Media Choice and Moderation Online*

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May 21, 2015

Abstract

Despite decades of frequent hypothesizing and inconclusive testing, evidence for partisan selective exposure remains elusive. Research on this venerable phenomenon tends to be either observational (using behavioral measures of exposure) or experimental (in which researchers manipulate the content of treatments in a laboratory setting). This study advances the literature on several fronts. First, I focus on Internet media, already the dominant source of political information for younger Americans and quickly becoming so for the rest of the population. This has theoretical implications for how a fragmented, high-choice environment moderates traditional media effects. Second, in collaboration with an online polling firm, I collect unique and unprecedented data tracking the real-time browsing behavior of a panel of Internet survey respondents. Third, I supplement this observational portrait with an online field-experimental design to test the real-world behavior of a smaller but similar subject pool. In both studies, I employ a novel measurement strategy that allows me to capture trace data on individuals' actual consumption of political media. This allows me to directly study how open-ended search strategies for political information vary by partisan affiliation. In doing so, I uncover consistent evidence of moderation rather than selective exposure. I also find mixed evidence on the downstream effects of media consumption on attitudes, opinions, and knowledge.

*I would like to thank Noah Buckley, Alex Coppock, Trish Kirkland, Robert Shapiro, Gaurav Sood, and participants at Columbia University's American Politics Workshop for helpful comments and suggestions. Special thanks to Doug Rivers and Brian Law at YouGov for facilitating access to the Pulse panel data. Some of the analysis was made possible by Columbia University's Hotfoot High Performance Computing (HPC) Cluster.

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1 Introduction

Despite vast changes in the way Americans seek out and receive information about the world around them, political scientists still tend to conceptualize media effects within a largely 20th-century framework. Campaigns target smaller and smaller niches; new media outlets tailor their content to narrower audiences; videos of off-the-cuff remarks spread virally within social networks. But unless the outcome of such processes can be measured in aggregate vote returns or large-scale tracking surveys, they seemingly remain illusory.

Making persuasive inferences about media effects requires surmounting an unusually challenging set of obstacles. Those outlined by Bartels (1993) as particularly difficult in a classic overview remain so today: with aggregate time-series data, media coverage can easily be confounded by the events precipitating it; with individual-level cross-sectional data, self-reported exposure may be correlated with political interest or other unobserved predispositions; and laboratory experiments, while ruling out alternative causal factors, still remain open to criticisms about external validity.

Adding to these challenges, measurement of exposure to media content in the real world tends to be based on unreliable self-reports, irregularly validated proxies, or aggregate traffic data that masks individual-level variation. When innovative solutions to the problem present themselves, they can be leveraged to produce useful observational evidence on media exposure. But to generate convincing evidence on effects, such a measurement strategy needs to be coupled with an experimental approach.

Changes in technology and Americans' media habits both add to the difficulties and point the way toward possible solutions. One the one hand, a handful of network and cable TV channels is now thousands of potential sources of news and information about politics, driven by online-only (and often partisan) new media outlets and feeds on social networks (Mutz and Young 2011). On the other, the proliferation of data on users' online habits means that it is now more possible than ever to track individual behavior (King 2011; Bond et al. 2012).

This study explores how media effects play out online. More and more Americans get their news and information about politics online: According to Pew, in 2013 82% of Americans said they got news on a computer, and over half (54%) did so on a mobile device (Pew 2014). While online media are not yet the primary source of political information for most Americans, they are for the youngest age groups, and the upward trend continues.

In some ways, these changes may make identifying media effects harder than ever. For example, the "forced-choice" paradigm assumed by earlier studies of media effects is not well-suited to an information environment that people can, in part, construct and customize for themselves (Arceneaux and Johnson 2010). Selective exposure affects not only whether individuals see and hear particular content, but how they consciously (and unconsciously) organize their entire media diets (Bennett and Iyengar 2008, p. 724). Theoretically, this means that substantively meaningful self-selection could have occurred well before exposure to any particular content is observed; passive consumption is perfectly compatible with active curation. This insight merely pushes concerns about self-selection of congenial information back one level: to the strategies and defaults arranged in advance rather than the particular content heard or viewed on a particular occasion.

The most convincing evidence of media effects will come from research conducted in naturalistic settings. This study uses a field-experimental design to identify the causal effect of individuals' informational search strategies online. I combine this approach with direct measures of subjects' Internet behavior to test whether partisan selective exposure mediates the effects of political information online. In doing so, I provide the first test of whether both a "balanced" news diet and selective exposure can coexist within the same framework.

The paper proceeds as follows. First, I provide a brief overview of the most recent evidence on media effects and selective exposure. Second, I outline a new theoretical contribution to the study of media effects in the context of open-ended information search. I then introduce and provide unique observational evidence on online media exposure. Next, I describe the design and measurement strategies of the field experiment and report the results. A discussion then concludes.

2 A Motivating Puzzle

Since the original Columbia studies, research within the "minimal effects" paradigm has often relied on the mechanism of selective exposure to explain null findings of media effects (Lazarsfeld, Berelson and Gaudet 1944; Klapper 1960; Bennett and Iyengar 2008; Arceneaux and Johnson 2010). Yet, as a growing number of scholars have documented, the evidence for selective exposure is not as strong as initially claimed. This leads to two related puzzles: First, why does the selective exposure hypothesis find support in laboratory experiments but not observational studies using real-world behavioral measures? And second, if selective exposure is not as common as once believed, why are media effects outside the lab still modest and fleeting, if they can be identified at all?

These questions cut to the core of longstanding questions about the extent to which democratic citizens are exposed to competing viewpoints, thought to be a prerequisite for informed collective decision-making (Mutz 2006; Jamieson and Cappella 2008; Shapiro 2013). In the worst-case scenario, most forcefully articulated in the context of the 21st-century fragmented media environment by Sunstein (2007), people elect to consume only ideologically congenial information (the "Daily Me"), resulting in an echo-chamber effect and, ultimately, increasing polarization (see also Negroponte 1995). Others have additionally worried that hidden algorithms could speed along this process by replicating and reinforcing people's preferences, especially on social media (Pariser 2011).

Since the critical review of Sears and Freedman (1967), however, skeptics have questioned whether people are as selective in their choices of how to receive information as previously supposed. More recently, advances in measurement and data collection have backed up these claims. For instance, Gentzkow and Shapiro (2011) use aggregate web traffic data to conclude that ideological segregation online is less severe than for national newspapers or in face-to-face social networks. On Twitter, Barberá (2014) uses panel data to show evidence of cross-cutting follow patterns that lead to moderation rather than polarization.

This body of work sits alongside research uncovering at least suggestive evidence of selective exposure (Garrett 2009, 2013). First, there is experimental evidence from the laboratory: The main finding of the studies conducted by Arceneaux and Johnson (2010) is that, consistent with Prior (2007), allowing subjects the choice to "tune out" of political programming moderates the polarizing effect of political news content. But among subjects who select *in* to political news, there is an apparent sorting effect whereby subjects spend more time with pro-attitudinal than counter-attitudinal content. Other research finds a polarizing effect driven by people who are already fairly extreme in their views (Levendusky 2013). Second, using panel data of over-time dynamics during a presidential campaign, there is evidence of partisan selective exposure (Stroud 2008).

How can these findings be reconciled? It is likely that research using self-reported measures exaggerate the evidence for selective exposure, given that people may be more likely to remember using sources that align with their political identities. Another possibility is that evidence of effects in the laboratory do not generalize to the real world and that given day-to-day distractions and competing demands for time, people's propensity to select sources of congenial information is lower than expected. Finally, it may be the case that under certain conditions, people can be induced to behave in ways consistent with the selective exposure hypothesis, but under others they fall back on more balanced media consumption habits.

I hypothesize that these conditions are determined by the requirements of a given task. Seeking out particular information—for example, in the context of an upcoming election or an issue that could affect an individual personally—means relying on a set of open-ended search strategies that may differ markedly from day-to-day forms of media consumption. Central to the distinction between these two modes is the idea of *passive* versus *active* reception of information. The former occurs within a context of ingrained habits and defaults, while the latter may depend on cues and heuristics that help people navigate less familiar territory (e.g., Popkin 1991).

More concretely, the picture given by the best observational evidence on media choice is that of a vast middle: Most people turn to large, relatively centrist sources of new for political information. This pattern could be driven by a number of mechanisms. Perhaps it is explained by a pattern of preference for ideologically centrist content (Fiorina, Abrams and Pope 2011). Perhaps it reflects a general lack of interest in political information or is even a byproduct of people's preferences for *non-political* content. Or perhaps it is a result of something else: the defaults and browsing habits of the online public.

I focus on this last possibility because it has the potential to explain how selective exposure as a mechanism could operate even against the backdrop of a relatively balanced aggregate news diet. According to this view, media consumption habits are the result of an interaction between preferences for content (whether for entertainment versus news, or for a certain ideological slant) and the information environment—the number of available sources, the cost of switching, etc. But while contemporary accounts of Internet media tend to assume costs that approach zero, they fail to take into account the hidden obstacles and defaults that structure people's habits online.

To take the simplest example, modern web browsers come pre-loaded with bookmarks for large news and entertainment sites (such as AOL and Yahoo). Many people still use portals for email and other services which link to headlines, weather and other information. Sometimes, such sites automatically load on startup. It is not hard to customize one's settings, but doing so already requires preferences over sources—a perceived cost that may be too high for many people. For individuals with passing or intermittent interest in politics, for example, such built-in choices may be sufficient for most day-to-day needs.

Conversely, people may need to rely on different strategies when actively seeking out information (e.g., Iyengar et al. 2008). Rather than passively relying on semi-hidden defaults, it becomes necessary to use search engines, query one's friends or social networks (crowdsourcing), or consciously think about particular sources of information that would be useful. If the passive mode encourages a tendency toward centrism and homogeneity in media choices, active search creates opportunities for personal predispositions to creep in at every stage. Search engines and social networks can customize results based on past preferences and actions, reinforcing past biases; shortcuts based on partisan or ideological affinity could drive decisions about media sources.

One interesting implication of this distinction between active and passive search processes is that the former could eventually become incorporated into one's media diet—novel strategies transforming into defaults. While speculative, this would predict a long-term over-time trend toward more ideologically segregated media diets, even from a relatively centrist starting point. This would explain the findings of Stroud (2008), who found evidence of partisan selective exposure in panel survey data (including respondents who reported online media use).

