

Raising the Floor or Closing the Gap? How Media Choice and Media Content Impact Political Knowledge

Thomas J. Leeper*

July 4, 2018

Abstract

Mass media are frequently cited as having the potential to inform the public, raising knowledge levels and reducing political knowledge gaps between citizens. But media are also seen as a force for segmentation, disengagement, and widening differences between citizens. If media have no effect on political knowledge, gaps between the engaged and disengaged persist regardless of who is exposed to news because no one learns. But gaps can also persist even if everyone learns from the news, particularly if learning effects are heterogeneous across those inclined and disinclined to seek out news and/or across environments that consist of different media alternatives. Yet past research on political communication has not sufficiently linked media choice to debates about possibly heterogeneous effects of media exposure on political knowledge levels. The present study contributes a novel and large-scale choice-based experiment on knowledge of the ongoing crisis in Syria that finds media effects are relatively homogeneous across those with different media preferences and across different media environments. This suggests that under most conditions — even when everyone learns from the news — knowledge gaps between the politically engaged and disengaged are widened or at least sustained after incidental exposure to politics. While closing such gaps may be impossible, the results have important implications for understanding how citizens learn about politics and how to study learning from self-selected media experiences.

*Department of Government, London School of Economics and Political Science. The author graciously acknowledges support from NSF Doctoral Dissertation Improvement Grant SES-1160156. This paper also reports survey results found through searches of the iPoll databank and archives of the Roper Center for Public Opinion Research. Data, replication code, and experimental materials will be available on the Harvard Dataverse: <https://dataverse.harvard.edu/dataverse/leeper>. Thanks are due to Kevin Arceneaux, Jason Barabas, Matt Baum, Hajo Boomgaarden, Jamie Druckman, Frederik Hjorth, Shanto Iyengar, Jenn Jerit, Matt Levendusky, Paul Marx, Rasmus Tue Pedersen, Markus Prior, and Josh Robison, as well as seminar participants at Aarhus University, the London School of Economics, McGill University, Louisiana State University, Pompeu Fabra University, the European University Institute, and the reviewers and editor at BJPS for helpful feedback on various stages of this project. This paper was previously presented at the 2013 Annual Meeting of the American Political Science Association, Chicago, IL, the 2014 Annual Meeting of the Danish Political Science Association, Vejle, Denmark, and the 2015 Annual Meeting of the Midwest Political Science Association Annual Meeting.

The question of what — if anything — citizens know about politics has provoked one of the most central and longest-running debates in the history of political science. That citizens seem to know little creates tensions between researchers’ understanding of democratic theory and the empirical realities of contemporary citizenship (see Althaus 2006; Converse 1964; Berelson 1952; Neuman 1986; Delli Carpini and Keeter 1997). Determining whether citizens know enough about politics, however, necessarily begs the question, inviting circular logics and normative debates about information sufficiency and the definition of competence (Lupia 2015). A more fruitful path tries to answer an empirical question: “How do citizens become knowledgeable about politics?” One major factor has been consistently highlighted in the extant literature: exposure to political information via mass media (see, for a review, Barabas et al. 2014).¹ And while education has been shown to increase “civics” knowledge, policy-specific knowledge of current affairs issues is thought to be much more dependent on media use.

In these debates, considerable attention is paid to absolute levels of political knowledge; that is, how much citizens know about particular political topics and the extent to which knowledge varies across groups in society. If knowledge levels are low — and knowledge is seen as politically important — then understanding how to increase absolute levels of political knowledge is clearly important. A related but distinct question is whether variation in political knowledge levels appears to constitute “knowledge gaps.” The politically engaged appear to know much more about politics than those who are less educated or who are politically disengaged. These gaps matter if they influence citizens’ opinion formation, vote choices, or other political behaviour (see Tichenor, Donohue, and Olien 1970). Some research

¹Other factors, such as education, interpersonal discussion, feedback from policy and participation, and the influence of social media, have also gained attention but education and mass media are understood to be major factors causing citizens to learn about politics. See, variously, Albertson and Lawrence (2009); Delli Carpini and Keeter (1997); Barabas and Jerit (2009); Jerit, Barabas, and Bolsen (2006); Tichenor, Donohue, and Olien (1970); Gaziano (1983); Chaffee, Zhao, and Leshner (1994); Delli Carpini, Keeter, and Kennamer (1994); Jennings (1996); Nie, Junn, and Stehlik-Barry (1996); Prior (2003); Eveland, Marton, and Seo (2004); Prior (2005).

suggests knowledge gaps are widened over time and over the course of political debates due to selective (in)attention to political news with those already knowledgeable becoming more so and others learning less. In this view, knowledge gaps are persistently exacerbated by further media exposure. But other research on “incidental” or “byproduct” media exposure suggests that knowledge gaps are easily lessened or even eliminated by casual exposure to political content in the course of a largely apolitical news diet. However, this literature is characterized by substantial inferential challenges in disentangling selection biases from the causal influence of media.

Previous attempts to disentangle this have used media content analysis to provide insight into the information environment survey respondents might experience (Jerit, Barabas, and Bolsen 2006; Barabas and Jerit 2009). While this technique offers a substantial improvement over mere correlations of self-reported media exposure and political knowledge, they depend on ecological variations in media coverage rather than precisely measured individual-level variation in news use. Triangulation of previous observational results using alternative methods therefore has significant value for our collective understanding of media effects. Given the limitations associated with extant survey and experimental approaches for assessing the influence of media, there is an understandable defeatism epitomized by Bartels’ (1993) famous quip that “[t]he state of research on media effects is one of the most notable embarrassments of modern social science” (267). Yet research on knowledge gaps (and media effects more generally) is too important to be abandoned in the face of empirical difficulties.

This paper suggests that studying media influence on knowledge and other outcomes requires theory and methods that accommodates potentially heterogeneous effects of media across citizens with preferences for different media content. While other sources of heterogeneity are also important, heterogeneity due to differences in media preferences is frequently highlighted as a potential driver of echo chambers and political disengagement (for a review see Prior 2013). Heterogeneity in effects across media preferences is also particularly given that the degree to which choices can be *realized* depends on the content on offer from the me-

dia environment. Heterogeneity in learning across other sources of variation (e.g., education, age, labor market status) do not necessarily feature the same kind of complex interaction between individual characteristics, the media landscape, and outcomes.

To assess such heterogeneity, the paper presents a choice-focused paradigm for studying mass media influence. A novel “preference trial” experiment, which observes and manipulates media choices and media content, enables a clear assessment of the average effect of media exposure on political knowledge levels as well as potentially heterogeneous effects between those who are inclined and disinclined to seek out political news and between different types of media environments. More specifically, the design uses participants’ behaviour to reveal their content preferences and, conditional on those preferences and the set of choices available to participants, exposes any heterogeneity in the effects of exposure to news content. Rather than heterogeneous effects, however, this study finds individuals inclined and disinclined to seek out news learn from it the same amount. Comparisons of various counterfactual conditions further demonstrate that these uniform gains in policy-specific knowledge sustain and even widen gaps between those inclined and disinclined to seek out political content. Indeed, there are no realistic conditions where gaps are overcome by media exposure, raising questions about whether closing such gaps is possible or even a normatively appropriate standard. Ultimately, the findings have important implications for understanding political knowledge, media effects, selective exposure, and the study of media use.

Media Effects on Political Knowledge

Political knowledge is widely seen as an important resource for citizens because it shapes opinions, decision-making, and behavior (Delli Carpini and Keeter 1997; Barabas et al. 2014). It is also fairly uncontroversial to claim that mass media influence citizens’ political knowledge (at least knowledge of “surveillance” facts; Barabas et al. 2014). Yet the evidence for the informational value of mass media suffers from theoretical and empirical limitations.

Theoretically, this literature tends to attribute variations in knowledge to either (a) attention to different types of media (i.e., “effects” of television versus newspapers), or (b) quantitative variations in the amount of respondents’ exposure to media (i.e., hours of television viewing).² While potentially theoretically compelling, both approaches are substantially limited by available data. Self-reported media use is typically the only available operationalization of media exposure and, as such, the key independent variable in analyses of knowledge is subject to various response biases and the substantial measurement error (see, for example, Prior 2009*b,a*, 2013; Dilliplane, Goldman, and Mutz 2012).

Practical limitations aside, the theoretical focus on media types and quantitative measures of exposure tends to downplay the importance of the actual information the received media convey to their audiences. Given sizeable variations in informational content across media, channels, sources, and articles, theorizing media effects in terms of the quantity of one’s exposure — or exposure to “types” of media — is fundamentally limiting. This approach in the study of political knowledge is all the more surprising given that research into the effects of media on other outcomes — such as opinions, beliefs, issue importance, behaviors, etc. — has been successfully studied through a theoretical lens attentive to content variations in tandem with an experimental paradigm that enables clear causal inferences about the influence of media messages (see, variously, Ansolabehere et al. 1994; Arceneaux and Johnson 2012; Baumgartner, Linn, and Boydston 2010; Berinsky and Kinder 2006; Brewer 2003; Brewer and Gross 2005; Chong and Druckman 2007; Druckman 2001, 2004, 2005; Druckman and Parkin 2005; Druckman, Fein, and Leeper 2012; Iyengar, Peters, and Kinder 1982; Iyengar and Kinder 1987; Levendusky 2013*b*; McCombs and Shaw 1972; Miller and Krosnick 2000; Mutz 2007; Nelson, Clawson, and Oxley 1997; Niederdeppe, Gollust, and Barry 2014; Petty and Cacioppo 1986; Schuldt, Konrath, and Schwarz 2011; Tichenor, Donohue, and Olien 1970). This enormous body of evidence demonstrates unequivocally

²For example, headlines frequently point out differences in knowledge between viewers of different channels and programs, but such differences are obviously causally uninterpretable given selection bias.

that content differences matter for a whole host of individual outcomes but knowledge is rarely studied in this way. Research explaining variation in political knowledge can therefore benefit from the theoretical and empirical perspectives offered by the broader media effects literature, where theory focuses on what content is received by different individuals.

That theoretical focus on the effects of content-specific variations similarly invites the use of the same *experimental* rather than *observational* paradigm adopted in the broader media effects literature. A review of studies of political knowledge literature, however, reveals a substantial reliance on cross-sectional observational “causes of effects” research designs that involve the regression of a knowledge scale on a set of possible explanatory variables drawn from a single survey (see, variously, Baum 2002, 2003*b,a*; Brians and Wattenberg 1996; Chaffee and Frank 1996; Chaffee, Zhao, and Leshner 1994; Delli Carpini and Keeter 1997; Donohue, Tichenor, and Olien 1975; Eveland, Marton, and Seo 2004; Eveland and Scheufele 2000; Eveland, Seo, and Marton 2002; Fraile and Iyengar 2014; Gaziano 1983; Genova and Greenberg 1979; Grönlund and Milner 2006; Holbrook 2002; Jerit, Barabas, and Bolsen 2006; Kull, Ramsay, and Lewis 2003; Kwak 1999; Liu and Eveland 2005; Prior 2003, 2005, 2007; Moore 1987; Tichenor, Donohue, and Olien 1970; Valentino, Hutchings, and Williams 2004; de Vreese and Boomgaarden 2006).³ The limitations of this approach for providing causal inference are now well-known (Samii 2016; Gelman and Imbens 2013; Manski 1999; Morgan and Winship 2015). As Gelman and Imbens (2013) write: “[a] reverse causal question does not in general have a well-defined answer, even in a setting where all possible data are made available” (6). Selection biases, measurement error in the regressors, and the possibility of a reverse causality all limit the utility of this approach. Another body of literature relies on panel designs across the course of political campaigns, but these approaches retain the

³There are a few experimental studies (e.g., Norris and Sanders 2003). And there is, of course, an abundant methodological literature on the proper measurement of knowledge on surveys (Mondak 1999; Mondak and Davis 2001; Barabas 2002; Luskin and Bullock 2005; Prior and Lupia 2008; Sturgis, Allum, and Smith 2008; Miller and Orr 2008; Prior 2014; Shulman and Boster 2014), but this is rarely aimed at understanding the causes of knowledge.

limitations of classical measurement error (for example, Eveland et al. 2005; Dimitrova et al. 2014; McCann and Lawson 2006; Hansen and Pedersen 2014). As such, we know less about the relationship between media exposure and political knowledge than the volume of studies on the topic might suggest. An apparent cross-sectional correlation between media exposure and knowledge might reflect the causal influence of media on knowledge, the influence of knowledge on media use, both, or even neither.

What is even less understood than the average effect of media on knowledge is whether there is heterogeneity in the effects of media exposure on knowledge. Understanding the size and scope of heterogeneity is critical for evaluating how citizens might learn about politics. For example, differences in knowledge levels might exist simply because those who are political engaged learn more from the same media content (i.e., effect heterogeneity) or because they learn the same amount from that content (i.e., effect homogeneity) but just happen to be exposed more often. The former pattern is one of effect heterogeneity, the latter is one characterized only by self-selection. Amounts of exposure and amounts of learning might both play a role in explaining variation in political knowledge, but extant research has been unable to successfully unpack the difference because observational data never characterize citizens' political knowledge under counterfactual conditions where they receive anything other than their preferred content.

Both self-selection into media as well as potentially heterogeneous effects of exposure might contribute to observed variations in political knowledge levels. Individuals might opt-in to media because of stable preferences over media content (Prior 2007), thus creating baseline differences in knowledge among different media audiences *before* they are exposed to any content at all.⁴ Then, those selection-induced gaps are affected by further learning from media to the extent that (1) chosen media provide distinct content and (2) media effects

⁴Notably, the most prominent studies of media influence on knowledge (Baum 2002; Prior 2007) both rely on the paradigmatic cross-sectional regression approach. While those studies engaged a debate about potential effect heterogeneity across those inclined and disinclined to follow the news, neither offered a well-identified research design. As such, they document selection biases or effect, or both, or neither.

may be heterogeneous across individuals with different media preferences. Together, these patterns mean we cannot understand media effects on political knowledge without carefully attending to both selection and potentially heterogeneous effects across those who make distinct media choices within a given media environment.

This argument differs from much existing work. While experimental research documents that citizens are affected by media, such research typically ignores the media selection behavior that is essential for understanding knowledge gaps (but see Druckman, Fein, and Leeper 2012; Leeper 2014; Gaines and Kuklinski 2011*a*). A similar argument has been made about the theory and study of partisan media on voting, issue attitudes, and polarization but with little if any attention to political knowledge (see Levendusky 2013*a*; Arceneaux and Johnson 2012). All of this work has explored the idea put forth by Hovland (1959) that “In an experiment the audience on whom the effects are being evaluated is one which is fully exposed to the communication. On the other hand, in naturalistic situations with which surveys are typically concerned, the outstanding phenomenon is the limitation of the audience to those who expose themselves to the communication” (Hovland 1959, 9). Or, as Bennett and Iyengar (2008) similarly note, “manipulational control actually weakens the ability to generalize to the real world where exposure to politics is typically voluntary” (724). While the present research is in the same vein as these choice-focused theories and methods of studying partisan media effects, the experimental tests diverge from this work and the present research focuses on political knowledge rather than attitudinal outcomes.

I argue that individuals who prefer politics are most likely to engage in media use behaviors that are likely to expose them to political content, while those who prefer other content are likely to engage in media use behaviors that let them avoid political content. The outcomes that result from that exposure therefore depend upon citizens’ media choice behavior, the alternatives available to them in the media environment, and the degree of heterogeneity in effects of political news exposure. When individuals can choose what media content they receive, and effects of media are homogeneous, we should expect one of three

possible consequences:

1. *Choices satisfied*: when media choices determine exposure (i.e., those who prefer politics receive it and those who prefer something else do not), knowledge levels increase for some and not for others, raising aggregate knowledge levels but widening knowledge gaps, and
2. *All news*: when everyone is exposed to political content, regardless of their media content preferences, aggregate knowledge levels *increase* and any existing knowledge gaps *persist*,
3. *Choices ignored*: when individuals who prefer to avoid politics are unintentionally exposed and when political information is withheld from those who prefer such content, aggregate knowledge levels increase and the knowledge gap *narrows*.
4. *All entertainment*: when no one is exposed to political content, no one learns regardless of their preferences or any existing knowledge.

If effects are instead heterogeneous, then the story is more complicated. If those inclined to politics learn more than others, then levels increase in (1-3) but gaps between groups widen even in (1-2). If those inclined to politics instead learn less than others, then rising knowledge levels under any circumstance (1-3) mean rising levels and narrowing gaps between the groups. No previous research has disentangled these complex linkages between media self-selection, the contents of the media environment, and the effects of exposure on learning. To do so requires a research design that both allows for the expression of ecologically realistic media use behaviors but also manipulates the content that individuals receive in order to causally identify the effects of media exposure conditional on that behavior.

A Choice-based Experiment

If theory dictates that the effects of media exposure on political knowledge are linked to media choices, then empirical research needs to be able to observe selection of content *and*

observe effects of exposure to content across those with different choice behaviour, unequivocally distinguishing selection biases from causal effects. While the experimental paradigm of media effects research offers *causal identification*, it says nothing about *selection*. While the observational paradigm of media effects research makes claims about selection, effects, or both, it cannot disentangle them. An increasing number of studies have therefore used choice-based experimental designs to understand the self-selection problem (see Gaines and Kuklinski 2011*b,a*; Levendusky 2012, 2013*a*; Arceneaux, Johnson, and Murphy 2012; Arceneaux and Johnson 2012; Druckman, Fein, and Leeper 2012; Leeper 2014).⁵ Arceneaux and Johnson (2012), for example, have used a selective exposure paradigm to compare expected aggregate outcomes (e.g., issue attitudes) under counterfactuals of forced exposure and media choice. This design tests for the effect of the *choice set* on the outcome of interest without any need to directly measure what choice participants make among those available. That is, “choice” is treated as an experimental condition akin to forced exposure.

A harder problem to solve, however, relates to determining the effect a given media treatment has *conditional on having made a particular choice*. In other words, given an individual expresses a choice behaviorally, what is the effect of the treatment they prefer versus disprefer to receive? This is the problem of choice-related heterogeneity, which extant research largely leaves unresolved. As an exception, Levendusky (2013*a*) applied a design wherein participants were asked for their preference between Fox News, MSNBC, and PBS alternatives and then performs subgroup analysis of a randomized experiment across these self-identified groups of respondents. Respondents are therefore differentiated based upon a self-report measure and their preferences were ultimately not respected in the experiment.

The present research adopts a related approach borrowing from the same methodological literature on *patient preference trials* (Sidani et al. 2009; Burke et al. 2008; Clark et al. 2008; Floyd and Moyer 2010). In this design, individuals make choices among alternative

⁵Another line of research uses the “dynamic process-tracing environment” (see, for example, Lau and Redlawsk 2006) to examine outcomes after a stream of choices rather than counterfactuals between different exposures resulting from a single instance of choice.

treatments from within a finite choice set. Subsequently, individuals are randomly assigned to a particular treatment. For example, a patient chooses between surgical and non-surgical therapies, but is randomly assigned to their actual treatment. Conditional on choice, the randomized treatments expose the necessary counterfactual outcomes for inferring the effects of each treatment among individuals making different choices. The observation of behaviorally expressed choice additionally means that aggregate outcome distributions can also be inferred across different counterfactual realities (e.g., a reality where everyone receives their preferred content versus a reality where everyone regardless of preferences is exposed to political information). The design is the perfect framework for studying media influence on knowledge because it captures and disentangles both theoretically important elements of media effects: self-selection into content (e.g., news channels or stories) and the effects of content on outcomes.⁶

Experimental Design and Procedures

To test the effects of media choices and media content on political knowledge, I implemented patient preference trial using a large sample of online participants in the autumn of 2012. The design follows directly from the theorized relationships between media choices, media effects, and knowledge gaps. At the beginning of the experiment, participants were asked some general questions about their media use and then participated in the patient preference trial before finally answering factual questions about an ongoing political issue. Participants were told that the investigators were interested in peoples' reactions to some articles, which

⁶Whereas previous efforts to understand incidental exposure to political information have used surveys to measure self-reported exposure to the types of programs that might contain incidental political content (e.g., late-night satirical news or soft news programs; Brewer and Cao 2006; Parkin 2009; Baek and Wojcieszak 2009; Kim and Vishak 2008; Xenos and Becker 2009; Morris and Francia 2009), the patient preference trial clearly disentangles selection from effects. Gaines and Kuklinski (2011*b*) and Leeper (2017) use this design to assess effects of a short textual vignette on opinions for those inclined and disinclined to select the vignette but (a) focus on opinion outcomes rather than knowledge, and (b) use experimental stimuli that — like Levendusky (2013*a*) — that do not respect the respondent's content preference. The present design attempts to mitigate the risks of that approach.

the survey would ask about after they finished reading them.

The experiment then involved three manipulations. The first manipulation involves whether individuals were randomly assigned to a set of news stories or whether they had the opportunity to choose what set of articles to read. This manipulation thus allows for a comparison of traditional “captive exposure” experiments to results of the preference trial, to ensure that the act of expressing a preference over alternative content did not substantially modify subjects’ behaviour (an exclusion restriction).⁷ Those in the randomized conditions were simply instructed they would read some news articles and then were shown either four political news articles or four entertainment articles in sequence. The second manipulation altered the set of content alternatives available to the participants in the choice-based (preference trial) conditions and the third manipulation altered what issue-relevant content participants received. I discuss each of these manipulations in turn. Figure 1 shows the experimental design, conditions and sample sizes.

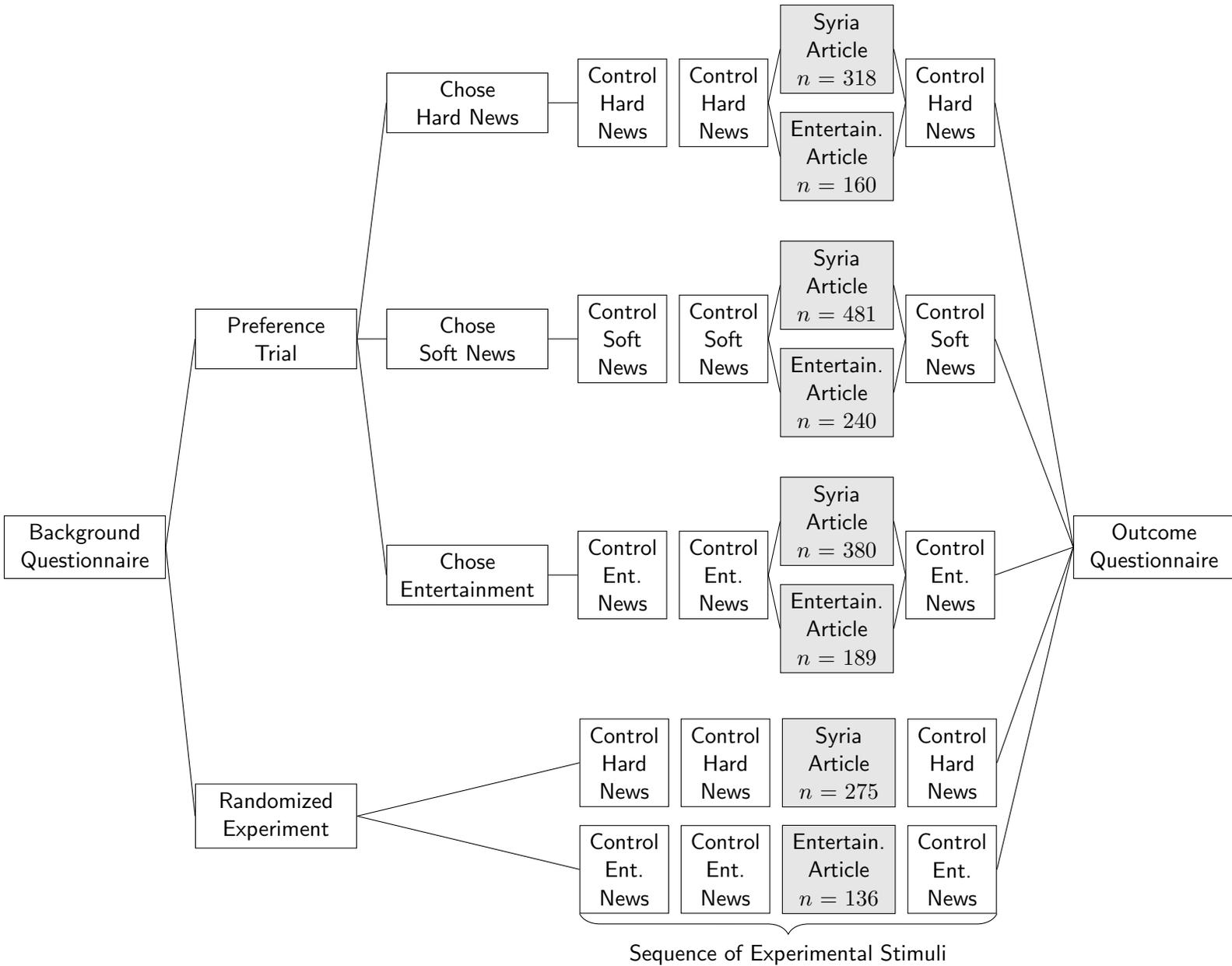
The second manipulation involved what set of alternatives each preference trial participant received in their media “choice set.” Participants were able to pick what kinds of articles they wanted to read from a set of two or three alternative described as:

- “Political and national government news”
- “Human interest stories and lifestyle news”
- “Celebrity and entertainment news”

These alternatives correspond roughly to “hard,” “soft,” and “entertainment” news. Some participants were offered all three alternatives, while the remainder were offered pairs of

⁷One concern is that assignment to the preference trial arm of the study had a direct effect on the outcomes. This does not appear to have been the case as the outcome (issue knowledge) did not differ between those receiving the news content in the preference trial ($\bar{x} = 0.49$) compared to those in captive conditions ($\bar{x} = 0.49$, $t = 0.22$, $p \leq 0.83$), nor between those in the preference trial ($\bar{x} = 0.32$) and random assignment ($\bar{x} = 0.33$) conditions who were ultimately assigned to entertainment content ($t = 0.29$, $p \leq 0.78$). Given that the assignment to the preference trial and random assignments arms of the experiment appeared inconsequential, the exclusion restriction does not appear to have been violated.

Figure 1: Experimental Design, Sequence of Stimuli, and Treatment Group Sizes



Note: Differences in sample sizes within the randomized experiment and within each “arm” of the preference trial reflect random assignment. Differences in sample sizes across the arms of the preference trial reflect sample segmentation from participants’ initial choices.

alternatives (i.e., hard and entertainment; soft and entertainment; hard and soft).⁸ This manipulation is doubly useful. First, it allows a test related of whether the influence of media depends on the particular choices alternative (to foreshadow: it does not).⁹ Second, it allows for a construct validity test by which it is possible to compare participants' *behaviourally expressed* preferences in the trial to their *stated* interests in different types of news (as stated on the initial, pretreatment questionnaire).

