only a modest portion of the population, depending on which measure of viewership the authors use (either recall of watching Fox News or diary records of viewership, which show a smaller audience) this translates into a persuasion rate among those exposed to the channel of between approximately 5 percent and nearly 30 percent of the audience not already voting Republican in the presidential race. Given that citizens are unusually well informed and consistent in their presidential voting behavior, compared to voting for lower offices, these results suggest media slant can have a powerful political influence.

The large behavioral effects reported in DellaVigna and Kaplan (2007) accord with recent laboratory experimental evidence in which political advertising has substantial persuasive effects in gubernatorial and senate races (Stephen Ansolabehere and Shanto Iyengar 1995). In addition, a growing literature has employed randomized field experiments to measure the turnout effects of campaign mailings, phone calls, and face-to-face canvassing (Alan S. Gerber and Donald P. Green 2000, and Green and Gerber 2008). According to meta-analysis of dozens of studies of each of the alternative methods of voter mobilization, door-to-door canvassing prior to the election often has a large effect on voter turnout, raising turnout by approximately 8 percentage points in a typical election, while phone calls and mailings have more modest but still notable effects. A live phone call from a commercial firm raises turnout around 0.5 percentage points, a call from a volunteer raises turnout 2.5 percentage points, and several pieces of campaign mail boosts turnout by approximately 1 percentage point (Green and Gerber 2008).

In our experiment, we find no effect of receiving either paper on knowledge of political events, opinions of those events, or on voter turnout in the 2005 gubernatorial election. However, receiving either paper led to more support for the Democratic candidate, suggesting that media slant mattered less in this case than exposure to media. There was also some evidence of increased voter turnout in the 2006 election among those receiving either paper.

We contribute to the literature on the effect of media on politics by performing what appears to be the first field experiment measuring the effect of newspapers on political attitudes and behavior. Field experimentation has some advantages over the previous research strategies, namely use of a naturalistic setting and an assurance of orthogonality of treatment to observable and unobservable characteristics. Our study has some important limitations, the most important of which is the relatively small sample size. The sample size for the post-election survey was 1,081 interviews and the sample size for the data merged with administrative voting records is still only 2,571. Consequently, the standard errors of the estimated treatment effects were often large. Our standard errors are such that we fail to reject (two-sided, at the five percent level) the null hypothesis of no effect if we obtain a treatment effect estimate of less than a 3.6 percentage point change in voter turnout (using state voting data), a 7 percentage point increase in likelihood of voting for the Democratic candidate (using survey data), or less than an 8.6 percentage point differential between the *Post* and *Times* groups in likelihood of voting for the Democratic candidate.

The paper proceeds as follows. Section I describes the experiment in more detail. Section II presents the results. Section III discusses the implications of the findings, limitations of the research, and directions for future work.

I. Experimental Design and Data

A. *Experimental Procedures*

Households were drawn from a sample of residents in Prince William County, VA, a county 25 miles outside of Washington, DC. The county was suitable for our study because it is within the circulation of both the *Times* and the *Post* but far enough away from the Washington, DC area that the sample is not dominated by citizens involved professionally with politics. We sampled individuals from two lists: a list of registered voters and a consumer database list. Roughly equal proportions were included in the sample from each list.

We performed a baseline survey in September 2005.[3] We asked individuals if someone at the household received either the *Post* or the *Times*, and we excluded from the study those who said they received either newspaper. This perhaps is the most important issue to note regarding the formation of the sample frame. We are studying individuals who do *not* already subscribe to a newspaper, hence are examining the effect of exposing individuals who, on average, are less exposed to the media than the average individual. We also asked a number of other questions about newspaper readership and politics. Only individuals who completed all questions in the initial phone survey were included in the experimental sample.

Individuals were then randomly assigned to one of three groups: the *Post*, the *Times*, or a control group.[4] Participants received a postcard in the mail that said "Congratulations! You have won a free ten-week subscription to the *Washington Times*(*Post*)!"[5] Table 1A shows sample statistics from the baseline interview for the entire sample, broken down by treatment group. The lowest p-value for a test of independence across groups was 0.18 (for gender). Using the treatment assignment as the dependent variable in a multinomial logit model produced a p-value for the joint significance of the covariates of $p = 0.95$.

There were three noncompliance issues to note regarding treatment administration. First, 6 percent of households in the treatment groups opted out of the free subscription. In our analysis, we focus on intent to treat effects and include all treatment group subjects even if they cancelled. Second, some addresses (76 for the *Times*, 1 for the *Post*) were deemed "undeliverable."[6] Third, when we sent the list of households to the *Washington Post*, we were informed that 75 (out of 965) were already on the

[3] The complete survey is available at http://karlan.yale.edu/p/index.php.

[4] The study was launched in two waves each a week apart. This was done primarily due to capacity constraints in starting new subscriptions at the newspapers. Prior to the first wave, 50 households were removed at random for a small pilot study to gauge the refusal rate and get experience with the logistics of starting and stopping newspaper delivery. These households are excluded from the analysis.

[5] The postcard continued, "We have held a drawing to award free ten-week subscriptions of the *Washington Times* to households in Prince William County. Delivery begins this week. Delivery will automatically end after ten weeks, you do not need to call to cancel. However, if you want to cancel before the end of the ten weeks, please call 1-800-xxx-xxxx and we will remove you from this promotion. Thank you for trying out the newspaper."

[6] We verified that the papers we had ordered were actually delivered by having a research assistant observe a random sample of the treatment group households during the first wave. The *Times*, after reviewing the full list, reported that there were 76 addresses to which they were unable to deliver. The *Post* was able to deliver to all but one of the addresses. Undeliverable addresses are included in all analyses, but it is useful to note that these 76 (8 percent) addresses may be different along important characteristics, such as income.