If this picture is accurate, then the observed lack of evidence for selective exposure in observational data could be a result of choice architecture as much as people's preferences for diversity in media sources (e.g., Sunstein and Thaler 2008). Likewise, when the structure of the information environment favors ideological segregation–as it arguably does on Twitter, where it is easy to find co-partisans and retweet only their content to like-minded followers—aggregate data follows the same pattern (Conover et al. 2011). One question this paper poses is whether the selective exposure mechanism can be separated from the media context, and whether the persuasive effects of media content vary as a result.

This paper is structured in two parts. First, I take advantage of a large and unique survey panel whose members record real-time tracking data on their online browsing habits. Merged with individual-level survey responses, this data offers an unprecedented look at the interaction between political predispositions and media diet. In this first section, I provide an overview of the data and offer an observational look at the political browsing habits of the survey's respondents. Second, I supplement this analysis with an online field experiment combining a similar direct measure of respondents' online media exposure with individual-level covariates.

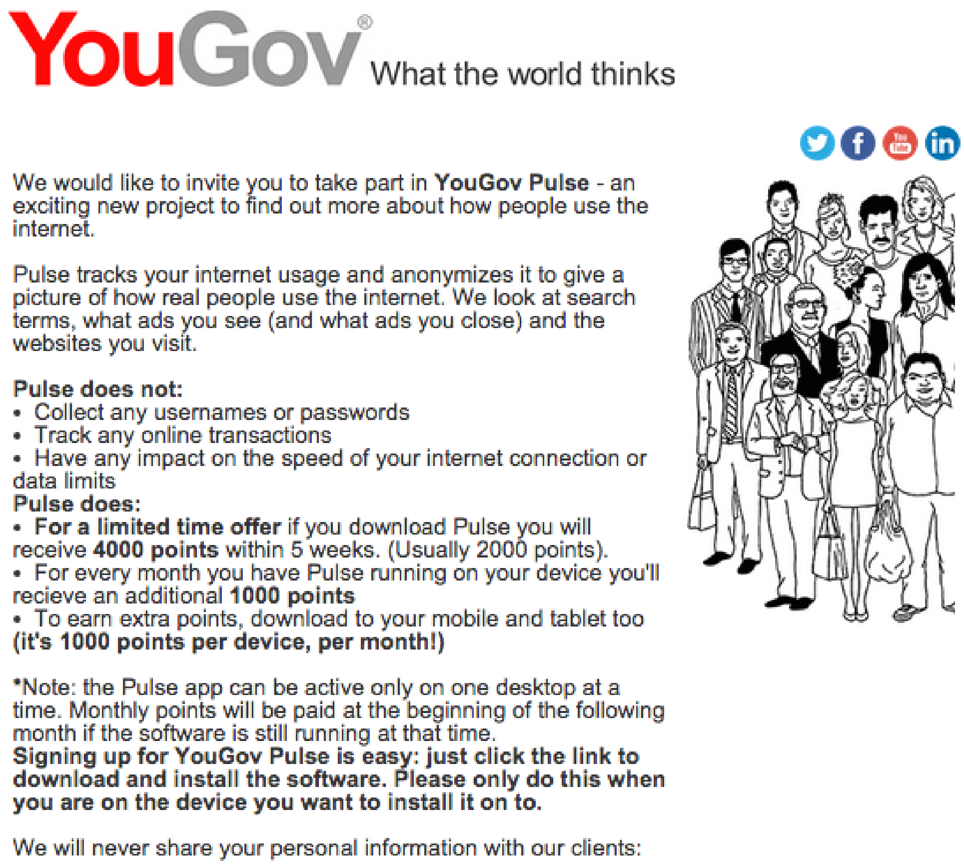
3 Balance or Bias in Online Media Exposure?

The first step of this investigation is to make use of a unique data set: individual-level survey data merged with continuous tracking of panelists' Internet browsing behavior. This data, which is completely anonymized, was collected by the online polling firm YouGov as part of an ongoing attempt to gauge whether survey respondents are willing to install tracking software (called Wakoopa). This data provides direct evidence of respondents' media habits for political (and non-political) information. It is unique in that it combines data on site visits with individual-level survey responses. For this particular panel, there are no limits to the types of websites that can be included in the data. Moreover, the software tracks web traffic (minus passwords and financial transactions) for all browsers installed on a user's computer and cannot be blocked.

The online tracking panel is currently branded as YouGov Pulse (see Figure 1). Panelists are recruited from YouGov's traditional participant pool via incentives. At least initially, these incentives have been very strong: 4,000 "points" for signing up and downloading the Wakoopa software—roughly 8 times the number offered for a typical survey—and 1,000 additional points every month. Participants in online surveys can redeem these points for clothing, prepaid gift cards, and other merchandise.

The data set contains more than 6.3 million observations at the respondent-site level, covering panelists who installed the tracking software on their desktop computers (excluding mobile phones). This sample includes site visits from 1,392 individuals over a three-week period in 2015, from February 27 to March 19. Since respondents were not recruited using random sampling, YouGov typically employs sample-matching weights to make its results representative of the general population, although I have not yet been able to obtain such weights for the panel (Rivers 2006). For now, I refrain from making overgeneralizations but note that the results below closely match similar analyses from the Mechanical Turk sample in the second study.

Table 1 summarizes the demographic characteristics of the sample. Perhaps most notably, it skews younger and more educated than the general population, although the gender, racial and party breakdown are fairly representative and capture a more diverse cross-section than the MTurk samples dis-



YouGov® What the world thinks

We would like to invite you to take part in **YouGov Pulse** - an exciting new project to find out more about how people use the internet.

Pulse tracks your internet usage and anonymizes it to give a picture of how real people use the internet. We look at search terms, what ads you see (and what ads you close) and the websites you visit.

Pulse does not:
- Collect any usernames or passwords
- Track any online transactions
- Have any impact on the speed of your internet connection or data limits

Pulse does:
- **For a limited time offer** if you download Pulse you will receive **4000 points** within 5 weeks. (Usually 2000 points).
- For every month you have Pulse running on your device you'll recieve an additional **1000 points**
- To earn extra points, download to your mobile and tablet too **(it's 1000 points per device, per month!)**

*Note: the Pulse app can be active only on one desktop at a time. Monthly points will be paid at the beginning of the following month if the software is still running at that time.
Signing up for YouGov Pulse is easy: just click the link to download and install the software. Please only do this when you are on the device you want to install it on to.

We will never share your personal information with our clients:

Figure 1: Screenshot of an email sent to YouGov panelists on April 8, 2015, inviting participation in YouGov Pulse.

cussed below.

Most directly, this data allows me to test whether respondents silo themselves into informational cocoons according to partisanship or ideology. In order to measure the general ideological orientation of individual political websites, I employ data from the Internet analytics firm comScore, which maintains a 12,000-person survey panel of the general Internet audience called Plan Metrix. Employing both direct responses and imputation, comScore provides estimates of the overall demographic composition of individual sites' audiences. Using these estimates from March 2015, I create two separate audience-based measures of website slant. The first employs the ideological self-placement of site visitors in the Plan Metrix panel. I take the share of respondents who classify themselves as "very conservative" or "somewhat conservative" as a fraction of those who place themselves anywhere on the 5-point ideological scale (also including "middle of the road," "liberal," and "very liberal"). This creates an index of conservative readership that can take values from 0 to 1, although practically

Table 1: YouGov Pulse Sample: Demographics

Category	Proportion	Category	Proportion
18-30	0.233	Male	0.440
31-40	0.253	Female	0.560
41-50	0.156	Dem	0.367
51-60	0.230	Rep	0.224
61-70	0.113	Indep	0.305
Over 70	0.015	Other	0.038
No HS	0.033	White	0.679
High school	0.218	Black	0.103
Some college	0.377	Hispanic	0.069
College	0.250	Asian	0.059
Postgrad	0.123		

speaking the scores do not go above 0.85. The second measure of slant uses the partisanship of Plan Metrix panelists: In a similar way, I compute the Republican share of those who identify with either party.

The measures correlate with each other fairly well ($r = 0.50$). Since this is not perfect, I present results using both measures for completeness. While the estimates have high face validity, one peculiarity is evident from the figures below: most popular sites tend to cluster somewhat left of center (0.5). This is an artifact of the measures' construction: Since no site achieves 100% conservative readership, the distribution is pushed to the left. Relative ideological placements are not affected, however. Table 2 displays a sample of the most-visited websites in the YouGov Pulse panel, along with the number of visitors logged over the three-week period and both measures of slant. Immediately apparent is that MSN News—a mainstream, centrist news and information portal—is by far the most popular source, by almost an order of magnitude. This somewhat matches the available comScore figures, which show that among political and news sites logged in the YouGov data, MSN is the second most visited overall (to CNN.com). Looking at the partisan and ideological slant measures, it is clear that conservative sites such as the Drudge Report and Townhall have higher scores than left-leaning sources such as *Slate*. (One possible anomaly is Daily Kos, a left-liberal site whose ideological score appears more accurate than the partisan one.)

Before combining these two sources of data together, I categorized the YouGov Pulse panel's site visits so that I could separate those relating to news and politics from the rest of the web traffic.[1] I

[1] I used a combination of Wakoopa's proprietary categorization scheme and keyword searches to categorize roughly 73% of website visits in the sample. The remaining sites comprise a "long tail" with very few visits each.

Table 2: The partisan and ideological slant scores of some of the most-visited sites in the sample, arranged in reverse order of popularity. All scores are listed in Appendix B.

Site	# Visits	Partisan	Ideo	Site	# Visits	Partisan	Ideo
msn news	36263	0.447	0.339	bbc	1976	0.445	0.326
yahoo news	5000	0.451	0.341	theblaze.com	1640	0.496	0.348
foxnews.com	4777	0.521	0.372	cnn.com	1565	0.436	0.319
townhall.com	4372	0.571	0.450	breitbart.com	1317	0.507	0.404
buzzfeed.com	4077	0.412	0.285	nbcnews.com	1103	0.461	0.333
huffingtonpost.com	3898	0.453	0.337	wsj.com	994	0.46	0.362
nytimes.com	2532	0.449	0.332	telegraph.co.uk	952	0.432	0.324
news.google.com	2253	0.424	0.331	freep.com	914	0.503	0.350
drudgereport.com	2201	0.624	0.454	slate.com	852	0.414	0.285
daily kos	2148	0.483	0.331	nypost.com	764	0.456	0.362
washingtonpost.com	2025	0.471	0.351				

also removed visits to local news websites to focus on national sources. Confirming similar findings elsewhere (Flaxman, Goel and Rao 2013), the resulting share devoted to news and information about national politics is strikingly low: 1.6 percent of all visits, or just over 102,000 out of the 6,319,441 observations in the sample. I then separately aggregate the number of visits per site for all respondents as well as those who identify as Democrats and those who identify as Republicans. Overall, I could match 208 politics and news websites in the sample to a measure of slant. For Democrats only, this number is somewhat lower, at 175, and for Republicans there were 133 (Table 1 shows that there were fewer Republicans in the sample, possibly driving this disparity).

