MANAGEMENT SCIENCE
Vol. 57, No. 9, September 2011, pp. 1640–1654
ISSN 0025-1909 | EISSN 1526-5501 | 11 | 5709 | 1640

http://dx.doi.org/10.1287/mnsc.1110.1392

Newspaper Reports and Consumer Choice: Evidence from the Do Not Call Registry

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Despite annual expenditures on public relations exceeding $19.42 billion, U.S. businesses lack practical guidance about the effectiveness of publicity in mass media. Here, we assemble a rich and novel data set to gauge the impact of news reports on consumer sign-ups with the U.S. Do Not Call (DNC) Registry. Using multiple identification strategies, we found robust evidence that news reports increased consumer registrations. Specifically, a 1% increase in the number of news reports increased DNC registrations by 0.018%. The impact increased with mention of the toll-free telephone number and URL, but decreased with the length of the headline and main text. Furthermore, we found evidence that reports affect behavior through persuasion as well as information—the impact on registration was higher for reports that mentioned the number of other people registering. Finally, the impact of news reports on consumer registration was stronger in national than local newspapers and in politically neutral and Democrat than Republican newspapers.

Key words: advertising; journalism; information; persuasion; policy; publicity
History: Received February 23, 2010; accepted May 6, 2011, by Pradeep Chintagunta and Preyas Desai, special issue editors. Published online in *Articles in Advance* July 25, 2011.

1. Introduction

The media provide the key infrastructure for governance and a well-functioning polity: "the central purpose of journalism is to provide citizens with accurate and reliable information they need to function in a free society" (Pew Research Center 2009). Industry likes to communicate through publicity in media because the media have broad reach, and publicity is, apparently, "free," whereas advertising and sales promotion are costly (Smith 2008). Moreover, consumers may be less skeptical of media publicity than advertising. U.S. industry spent considerable resources to cultivate the "free" publicity: in 2008, wages of public relations staff exceeded $19.4 billion.[1]

Despite the apparent attractiveness of media publicity and the considerable expenditures, industry lacks guidance about the effectiveness of publicity in mass media (Smith 2008). To what extent does publicity in mass media affect the behavior of consumers who are not actively seeking information? Managers can use the elasticity of demand with respect to media publicity in marketing strategy just as they apply advertising elasticities. How does the content of the publicity affect consumer behavior? Understanding the mechanisms by which publicity affects consumer behavior would help in preparing the content of publicity. How does the impact of publicity depend on characteristics of the medium? Knowing the impact of media characteristics would help in directing publicity to the target consumers.

We address these questions in a novel context—the U.S. Do Not Call (DNC) Registry. The U.S. government established the DNC Registry as a free service to help consumers opt out of telemarketing. The DNC Registry is a relatively neutral government service, so newspapers would not be likely to exaggerate or try to influence readers on the issue. Furthermore, the DNC Registry was widely reported in newspapers but not advertised—which allows us to analyze the impact of newspaper publicity without worrying about any confound from advertising. Hence, the

[1] This is the authors' calculation based on data from the U.S. Bureau of Labor Statistics. U.S. advertising expenditure in 2008 was $141.7 billion (TNS Media Intelligence 2009). Comparing the impact of publicity versus advertising on the demand for commercial items would be challenging because most businesses employ both advertising and publicity.

DNC Registry provides a neat setting to study the impact of news reports on consumer choice.

We assembled a rich and novel data set from multiple sources. Our data set included 2,004 reports of the DNC Registry in 136 newspapers that were circulated in over 2,300 counties. We matched the DNC registrations and news reports with an array of newspaper and county characteristics to compile a panel data set by county-week. We then applied panel-data estimation techniques and multiple identification strategies.

The key challenge in identifying the impact of news reports on DNC registration is that news reports could be endogenous—newspapers may increase coverage of the DNC Registry because many people are signing up, or because they cater to readers who are relatively more sensitive to privacy. Besides including relevant demographic controls, we applied four different identification strategies. The first focused on news reports published before the opening of the DNC Registry. Clearly, these reports could not have been driven by consumer registrations. The next strategy considered the extent to which telephone numbers from a state-level do not call registry were added to the federal registry. If news reports affected DNC registration, the response of consumers to news reports would be lower in states that added more telephone numbers to the federal registry. The third strategy compared the impact of news reports in local versus national papers. National titles do not tailor their content to local preferences. The fourth strategy applied instrumental variables (IVs) using newsprint consumption, staff size, and page count as instruments for news reports.

We found robust evidence that newspaper reports did increase DNC registration. The estimate of our preferred specification implied that a 1% increase in news reports increased DNC registrations by 0.018%. This provides a robust indication of the "sales-response function" to newspaper publicity.

To investigate the impact of the content of publicity, we compiled various characteristics of the news reports. The impact of news reports on DNC registration increased with mention of the toll-free telephone number and URL, which suggests that reports affected consumer behavior through information. Furthermore, the impact of news reports also increased with mention of the number of people registering, which suggests that reports also affected consumer behavior through persuasion.

Our third issue was how the impact of publicity depended on characteristics of the medium. Reports in national newspapers had almost four times the impact of reports in local newspapers (after controlling for differences in circulation). Furthermore, motivated by previous studies of slant in newspapers (Gentzkow and Shapiro 2008), we compared the impacts on DNC registration of reports in Democratic and Republican versus neutral newspapers. Politically neutral newspapers had the largest impact, followed by Democratic newspapers, whereas Republican newspapers had no significant effect.

Our main contributions are as follows. We provide robust evidence of the impact of publicity on consumer behavior with a precise estimate of the "sales response" in a particular context. We identify the impact of publicity as being through information as well as persuasion. Finally, we show that the geographical scope and politics of the medium itself also matter. These findings have immediate and specific implications for managerial practice as well as public policy.

2. Related literature

One of the celebrated four Ps of marketing strategy is promotion, which comprises advertising, publicity, and sales promotion. Previous research into the impact of publicity on consumer behavior has considered publicity through various channels, including online word of mouth (Godes and Mayzlin 2004, 2009; Boatwright et al. 2006; Chevalier and Mayzlin 2006; Liu 2006; Chintagunta et al. 2010), specialized publications (Simonsohn 2011), newspapers (Ahluwalia et al. 2000, Gentzkow and Shapiro 2006, Xiang and Sarvary 2007, Alsem et al. 2008, Gerber et al. 2009, Schulhofer-Wohl and Garrido 2009), radio and television (Strömberg 2004, Gentzkow 2006, DellaVigna and Kaplan 2007), and multiple media (Kalaitzandonakes et al. 2004, Brown and Minty 2008).

Our study departs from previous research on publicity in three significant ways. First, most previous studies focused on the medium itself (e.g., online newsgroups, newspaper, radio) and were silent on the impact of publicity of a particular product or event. Here, we study newspaper coverage of a specific service—the DNC Registry—*exclusively*, and so we could distinguish the impact of news reports from the impact of the medium.

Second, with 2,004 reports in 136 newspapers, we could compare the impact of news reports in media with different characteristics—national versus local papers and papers with different politics—and we could provide a numerical estimate of the "sales response" of newspaper publicity.