TABLE 1A—SUMMARY STATISTICS FROM BASELINE SURVEY
(Sample frame: all baseline survey respondents, mean, standard errors, and standard deviations)

	All (1)	Control (2)	*Post* (3)	*Times* (4)	*p*-value (5)
% female	34.76	34.44	33.01	37.02	0.18
	(0.84)	(1.28)	(1.53)	(1.59)	
	[47.63]	[47.54]	[47.05]	[48.31]	
% voted in 2004 (self-report)	88.62	88.51	88.82	88.57	0.99
	(0.78)	(1.22)	(1.44)	(1.45)	
	[31.77]	[31.91]	[31.54]	[31.86]	
% voted in 2002 (self-report)	48.08	49.04	45.76	49.06	0.48
	(1.23)	(1.92)	(2.27)	(2.28)	
	[49.98]	[50.03]	[49.87]	[50.04]	
% voted in 2001 (self-report)	7.30	7.07	7.66	7.28	0.93
	(0.64)	(0.98)	(1.21)	(1.19)	
	[26.03]	[25.65]	[26.62]	[26.00]	
% from consumer list	50.91	52.58	49.95	49.37	0.24
	(0.86)	(1.32)	(1.61)	(1.62)	
	[50.00]	[49.95]	[50.03]	[50.02]	
% get news or political magazine	9.20	9.36	8.81	9.37	0.88
	(0.50)	(0.77)	(0.91)	(0.95)	
	[28.91]	[29.13]	[28.36]	[29.15]	
% prefers Democratic candidate for governor in VA	14.43	14.53	14.61	14.11	0.94
	(0.61)	(0.93)	(1.14)	(1.13)	
	[35.15]	[35.25]	[35.34]	[34.83]	
% no preference in VA governor race	14.82	14.18	15.54	15.05	0.63
	(0.61)	(0.92)	(1.17)	(1.16)	
	[35.53]	[34.89]	[36.25]	[35.78]	
% in wave 2 of random assignment	37.14	36.87	37.31	37.37	0.96
	(0.84)	(1.28)	(1.56)	(1.57)	
	[48.32]	[48.26]	[48.39]	[48.40]	
% participating in follow-up survey	32.30	31.70	32.02	33.47	0.65
	(0.81)	(1.23)	(1.50)	(1.53)	
	[46.77]	[46.55]	[46.68]	[47.21]	
Number surveyed—baseline	3,347	1,432	965	950	

Notes: Standard errors reported in parentheses; standard deviations in brackets. Column 5 reports the *p*-values for chi-squared tests of independence between treatments for each variable. The second through fourth rows (percent voted) apply only to the voter registration (i.e., nonconsumer) sample frame. All regressions in Tables 2–4 include controls for which sample frame provided the observation. A multinomial logit model predicting assignment to treatment using all of the above baseline variables yields a chi-squared test value of 9.21 (d.f. 18, *p*-value of 0.95).

Post subscription list (although it may be the case that these households were receiving only Sunday delivery). The *Times* has a lower subscription rate and reported only five households already subscribing. As group assignment was random, this suggests that some portion of the control group and *Times* treatment group, perhaps around 8 percent, was getting the *Post* at least on Sunday, and a much smaller portion of the *Post* treatment group and the control group was getting the *Times*. Since the treatment effect estimates are based on the difference in treatment rates between the treatment and control group, this suggests the treatment effect should be interpreted as the effect of boosting the household exposure rate to the *Post* by approximately 92 percentage points rather than 100 percentage points. Thus, any observed difference between the *Post* treatment group and the other groups will tend to underestimate, most likely by a small amount, the impact of exposure to the *Post*.

B. *Background on Newspapers and Media Coverage*

We followed the news reporting of both papers throughout the study period and recorded the choice of topics and headlines in each paper. Previous researchers have found that, as is widely believed, the *Post* leans left and the *Times* leans right (Groseclose and Milyo 2005). Our analysis of coverage confirms this assessment. While both papers gave extensive attention to the Iraq War, the *Times* had three above the fold headlines mentioning Iraqi efforts at forging a constitution and only one mention of the controversies involving Iraq detainees. The pattern for the *Washington Post* was the opposite, featuring three stories on detainees and one on the constitution. The *Post* gave much greater attention to Republican political difficulties. The disclosure of the identity of Valerie Plame, a Central Intelligence Agency (CIA) officer married to an administration critic, was given very extensive coverage in the *Post* and much less prominence in the *Times*. In place of the *Post*'s emphasis on the Plame issue and other administration controversies, the *Times* gave more coverage to the filling of the impending Supreme Court vacancy. The *Times* had about twice as many above the fold stories on the nominations of Harriet Miers and Samuel Alito for the Supreme Court. Both papers covered Hurricane Wilma, but the *Post* also had several stories about preparations for and the response to Hurricane Katrina.[7]

The manner in which subjects were covered also suggests the *Times* was more favorable to the administration than was the *Post*. A comparison of same day headlines illustrates this. On Tuesday, October 18, 2005, both papers had front page stories on Iraq. The *Times* story had the headline "No tears for Saddam in Iraq," while the *Post* ran the headline "Iraqis Say Airstrikes Kill Many Civilians." On November 4, 2005, the *Times* ran a story with the headline "Recruits Join Armed Forces Seeking War—A Sort of Vendetta Spurs Youth to Enlist After 9/11," whereas on November 7, 2005, the *Post* ran a story with the headline "Youths in Rural U.S. Are Drawn to Military—Worries About Jobs Outweigh War Fears." When Miers nomination to the Supreme Court was withdrawn, a *Times* headline read simply "Miers Rules Herself Out" (October 28, 2005), while the *Post* used the headlines "Miers Withdrawn as Nominee for the Court" and "Nomination was Plagued by Missteps from the Start."