Does the Internet facilitate the Daily Me? Figure 2, which plots the density of site visits against the measure of ideological slant, shows that this is generally not the case. Most site visits in the YouGov Pulse sample cluster around a handful of relatively centrist sources such as CNN, MSN, and Yahoo News. The density curves for Democrats and Republicans are similar to each other and also to the curve for the sample as a whole. The distributions are not bimodal, as extreme ideological segregation would predict. However, there are two smaller bumps at the extremes, corresponding on the right to popular conservative sites Townhall and the Drudge Report. The bump on the left is actually Buzzfeed which, although its overall audience might lean liberal, is generally considered a mainstream news and entertainment site. The corresponding graph using the partisan slant measure rather than the ideological measure is given in Appendix C. The general pattern is the same, although the scores are somewhat more dispersed.

The evidence, then, is consistent with a view that—among the small fraction of respondents who

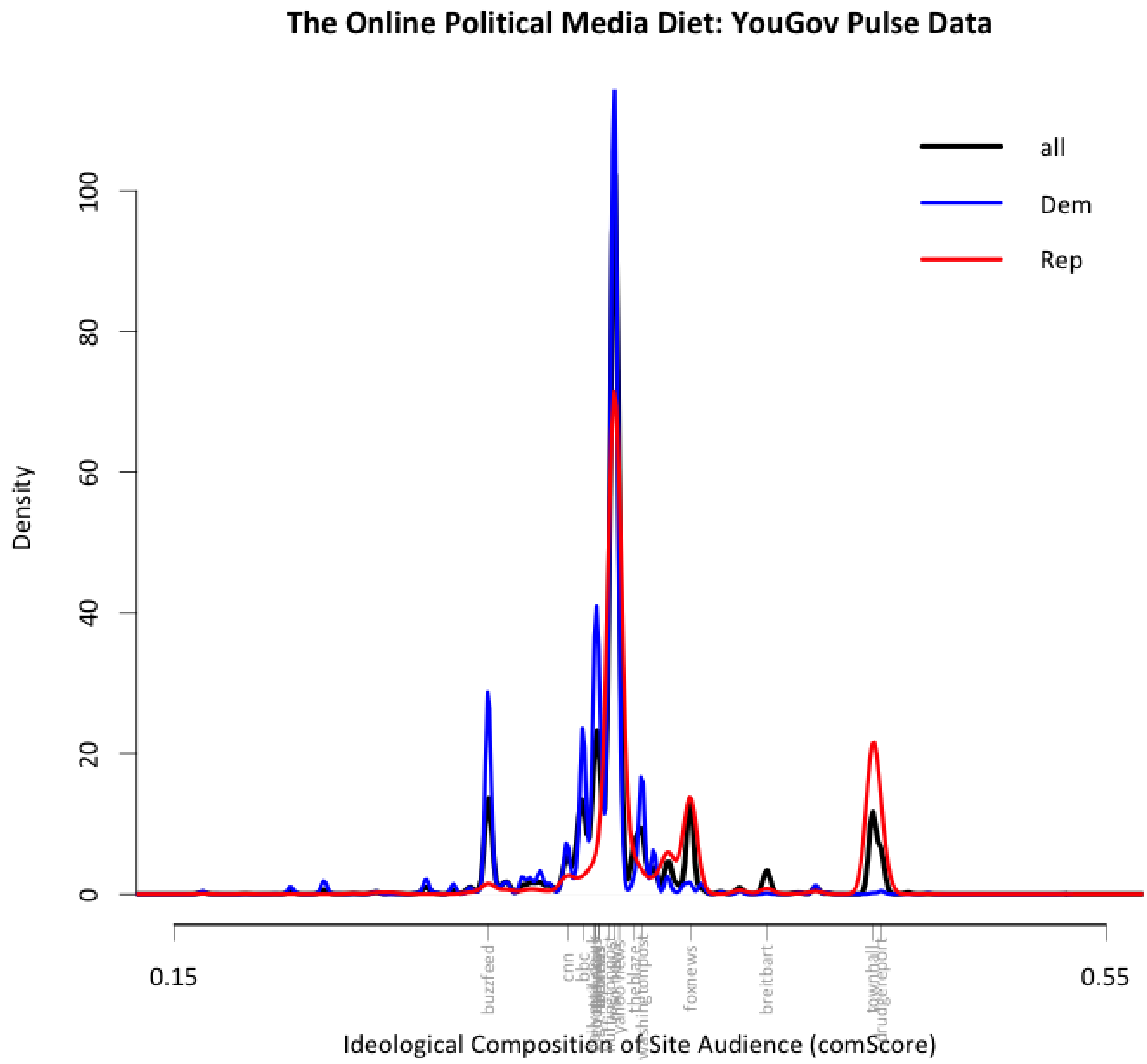


Figure 2: Density plot of aggregate site visits from the YouGov Pulse sample. Site ideological slant on the x-axis is measured using comScore data on audience composition. N = 102,128 visits.

actively visit news and politics websites—the preponderance of the content encountered is relatively centrist and balanced ideologically. At least in this sample, there also appears to be suggestive evidence of a smaller, intense subgroup of Republicans who (possibly in addition to mainstream sources) consume conservative, but not liberal, news and information about politics. A similar bump on the left, corresponding to the popular viral site Buzzfeed, seems to be an artifact of the ideological measure and not an indication of symmetric echo chambers in online media diets.

4 Experimental Design

It is possible that this observational picture masks substantial variation. In order to gain causal leverage on Internet search behavior in a real-world environment, this study uses an online field experimental design. I take advantage of several innovations to improve measurement and inference. First, I capture outcomes on actual browsing behavior via a small piece of software installed beforehand (with permission) on subjects' computers. Second, I randomly assign panelists to receive an email treatment designed to induce a purposeful, open-ended search for information about a particular, low-salience political issue: the regulation of for-profit colleges and universities. This enables a comparison between two sets of potential outcomes: those of subjects relying on default strategies for processing political information, and those induced to expend additional effort to seek out novel information.

4.1 Measurement Strategy

With a few recent exceptions, studies of media effects have traditionally relied on self-report methods of varying quality for measuring exposure (see Price and Zaller 1993; Prior 2009*a*,*b*). Elsewhere, I summarize various attempts to overcome the measurement difficulties and propose a method of capturing actual browsing behavior in studies of online media effects (Guess 2015). The method takes advantage of data about subjects' Internet history collected in their web browsing software. I use a browser plug-in to capture a subset of that history (for a prespecified list of sites and over a fixed length of time) and record it alongside matching survey responses. Using this approach, I found that the most commonly used survey-based measures of media exposure generate a large amount of overreporting.

I adapt this method for the current study's measurement strategy as follows. I created a small piece of software for Google Chrome browsers that saves trace data from a user's web history: whether or not any site from a predetermined list has been visited in the past five days. When manually activated, the browser plug-in takes this snapshot and transmits it to a Qualtrics database, linked with the (anonymized) responses from the same user's survey input. In order to set up a panel of subjects with this software installed on their primary computers, I posted a survey on MTurk collecting pretreatment covariates and requesting that respondents optionally install the software for a 50-cent bonus (on top of the small payment offered for completing the short survey regardless).[2]

[2]MTurk respondents could click for a list of the URLs scanned by the browser plug-in. I also provided the source code for the software, written in JavaScript using Google's Chrome API. These steps were intended to reassure potential subjects

One limitation of the direct measurement approach is that it assumes that the list of URLs the software can search for in subjects' histories is exhaustive. For generic applications, this seems like a defensible assumption. However, for studies of information search behavior, some users might seek out sources that are hard to predict in advance.

To generate the list of sources that the software can capture, I combined two approaches. First, I collected data on average monthly unique visitors from comScore, collecting sites listed in the "General News" category with at least 300 per month, those listed under "Politics" with at least 50, and those in the larger "News/Information" category with at least 1,500. I removed sites that were primarily local, non-U.S., or non-news-related to maintain the focus of the list. Finally, I supplemented this list with the sites of the top 10 newspapers by total average circulation and entries from a number of partisan blog directories. The total number of URLs generated using this first approach is 156.

I augmented this list of general news and politics sources using a second approach more directly tailored to the particular design of this study. Thinking through the possible steps of how one might look up information about a novel political topic, I used as wide a net as possible for collecting URLs to check for: search engine results from various queries related to the issue ("for-profit education," "for-profit colleges," etc.); Wikipedia pages, social media, explainer sites, higher education news sources, partisan sites, and even political non-news sites (such as Senator Tom Harkin's page dedicated to the issue). This generated an additional 72 links, for a total of 228.[3]

4.2 Subject Recruitment

I recruited subjects via Mechanical Turk (MTurk), an online marketplace for requesting and completing self-contained, relatively straightforward tasks Berinsky, Huber and Lenz (2012). Of 1,500 initial respondents, N=467 both agreed to install the browser widget and successfully did so. From this experimental sample, I then randomized subjects into either treatment and control, as described in the next subsection. All subjects in the sample were in the U.S. and had at least a 95% approval rate on previous MTurk tasks (sample characteristics: 65.7% male; 81.8% white, 6.2% black, 6.9% Hispanic; 50.5% college-educated; 43.3% Democrat, 14.3% Republican; median age, 29).

that the software does not collect identifying information or perform open-ended searches of users' browser history. At the time the study was conducted, Amazon's MTurk terms of service prohibited requiring workers to install software as a condition for being paid.

[3]See Appendix A for a full list of sites included.

