Identities and issue opinions

Learning from climate change

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Abstract

In this dissertation, I explore the formation of issue-specific opinions, in particular public opinion about climate change in the United States. More specifically, I analyze whether people use social groups and identities as mental "shortcuts" in order to form an opinion about complicated political topics such as climate change. I study three identity-related factors that may drive people's opinions about particular issues: partisan media content; the interests of social in-groups; and opinion cues from fellow partisans. Overall, I find that partisan identities are likely to have important effects through the media content that they expose Americans to. Other, more direct pathways for the opinion effects of identity, however, turn out to be surprisingly weak. I find no evidence that Americans' opinions are motivated by the material interests of their in-groups; nor that Americans change their opinions to align with the consensus among their in-party members.

In chapter 2, I ask what strategies partisan media use to fit real-world events into ideological narratives. I look at whether or not they connect events to related political issues (e.g. hurricanes and climate change), and whether each side is able to fit events into its existing set of issue positions. Using natural language processing and crowd-sourcing, I analyze almost 2 million hours of radio from hundreds of talk shows. I find that in the aftermath of an event, both ideological sides give far more attention to related political issues. At the same time, there are huge gaps between the positions that liberal and conservative shows tend to take on those issues, and events they do very little to close those gaps. Events turn up the volume of the discussion, without changing its ideological tune. This way, shared experiences could be turned into polarizing

factors.

Next, in chapter 3, I investigate whether people change their attitudes about societal issues when they learn that those issues affect others like them. In three pre-registered survey experiments, I find that these in-group interest cues have little to no effect on issue-specific attitudes. This is true for social groups based on gender, race/ethnicity, and sexual orientation. People who closely identify with an in-group do not react more strongly to the group interest information. The findings raise new questions about exactly when and why people's group memberships influence their political attitudes.

Finally, in chapter 4, I ask whether people change their opinion when they learn the distribution of opinions among members of their own party (or of the out-party). I also compare the effect of these "mass cues" to the effect of elite cues—information about politicians and their stances on an issue. I run two pre-registered survey experiments—one national, and one on an Amazon Mechanical Turk convenience sample—and draw two unexpected conclusions. First, I find that mass cues have no noticeable effect on opinions. When participants learn that a stance is shared by almost all members of their in-party, they do not move their own opinion closer that stance. Neither are they affected by learning about consensus among the out-party. Second, I am unable to replicate the well-established effect of elite cues. Combined with a closer inspection of the literature on cues, these findings suggests that cueing effects might be quite context-dependent.

Chapter 1

Introduction

Democrats and Republicans are drifting further and further apart in their average opinions about climate change (Pew Research Center, 2016). In fact, highly educated partisans differ more in their understanding of climate change than party members with less education (McCright and Dunlap, 2011; Hamilton, 2011). Studies on the effect of messaging on conservatives' climate beliefs tend to find that those messages are most effective if they build on conservative values or ideology (Campbell and Kay, 2014; Feinberg and Willer, 2013). But party affiliations are not the only group identities connected to climate opinion—for example, Evangelicals and other traditionalist Christians are less in favor of government action against climate change, even though they are no less likely to see environmental degradation in general as a problem (Sherkat and Ellison, 2007).

While these findings about climate opinion are valuable pieces of information by themselves, they are also mirror images of general trends in US public opinion. Partisan sorting, especially among the highly educated, exists on many topics besides climate change (Hetherington, 2009; Levendusky, 2009; Drummond and Fischhoff, 2017). Persuasion in general is more effective when the arguments are in line with a person's existing beliefs or values (Campbell and Kay, 2014; Feinberg and Willer, 2013; Slothuus, 2010). And Barker and Bearce (2013) connect Evangelicals' limited concern about climate change to their end-time theology (belief in the Second Coming of Jesus), which also makes them less worried about other long-term issues such as government debt.