Third, in most previous studies, the content—movie and television reviews, product comparisons, elections, and the economy—was inherently evaluative and would serve to *persuade* consumers. Here, we could distinguish informative from persuasive content, and so distinguish the impact of descriptive versus evaluative publicity on consumer behavior. Furthermore, in most previous studies, the consumer actively sought information. In contrast, our setting is one where the consumer receives the information in a passive way. Hence, our findings would inform managerial practice where information is pushed through the mass media, rather than pulled through by consumers.

In the research closest to ours, Brown and Minty (2008) studied the impact of media coverage of the 2004 Boxing Day Tsunami in the ABC, CBS, and NBC evening news, the *New York Times*, and the *Wall Street Journal* on online donations to eight U.S. charities. Each additional newspaper report was associated with a 17%–21% increase in donations. However, Brown and Minty (2008) could not rule out this large estimate being due to reverse causation or confounded by information from other sources.

3. U.S. Do Not Call Registry

The U.S. Federal Trade Commission (FTC) opened the Do Not Call Registry on June 27, 2003.[2] Registrations prior to September 1, 2003, were effective from October 1, 2003, whereas all subsequent registrations were effective only after a 90-day waiting period.

The telemarketing industry opposed the DNC Registry in U.S. courts. On September 23, 2003, U.S. District Court Judge Lee R. West of Oklahoma enjoined the DNC Registry on grounds that the FTC did not have the relevant authority. Congress quickly passed a bill to provide the FTC with the authority, and President Bush signed the legislation into law on September 29, 2003. However, on October 1, 2003, U.S. District Court Judge Edward W. Nottingham of Colorado enjoined the registry on grounds that it violated the constitutional right to free speech. The FTC suspended the DNC Registry until October 7, 2003, when the U.S. Court of Appeals for the 10th Circuit suspended the District Court order, allowing the registry to take effect. Subsequently, on February 17, 2004, the U.S. Court of Appeals overruled the District Court and held that the DNC Registry was constitutional. Finally, on October 4, 2004, the U.S. Supreme Court ended the telemarketers' legal action by declining to hear their appeal.

Figure 1 plots the number of newspaper reports and average DNC registration rates by day. Panel (a) shows that the peaks in registration often coincided with increases in newspaper reports. Panel (b) provides a snapshot around the opening of the DNC Registry on June 27, 2003. Panel (c) shows the next peak of newspaper reports around September 23, 2003, when Judge West enjoined the DNC Registry. Finally, panel (d) shows the situation around October 1, 2003, when Judge Nottingham enjoined the DNC Registry. It is evident from panels (c) and (d) that newspaper reports were driven by exogenous events and that registrations followed rather than preceded newspaper reports.

4. Data

The FTC provided us with DNC registration data from the beginning of the registry in June 27, 2003. We used the registrations up to the end of June 2004 (one year after the registry opened) because Americans tend to move during the summer (Hansen 1998), and so some DNC entries after June 2004 could be "reregistrations."[3]

The FTC records showed registrations by redacted telephone number for each area code and exchange, e.g., (617) 363–xxxx, by date of registration. We matched the DNC registrations to the respective counties, and so identified the registrants' geographical locations.[4] We then merged the registrations with county-level demographic data from the 2000 U.S. Census.

Using the proprietary news database Factiva, we searched for newspaper reports including the words "do not call" between June 1, 2003, and June 30, 2004.[5] Our search yielded 2,004 reports in 136 newspapers, which included republications in local newspapers of reports from wire services such as the Associated Press. For each report, we recorded the name of the publication, the date, and various characteristics—in particular, whether the report mentioned number of people registering, e.g., "The FTC estimates some 60 million phone numbers will be registered out of 166 million residential phone numbers in America" (Venezia 2003); whether the report included a toll-free number or URL; and the lengths of the

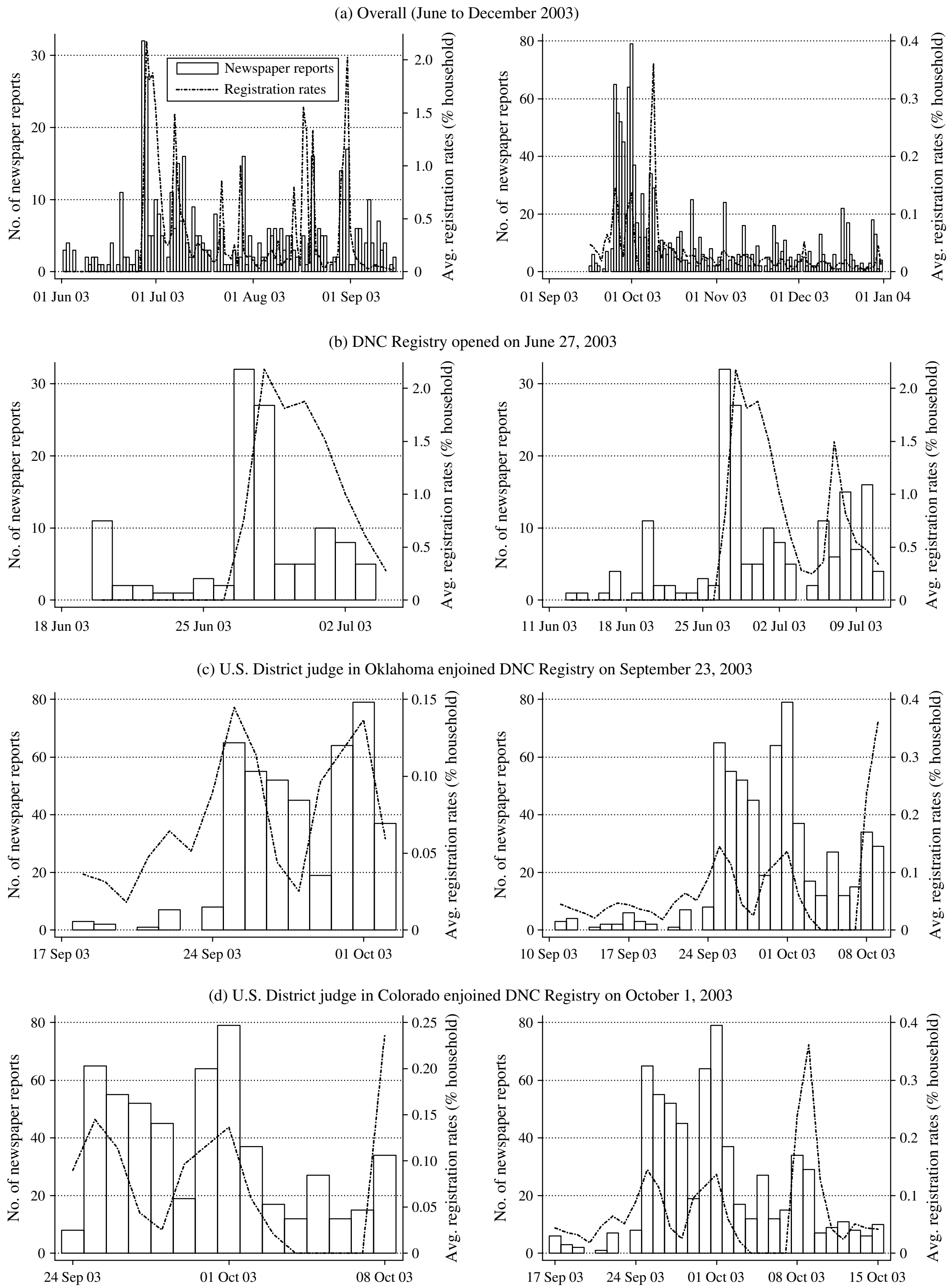
[2] The following review of legal actions against the DNC Registry is based on data from the U.S. Federal Trade Commission (2003b).