Among the pretreatment media use questions was a battery of items measuring participants' preferences for different types of media content. The battery of items asked: "Now, I'm going to read you a list of different types of news. Please tell me how closely you follow this type of news either in the newspaper, on television, on radio, or on the internet. . . very closely, somewhat closely, not very closely, or not at all closely?" Responses were provided for thirteen different news content categories on a four-point scale, coded to range from 0 to 1, with higher scores indicating higher preference for each type of news. Consistent with previous work (see Prior 2005), the analysis focuses on a simple differential item ranging from -1 to 1 that is constructed by subtracting each individual's stated entertainment preference from their stated preference for national political news.¹⁰

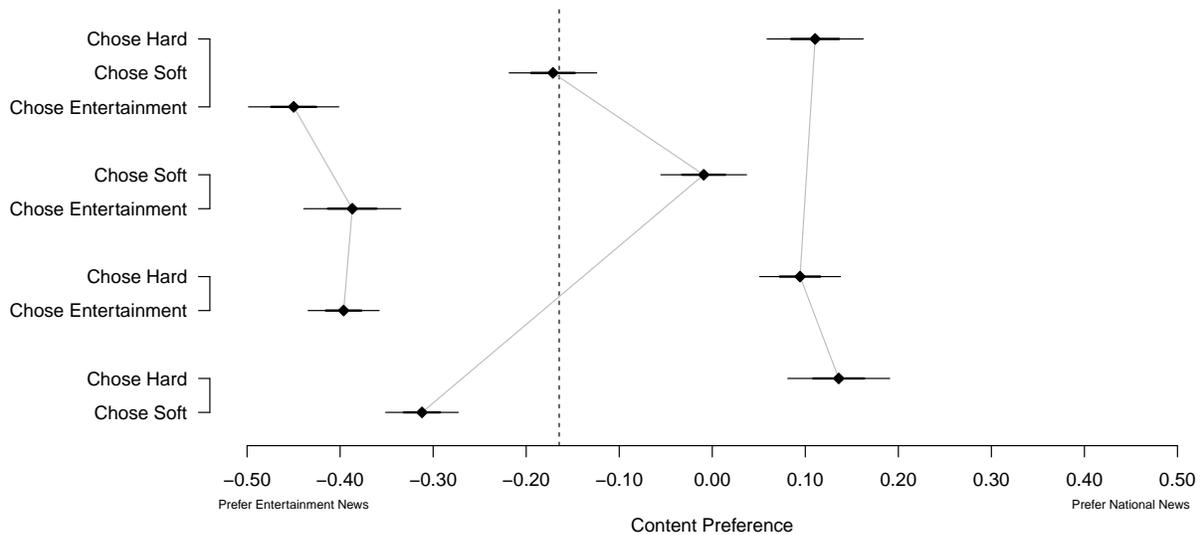
Figure 2 displays mean news–entertainment preference differentials for those selecting each of the three news categories within each choice set. As the top three bars of the figure make clear, preferences vary considerably across those choosing each of the three different types of news. Those who strongly prefer national politics to entertainment (according to

⁸Though these choicesets are somewhat stylized, they are meant to represent the ecological conditions that individuals face in a dynamic high–choice landscape and capture institutional variations in the availability of news and entertainment (Prior 2007; Aalberg, van Aelst, and Curran 2010). Future research might consider alternative choicesets.

⁹Supplemental Appendix C further shows that the randomization into different choicesets appeared to have no direct effect on any outcome variable.

¹⁰Supplemental Appendix D includes additional descriptive information about these items drawn from the experiment and from Pew Research Center surveys, including details of the items' macro-level stability over the period 1989 to 2012, correlation tables and exploratory factor analyses, and an analysis of the robustness of the items to alternative measurement techniques.

Figure 2: Content Preferences of Respondents, by Choice and Choice Set



Note: Figure displays mean scores on the national-entertainment preference differential, with bars for one and two standard errors, among those making each choice from each of the four choice sets. The dashed vertical line represents the mean score on the preference differential item (-0.16).

their self-reported interests) choose the hard news option, while those who prefer entertainment to national politics choose celebrity news. Those choosing soft news fall somewhere in between. The lower sets of bars show how preferences relate to choices when only two types of news are available. When the choice is between soft and entertainment news, those preferring entertainment are much more likely to choose it while those preferring national political content are absorbed into the soft news audience. In the third set of bars, individuals select very consistently based on their preferences: entertainment is chosen by those preferring it while national politics is chosen by those who prefer it. Finally, in the bottom set of bars, those preferring national politics continue to choose hard news while those preferring entertainment opt into soft news instead.¹¹ These results offer some assurance that

¹¹The audience for soft news varies considerably by what alternatives are available. In the full choicest, the soft news audience is those without strong preferences for either political or entertainment content. In the second set, however, this group has a stronger preference for national political content, while in the final set, the soft news audience prefers entertainment. Constraints on choice push people into alternatives they disavour, while an abundance of choice fragments the audience along lines of content preferences.

participants’ behaviourally revealed preferences reflect their stated preferences, suggesting the experiment has some ecological validity.

The final manipulation randomly assigned participants to read an issue-relevant or a control article. Specifically, participants in every group were presented with four articles in sequence, three of which were consistent with the choice they indicated and one of which (the third article in the sequence) was randomly assigned (again, see Figure 1). The manipulated story addressed either nonpolitical content (the Academy Awards) or provided respondents with information about an important ongoing political issue at the time of the study: conflict in Syria. In every condition, the three choice-consistent stories were all unrelated to the study’s outcome of interest (knowledge of Syria). Participants in the “captive” randomized experiment were randomly assigned to one of two streams of content: (1) news, where the third article was issue-relevant, or (2) four entertainment stories. For those in the preference trial conditions, two thirds of participants received a randomly assigned article about Syria (e.g., an individual chose “hard news” and received four hard news stories, one of which was about Syria) and the remainder were assigned to receive an entertainment article (e.g., an individual chose “hard news” and received three hard news stories unrelated to Syria and one article about the Academy Awards). This manipulation provides useful counterfactuals that disentangle selection from effects because each subset of the sample (as defined by behaviourally expressed choices in the preference trial) is observed in alternative states of the world where they do or do not receive information about Syria.¹²

All of the news articles used in the study were modified from recent news coverage and

¹²To implement the third experimental manipulation, three treatment articles were created. One that presented the Syria issue by focusing on the politics of rebel groups during the war, one that presented the Syria issue by focusing on the effects of the war on children’s schooling, and one that said nothing about Syria (focusing on the schedule for the Academy Awards). The two Syria stories articles were modified to mention the same five pieces of information that would be assessed in the outcome knowledge battery. Different versions were created to assess the robustness of the results to variations in the particular treatment articles being used. Because the results for the two articles are largely identical, these conditions are combined (and labelled “news”) in the paper. Supplemental Appendix A.3 compares the two different conditions, providing full results.

edited to be 800 to 850 words in length. A key feature of the design is that, unlike the intentionally mundane material used in most political communication experiments, all stories including the non-manipulated “control” articles were intentionally drawn from contemporary news coverage and edited to catch the attention of participants (being in line with their chosen type of news).¹³ Supplemental Appendix E contains some validation checks related to reading times for the articles. Conditional on choice, there were no significant differences in reading times between those who received the news treatment article and entertainment control article.

Issue Context

The Syria issue was a reasonable topic at the time of the study and fortuitous given the implications of the continuing Syrian civil war on international and, especially, European politics. From an experimental design perspective, the issue was useful precisely because it was not a topic of major news coverage or public concern in the United States, where the study was conducted. At the time of data collection, violence in Syria had garnered the close attention of less than half of the American public (see first column of Table 1). According to polling by CNN, as of February, 2012, 25% of the American public indicated believing that the U.S. had a responsibility to intervene in Syria.¹⁴ By May, that number had increased to 33%.¹⁵ Though no directly comparable survey data is available, by August 29% were very concerned and 43% were somewhat concerned about the situation in Syria,¹⁶

¹³The full text of all articles are shown in Supplemental Appendices E and F.

¹⁴Answers based upon the question “Do you think the United States has a responsibility to do something about the fighting in Syria between government forces and anti-government groups, or doesn’t the United States have this responsibility?” from iPoll study USORC.021412B, 2/10–2/13/2012.

¹⁵iPoll study USORC.060612A, 5/29–5/31/2012.

¹⁶Answers based upon the question “In general, how concerned are you about the situation in Syria—very concerned, somewhat concerned, not very concerned, or not concerned at all?” from iPoll study USORC.081512, 8/7–8/8/2012.

and 46% favored U.S. aerial military involvement¹⁷ but only 32% favored U.S. use of ground forces.¹⁸ Thus, while the American public seemed to be increasingly concerned about the situation in Syria, it had not garnered a large share of the public’s attention and less than half of the public was favorable toward U.S. military involvement of any kind. Indeed, as Table 1 shows, most of the American public reported (between January and August 2012) not following news about Syria particularly closely and only a tiny fraction of the public indicated that it was the news story they were following most closely. The issue thus serves as an interesting case for studying levels of political knowledge. Given that the Syrian crisis has since come to the forefront of international media attention, the experiment provides an interesting examination of media effects from *before* the issue received major coverage.

Outcome Measures and Analysis

After reading all four articles, respondents were asked some general questions about their reactions to the content they read (e.g., “were the articles too long, too short, or just the right length?”), then answered questions on their knowledge about Syria.¹⁹ Finally, respondents answered some demographic questions and the study concluded.

Knowledge was measured using five items about Syria and the ongoing conflict, which were scored as correct or incorrect²⁰ and then additively scaled and divided by 5 to form a measure of proportion of correct knowledge questions ranging from 0 to 1. All questions

¹⁷“Would you favor or oppose the US (United States) and other countries using military airplanes and missiles to try to establish zones inside Syria where the opposition forces would be safe from attacks by the Syrian government?”

¹⁸“And would you favor or oppose the US (United States) and other countries using ground troops to try to establish zones inside Syria where the opposition forces would be safe from attacks by the Syrian government?”

¹⁹The study also measured issue attitudes and attitude certainty. Details on these results are included in Supplemental Appendix B.

²⁰“Don’t know” and blank responses were treated as incorrect. Supplemental Appendix A reports, among other things, the mean number of “don’t know” responses to each item. These proportions tended to be high overall, with the mean number of such responses being 1.88.

Table 1: Public Attention to Violence in Syria

Survey Dates	Very Closely	Fairly Closely	Not too closely	Not at all closely	% Following as Top Story
1/12–1/15/2012 ^a	12	17	26	45	3
2/9–2/12/2012 ^b	17	23	23	36	5
2/23–2/26/2012 ^c	18	24	21	35	4
3/8–3/11/2012 ^d	17	23	23	37	6
3/15–3/18/2012 ^e	16	26	27	30	4
4/5–4/8/2012 ^f	15	21	27	37	2
4/12–4/15/2012 ^g	14	23	25	37	3
5/31–6/3/2012 ^h	12	25	25	37	–
6/28–7/1/2012 ⁱ	13	19	26	42	–
7/19–7/22/2012 ^j	17	24	23	36	–
8/16–8/19/2012 ^k	12	24	26	36	–

Note: Survey data from Roper Center iPoll database study numbers: a. Survey USP-SRA.011812NII, n=1,008; b. Survey USPSRA.021412NII, n=1,029; c. Survey USP-SRA.022812NII, n=1,005; d. Survey USPSRA.031512NII, n=1,005; e. Survey USP-SRA.032112NII, n=1,009; f. Survey USPSRA.041012NII, n=1,000; g. Survey USP-SRA.041712NII, n=1,002; h. Survey USPSRA.060612NII, n=1,007; i. Survey USP-SRA.070212NII, n=1,006; j. Survey USPSRA.080112ANII, n=1,001; k. Survey USP-SRA.082712ANII, n=1,005.

Table 2: Issue Knowledge, by Question

	T1	T2
Who is the current president of Syria?	62.8%	61.2%
What is the name of the main rebel group fighting in Syria?	39.2%	42.8%
What is the largest city in Syria?	61.0%	13.5%
Has the United Nations Security Council passed a resolution condemning the violence in Syria?	25.5%	16.9%
What position has the Russian government taken toward the violence in Syria?	37.7%	37.6%
Scale Mean	0.43 (0.33)	0.27 (0.32)
n	2221	2221

were asked at both time 1 and separate scales were created for each time period. Table 2 lists the questions (with correct answers in parentheses) and shows the percentages of all respondents correctly answering each of the knowledge questions and the bottom rows reports mean scores and standard deviations on the overall scale. The choice of these particular political knowledge items can, of course, be criticized. The goal when selecting items was to obtain a concise battery of surveillance knowledge items of varying “difficulty.” Assuaging some concerns, the reported results are also robust to alternative specifications of the knowledge outcome, including item-specific analyses of each knowledge question, analysis of “don’t know” responses, and an item-response theory (IRT) specification of the analysis (see Supplemental Appendix A).

Given randomization throughout the design, the analysis is quite straightforward and involves simple comparisons of participants’ mean levels of knowledge in each experimental condition. Treatment effects are defined and estimated as the mean-difference in knowledge between those randomly assigned to the Syria news story and those randomly assigned to the control article about the Academy Awards.²¹ After examining this difference in the aggregate, the results section compares that effect within the subgroups defined by the manipulated choice sets and participants’ behaviourally revealed media preferences. Effect heterogeneity across choice contexts and subgroups of those inclined and disinclined to seek out news media is assessed through a comparison of this simple mean-difference for each subgroup.

Participants

Participants in the study were recruited from Amazon Mechanical Turk and paid \$0.75 for their participation. A total of 2,221 participants took part in the study, which took about 15

²¹An alternative analytic approach would be to use estimators defined by Gaines and Kuklinski (2011*b*). Results using this approach are included in Supplemental Appendix A and are substantively and statistically identical to those reported in the body of the paper.

minutes to complete. Implementation took place between October 4 and October 15, 2012.²² While some have raised concerns about the validity of MTurk, an increasingly large body of evidence suggests the relatively diverse set of participants recruited from the platform (Berinsky, Huber, and Lenz 2012; Huff and Tingley 2015) experience causal dynamics comparable to other convenience and representative samples (Mullinix et al. 2015; Krupnikov and Levine 2014). Though in no way representative of the U.S. population, respondents constituted a diverse sample: 60.4% were female; 78.5% were white, 7.1% were African American, 6.2% were Asian American, 5.0% were Hispanic; 48% had college degrees; median age range was 25-34; 35.6% were Democrats and 38.0% were Republicans; 49.6% identified as liberal. Respondents expressed moderate levels of interest in politics (mean=0.6, sd=0.3, on a 0-1 scale). The cost of the platform also enables a much larger sample size (and associated statistical power) than would otherwise be feasible. As in any research, the use of a convenience sample — like the choice of a focal issue and research setting — may of course limit the generalizability of the results across settings, persons, issues, or outcomes.

²²Participants were also invited to participate in a follow-up wave of interviewing (time 2) three weeks after completing the first wave with an eye toward understanding the durability of learning over-time. Respondents were recontacted using MTurkR (Leeper 2012, 2013) via an email with the subject line “Complete 5-minute follow-up survey for \$.25.” and body that read as follows: “Thanks for completing my survey a few weeks ago. Complete a 5-minute follow-up survey (20 questions) to earn a \$.25 bonus. You can complete the survey the link below: {LinkToStudy} Thanks so much for your participation! Bonuses will be paid in a few days.” The recontact rate for the second wave was 62%, with all responses gathered between October 25 and October 30, 2012. Despite some attrition by time 2, demographics for those participating in both panel waves were similar to the overall sample: 61.7% were female; 79.5% were white, 6.0% were African American, 6.7% were Asian American, 4.7% were Hispanic; 53.0% had college degrees; the median age range was 25-34; 36.5% were Democrats and 39.8% were Republicans; 50.6% identified as liberal; and levels of general political and interest were identical to the those for the whole sample. The key finding for the over-time results is that those who were assigned to entertainment conditions learned about the issue between time 1 and time 2 (or cheated on their answers in the time 2 responses), while those assigned to news conditions retained their knowledge over time. Full results for time 2 are reported in the Supplemental Appendix.

Results

Receiving mediated information about Syria increased knowledge about the issue. Scores on the knowledge scale were compared across respondent groups that received the news and entertainment treatments. Treatment group means were 0.49 (SE=0.01) for the news condition and 0.32 (0.03) for the entertainment condition, a difference that indicates a sizable and statistically significant ($p = 0.00$) gain in issue-relevant political knowledge among those exposed to political news. Receiving mediated information about Syria clearly increases knowledge.

But how are knowledge levels (and the gaps between them) affected by the interaction between media effects and media choices? Table 3 shows mean levels of issue knowledge among individuals assigned to the news article or the entertainment article, separately for those choosing entertainment, soft news, or hard news during the preference trial, and in the captive conditions. The mean knowledge levels among those assigned to the entertainment article represent baseline knowledge levels among these segments of the sample given that they were exposed to no issue-relevant information during the study. As should be clear, those choosing entertainment or soft news knew the least about the issue as a baseline, with those in the true control group (chose entertainment, received entertainment) correctly answering just over one question about the issue (mean knowledge of 0.24, SE=0.02).

Similarly, as a baseline, those choosing to read hard news (but without having received the issue-relevant article) knew substantially more (mean knowledge of 0.46, SE=0.03). A knowledge gap is revealed by individuals' selective exposure to different categories of news. Recall that individuals in the experiment were given a choice between *general* categories of news content to read but were assessed on *issue-specific* knowledge, so these differences should be understood as a preference-related knowledge gap rather than a self-selection bias wherein those more knowledgeable about Syria specifically opted to read news about Syria.

When looking to the values representing exposure to the news story, we see that all individuals learn from news exposure. Even after receiving the Syria story, however, those

Table 3: Issue Knowledge Level by Choices and Randomized Treatment

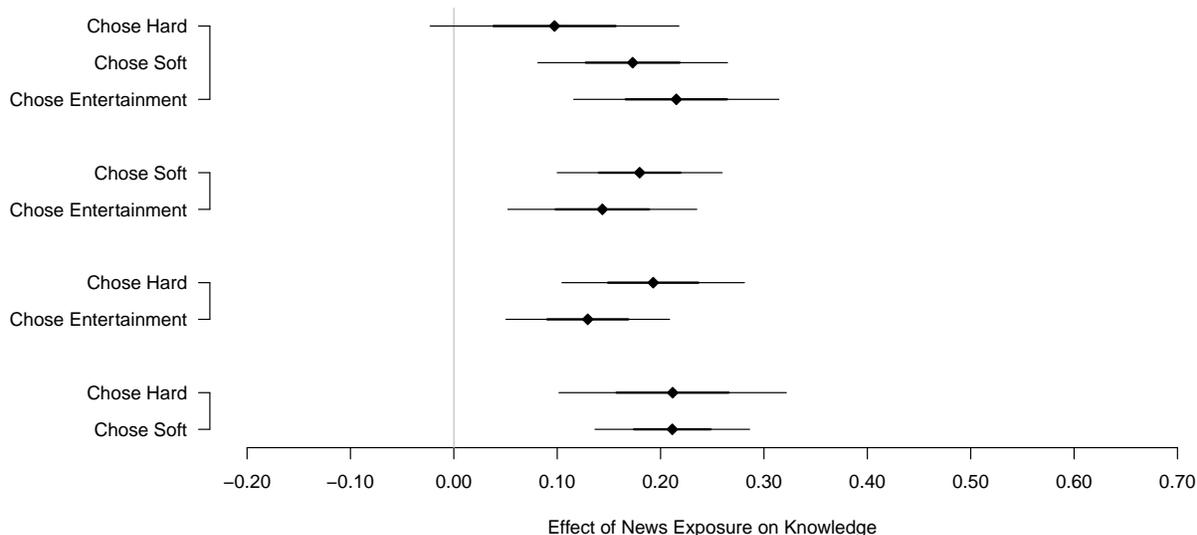
Choice	Treatment	n	Knowledge
Hard	News	318	0.63 (0.02)
	Entertainment	160	0.46 (0.03)
Soft	News	481	0.47 (0.01)
	Entertainment	240	0.28 (0.02)
Entertainment	News	380	0.39 (0.02)
	Entertainment	189	0.24 (0.02)
Captive	News	275	0.49 (0.02)
	Entertainment	136	0.32 (0.03)

Note: Cell entries are treatment group means levels of knowledge, with standard errors in parentheses. Variation in group sizes between the “Choice” alternatives reflect self-selection; variation in group sizes within the “Choice” alternatives reflect the combination combination of two conditions into the “News” treatment. The Supplemental Appendices provide complete analyses separating these conditions.

choosing entertainment know the least, those choosing soft news know somewhat more, and those choosing hard news know the most. If everyone were forcibly (albeit incidentally) exposed to news about Syria, the pre-exposure political knowledge gap would remain firmly intact. Despite statistically significant gains in knowledge levels among all three groups (those choosing hard, soft, and entertainment content), the gap between those preferring news and avoiding it remains and those who already knew the most (the hard news choosers) remain by far the most knowledgeable.

Importantly, these effects of exposure on levels of knowledge are uniform across different choice sets and different media choices. Figure 2 shows treatment effects (the mean-difference in knowledge between “news” exposure and “entertainment” exposure) conditioned on choice set and choice. In nearly every condition, exposure to the Syria story increases knowledge, with the exception of those choosing hard news from a three-item choicest set (though this effect is in a direction consistent with the other conditions). Rather than the opportunity for media choice segmenting those who are engaged and capable of learning from those who are disengaged and learn less, it appears that anyone can learn (and learn roughly the same

Figure 3: Treatment Effects on Knowledge Levels, by Choice and Choice Set



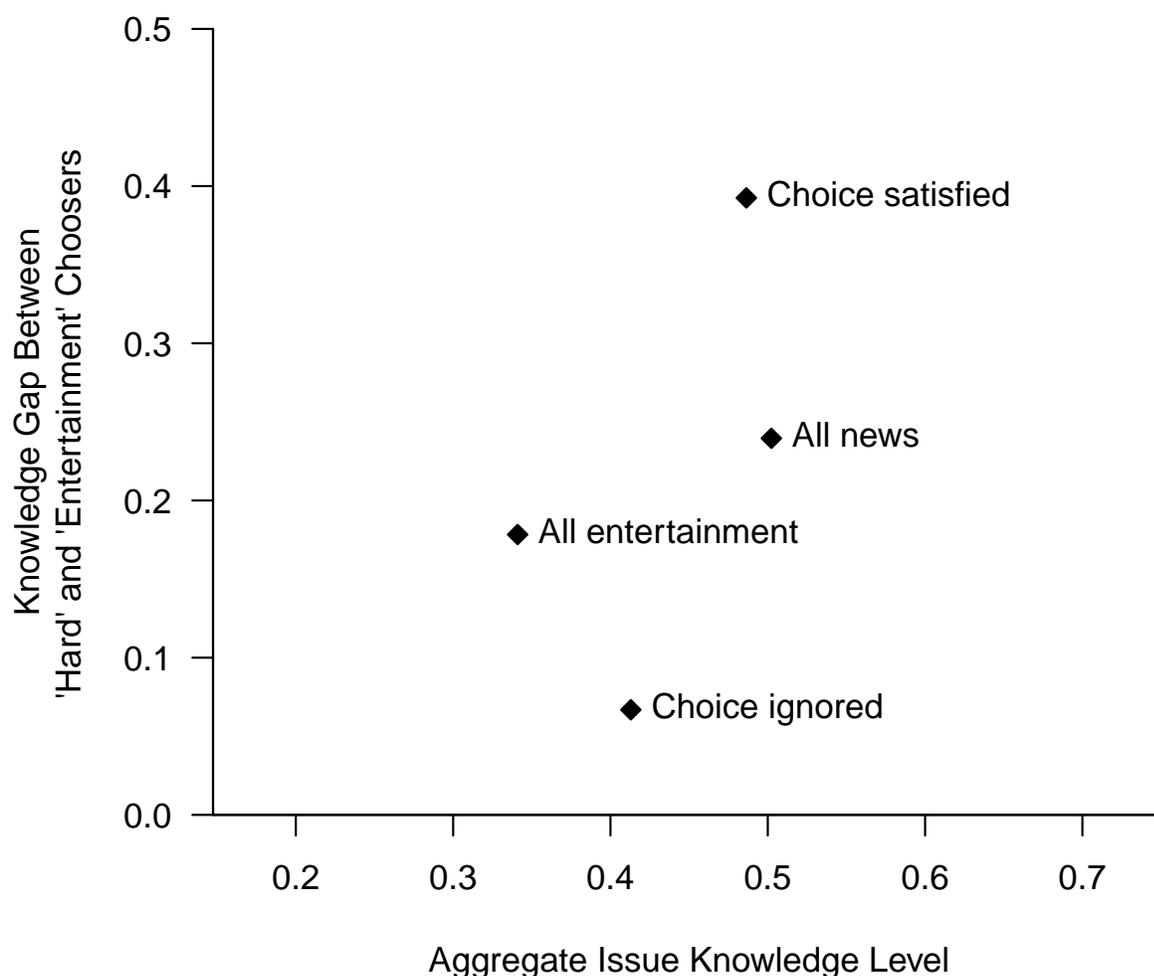
Note: Figure displays average treatment effects of news exposure in each self-selected choice group (separately for each choice set), with bars for one and two standard errors.

amount) if given at least an incidental exposure to news.²³

But how should we interpret these results? One way is to consider a trade-off between satisfying citizens’ media preferences (by providing them with their preferred content) and the democratic goal of maximizing total political knowledge. To do so, we can examine knowledge levels and the knowledge gap between the politically engaged and disengaged across various counterfactual conditions revealed through the experiment’s interaction of choices and content provision. Figure 4 shows the mean level of knowledge across all individuals in a subset of the sample and the mean-difference in knowledge between those inclined to choose political news versus entertainment within that subsample. In line with the theoretical expectations, four counterfactuals are considered: (a) choices are satisfied (news choosers

²³Noteworthy, however, is that by two to three weeks after exposure, the effects of news also dissipates uniformly across all types of individuals in all conditions because those assigned to the entertainment story showed a slight increase in knowledge over the post-treatment period. See Supplemental Appendix A for full results. This may reflect participants search for information outside of the experiment (see Druckman, Fein, and Leeper 2012), or possibly some cheating behavior on the part of respondents. Unfortunately, it is not possible to test either of these possibilities.

Figure 4: Knowledge Levels and Knowledge Gaps in Several Counterfactuals



Note: Figure summarizes counterfactual conditions extracted from the experimental data wherein individuals do or do not receive their preferred content. The x-axis shows the average knowledge levels in each counterfactual reality and the y-axis shows the mean-difference in knowledge between those inclined to choose political news versus entertainment. Individuals choosing soft news are ignored in these calculations.

get news and entertainment choosers get entertainment), (b) everyone is allocated entertainment, (c) everyone is allocated news, and (d) participants' choices are ignored, with everyone receiving the opposite of their preferred content.

When choices are satisfied, a large gap in knowledge exists because those who choose news and those who prefer entertainment — levels rise for the former and persist for the latter. If citizens receive their preferred content, knowledge gaps (at least immediately) are likely to widen even if some citizens learn. The citizenry in a world where everyone is exposed to news is, unsurprisingly, more knowledgeable on average. Yet the public is on average only slightly more knowledgeable in this condition than when only news choosers receive news. By contrast, when everyone receives entertainment instead of news (regardless of their choices), a knowledge gap persists and knowledge on average is low.

The final counterfactual is worth considering: when choices are ignored — news choosers get entertainment and entertainment choosers get news — the knowledge gap is almost entirely closed. But this narrowed knowledge gap comes at the expense of the average level of knowledge in the group as a whole; the public knows only slightly more on average than if their media choices were satisfied. This is the only condition in which political knowledge gaps are substantially narrowed. Yet such a world is hardly democratically palatable. What is better: a world where political knowledge is equitable and low or a world where it is inequitable and high?

Discussion

The present study shows that the effects of exposure to political news appear to be homogeneous across individuals with different media preferences and across different media choice environments. The experiment therefore demonstrates unequivocally that media exposure can increase knowledge level in the aggregate as well as increase issue-specific knowledge among those inclined and disinclined to receive that content. What then explains variations

in political knowledge levels across those with different media preferences? It appears that self-selection rather than differential effects is the answer. While it could be that both selection and effect heterogeneity explain this gap, it appears only the former is consequential in this case. These results have important implications for our understanding of political knowledge, political communication effects, and the design of experiments.