Both newspapers gave the governor's race extensive coverage. The *Post* had 15 stories on the front page or first page of the Metro section while the *Times* had ten articles. National events may have affected the gubernatorial race in Virginia as well. News reports cited national issues as persuasive to many voters in the Virginia election.[8]

[7] Two tables summarizing the front page, above-the-fold stories, by topic, for the *Post* and the *Times*, along with a listing of every headline as well as every headline on the Metro page that related to the Virginia gubernatorial election, are available at http://karlan.yale.edu/p/index.php.

[8] "Moments foreshadowing a political collapse" *The Virginian-Pilot*, November 13, 2005; "'Twas a Famous Victory, & Republicans Have Some Issues" *Richmond Times Dispatch*, November 13, 2005; "New GOP Agenda, Many Things Combined to Cripple Kilgore's Gubernatorial Hopes" *Richmond Times Dispatch*, November 13, 2005.

TABLE 1B—SUMMARY STATISTICS FROM BASELINE SURVEY
(Sample frame: only those who completed the follow-up survey, mean, standard errors, and standard deviations)

	All (1)	Control (2)	*Post* (3)	*Times* (4)	*p*-value (5)
% female	32.86	31.54	36.84	30.89	0.21
	(1.44)	(2.20)	(2.77)	(2.61)	
	[46.99]	[46.52]	[48.32]	[46.28]	
% voted in 2004 (self-report)	90.70	92.58	89.24	89.53	0.44
	(1.23)	(1.74)	(2.47)	(2.34)	
	[29.07]	[26.27]	[31.09]	[30.70]	
% voted in 2002 (self-report)	55.99	57.64	50.63	58.72	0.27
	(2.10)	(3.27)	(3.99)	(3.76)	
	[49.68]	[49.52]	[50.15]	[49.38]	
% voted in 2001 (self-report)	8.41	9.17	8.23	7.56	0.84
	(1.17)	(1.91)	(2.19)	(2.02)	
	[27.78]	[28.92]	[27.57]	[26.51]	
% from consumer list	48.29	49.56	48.87	45.91	0.59
	(1.52)	(2.35)	(2.85)	(2.80)	
	[49.99]	[50.05]	[50.07]	[49.91]	
% get news or political magazine	11.29	10.35	11.00	12.89	0.54
	(0.96)	(1.43)	(1.78)	(1.88)	
	[31.66]	[30.50]	[31.34]	[33.57]	
% prefers Democratic candidate for governor in VA	19.43	19.60	21.04	17.61	0.55
	(1.20)	(1.87)	(2.32)	(2.14)	
	[39.58]	[39.74]	[40.82]	[38.15]	
% no preference in VA governor race	12.86	13.22	10.03	15.09	0.16
	(1.02)	(1.59)	(1.71)	(2.01)	
	[33.49]	[33.90]	[30.09]	[35.86]	
% in wave 2 of random assignment	35.06	35.02	38.51	31.76	0.21
	(1.45)	(2.24)	(2.77)	(2.61)	
	[47.74]	[47.76]	[48.74]	[46.63]	
Number surveyed—follow-up	1,081	454	309	318	

Notes: Standard errors reported in parentheses; standard deviations in brackets. Column 5 reports the *p*-values for chi-squared tests of independence between treatments for each baseline variable. A multinomial logit model predicting assignment to treatment using all of the above baseline variables yields a chi-squared test value of 17.62 (d.f. 18, *p*-value of 0.48).

C. *Outcome Data*

During the week after the November election, we reinterviewed 1,081 of the 3,347 individuals in our sample for the follow-up survey. Response rates of 30 or 40 percent are typical in the public opinion literature (Herbert Asher 2004).[9] Survey respondents were not told of any link between the free subscriptions and the phone surveys. The follow-up survey asked questions about the 2005 Virginia gubernatorial election (e.g., did the subject vote, which candidate was voted for or preferred), national politics (e.g.,

[9] Public opinion literature suggests that increasing the response rate from 30–40 percent to 60 percent produces similar results for many topics including attention to media, engagement in politics, and social and political attitudes (Scott Keeter et al. 2000). The complete set of dispositions was: Survey completed 31.8 percent, Refused to answer/Not interested 29.7 percent, Person not available 10.3 percent, Answering machine 9.8 percent, Partial survey/refused 6.0 percent, Disconnect 4.1 percent, Do not call/irate 3.1 percent, Ring no answer 1.7 percent, Wrong number 1.5 percent, Language barrier 1.1 percent, Busy signal 0.8 percent, Rescheduled call 0.1 percent, Deceased 0.03 percent.

TABLE 1C—SUMMARY STATISTICS FOR OUTCOME MEASURES
(Sample frame: only those who completed the follow-up survey, mean, standard errors, and standard deviations)