[3] In addition, newspaper reports of the DNC Registry tailed off significantly after February 2004.

[4] We identified the counties served by each telephone exchange using the North American Local Exchange NPA-NXX Database. We matched the DNC registrations to counties because we could not identify the individual households who made the registrations. So, we could only use their geographical location—county—to identify their demographic characteristics. For telephone exchanges spanning multiple counties, we allocated the registrations to the respective counties based on the relative number of households as reported by the 2000 U.S. Census. We excluded mobile phone registrations because U.S. mobile phone numbers are not associated with any geographical location.

[5] Factiva (http://factiva.com/) is a comprehensive newspaper database provided by Dow Jones. We compiled the newspaper reports from June 1, 2003, about one month before the opening of the DNC Registry on June 27, 2003. By doing so, we could capture the extensive media attention *preceding* the launch of the DNC Registry. Both registration data and newspaper reports were specified to end in June 2004.

Figure 1 Newspaper Reports and DNC Registration



Note. In panel (a), left and right figures depict June–September and September–December, 2003. In panels (b)–(d), left and right figures depict daily registrations in windows of ±7 and ±14 days with different scales.

headline and main text. For validation and falsification analyses, we also collected data sets of newspaper reports including the words "anti-spam," "junk mail," "identify theft," or "phishing," but excluding the words "do not call" over the same period.

The impact of a newspaper report in a particular county would depend on the circulation of the newspaper in that county. From the Audit Bureau of Circulation (ABC), we obtained the circulation of each newspaper by county.[6] We then calculated the intensity of newspaper reports of the DNC Registry in county i in week t as

$$I_{it} = \sum_j (R_{jt} \times c_{ij}), \qquad (1)$$

where R_{jt} is the number of reports of the DNC Registry in newspaper j in week t, and c_{ij} is the circulation of newspaper j in county i. Effectively, I_{it} measures the total *circulation* of news reports of the DNC Registry in county i in week t.[7]

We further compiled other newspaper characteristics to supplement the newspaper report data. From the *Editor & Publisher International Year Book 2004* (Editor & Publisher 2004), we obtained the annual newsprint consumption and average number of pages per issue. From *Bacon's Newspaper Directory 2005* (Bacon's Information 2004), we procured the total number of employees at each newspaper. From websites that document newspaper politics, we compiled newspaper endorsements, if any, of candidates in the 2004 U.S. presidential election.[8] Finally, from the *Atlas of U.S. Presidential Elections*, we obtained the percentage of support in each county for the Republican candidate (George W. Bush), the Democratic candidate (John Kerry), and third-party candidates in the 2004 U.S. presidential election, and the number of registered voters.[9]

Prior to the opening of the federal DNC Registry in June 2003, 27 states had already established state-level "do not call" registries (Varian et al. 2004). Subsequently, some states added the telephone numbers on their state registry to the federal registry (U.S. Federal Trade Commission 2005). We compiled the percentage of state-registered numbers added to the federal registry from the U.S. Federal Trade Commission (2003a) and used the information in one set of estimates to infer the impact of news reports on DNC registration. Tables 1 and 2 present summary statistics and correlations of the data. Our data set, including characteristics of the newspapers and news reports, consumer demographics, and 2004 presidential election information, was specified at the county-week level.[10]

5. Model

The basic model for analysis is a county-level error-components model of the form

$$\ln Q_{it} = \alpha + \sum_k \beta_k \ln x_{ik} + \lambda \ln I_{it} + \delta_i + \tau_t + \varepsilon_{it}, \qquad (2)$$

where Q_{it} is the number of DNC registrations in county i in week t; I_{it}, as defined in (1) above, is the number of DNC newspaper reports weighted by circulation in county i in week t; x_{ik} is a set of k demographic characteristics in county i; δ_i represents county-specific effects; τ_t represents time-specific effects; and ε_{it} captures any residual random errors. We specified all continuous variables in logarithms.[11]

Our data set contained more than 3,000 cross-sectional units (county) and 53 time periods (weeks). To check the robustness of our results, we estimated multiple models, with alternative assumptions on the county-specific effects, the error structure, and their correlations with the news reports variable, and with different samples and specifications.

Telemarketing and consumer privacy are regulated by federal and state but not county governments. Accordingly, we included a set of state-specific

[6] In the online appendix (available at http://www.comp.nus.edu.sg/~ipng/research/newsppr_appx.pdf), we explain the computation of the weekly circulation of a newspaper in a county.

[7] The measure I_{it} is similar to the concept of "gross rating points" commonly used in marketing.

[8] See, for example, http://www.dkosopedia.com/wiki/2004_Media_Endorsements and http://www.gwu.edu/~action/2004/cands/natendorse5.html (accessed January 25, 2010). We also referred to the *Editor & Publisher International Year Book 2004* for each newspaper's self-declared political affiliation. However, this information was not useful: 93% of newspapers declared themselves to be "independent."

[9] See http://www.uselectionatlas.org/.

[10] We selected the county as the unit of analysis for two reasons. First, we could associate each registration with a county but not individual newspaper or news reports. Second, a registration may be influenced by multiple news reports (e.g., people read one newspaper at home and another at the workplace; Gentzkow and Shapiro 2008), and so measurement error would arise if we associated each DNC registration to a single news report. We grouped the observations by week because the daily registrations are subject to more random variation, resulting in estimates that are less reliable. In robustness checks (reported in the online appendix), we replicated most of the estimates at the daily level and found similar results.

[11] Empirical analyses often fit better with economic variables specified in logarithm (Wooldridge 2006, pp. 197–200). As appropriate, we added one to the variable to avoid logarithms of zeroes. With the double-log specification, all estimated coefficients can be directly interpreted as elasticities. Furthermore, we included the number of households as an explanatory variable, and so the dependent variable in (2) is equivalent to the county-level registration *rate* in week t. As reported in Table 5 below, our finding on the impact of news reports is robust to excluding the number of households as a covariate, i.e., when the dependent variable is the absolute number of registrations.