4.3 Timeline

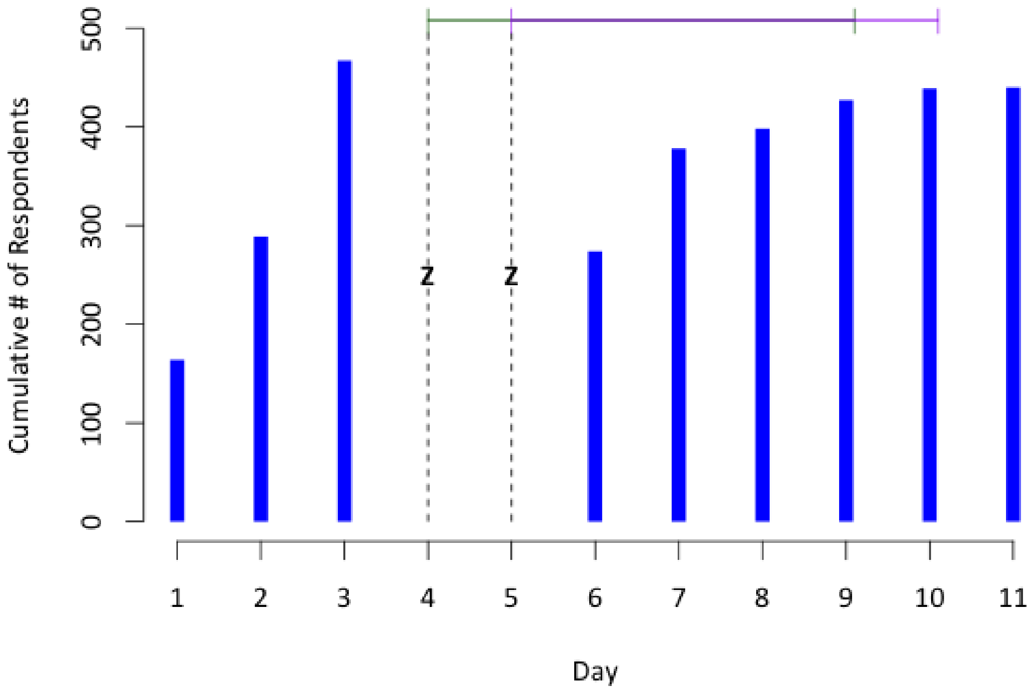


Figure 3: Study timeline. The green and purple lines indicate the "memory" of the browser plug-in software, which could only capture site visit data for the previous 5 days.

Figure 3 visualizes the timeline of the study by showing the cumulative number of respondents in each wave in blue. After the initial collection of pretreatment covariates and installation of tracking software, the 467 subjects were assigned to treatment and control via complete random assignment (days 4 and 5 of the study).

The pretreatment survey asked questions about attitudes on a number of different issues, in addition to demographic and political covariates. It was designed in such a way as to make it difficult for a respondent to connect the content of the survey with the subsequent treatment. The treatment itself was an emailed encouragement to seek out information about one of the political topics asked about in the previous survey (sent twice, 1-3 days after the initial survey). The MTurk system allows requesters to send messages to workers who have completed previous tasks (in this case, the pretreatment survey). Here, an otherwise irksome feature of this particular subject pool—the fact that many workers complete dozens of different tasks a day, including many social science surveys—becomes an advantage: Anecdotal evidence from communications with workers suggests that it is unlikely that an emailed request to complete a subsequent task would be tied back to a given earlier survey. This is crucial because while pretreatment covariates are useful for producing efficient estimates, they should be collected in a way that minimizes the likelihood of demand effects. Another advantage of this type of treatment is that it is ecologically valid: MTurk workers often receive electronic requests to complete additional tasks, and the present treatment is delivered in the context of subjects' real-world, day-to-day information environments.

This overall approach is similar in spirit to that of Albertson and Lawrence (2009), who analyzed an encouragement design in which telephone survey respondents were randomly assigned to be asked to watch educational television programs. Follow-up surveys then asked respondents whether they complied and collected post-treatment measures of knowledge, attitudes, and salience. I collected the first two outcomes, although I only measured knowledge post-treatment in order to avoid anticipation or other demand effects. After giving subjects at least two days to comply with the encouragement, I then sent another emailed request (to those in both treatment and control) for responses to a follow-up survey (days 6-11). The recontact rate was high, at 93.4%.

In both the pretreatment and post-treatment surveys, I measured subjects' media exposure via the tracking software. As as a result, I can construct measures of subjects' initial (baseline) recent media exposure and the additional sites visited after that point for those who did and did not receive the treatment.

4.4 Treatment

In designing the treatment, I selected an encouragement intended to activate the kind of search for novel political information that would uncover the influence of selective exposure, if it is an identifiable mechanism. In particular, I asked subjects to spend some time learning about a political topic (regulation of for-profit colleges and universities) that affects many Americans and could conceivably be mapped onto the partisan divide, but is not salient in current political debates.

To more specifically invoke the mechanisms I was interested in, I worded the encouragement in the following way: "We'd like to follow up soon with a short survey to gauge your responses to a few more questions. In the survey, we will ask about the controversy over for-profit education. To prepare for that questionnaire, we'd like you to familiarize yourself with the issue. Feel free to look up information online the way you usually do, using whatever methods or sources you are comfortable with to learn about for-profit colleges."

5 Results

Here, I analyze the web browser data to look for patterns of selective exposure in the treatment and control groups. First, I show below that the treatment was effective: It caused respondents in the treatment group to seek out information about an issue that, by and large, they were not highly knowledgeable about. I created an indicator for respondents who visited at least one site on the list of sources specifically about for-profit education captured by the browser plug-in (72 possible), and regressed it on treatment assignment. Given the possibility that some sources were not included in the list of websites searched by the software, this is a lower bound. Table 3 shows that being assigned to treatment caused a roughly 10-percentage-point boost in the share of subjects who searched for information about the topic online—a substantively large magnitude, but still a relatively low level of compliance.

Table 3

	Dependent variable:
	Visited Pages About For-profit Colleges
Assigned Treatment	0.10***
	(0.04)
Constant	0.04
	(0.03)
Observations	467
Adjusted R^2	0.02
Note:	*p<0.1; **p<0.05; ***p<0.01 OLS, standard errors in parentheses.

5.1 Selective Exposure?

In looking for evidence of "selective exposure" in this section, I test whether subjects' partisan predispositions predict patterns of media choice. Strictly speaking, I cannot make a causal claim: partisan and ideological commitments are not randomly assigned. However, we can observe whether subjects' behavior in the treatment and control conditions seem to differ systematically in ways that are instructive. To simplify presentation, I display the results—heterogeneous effects of treatment on Democrats and Republicans' partisan media diets—graphically below.

First, however, I need an approximate measure of media outlets' partisan leanings. For the purposes of this analysis, as described above, I again use the survey data from comScore. To construct an individual-level measure of the partisan lean of subjects' media diets, I simply compute the average of the comScore partisanship index (Republican share divided by the Democratic and Republican share) for each site visited at least once by that person.

I begin with a plot of the correlation between subjects' party identification and the partisan lean of their (pre-treatment) media diets. Figure 4 shows that there is such a correlation but that it is weak—an increase from just under 0.45 to approximately 0.47 going from strong Democrats to strong Republicans. In general most people consume political media near the "center," where the center in this case is, again, somewhat shifted to the left. Furthermore, one can see that the correlation may partially be driven by extreme outliers: a handful of individuals with homogenous, very liberal or very conservative media diets that correspond to their stated political leanings.

What happens when we try to induce individuals to seek out new information? Do they rely upon defaults and habits? If so, do these defaults nudge people in the direction of centrist mass media or partisan sources? To shed light on these questions, I run some simple linear models investigating the heterogeneous effects of the treatment on subjects' media diets. Table 4 shows the effects of treatment on post-treatment media diet slant. Model 1 shows that the treatment alone has a small but statistically significant effect in the leftward direction. I make no explicit prediction about this main effect, but it may represent the overall critical nature of commentary about for-profit education online, which would tend to support robust federal action and express skepticism of the industry's motives. Model 2 adds the pre-treatment measure of media diet, which was captured in the first survey wave using the browser extension software. Not surprisingly, people's over-time media habits are strongly correlated.

Since it is highly prognostic, I include the pre-treatment measure in the subsequent models to improve the precision of the estimates. In Models 3 and 4, we see a consistent pattern regardless of whether we investigate heterogeneity by party or ideological leaning: the negative (but small) statistically significant interaction coefficient implies that the treatment pushes subjects' media diets in a more leftward direction the more *conservative* they are. This is the opposite of what we would expect

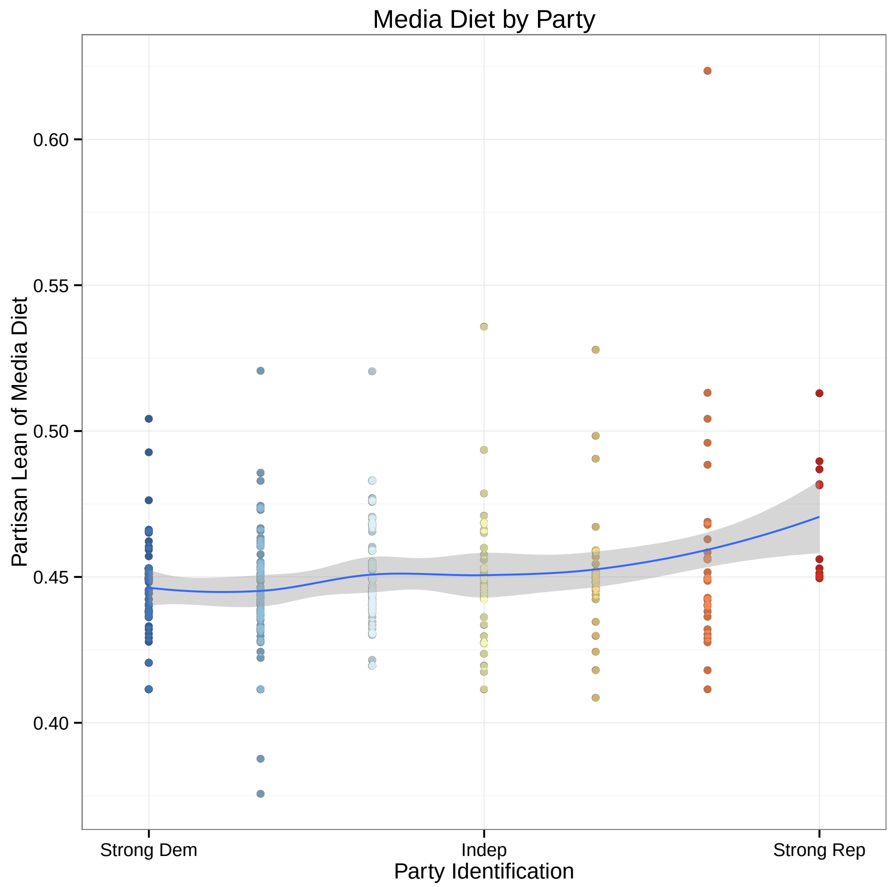


Figure 4: This plot shows the correlation between individuals' political predispositions (as measured by the standard 7-point party identification scale) and the average partisanship of their media diet (before treatments were administered, in the first survey wave).

from selective exposure. Rather than seeking out reinforcing information, partisans in the sample sought out heterogeneous sources, resulting in moderation in their diets overall.