	All (1)	Control (2)	*Post* (3)	*Times* (4)
% voted 2005—self-reported from survey	0.73	0.73	0.72	0.74
	(0.01)	(0.02)	(0.03)	(0.02)
	[0.44]	[0.45]	[0.45]	[0.44]
% voted 2005—administrative state voting data[a]	0.57	0.56	0.57	0.56
	(0.01)	(0.02)	(0.02)	(0.02)
	[0.50]	[0.50]	[0.49]	[0.50]
% voted 2006—administrative state voting data[a]	0.64	0.63	0.65	0.66
	(0.01)	(0.01)	(0.02)	(0.02)
	[0.48]	[0.48]	[0.48]	[0.47]
% voted for Democrat in 2005 VA election	0.45	0.41	0.49	0.45
	(0.02)	(0.03)	(0.04)	(0.03)
	[0.50]	[0.49]	[0.50]	[0.50]
% did not vote, but preferred Democrat	0.40	0.42	0.41	0.35
	(0.03)	(0.05)	(0.06)	(0.05)
	[0.49]	[0.50]	[0.50]	[0.48]
% voted for or preferred Democrat	0.43	0.41	0.47	0.42
	(0.02)	(0.02)	(0.03)	(0.03)
	[0.50]	[0.49]	[0.50]	[0.50]
Most important problem	0.92	0.92	0.93	0.91
(1 = issue other than scandals, 0 = scandals)	(0.01)	(0.01)	(0.01)	(0.02)
	[0.27]	[0.27]	[0.25]	[0.28]
Most important issues in Iraq	0.52	0.53	0.49	0.55
(1 = Iraq constitution or Hussein trial, 0 = other)	(0.02)	(0.02)	(0.03)	(0.03)
	[0.50]	[0.50]	[0.50]	[0.50]
Progress in Iraq	1.35	1.36	1.33	1.35
(3 = going very well, 0 = going very badly)	(0.03)	(0.04)	(0.06)	(0.06)
	[0.97]	[0.93]	[1.01]	[1.00]
Leak case	1.75	1.74	1.72	1.79
(3 = did nothing wrong, 1 = something illegal)	(0.02)	(0.04)	(0.05)	(0.04)
	[0.74]	[0.74]	[0.74]	[0.74]
Alito confirmation	2.34	2.37	2.27	2.38
(3 = should confirm, 1 = should not confirm)	(0.02)	(0.03)	(0.04)	(0.04)
	[0.65]	[0.65]	[0.67]	[0.63]
Specific issue index	0.02	0.03	−0.03	0.04
(standardized values of above 5—higher scores conservative)	(0.02)	(0.03)	(0.04)	(0.04)
	[0.67]	[0.67]	[0.70]	[0.65]
Republican favorable	2.47	2.50	2.41	2.48
(4 = very favorable, 1 = very unfavorable)	(0.03)	(0.05)	(0.06)	(0.06)
	[1.01]	[1.02]	[1.00]	[1.02]
Democrat unfavorable	2.62	2.63	2.57	2.65
(4 = very unfavorable, 1 = very favorable)	(0.03)	(0.05)	(0.06)	(0.05)
	[0.95]	[0.97]	[0.95]	[0.93]
Bush approval	2.43	2.49	2.37	2.41
(4 = strong approval, 1 = strong disapproval)	(0.04)	(0.06)	(0.08)	(0.08)
	[1.32]	[1.31]	[1.31]	[1.34]
Conservatism	4.51	4.56	4.38	4.58
(7 = extreme conservative, 1 = extreme liberal)	(0.05)	(0.07)	(0.09)	(0.08)
	[1.45]	[1.43]	[1.50]	[1.44]
Broad policy index	0.00	0.03	−0.06	0.02
(standardized values of above 4—higher scores conservative)	(0.02)	(0.04)	(0.04)	(0.04)
	[0.79]	[0.79]	[0.79]	[0.78]
% knew number dead in Iraq	0.78	0.78	0.78	0.79
	(0.01)	(0.02)	(0.02)	(0.02)
	[0.41]	[0.41]	[0.42]	[0.41]

TABLE 1C— SUMMARY STATISTICS FOR OUTCOME MEASURES (*Continued*)

	All (1)	Control (2)	*Post* (3)	*Times* (4)
% identified Libby as involved in leak	0.74	0.75	0.70	0.75
	(0.01)	(0.02)	(0.03)	(0.02)
	[0.44]	[0.43]	[0.46]	[0.43]
% identified Miers as Supreme Court nominee	0.78	0.79	0.73	0.81
	(0.01)	(0.02)	(0.03)	(0.02)
	[0.42]	[0.41]	[0.45]	[0.39]
Fact index	−0.01	0.01	−0.08	0.04
(standardized values of above 3)	(0.02)	(0.03)	(0.04)	(0.04)
	[0.74]	[0.73]	[0.76]	[0.71]
Number surveyed—follow-up	1,081	454	309	318
Number merged (administrative voting data)	2,571	1,087	748	736

Notes: Standard errors in parentheses; standard deviation in brackets. Number of observations varies due to refusal to answer or "no opinion" responses.

[a] Sample frame for the administrative data outcomes in rows 2 and 3 include all individuals for whom we successfully matched state administrative data with the baseline, whereas all other rows include only the individuals who completed the follow-up survey.

favorability ratings for Bush, the Republicans, the Democrats, support for Supreme Court nominee Alito), and knowledge of news events (e.g., does subject know the number of Iraq war dead, has subject heard of I. Lewis "Scooter" Libby).

For analyzing the effects on voter turnout, we also obtained administrative records of individual voter turnout and successfully merged these data for 2,571 (76.8 percent) of the individuals in the baseline (not all individuals in the sample frame were registered voters).

Table 1B shows baseline sample statistics for the subsample of subjects that completed the post-election survey, and Table 1C shows the summary statistics for all outcome measures analyzed in further tables. Table 1B shows that assignment to treatment appears orthogonal to all covariates, even after attrition. Appendix Table A1 provides further evidence of this by examining whether the interaction of baseline covariates and assignment to treatment predicts attrition. We do not find that either treatment led to a sample selection bias in terms of the characteristics of individuals who responded to the follow-up survey. Individuals who voted in 2002 and subscribed to a news magazine (hence are more engaged in politics), as well as those who preferred the Democratic candidate for governor in the baseline, were more likely to complete the follow-up phone survey. However, this sample selection bias is not correlated with assignment to treatment (Appendix Table A1, column 2). Regardless, all results we present later in the paper include these same baseline covariates in order to address potential bias from the sample response. If there is attrition based on unobserved variables that are correlated with the outcome measures but not predicted by the observables, our results may be biased.