Table 1 Summary Statistics

Variables	N	No. of counties	Mean	Std. dev.	Min	Max
DNC registrations	165,625	3,125	311.609	3,590.918	0	397,545.800
No. of newspaper reports on "do not call"	165,625	3,125	4.042	16.102	0	1,162.800
With toll-free number	165,625	3,125	0.658	4.510	0	196.800
With URL	165,625	3,125	0.757	4.830	0	234.900
Mentioning no. of registrations	165,625	3,125	3.332	14.146	0	1,106.000
From national papers	165,625	3,125	0.879	3.397	0	400.300
From local papers	165,625	3,125	3.163	15.048	0	1,162.800
Endorsing Republican candidate	165,625	3,125	1.202	7.998	0	509.800
Endorsing Democratic candidate	165,625	3,125	1.809	11.555	0	653.000
Endorsing neither candidate	165,625	3,125	0.937	4.088	0	259.500
No. of characters in headline (in thousands)	165,625	3,125	0.197	0.801	0	57.346
No. of characters in main text (in thousands)	165,625	3,125	14.591	66.830	0	4,904.491
Frequency of DNC mention	165,625	3,125	0.018	0.089	0	7.057
No. of focused reports on "do not call"	165,625	3,125	1.614	9.935	0	718.800
No. of peripheral reports on"do not call"	165,625	3,125	2.428	9.722	0	444.000
No. of news reports on "anti-spam"	165,625	3,125	1.507	6.332	0	293.400
No. of news reports on "junk mail"	165,625	3,125	0.917	4.757	0	156.600
No. of news reports on "identify theft"	165,625	3,125	1.983	8.016	0	245.600
No. of news reports on "phishing"	165,625	3,125	0.430	3.444	0	170.400
Proportion of population:						
Voting for Republican candidate	164,141	3,097	0.266	0.072	0.038	0.597
Voting for Democratic candidate	164,141	3,097	0.171	0.064	0.027	0.464
Voting for other candidates	164,141	3,097	0.005	0.003	0	0.068
Registered as voter but did not vote	157,516	2,972	0.193	0.073	0	0.678
Who did not register	157,516	2,972	0.368	0.101	0	0.784
No. of households (in thousands)	165,625	3,125	33.699	104.437	0.185	3,133.774
Average household size	165,625	3,125	2.630	0.239	2.073	5.127
Median household income ($K)	165,625	3,125	35.354	8.864	12.692	82.929
Average commuting time (mins)	165,625	3,125	23.425	5.652	6.275	48.667
Unemployment rate (%)	165,625	3,125	3.438	1.522	0	32.863
Retail density (stores per sq. mile)	163,134	3,078	0.398	1.610	0	61.438

Notes. All newspaper report variables were weighted by the corresponding circulation in the county. All variables were computed at the county-week level. Number of observations, $N =$ number of counties $\times$ 53 weeks.

dummy variables in the county characteristics, x_{ik}, to control for state-specific regulations/effects. We clustered the idiosyncratic errors, ε_{it}, by county to control for intertemporal correlations in DNC registrations within counties. Together with the county effects, δ_i, our analysis focused on explaining differences in registrations from countywide averages.[12]

6. Results

We first estimated a random effects (RE) model of DNC registration on various demographic factors—number of households, household size and income, commuting time, unemployment rate, and retail density—that have been previously identified as affecting DNC registrations (Varian et al. 2004). Essentially, we assumed that $\delta_i \sim \mathrm{N}(0, \sigma_\delta^2)$.[13] As reported in Table 3, column (1), the results were generally consistent with a priori expectations.[14]

We next estimated (2), including the number of news reports weighted by circulation, I_{it}. Table 3, column (2), reports this baseline estimate. The coefficient of the weighted number of news reports, 0.018 ($p < 0.01$), was positive and very precisely estimated. With the double-log specification, the coefficient represents the estimated elasticity. Accordingly, a 1% increase in news reports of the DNC Registry,

[12] The Direct Marketing Association also provides a service to opt out of direct marketing, for a $5 fee. This would be reflected in the error term.

[13] The key advantage of this specification is that we could estimate the coefficients of the county demographic variables, x_{ik}, despite their invariance over time. We could then examine whether the signs of these coefficients matched with our a priori expectations.

[14] Registrations and the number of households should be positively correlated. The likelihood that any one member of a household receives a telemarketing call should decrease in household size, and so larger households would have less demand for DNC. High-income and employed people may incur a higher (time) cost in receiving telemarketing calls, and so income and unemployment would be positively and negatively correlated, respectively, with DNC registrations. People who spend more time commuting to work would have relatively less time at home and so are less bothered by telemarketing calls. Finally, retail density reflects the demand for shopping, so in counties with more shops, the demand for DNC may be lower because consumers are generally more receptive of marketing.

Table 2 Correlations

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1 *DNC registrations*	1																		
2 *No. of newspaper reports on "do not call"*	0.06	1																	
3 *With toll-free number*	0.12	0.48	1																
4 *With URL*	0.12	0.52	0.91	1															
5 *Mentioning no. of registrations*	0.07	0.96	0.51	0.55	1														
6 *From national papers*	0.07	0.40	0.17	0.21	0.41	1													
7 *From local papers*	0.05	0.98	0.47	0.51	0.94	0.21	1												
8 *Endorsing Republican candidate*	0.04	0.60	0.36	0.41	0.55	0.14	0.61	1											
9 *Endorsing Democratic candidate*	0.04	0.80	0.28	0.31	0.79	0.27	0.80	0.11	1										
10 *Endorsing neither candidate*	0.05	0.39	0.27	0.28	0.39	0.54	0.30	0.07	0.14	1									
11 *No. of characters in headline* (in thousands)	0.05	0.95	0.42	0.46	0.92	0.35	0.93	0.60	0.75	0.36	1								
12 *No. of characters in main text* (in thousands)	0.05	0.89	0.35	0.40	0.87	0.32	0.88	0.47	0.78	0.31	0.87	1							
13 *Frequency of DNC mention*	0.06	0.90	0.51	0.55	0.87	0.43	0.86	0.57	0.68	0.41	0.84	0.73	1						
14 *No. of focused reports on "do not call"*	0.07	0.82	0.55	0.61	0.84	0.42	0.79	0.51	0.63	0.37	0.78	0.67	0.90	1					
15 *No. of peripheral reports on "do not call"*	0.04	0.82	0.23	0.25	0.74	0.24	0.82	0.47	0.69	0.27	0.78	0.79	0.58	0.35	1				
16 *No. of news reports on "anti-spam"*	0.03	0.32	0.11	0.12	0.30	0.11	0.32	0.15	0.31	0.09	0.33	0.33	0.26	0.22	0.31	1			
17 *No. of news reports on "junk mail"*	0.03	0.19	0.12	0.11	0.18	0.09	0.18	0.09	0.17	0.09	0.17	0.16	0.16	0.14	0.17	0.16	1		
18 *No. of news reports on "identify theft"*	0.04	0.22	0.13	0.17	0.20	0.07	0.22	0.13	0.19	0.08	0.21	0.18	0.18	0.14	0.23	0.17	0.13	1	
19 *No. of news reports on "phishing"*	0.02	0.13	0.02	0.04	0.13	0.01	0.13	0.03	0.15	0.02	0.13	0.13	0.08	0.06	0.14	0.15	0.05	0.22	1

Notes. Only correlations between DNC registration and news reports characteristics are reported. The correlations of these variables with other variables (presidential voting, county demographics, etc.) were generally small (below 0.30 in absolute value).

weighted by circulation in the county, was associated with a 0.018% increase in DNC registrations.

One concern with the RE model is that the estimates would be inconsistent if the county random effects were correlated with the regressors. Our next two estimates addressed this issue. Table 3, column (3), reports fixed effects (FE) estimates of (2), which are consistent even if the county effects, δ_i, are correlated with the regressors. With county fixed effects, the number of households and other non-time-varying county demographics could not be identified. Table 3, column (4), reports the Hausman and Taylor (1981) model, which allows correlations between δ_i and the regressors while still identifying the county demographics. In both estimates, the coefficient of news reports was 0.017 ($p < 0.01$), which was very close to the RE estimate of 0.018. Furthermore, the Hausman test suggested that the RE estimates were not inconsistent ($\chi^2 = 45.42$, $p = 0.76$). Accordingly, we preferred the RE model, and in the remaining estimates we used random effects.[15]

An obvious concern with the above analysis is that the number of news reports could be endogenous—relevant variables could be omitted, news reports could have been driven by consumer DNC registrations (reverse causation), or newspapers might have tailored coverage of the DNC Registry according to consumer preferences for privacy. Other than including comprehensive demographic controls, we applied four distinct identification strategies.