The study shows that increasing political knowledge through media exposure requires either (a) forced exposure to political news, (b) an inclination on the part of citizens to view political content, or (c) incidental exposure to political news for those who would prefer to see something else also appears to be effective. All are paths to increasing knowledge levels. These processes, however, do not produce equivalent impacts on aggregate knowledge or knowledge gaps. The most likely of these conditions — where individuals receive their preferred content — means modestly high overall knowledge but a widening knowledge gap; the conditions where politics avoiders are incidentally exposed to politics (see also Baum 2002) also mean rising knowledge but a sustained (not narrowed) knowledge gap.

It is only in the unlikeliest of circumstances — where individuals only receive their *dispreferred* content — can political knowledge gaps be narrowed. This is the only way to *equalize* political knowledge across the electorate, yet equalizing knowledge levels in this way comes at the expense of informing those who are already inclined to receive political content. This raises significant normative questions about academic debate surrounding political knowledge. While equalizing knowledge might appear desirable, it is not clear that such a goal is more important than simply increasing aggregate knowledge. Similarly, while forcing everyone to view news would be an effective strategy for raising the floor of low knowledge levels, it would not necessarily close gaps given pre-existing differences in knowledge. This normative tension between these two seemingly related goals merits further consideration.

Speaking to media effect literature more broadly, the present research contributes further evidence that media effects occur as an interaction between individual choices and content provision (Prior 2007). The presence of entertainment alternatives in a media environment

matters only insofar as those entertainment channels only provide entertainment content and self-selection completely segments the public into politics users and politics avoiders. While much past research has been attentive to either the factors that explain different practices of media use *or* to differences in the informational content provided by different outlets, media, and environments, greater attention should be paid to how these factors interact to impact politically relevant outcomes. Consequently, scholars and commentators concerned with media effects should focus much more attention on the information provided by different media systems (see, for example, Aalberg, van Aelst, and Curran 2010). Individual citizens cannot be blamed for following their preferences toward particular content, but the media system can be structured in such a way as to provide useful political information even to those avoiding political news. Similarly, given this interplay between choices and institutions, more attention should be paid to the drivers of selective exposure aside from partisanship, ideology (Stroud 2011; Arceneaux, Johnson, and Murphy 2012; Arceneaux and Johnson 2012; Levendusky 2012, 2013*a*), or entertainment tastes (Prior 2007).

In terms of experimental design, this study highlights the value of studying selection into media and the effects of media together. Captive exposure experiments have provided credible but perhaps unrealistic estimates of media effects without accounting for media selection (see, for reviews, Nelson, Bryner, and Carnahan 2011; Kinder 1998, 2003), survey-based analyses show either selection or effects but cannot disentangle influence from selection bias. Empirical research on media effects must therefore move toward directly observing (and directly manipulating) these media preferences and behaviors in order to avoid the causal ambiguity that plagues the literature. The findings add to a growing body of evidence that “captive” exposure experiments misrepresent political processes of media exposure and opinion dynamics (Gaines and Kuklinski 2011*a,b*; Druckman, Fein, and Leeper 2012; Feldman et al. 2013; Leeper 2014). Future work might apply these methods in other contexts and begin to incorporate other complexities of the current media landscape, like social recommendation (Messing and Westwood 2014) and content provision through choice-tailored

algorithms (Boczkowski and Mitchelstein 2013; Knobloch-Westerwick et al. 2005).

More than sixty years ago, Downs (1957) pointed out that “Society’s free information stream systematically provides some citizens with more politically useful information than it provides others” (221). Some are exposed to politics because they choose it, others because of who they know and where they work encounter politics indirectly, and others live in situations that expose them to politics only infrequently. (Downs 1957, 222–23). Americans at the time of this study knew little about the conflict in Syria, but regardless of their taste for political or entertainment news, it was possible for them to learn a substantial amount about the issue through a relatively brief and perhaps incidental exposure. Yet we still do not know enough about how preferences and choices interact with the media environment and its information provision to produce media effects. Choice-based experiments offer a clear way forward.

References

- Aalberg, Toril, Peter van Aelst, and James Curran. 2010. "Media Systems and the Political Information Environment: A Cross-National Comparison." *Harvard International Journal of Press/Politics* 15(3): 255–271.
- Albertson, Bethany L., and Adria Lawrence. 2009. "After the Credits Roll: The Long-Term Effects of Educational Television on Public Knowledge and Attitudes." *American Politics Research* 37(2): 275–300.
- Althaus, Scott L. 2006. "False Starts, Dead Ends, and New Opportunities in Public Opinion Research." *Critical Review* 18(1): 75–104.
- Ansolabehere, Stephen, Shanto Iyengar, Adam F. Simon, and Nicholas A. Valentino. 1994. "Does Attack Advertising Demobilize the Electorate?" *American Political Science Review* 88(4): 829–838.
- Arceneaux, Kevin, and Martin Johnson. 2012. *Changing Minds or Changing Channels? Media Effects in the Era of Viewer Choice*. Chicago, IL: Chicago University Press.
- Arceneaux, Kevin, Martin Johnson, and Chad Murphy. 2012. "Polarized Political Communication, Oppositional Media Hostility, and Selective Exposure." *The Journal of Politics* 74(01): 174–186.
- Baek, Young Min, and Magdalena E. Wojcieszak. 2009. "Don't Expect Too Much! Learning From Late-Night Comedy and Knowledge Item Difficulty." *Communication Research* 36(6): 783–809.
- Barabas, Jason. 2002. "Another Look at the Measurement of Political Knowledge." *Political Analysis* 10(2): 209–209.
- Barabas, Jason, and Jennifer Jerit. 2009. "Estimating the Causal Effects of Media Coverage on Policy-Specific Knowledge." *American Journal of Political Science* 53(1): 73–89.

- Barabas, Jason, Jennifer Jerit, William Pollock, and Carlisle Rainey. 2014. "The Question(s) of Political Knowledge." *American Political Science Review* 108(4): 840–855.
- Bartels, Larry M. 1993. "Messages Received: The Political Impact of Media Exposure." *American Political Science Review* 87(2): 267–285.
- Baum, Matthew A. 2002. "Sex, Lies, and War: How Soft News Brings Foreign Policy to the Inattentive Public." *American Political Science Review* 96(1): 91–109.
- Baum, Matthew A. 2003a. "Soft News and Political Knowledge: Evidence of Absence or Absence of Evidence?" *Political Communication* 20(2): 173–190.
- Baum, Matthew A. 2003b. *Soft News Goes to War: Public Opinion and American Foreign Policy in the New Media Age*. Princeton, NJ: Princeton University Press.
- Baumgartner, Frank R., Suzanna Linn, and Amber E. Boydston. 2010. "The Decline of the Death Penalty: How Media Framing Changed Capital Punishment in America." In *Winning with Words: The Origins and Impact of Political Framing*. New York, NY: Routledge, 159–184.
- Bennett, W. Lance, and Shanto Iyengar. 2008. "A New Era of Minimal Effects? The Changing Foundations of Political Communication." *Journal of Communication* 58: 707–731.
- Berelson, Bernard R. 1952. "Democratic Theory and Public Opinion." *Public Opinion Quarterly* 16(3): 313–330.
- Berinsky, Adam J., and Donald R. Kinder. 2006. "Making Sense of Issues Through Media Frames: Understanding the Kosovo Crisis." *The Journal of Politics* 68(3): 640–656.
- Berinsky, Adam J., Gregory A. Huber, and Gabriel S. Lenz. 2012. "Evaluating Online Labor Markets for Experimental Research: Amazon.com's Mechanical Turk." *Political Analysis* 20(3): 351–368.

- Boczkowski, Pablo J., and Eugenia Mitchelstein. 2013. *The News Gap: When the Information Preferences of the Media and the Public Diverge*. MIT Press.
- Brewer, Paul R. 2003. "Values, Political Knowledge, and Public Opinion About Gay Rights: A Framing-Based Account." *Public Opinion Quarterly* 67(2): 173–201.
- Brewer, Paul R., and Kimberly Gross. 2005. "Values, Framing, and Citizens' Thoughts about Policy Issues: Effects on Content and Quantity." *Political Psychology* 26(6): 929–948.
- Brewer, Paul R., and Xiaoxia Cao. 2006. "Candidate Appearances on Soft News Shows and Public Knowledge About Primary Campaigns." *Journal of Broadcasting & Electronic Media* 50(1): 18–35.
- Brians, Craig Leonard, and Martin P. Wattenberg. 1996. "Campaign Issue Knowledge and Salience: Comparing Reception from TV Commercials, TV News and Newspapers." *American Journal of Political Science* 40(1): 172–193.
- Burke, L.E., M. Warziski, M.A. Styn, E. Music, A.G. Hudson, and S.M. Sereika. 2008. "A Randomized Clinical Trial of a Standard Versus Vegetarian Diet for Weight Loss: The Impact of Treatment Preference." *International Journal of Obesity* 32(1): 166–176.
- Chaffee, Steven H., and Stacey Frank. 1996. "How Americans Get Political Information: Print Versus Broadcast News." *The Annals of the American Academy of Political and Social Science* 546: 48–58.
- Chaffee, Steven H., Xinshu Zhao, and Glenn Leshner. 1994. "Political Knowledge and the Campaign Media of 1992." *Communication Research* 21(3): 305–324.
- Chong, Dennis, and James N. Druckman. 2007. "Framing Public Opinion in Competitive Democracies." *American Political Science Review* 101(4): 637–655.
- Clark, Noreen M., Nancy K. Janz, Julia A. Dodge, Lori Mosca, Xihong Lin, Qi Long, Roderick J.A. Little, John R.C. Wheeler, Steven Keteyian, and Jersey Liang. 2008. "The

- Effect of Patient Choice of Intervention on Health Outcomes.” *Contemporary Clinical Trials* 29(5): 679–686.
- Converse, Philip E. 1964. “The Nature of Belief Systems in Mass Publics.” In *Ideology and Discontent*, ed. David Apter. New York: Free Press , 206–261.
- de Vreese, Claes H., and Hajo G. Boomgaarden. 2006. “News, Political Knowledge and Participation: The Differential Effects of News Media Exposure on Political Knowledge and Participation.” *Acta Politica* 41(4): 317–341.
- Delli Carpini, Michael X., and Scott Keeter. 1997. *What Americans Know about Politics and Why It Matters*. New Haven, CT: Yale University Press.
- Delli Carpini, Michael X., Scott Keeter, and J. David Kenamer. 1994. “Effects of the News Media Environment on Citizen Knowledge of State Politics and Government.” *Journalism Quarterly* 71(2): 443–56.
- Dilliplane, Susanna, Seth K. Goldman, and Diana C. Mutz. 2012. “Televised Exposure to Politics: New Measures for a Fragmented Media Environment.” *American Journal of Political Science* In press: 32–47.
- Dimitrova, Daniela V., Adam Shehata, Jesper Strömbäck, and Lars W. Nord. 2014. “The Effects of Digital Media on Political Knowledge and Participation in Election Campaigns: Evidence From Panel Data.” *Communication Research* 41(1): 95–118.
- Donohue, G.A., P.J. Tichenor, and C.N. Olien. 1975. “Mass Media and the Knowledge Gap: A Hypothesis Reconsidered.” *Communication Research* 2(1): 3–23.
- Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper & Row.
- Druckman, James N. 2001. “The Implications of Framing Effects for Citizen Competence.” *Political Behavior* 23(3): 225–256.
- Druckman, James N. 2004. “Priming the Vote: Campaign Effects in a U.S. Senate Election.”

- Political Psychology* 25(4): 577–594.
- Druckman, James N. 2005. “Media Matter: How Newspapers and Television News Cover Campaigns and Influence Voters.” *Political Communication* 22(4): 463–481.
- Druckman, James N., and Michael Parkin. 2005. “The Impact of Media Bias: How Editorial Slant Affects Voters.” *The Journal of Politics* 67(04): 1030–1049.
- Druckman, James N., Jordan Fein, and Thomas J. Leeper. 2012. “A Source of Bias in Public Opinion Stability.” *American Political Science Review* 106(2): 430–454.
- Eveland, William P., and Dietram A. Scheufele. 2000. “Connecting News Media Use with Gaps in Knowledge and Participation.” *Political Communication* 17(3): 215–237.
- Eveland, William P., Andrew F. Hayes, Dhavan V. Shah, and Nojin Kwak. 2005. “Understanding the Relationship Between Communication and Political Knowledge: A Model Comparison Approach Using Panel Data.” *Political Communication* 22(4): 423–446.
- Eveland, William P., Krisztina Marton, and Mihye Seo. 2004. “Moving beyond ‘Just the Facts’: The Influence of Online News on the Content and Structure of Public Affairs Knowledge.” *Communication Research* 31(1): 82–108.
- Eveland, William P., Mihye Seo, and Krisztina Marton. 2002. “Learning From the News in Campaign 2000: An Experimental Comparison of TV News, Newspapers, and Online News.” *Media Psychology* 4(4): 353–378.
- Feldman, Lauren, Natalie Jomini Stroud, Bruce Bimber, and Magdalena E. Wojcieszak. 2013. “Assessing Selective Exposure in Experiments: The Implications of Different Methodological Choices.” *Communication Methods and Measures* (August): 37–41.
- Floyd, Anna H L, and Anne Moyer. 2010. “Effects of Participant Preferences in Unblinded Randomized Controlled Trials.” *Journal of Empirical Research on Human Research Ethics* 5(2): 81–93.

- Fraile, Marta, and Shanto Iyengar. 2014. "Not All News Sources Are Equally Informative: A Cross-National Analysis of Political Knowledge in Europe." *The International Journal of Press/Politics* 19(3): 275–294.
- Gaines, Brian J., and James H. Kuklinski. 2011a. "Experimental Estimation of Heterogeneous Treatment Effects Related to Self-Selection." *American Journal of Political Science* 55(3): 724–736.
- Gaines, Brian J., and James H. Kuklinski. 2011b. "Treatment Effects." In *Cambridge Handbook of Experimental Political Science*, eds. James N. Druckman, Donald P. Green, James H. Kuklinski, and Arthur Lupia. New York: Cambridge University Press, 445–458.
- Gaziano, Cecilie. 1983. "The Knowledge Gap: An Analytical Review of Media Effects." *Communication Research* 10(4): 447–486.
- Gelman, Andrew, and Guido W. Imbens. 2013. "Why ask Why? Forward Causal Inference and Reverse Causal Questions." NBER Working Paper 19614.
- Genova, B.K.L., and Bradley S. Greenberg. 1979. "Interests in News and the Knowledge Gap." *Public Opinion Quarterly* 43(1): 79.
- Grönlund, Kimmo, and Henry Milner. 2006. "The Determinants of Political Knowledge in Comparative Perspective." *Scandinavian Political Studies* 29(4): 386–406.
- Hansen, Kasper M., and Rasmus Tue Pedersen. 2014. "Campaigns Matter: How Voters Become Knowledgeable and Efficacious During Election Campaigns." *Political Communication* 31(2): 303–324.
- Holbrook, Thomas M. 2002. "Presidential Campaigns and the Knowledge Gap." *Political Communication* 19: 437–454.
- Hovland, Carl I. 1959. "Reconciling Conflicting Results Derived from Experimental and Survey Studies of Attitude Change." *American Psychologist* 14(1): 8–17.

- Huff, Connor, and Dustin Tingley. 2015. “‘Who are these people?’ Evaluating the Demographic Characteristics and Political Preferences of MTurk Survey Respondents.” *Research & Politics* 2(3): 1–12.
- Iyengar, Shanto, and Donald R. Kinder. 1987. *News That Matters: Television and American Opinion*. Chicago, IL: The University Of Chicago Press.
- Iyengar, Shanto, Mark D. Peters, and Donald R. Kinder. 1982. “Experimental Demonstrations of the ‘Not-So-Minimal’ Consequences of Television News Programs.” *American Political Science Review* 76(4): 848–858.
- Jennings, M. Kent. 1996. “Political Knowledge Over Time and Across Generations.” *Public Opinion Quarterly* 60(2): 228.
- Jerit, Jennifer, Jason Barabas, and Toby Bolsen. 2006. “Citizens, Knowledge, and the Information Environment.” *American Journal of Political Science* 50(2): 266–282.
- Kim, Young Mie, and John Vishak. 2008. “Just Laugh! You Dont Need to Remember: The Effects of Entertainment Media on Political Information Acquisition and Information Processing in Political Judgment.” *Journal of Communication* 58(2): 338–360.
- Kinder, Donald R. 1998. “Communication and Opinion.” *Annual Review of Political Science* 1(1): 167–197.
- Kinder, Donald R. 2003. “Communication and Politics in the Age of Information.” In *Oxford Handbook of Political Psychology*, eds. David O. Sears, Leonie Huddy, and Robert Jervis. New York: Oxford University Press.
- Knobloch-Westerwick, Silvia, Nikhil Sharma, Derek L. Hansen, and Scott Alter. 2005. “Impact of Popularity Indications on Readers’ Selective Exposure to Online News.” *Journal of Broadcasting & Electronic Media* 49(3): 296–313.
- Krupnikov, Yanna, and Adam Seth Levine. 2014. “Cross-Sample Comparisons and External

- Validity.” *Journal of Experimental Political Science* 1(March): 59–80.
- Kull, Steven, Clay Ramsay, and Evan Lewis. 2003. “Misperceptions, the Media, and the Iraq War.” *Political Science Quarterly* 118(4): 569–598.
- Kwak, Nojin. 1999. “Revisiting the Knowledge Gap Hypothesis: Education, Motivation, and Media Use.” *Communication Research* 26(4): 385–413.
- Lau, Richard R., and David P. Redlawsk. 2006. *How Voters Decide: Information Processing in Election Campaigns*. New York: Cambridge University Press.
- Leeper, Thomas J. 2012. “MTurkR: Access to Amazon Mechanical Turk Requester API.” R package version 0.1.
- Leeper, Thomas J. 2013. “Crowdsourcing with R and the MTurk API.” *The Political Methodologist* 20(2): 2–7.
- Leeper, Thomas J. 2014. “The Informational Basis for Mass Polarization.” *Public Opinion Quarterly* 78(1): 27–46.
- Leeper, Thomas J. 2017. “How Does Treatment Self-Selection Affect Inferences About Political Communication?” *Journal of Experimental Political Science* 4(1): 21–33.
- Levendusky, Matthew S. 2012. *Partisan News that Matters: How Partisan News Impacts American Politics*. Chicago, IL: The University of Chicago Press.
- Levendusky, Matthew S. 2013a. “Partisan Media Exposure and Attitudes Toward the Opposition.” *Political Communication* 30(4): 565–581.
- Levendusky, Matthew S. 2013b. “Why Do Partisan Media Polarize Viewers?” *American Journal of Political Science* xx(xx): no–no.
- Liu, Yung-i, and William P. Eveland. 2005. “Education, Need for Cognition, and Campaign Interest as Moderators of News Effects on Political Knowledge: An Analysis of the Knowledge Gap.” *Journalism and Mass Communication Quarterly* 82(4): 910.

- Lupia, Arthur. 2015. *Uninformed: Why People Know So Little about Politics and what We Can Do about it*. Oxford University Press.
- Luskin, Robert C., and John G. Bullock. 2005. "'Don't Know' Means 'Don't Know': DK Responses and the Public's Level of Political Knowledge." *The Journal of Politics* 73(2): 547–557.
- Manski, Charles F. 1999. *Identification Problems in the Social Sciences*. Cambridge, MA: Harvard University Press.
- McCann, James A., and Chappell Lawson. 2006. "Presidential Campaigns and the Knowledge Gap in Three Transitional Democracies." *Political Research Quarterly* 59(1): 13–22.
- McCombs, Maxwell E., and Donald L. Shaw. 1972. "The Agenda-Setting Function of Mass Media." *Public Opinion Quarterly* 36(2): 176–187.
- Messing, Solomon, and Sean J. Westwood. 2014. "Selective Exposure in the Age of Social Media: Endorsements Trump Partisan Source Affiliation When Selecting News Online." *Communication Research* 41(8): 1042–1063.
- Miller, Joanne M., and Jon A. Krosnick. 2000. "News Media Impact on the Ingredients of Presidential Evaluations: Politically Knowledgeable Citizens are Guided by a Trusted Source." *American Journal of Political Science* 44(2): 301–315.
- Miller, Melissa K., and Shannon K. Orr. 2008. "Experimenting with a 'Third Way' in Political Knowledge Estimation." *Public Opinion Quarterly* 72(4): 768–780.
- Mondak, Jeffery J. 1999. "Reconsidering the Measurement of Political Knowledge." *Political Analysis* 8(1): 57.
- Mondak, Jeffery J., and Belinda Creel Davis. 2001. "Asked and Answered: Knowledge Levels When We Will Not Take 'Don't Know' For An Answer." *Political Behavior* 23(3): 199–224.
- Moore, David W. 1987. "Political Campaigns and the Knowledge-Gap Hypothesis." *Public*

Opinion Quarterly 51(2): 186.

Morgan, Stephen L., and Christopher Winship. 2015. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. 2nd ed. New York: Cambridge University Press.

Morris, Jonathan S., and P. L. Francia. 2009. "Cable News, Public Opinion, and the 2004 Party Conventions." *Political Research Quarterly* 63(4): 834–849.

Mullinix, Kevin J., Thomas J. Leeper, James N. Druckman, and Jeremy Freese. 2015. "The Generalizability of Survey Experiments." *Journal of Experimental Political Science* 2: 109–138.

Mutz, Diana C. 2007. "Effects of 'In-Your-Face' Television Discourse on Perceptions of a Legitimate Opposition." *American Political Science Review* 101(4): 621–635.

Nelson, Thomas E., Rosalee A. Clawson, and Zoe M. Oxley. 1997. "Media Framing of a Civil Liberties Conflict and Its Effect on Tolerance." *American Political Science Review* 91(3): 567–583.

Nelson, Thomas E., Sarah McKinnon Bryner, and Dustin Carnahan. 2011. "Media and Politics." In *Cambridge Handbook of Experimental Political Science*, eds. James N. Druckman, Donald P. Green, James H. Kuklinski, and Arthur Lupia. New York: Cambridge University Press, 201–213.

Neuman, W. Russell. 1986. *The Paradox of Mass Politics: Knowledge and Opinion in the American Electorate*. Harvard University Press.

Nie, Norman H., Jane Junn, and Kenneth Stehlik-Barry. 1996. *Education and Democratic Citizenship in America*. Chicago, IL: University of Chicago Press.

Niederdeppe, Jeff, Sarah E. Gollust, and Colleen L. Barry. 2014. "Inoculation in Competitive Framing: Examining Message Effects on Policy Preferences." *Public Opinion Quarterly*

78(3): 634–655.

Norris, Pippa, and David Sanders. 2003. “Message or Medium? Campaign Learning During the 2001 British General Election.” *Political Communication* 20(3): 233–262.

Parkin, Michael. 2009. “Taking Late Night Comedy Seriously: How Candidate Appearances on Late Night Television Can Engage Viewers.” *Political Research Quarterly* 63(1): 3–15.

Petty, Richard E., and John T. Cacioppo. 1986. “The Elaboration Likelihood Model of Persuasion.” *Advances in Experimental Social Psychology* 19: 123–205.

Prior, Markus. 2003. “Any Good News in Soft News? The Impact of Soft News Preference on Political Knowledge.” *Political Communication* 20(2): 149–171.

Prior, Markus. 2005. “News vs. Entertainment: How Increasing Media Choice Widens Gaps in Political Knowledge and Turnout.” *American Journal of Political Science* 49(3): 577–592.

Prior, Markus. 2007. *Post-Broadcast Democracy: How Media Choice Increases Inequality in Political Involvement and Polarizes Elections*. New York: Cambridge University Press.

Prior, Markus. 2009a. “The Immensely Inflated News Audience: Assessing Bias in Self-Reported News Exposure.” *Public Opinion Quarterly* 73(1): 130–143.

Prior, Markus. 2009b. “Improving Media Effects Research through Better Measurement of News Exposure.” *The Journal of Politics* 71(03): 893–908.

Prior, Markus. 2013. “The Challenge of Measuring Media Exposure: Reply to Dilliplane, Goldman, and Mutz.” *Political Communication* 30(4): 620–634.

Prior, Markus. 2014. “Visual Political Knowledge: A Different Road to Competence?” *Journal of Politics* 76(1): 41–57.

Prior, Markus, and Arthur Lupia. 2008. “Money, Time, and Political Knowledge: Distinguishing Quick Recall and Political Learning Skills.” *American Journal of Political Science* 52(1): 169–183.

- Samii, Cyrus. 2016. "Causal Empiricism in Quantitative Research." *Journal of Politics* 78(3): 941–955.
- Schuldt, Jonathan P., Sara H. Konrath, and Norbert Schwarz. 2011. "'Global Warming' or 'Climate Change'?": Whether the Planet is Warming Depends on Question Wording." *Public Opinion Quarterly* 75(1): 115–124.
- Shulman, Hillary C., and Franklin J. Boster. 2014. "Effect of Test-Taking Venue and Response Format on Political Knowledge Tests." *Communication Methods and Measures* 8(3): 177–189.
- Sidani, Souraya, Joyal Miranda, Dana Epstein, and Mary Fox. 2009. "Influence of Treatment Preferences on Validity: A Review." *Canadian Journal of Nursing Research* 41(4): 52–67.
- Stroud, Natalie Jomini. 2011. *Niche News: The Politics of News Choice*. New York: Oxford University Press.
- Sturgis, Patrick, Nick Allum, and Patten Smith. 2008. "An Experiment on the Measurement of Political Knowledge in Surveys." *Public Opinion Quarterly* 72(1): 90–102.
- Tichenor, P.J., G.A. Donohue, and C.N. Olien. 1970. "Mass Media Flow and Differential Growth in Knowledge." *Public Opinion Quarterly* 34(2): 159–170.
- Valentino, Nicholas A., Vincent L. Hutchings, and Dmitri Williams. 2004. "The Impact of Political Advertising on Knowledge, Internet Information Seeking, and Candidate Preference." *Journal of Communication* 54(2): 337–354.
- Xenos, Michael, and Amy B. Becker. 2009. "Moments of Zen: Effects of The Daily Show on Information Seeking and Political Learning." *Political Communication* 26(3): 317–332.

A Supplemental Analysis of Knowledge Questions

This appendix reports supplemental results related to the main outcome measure: knowledge of Syria. Specifically, it provides (1) descriptive statistics for knowledge levels at time 1 and time 2, (2) descriptive statistics about “don’t know” responses to the knowledge questions, (3) a descriptive summary of the results, separating the two versions of the news treatment article, (4) an item-response theory (IRT) analysis of treatment group means and experimental effects, (5) alternative estimates of the main experimental results using estimators introduced by Gaines and Kuklinski (2011), (6) describes item-specific analyses of treatment group means and experimental effects for the five items used in the political knowledge scale, and (7) item-specific analysis of “don’t know” responses.