Since interactions can be difficult to interpret, I further investigate these effects graphically. Figure 5 plots the distribution of individuals' average media diet partisanship for four groups: Democrats assigned to receive treatment, Republicans assigned to receive treatment, Democrats in the control group, and Republicans in the control group. Strikingly, as with the YouGov Pulse data, the same overall pattern of centrist media exposure can be seen here, with one significant exception. Republicans in the control group had a more conservative overall media diet on average, but after treatment was administered, the distribution of Republican subjects' media diet partisanship moved sharply to the left. Thus it is the relative moderation of Republicans in the sample driving the overall effect heterogeneity. After that shift, the distributions of the four groups is essentially identical, again casting

Table 4

	Dependent variable:			
	Media Diet Partisanship			
	(1)	(2)	(3)	(4)
Assigned Treatment	-0.01^{***}	-0.01^{*}	0.01^{*}	0.01^{*}
	(0.004)	(0.003)	(0.01)	(0.01)
Pre-treatment Media Diet		0.49^{***}	0.49^{***}	0.46^{***}
		(0.05)	(0.06)	(0.05)
Party ID			0.01^{***}	
			(0.002)	
Ideology				0.01^{***}
				(0.002)
Treated x Party			-0.01^{***}	
			(0.002)	
Treated x Ideology				-0.01^{***}
				(0.002)
Constant	0.46^{***}	0.23^{***}	0.21^{***}	0.23^{***}
	(0.003)	(0.02)	(0.03)	(0.02)
Observations	286	229	217	229
Adjusted R^2	0.03	0.29	0.33	0.32
Note:	*p<0.1; **p<0.05; ***p<0.01 OLS, standard errors in parentheses. Party and ideology coded using 7-point scales.			

doubt on the selective exposure hypothesis, at least for this particular issue.

5.2 More Comparisons

Finally, below I make further observational comparisons between groups in the MTurk sample. In Figure 6, I look at the distribution of overall visits to political news sources by different subgroups. Both plots show essentially the same pattern: more or less identical distributions centered around relatively moderate, popular websites. On the left, we see that treatment (gray lines) appears to boost the overall volume of site visits but does not measurably alter the ideological flavor of people's media choices. And in the right panel, there appear to be few differences in the site traffic of Democrats and Republicans within the treatment group. Again there is a slight bump on the right, corresponding to the Drudge Report, but the picture is not one of overwhelming ideological segregation. (The right panel is comparable to Figure 2, which shows similar site visit patterns in the Pulse data.)

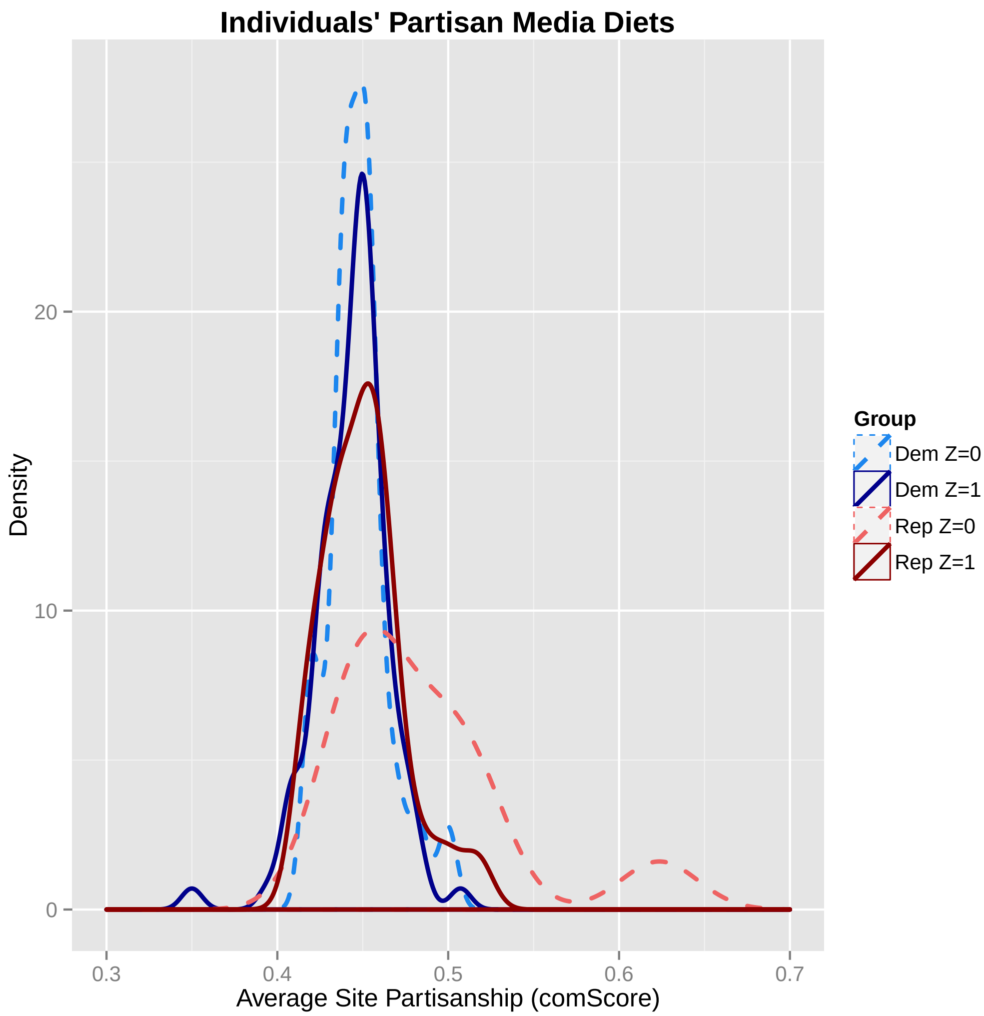


Figure 5: Density plot of individuals' media diet partisanship, as measured by the mean comScore partisan slant of all sites visited by each respondent post-treatment. Lines average over party and treatment subgroups. Leaners are coded as partisans.

6 Does Exposure Change Attitudes or Knowledge?

Until now, I have examined the effects of the treatment on participants' media habits. In the previous section, I described how participants can be induced to alter their media diet—in particular, Republicans and conservatives select somewhat more *liberal* sources on average, reducing the conservative lean of their information environment and producing moderation overall. Beyond the immediate effect on exposure to specific content, however, an important question is whether any downstream shifts in attitudes, opinions, or knowledge can be identified.

Table 5 summarizes the results, combining responses from the second survey wave with the orig-

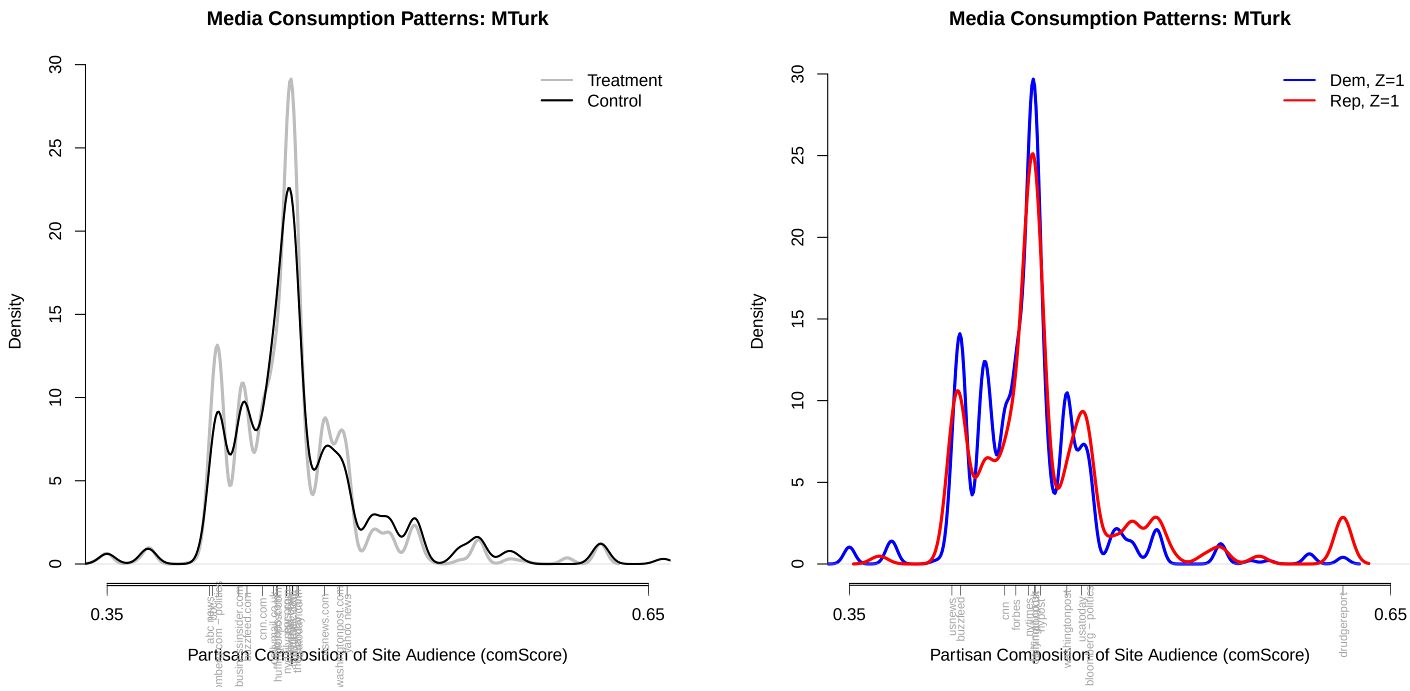


Figure 6: Left, comparison of treatment (gray) and control (black) site exposure densities. Right, comparison of Democrats and Republicans within the treatment group (including leaners).

inal treatment assignment and pre-treatment covariates. It is immediately evident that there are no identifiable effects on views about for-profit colleges[4] or how to regulate them[5]; the estimated coefficients on the treatment vector are small and highly variable. By contrast, pre-treatment views are highly correlated with post-treatment responses. In the third and fourth columns, however, a more complicated picture emerges for the effects of treatment on knowledge. First, from Column 3 we see that a main effect is not evident. Column 4 shows evidence of heterogeneity by party affiliation, however: Being assigned to receive the encouragement message appears to have had no effect on Democrats ($-0.37 - 0.34 + 0.70 = -0.01$) but actually *reduced* knowledge about for-profit colleges among Republicans ($-0.37 - 0.79 + 1.01 = -0.15$).