6.1. Day 1

First, we considered the impact of newspaper reports published between June 1 and June 26, 2003, on DNC

[15] We further considered two alternative models. The first was a general linear panel-data model estimated by feasible generalized least squares regression, which allowed for heteroscedasticity and autoregressive errors. As reported in the online appendix, the coefficient of news reports was 0.022 ($p < 0.01$), which was statistically significant and close to the baseline estimate. The second was a mixed model with a *random coefficient* for the number of news reports, random county effects, and fixed coefficients for all other variables. Referring to (2), instead of a fixed λ for all counties, we specified $\lambda_i = \lambda + \varsigma_i$, $\varsigma_i \sim \mathrm{N}(0, \sigma_\lambda^2)$. As reported in the online appendix, the mean effect of news reports was $\lambda = 0.019$ ($p < 0.01$), which was similar to the RE and FE estimates, and $\sigma_\lambda = 0.029$, which was statistically significant ($p < 0.01$). Hence, the impact of news reports on DNC registrations varied across counties.

Table 3 Newspaper Reports

Variables	(1) RE: Demographics	(2) RE: Reports	(3) FE: Reports	(4) RE: Hausman–Taylor	(5) OLS: Day 1	(6) RE: State registry	(7) RE: Local/national	(8) 2SLS RE: Newsprint	(9) 2SLS RE: Editorial staff	(10) 2SLS RE: Pages
No. of households	0.980***	0.978***		0.978***	1.104***	0.988***	0.975***	0.976***	0.954***	0.997***
	(0.008)	(0.008)		(0.007)	(0.011)	(0.010)	(0.008)	(0.002)	(0.003)	(0.004)
Household size	−1.088***	−1.086***		−1.086***	−1.254***	−0.944***	−1.082***	−1.078***	−1.040***	−1.036***
	(0.116)	(0.115)		(0.096)	(0.155)	(0.158)	(0.115)	(0.026)	(0.028)	(0.033)
Household income ($K)	0.373***	0.363***		0.364***	0.464***	0.305***	0.357***	0.268***	0.159***	0.242***
	(0.060)	(0.059)		(0.048)	(0.082)	(0.076)	(0.059)	(0.014)	(0.018)	(0.026)
Unemployment (%)	−0.157***	−0.158***		−0.158***	−0.009	−0.213***	−0.159***	−0.163***	−0.174***	−0.142***
	(0.032)	(0.032)		(0.032)	(0.048)	(0.038)	(0.032)	(0.009)	(0.009)	(0.012)
Commute time	−0.078**	−0.077**		−0.077**	0.044	−0.112**	−0.065*	−0.067***	−0.057***	−0.021
	(0.039)	(0.039)		(0.036)	(0.054)	(0.049)	(0.038)	(0.010)	(0.011)	(0.013)
Retail density	−0.063*	−0.067**		−0.067***	−0.006	−0.086**	−0.069**	−0.111***	−0.148***	−0.102***
	(0.033)	(0.033)		(0.024)	(0.038)	(0.038)	(0.033)	(0.007)	(0.008)	(0.009)
Reports		0.018***	0.017***	0.017***		0.039***		0.170***	0.343***	0.177***
		(0.002)	(0.002)	(0.002)		(0.003)		(0.008)	(0.017)	(0.030)
Reports prior to June 27					0.071***					
					(0.014)					
State addition						−0.352**				
						(0.149)				
Reports × *state addition*						−0.118***				
						(0.009)				
Reports in national papers							0.043***			
							(0.004)			
Reports in local papers							0.011***			
							(0.002)			
Constant	−2.251***	−2.211***	6.572***	−2.215***	−7.987***	−2.067***	−2.224***	−2.011***	−1.633***	−2.273***
	(0.239)	(0.238)	(0.008)	(0.189)	(0.325)	(0.306)	(0.238)	(0.056)	(0.068)	(0.087)
Observations	163,134	163,134	163,134	163,134	3,078	108,332	163,134	154,972	154,972	124,338
Counties	3,078	3,078	3,078	3,078		2,044	3,078	2,924	2,924	2,346
R^2			0.784		0.910					

Notes. The dependent variable is Log *registration*. All specifications included *number of households*, *household size*, *household income* ($K), *unemployment* (%), *commute time*, and *retail density* as controls, and state and week fixed effects. Column (1): Background demographics. Column (2): Baseline estimate of news reports with county random effects. Column (3): Estimate of news reports with county fixed effects. Column (4): Estimate by the Hausman–Taylor specification. Column (5): Ordinary least squares estimate of Day 1 registrations. Column (6): Accounting for addition of state registries to federal registry. Column (7): Distinguishing between reports in national versus local newspapers. Column (8): Two-stage least squares estimate with county random effects and newsprint consumption as instrument. Column (9): Two-stage least squares estimate with county random effects and editorial staff size as instrument. Column (10): Two-stage least squares estimate with county random effects and page count as instrument. Robust standard errors clustered by county in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

registrations on the opening day, June 27, 2003. Obviously, news reports published before June 27 could not have been influenced by the registrations on June 27. As reported in Table 3, column (5), the coefficient of pre–June 27 news reports, 0.071, was positive and statistically significant. More importantly, the elasticity was substantially larger than 0.018, as estimated over the entire period of study. This difference is expected because the prelaunch newspaper reports would have built up a pent-up demand for the DNC Registry when it opened. Furthermore, the people who registered early would be those with the strongest preference for the DNC Registry, leaving those with weaker preference (and less sensitive to news reports) to register later.

Although the Day 1 model effectively addressed reverse causation, it is still subject to selection bias—newspapers in counties where consumers are more sensitive to privacy could have provided more coverage of the DNC Registry.[16] The next three identification strategies addressed this issue.

[16] As reported in the online appendix, we conducted a two-step cross-sectional regression to examine the possibility of selection bias. In the first step, we regressed the number of DNC registrations on June 27, 2003 (Day 1), on county demographic and state dummy variables and calculated the *predicted number of DNC registrations*. Next, we regressed the number of news reports between June 1 and June 26, weighted by circulation, on the *predicted number of DNC registrations* (obtained from Step 1), state dummy variables, and newspaper characteristics. The coefficient of the *predicted number of DNC registrations on Day 1* was 0.278 ($p < 0.01$), indicating that newspapers might have increased coverage of the DNC Registry prior to the opening of the registry because they expected their readers to be interested in such reports.

6.2. State DNC Registries

Our second strategy was to apply heterogeneous treatments—exploiting differences between counties according to the extent to which telephone numbers on the state-level DNC Registry, if any, were added to the federal registry. Referring to (2), suppose that

$$\lambda = \lambda_0 + \lambda_1 s_i, \tag{3}$$

where s_i is the percentage of registrations added from the state registry. Substituting in (2),

$$\ln Q_{it} = \alpha + \sum_k \beta_k \ln x_{ik} + \lambda_0 \ln I_{it} + \lambda_1 s_i \ln I_{it} + \delta_i + \tau_t + \varepsilon_{it}. \tag{4}$$

The impact of news reports should be *smaller* to the extent that the state added state-registered telephone numbers to the federal registry. Hence, our hypothesis was that $\lambda_1 < 0$. If, however, news reports had no impact on DNC registration or if the impact was actually due to newspapers catering to differences in privacy concern, then we would expect $\lambda_1 = 0$.