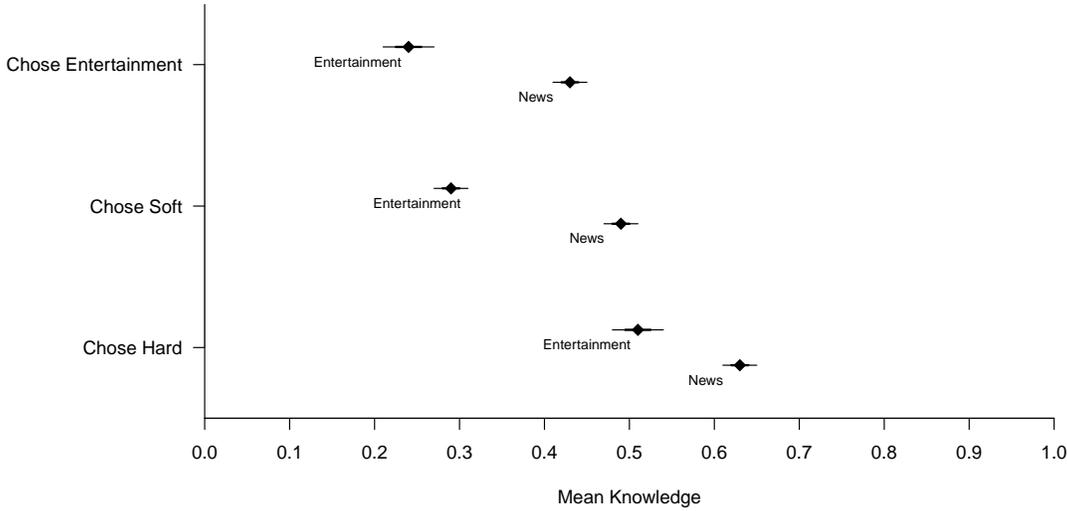
A.1 Treatment Group Means, by Time

Overall the results show a fair degree of stability in knowledge levels between time 1 and time 2, with — perhaps surprisingly — slight increases in knowledge among those randomly assigned to the entertainment article:

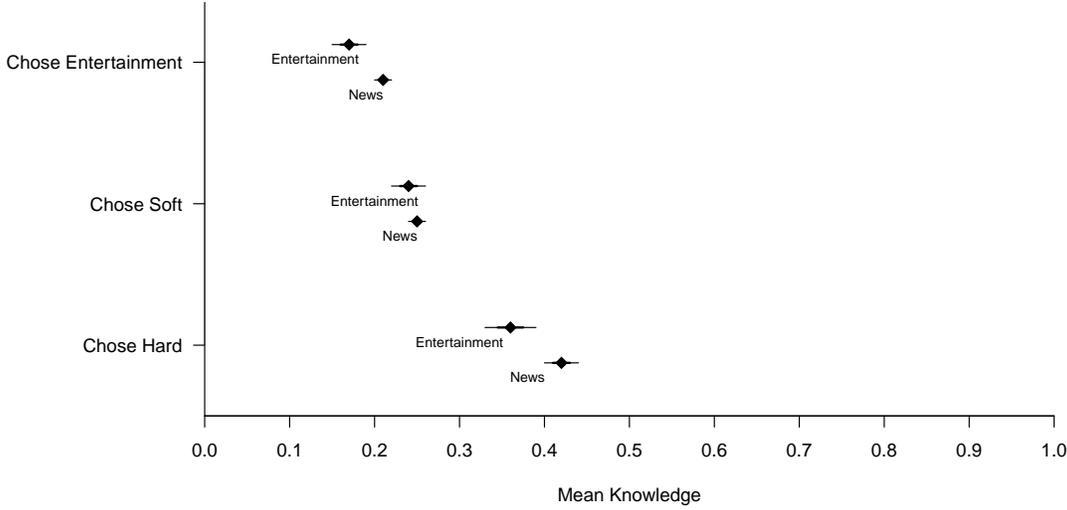
Choice	Treatment	n (All)	Knowledge (T1)	n (Both Waves)	Knowledge (T1)	Knowle
Hard	News	318	0.63 (0.02)	230	0.63 (0.02)	0.
	Entertainment	160	0.46 (0.03)	108	0.51 (0.03)	0.
Soft	News	481	0.47 (0.01)	303	0.49 (0.02)	0.
	Entertainment	240	0.28 (0.02)	157	0.29 (0.02)	0.
Entertainment	News	380	0.39 (0.02)	219	0.43 (0.02)	0.
	Entertainment	189	0.24 (0.02)	115	0.24 (0.03)	0.
Captive	News	275	0.49 (0.02)	173	0.54 (0.02)	0.
	Entertainment	136	0.32 (0.03)	90	0.36 (0.03)	0.

Note: Cell entries are treatment group means levels of knowledge, with standard errors in parentheses. Data include only those respondents completing both survey waves.

The result is pattern is that while treatment effects manifest at time 1 (showing only those completing both waves):



Those results have faded by time 2, with the only remaining between-group differences reflecting the initial gaps in knowledge between those with different behaviourally expressed media preferences:



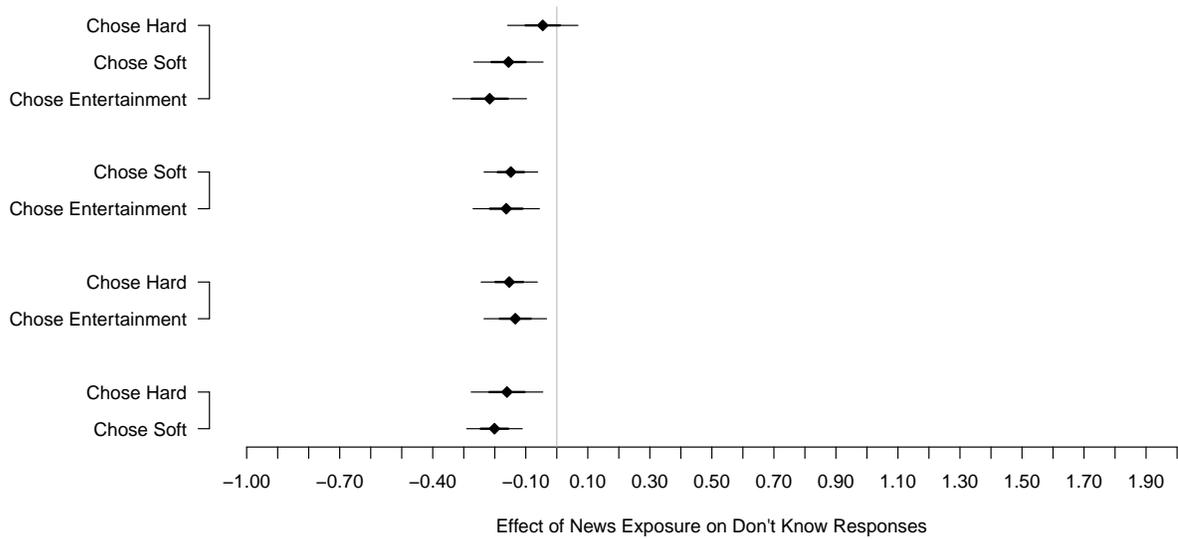
A.2 “Don’t Know” Responses

The table below reports proportions of “don’t know” responses to each knowledge question at each panel wave and the mean scale of “don’t knows” (total DKs divided by 5). As should be clear (and is consistent with the number of correct responses), many participants did not know the correct answers to the policy-relevant questions. On average, participants supplied 1.88 (time 1; $(0.38 * 5)$) and 1.20 (time 2; $(0.24 * 5)$) “don’t know” responses across the entire outcome battery.

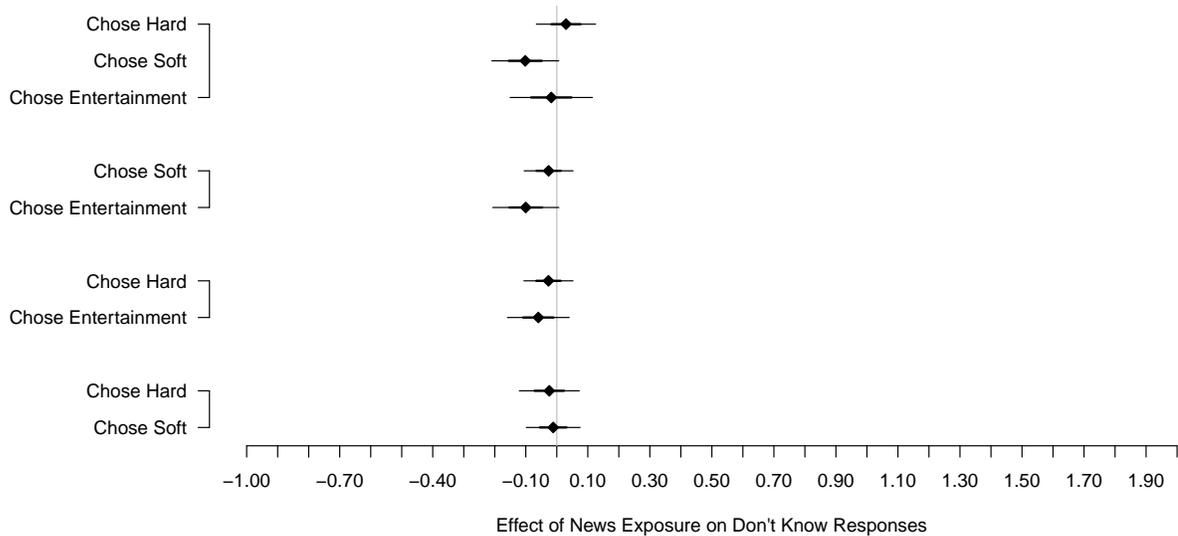
	T1	T2
Who is the current president of Syria?	26.4%	15.8%
What is the name of the main rebel group fighting in Syria?	43.4%	25.4%
What is the largest city in Syria?	24.6%	13.5%
Has the United Nations Security Council passed a resolution condemning the violence in Syria?	43.8%	32.0%
What position has the Russian government taken toward the violence in Syria?	49.6%	33.6%
Mean Number of Don’t Know Responses	0.38 (0.34)	0.24 (0.32)
n	2221	2221

Translating these results into experimental treatment effects, we see that much of the main results of the study are being driven by those in the entertainment conditions reporting a “don’t know” response compared to those in the news conditions reporting correct responses. There is a generally homogeneous pattern of effects across preferences and choice sets, closely mirroring the results reported in the paper.

Here are the results for time 1:



By time 2, however, these effects have largely dissipated:

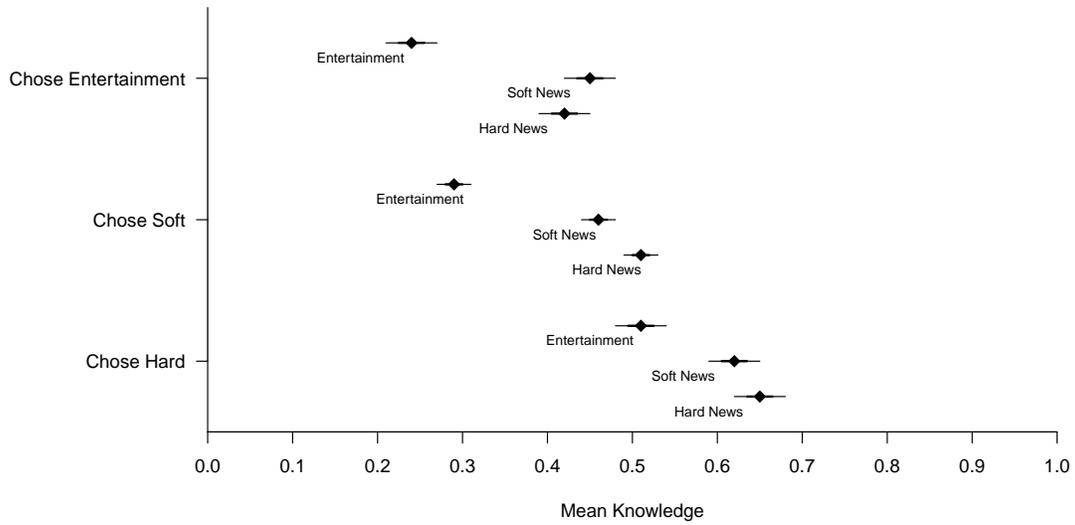


A.3 Results, Separating Hard and Soft News

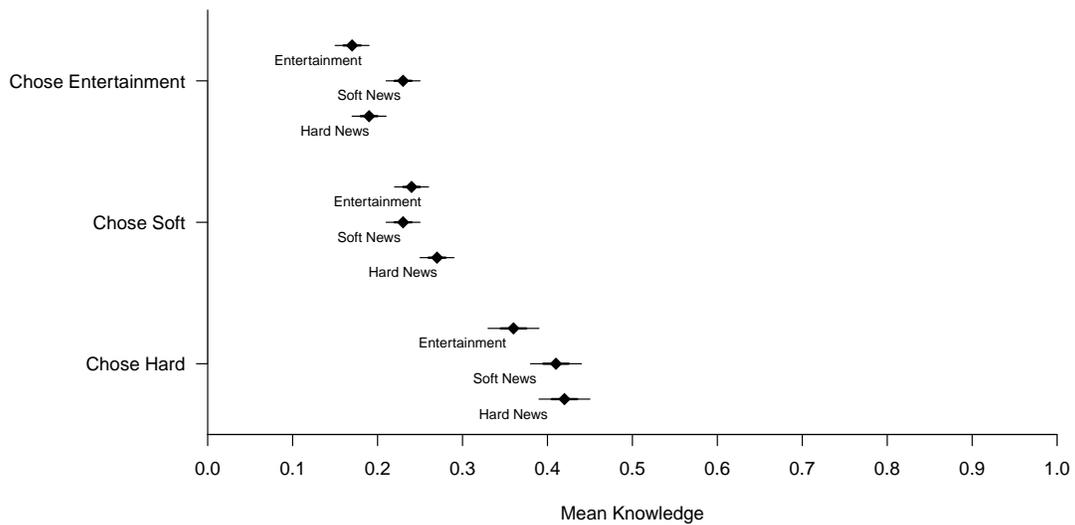
The experimental results reported in the paper merge two different versions of the Syria news article that were used in the experiment. These articles (as seen in Appendix E) use either a “hard” or “soft” news angle on the Syria issue. The results, when separating respondents by the hard or soft article, are largely the same as those reported in the body of the paper. The table below reports experimental group cell sizes broken out by news article:

<i>Participant's Choice</i>	<i>Manipulated Story</i>		
	Syria (Hard)	Syria (Soft)	Entertainment
Chose Hard	n=159	159	160
Chose Soft	240	241	240
Chose Entertainment	190	190	189
Captive (no choice)	138	137	136

The figure below shows mean levels of knowledge in each treatment group. As should be clear, the results are largely the same for these two conditions.



T1

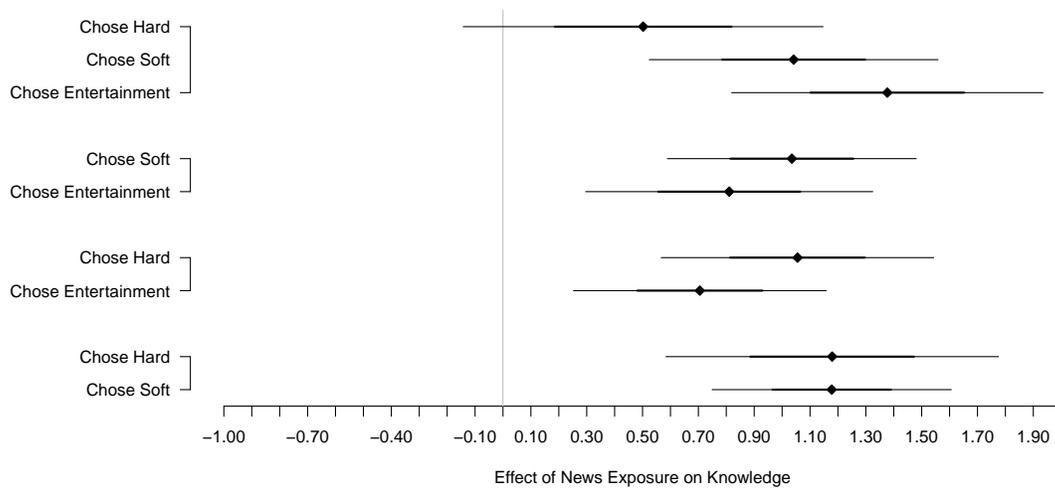


T2

A.4 Item Response Theory (IRT) Analysis

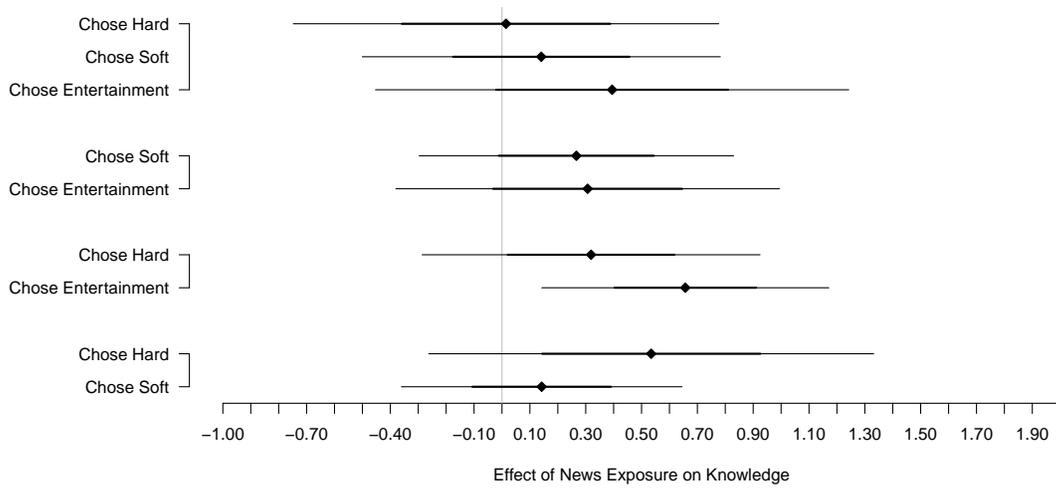
The results reported in the body of the paper rely on a simple additive scale of the five knowledge items to measure effects, but an alternative approach is to estimate an item-response theory (IRT) model of ability based on the raw items. This section reports the results of a one-parameter Rasch model, which yields estimates of both item parameters (difficulties) and person parameters (in this case, knowledge levels). These person parameter estimates are then used to estimate treatment group means and treatment effects.²⁴

The question is whether the experimental results are robust to this alternative measure of the outcome. The figure below indicates that the results are largely identical to those reported in the body of the paper. Mean levels of knowledge in each condition (on the latent knowledge dimension produced by the IRT estimation procedure) vary in precisely the same way reported in the main text and, as such, the treatment effects closely mirror those reported in the main body of the paper for time 1:



²⁴For the purposes of this subsection, the results reflect the complete set of respondents at time 1 (including those who did not participate in time 2). The results are consistent if time 2 nonrespondents are excluded.

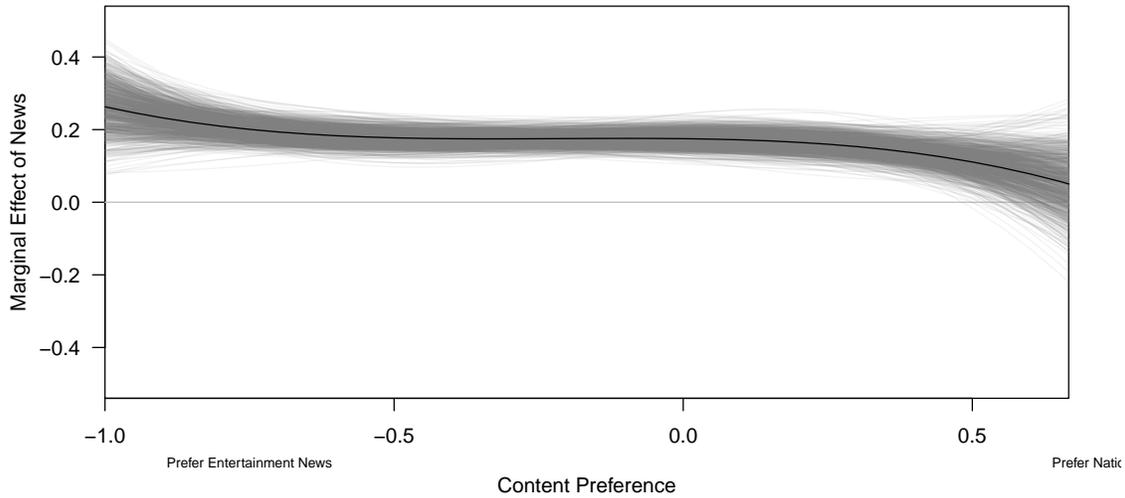
and time 2:



A.5 Analyses Ignoring Behavior

One could argue that because of the strong connection between respondents' stated preferences and their selection of media content within the preference trial arm of the study, it was not actually necessary to observe participants' behavior when their self-reported preferences alone would have sufficed. There is some truth to this argument because regardless of whether media preferences are stated or revealed, they are not randomly assigned. Given that revealed choices differed substantially across media choicesets, however, there was a considerable amount learned by requiring responds to reveal their preferences through an active choice. Indeed, even though the results suggest a substantial degree of effect homogeneity across those with different preferences (see, for example, the figure below showing effects conditional only on preferences), the use of choice *would have* allowed for a much more detailed analysis of effect heterogeneity, had it been present. The reliance on revealed preferences is therefore a strength of a design that facilitates the important substantive finding of effect homogeneity, rather than a weakness of the empirical approach.

Treatment Effect by Self-Reported Content Preferences



Note: Figure displays average marginal effect of the Syria news story versus the entertainment story, across the observed range of self-reported national-entertainment preference differential, from a regression of knowledge on media preferences (to the third power). Gray lines represent bootstrapped estimates.

A.6 Gaines–Kuklinski Estimators

Gaines–Kuklinski estimators²⁵ estimate (potentially heterogeneous) causal effects of a treatment for two population subgroups: (1) those inclined to choose the treatment, and (2) those disinclined to choose the treatment (or, equivalently, inclined to choose control). The estimators are useful in three-group, “hybrid” experiments in which one set of participants are assigned to treatment, another set is assigned to control, and a final set is given a choice between the two. The two G–K estimators then provide treatment effect estimates for the two subpopulations. The current design provides direct estimates of those effects by randomizing treatment assignment after participants reveal preferences (in the patient preference trial arm of the study) and therefore the Gaines–Kuklinski estimators are not needed. However, because the present design also includes a randomized experiment, it is possible to check the validity of the preference trial estimates using the indirect G–K estimators.

²⁵Proposed in: Brian J. Gaines and James H. Kuklinski. 2011. “Experimental Estimation of Heterogeneous Treatment Effects Related to Self-Selection.” *American Journal of Political Science* 55(3): 724–736. doi:10.1111/j.1540-5907.2011.00518.x.

These G–K estimators are identified if we assume (1) that, given random assignment, the choice behavior of those in the choice conditions is on average identical to the unobserved choice behavior of those in the randomized conditions, and (2) the equivalence of potential outcomes for a randomly assigned message versus and the same self-selected message (i.e. that there is no effect of the assignment mechanism; an exclusion restriction). As shown in the body of the paper, the latter and more crucial assumption is quite plausible in the present case.

To fit the G–K estimators to the present design, we also have to assume two choice alternatives, so we focus only on those choosing “hard.” Further, we focus only on those individuals in the preference trial conditions who were randomly assigned to receive their preferred content. In other words, we drop individuals who selected news but randomly received entertainment and those that selected entertainment but randomly received news.²⁶

Under the above assumptions and this data subsetting procedure, the G–K estimators are defined as (1) an effect for the *Prefer News* group:

$$t_s = \frac{\bar{Y}_{Choice} - \bar{Y}_{Entertainment}}{\hat{\alpha}} \quad (1)$$

where \bar{Y}_{Choice} is the mean outcome value among all respondents assigned to the preference trial arm and $\hat{\alpha}$ is the proportion of these individuals choosing news. This is the effect of the news article for those that prefer news. And the second effect is: (2) an effect for the *Prefer Entertainment* group:

$$t_n = \frac{\bar{Y}_{News} - \bar{Y}_{Choice}}{1 - \hat{\alpha}} \quad (2)$$

capturing the effect of the news article among those that would prefer the entertainment content.

The results from the G–K estimators closely align with the results in the body of the paper:

Group	Knowledge (T1)	Knowledge (T2)	DKs (T1)	DKs (T2)
Full Sample	0.19 (0.04)	0.01 (0.04)	-1.02 (0.21)	-0.20 (0.19)
Prefer News	0.23 (0.07)	0.05 (0.07)	-0.96 (0.40)	-0.06 (0.36)
Prefer Entertainment	0.15 (0.06)	-0.02 (0.06)	-1.07 (0.33)	-0.32 (0.29)

For those that prefer news, the news article has a large positive effect of a size (0.23) nearly equivalent to that reported in the body of the paper. The effect for those that prefer

²⁶Due to random assignment of the treatment article, this exclusion of respondents is statistically and substantively inconsequential as the excluded respondents are identical to the included respondents in expectation.

entertainment is smaller (0.15) but also within the confidence intervals of the analogous effects reported in the paper. For both groups, the effect of the treatment article disappears by time 2, consistent with the previously reported results. The G–K estimators also indicate that these effects are due to significant reductions in “don’t know” responses at time 1, a pattern that disappears by time 2. That the main preference trial effect estimates and the G–K estimators are so similar provides a great deal of confidence that the effects are genuine and not sensitive to the specifics of the experimental design or methods of analysis. This analysis also constitutes the first empirical comparison of G–K estimators for a hybrid design against a preference trial benchmark.