One limitation of this study is that while we know which households received newspapers, we cannot be sure that the newspapers were read. Our follow-up survey provides three measures of the effect of newspaper provision on newspaper reading: whether subjects receive a newspaper, which newspaper they receive, and the frequency with which they read a newspaper. Table 2 shows the relationship between treatment assignment and five distribution and readership outcomes. All

Table 2—Paper Distribution and Readership
(OLS)

	Receives a newspaper (1)	Receives the *Post* (2)	Receives the *Times* (3)	Frequency reads a paper (0 = Never, 3 = Every day) (4)	Reads at least several times/week (5)
Panel A: Separate treatment effects estimated for Washington Post *and* Washington Times					
Washington Post treatment	0.287	0.344	−0.006	0.151	0.089
	(0.035)	(0.035)	(0.021)	(0.084)	(0.038)
Washington Times treatment	0.100	0.031	0.133	0.086	0.057
	(0.034)	(0.035)	(0.021)	(0.083)	(0.037)
Adjusted R^2	0.32	0.18	0.19	0.24	0.18
F-test: *Post* = *Times*	24.76	66.96	37.81	0.51	0.63
p-value	0.00	0.00	0.00	0.48	0.43
Panel B: Pooled treatment effect estimated for receiving either newspaper					
Received either *Post* or *Times* treatment	0.191	0.183	0.066	0.118	0.072
	(0.029)	(0.031)	(0.018)	(0.070)	(0.032)
Adjusted R^2	0.30	0.11	0.15	0.24	0.18
Observation counts for both panels					
Observations	1,080	1,080	1,080	1,075	1,075
Refused/no opinion	1	1	1	6	6
Total surveyed in follow-up	1,081	1,081	1,081	1,081	1,081

Notes: Standard errors in parentheses. Dependent variables are dummy variables based on responses to the post-election survey. We include the following covariates: gender; reported age; three separate indicators for voting in the 2001, 2002, and 2004 general elections; an indicator for whether the respondent was drawn from a consumer list; self reports of receiving any news or political magazines; baseline survey self reports of preferring the Democratic candidate in the gubernatorial election and having no preference in the gubernatorial election; and an indicator for wave of the study. If a covariate value was missing, an indicator variable was included and the covariate was coded as zero. We include strata indicators, variables for each strata formed prior to the randomization, which included unique combinations of the following: intention to vote, receive a paper (non-*Post*/non-*Times*), mentions ever reading a paper, gets a magazine, and asked whether they wish they read the paper more. We also include surveyor/date indicators, a set of indicator variables for each unique combination of surveyor, and date for the follow-up survey.

pooled treament effect estimates are statistically significant at the .01, .05, or .1 level and the pattern of results matches the subject treatment group assignment (i.e., the *Post* treatment group reported receiving the *Post* and not the *Times*, and the *Times* treatment group reported receiving the *Times* and not the *Post*). The coefficients, however, suggest less than full readership of those in the treatment groups. No doubt some of this is due to individuals ignoring the free subscription altogether, but it may partly be due to the wording of the question. Individuals may have answered "no" to "receiving a newspaper" because they did not see their free trial as a true subscription. Additionally, questions about whether one reads the paper may have been interpreted as inquiring about typical rather than very recent behavior. We also found in the baseline survey that some respondents reported not receiving a newspaper, when in fact the newspaper was delivered to them regularly. There is further evidence that the newspapers were not disregarded. The *Post* informed us that, as of March 2006 (three months after the free subscription ended), approximately 17 percent of the treatment group had decided to subscribe to the *Post*.[10]

[10] The *Times* did not provide us the comparable resubscription figure.

TABLE 3—EFFECT OF TREATMENT ON POLITICAL KNOWLEDGE AND ATTITUDES
(OLS)

	Fact Accuracy Index (higher is more accurate) (1)	Specific Issue Index (higher is conservative) (2)	Broad Policy Index (higher is conservative) (3)
Panel A: Separate treatment effects estimated for Washington Post *and* Washington Times			
Washington Post treatment	−0.023	−0.045	−0.085
	(0.057)	(0.049)	(0.055)
Washington Times treatment	0.047	−0.027	−0.051
	(0.056)	(0.048)	(0.054)
Adjusted R^2	0.16	0.25	0.32
F-test: *Post* = *Times*	1.32	0.12	0.33
p-value	0.25	0.73	0.57
Panel B: Pooled treatment effect estimated for receiving either newspaper			
Received either *Post* or *Times* treatment	0.013	−0.036	−0.068
	(0.047)	(0.041)	(0.046)
Adjusted R^2	0.15	0.25	0.32
Observation counts for both panels			
Observations	1,080	1,081	1,076
Refused/no opinion	1	0	5
Total surveyed in follow-up	1,081	1,081	1,081

Notes: Standard errors in parentheses. Dependent variables are indexes constructed by summing the standard deviations from the mean for each of the specific questions for each index. See Appendix Table A2 for regressions showing the treatment effects for each question used in the construction of the indices. The Fact Accuracy Index is based on responses to three factual questions (identified number dead in Iraq in a closed-ended question, identified "Scooter" Libby from a list of four individuals as Dick Cheney's chief of staff who recently resigned, identified Harriett Miers from a list of four individuals as a recent female US Supreme Court nominee). The Specific Issue Index is based on five questions on political issues (three questions about Iraq and the war, a question on the Plame leak, a question about the Alito confirmation). The Broad Policy Index is based on four questions about attitudes toward the political parties, President Bush, and ideological self-placement on a 7 point scale.

We include the following covariates: gender; reported age; three separate indicators for voting in the 2001, 2002, and 2004 general elections; an indicator for whether the respondent was drawn from a consumer list; self reports of receiving any news or political magazines; baseline survey self reports of preferring the Democratic candidate in the gubernatorial election and having no preference in the gubernatorial election; and an indicator for wave of the study. If a covariate value was missing, an indicator variable was included and the covariate was coded as zero. We include strata indicators, variables for each strata formed prior to the randomization, which included unique combinations of the following: intention to vote, receive a paper (non-*Post*/non-*Times*), mentions ever reading a paper, gets a magazine, and asked whether they wish they read the paper more. We also include surveyor/date indicators, a set of indicator variables for each unique combination of surveyor, and date for the follow-up survey. All results remain qualitatively similar, and statistical significance remains as-is, using probit or ordered probit specifications instead of OLS.