Table 3, column (6), reports the estimate of (4). The coefficient of news reports, 0.039, was positive and significant, and more than double the baseline estimate (column (2)). The coefficient of the percentage of registration added from the state registry, s_i, was negative and significant. More importantly, the coefficient of the interaction between the number of news reports and the percentage of numbers added by the state, −0.118, was *negative* and significant. This was consistent with the hypothesis that $\lambda_1 < 0$; that is, to the extent that state-registered numbers were added to the federal registry, consumers were less responsive to news reports of the federal DNC Registry.

6.3. National vs. Local Newspapers

Our third identification strategy distinguished between national newspapers (*New York Times* and *USA Today*) and other newspapers. National newspapers are limited in their ability to tailor their content to the county level. As reported in Table 3, column (7), the impact of reports in national newspapers, 0.043, was substantially *larger* than that of reports in local newspapers, 0.011. This difference is not consistent with the impact of news reports being explained by local newspapers tailoring their coverage to local preferences over privacy. It is important to stress that our measures of news reports were weighted by circulation, and so the results imply that, controlling for circulation, reports in national newspapers had stronger impact on DNC registration than reports in local papers.

6.4. Instrumental Variables

Our fourth identification strategy made use of IVs. Suitable instruments would be factors that shifted newspapers' reports of the DNC Registry but not DNC registration *itself*. We selected three instruments—annual newsprint consumption, editorial staff size, and number of pages per issue. A newspaper that used more newsprint, with a larger editorial staff, and with more pages would carry more content, including reports of DNC registration. However, it is unlikely that the federal DNC Registry, which is not a major, sustained issue, would materially affect these newspaper attributes. Moreover, newsprint consumption, editorial staff size, and pages were unlikely to correlate with any tendency of newspapers to tailor coverage of the DNC Registry to readers' interest.

We performed IV estimation using generalized (for county random effects) two-stage least squares regression. Columns (8)–(10) of Table 3 report estimates with news reports instrumented by newsprint consumption, staff size, and page count, respectively. The sample size was smaller owing to limited data on the instruments. The coefficient of news reports ranged from 0.170 to 0.343, and was an order of magnitude larger than the baseline estimate.

Upon considering the various estimates, we preferred the basic model, (2), estimated by random effects over the whole period, as reported in Table 3, column (2). This specification was parsimonious and provided a conservative estimate of the elasticity of DNC registration with respect to news reports, 0.018, and was buttressed by estimates obtained through four different identification strategies.

6.5. Robustness Checks

We checked the robustness of our findings in multiple ways. For convenient reference, Table 4, column (1), presents the baseline estimate from Table 3, column (2). For brevity, from this point onward, we do not report the demographic variables or constant.

First, we considered the impact of lagged reports. The impact of news reports should diminish over time, so reports published in the week and two weeks before should have smaller impact than the reports published in the same week. Indeed, as reported in Table 4, columns (2) and (3), the impact did decline over time. The coefficient of the lagged reports was less than half the baseline estimate, and the coefficient of the twice lagged reports was less than a quarter the baseline estimate.

Next, we limited the sample to counties with newspaper reports of the DNC Registry, therefore excluding counties in which there was no circulation of DNC news. This would reduce unobserved heterogeneity across counties. Given that all of the sample counties in this estimate were exposed to some news reports of the DNC Registry, they must be relatively more similar than counties with no reports at all. As reported in Table 4, column (4), the estimated coefficient, 0.007,

Table 4 Robustness

Variables	(1) Baseline	(2) Lag	(3) Lag 2	(4) Excluding counties with no reports	(5) Absolute registration	(6) Linear specification	(7) Daily registration	(8) Validation: Anti-spam	(9) Falsification: Junk mail	(10) Falsification: Identity theft	(11) Falsification: Phishing
Reports	0.018*** (0.002)			0.007*** (0.002)	0.018*** (0.002)	5.913*** (1.736)	0.033*** (0.002)				
Reports lagged		0.008*** (0.002)									
Reports lagged twice			0.004* (0.002)								
Reports of anti-spam								0.006** (0.002)			
Reports of junk mail									−0.004* (0.002)		
Reports of identity theft										0.004* (0.002)	
Reports of phishing											0.006 (0.005)
Observations	163,134	160,056	156,978	123,649	163,134	163,134	295,488	163,134	163,134	163,134	163,134
Counties	3,078	3,078	3,078	2,333	3,078	3,078	3,078	3,078	3,078	3,078	3,078

Notes. The dependent variable is Log *registration* (except for column (6)). All specifications included *number of households* (except column (5)), *household size*, *household income* ($K), *unemployment* (%), *commute time*, and *retail density* as controls (not reported), and state and week fixed effects. All estimates with county random effects. Column (1): Baseline estimate. Column (2): Reports lagged by one period. Column (3): Reports lagged by two periods. Column (4): Excluding counties with no news reports on DNC. Column (5): Excluding *number of households* in the estimation, the dependent variable is thus effectively log *registration*, not log *registration rate*. Column (6): Dependent variable is absolute (not log) registration and explanatory variables are absolute (not log). Column (7): Analysis of daily (not weekly) registration. Column (8): Validation exercise regressing DNC registration on news reports of "anti-spam." Column (9): Falsification exercise regressing DNC registration on news reports of "junk mail." Column (10): Falsification exercise regressing DNC registration on news reports of "identity theft." Column (11): Falsification exercise regressing DNC registration on news reports of "phishing." Robust standard errors clustered by county in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

was positive and about half the baseline estimate, and significant.

Then, we considered whether the results were sensitive to the specification, (2). One possible concern was that the dependent variable was the *rate* of DNC registration rather than the absolute number of DNC registrations. To check, we excluded the number of households from the estimation, which effectively rendered the dependent variable as the absolute number of DNC registrations. As reported in Table 4, column (5), the estimated coefficient, 0.018, was identical to the baseline estimate. As reported in the online appendix, the results for other specifications were little affected by the exclusion of the number of households from the estimates.

Another possible concern was the double-log specification. To check, we estimated a linear specification. As reported in Table 4, column (6), the estimated coefficient was 5.913, and it was statistically significant ($p < 0.01$). Based on the mean registration and news reports, the implied elasticity was $5.913 \times 4.042/311.609 = 0.077$. This is considerably larger than the elasticity from the baseline double-log specification.

Yet another possible concern was that the analysis was framed by week. To check, we estimated a daily specification. As reported in Table 4, column (7), the estimated coefficient, 0.033, was positive and significant. As reported in the online appendix, the results for most other specifications are robust to framing by day instead of week.