Relatedly, it is possible to test whether the average treatment effect appears to be the same in the captive arm of the experiment as in the preference trial (ignoring preferences). The following table shows that means in the “news” and “entertainment” conditions across the two arms are, in fact, identical:

	News Condition Mean	Entertainment Condition Mean
Randomized Conditions	0.49 (0.02)	0.32 (0.03)
Choice Conditions	0.49 (0.01)	0.32 (0.01)
Combined	0.49 (0.01)	0.32 (0.01)

A.7 Item-specific Analysis of Treatment Effects

The paper reported overall treatment group means and treatment effects, averaging across all five knowledge items. It is also worth separately examining mean levels of knowledge for each item and, similarly, effects on each item. The table below reports the proportion of correct responses to each knowledge item (at time 1) by experimental condition:

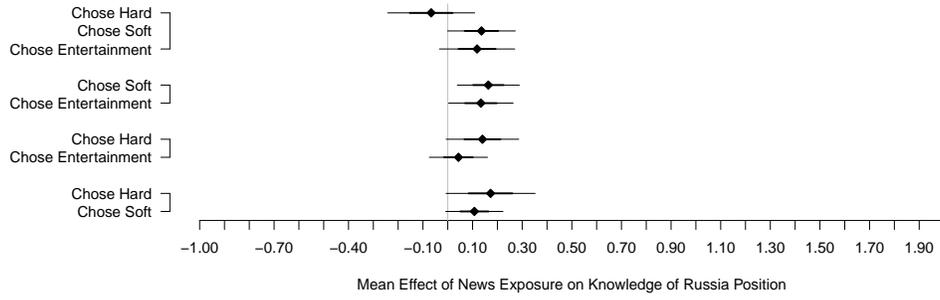
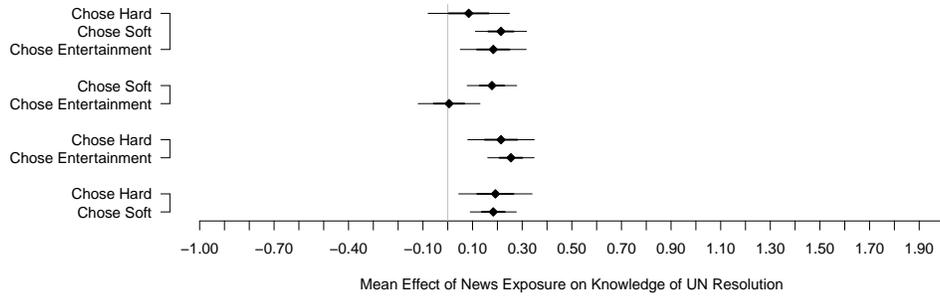
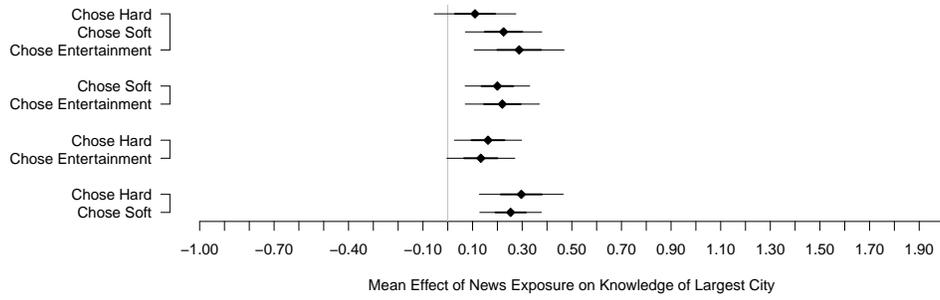
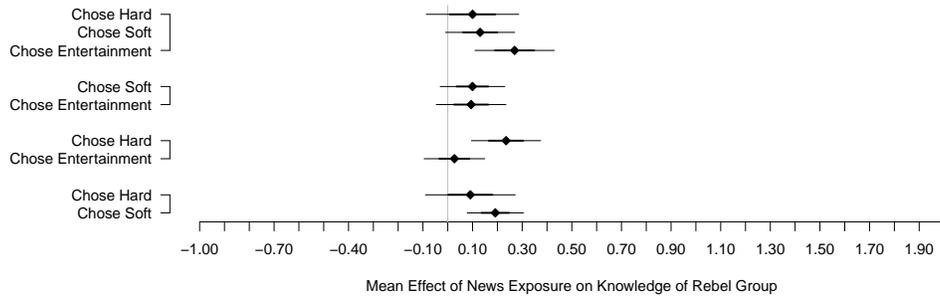
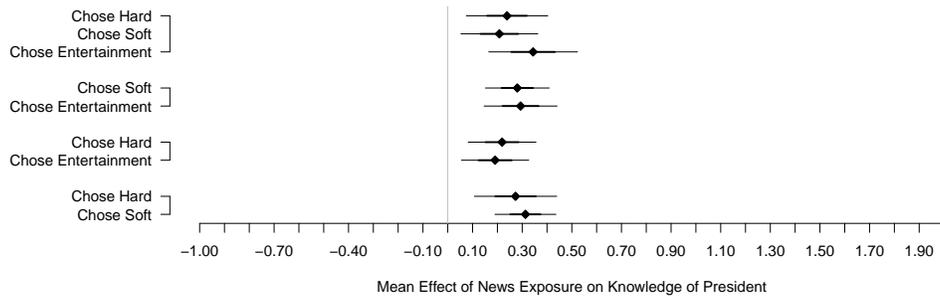
Choice	Treatment	President	Largest City	Rebel Group	UN	Russia
Hard	News	0.84 (0.01)	0.57 (0.02)	0.81 (0.01)	0.39 (0.03)	0.61 (0.02)
	Ent.	0.60 (0.03)	0.41 (0.05)	0.62 (0.03)	0.22 (0.06)	0.52 (0.04)
Soft	News	0.69 (0.01)	0.41 (0.03)	0.66 (0.02)	0.29 (0.03)	0.39 (0.03)
	Ent.	0.42 (0.04)	0.27 (0.05)	0.43 (0.04)	0.10 (0.06)	0.26 (0.05)
Ent.	News	0.62 (0.02)	0.34 (0.03)	0.56 (0.02)	0.28 (0.04)	0.29 (0.04)
	Ent.	0.36 (0.05)	0.24 (0.06)	0.36 (0.05)	0.12 (0.07)	0.20 (0.06)
Captive	News	0.71 (0.02)	0.48 (0.03)	0.72 (0.02)	0.32 (0.04)	0.40 (0.04)
	Ent.	0.51 (0.04)	0.27 (0.06)	0.53 (0.04)	0.12 (0.08)	0.26 (0.06)

The corresponding results for time 2 are below:

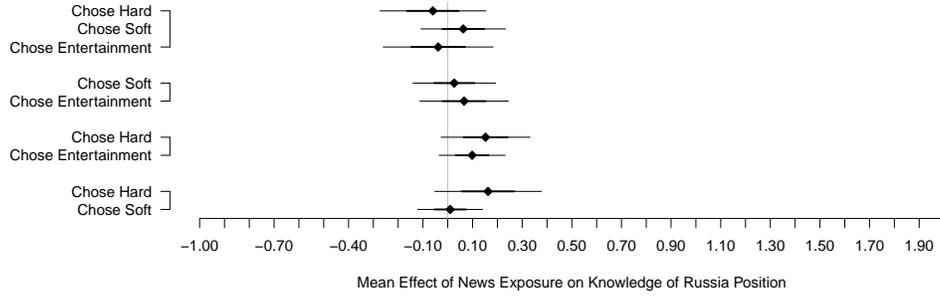
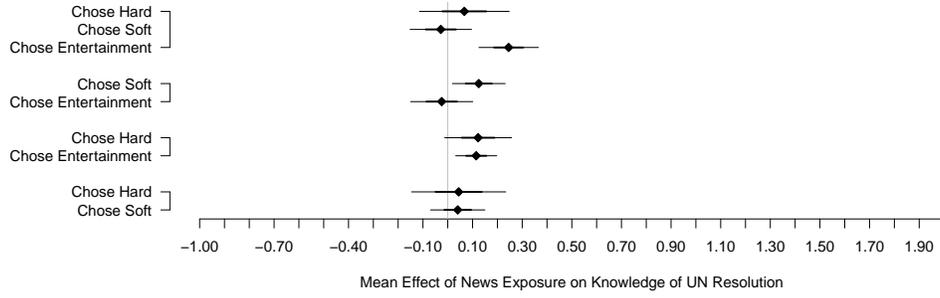
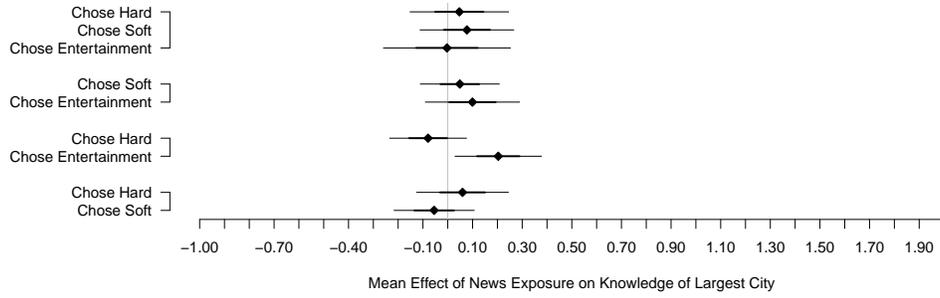
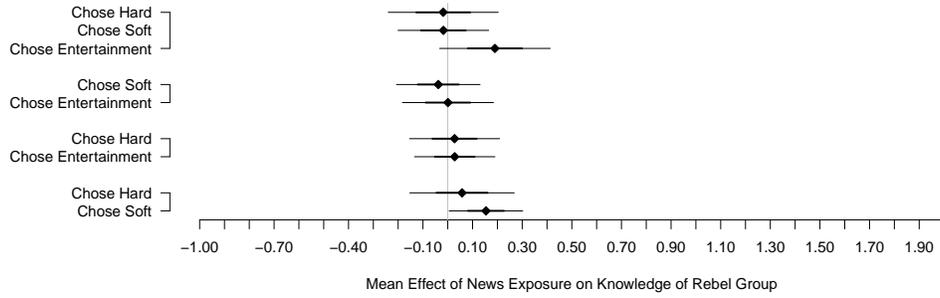
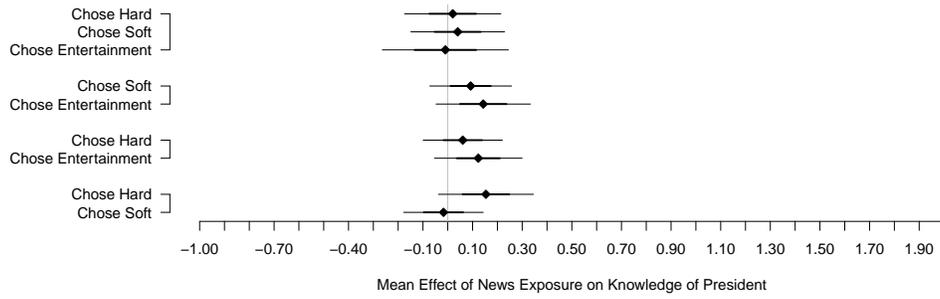
Choice	Treatment	President	Largest City	Rebel Group	UN	Russia
Hard	News	0.79 (0.01)	0.57 (0.03)	0.75 (0.02)	0.26 (0.05)	0.62 (0.03)
	Ent.	0.71 (0.03)	0.55 (0.04)	0.75 (0.02)	0.18 (0.08)	0.53 (0.05)
Soft	News	0.58 (0.02)	0.43 (0.03)	0.61 (0.02)	0.16 (0.05)	0.32 (0.04)
	Ent.	0.53 (0.04)	0.38 (0.05)	0.58 (0.03)	0.11 (0.07)	0.29 (0.06)
Ent.	News	0.54 (0.03)	0.34 (0.05)	0.56 (0.03)	0.15 (0.06)	0.27 (0.05)
	Ent.	0.45 (0.05)	0.29 (0.07)	0.45 (0.05)	0.06 (0.09)	0.23 (0.07)
Captive	News	0.64 (0.03)	0.43 (0.04)	0.71 (0.02)	0.20 (0.06)	0.37 (0.05)
	Ent.	0.63 (0.04)	0.37 (0.07)	0.60 (0.04)	0.16 (0.09)	0.34 (0.07)

If we translate these levels of knowledge into condition-specific treatment effects, the results for both time periods are broadly consistent with the aggregated results reported in the body of the paper. At time 1, it appears that much of the movement in political knowledge across conditions is driven by gains in knowledge of the “easy” items (the Syrian President and the country’s largest city), but gains in knowledge are also clearly seen on the other items. Some of the particular treatment effects differ from those in the aggregate results but this is expected given (1) the modest sample sizes at this level of analysis and (2) the increase in uncertainty due to item-level (rather than scale-level) analysis.

Below are the item-specific results for time 1:



And, additionally the results for time 2:



A.8 Item-specific Analysis of “Don’t Know” Responses

The results also indicate that much of the action is not moving respondents from incorrect to correct answers, but rather from “don’t know” responses to correct responses. This is clear in proportions of “don’t know” responses to each question at time 1:

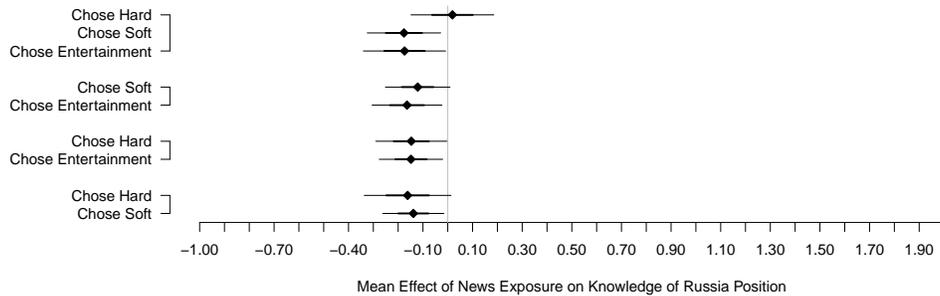
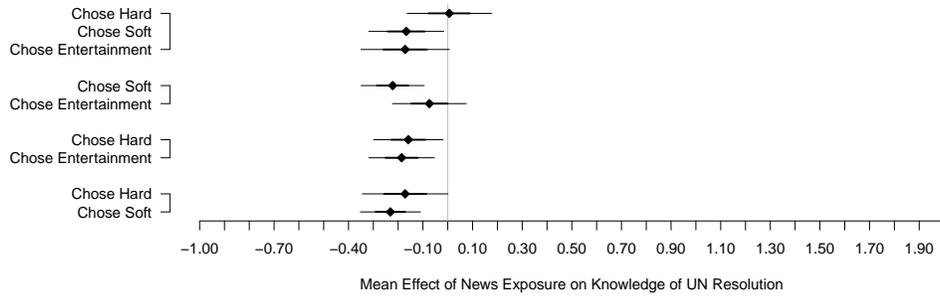
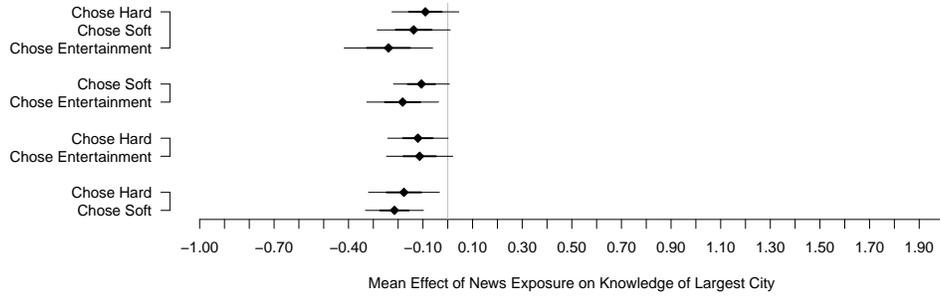
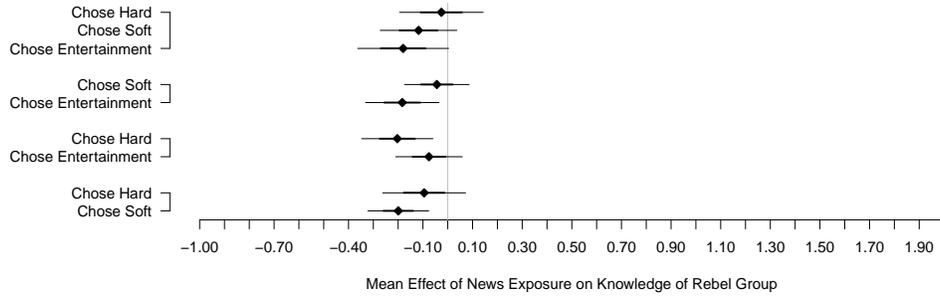
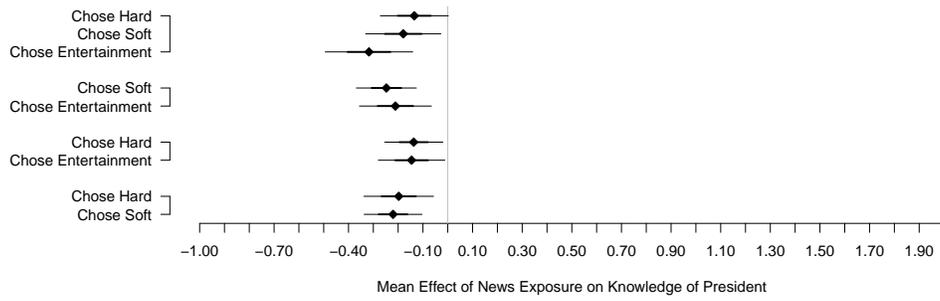
Choice	Treatment	President	Largest City	Rebel Group	UN	Russia
Hard	News	0.09 (0.05)	0.29 (0.04)	0.11 (0.05)	0.28 (0.04)	0.31 (0.04)
	Ent.	0.24 (0.06)	0.41 (0.05)	0.24 (0.06)	0.39 (0.05)	0.41 (0.05)
Soft	News	0.21 (0.04)	0.43 (0.03)	0.21 (0.04)	0.40 (0.03)	0.47 (0.02)
	Ent.	0.43 (0.04)	0.56 (0.03)	0.36 (0.04)	0.61 (0.03)	0.62 (0.02)
Ent.	News	0.28 (0.04)	0.47 (0.03)	0.29 (0.04)	0.49 (0.03)	0.57 (0.02)
	Ent.	0.49 (0.04)	0.60 (0.03)	0.46 (0.04)	0.63 (0.03)	0.72 (0.02)
Captive	News	0.20 (0.05)	0.36 (0.04)	0.17 (0.05)	0.38 (0.04)	0.46 (0.03)
	Ent.	0.40 (0.05)	0.56 (0.04)	0.32 (0.06)	0.56 (0.04)	0.62 (0.03)

The results for time 2 (below) show much lower proportions of “don’t know” responses among those in the entertainment conditions and little change in for those assigned to the news conditions. This would suggest either (1) that respondents in the entertainment conditions learned in between time 1 and time 2, possibly stimulated by exposure to the questions, and/or (2) these respondents were more likely to engage in cheating behaviour at time 2 by looking up correct answers. Unfortunately it is not possible to distinguish these mechanisms from one another.

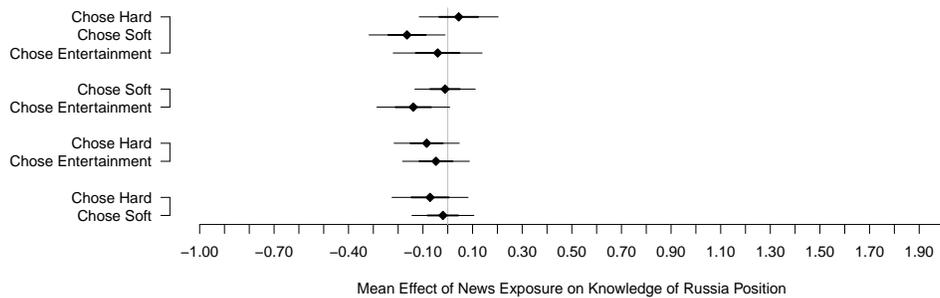
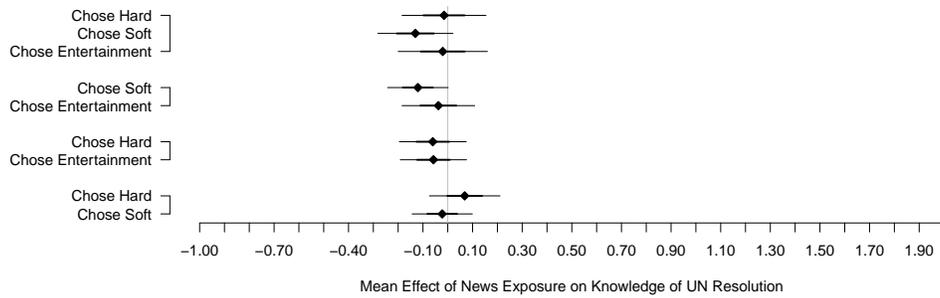
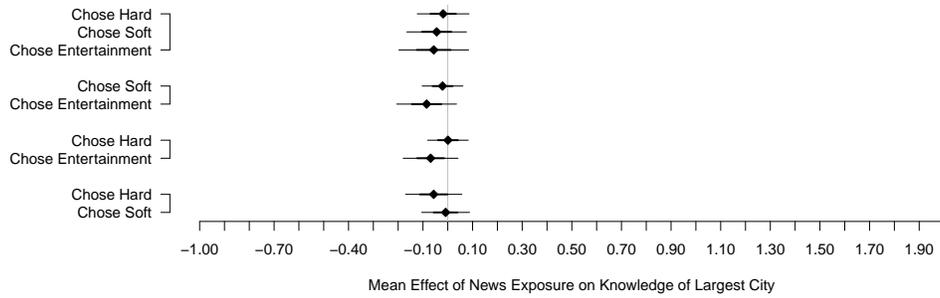
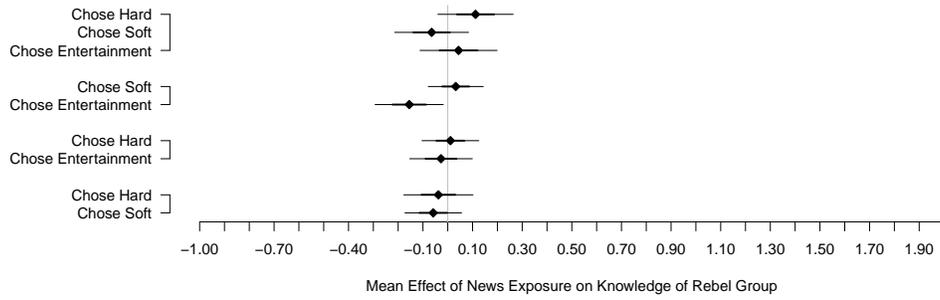
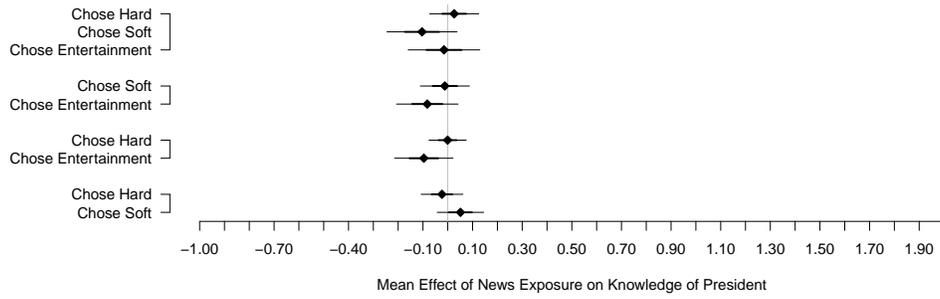
Choice	Treatment	President	Largest City	Rebel Group	UN	Russia
Hard	News	0.07 (0.05)	0.21 (0.04)	0.08 (0.05)	0.26 (0.04)	0.23 (0.04)
	Ent.	0.07 (0.07)	0.19 (0.06)	0.10 (0.07)	0.28 (0.06)	0.28 (0.06)
Soft	News	0.19 (0.04)	0.27 (0.03)	0.14 (0.04)	0.30 (0.03)	0.36 (0.03)
	Ent.	0.20 (0.05)	0.30 (0.05)	0.16 (0.05)	0.39 (0.04)	0.41 (0.04)
Ent.	News	0.18 (0.04)	0.26 (0.04)	0.15 (0.04)	0.36 (0.03)	0.36 (0.03)
	Ent.	0.25 (0.05)	0.31 (0.05)	0.22 (0.06)	0.40 (0.04)	0.43 (0.04)
Captive	News	0.15 (0.05)	0.28 (0.04)	0.12 (0.05)	0.32 (0.04)	0.33 (0.04)
	Ent.	0.16 (0.07)	0.24 (0.07)	0.16 (0.07)	0.33 (0.06)	0.37 (0.05)

The following pages show treatment effects using “don’t know” responses. Those in the news conditions were consistently less likely to say “don’t know” compared to those in the entertainment conditions and the effects were relatively consistent across preferences and choice sets. These effects are mostly diminished by time 2, however, due to the substantial drops in “don’t know” responses among those in the entertainment conditions.

Below are the item-specific results for time 1:



And, additionally the results for time 2:

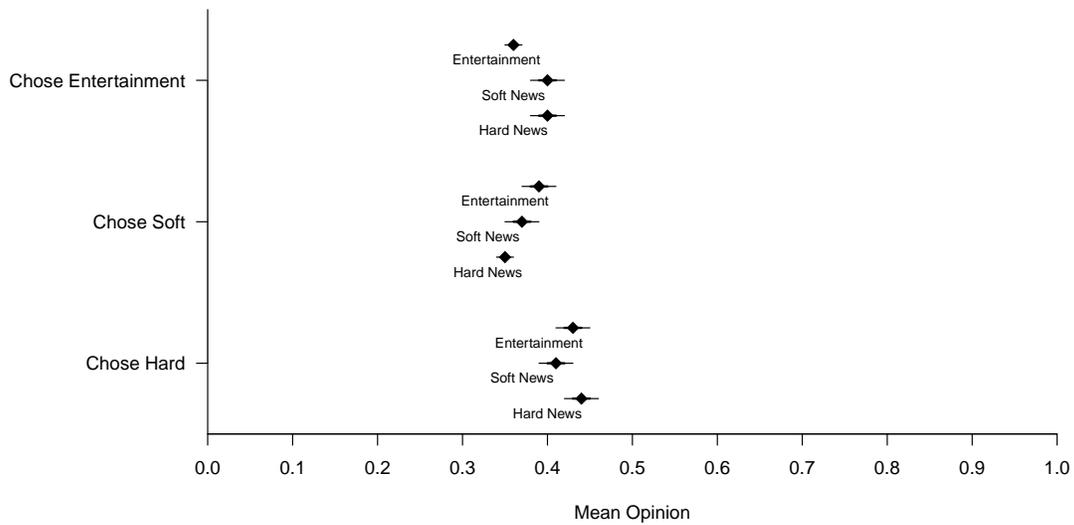


B Results for Attitudes and Attitude Certainty

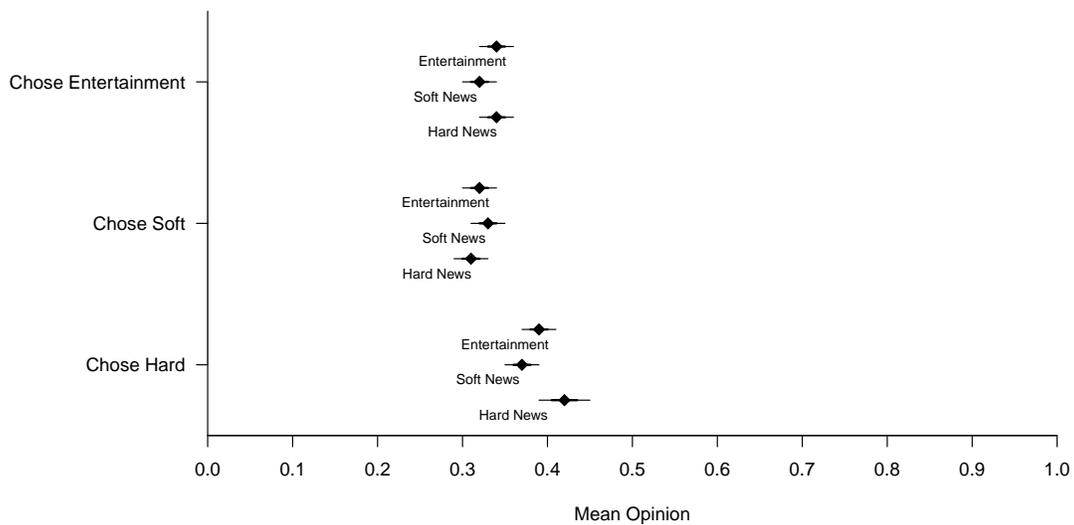
The measure of attitude toward intervention in Syria is based on six opinion questions asked at both waves. Two of these questions asked about perceived responsibility for intervention in Syria and four asked about specific types of intervention in the form of supplying arms, strategic bombing, the defense of rebel safe zones, and the use of ground troops. (All question wordings were drawn from searches of the iPoll databank.) Responses to the opinion questions were additively scaled and divided by 6 to range from 0 to 1. Attitude certainty was measured by a single item on their certainty of their opinion about U.S. involvement in Syria on a five-point scale from “not at all certain” to “extremely certain,” scaled 0 to 1.