II. Results

We measured the effect of the newspapers on political knowledge and attitudes (Table 3) and voting behavior (Table 4). All models include the baseline covariates, and fixed effects for strata, survey date, and surveyor.

First, we examine whether the treatments led to greater knowledge of political issues in the news. The dependent variable in Table 3, column 1 is an index of whether the respondent correctly answered three factual questions. The effects on the individual questions that comprise the indices are shown in Appendix Table A2. Neither of the newspapers improved the subject's ability to answer factual questions about the recent news, and no effect is found in aggregate. Table 3, columns 2 and 3 report two indices of political attitudes. Column 2 is a "Specific Policy" index of

TABLE 4—EFFECT OF TREATMENT ON VOTING BEHAVIOR IN VIRGINIA GOVERNORS RACE
(OLS)

	Voted in 2005 election[a] (1)	Voted in 2005 election[b] (2)	Voted in 2006 election[b] (3)	Voted for Democrat (set to missing if did not vote)[a] (4)	Voted for Democrat (set to zero if did not vote)[a] (5)
Panel A: Separate treatment effects estimated for Washington Post *and* Washington Times					
Washington Post treatment	−0.001	0.011	0.025	0.112	0.072
	(0.033)	(0.019)	(0.019)	(0.045)	(0.035)
Washington Times treatment	0.005	−0.006	0.031	0.074	0.060
	(0.033)	(0.019)	(0.020)	(0.045)	(0.035)
Adjusted R^2	0.21	0.39	0.31	0.31	0.26
F-test: *Post* = *Times*	0.03	0.65	0.10	0.58	0.09
p-value	0.86	0.42	0.75	0.44	0.76
Panel B: Pooled treatment effect estimated for receiving either newspaper					
Received either *Post* or *Times* treatment	0.002	0.003	0.028	0.093	0.066
	(0.028)	(0.016)	(0.016)	(0.038)	(0.029)
Adjusted R^2	0.21	0.39	0.31	0.31	0.26
Observation counts for both panels					
Observations	1,079	2,571	2,571	718	1,003
Refused/not asked	2			363	78
Total not merged (columns 2 and 3)		776	776		
Total surveyed in follow-up	1,081			1,081	1,081

Notes: Standard errors in parentheses. The following covariate variables are included in all specifications: gender; reported age; three separate indicators for voting in the 2001, 2002, and 2004 general elections; an indicator for whether the respondent was drawn from a consumer list; self report of receiving any news or political magazines; baseline survey self reports of preferring the Democratic candidate in the gubernatorial election and having no preference in the gubernatorial election; and an indicator for wave of the study. If a covariate value was missing, an indicator variable was included and the covariate was coded as zero. We include strata indicators, which are variables for each strata formed prior to the randomization, which included unique combinations of the following: intention to vote, receive a paper (non-*Post*/non-*Times*), mentions ever reading a paper, gets a magazine, and asked whether they wish they read the paper more. All results remain qualitatively similar, and statistical significance remains as-is, using probit specifications instead of OLS.

Data source:

[a] Survey.

[b] Administrative voting records.

five specific issues (most important problem in America, most important problem in Iraq, progress in Iraq, investigation regarding Plame, and the Alito confirmation). Column 3 is a "Broad Policy" index of four broad political views (Bush's approval rating, Republican favorable rating, Democrat favorable rating, and respondent's reported political ideology). For each of these indices, more conservative is a higher number.

The *Post* and *Times* treatment had no significant effect on shifting either the Broad Policy index toward the Democrats (*Post* coefficient of −0.085 std, $p < 0.12$; *Times* coefficient of −0.051 std, $p < 0.34$; *Post* and *Times* groups, −0.068 std, $p < 0.14$) or the Specific Policy index (*Post* coefficient of −0.045 std, $p < 0.35$; *Times* coefficient of −0.027 std, $p < 0.57$). Considering the newspaper treatment groups together, we also find negative (toward liberal) point estimates, but they are not statistically significant.

Table 4 shows the effect of the newspapers on voter turnout (self-reported and from administrative data) and which candidate individuals voted for or preferred.

The first set of regressions measure the turnout effects. There was no effect on either self-reported or administratively measured turnout for the 2005 election (panel B, column 1: coefficient for self-reported outcome is 0.2 percent, s.e. = 2.8 percent; column 2: coefficient for administrative outcome is 0.3 percent, s.e. = 1.6 percent). In November 2006 (column 3), however, the administrative data show a 2.8 percentage point higher voter turnout, with a standard error of 1.6 percentage points ($p < 0.11$). It is surprising to see a result in 2006 but not in 2005. This could be a result of the post-election receipt of the remainder of the ten-week newspaper subscriptions or the treatment that resulted from the 17 percent of the *Post* treatment group who renewed their subscription after the free period ended.

The dependent variables in columns 4 and 5 of Table 4 are dummy variables equal to one if the subject reported voting for the Democratic candidate in the 2005 gubernatorial election. In column 4, the sample is restricted to those who reported voting, while column 5 includes all respondents (coding those who did not vote at all as zero, i.e., as not voting for the Democratic candidate). The newspapers had an effect on which candidate the subject supports. Getting the *Post* is estimated to increase the probability of selecting the Democrat by 11.2 percentage points ($p < 0.014$) among those who reported voting (column 4) and by 7.2 percentage points ($p < 0.043$) across all respondents. Contrary to initial expectations, the right leaning *Times* was also associated with an increase in the probability of a Democratic vote in the Virginia governor's race. The effect was about ¾ as large as that estimated for the *Post* treatment (7.4 percentage points and $p < 0.10$, and 6.0 percentage points and $p < 0.084$, respectively for columns 4 and 5). The difference between the *Post* and *Times* point estimates is not statistically significant.