Finally, we conducted a set of validation and falsification exercises, using news reports on other issues of privacy and information security. Table 4, column (8), reports a validation exercise on reports of "anti-spam." Public concern about spam led the U.S. Federal Trade Commission to propose and Congress to pass the CAN-SPAM Act in December 2003. News reports of spam might well bring to mind unwanted solicitations by telephone as well as e-mail, and therefore prompt people to register with the DNC Registry. Indeed, the coefficient of anti-spam reports, 0.006, was positive, but only one third of the coefficient of DNC reports. This result is consistent with news reports of spam increasing the salience of concerns over telemarketing but to a much smaller extent than news reports of the DNC Registry.

Columns (9)–(11) of Table 4 report falsification exercises on reports of "junk mail," "identity theft," and "phishing," respectively. These concerns are further removed from DNC registration, and no specific legislation was enacted in 2003–2004 against these menaces. Indeed, the estimated impact of the respective news reports on DNC registration were negative (junk mail) or insignificant (identity theft and phishing).

Table 5 News Content

Variables	(1) Baseline	(2) DNC frequency	(3) Report focus	(4) Informative content	(5) Persuasive content	(6) Text length	(7) Content measures
Reports	0.018***	0.013***		−0.001	0.002	0.077***	0.047***
	(0.002)	(0.002)		(0.002)	(0.004)	(0.007)	(0.007)
DNC frequency		0.145***					
		(0.026)					
Reports focused on DNC			0.054***				
			(0.003)				
Reports peripheral to DNC			−0.002				
			(0.002)				
Reports with toll free number				0.058***			0.055***
				(0.006)			(0.005)
Reports with URL				0.021***			0.022***
				(0.005)			(0.005)
Reports with no. of registrations					0.019***		0.009**
					(0.004)		(0.004)
Headline length						−0.063***	−0.083***
						(0.010)	(0.010)
Text length						−0.033***	−0.027***
						(0.005)	(0.005)
Observations	163,134	163,134	163,134	163,134	163,134	163,134	163,134
Counties	3,078	3,078	3,078	3,078	3,078	3,078	3,078

Notes. The dependent variable is Log *registration*. All specifications included *number of households*, *household size*, *household income* ($K), *unemployment* (%), *commute time*, and *retail density* as controls (not reported), and state and week fixed effects. All estimates with county random effects. Robust standard errors clustered by county in parentheses.

$^{**}p < 0.05$; $^{***}p < 0.01$.

In summary, we conclude that the various checks suggest that our finding that news reports of the DNC Registry had a significant effect on DNC registration was robust to differences in timing, sample, and specification. Our finding was further supported by validation and falsification exercises.

7. Content

We now address our second research question—how did the content of the publicity affect consumer behavior? We extracted various characteristics of news reports to address this question. For convenient reference, Table 5, column (1), presents the baseline estimate from Table 3, column (2).

First, we included the frequency of the words "do not call" in the news reports. As reported in Table 5, column (2), the coefficient of the frequency was positive and significant. Next, we manually classified the news reports as either focused on the DNC Registry or only peripheral to the DNC Registry. As reported in Table 5, column (3), the focused reports had a strong and significant effect on DNC registration, whereas the peripheral reports had no significant effect. The coefficient of focused DNC reports, 0.054, was triple the (baseline) coefficient of all DNC reports, both focused and peripheral.

Finally, we investigated the impact of including a toll-free number or URL, or mentioning the number of people registering, and the length of the headline and main text with each of these measures weighted by circulation. Table 5, column (4), reports the impact of informative content: the impact of news reports increased significantly with the inclusion of both the toll-free number and URL. Newspaper reports of the DNC Registry inform consumers about the DNC Registry *itself*. The toll-free number and URL, however, inform consumers about registration *channels*. Such content provided additional information to consumers and had incremental positive impact on registration.

Next, we considered the impact of persuasive content. A person might infer the value of the DNC Registry from the number of other people registering (Bikhchandani et al. 1998). As reported in Table 5, column (5), the impact of news reports increased significantly with mention of the number of people registering. We interpret the empirical result as evidence that persuasive content affected DNC registration. Our finding adds to previous studies (Cai et al. 2009, Zhang 2010) by showing that observational learning takes place in very large public settings.[17]

[17] Furthermore, we read a sample of 10 reports and found the following content: nuisance imposed by telemarketing (four), positive experience with DNC (one), negative experience with DNC (one), politics (mention of Republican/Democrat) (two). Generally, the

Table 6 Summary Statistics of Newspapers by Politics

	Republican		Democratic		Neutral	
Variables	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Number of newspapers	46	—	52	—	7	—
Number of reports per newspaper	15.87	16.92	17.08	20.23	27.14	9.42
% reports with toll-free number	0.16	0.37	0.15	0.35	0.25	0.44
% reports with URL	0.18	0.38	0.16	0.37	0.24	0.43
Number of characters in headline	47.28	25.16	43.79	26.70	42.32	24.18
Number of characters in main body	3,145.99	1,868.84	3,509.77	2,591.35	3,193.00	2,067.84

Then, we considered the possibility of diminishing returns. As reported in Table 5, column (6), the impact of news reports diminished with the lengths of the headline and main text. The negative impact might be explained by readers skipping longer reports and those with longer headlines (Holmqvist et al. 2003). As a robustness check, in Table 5, column (7), we included all content measures and found results consistent with the estimates including the measures of content separately.

8. Media Characteristics

Our last research question was whether the impact of news reports varied with newspaper characteristics. One characteristic is the geographic scope of the newspaper. On this point, we found that, controlling for circulation, reports in national newspapers had almost four times the impact of reports in local newspapers (Table 3, column (7)).

Another possibly relevant characteristic is political affiliation. We classified newspapers according to whether they endorsed the Republican or Democratic major-party candidate or neither in the 2004 presidential election. Among 104 newspapers (76% of newspapers covered in our study), 46 endorsed the Republican candidate, 52 endorsed the Democratic candidate, and 7 endorsed neither major-party candidate.[18] Then, we reconstituted the newspaper report variable as

$$I_{it}^{p} = \sum_{j} (R_{jt}^{p} \times c_{ij}^{p}), \tag{5}$$

where p represents Democratic, Republican, or neutral. The model then became

$$\ln Q_{it} = \alpha + \sum_{k} \beta_k \ln x_{ik} + \lambda_D \ln I_{it}^{D} + \lambda_R \ln I_{it}^{R} + \lambda_N \ln I_{it}^{N} + \delta_i + \tau_t + \varepsilon_{it}. \tag{6}$$

If the politics of the newspaper had no impact on consumer registration, then $\lambda_D = \lambda_R = \lambda_N$. If, however, $\lambda_D \neq \lambda_R$, $\lambda_D \neq \lambda_N$, or $\lambda_R \neq \lambda_N$, then newspapers with different politics had differential impact on consumer registrations.

Table 1 reports summary statistics of newspaper reports in Republican, Democratic, and neutral newspapers. Table 6 reports descriptive statistics of the content of the news reports by the politics of the newspaper (not weighted by circulation). Neutral newspapers published most reports about the DNC Registry, followed by Democratic and Republican papers. Based on the various measures of content, there was no significant difference in the content of reports in newspapers by politics.[19]

Table 7, column (1), reports the estimates of the specification differentiating between newspapers by politics. News reports in neutral newspapers had the largest impact on DNC registration, with an elasticity of 0.050, whereas reports in Democratic newspapers had the next largest impact, with an elasticity of 0.012, and reports in Republican newspapers had no significant impact.