In terms of opinions, the effects of byproduct exposure appear to be relatively minimal. Indeed, in the no-choice control conditions, opinions in the hard news, soft news, and entertainment conditions were substantively and statistically indistinguishable from one another (for all pairwise comparisons, $p > 0.54$), with mean opinions in each group lying at approximately 0.40. This indicates that despite exposure to an article documenting the violence of the Syrian conflict (in the hard news conditions) or the impact of that violence on Syrian children (in the soft news conditions), opinions on the matter were not moved at all. By T2, opinions in these conditions had actually moved toward slightly less support for international intervention with treatment group means lying at approximately 0.35. Again, at T2, there were no significant differences between any pairwise comparisons ($p > 0.41$ in all cases). The pattern of opinions across the treatment groups is displayed in the above figure, with the no-choice control conditions displayed at the far right and means for those exposed to hard, soft, and entertainment news shown to the left as in the style of the previous figure.

Examining the bars for those self-selecting into hard, soft, and entertainment news, it becomes clear that the variation between selectors of different types of news that was apparent in issue knowledge does not appear as dramatically in terms of issue opinions. No subset of the sample is particularly supportive or opposed to U.S. intervention, which is in line with the nationally representative opinion data quoted earlier. Among those who chose hard news, there are no significant differences in opinions across those receiving the three types of byproduct information ($p > 0.90$ in all cases). Among those choosing soft news, the hard and soft news stories may have had a small negative effect on support for U.S. intervention as compared to the entertainment story but these effects were substantively small (and had p-values 0.08 and 0.10 for hard-entertainment and soft-entertainment comparisons, respectively). As with the knowledge scale, the respondents choosing entertainment provide the cleanest test of byproduct exposure, but in terms of opinion neither the hard nor soft stories appears to have significantly influenced respondents’ attitudes.



T1

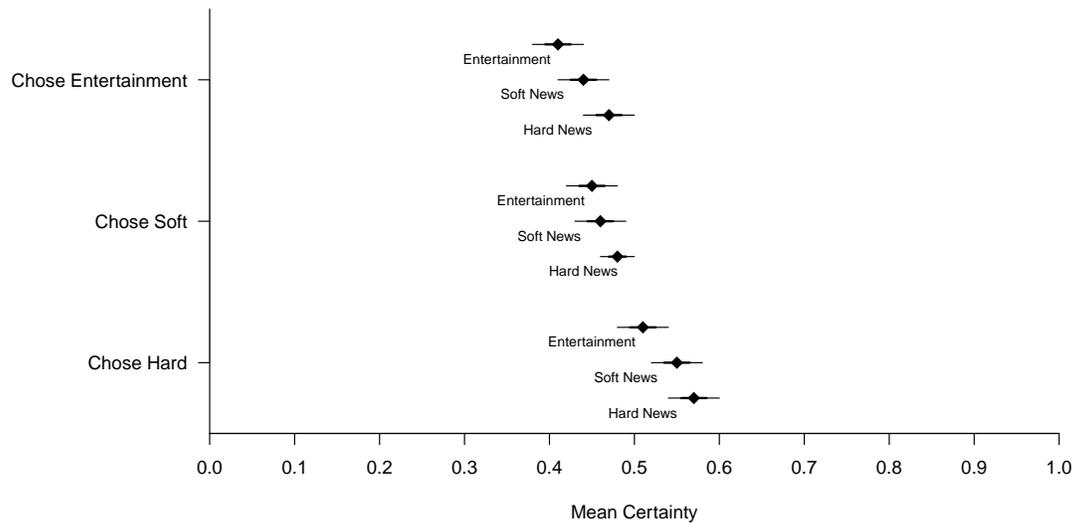


T2

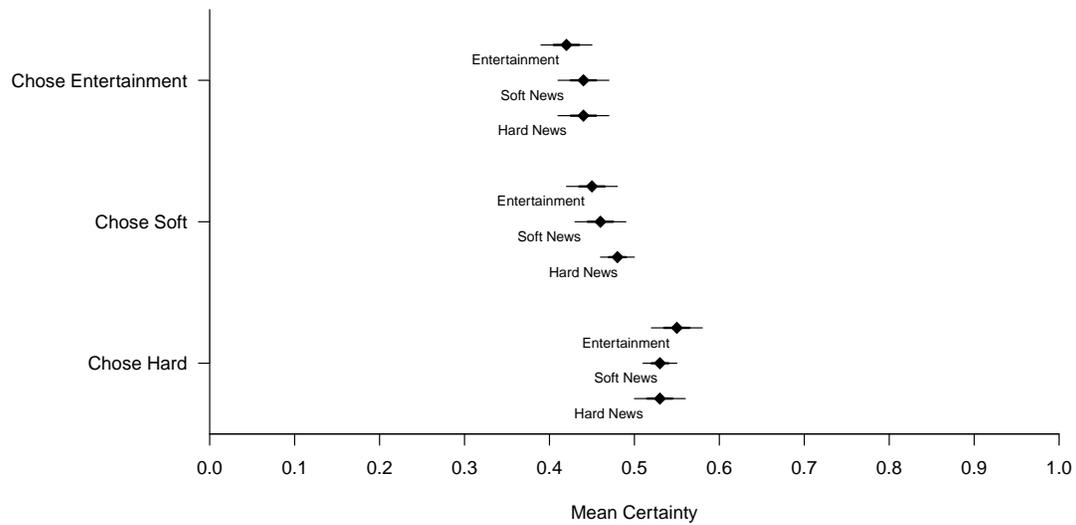
While opinions were relatively unmoved by byproduct exposure, respondents' certainty about their attitudes was influenced by the experimental stimuli. In the no-choice control conditions, treatment group means for attitude certainty in hard and soft news conditions were 0.52 (SE=0.03) and 0.49 (0.03) at T1, respectively, compared to only 0.42 (0.03) in the entertainment condition. These comparisons to the entertainment exposure appear to be statistically significant ($p = 0.01$ and $p = 0.06$), while the hard and soft conditions did not differ from one another ($p = 0.47$). These substantive differences remained at T2, with hard and soft news conditions remaining more certain about their attitudes than the entertainment condition ($p = 0.06$ and $p = 0.09$, respectively) but remained indistinguishable from one another ($p = 0.78$). The lack of responsiveness of attitudes to the news about the Syrian

conflict matched with an increase in attitude certainty around stable attitudes suggests that evaluations of that news coverage were largely reinforcing of respondents' pretreatment (and middle-of-the-road) viewpoints.

Yet these results differ dramatically from the results for those self-selecting into the three different types of news. The no-choice conditions showed that the experimental stimuli did — to some extent — have that effect under captive exposure. The figure below shows mean levels of attitude certainty (and associated standard errors) along the lines of the previous figure for opinion. The top panel shows the pattern of treatment group means at T1, while the lower panel reflects means at T2. The results just mentioned for the no-choice conditions can be seen in the right-most set of bars in each panel. Looking first at the results from T1, among hard news selectors, byproduct exposure to hard news increased attitude certainty 0.05 points over entertainment exposure ($p = 0.09$), but certainty among these respondents exposed to soft news was indistinguishable both from those in the hard-byproduct condition ($p = 0.65$) and the entertainment condition ($p = 0.23$). Among soft news selectors, there were no detectable gains in attitude certainty from exposure to either the soft or hard news stories ($p > 0.68$ in all cases). Similarly, among entertainment selectors (who again provide the cleanest test of byproduct exposure), there were no detectable increases in attitude certainty from byproduct exposure to either hard or soft news ($p > 0.59$ in all cases). Thus the only people who became more certain were those most likely to opt-in to political news.



T1



T2

These results mean that the pattern of apparent differences in attitude certainty among, for example, the three groups receiving hard news as a byproduct exposure (left three bars of the top panel of the above figure), such differences are not attributable to differential responsiveness to the stimuli but instead reflect pretreatment differences in attitude certainty that sustain even in the face of new information. The pattern of results looks substantively similar at T2, but with larger standard errors due to sample attrition. What small effects emerged at T1 had dissipated by T2. Finally, it is worth noting the contrast in apparent effects in the no-choice control conditions and in the self-selection conditions. Whereas the no-choice (or “captive”) conditions showed increases in attitude certainty due to issue-relevant information, once self-selection was involved, these effects are largely eliminated. While the more paradigmatic captive conditions showed an effect, the more realistic conditions of selective exposure showed no such pattern. The captive experimental results thus misled about the effects of byproduct exposure.

C Choicset Checks

One complexity of the experiment is the use of four different “choicset” conditions, in which some respondents received three choices (hard, soft, entertainment) and others received combinations of only two. Aside from adding complexity, it means that the outcomes of interest — namely, knowledge but also other factors (e.g., issue opinions) — may have been affected by the choicset itself. To check this, I performed one-way ANOVA analyses for three outcomes: knowledge, opinion, and attitude certainty, separately at Time 1 and Time 2. These tests tell us whether there was any variation in these outcomes directly attributable to the choicset (which would require the analysis to systematically control for choicset). The consistent finding is that there is no direct effect of the choicset on knowledge:

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.03	0.03	0.26	0.6108
Residuals	2209	235.65	0.11		

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.02	0.02	0.17	0.6785
Residuals	2209	227.78	0.10		

Nor are there effects on opinions about intervention in Syria:

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.01	0.01	0.33	0.5661
Residuals	2051	78.71	0.04		

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.09	0.09	1.50	0.2204
Residuals	1348	77.75	0.06		

Or certainty about those opinions:

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.07	0.07	0.77	0.3818
Residuals	2127	204.00	0.10		

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
choicset	1	0.01	0.01	0.09	0.7702
Residuals	1385	120.70	0.09		

D Descriptive Statistics, Preference Items

This appendix describes various descriptive analyses of the content preference items drawn both from the experiment and from surveys conducted by the Pew Research Center. I begin by examining preferences as measured in the experiment. As is clear from columns 1 and 2 of the table immediately below, the most popular content types were those related to national political news, news about science and technology, and health news. Columns 3 and 4 report two measures of over-time stability of these measures. Column 3 reports the simple test-retest reliability of each item, with correlations as low as 0.59 for health news and as high as 0.86 for sports news. Column 4 shows further, when the upper two categories and lower two categories of the preference measures are collapsed to dichotomize the measures, that 76%–90% of respondents reported the same preference (high or low) for each type of news in both panel waves. This over-time reliability suggests that individuals can articulate their preferences for different media content.

	<i>t</i> 1 Mean (SE)	<i>t</i> 2 Mean (SE)	<i>t</i> 1/ <i>t</i> 2 Correlation	Stability (%)
Business and finance	0.42 (0.01)	0.44 (0.01)	0.63	76.7
Local government	0.52 (0.01)	0.55 (0.01)	0.73	81.7
Entertainment	0.43 (0.01)	0.45 (0.01)	0.68	80.2
Science and technology	0.46 (0.01)	0.43 (0.01)	0.68	79.7
International affairs	0.34 (0.01)	0.35 (0.01)	0.70	85.4
Sports	0.48 (0.01)	0.47 (0.01)	0.62	77.3
News about political figures and events in Washington	0.32 (0.01)	0.32 (0.01)	0.86	90.2
Health news	0.54 (0.01)	0.53 (0.01)	0.71	82
Religion	0.45 (0.01)	0.44 (0.01)	0.62	77.8
Consumer news	0.50 (0.01)	0.49 (0.01)	0.59	76.6
Crime	0.45 (0.01)	0.43 (0.01)	0.53	71.8
People and events in your own community	0.23 (0.01)	0.22 (0.01)	0.66	86.4
Culture and the arts	0.40 (0.01)	0.39 (0.01)	0.66	79.4

Note: Results based upon respondents completing both panel waves ($n=1,395$). Response stability is calculated by collapsing the top two and bottom two response categories and

calculating the proportion of respondents falling in the same collapsed category at both $t1$ and $t2$. $t1/t2$ correlation is calculated on the original data.

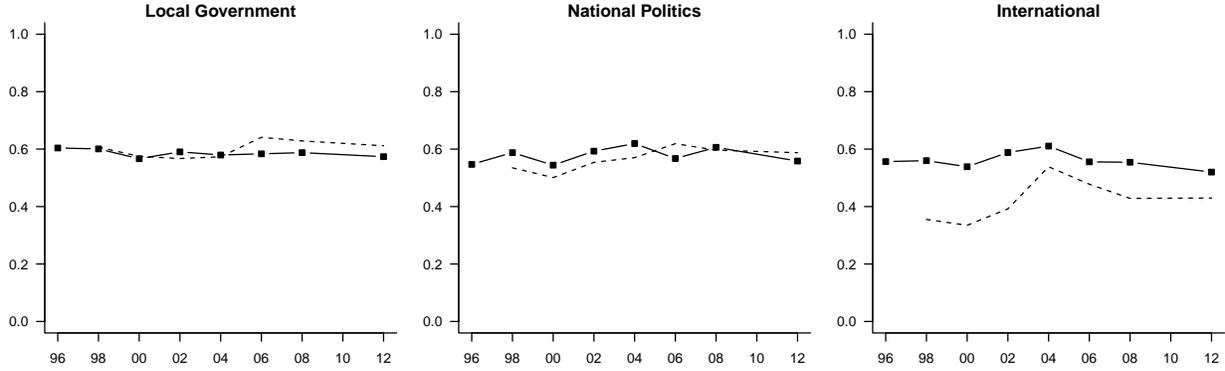
The table below reports descriptive means (and standard deviations) for these items as measured by the Pew Research Center on surveys from 1989–2012.

	1989	1996	1998	2000	2002	2004	2006	2008	2012
Local	0.69 (0.29)	0.60 (0.31)	0.60 (0.31)	0.57 (0.32)	0.59 (0.32)	0.58 (0.33)	0.58 (0.32)	0.59 (0.32)	0.57 (0.33)
National	0.71 (0.29)	0.55 (0.29)	0.59 (0.30)	0.54 (0.32)	0.59 (0.31)	0.62 (0.32)	0.57 (0.31)	0.61 (0.31)	0.56 (0.32)
International	0.57 (0.32)	0.56 (0.29)	0.56 (0.30)	0.54 (0.31)	0.59 (0.32)	0.61 (0.32)	0.56 (0.32)	0.55 (0.32)	0.52 (0.32)
Entertainment	0.64 (0.30)	0.53 (0.30)	0.53 (0.30)	0.51 (0.31)	0.50 (0.31)	0.50 (0.31)	0.45 (0.32)	0.43 (0.31)	0.43 (0.30)
Business	0.72 (0.29)	0.49 (0.32)	0.51 (0.34)	0.48 (0.33)	0.48 (0.34)	0.49 (0.33)	0.47 (0.34)	0.51 (0.34)	0.50 (0.34)
Community	0.75 (0.29)	0.69 (0.28)	0.69 (0.29)	0.63 (0.30)	0.66 (0.31)	0.64 (0.31)	0.63 (0.31)	0.60 (0.31)	0.62 (0.31)
Sports		0.52 (0.37)	0.52 (0.38)	0.49 (0.39)	0.48 (0.38)	0.48 (0.38)	0.45 (0.38)	0.44 (0.39)	0.48 (0.39)
Science	0.74 (0.28)	0.57 (0.31)	0.58 (0.33)	0.56 (0.32)	0.51 (0.33)	0.52 (0.33)	0.50 (0.33)	0.49 (0.32)	0.51 (0.33)
Crime	0.55 (0.38)	0.73 (0.28)	0.70 (0.29)	0.64 (0.30)	0.66 (0.29)	0.66 (0.30)	0.63 (0.31)	0.61 (0.31)	0.60 (0.33)
Health	0.57 (0.31)	0.69 (0.29)	0.70 (0.28)	0.65 (0.31)	0.63 (0.31)	0.63 (0.31)	0.63 (0.31)	0.58 (0.33)	0.59 (0.32)
Consumer	0.62 (0.35)	0.54 (0.30)	0.54 (0.31)	0.51 (0.31)	0.49 (0.32)	0.52 (0.31)	0.49 (0.32)	0.51 (0.31)	NaN (NA)
Religion	0.54 (0.34)	0.48 (0.34)	0.49 (0.34)	0.50 (0.35)	0.52 (0.34)	0.51 (0.35)	0.47 (0.35)	0.44 (0.36)	NaN (NA)
Culture		0.45 (0.31)	0.45 (0.33)	0.43 (0.32)	0.41 (0.33)	0.44 (0.33)	0.40 (0.33)	0.40 (0.33)	NaN (NA)

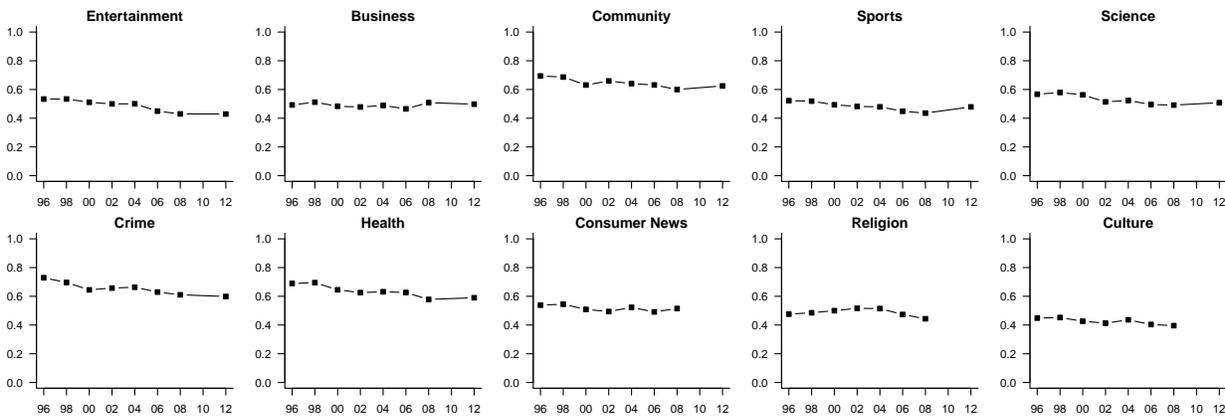
Note: Cell entries are means and standard deviations for responses to the content preference battery of items. The question wording was the same as in the online experiment, but for years 1989–2006 the question did not include “on the internet.” Empty cells reflect items not asked in a given year. Data for 1989 come from topline for a nationally representative Times-Mirror poll, August 9–August 28, 1989 (n=1,507).

D.1 Macro-level Preference Stability

As some initial description, the figure below shows mean ratings for local, national, and international political content preferences over the entire period 1996–2012. The dashed lines in each panel additionally show means for an alternative “habit” measure (described in Appendix D.3) across the period 1998–2012. As is clear in both measures across all three types of news content, preferences for exposure to different types of political news are incredibly stable over-time. This stability suggests preferences are independent of variations in the information choice set (i.e., the media landscape, which changed substantially over the 1996–2012 period with growth of cable news and entertainment, as well as the expansion of the internet).



The stability in preferences for political news also holds in preferences for other types of content.²⁷ The figure below shows means for the ten other types of news probed in the Pew surveys over the same period. Thus, despite a changing media landscape (i.e., changes in supply), preferences (or demand) for content have not shifted dramatically.



D.2 Preference Dimensionality

A focus on preferences for different types of news information also raises questions about the dimensionality of those preferences. Are preference for different types of news actually independent or do they reflect the single dimension implied by generalist models of political awareness? Correlation matrices for each of the content preference items from both the experiment and the Pew samples (for years 1996–2006) are shown below:

²⁷Note that consumer news, religion, and culture were not measured after 2008.

Content Preference Correlation Matrix (Experiment)

	Nat'l	Int'l	Ent.	Bus.	Comm.	Sports	Science	Crime	Health
Local	0.55	0.36	0.35	0.04	0.09	0.17	0.11	-0.09	0.22
National		0.43	0.34	0.06	0.12	0.26	0.06	-0.1	0.35
International			0.27	0.08	0.09	0.23	0.22	-0.06	0.26
Entertainment				0.5	0.36	0.32	0.09	0.08	0.19
Business					0.4	0.3	0.01	0.29	0.09
Community						0.37	0.06	0.29	0.11
Sports							0.02	0.18	0.44
Science								0.14	0.04
Crime									0.04

Content Preference Correlation Matrix (Pew Surveys)

	Nat'l	Int'l	Ent.	Bus.	Comm.	Sports	Science	Crime	Health
Local	0.46	0.37	0.1	0.34	0.48	0.16	0.25	0.31	0.31
National		0.53	0.1	0.37	0.26	0.16	0.3	0.27	0.28
International			0.06	0.39	0.2	0.13	0.4	0.23	0.28
Entertainment				0.07	0.2	0.18	0.08	0.24	0.21
Business					0.19	0.2	0.4	0.18	0.26
Community						0.14	0.14	0.35	0.3
Sports							0.14	0.15	0.07
Science								0.17	0.28
Crime									0.31

As should be clear, these items clearly do not correlate evenly, suggesting that preferences for news content is not unidimensional (i.e., preference for more news in general means preference for more of all types of news). Instead, individuals can favor one type of news content at the expense of other types. But even if the items as a whole do not scale, is there any evidence that they scale in some other way? The tables below show exploratory factor analyses using two- and three-factor solutions for both promax and varimax rotations using the experimental data:

2-Factor and 3-Factor Promax Rotations (Experiment)

	Factor1	Factor2	Factor1	Factor2	Factor3
Local	0.70	-0.09	0.73	0.02	-0.07
National	0.83	-0.10	0.74	-0.06	0.11
International	0.55	-0.01	0.49	-0.01	0.12
Entertainment	0.30	0.53	0.44	0.64	-0.11
Business	-0.14	0.79	-0.02	0.78	-0.02
Community	-0.02	0.60	-0.03	0.47	0.20
Sports	0.23	0.41	-0.03	0.10	0.77
Science	0.12	0.04	0.14	0.06	-0.03
Crime	-0.26	0.46	-0.27	0.32	0.18
Health	0.40	0.09	0.19	-0.13	0.55

2-Factor and 3-Factor Varimax Rotations (Experiment)

	Factor1	Factor2	Factor1	Factor2	Factor3
Local	0.67	-0.01	0.69	-0.02	0.11
National	0.80	-0.01	0.73	-0.04	0.27
International	0.55	0.05	0.50	0.02	0.23
Entertainment	0.45	0.54	0.51	0.57	0.04
Business	0.09	0.74	0.11	0.76	0.01
Community	0.15	0.57	0.09	0.54	0.20
Sports	0.35	0.42	0.14	0.37	0.70
Science	0.13	0.05	0.14	0.04	0.01
Crime	-0.12	0.41	-0.17	0.38	0.11
Health	0.43	0.13	0.27	0.06	0.54

The tables below repeat that analysis using data from the Pew surveys (1996–2006):

2-Factor and 3-Factor Promax Rotations (Pew)					
	Factor1	Factor2	Factor1	Factor2	Factor3
Local	0.26	0.49	0.21	0.71	-0.08
National	0.64	0.07	0.59	0.22	-0.08
International	0.81	-0.13	0.76	0.02	-0.09
Entertainment	-0.09	0.35	-0.14	-0.13	0.63
Business	0.60	-0.02	0.57	0.00	0.03
Community	-0.18	0.83	-0.15	0.56	0.29
Sports	0.14	0.14	0.12	-0.02	0.22
Science	0.60	-0.09	0.59	-0.15	0.11
Crime	0.05	0.48	0.02	0.16	0.44
Health	0.22	0.33	0.19	0.06	0.37

2-Factor and 3-Factor Varimax Rotations (Pew)					
	Factor1	Factor2	Factor1	Factor2	Factor3
Local	0.42	0.55	0.36	0.69	0.12
National	0.63	0.28	0.60	0.33	0.10
International	0.72	0.15	0.70	0.18	0.08
Entertainment	0.04	0.30	0.02	0.05	0.52
Business	0.55	0.18	0.55	0.15	0.14
Community	0.12	0.72	0.09	0.58	0.35
Sports	0.18	0.18	0.17	0.09	0.22
Science	0.54	0.11	0.55	0.04	0.18
Crime	0.22	0.47	0.19	0.30	0.44
Health	0.32	0.38	0.31	0.23	0.38

These solutions suggest that national/international and local content preferences seem to constitute different latent preferences, separate from preferences for other content. While a preference for entertainment has been emphasized in past work, these results also suggest that it is orthogonal to — rather than an opposite of — a preference for national politics. It is also particularly worth highlighting the relatively clear independence of preferences for national or international and local or community news. The relatively weak correlations between the national and local preferences highlight the underappreciated federal character of media exposure. For individuals to learn about and evaluate different levels of government

separately, they must recognize the distinctiveness of those levels and pay attention to them differently. As such, the study of media exposure might benefit from greater consideration of how respondents define ‘politics’ and better specify the content (rather than sources) of the public’s selective exposure to subsets of available political information.

Another obvious question relates to construct validity. Are content preferences a theoretically novel construct or are they merely a semantic innovation? The experimental survey additionally included a measure of general political interest. This item correlated strongly with national content preference, but had a weaker relationship with international content preference and a very weak correlation with both local content preference and community content preference.²⁸ “Political interest” as typically measured might therefore be understood as a preference for engagement with national politics, a measure which fails to capture the public’s concern for other aspects of politics. Such an interpretation would also be ecologically consistent with the evidence of stability in content preferences noted earlier and the stability of national political interest. That discrete choices seem to reflect stable differences in preferences for different kinds of information suggests simultaneous needs to understand the origins of these stable individual-differences and identify the contextual variations that interact with such stable preferences.

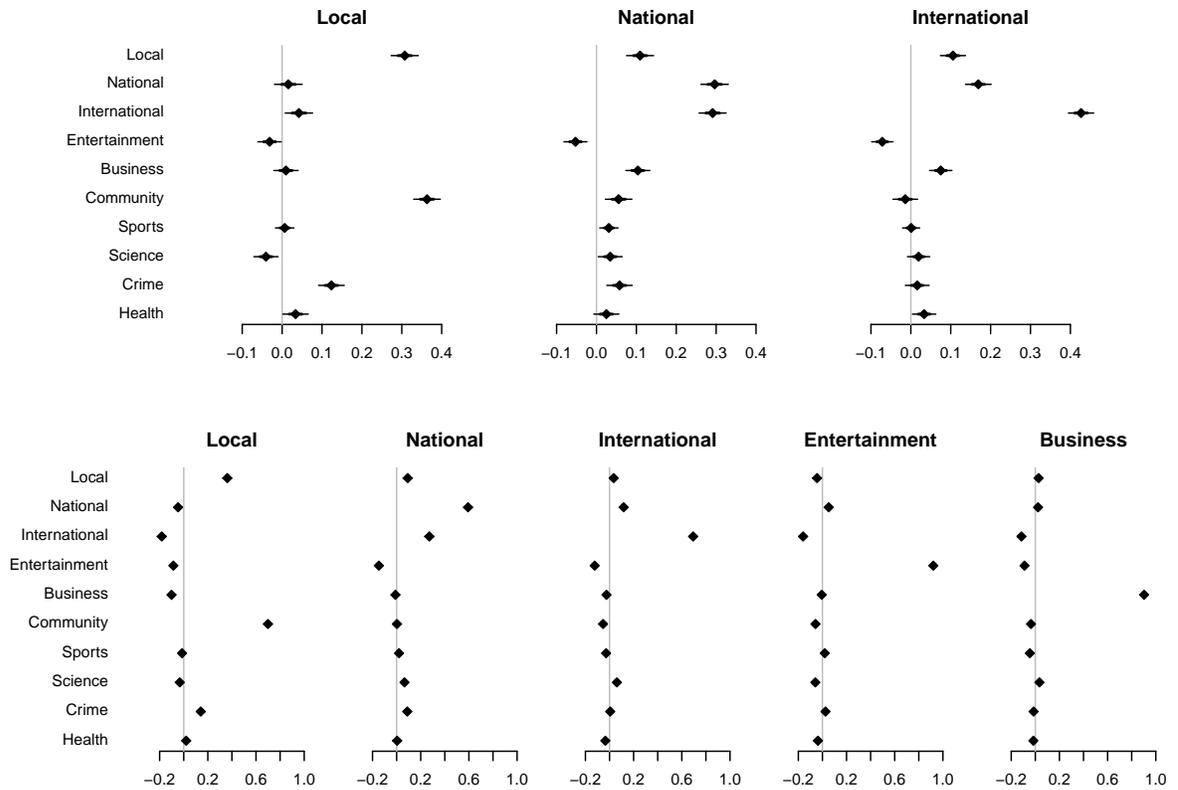
D.3 Alternative Measures of Content Preferences

Given any concerns about the operationalization of content preferences, it is worth asking whether the construct is robust to alternative operationalizations. The Pew surveys also asked a secondary measure regarding habitual attention to international, national, and local community news: “Which of the following two statements best describes you: ‘I follow international news closely *only* when something important is happening’ or ‘I follow international news closely most of the time, whether or not something important is happening’?” Respondents answers were coded 0 for agreeing with the former statement and 1 for agreeing with the latter statement.²⁹ Means for these measures were seen in the figures below. The online panel survey included both these three habit items as well as two additional questions probing habitual attention to entertainment news and business news.