III. Conclusion

Our investigation of the effect of newspapers on political attitudes, behavior, and subject knowledge of news events found that even short exposure to a daily newspaper appears to influence voting behavior and may affect turnout behavior.

Contrary to our expectations, despite the slant of the newspapers the effects were qualitatively similar for the *Post* and the *Times*. One explanation for our findings is the particular news environment, which was politically challenging for Republicans. During the period that the subjects received the papers, Bush's approval rating fell from approximately 40 to 37 percent nationwide.[11] There was a clear difference in the way a right-leaning paper and a left-leaning paper covered the news, but both papers covered war casualties and political controversies such as the Plame investigation and the widely criticized Miers Supreme Court nomination. It may be that what the coverage had in common was more important than any differences between the newspapers.

A second explanation for why the *Times* and *Post* had similar effects may be that the Democratic candidate for governor was a conservative leaning Democrat, and thus even though the *Times* endorsement went to the Republican, the articles and

[11] These numbers are based on an average of the polls compiled by the Web site www.pollingreport.com. The average percentage of respondents approving of Bush's job performance using all polls in the field with starting dates between October 12–October 18 ($N = 4$ polls) was 40.0, starting October 19–October 25 ($N = 3$) was 41.3, and starting November 9–November 16 ($N = 3$) was 37.3.

opinions put forward were not heavily against the Democratic candidate. A third explanation is sampling error. Given the 4 percentage point standard errors for each treatment group, it is possible that there are meaningful differential effects, consistent with the news slant of the papers, which we did not detect due to inadequate power. Hence, while prior beliefs about media and bias should be updated in light of the evidence we present, it would be useful for future work to obtain more precise measurement of the differential treatment effects through use of larger samples.

Finally, there is some evidence that getting *either* newspaper produced an increase in voter turnout especially in the 2006 national elections. Our finding that turnout among the treatment groups was about 3 percentage points higher than the control group is consistent with previous work showing positive turnout effects from newspaper reading (Gentzkow 2006) and suggests newspaper exposure might have an important long term effect on the level of political interest. However, this result was nonexistent for the immediate election in 2005 and of only borderline statistical significance for 2006. Therefore, both the existence of an effect and its potential mechanism should be treated guardedly until further study.

There were also some important outcomes that were not affected by treatments. There is only limited evidence that the newspapers increased the subject's factual knowledge of politics. Since conveying facts about politics is a plausible channel for how the papers might induce change in opinions about candidates and the decision to vote, this null result stands in contrast to the measured effects on candidate preference and turnout. However, while greater political information among subjects in the treatment groups would have helped to explain the movement in candidate preference among the subject groups, changes in opinion often occur without a subject being able to recall the facts that caused opinions to shift. Results of this type are commonplace in political science. Drawing on work from psychology (Norman H. Anderson and Stephen Hubert 1963, and W. A. Watts and W. J. McGuire 1964), political scientists have constructed models of online processing of political information, where citizens update their judgments in response to the flow of information but do not retain memory of the particular facts that caused them to revise their views (Milton Lodge, Kathleen M. McGraw, and Patrick Stroh 1989; Lodge and Stroh 1993). These models receive substantial empirical support (Lodge, Marco R. Steenbergen, and Shawn Brau 1995).

Our field experiment directly addresses the problem of selection bias in standard observational studies. As in all empirical work, experimental or not, there is still the important question of generalizing from our particular findings. Any broad inferences from this study to the effects of media bias on political decisions, in general, should recognize that the results may depend on several specific features of our experiment such as the political context, choice of subjects, intensity of treatment, length of the study, timing of the study, and choice of media outlets. Of particular interest would be to investigate whether the findings we report, which suggest that the common effects of greater news exposure are of greater consequence than the news slant, generalize to periods with a more balanced news flow and are confirmed in larger studies that measure differential effects across treatment groups with greater precision. To address these issues, we suggest that this field experiment approach should be applied in different political contexts, with different subjects, for different durations, using different media.

Appendix

Table A1—Analysis of Participation in the Follow-Up Survey, Probit
(Dependent variable = 1 if survey successfully completed in follow-up phone call)

	(1)	(2)
Post treatment group	0.003	−0.048
	(0.020)	(0.046)
Times treatment group	0.018	0.052
	(0.020)	(0.050)
Female	−0.026	−0.040
	(0.017)	(0.026)
Voted in 2002	0.095	0.103
	(0.024)	(0.038)
From consumer database sample frame	0.044	0.046
	(0.021)	(0.032)
Subscribes to news magazine	0.069	0.026
	(0.029)	(0.043)
Reported preferring Democratic candidate for governor	0.126	0.126
	(0.026)	(0.040)
Wave 2 of experiment	−0.037	−0.035
	(0.017)	(0.026)
Post × female		0.094
		(0.045)
Post × voted in 2002		−0.037
		(0.054)
Post × from consumer database sample frame		0.011
		(0.050)
Post × subscribes to news magazine		0.053
		(0.071)
Post × reported preferring Democratic candidate for governor		0.032
		(0.059)
Post × wave 2 of experiment		0.043
		(0.043)
Times × female		−0.040
		(0.040)
Times × voted in 2002		0.014
		(0.057)
Times × from consumer database sample frame		−0.018
		(0.049)
Times × subscribes to news magazine		0.092
		(0.072)
Times × reported preferring Democratic candidate for governor		−0.031
		(0.056)
Times × wave 2 of experiment		−0.048
		(0.039)
N	3,347	3,347
Pseudo-R^2	0.018	0.023
Mean dependent variable	0.32	0.32
Pr(*Times* interaction variables ≅ 0)		0.23
Pr(*Post* interaction variables ≅ 0)		0.55

Notes: Standard errors in parentheses. Indicator variable included (but not reported) if gender information is missing (applicable for 134 observations). All variables (except assignment to treatment and gender) are from the baseline survey.