To better understand the impact of newspaper politics, we estimated a model accounting for voter behavior as well—specifically, we included the percentages of people in a county who voted for the Republican, Democratic, or third-party candidates, registered to vote but did not, or did not register at all, as independent variables. We further interacted these voting profiles with the news report variables (Democratic, Republican, and neutral) to estimate how the politics of news reports affected different types of voters.

As column (2) of Table 7 reports, DNC registration varied with voting behavior, an effect not observed previously (Varian et al. 2004, 2005). Relative to people

content varied with time. Reports first covered the nuisance imposed by telemarketing, then consumer experience, and, later, judicial and congressional action to suspend and then restore the registry.

[18] The total number of endorsements was 105. The discrepancy was due to the *Chattanooga Times Free Press*, which the Audit Bureau of Circulation reported as a single newspaper, but comprised two newspapers—the *Chattanooga Free Press*, which endorsed the Republican candidate, and the *Chattanooga Times*, which endorsed the Democratic candidate.

[19] The DNC Registry did not limit calls by political parties or charitable organizations. Therefore, newspapers with different political bias should not differ in their coverage of the DNC Registry.

Table 7 Newspaper Politics

		(2) With voter interactions			
Variables	(1) Reports		Republican paper	Democrat paper	Neutral paper
Reports in Republican papers	0.001 (0.003)				
Reports in Democratic papers	0.012*** (0.003)				
Reports in neutral papers	0.050*** (0.004)				
% *Republican vote*		1.468*** (0.216)			
% *Democratic vote*		0.815*** (0.222)			
% *third-party vote*		11.200*** (2.849)			
% *registered but did not vote*		−0.175 (0.150)			
% *Republican vote × reports*			−0.088*** (0.031)	−0.124*** (0.038)	−0.037 (0.034)
% *Democratic vote × reports*			−0.133*** (0.037)	−0.046 (0.041)	−0.037 (0.038)
% *third-party vote × reports*			−0.204 (0.990)	−1.218 (1.334)	−1.772* (1.010)
% *registered but did not vote × reports*			0.132*** (0.039)	0.065* (0.039)	0.169*** (0.037)
% *× did not register × reports*			0.059*** (0.017)	0.118*** (0.018)	0.108*** (0.018)
Observations	163,134		156,085		
Counties	3,078		2,945		

Notes. The dependent variable is Log *registration*. All specifications included *number of households*, *household size*, *household income* ($K), *unemployment* (%), *commute time*, and *retail density* as controls (not reported), and state and week fixed effects. All estimates with county random effects. In the interaction terms, the percentage vote is interacted with the number of reports in newspapers with political affiliation as indicated at the header of the corresponding column. Robust standard errors clustered by county in parentheses.
$^{*}p < 0.1$; $^{***}p < 0.01$.

who did not register to vote, Republican, Democrat, and third-party voters were more likely to register for the DNC Registry. There was some indication that newspapers with different political affiliations affected voters according to their political interests. Among Republican voters, reports in Democratic newspapers had a *negative* impact on DNC registrations, with an elasticity of −0.124. Among Democratic voters, reports in Republican newspapers also had a *negative* impact, with an elasticity of −0.133. These results suggest that readers are aware of possible political slant in newspapers and adjust accordingly when acting on information in newspaper reports.[20]

9. Concluding Remarks

Exploiting data from various independent sources and using multiple specifications and estimation methods, we found robust evidence that newspaper reports of the federal DNC Registry affected consumer registration. Our results provide guidance on the *magnitude* of the consumer response to newspaper publicity (i.e., the sales-response function). We found that a 1% increase in news reports of the DNC Registry was associated with a 0.018% increase in DNC registrations.

Our results on the content of reports have important practical implications for publicity. Informative

[20] For effective identification of the impact of news reports of particular characteristics (national newspaper, endorsing Democratic candidate, etc.), the coefficients of the various news reports variables in a single regression should be significantly different from each other. A set of postregression Wald tests show that this was indeed the case—the coefficients of reports in national and local newspapers (Table 3, column (7)), Republican, Democratic, and neutral newspapers (Table 7, column (1)), and the news report–voter interactions (Table 7, column (2)) were significantly different, with all p-values less than 0.01.

content such as the means to access a product/service (e.g., toll-free number, URL) could be helpful in promoting the use of the product/service, but a long report, or even a long headline, could hurt. We also found robust evidence that persuasive reporting mattered. These findings have immediate bearing on how managers prepare press statements and work with the media to generate publicity.

Finally, our findings regarding newspaper scope and politics have striking managerial implications. Even adjusted for circulation, national newspapers were more impactful than local newspapers. Media generally base advertising rates on circulation, so managers expect to pay more for more eyeballs. Our results suggest that, for the same rate per eyeball, managers should prefer national to local newspapers.

Furthermore, instead of merely considering the reach and demographics of readership, managers must also attend to the politics of media. In the DNC context, publicity in neutral newspapers was most effective, and Democratic newspapers next most effective, whereas publicity in Republican newspapers was not effective. To the extent that our findings generalize to other goods and services, managers would achieve greater impact by focusing their publicity efforts at neutral rather than politically affiliated media.

This study is subject to several limitations. First, our Factiva search yielded reports in only 136 of 1,197 U.S. newspapers audited by the Audit Bureau of Circulations. If reports in various newspapers are correlated and these are correlated with reports in television and other media—for example, through their publication of the same wire service reports—our estimates might have exaggerated the effect of newspaper reports on consumer registration.

Second, the impact of newspaper reports could be more precisely estimated if we had observations of consumer behavior at the individual newspaper level. The challenge is to observe the characteristics of those who registered with the DNC Registry and to associate each newspaper report with a particular individual or household.

Third, we lacked data on newspaper readership other than circulation. If the newspaper reports exerted impact through self-selection of readership, then our estimates could have been biased. Our estimation strategies using state-level registries, national versus local newspapers, and instrumental variables helped ascertain the positive impact of news reports, but they may not give precise estimates of the *magnitude* of the impact. It would be helpful for future research to control for unmeasured heterogeneity among the studied counties and newspapers, perhaps by augmenting the data set with readership information.

This study reveals that an important marketing function—media publicity—interacts with politics. Even for an apparently nonpolitical service, the impact of newspaper reports depended on the politics of the media. One avenue for future research would be to investigate whether media of different political affiliation report nonpolitical items with systematically different prominence and slant. Another important direction for future research would be to study whether politics of the media also affect the sales response to advertising. Does the effectiveness of advertising also vary with the political slant of the medium? The results of these studies would have obviously important implications for management and public policy.

Acknowledgments

The authors thank Hal Varian for directing them to the federal Do Not Call Registry, and the department editors, associate editor, anonymous reviewers, Junhong Chu, Matthew Gentzkow, Jesse Shapiro, Jasjit Singh, Peter Zeitz, and seminar participants at the City University of Hong Kong, the Industrial Organization and Management Strategy Workshop 2010, the National University of Singapore, and the University of California, Davis for valuable comments. They also thank Lulu Chen, Emily Jothidas, Huang Peiying, Elaine Tam, Gu Qian, and Lin Zhijie for assistance with compiling the data. This research was supported in part by Singapore Ministry of Education Grants R253-000-030-112 and R253-000-034-112, and Hong Kong SAR Central Policy Unit and Research Grants Council Project HKUST 1001-PPR-3.

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