²⁸The political interest question was phrased as “In general, how interested are you in politics and public affairs?” with responses recorded on the four-point scale used by the American National Election Studies. Correlations between interest and content preferences were as follows: National: 0.66; International: 0.49; Local: 0.28; Community: -0.02 ; Business: 0.32; Entertainment: -0.15 ; Sports: 0.10 .

²⁹Note that on the all the Pew surveys as well as the online panel survey, the habit measures were asked immediately after the content preference battery, which might have induced some consistency biases in responding.

Regressing these habit measures on the preference battery shows the extent to which the preference measures are robust to this alternative measurement. The figure below displays these regression results (points are regression coefficients with bars representing one- and two-standard errors). The top panel shows regression results for the first wave of the experiment and the bottom panel shows regression results for the Pew surveys (1998–2006). Year fixed effects for the Pew surveys are not shown.



In the top panel, the left figure shows regression results for local habit, with preferences for exposure to local government and community news (and to a much lesser extent, crime) uniquely predicting expression of habitual attention to local news. The middle figure shows a somewhat messier result with both national and international content preferences relating strongly to habitual national attention. The right figure shows international attention habit to be mostly shaped by a preference for international news, with smaller effects of preferences for national, business, and local news. For both habitual attention to national and international news, a preference for entertainment uniquely contributes a negative effect. The lower panel of the below figure shows identical results for the online panel. The two additional types of news asked on the panel survey (about entertainment and business news) also show this same pattern of relationships to stated preferences, suggesting the measures have both

convergent and discriminant validity with regard to peoples' preferences for different types of news.

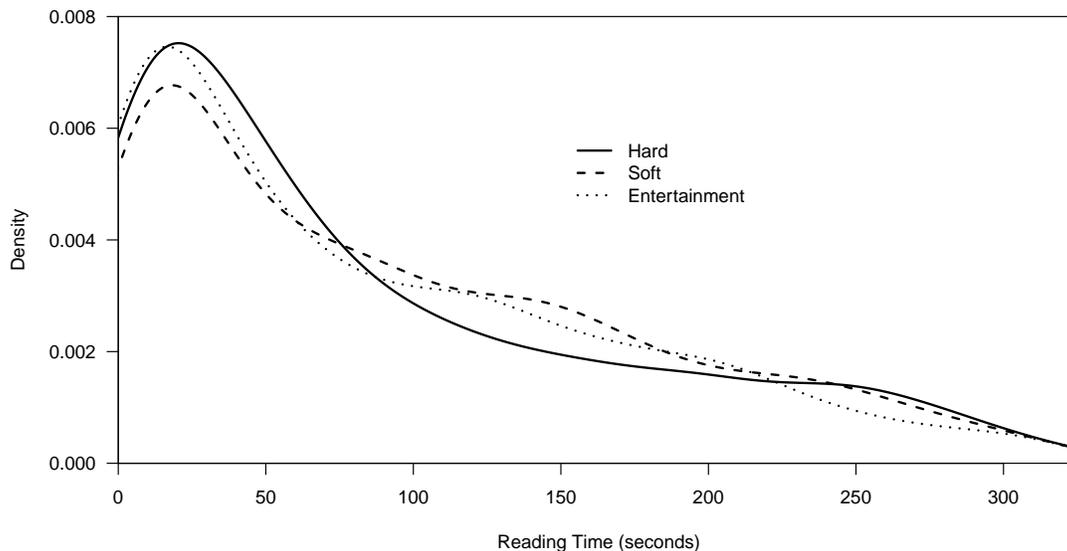
E Treatment Articles

Reading Times

A concern in any experiment is treatment compliance: that is, whether participants in the study actually received the treatment as assigned. The results reported in the body of the paper indicate that at least some participants must have received the treatment articles because there were consistent and sizable treatment effects. That said, one might still desire some kind of manipulation check. Given the focal outcome is issue knowledge, it is difficult to craft a manipulation check that is distinct from the outcome variable of interest.

It is possible, however, to examine reading times for the three target articles. The figure below displays kernel density plots of reading times for each of the three treatment articles (hard, soft, and entertainment). The data are trimmed of the top 5% of reading times due to extreme outliers (e.g., a very small number of participants leaving the survey open on the target page for several hours).

As should be visually apparent, reading times for all three articles were similar indicating that none of them was particularly more or less engaging or more or less difficult to read. Median reading time across all of the treatment articles was just over one minute on average (69 seconds) and mean reading times were somewhat longer (101 seconds). Given the length of the articles, this indicates that if participants in the study read at an average speed (something that we cannot test) the typical participant likely read about one-third to one-half of their assigned treatment article. That said, a sizable proportion of the participants spent upwards of three to five minutes reading these articles, which would indicate (again, at average reading speeds) that they read the entire article.



Another concern is that reading times might vary between conditions. For example, one might be concerned that the act of expressing a preference among the different sets of news articles might affect how long participants spent reading the manipulated article. This did not appear to be the case. Reading times were similar for those in the randomized

experiment ($\bar{x}_{Exp} = 100.6$ sec.) and the preference trial ($\bar{x}_{Exp} = 102.3$ sec.) and these times did not significantly differ from one another ($t = 0.33$, $p \leq 0.74$). There was some variation in reading times across those with preferences for different types of content ($F_{(7,2154)} = 7.30$, $p \leq 0.05$), with those preferring entertainment generally reading for less than one minute but conditional on choice there were no significant differences in reading times between those who received the news or entertainment treatment articles. This means the results are also unlikely to be due to simple procedural differences in which participants spent more or less time an article because it was unexpected.

Hard News Treatment Article

Rebels unite in fight for Syria's largest city

ALEPPO, SYRIA — Rebels have taken a major stride in uniting their ranks in the battle for Syria's largest city, giving them hope they could tip the balance after three months of bloody, stalemated combat in Aleppo, Syria's largest city and one of the biggest prizes of the civil war.

President Bashar Assad's government troops are retaliating against more effective rebel attacks with increasingly devastating bombardment, and civilians are bearing the brunt, with their neighborhoods left in ruins.

A new military council was announced Sept. 9. It brings together two of the biggest rebel players in Aleppo including the Free Syrian Army, the largest rebel group. The council should allow for better coordinated attacks against the 30 percent of the city still in regime hands.

The rebels have long been hampered by their division into dozens of competing groups, some with better links to funding and weapons, while others have more manpower.

"Before we made this council, the military aid used to come to just one man, and the people on the ground would get nothing. By forming this council, now aid comes to everyone, and everyone gets part of it," said Abdel Aziz Salameh, a former honey trader, based in the town of Tel Rifaat. He runs a network of fighters in the province, and described how assaults often had to be called off when his men ran out of ammunition after days of hard fighting.

No rebel group admits to getting weapons or ammunition from abroad. They say instead that they get funds from Syrians abroad and use it to buy weapons from smugglers. Their hesitance to admit to foreign assistance reflects an international stalemate over the Syrian conflict. Despite pressure from Western governments, the United Nations Security Council has yet to formally condemn the violence or call for international intervention. Russia and China have been outspoken in their opposition to a Security Council resolution.

The uprising against President Bashar Assad began in March 2011, when protests calling for political change were met by a violent government crackdown by government troops. Many in the opposition took up arms, and activists say more than 23,000 people have been killed. Daily death tolls now approach 200 and the last month was the bloodiest yet.

Salameh's one-time rival is Col. Abdel Jabbar Aqidi, a recently defected officer from Assad's military and the official representative of the Free Syrian Army. He received the lion's share of the funding from Syrians abroad, but did not have the manpower to take advantage of it. "Unity and coordination make us more effective in the revolution," he says.

The rebel assault on Aleppo, a city of about 3 million, began in July after the government crushed a similar attack on the capital of Damascus. In this case, however, the forces were more evenly matched and to the surprise of many, the outgunned rebels not only held on but expanded their hold on the city in fierce urban combat.

While the rebels are poorly equipped and lacking much organization, their successes have as much to do with their tenacity as the state of the Syrian army in Aleppo. The Syrian army, whose *raison d'être* until recently was a full scale tank war with Israel, is also not accustomed to counterinsurgency tactics in urban environments - something the rebels, for all their flaws, appear to have adapted to quite quickly.

Already there have been some successes, including the overrunning of a military barracks in northern Aleppo. The barracks was a key government bastion and part of a string of regime strong points being targeted by the rebels, including the military intelligence headquarters. The response, however, was swift — government artillery and plans unleashed a withering bombardment of rebel-controlled areas nearby.

“A pattern has emerged in recent weeks in areas where government forces, pushed into retreat by opposition forces, are now indiscriminately bombing and shelling lost territory - with disastrous consequences for the civilian population,” said the London-based Amnesty International in a briefing paper Wednesday.

Residents report the use of “barrel bombs” that appear to be large drums packed with explosives that can take down whole buildings.

Despite occasionally shooting down helicopters, most recently over Damascus on Thursday, the rebels are still struggling to confront this airborne menace for civilians both inside the city and in the country.

The lack of progress on the ground and civilian frustration has been part of the impetus for the rebels to put aside their differences and work together. Yet the new council includes just 80 percent of the estimated 8,000-10,000 rebels fighting the regime in and around Aleppo.

The powerful smuggler-turned-rebel-leader Abu Ibrahim, who controls the border crossing with Turkey through his Northern Storm brigade, declined to join with his 700 fighters.

“The rebels should be organized like an ordinary military, with the men totally under control of the leadership and each man knowing his role,” he said, dismissing Aqidi and Salameh as weak leaders with little operational control of their men.

When asked if he would eventually be part of the effort to unite the rebels, Aqidi only smiled and declined to comment.

Soft News Treatment Article

School starts in Syria, but thousands not in class

ALEPPO, SYRIA — Nine-year-old Rawan Mustafa knew she would miss school this year.

“I come here to find books to take them home and read — my sister helps me,” Rawan said Sunday, picking her way through the rubble of shattered walls, half-burned work books and smashed glass that was once her school.

Sunday was the official first day of school in Syria, but the country’s agonizing civil war between President Bashar Assad’s government and a loose coalition of rebel groups is keeping

thousands of students like Rawan out of classrooms in Aleppo — the nation’s largest city — and all across the country.

Many schools have been destroyed or occupied by refugees. Some parents are simply too afraid to send their children to school over fears of violence between Assad’s forces and rebel fighters, which has left more than 23,000 civilians dead. Still others are living in refugee camps outside the country with only limited access to an education.

The U.N. children’s agency says it is difficult to know precisely how many Syrian children have been out of school for an extended period due to the conflict, which started 18 months ago and leaves hundreds of civilians dead each day.

Dina Craissati, UNICEF’s regional education adviser, said hundreds of thousands of Syrian children who have been displaced from their homes are having difficulty accessing education. Outside Syria’s borders, the U.N. has registered more than 250,000 refugees, including children.

Some 2,000 schools have been damaged in the conflict and others are being used as sanctuaries for those displaced, Syrian Education Minister Hazwan al-Wazz said. Still, the government says 22,000 schools are operating and more than 5 million students attended school on Sunday.

Mohammed Rakani, a 10-year-old from the Damascus suburb of Sbeineh, was not among them. He is staying with his family at the Somayya al-Makhzomiya School in Damascus, which is sheltering more 300 people from 65 families.

“I want to return to my school, which I love so much. I am now in the fourth grade,” Mohammed said during a government-escorted trip to the school on Sunday.

In the north, where the opposition wields much more control, rebel officials say they are too focused on getting enough food and medical supplies into the country to concentrate on schools. There are also fears that any makeshift schools set up may attract airstrikes, which come daily.

“Our main focus is food, shelter and medical care right now,” said Seif al-Haq, a rebel with the rebel group the Free Syrian Army, which is responsible for civilian affairs in the northern city of Aleppo and the surrounding countryside.

“No schools are running in the liberated areas of Aleppo, they were all destroyed, but we are trying to run makeshift classes underground,” he said.

Another problem is that many teachers have joined the hundreds of thousands of Syrians who have fled the province. Standing amid the ruins of the school, Mohammed Ibrahim, 13, said his Arabic teacher Abdel-Razaq was his favorite because he never got angry. But Abdel-Razaq was gone now, along with half of Mohammed’s friends.

“It makes me sad that they are not here,” said Ibrahim, whose favorite subject is math because he said it makes his brain work. “It’s boring without them.”

Even in villages considered “safe,” schools aren’t opening, often because they are filled with refugees. In the town of Souran, the newly built school is still in perfect condition, but each classroom is now filled with a different extended family who sleep side by side on the floor on thin mattresses.

It’s not clear why so many schools have been hit in the fighting. In some cases rebels have apparently used the facilities, as may have been the case in Tel Rifaat where one classroom had some tattered military fatigues on the floor.

The only thing that seems likely to get children back to school is an end to the conflict. That, however, may be months away. Despite pressure from Western governments, the United Nations Security Council has yet to formally condemn the violence or call for international intervention. Russia and China have been outspoken in their opposition to a Security Council resolution.

“Today we should all be in school, but we can’t go because of the planes,” said Dargham Yassin, a bespectacled little boy who like many Syrian children looks much younger than his 12 years. Without school, he’s not sure how he will fill his days. “I’ll do nothing,” he said. “I’ll go feed the sheep.”

Syrians who have escaped their country to refugee camps in neighboring Jordan also lack access to education. At the Zaatari refugee camp in Jordan, two-thirds of refugees are under 18, while 5,000 are 4 and younger.

Save the Children and UNICEF believe part of the healing process for the camp’s children will mean going back to school. “They have been through horrific times and have seen extreme violence,” said Dominique Hyde, UNICEF’s representative to Jordan. “They need as soon as possible not only to come back to education but also to some sort of routine and stability. School provides one of the best ways.”

Entertainment News Treatment Article

Why the Oscars timetable change is bad for movies

LOS ANGELES — The Academy’s calendar changes that were announced on Tuesday — moving up the close of nomination voting from Jan. 13 to Jan. 3 and the noms announcement from Jan. 15 to Jan. 10, but moving back the start of phase two voting from Feb. 1 to Feb. 8, nearly a month thereafter — might not seem that significant at first glance. But make no mistake: they could have a major impact on awards season and on the viewing experiences of the Academy’s own members.

The Academy claims that it made these changes as part of “an effort to provide members and the public a longer period of time to see the nominated films” during phase two. This it will do. But what it left unsaid is that Academy members are less in need of a longer phase two, during which they are working from a short checklist of must-see films (the nominees), than they are of a longer phase one, during which they are ostensibly expected to scour the entire field of films released during the calendar year and pick those that are most worthy of being called Oscar nominees.

In recent years, Academy members have often told me that they barely have time to watch even the most high-profile films that they were invited to see at screenings or on screeners prior to the close of phase one, let alone smaller gems from distributors that lack the finances to compete with the majors when it comes to promoting films. Now, thanks to the shortening of the phase one period by 10 days, there will be considerably less time for those smaller films to break through the noise and catch on through word-of-mouth buzz. This will likely result in even more nominations for films from studios that can afford to spend big money during phase one on dinners, parties, advertisements and other forms of promotion.

I’m sure that this was not the primary intent of the Academy in making this decision. More likely, the board of governors wanted to try to grab back some of the thunder that has

been stolen in recent years by the ever-increasing number of awards shows and film festivals that have been scheduled during the run-up to the Oscars — among them the Indie Spirit Awards (first held in 1984), the Producers Guild of America Awards (first held in 1990), the Screen Actors Guild Awards (first held in 1995) and the Critics' Choice Awards (first held in 1996). Thanks to them, by the time the big show finally rolls around, much of the general public has already seen their favorite stars on one red carpet or another and can tell you which film or person is most likely to win the major awards because a pattern has already emerged.

The new Academy calendar does address that concern, to an extent, by putting the Oscar nominations announcement before the Golden Globes ceremony, and is so doing stealing some thunder from that show. But if the Academy wanted to minimize other shows, it could do so in a better way: namely, by moving the entire phase one and phase two up on the calendar (which would have drained those earlier events of much of their importance, since they'd either have to take place prematurely, before many contenders were released or, anti-climactically, after the Oscars had already taken place). The Academy also could have announced a change of plans months ago, rather than after many studios — large and small — had set end-of-the-year release dates for their contenders that now leave those films' Oscar prospects in jeopardy.

Just hours before the Academy's announcement, for instance, Universal announced that it had decided to push back the release date of its big contender, Tom Hooper's *Les Misérables*, from Dec. 14 to Dec. 25, reportedly because the studio hoped that opening it over the holidays would help it to find a larger audience amongst both the general public and Academy members.

Now, however, we know that voters will need to pack a ton of screenings into their holiday break if they are going to factor the end-of-the-year releases into their voting decisions before returning their ballots by Jan. 3. More than a half-dozen top contenders are set to be released around that time. I'm not sure that it's realistic to expect that many Academy members will get to all of those before the deadline. But I am pretty confident that the folks at Universal wish that they could take back that press release right about now.

The studios that locked themselves into December release dates are almost certainly going to have to do something that they really hate doing in order to make sure that Academy members have time to see their films — namely, send out screeners of and/or stream their films before they open theatrically. The studios and filmmakers associated with these films would much prefer that they be seen on the big screen, not only because they can be most fully appreciated that way, but also because private viewings exponentially increase the risk of someone pirating the film, which could severely hurt a film at the box-office.

But, really, what other choice do they have?

F Control Articles

The following sections describe the “control” articles used in the study. If a participant chose, for example, the entertainment news option, they were shown the three articles below in addition to one of the treatment articles shown in Appendix E.

Hard News Control Articles

New mileage standards would double fuel efficiency

WASHINGTON — The Environmental Protection Agency has finalized new fuel economy rules that will force automakers to nearly double the average gas mileage of all new cars and trucks sold by 2025.

The rules mean that the average mileage per gallon must hit 54.5 in 13 years, up from 28.6 mpg at the end of last year. The requirements will be phased in gradually between now and then, and automakers could be fined if they don’t comply.

The regulations, announced Tuesday, will change the cars and trucks sold in U.S. showrooms, with the goal of slashing greenhouse gas emissions and fuel consumption. Automakers will need to improve gasoline-powered engines, and sell more alternative fuel vehicles. Critics say the rules will make cars unaffordable by adding thousands of dollars to the sticker price.

The “Corporate Average Fuel Economy,” or CAFE standards, will vary by automaker depending on the mix of models they sell. The requirements will be lower for companies such as General Motors, Ford and Chrysler, which offer more pickup trucks. The standards can be lowered by the government if people suddenly start buying less-efficient vehicles in the future, although few expect that to happen.

The administration says the latest changes will save families up to \$7,400 on fuel over the life of a vehicle. The standards also are the biggest step the U.S. government has ever taken toward cutting greenhouse gas emissions, EPA Administrator Lisa Jackson said. Tailpipe emissions from cars and light trucks will be halved by 2025.

Already, automakers have committed to an average of 35.5 mpg by model year 2016 under a deal reached with the government three years ago.

In the arcane world of government regulations, the rules don’t mean that each new car or truck will get 54.5 mpg. The average vehicle will get closer to 40 mpg in real-world driving. Automakers will be able to sell pickup trucks and less-efficient vehicles as long as that’s offset somewhat by smaller vehicles that already can get upward of 40 mpg.

Automakers can reduce the mileage they’re required to get with credits for selling natural gas and electric vehicles, changing air conditioning fluid to one that pollutes less, and adding stop-start circuits that temporarily shut off the engine at stop lights.

Automakers have already been adding technology to boost the efficiency of gasoline-powered engines, mainly because people want to spend less at the pump. Fuel economy is the top factor people consider when buying a car in the U.S., according to the research firm J.D. Power and Associates. The national average for gasoline hit \$3.76 Tuesday, the highest price ever for this time of year.

Gas mileage has been rising for the past five years because government regulations and high gas prices have encouraged smaller vehicles and engines. The average new car now goes almost four miles farther on a gallon of gas than it did in October of 2007, according to the University of Michigan Transportation Research Institute.

Market demand for more efficient vehicles already has pushed the auto industry to boost mileage with an array of technology, said Roland Hwang, transportation director for the Natural Resources Defense Council. “We’re pleasantly surprised to see how fast the industry is moving,” he said.

The administration estimates that the new rules, combined with those that began in 2011, will raise the cost of a new car about \$2,800 by 2025. The estimates are based on 2010 dollars. But the government says the net savings from the requirements still will be \$3,500 to \$5,000 because people will spend less on gas.

The new rules were adopted after an agreement between the administration and 13 automakers last year. That’s a change from the past, when automakers fought the regulations, saying they cost too much.

Industry leaders repeatedly told the Obama administration that they wanted one nationwide fuel standard, fearing separate mileage standards from California and other states.

“They wanted certainty so that as they invest in the future they will know what rules they are playing by,” the EPA’s Jackson said.

Fuel economy standards were first imposed on U.S. automakers in the 1970s. The aim was to make cars more efficient and reduce the nation’s dependence on foreign oil at time when the Arab oil embargo was creating gasoline shortages. The administration says this is the first update in decades.

The National Highway Traffic Safety Administration will enforce the standards, calculating the average mileage of cars sold by each automaker. Automakers can be fined if they don’t comply. The requirements, which can be imposed without congressional approval, will be reviewed in 2018 and could be reduced if the technology isn’t available to meet the standards.

The rules are tough, but General Motors, the largest U.S. car company, will roll out features to comply, spokesman Greg Martin said.

“Consumers want higher fuel efficiency in their cars and trucks, and GM is going to give it to them,” he said.

Citing obesity link, NYC health panel backs ban on super-size sodas

NEW YORK — New York City will restrict sales of sugary soft drinks to no more than 16 ounces a cup in restaurants, movie theaters, stadiums and arenas after the Board of Health approved Mayor Michael Bloomberg’s plan.

The 8-0 vote with one abstention today by the panel’s members, who were appointed by the mayor and confirmed by the City Council, rejected arguments from Coca-Cola Co. (KO), PepsiCo Inc. (PEP) and restaurant companies whose coalition says the issue is about freedom to choose. The group’s website says it has attracted more than 250,000 New Yorkers opposed to the plan.

“This is the single biggest step any city has ever taken to curb obesity, certainly not the last step that lots of cities are going to take,” Bloomberg said at a City Hall news conference after the vote. “We believe it will save many lives.”

The rules are the latest of several Bloomberg nutrition initiatives during the past nine years, including a requirement that chain restaurants post calorie counts on menus. The health board and City Council banned artery-clogging trans-fats from restaurants and prepared foods, and the mayor blocked sugary soft drinks from vending machines in schools and city buildings.

“Obesity for the first time in the history of the world will kill more people this year in the world than starvation,” the mayor said. “It has gone from becoming a rich person’s disease to a poor person’s disease. It’s the only public health issue that’s getting worse.”

Restaurants, movie theaters and other outlets have six months to comply or face a \$200 fine each time there’s a violation, the health department said. The ban doesn’t apply to convenience stores and groceries that don’t act primarily as purveyors of prepared foods, which are regulated by New York state. The rules do allow consumers to buy as many of the smaller drinks as they want and to get refills.

A ban is “on-the-margin bad” for Coca-Cola and PepsiCo, Thomas Mullarkey, a Chicago-based analyst at Morningstar Inc., said in an interview before the vote. “But I don’t think it’s bad enough to move the needle on their stock prices.”

The American Beverage Association and the National Restaurant Association opposed the restrictions. Calories from sugary beverages as a percentage of Americans’ diets are declining, both groups say. U.S. soft-drink sales have fallen for seven straight years, according to Beverage Digest.

Yet the implementation of the ban comes on the heels of studies published last week in the New England Journal of Medicine that researchers say provide the strongest evidence yet that sugary drinks play a leading role in the nation’s obesity crisis.

“I know of no other category of food whose elimination can produce weight loss in such a short period of time,” said Dr. David Ludwig, director of the New Balance Foundation Obesity Prevention Center at Boston Children’s Hospital, who led one of the studies. “The most effective single target for an intervention aimed at reducing obesity is sugary beverages.”

Previous research on the subject has been mixed, and beverage makers fiercely contest the idea that a single source of daily calories can bear so much responsibility.

“We know, and science supports, that obesity is not uniquely caused by any single food or beverage,” said the American Beverage Association (ABA) in a statement. “Studies and opinion pieces that focus solely on sugar-sweetened beverages, or any other single source of calories, do nothing meaningful to help address this serious issue.”

A report released this week projected that at least 44 percent of U.S. adults could be obese by 2030, compared to 35.7 percent today, bringing an extra \$66 billion a year in obesity-related medical costs.

Sugary drinks are in the crosshairs because from 1977 to 2002 the number of calories Americans consumed from them doubled, government data show, making them the largest single source of calories in the diet. Adult obesity rates, 15 percent in the late 1970s, more than doubled in that period. The ABA points out, however, that consumption has since fallen, yet obesity rates keep rising.

The new studies, conducted separately by Boston Children’s Hospital, the Harvard School of Public Health, and VU University in Amsterdam, show unequivocally that the largest driver of increases in obesity and caloric consumption is sugary drinks, says NYC Health Commissioner Thomas Farley.

The panel's vote today followed three months of public feedback. The health department said it received 38,000 comments, with 32,000 in support and 6,000 opposed.

As board member Susan Klitzman, a professor and director of the Urban Public Health Program at Hunter College in Manhattan, put it, "To not act would be criminal."

New reports show rapid growth in six-figure student loan debt over past decade

NEW YORK — Very few college graduates have student loan debt exceeding \$100,000, but that doesn't mean we can't still learn from studying these rarities.

Mark Kantrowitz, Publisher of Fastweb.com and FinAid.org, published a new paper this week titled "Who Graduates College with Six-Figure Student Loan Debt?" It uses information from data analysis systems for the 1992-93, 1995-96, 1999- 00, 2003-04 and 2007-08 National Postsecondary Student Aid Study to look at students who graduate with more than \$100,000 in student loan debt.

Kantrowitz said news articles about students graduating with six-figure debt levels are alarming but not the norm.

"These stories have shock value and sensationalize the student debt problem, but the borrowers depicted in these stories are not representative of typical college graduates," Kantrowitz writes, going on to add "Nevertheless, much can be learned by examining extreme examples. Extrema can help identify the strengths and weaknesses of the student loan system."

The average student graduates college with around \$22,000 to \$27,000 in debt. A vast majority of the students with six-figure debt pursue various professional degrees; According to Kantrowitz, 36.2 percent of law school graduates and 49.0 percent of medical school graduates graduated with six-figure debt.

Nearly three-quarters of undergraduates graduating with six-figure student loan debt come out of non-profit colleges, 24 percent came from public colleges and another 3 percent came from for-profits.