These findings are clearly against expectations. Given the limited power of the experiment and the possibility that conditional average treatment effects of this kind may be due to chance, I hesitate to speculate about the significance of these results. Notably, these are intent-to-treat estimates focusing

[4]This is a 7-point scale from "For-profit colleges are extremely bad for American education" to "For-profit colleges are extremely good for American education."

[5]This was a 5-point scale from "The government should regulate for-profit colleges much more strongly" to "The government should regulate for-profit colleges much less strongly," with more regulation coded as positive.

Table 5

	Dependent variable:			
	View	Regulate	Knowledge	
	(1)	(2)	(3)	(4)
Pre-treatment View	0.74^{***}			
	(0.03)			
Pre-treatment Regulate		0.61^{***}		
		(0.03)		
Assigned Treatment	−0.25	0.05	0.08	−0.37
	(0.21)	(0.13)	(0.17)	(0.26)
Incognito	0.06	0.09	−0.08	−0.16
	(0.47)	(0.29)	(0.59)	(0.58)
Chrome	0.25	0.13	0.09	0.17
	(0.21)	(0.13)	(0.27)	(0.26)
Democrat	−0.06	0.10		−0.34
	(0.26)	(0.16)		(0.32)
Republican	0.27	−0.13		-0.79^{*}
	(0.34)	(0.21)		(0.41)
Income				0.08^{**}
				(0.03)
Education				0.08
				(0.05)
Treatment x Dem	0.24	−0.11		0.70^{*}
	(0.29)	(0.18)		(0.36)
Treatment x Rep	0.11	−0.03		1.01^{**}
	(0.39)	(0.24)		(0.47)
Constant	0.61^{**}	1.53^{***}	3.77^{***}	3.35^{***}
	(0.29)	(0.20)	(0.29)	(0.40)
Observations	436	436	436	436
Adjusted R^2	0.55	0.49	−0.01	0.03
Note:	*p<0.1; **p<0.05; ***p<0.01			
	OLS, standard errors in parentheses.			

on the effect of being assigned to treatment rather than the effect of *complying* with the treatment—that is, seeking out information about for-profit colleges if and only if encouraged to do so.

6.1 Attitude Change: Puzzling Additional Results

Last year, I implemented a pilot version of the same study on MTurk, with minor differences (N=348).[6] I include the results here to highlight the fact that the findings are not consistent: While I found null effects on opinions above, I found strong effects previously (but none for knowledge). Clearly, more research is needed.

Table 6 shows the results of regressions of post-treatment attitudes on treatment assignment, pre-treatment views, partisanship, and interactions. Despite the fact that the treatment did not specify the valence of content to look up, it seemed to have an overall negative effect on subjects' views toward for-profits (possibly as a result of negative press in recent years). Column 1 shows the main effect, a modest 0.38-point decrease along the 7-point attitude scale. Column 2 shows no average difference in views between Democrats or Republicans. Column 3 includes interactions between treatment and the party indicators, which suggest that essentially all of the change in views is driven by Democrats becoming less supportive. Republicans are also estimated to move in the same direction, but this is not significant (likely because the sample had far fewer Republican-leaning subjects). The last three columns show that the heterogeneous effects worked for partisan identification but not ideological self-placement.

I show essentially the same results, but for the dependent variable measuring subjects' views about the regulation of for-profit colleges and universities, in Appendix D. In short, the treatment had a positive effect on subjects' belief in regulating the schools.

In this pilot study, I also tested for effects on knowledge, measured in the post-treatment but not pre-treatment survey, and salience. I found mixed to null results for both, although the estimates for knowledge came close to statistical significance. It is possible that improving the efficiency of the estimates with pretreatment covariates would have allowed me to identify significant effects on knowledge, although I specifically sought to avoid priming subjects with knowledge questions before the treatment was administered.

[6]Out of 1,000 initial respondents, 348 both agreed to install the browser widget and successfully did so. From this experimental sample, I then block randomized subjects into either treatment and control. Sample characteristics: 61% male; 79% white, 6% black, 7.5% Hispanic; 47% college-educated; 40.5% Democrat, 14% Republican; mean age, 32.

Table 6: Views toward for-profit colleges, earlier study.

	DV: View (T2)				
	(1)	(2)	(3)	(4)	(5)
Pre-treatment View	0.68***	0.66***	0.68***	0.65***	0.65***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Assigned Treatment	−0.38***	−0.37***	−0.01	−0.43***	−0.19
	(0.12)	(0.12)	(0.17)	(0.12)	(0.23)
Democrat		−0.21	0.15		
		(0.13)	(0.19)		
Republican		0.07	0.35		
		(0.18)	(0.26)		
Treatment x Dem			−0.69***		
			(0.26)		
Treatment x Rep			−0.59		
			(0.37)		
Liberal				−0.33**	−0.18
				(0.15)	(0.22)
Conservative				−0.15	0.14
				(0.19)	(0.26)
Treatment x Lib					−0.25
					(0.29)
Treatment x Con					−0.60
					(0.37)
Constant	1.33***	1.45***	1.22***	1.67***	1.53***
	(0.15)	(0.17)	(0.18)	(0.21)	(0.24)
Observations	318	318	318	304	304
Adjusted R^2	0.51	0.51	0.52	0.51	0.51

Note: *p<0.1; **p<0.05; ***p<0.01
Weighted regressions (to take into account block randomization), robust standard errors in parentheses.

One possible reason for the discrepancy in findings is a change in the way I recorded the primary dependent variables in the second survey wave. In the pilot study, I simply asked subjects who returned for the second wave their opinions on for-profit colleges and how to regulate them, along with a set of knowledge questions. The attitude and policy opinion questions were worded identically to the ones I asked in the pre-treatment survey. It is possible that treated respondents remembered the previous questions and sought to give the "correct" (more critical) answers given the tenor of the coverage they may have exposed themselves to. In the later study, by contrast, I included the same questions in the second wave—but, as in the pre-treatment survey, I grouped them in with questions on unrelated subjects in an attempt to mask the goal of the study. Thus, it is possible that the earlier findings were essentially the result of demand effects (see, e.g., Green, Calfano and Aronow 2014).

7 Discussion

In this two-part investigation, I show that—at least among respondents in an opt-in sample—there is no strong evidence that individuals cluster into ideologically segregated information cocoons. Aggregated over more than 100,000 visits, the ideological flavor of most content respondents exposed themselves to is generally centrist. Rather than the strong bimodal prediction made by theorists who express concerns about online echo chambers, the most-visited media sources heavily cluster in the middle. One possible caveat to this conclusion is suggestive evidence of a smaller group of conservatives who consume primarily right-leaning content, but this would have to be replicated with either a more representative sample or with properly weighted data.[7]

I couple this observational portrait of panelists' balanced media diets with an experimental design that likewise does not reveal strong evidence of ideological segregation. In particular, Figure 6 shows how aggregate site visits cluster around the center (with another small bump on the right corresponding to Drudge). Randomly inducing participants to educate themselves about a novel, non-salient political issue does not induce patterns of partisan selective exposure; if anything, there is evidence of a heterogeneous effect in which Republicans expose themselves to more liberal content, resulting in overall moderation of media diets.

Regardless of the patterns of exposure I document, evidence of media effects remains elusive. In

[7]In the version of the figure produced using the partisan, rather than the ideological, measure of media slant (shown in Appendix C), there is also a bump on the center-left corresponding to Democrats visiting Talking Points Memo.

the main experimental study, I find no measurable effects of being encouraged to seek out information about for-profit colleges on attitudes about the issue or opinions about how to regulate them. There is only suggestive evidence of possible differential effects on knowledge, although it is difficult to interpret. In the pilot study, I found positive effects on attitudes and opinions, but the dependent variable in wave 2 was collected in a fashion that may have encouraged demand effects for subjects in the treatment group.

It is possible that a different choice of issue might have resulted in different findings. For instance, an issue that is more salient, better maps onto the partisan divide, or is more likely to trigger "hot cognition" may have induced participants to seek out reinforcing information (Redlawsk 2002; Lodge and Taber 2005). Issues of this type may generate a different pattern of results than those reported here. I note that while further tests should be undertaken, the current evidence of moderation on a non-salient, potentially cross-cutting policy issue directly addresses the greatest concerns of the informational cocoon theorists. For if even novel information about underpoliticized issues reverberate in mutually exclusive echo chambers, then the prospects for deliberation and consensus are especially dire.

As a result of treatment, subjects likely relied on the kinds of low-cost defaults that typically direct readers to mainstream and centrist (rather than niche and extreme) sources: search engines, homepages, and bookmarks. The results suggest that subjects who sought out information about a complex, non-salient political topic were not generally following a directional motivation or seeking out only congenial content (Kruglanski 1999). Or, at least, that motivation was not strong enough to overcome the cost of overcoming ingrained media consumption habits and the defaults built into the typical Internet user's daily browsing environment. Perhaps, then, rather than facilitating invisible "filter bubbles," defaults can actually serve as a moderating filter for new information (Pariser 2011).

Aside from the particular mechanism of the exposure findings, there are multiple potential explanations for the lack of observed media effects. One possibility, as mentioned before, is that some sort of demand effect was at play in which respondents in the pilot study's treatment group sought to give the "preferred" answer to the post-treatment questions about for-profit colleges and how to regulate them. In essence, subjects who remembered having been asked to research the issue may have connected that task to the survey items. This would have been less of an issue with the main study, which embedded the primary questions of interest in a larger survey with unrelated items.