TABLE A2— THE EFFECT OF TREATMENT ON KNOWLEDGE AND ATTITUDES
OUTCOMES ON SPECIFIC QUESTIONS
(OLS)

Panel A: Fact Accuracy Index (Table 3, column 1)

	Knew number dead in Iraq = 1 (1)	Identified Libby as involved in leak = 1 (2)	Identified Miers as Supreme Court nominee = 1 (3)
Separate treatment effects estimated for Washington Post *and* Washington Times			
Washington Post treatment	0.024	−0.026	−0.035
	(0.034)	(0.035)	(0.033)
Washington Times treatment	0.020	0.006	0.021
	(0.033)	(0.035)	(0.033)
Adjusted R^2	0.04	0.10	0.11
F-test: *Post* = *Times*	0.01	0.70	2.43
p-value	0.92	0.40	0.12
Pooled treatment effect estimated for receiving either newspaper			
Received either *Post* or *Times* treatment	0.022	−0.009	−0.006
	(0.028)	(0.029)	(0.028)
Adjusted R^2	0.04	0.11	0.11
Observation counts for both panels			
Observations	1,077	1,067	1,074
Refused/no opinion	4	14	7
Total surveyed in follow-up	1,081	1,081	1,081

Panel B: Specific Issue Index (Table 3, column 2)

	Most important problem (1 = issue other than scandals, 0 = scandals) (4)	Most important issues in Iraq (1 = constitution or Hussein trial) (5)	Progress in Iraq (1 = very badly, 4 = very well) (6)	Leak case (3 = no one did anything wrong, 1 = something illegal) (7)	Alito confirmation (3 = should confirm, 1 = should not confirm) (8)
Separate treatment effects estimated for Washington Post *and* Washington Times					
Washington Post treatment	0.024	−0.039	−0.035	0.015	−0.059
	(0.023)	(0.041)	(0.074)	(0.066)	(0.054)
Washington Times treatment	−0.016	−0.017	−0.052	0.020	0.029
	(0.022)	(0.040)	(0.073)	(0.062)	(0.052)
Adjusted R^2	−0.01	−0.17	0.22	0.16	0.18
F-test: *Post* = *Times*	2.68	0.24	0.04	0.00	2.32
p-value	0.10	0.63	0.83	0.94	0.13
Pooled treatment effect estimated for receiving either newspaper					
Received either *Post* or *Times* treatment	0.003	−0.028	−0.044	0.017	−0.013
	(0.019)	(0.034)	(0.062)	(0.053)	(0.045)
Adjusted R^2	−0.01	0.17	0.22	0.16	0.18
Observation counts for both panels					
Observations	1,074	982	1,042	899	971
Refused/no opinion	7	99	39	182	110
Total surveyed in follow-up	1,081	1,081	1,081	1,081	1,081

TABLE A2— THE EFFECT OF TREATMENT ON KNOWLEDGE AND ATTITUDES
OUTCOMES ON SPECIFIC QUESTIONS
(OLS) (*Continued*)

Panel C: Broad Policy Index (Table 3, column 3)	Republican favorable (4 = very favorable, 1 = very unfavorable) (9)	Democrat unfavorable (4 = very unfavorable, 1 = very favorable) (10)	Bush approval rating (4 = strong approval, 1 = strong disapproval) (11)	Conservatism (7 = extreme conservative, 1 = extreme liberal) (12)
Separate treatment effects estimated for Washington Post *and* Washington Times				
Washington Post treatment	−0.078	−0.004	−0.148	−0.145
	(0.080)	(0.073)	(0.099)	(0.115)
Washington Times treatment	−0.119	0.069	−0.146	−0.014
	(0.079)	(0.072)	(0.097)	(0.114)
Adjusted R^2	0.18	0.23	0.30	0.16
F-test: *Post* = *Times*	0.24	0.87	0.00	1.11
p-value	0.63	0.35	0.98	0.29
Pooled treatment effect estimated for receiving either newspaper				
Received either *Post* or *Times* treatment	−0.099	0.033	−0.147	−0.078
	(0.066)	(0.060)	(0.082)	(0.096)
Adjusted R^2	0.18	0.23	0.30	0.16
Observation counts for both panels				
Observations	1,021	1,022	978	1,033
Refused / no opinion	60	59	103	48
Total surveyed in follow-up	1,081	1,081	1,081	1,081

Notes: Standard errors in parentheses. Dependent variables include factual questions (ability to identify the number dead in Iraq in a closed-ended question, identification of "Scooter" Libby from a list of four individuals as Dick Cheney's chief of staff who recently resigned, identification of Harriett Miers from a list of four individuals as a recent US Supreme Court nominee), political opinion quesitons (a closed-ended question about the most important problem facing the country, a closed-ended question about the most important problems in the Iraq War, attitudes about the leak case, the Alito confirmation), and attitudes about general national issues (Bush approval, favorability towards Republicans and Democrats, and Conservatism). We include covariates: gender, reported age, three separate indicators for voting in the 2001, 2002, and 2004 general elections, an indicator for whether the respondent was drawn from a consumer list, self report of receiving any news or political magazines, and baseline survey self reports of preferring the Democratic candidate in the gubernatorial election and having no preference in the gubernatorial election, and an indicator for wave of the study. If a covariate value was missing, an indicator variable was included and the covariate was coded as zero. We also include strata indicators, variables for each of the strata formed prior to the randomization, which included unique combinations of the following: intention to vote, receive a paper (non-*Post*/non-*Times*), mentions ever reading a paper, gets a magazine, and asked whether they wish they read the paper more. We also include surveyor/date indicators, which are a set of indicator variables for each unique combination of surveyor, and date for the follow-up survey. All results remain qualitatively similar, and statistical significance remains as-is, using probit or ordered probit specifications instead of OLS.

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