The latest report from the New York Federal Reserve, the "2012 Q2 Quarterly Report on Household Debt and Credit," showed that while household debt is down nearly \$1.3 trillion since its peak in the third quarter of 2008, student loan debt continues to rise, increasing by \$303 billion over the same period.

Outstanding student debt stood at \$914 billion as of June 30, 2012, according to the New York Fed's report. Education loans held steady throughout the Great Recession, even as other consumer debts like credit cards and auto loans fell dramatically. Student debt takes up 8 percent at \$914 billion. In 2005, overall student loan debt was \$363 billion.

Delinquency rates also increased on student loans. The percent of student loan balances 90 or more days delinquent increased from 8.7 percent to 8.9 percent in the second quarter of 2012.

Unsurprisingly, students from high-income families are less likely to come out with these extreme student debt levels. Yet, low-income students are also less likely to graduate with six-figure debt levels than students from middle-income households. This is likely to have a connection to poor students being eligible for more need-based aid; including scholarships, subsidized student loans and Pell grants. A similar case plays out for veterans; presumably because they have generous benefits from the government to help pay for college, very few come out with six-figure debt.

Students who borrow from private student loan programs are more likely to graduate with six-figure student loan debt, Kantrowitz found. Private loans only make up about 15 percent of the student loan market, but these private lenders reported to be providing 592 percent more in 2007-08 than they did a decade earlier. Private loans were also flagged in a recent Consumer Financial Protection Bureau study as mimicking trends of the subprime housing crisis which helped bring about the Great Recession.

Kantrowitz's paper revealed a very similar trend in massive debt loads as with the overall student loan trends. An extremely small amount of students had six-figure loan debt, but according to the paper, no undergraduates had student debt levels that high until 2003. It has increased significantly over the past decade, which would follow federal reserve data showing the consumer student loan debt increasing 275 percent from 2003 to 2012.

Kantrowitz does have some suggestions for how to beat this extreme debt problem. For instance, federal and state governments need to stop cutting appropriations for higher education institutions and grant aid — it's putting the pinch on low and middle class families and harming government budgets in the long term.

"These problems manifest themselves first among low and moderate income students," Kantrowitz says. "Cutting federal and state support of postsecondary education is short-sighted. For example, people who have Bachelor's degrees pay more than twice as much federal income tax as people who have just a high school diploma."

Kantrowitz also recommends the U.S. Department of Education do a better job monitoring "basic heartbeat statistics" concerning student debt, and requiring more disclosure summarizing the loan repayment process. He also recommends limiting how much someone can take out in student loans based on the projected salary for their field of study. His paper found undergrads majoring in theology, architecture and history are "much more likely to graduate with six-figure student loan debt than students majoring in other fields of study." These are certainly not career areas that are expanding.

Soft News Control Articles

Actor Leonardo DiCaprio's motives for investing in Fisker Automotive

NEW YORK — Reclining on a low, dark blue sofa in a penthouse overlooking Manhattan's jagged skyline, triple-A-list actor, millionaire and eco-warrior Leonardo DiCaprio smooths his hair and exhales deeply as he remembers his first-ever car memory. "Oh boy..." he says.

A few weeks ago, DiCaprio took a financial interest in luxury car company Fisker Automotive, and we want to know why.

But before we get to that, first we want to know a bit more about Leo's car history. Like pretty much everyone else in the world, it starts modestly. When the young DiCaprio wasn't reading his dad's comics in the back of a knackered estate, he could usually be found under the hatch of his mom's silver Datsun 210. "We used to go everywhere in that car. I think she also had a Ford Pinto at one time, which is the eternal joke of horrible cars, right?"

Right. The energetic-sounding small Ford is remembered more for its ability to burn up its occupants than the road, thanks to a poorly positioned fuel tank.

But I start to have my doubts about Leo's car knowledge when he starts talking about how much fun it was powering the large, yellow Duesenberg around the Australian set of his current film project, a remake of *The Great Gatsby*. Duesenberg? I don't know much about F. Scott Fitzgerald's work, but if I know one thing for sure, Jay Gatsby didn't drive a Duesenberg. His car was big and yellow, but it was also definitely a Rolls-Royce Phantom I.

Deciding not to interrupt him, I make a note to check the film credits of the new movie later and carry on. But I have to tell you now he was right. The new film does feature a Duesenberg clone in the place of the old Rolls, so I take it all back and DiCaprio's score is unblemished.

When he's not powering around in murderously fast, classic car clones, DiCaprio says he likes to drive himself, not be driven, wherever he may be going. He also breaks the movie-star mould by not having a huge car collection at his home in LA. "I have my Fisker, obviously, and I have a Lexus hybrid, but I don't own anything else," he says. "Ever since I learned about hybrid vehicles, that's all I've really owned." He has a few motorbikes, he says, but nothing more.

The reason for this lack of extra driveway bling, is, of course, DiCaprio's desire to help the planet. Something that first stirred in him when then Al Gore invited him to the White House and gave him an environmental 101 talk. "That was the defining moment," he says. "That's when I wanted to know more about what I can do, how I can get more involved."

From that moment in the late Nineties, DiCaprio has been on a mission to do as much environmental work as possible.

The first the general public knew about his green credentials was Leo's appearance at the Oscars at the wheel of a Toyota Prius.

Little did he know that his appearance at the Oscars was the main reason Henrik Fisker started the company. "No...really?" he says incredulously, looking at Henrik sitting next to him. "Yeah," says Henrik. "I saw you getting out of the Prius, and I thought there's got to be a market for an environmentally friendly car which goes beyond the Prius. That was my first inspiration."

“Wow,” says Leo, looking genuinely surprised and pleased. “I had no idea. That is cool.” And it is. Here’s an activist actor trying to get a message across, who finds out his actions are the reason for the creation of a car company he likes and respects so much he has just invested in it. It could almost be a script for a film.

So what drew DiCaprio to Fisker in the first place? “I started trying hybrids, which were fantastic. But you can say that there are lots of vehicles out there that get equal or better mileage. Then I bought two electric vehicles, which I found I never wanted to drive. I was afraid of being stranded on the Pacific Coast Highway and standing at a restaurant for six hours while my vehicle’s plugged in.

“So the idea of this extended-range Fisker, which has the ability to be electric for a full day if you’re not doing a road trip, but also allows you to take a spontaneous road trip if you decide to...” And the looks must have helped. “Oh yes, it’s fantastic-looking. It drives like a sports car. It’s amazing. It was a natural progression. I wanted to be part of the company and invest in it.”

But, other than hoping to make a financial return on his investment, and have Fisker help to raise awareness of his Foundation, is there anything else he is hoping to achieve with the partnership? “I think the only thing would be to make cars that are lower in cost. And that’s coming soon with the Atlantic [Fisker’s upcoming smaller car]. Because you want everyone to be able to drive a vehicle like this.”

’The Biggest Loser’ To Feature Teen Contestants, Tackle Childhood Obesity

LOS ANGELES — For its 14th season, weight loss reality show “The Biggest Loser” is targeting a new demographic: overweight teenagers. The bootcamp-style weight loss program will be taking on childhood obesity for the first time — and not everyone is on board. The decision to include young contestants in the new season has incited strong reactions both for and against the idea.

Season 14 of “TBL,” which will premiere in January 2013, will feature three teams of six contestants each, one of whom will be a teen between the ages of 13 and 17. According to NBC, host Jillian Michaels’ goal with the new season is no less than ending childhood obesity.

“As a former overweight teen, I know firsthand how dramatically weight issues can affect every aspect of a child’s life,” Michaels said in a recent statement. “Having recently become a mother of two, I am more passionate than ever about helping empower children and families with the information and resources they need to live a healthier life.”

Michaels, who took a break from “Biggest Loser” to pursue adoption of a daughter, will join trainers Bob Harper and Dolvett Quince in working with the young contestants, with medical staff and childhood obesity experts also involved.

The hard-driving trainer, who also has an infant son with her partner, says it remains to be seen whether motherhood will alter her overall approach toward adults who are “killing themselves” with bad health habits.

“I am a different person, motherhood changes your DNA. However, being a mom and doing this job are very different aspects of my life and personality,” she said, adding, “I’m not going to make any promises.”

Michaels acknowledged that childhood obesity was a controversial topic which would need to be handled with care. In an interview with Al Roker, Michaels said:

“The producers of the show have been consulting some of the top experts — pediatricians, child psychologists — to help us deal with this in the most delicate and appropriate ways. For example, we won’t be saying things to kids like, ‘How much weight did you lose?’ It’s about getting them healthy, using words like ‘healthy.’ We won’t be getting them on a scale; it’s about getting them on a softball team — things like that. We’re very cognizant of how touchy it is, how controversial it is. And yet, of course, that’s right where I want to be, right in the sweet spot, right in the frying pan. I couldn’t miss an opportunity to be part of it.”

The show’s producers have stated that the teens will not be subject to the same tough-love tactics as the adult contestants. Moreover, their weigh-ins will not be featured on the show and they will not be subject to elimination. The focus of the teens’ transformation, Michaels commented, would be health — not weight or clothing size.

But not everyone is convinced. Many critics, some of whom are concerned parents, have expressed reservations about the exploitation of the teens on the show.

Though NBC and Michaels have expressed genuine concern about the rising childhood obesity rate, ratings are almost certainly a factor: Network brass announced in May that they were delaying *The Biggest Loser*’s 14th-season premiere until January 2013 to give the series “time to reboot in the face of declining ratings,” says Nellie Andreeva at Deadline. Michaels’ return to *The Biggest Loser* means that the series has recovered “one of its most recognizable stars,” and the new focus on child obesity has garnered media attention that *The Biggest Loser* has lacked in recent seasons. One looming question: Whether introducing contestants who are exempt from the rules will trigger a viewer backlash.

“Three overweight kids are about to be put out on a national stage for the rest of us to gawk at. As a former fat kid, I’m going to come right out and say it: this might just be the WORST thing you can do to your overweight kid!” wrote Jeanne Sager on Cafe Mom’s *The Stir* blog. “I can only guess these poor kids are in for a whole lot of public shaming.”

Teen obesity experts aren’t sold on the idea either. “I have concerns about this announcement,” says Dr. Rebecca Puhl, the Director of Research and Weight Stigma Initiatives at Yale University, in an interview with Hillary Reinsberg at BuzzFeed. *The Biggest Loser*’s traditional motivational tactics, which include yelling at contestants and encouraging exercise “to the point of illness and exhaustion,” are particularly dangerous for teenagers both physically and mentally. And even if the teens aren’t subjected to the same treatment, the show’s basic premise “still communicates that being a winner ultimately means losing the most weight.” If NBC really wants to help combat childhood obesity, they should “educate and promote activism” — not get teens involved in a weight-loss competition.

Economy leaves many returning students disappointed, deep in debt

LOS ANGELES — The weak economy and high unemployment have prompted many adults to head back to the classroom, armed with the promise that more education will bring them a higher paycheck and increased job security.

But now, some are learning the hard way that just earning a degree isn’t a guarantee of a good paycheck — or any paycheck at all — when the job market is so difficult.

“They get trapped in jobs that are little bit lower status, a little bit lower paying, than they might have been,” said Don Hossler, an education professor at Indiana University in Bloomington.

The situation can be discouraging for people who held steady jobs when times were good

and now are armed with a degree and few good job prospects. That is the situation facing Lewis Lemons III.

In 2006, Lemons made a decision that seemed to make sense at the time: He quit his \$20-an-hour job to go back to college with the hope of moving up the economic ladder. As a returning student, he had plenty of company. Between 2000 and 2010, there was a 42 percent increase in students over age 25 enrolling in postsecondary programs, according to the Department of Education. That compares to a 34 percent increase in enrollment of students between the ages of 18 and 24.

Six years later, Lemons has an undergraduate and graduate degree and is close to getting his MBA. He also has about \$80,000 in student loan debt and, after a stint of unemployment, he just landed a contract job — at \$18 an hour.

In retrospect, he says quitting his job to go back to school “was the worst decision I ever made.”

Lemons, who is now 32 and lives in Riverside, Calif., was working for a big health care company when he decided that he wanted to go back to school to study psychology. Although the job was steady and the benefits were good, he saw no career path in it.

“I figured, I don’t want to be making \$20 (an hour) for the rest of my life,” he said. “But look where I am now.”

He graduated with a psychology degree from UC Riverside in 2009 as the recession was officially ending and the unemployment rate was topping 9 percent. He soon landed a job doing social work, which he loved, even though it paid less than he’d been making before he went to school. Then he lost his social work job in 2011.

Lemons said he doesn’t blame anyone for his decisions. Still, it’s hard to accept that he will may never own a home or be able to help his kids pay for college because of the burden of his own \$80,000 in student loan debt.

Lemons’ debt is higher than many others’. But the nation’s rising levels of student loan debt has started raising alarm bells for some economists, who worry about the long-term effect that debt burden will have on their ability to do things like buy homes and cars, and retire.

By some government estimates, the nation’s total student loan debt burden now tops \$1 trillion, more than Americans’ credit card debt.

On an individual level, people who go to for-profit schools such as DeVry and the University of Phoenix are much more likely to borrow, and debt loads are much higher. They also are much less likely to complete their degrees.

Many see the for-profit schools as a convenient way for older students to earn a degree while working during the day or attending to family needs. But lately the programs have come under more scrutiny.

About 65 percent of people at private, nonprofit schools, and 56 percent of people at public colleges, graduate with their bachelor’s degrees within six years. Just 28 percent of students at private, for-profit schools complete their degrees in that time period, according to government data.

Hossler, the Indiana professor, said student loan debt is a thorny issue. Many people agree that the nation needs to get a better handle on ballooning debt loads. But if there are more rigorous thresholds for who can take on such debt, then there is the risk that low-income Americans will be excluded from the option of bettering themselves through

education. That's a major tenet of the American dream.

But that dream is now permanently out of reach for Lemons, whose bouts of joblessness and low-paying jobs have put a serious strain on his finances. He has had to file for bankruptcy and was forced to leave his apartment in July to avoid eviction.

He and his two boys, who are now 9, have been staying with his mom and his sister or at a hotel until he can get back on his feet financially. He's even had to accept food stamps. "It's embarrassing to say, but I went from being a social worker to being on the program," he said.

Entertainment News Control Articles

Looper: A futuristic thriller worth seeing

LOS ANGELES — A time-travel twister that pits a ruthless hit man (Joseph Gordon-Levitt) against his future self (Bruce Willis), “Looper” marks a huge leap forward for Rian Johnson (“Brick”). His grandly conceived, impressively mounted third feature shows a giddy, geeky interest in science-fiction, then forces it into the back seat and lets the multidimensional characters drive. In a genre infamous for loose ends, this thinking man’s thriller marshals action, romance and a dose of very dark comedy toward a stunning payoff. Reception should be solid, not stellar, with a long cult afterlife.

In the future, mobsters dispose of unwanted rivals by sending them 30 years back to the past, before time travel has been developed, and into the hands of a team of young screw-ups called “loopers” to do the killing. Why loopers? Because sooner or later, these live-in-the-moment assassins will wind up killing their time-displaced selves — or “closing the loop.” They’re rewarded, handsomely, and life is sweet until . . . well, until time travel is invented and they get booted back to face the barrel of their own blunderbusses.

You don’t have to be Albert Einstein to know that sending assassins back to the past is a bad idea — not for a movie, but as a system of gangland garbage disposal. Kick your unwanted trash into the future, and you’re rid of it, but blast a career killer back in time, and there’s a pretty strong chance the death-marked assassin will irrevocably alter the “future” from which he came if he can manage to escape.

That loophole, big enough to drive a plot through, is precisely what makes Johnson’s crazy idea work. Joe, played by Gordon-Levitt with pale blue contacts, puffy lips and a fake schnozz that takes some getting used to, is pretty unconflicted about killing strangers from the future, himself included. But when Older Joe arrives in the form of grizzled action star Willis, his 30-years-younger self flinches just long enough for the guy to get the upper hand, knocking Joe unconscious before disappearing into his own past.

Now, here’s where things get fun for the kind of sci-fi crowd that likes to diagram and debate the logic of time-travel stories. You’d think that Older Joe has the upper hand, able to anticipate the way his younger self reacted, but as cat-and-mouse games go, the young punk has a distinct advantage, since the slightest injury to Gordon-Levitt’s body travels forward to appear as scar tissue on Willis.

The pic demonstrates just how this works with Joe’s sidekick Seth (Paul Dano). After purposefully allowing his older self to escape (or “letting his loop run” in the parlance), Seth hides out at Joe’s place — not a smart idea, considering that Joe prizes money over friendship, and doesn’t put up much resistance before surrendering Seth to the syndicate chief (Jeff Daniels, whose blood runs cold behind a bearded smile). What follows is a truly disturbing death scene, as Seth’s loop (Frank Brennan) tries to hop the nearest train, only to see 30-year-old injuries start to appear all over his body, the result of the younger Seth being sadistically tortured offscreen.

Kill the kid and his loop goes, too — a rule that puts Older Joe in the awkward position of simultaneously having to run from, and protect, his younger self. Trickier from a storytelling standpoint is the fact that viewers don’t meet Willis until the first-act break, at which point the film must rapidly supply a romantic backstory for a character who, in the present reality,

technically does not yet exist. So, while Gordon-Levitt's Joe is a heartless hustler, Willis' older-and-wiser counterpart brings soul to the character, having discovered — and had to watch die — the love of his life. Willis can play the tough guy in his sleep, but it's the character's tenderness that makes possible the ruthlessness with which he sets about trying to change his own fate.

Complicated as it all sounds, Johnson paves the way with wall-to-wall voiceover. As in "Brick," the script's well-tooled lines are stilted enough to sound cool, and angled in the direction of comedy, relying on expressions less suggestive of a sci-fi future than they are of vintage film noir. Face-to-face with himself, young Joe hisses, "Why don't you do what old men do, and die?" For both thespians, the challenging roles amount to playing near-nihilism, while also subtly absorbing one another's characteristics.

The two actors look nothing alike, of course, which wouldn't be a big deal, if Johnson hadn't tried so hard to force a resemblance, burying Gordon-Levitt's striking mug under prosthetics (the most distracting being an application meant to simulate Willis' unique beak) instead of simply trusting the audience to care enough about Joe to see past the differences. The support team leverages Louisiana to suggest a 30-year-distant Kansas (and Shanghai for locations 30 years farther down the line) without requiring too many effects, though the digital work looks convincing when needed. If the imperfect yet promising "Brick" teased an exciting new voice, then "Looper" suggests big things ahead.

Five new shows you can't miss this fall

"The Mindy Project" "The Mindy Project" is the kind of show where Barbies talk, women make fun of kids and Bill Hader is the hottie the lead wants to marry. So, not typical. Nor is the lead. Mindy Lahiri (Mindy Kaling) may be attractive, charming, hilarious, great at her job, and has amazing friends, but her obsession with rom-coms and crappy taste in men makes her someone we can all identify and sympathize with. Kaling (writer, producer, co-star of "The Office") is delivering the comedy that everyone should be watching this season. "The Mindy Project" is as self-deprecating as her book, *Is Everyone Hanging Out Without Me? (And Other Concerns)*, and I don't think there's a woman — or man, even — around who can't relate to this frank and funny sitcom.

Why you should watch: It will make you laugh out loud. A lot. And that's all you can ask for in a comedy, right? Plus, it's teamed with "New Girl," making it arguably the most hilarious one-two punch this fall ("Happy Endings" and "Don't Trust the B in Apartment 23" comes in at a close second). And aside from Mindy, the series boasts one of my favourite new characters of the season: Danny Castellano. Trust me. You'll love him.

"Revolution" What would I do without the laptop I'm furiously typing on, a television to watch these shows on, my iPod to listen to my favourite songs, my iPad to play Words With Friends on, and my BlackBerry to check my emails and BBM friends with? Well, that's what happens in "Revolution," when all the electricity in the world shuts down and everyone is thrown into the Dark Ages 2.0. It relies heavily on flash-forward elements — which may or may not be your thing — but I dig the concept because a straightforward tell right from the beginning when the power shuts off would've been too tedious.

Why you should watch: It comes from J.J. Abrams ("Lost," "Fringe") and Erik Kripke ("Supernatural"), both of whom have the chops to make "Revolution" a fun ride. And unlike what I thought before I watched the first episode, "Revolution" has the makings of being

like “Lost,” not “Flash Forward.”

“Nashville” Even having not seen 2010’s “Country Strong,” I knew when I saw the previews for “Nashville,” it was basically a rip-off of the 2010 movie about an aging country star and a hot, young up-and-comer, with Connie Britton in Gwyneth Paltrow’s role and Hayden Panettiere in Leighton Meester’s. But that’s OK. Britton is fabulous as Rayna Jaymes, a legend who refuses to believe her time is up, while Panettiere is perfection as Juliette Barnes, an ambitious, entitled, sluttier Taylor Swift with a secret. Throw in the rest of the characters — those in the industry, those trying to make it in the industry, and Rayna and Juliette’s families — and what we’re left with is a gripping, well-rounded drama.

Why you should watch: While there’s more to the show than the women’s rivalry, it still is the main reason to watch. They may share the same twang, but Rayna is no Tami Taylor, and Panettiere is perfect as the bad girl who you just know we’ll feel sorry for in the coming weeks, even though I want to keep hating her. Oh, and country music may not be your thing, but the songs are sweet. “Glee” and “Smash,” watch out.

“The New Normal” It’s simple — if a show makes me laugh, out loud, more than twice, it has me. “The New Normal” is the latest from Ryan Murphy (who continues to wow me with “Glee” and freak my freak with “American Horror Story”), and sets out to show that couples of all colours, sizes and sexes want to have families, too. The premiere manages to be both sweet and mean, thanks to Goldie Clemmons (Georgia King), who may be the nicest woman (with a backbone) on television, and her grandmother, Jane Forrest a.k.a. Nana (Ellen Barkin), who makes Sue Sylvester seem caring and compassionate.

Why you should watch: Nana’s one of my new favourites of the season (can she, Danny Castellano and “New Girl’ ”s Nick Miller please hang out?), but I love the adorable, relatable, believable wannabe dads, Bryan and David. Andrew Rannells and Justin Bartha are perfection and I want to hang out with them. Well, go shopping with Bryan and watch the Super Bowl with David.

“Arrow” The pilot may be one of the best (it’s visually stunning, and its sheer entertainment factor and action scenes will have you gripped and on the edge of your seat), and shouldn’t be shrugged off as just another comic book series. Oliver/Arrow is a man trying to fix his corrupt city with his slick bow-and-arrow prowess, a hood and some raccoon-like makeup. Sounds cheesy, but it’s not. Not only can he physically beat the baddies to a pulp, but his archery and parkour skills would make Katniss Everdeen and “New Girl’ ”s Schmidt jealous with rage.

Why you should watch: Not only is Amell hot (like, haaaaawwwwt), he has the acting chops to back it up. Plus, there’s something very Bruce Wayne about Oliver Queen — always a good thing. And the series comes from Greg Berlanti (“Political Animals,” “Everwood”), David Nutter (“The X Files”) and Andrew Kreisberg (“Warehouse 13”), three savvy dudes in the industry.

‘No Doubt’ back with new album

LOS ANGELES — Most albums aren’t recorded overnight; just ask Steely Dan or Bruce Springsteen. However, No Doubt’s upcoming Push and Shove, out September 25th, is one of the longest-gestating pop albums in recent memory. According to bassist Tony Kanal, it’s a triumph that the album — the band’s first since 2001’s Rock Steady — was completed at all.

“We always knew it was going to happen,” Kanal says, “but there was a lack of clarity as to when it would happen. We went through a period when we thought, ‘Wow, will this ever get done?’ ”

Gwen Stefani hates it when people point out that No Doubt haven’t released an album in 10 years. “A lot of stuff happened during that time period,” she says. “Marriages, babies and, for me, two records and two clothing lines. So if you really worked out the math, you’d be like, ‘Wow, you guys are going fast.’ ”

The winding road to Push and Shove began in 2007 while lead singer Gwen Stefani was touring behind her second solo album, *The Sweet Escape*. In June, during the encore for one of her shows in Irvine, California, the other members of No Doubt joined her onstage for a four-song set that included their hits “Just a Girl” and “Hella Good.” “The response was so amazing that we said, ‘We have to start working,’ ” explains Kanal.

Not long after the show, the band convened to start writing new material, but Stefani was still exhausted from touring and was expecting her second child. The band was also hit with collective writers’ block. Those songwriting sessions and further ones in 2008 yielded little, and the band decided to give it a rest. “We know when it’s right and when it isn’t,” Kanal says. “We had to get that feeling again.”

Things only began clicking again in November 2009 after the band’s successful summer reunion tour. Kanal says he was skeptical of hitting the road and performing only old material, but the band was so energized afterward that they decided to give the new album another try. One song, “Undercover,” finally emerged, but work still progressed slowly throughout 2010, since Stefani was now the mother of two and only available three evenings a week. “We’d order in food and spend the first hour talking about life and kids,” Kanal says. “Between us, we have nine kids. But it was like, ‘This is fun again.’ ”

The band reconvened in November 2009, writing from around 4 p.m. to midnight in Kanal’s studio (featuring a view of Hollywood that Dumont describes as “distractingly gorgeous” — Kanal had to install curtains). Working together again just felt right. “I had gone on my tortured journey of working with outside songwriters,” Stefani says. “I would throw up if someone made me do that again.”

“Writing with other people is a learning experience,” Kanal adds, “but when you come back, and you’re sitting in a room together, it feels like home.”

No Doubt obsessively polished each song, Kanal says. “We would say, ‘OK, the B section isn’t good enough, let’s rewrite that,’ until we felt the song was great.” After laboring through 2010, they had nine songs (“Ten if we write the chorus for that last one,” says Kanal).

Finally, earlier this year, the band spent five months focusing on new material with producer Spike Stent (whose resume includes Oasis, Madonna and Coldplay). Even then, the band frantically rewrote and tossed out material. One track, “Back in Love,” was almost scrapped until the band realized the intro could make a terrific chorus, resulting in an entirely different dance song, “Looking Hot.” When the album was almost done, Stefani suggested they needed a ballad, and out came “Undone” — but again, only after it was nearly abandoned. “Gwen went on a family vacation in England and emailed me from there and said, ‘I think there’s something here with this song,’ so we went back to work on it,” says Kanal.

The band’s love for reggae and dancehall still comes through in the lead single, “Settle

Down,” and the title track (which featured a verse from Jamaican dancehall singer Busy Signal). “We want to sound modern, but we’re still influenced by ska, reggae and Eighties UK bands,” says Kanal. “There’s no way to escape who we are.” Kanal calls “Heaven” the band’s “homage to OMD, Depeche Mode and the Cure — the stuff we love to reference.”

“One More Summer” updates the band’s ska-pop sound with pounding dance beats, arena-size guitars and a beyond-catchy chorus — “One more summer/One more weekend/I’m your lover/You’re my weakness.” Drummer Adrian Young tells his bandmates he can’t wait to play the song live: “I might lose control.”

The one thing Push and Shove doesn’t have are leftovers for future deluxe editions. “In the past, we’d write 20 songs for a record,” Kanal admits. “This time, our time was limited. So, no, no outtakes!” More seriously, Kanal adds, “For everyone, taking that long break was very healthy. Gwen needed to get that out of her system. She couldn’t have made those [solo] albums with No Doubt. And we’ve learned that if you try to force it, it doesn’t work with this band.”

“It’s so exciting to have a record coming out,” Stefani says, almost vibrating with glee. “And we all want the same thing: for it to be modern and catchy and addictive. Once you make music that connects with people, it’s like you taste blood — you can’t go back.”