Still, if the findings from the study represent the "true" effect, then the question of why effects are muted or nonexistent remains. The era of "minimal effects" in which mass-media messages were thought to filter through community intermediaries and social groups may not resemble 21st-century America (Klapper 1960; Putnam 1995). But on the other hand, people are in some ways more networked than ever—at least online. Has the nature of the intermediaries simply changed? Another possible explanation is that some form of "transactive memory" is in play, in which people effectively "outsource" their knowledge to the Internet. Psychologists have argued, in essence, that a form of mental division of labor can take place within couples and among groups (Wegner, Giuliano and Hertel 1985; Hollingshead 1998; Kozlowski and Ilgen 2006), and recent research has applied this insight to the modern-day reality of ubiquitous access to reference sources online (Fisher, Goddu and Keil 2015). Given these findings, it seems plausible that subjects who complied with the encouragement treatment simply did not retain any new information (as measured by the knowledge items)—even if they felt they had learned about the topic of for-profit education. A final possibility is that there were effects, but that they were simply too fleeting to be captured in the follow-up survey, which for some subjects was several days after receiving the last encouragement message (e.g., Gerber et al. 2011).

As scholars continue to design studies of information dynamics and media choice, especially in online environments, these findings will hopefully reinforce the importance of taking into account the difficult-to-measure contextual factors that guide politically relevant information-seeking behavior.

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Appendix A: List of Sites

abcnews.go.com
america.aljazeera.com
digbysblog.blogspot.com
dish.andrewsullivan.com
krugman.blogs.nytimes.com
latino.foxnews.com
nbcpolitics.nbcnews.com
news.google.com
news.msn.com
news.yahoo.com
newswatch.nationalgeographic.com
about.com
aljazeera.com
americanthinker.com
anncoulter.com
antiwar.com
aol.com
ap.org
bbc.com
bbc.com
beforeitsnews.com
billoreilly.com
blackamericaweb.com
bloomberg.com
breitbart.com
businessinsider.com
buzzfeed.com
buzzya.com
c-span.org
cbsnews.com
cbsnews.com
chacha.com
chicagosuntimes.com
chicagotribune.com
cnn.com
cnn.com
commondreams.org
counterpunch.org
crookedtimber.org
crooksandliars.com
csmonitor.com
dailycaller.com
dailydot.com
dailykos.com
dailymail.co.uk
democratichub.com
democraticunderground.com
denverpost.com
dickmorris.com
drudgereport.com
economist.com
eschatonblog.com
examiner.com
factcheck.org

firsttoknow.com
fivethirtyeight.com
forbes.com
foreignaffairs.com
foreignpolicy.com
foxnews.com
foxnews.com
foxnewsinsider.com
freebeacon.com
freepatriot.org
freerepublic.com
frontpagemag.com
gopusa.com
govexec.com
govtrack.us
hotair.com
huffingtonpost.com
huffingtonpost.com
humanevents.com
instapundit.com
inthesetimes.com
latimes.com
littlegreenfootballs.com
mediaite.com
michellemalkin.com
mikehuckabee.com
motherjones.com
mrc.org
mrconservative.com
mtvu.com
nationalgeographic.com
nationaljournal.com
nationalmemo.com
nationalreview.com
nbcnews.com
newrepublic.com
newsbusters.org
newser.com
newsmax.com
newyorker.com
npr.org
nydailynews.com
nypost.com
nytimes.com
obamacarefacts.com
outsidethebeltway.com
pbs.org
pbs.org
politicalwire.com
politico.com
politicususa.com
politifact.com
powerlineblog.com
reagancoalition.com
realclearpolitics.com
realclearworld.com

reason.com
reddit.com
redstate.com
rightwingnews.com
rushlimbaugh.com
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thedailybeast.com
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themonkeycage.org
thenation.com
theweek.com
thinkprogress.org
townhall.com
trueactivist.com
truthout.org
twitchy.com
upi.com
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Appendix B: Measuring Sites' Political Slant

Table 7: List of online news sources and the corresponding partisan slant index, computed by dividing the percentage of the site's audience identifying as Republican in comScore's Plan Metrix panel by the percentage identifying with either party.

Site	% Rep (out of R+D)	Site	% Rep (out of R+D)
shortlist.com	0.230	nytimes.com	0.449
scotsman.com	0.235	yahoo news	0.451
smithsonianmag.com	0.257	dailymail.co.uk	0.453
lifenews.com	0.307	huffingtonpost.com	0.453
abcactionnews.com	0.337	dallasnews.com	0.454
lifesitenews.com	0.344	therightscoop.com	0.455
dw.de	0.362	nydailynews.com	0.455
mediaite.com	0.373	upi.com	0.456
talkingpointsmemo.com	0.374	nypost.com	0.456
baynews9.com	0.375	cbsnews.com	0.456
rawstory.com	0.378	abovetopsecret.com	0.459
philly.com	0.379	wsj.com	0.460
knoxnews.com	0.379	newsnet5.com	0.461
metro.co.uk	0.395	nbcnews.com	0.461
bostonglobe.com	0.395	ifyouonlynews.com	0.465
chron.com	0.400	mirror.co.uk	0.466
alternet.org	0.402	salon.com	0.467
cnsnews.com	0.403	express.co.uk	0.468
thesun.co.uk	0.403	wn.com	0.468
mentalfloss.com	0.403	aljazeera.com	0.470
usnews.com	0.407	washingtonpost.com	0.471
worldtruth.tv	0.407	huffingtonpost.ca	0.474
newsok.com	0.408	latimes.com	0.476
theatlantic.com	0.409	vox.com	0.476
addictinginfo.org	0.411	usatoday.com	0.479
buzzfeed.com	0.412	beforeitsnews.com	0.480
motherjones.com	0.413	daily kos	0.483
slate.com	0.414	bloomberg.com - politics	0.483
sfgate.com	0.414	newsnow.co.uk	0.484
9news.com	0.419	detroitnews.com	0.491
theguardian.com	0.423	theblaze.com	0.496
takepart.com	0.424	abc7news.com	0.499
news.google.com	0.424	freep.com	0.503
theweek.com	0.425	medium.com	0.503
chicagotribune.com	0.426	breitbart.com	0.507
npr.org	0.428	ynetnews.com	0.510
startribune.com	0.429	mercurynews.com	0.511
examiner.com	0.430	ijreview.com	0.516
cbc.ca	0.430	newsmax.com	0.519
time.com	0.432	foxnews.com	0.521
telegraph.co.uk	0.432	nymag.com	0.523
newsobserver.com	0.433	foreignpolicy.com	0.525
csmonitor.com	0.436	nzherald.co.nz	0.526
cnn.com	0.436	newsbusters.org	0.529
buffalonews.com	0.437	dailystar.co.uk	0.541
hawaiinewsnow.com	0.437	nationalreview.com	0.545
thedailybeast.com	0.442	mynews13.com	0.556
nj.com	0.443	politico.com	0.556
abc news	0.444	newsiosity.com	0.566
iflscience.com	0.444	townhall.com	0.571
vice.com	0.444	redflagnews.com	0.574
bbc	0.445	rightwingnews.com	0.577
msn news	0.447	daytondailynews.com	0.599
newyorker.com	0.447	news-leader.com	0.609
today.com	0.449	drudgereport.com	0.624
newsday.com	0.449	kingworldnews.com	0.718

Table 8: The ideological slant index is computed by dividing the % of the site's audience identifying as "very" or "somewhat" conservative by the % placing themselves anywhere.

Site	% Cons	Site	% Cons	Site	% Cons
scotsman.com	0.095	news-journalonline.com	0.306	cbsnews.com	0.348
duluthnewstribune.com	0.103	mediaite.com	0.307	pbs newshour	0.349
montgomerynews.com	0.119	whydontyoutrythis.com	0.307	nj.com	0.349
msnewsnow.com	0.162	news-leader.com	0.308	bloomberg.com - politics	0.350
national geographic	0.162	takepart.com	0.309	freep.com	0.350
dailyrecord.co.uk	0.168	onenewsnow.com	0.309	independent.ie	0.351
buzzle.com	0.181	yournewswire.com	0.311	washingtonpost.com	0.351
starnewsonline.com	0.189	theatlantic.com	0.311	usatoday.com	0.356
polar.com	0.193	usnews.com	0.313	medium.com	0.356
mansfieldnewsjournal.com	0.193	huffingtonpost.ca	0.315	wn.com	0.356
guardianlv.com	0.195	miamiherald.com	0.316	newsbusters.org	0.356
smithsonianmag.com	0.200	wvmetronews.com	0.316	newyorker.com	0.356
thenational.ae	0.203	mirror.co.uk	0.316	trueactivist.com	0.357
newstimes.com	0.208	euronews.com	0.317	newszoom.com	0.357
newstatesman.com	0.212	daytondailynews.com	0.317	breakingisraelnews.com	0.360
wsws.org	0.213	today.com	0.317	winknews.com	0.360
newsnow.co.uk	0.214	hawaiinewsnow.com	0.318	metro.co.uk	0.361
japantimes.co.jp	0.217	startribune.com	0.318	nypost.com	0.362
pasadenastarnews.com	0.219	cnn.com	0.319	wsj.com	0.362
manchestereveningnews.co.uk	0.221	toprightnews.com	0.319	jpost.com	0.363
bringmethenews.com	0.221	commondreams.org	0.320	israelnationalnews.com	0.365
irishtimes.com	0.223	theweek.com	0.322	ijreview.com	0.366
shortlist.com	0.227	sfgate.com	0.323	politico.com	0.369
expressnews.com	0.229	vice.com	0.323	americannews.com	0.370
theaustralian.com.au	0.230	telegraph.co.uk	0.324	dailynews.com	0.371
stuff.co.nz	0.235	about.com	0.324	foxnews.com	0.372
huffingtonpost.co.uk	0.235	npr.org	0.325	vox.com	0.376
newsone.com	0.237	bbc	0.326	theglobeandmail.com	0.379
thesun.co.uk	0.237	talkingpointsmemo.com	0.326	therightscoop.com	0.384
presstv.ir	0.242	newspapers.com	0.326	knoxnews.com	0.385
lifenews.com	0.243	cnsnews.com	0.326	news-record.com	0.385
minutemennews.com	0.247	nationalinterest.org	0.327	newsday.com	0.392
vancouversun.com	0.247	aljazeera.com	0.328	lifesitenews.com	0.392
enterprisenews.com	0.247	examiner.com	0.328	newsmax.com	0.393
collective-evolution.com	0.254	countercurrentnews.com	0.328	bangordailynews.com	0.396
sputniknews.com	0.254	alarabiya.net	0.329	redflagnews.com	0.399
addictinginfo.org	0.258	dailymail.co.uk	0.330	breakingnews.com	0.402
catholicnewsagency.com	0.260	voanews.com	0.331	theepochtimes.com	0.404
news.com.au	0.262	salon.com	0.331	breitbart.com	0.404
motherjones.com	0.270	news.google.com	0.331	newsiosity.com	0.411
newsok.com	0.273	daily kos	0.331	rightwingnews.com	0.416
globalpost.com	0.276	nytimes.com	0.332	mercurynews.com	0.419
newser.com	0.277	nydailynews.com	0.332	thedailybeast.com	0.425
nymag.com	0.277	nbcnews.com	0.333	nzherald.co.nz	0.427
cbc.ca	0.280	theguardian.com	0.334	express.co.uk	0.430
deseretnews.com	0.281	worldtruth.tv	0.335	gulfnews.com	0.431
rt.com	0.283	newsherald.com	0.335	charismanews.com	0.436
slate.com	0.285	onenewspage.com	0.335	news-gazette.com	0.438
buzzfeed.com	0.285	globalnews.ca	0.336	beforeitsnews.com	0.450
newrepublic.com	0.285	themoscowtimes.com	0.336	townhall.com	0.450
smh.com.au	0.285	csmonitor.com	0.337	standard.co.uk	0.450
rawstory.com	0.286	huffingtonpost.com	0.337	newsdaymarketing.net	0.451
abc.net.au	0.287	firstcoastnews.com	0.337	nationalreview.com	0.451
abovetopsecret.com	0.288	detroitnews.com	0.338	drudgereport.com	0.454
abs-cbnnews.com	0.288	ifyouonlynews.com	0.338	onlinenewspapers.com	0.454
dw.de	0.291	msn news	0.339	thejournal.ie	0.454
ynetnews.com	0.291	philly.com	0.339	ctvnews.ca	0.457
alternet.org	0.291	dallasnews.com	0.340	kingworldnews.com	0.465
universetoday.com	0.293	latimes.com	0.340	firstpost.com	0.466
mentalfloss.com	0.293	chicagotribune.com	0.340	spiegel.de	0.470
bostonglobe.com	0.293	newsweek.com	0.341	twcnews.com	0.473
heraldsun.com.au	0.295	upi.com	0.341	dailystar.co.uk	0.474
chron.com	0.299	yahoo news	0.341	lemonde.fr	0.479
iflscience.com	0.300	buffalonews.com	0.342	digitaljournal.com	0.497
time.com	0.303	independent.co.uk	0.345	newsminer.com	0.532
dailydot.com	0.303	madworldnews.com	0.346	erietvnews.com	0.579
jamaica-gleaner.com	0.305	foreignpolicy.com	0.347	marinij.com	0.588
newsobserver.com	0.305	theblaze.com	0.348	news-press.com	0.650
topix.com	0.305				

Appendix C: The Online Political Media Diet, Partisan Slant

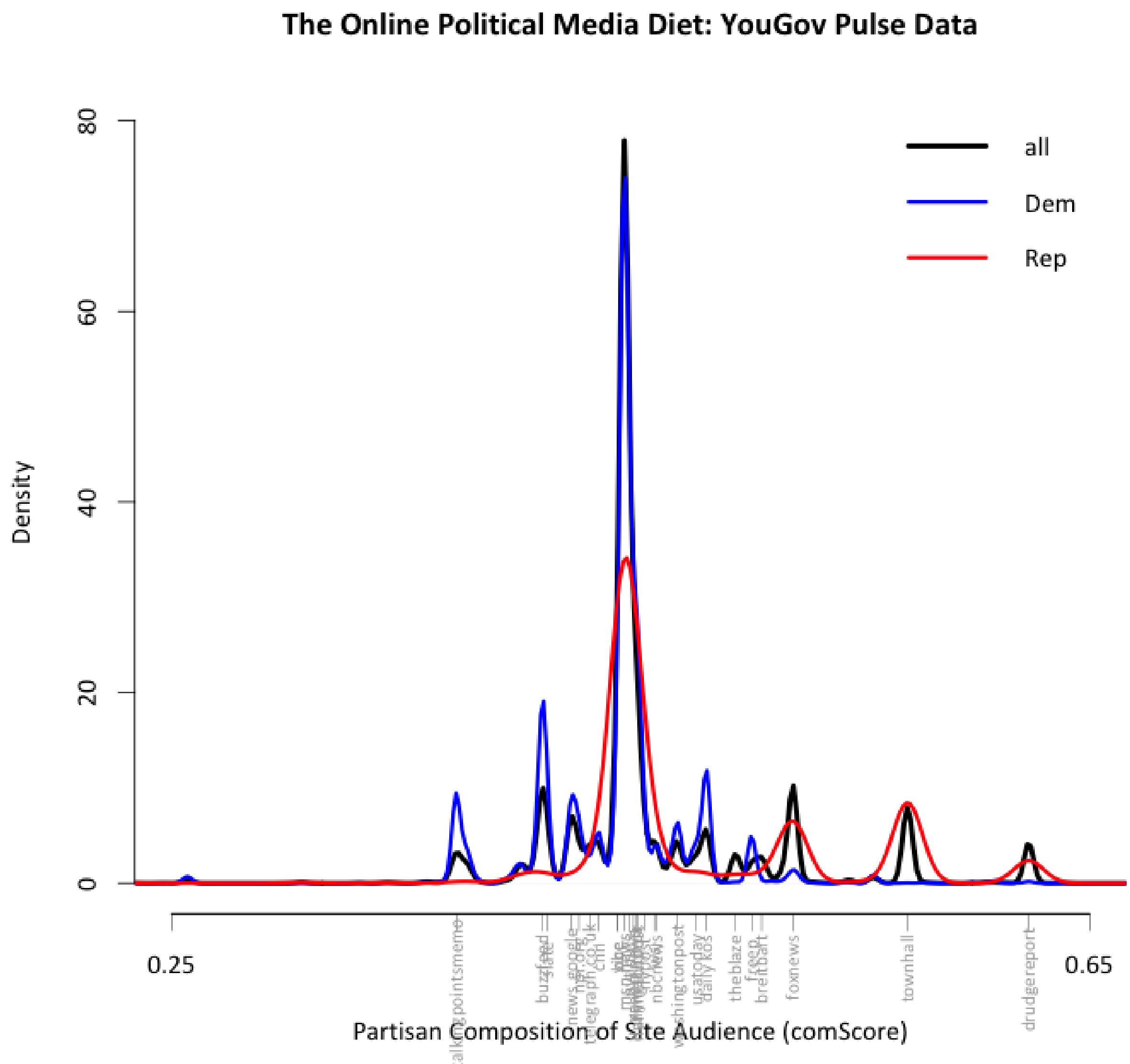

Figure 7: Density plot of aggregate site visits from the YouGov Pulse sample. Site partisan slant on the x-axis is measured using comScore data on audience composition (see Appendix B). N = 102,134 visits.

Appendix D: Additional Figures and Tables

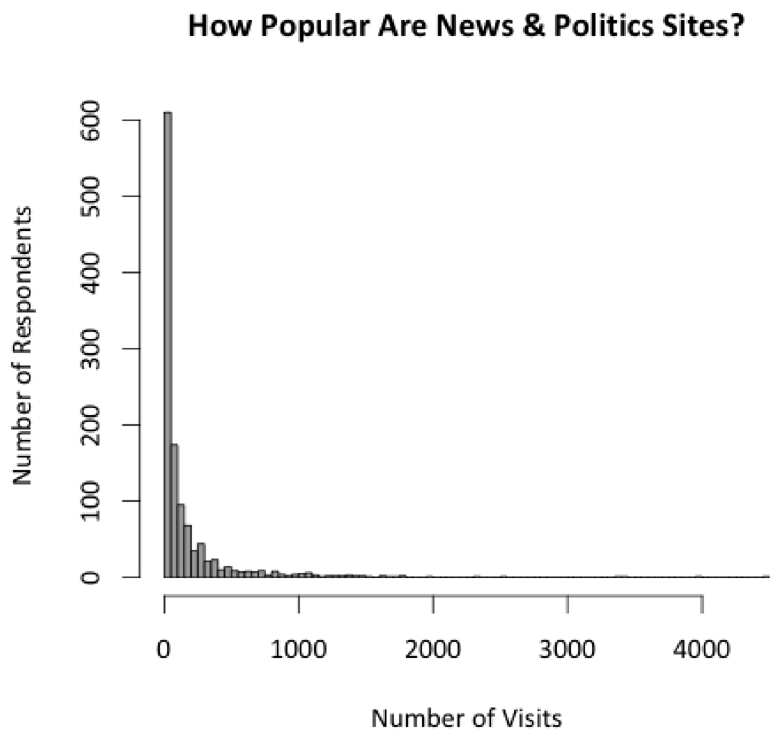

Figure 8: As this figure shows, most respondents in the YouGov Pulse panel visited no or very few political news sources during the three-week period the data was collected. Barely visible on the right are a tiny proportion of panelists who logged thousands of hits to political sites during that period.

Table 9: Opinions about regulating for-profit colleges, earlier study.

	DV: Regulate (T2)				
	(1)	(2)	(3)	(4)	(5)
Regulate (T1)	0.63***	0.60***	0.60***	0.59***	0.59***
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Z	0.17**	0.17**	0.30***	0.15**	−0.07
	(0.08)	(0.08)	(0.11)	(0.08)	(0.13)
D		0.11	0.26**		
		(0.08)	(0.12)		
R		−0.15	−0.08		
		(0.14)	(0.17)		
D:Z			−0.29*		
			(0.15)		
R:Z			−0.12		
			(0.29)		
Lib				0.02	−0.13
				(0.09)	(0.13)
Con				−0.17	−0.40**
				(0.14)	(0.19)
Lib:Z					0.27*
					(0.16)
Con:Z					0.43
					(0.27)
Constant	1.41***	1.48***	1.41***	1.60***	1.74***
	(0.23)	(0.24)	(0.25)	(0.27)	(0.27)
Observations	318	318	318	304	304
Adjusted R^2	0.51	0.51	0.52	0.49	0.50

Note: *p<0.1; **p<0.05; ***p<0.01
Weighted regressions, robust standard errors in parentheses.