In other words, findings on climate change attitudes so far are in line with broader knowledge about public opinion in the US. This suggests that studying how climate opinion is made can teach us about mechanisms that apply to public opinion in general. Each of the three projects in this dissertation is inspired by an observation about climate opinion formation that hints at something broader about how issue-specific attitudes are made. In each case, I hypothesize a mechanism, and then apply it to attitudes on climate change as well as other political topics. These mechanisms all center around the use of groups and social identities as cues that help people form opinions about political topics on

the basis of limited information. These identities can be party affiliations, but also demographics such as race or gender.

In one project, I analyze the content of partisan media, to better understand how partisan identities come to be connected to opposing issue positions. In the other two projects, I use survey experiments to randomly provide people with information about the association between their social identities and (some stance on) an issue. Both angles—observational and experimental—are needed to help us see the mechanisms behind public opinion formation.

The first project is connected to an intriguing observation made by Usry et al. (2019) in the aftermath of hurricane Florence. In the fall of 2018, this hurricane hit the coast of North Carolina hard, causing heavy flooding, tens of billions of dollars in property damage, and several dozen deaths. A little less than a year earlier, Usry et al. had polled North Carolinian voters about their climate opinions, and two weeks after the storm, they were able to field another poll. The researchers found that most voters became more concerned about climate change after the hurricane. However, highly educated and strongly partisan Republicans became more *skeptical* about climate change.

Because educated partisans are more likely to consume partisan media (Stroud, 2011), one possible explanation for this pattern would be the content of conservative media. To explore this possibility, in this project, I look at how political talk radio shows react to major events. Analyzing almost 2 million hours of radio, I find that even in partisan outlets, events have the power to draw attention to related political issues. The positions that liberal and conservative shows take on those issues, however, are as different after the event as they were before. For example, hurricanes cause spikes in climate change coverage even on conservative shows, but the coverage is largely skeptical in tone. In other words, events amplify discussions that already very sorted. Instead of turning events into a shared national experience, partisan media cause listeners to come away with wholly different interpretations of them, depending on their partisan identities.

The second project is about in-group interests: it explores whether learning that an issue affects people in our social group, changes our attitudes about that issue. The inspiration for this project comes from the fact that most people see climate change as a faraway problem: something that is distant in time, happens in distant places, and/or happens to people that are socially distant from them. Social distance from climate change—that is, the perception that the problem affects people who are not like us—is associated with lower concern about the problem (Spence et al., 2012).

In the project, I explore whether people change their perceptions of a societal problem, once they learn that it harms people not so different from themselves. I test this by randomly providing participants with new information about which social issues affect their group more than others. Across ten group–issue combinations, I find surprising and convincing evidence that these interest cues have very small effects at most. In other words, it is not enough to tell people that climate change is already hurting their in-group. More generally, the project teaches us that "group thinking" in politics seems not be based on interests—

a finding that overturns our default theories of how groups enter into public opinion formation.

Finally, the third project was kicked off by Wood and Vedlitz's (2007) finding that people have a weak but detectable tendency to side with the majority when asked about the seriousness of climate change. The authors found this effect even though the cue was hypothetical (along the lines of "what if 80% of Americans believed climate change is a serious problem?"). Observational evidence also suggests that norms contribute to the formation of climate opinions (Renn, 2011; van der Linden, 2014). This made me wonder about the effect of what I call mass cues: information about the distribution of opinion in a person's social group.

Mass cues are all the more relevant because people are rather misinformed about the distribution of opinions on all sorts of topics, including climate change. Both in Australia and in the US, there is strong evidence for an "egocentric bias": people tend to overestimate the number of fellow citizens who share their opinion on climate change. In addition, in the aggregate people underestimate the prevalence of believing in climate change and supporting climate policy (Mildenberger and Tingley, 2017; Leviston et al., 2013). Indeed, my study shows that people are far off in their perceptions of opinion distributions about a range of topics. However, it also shows that these errors may not be as consequential as they seem: overall, mass cues have very little effect on participants' attitudes.

As a whole, the findings in this dissertation are surprising in light of theories about the strength of group identities in shaping political behavior. Both older and more recent theories suggest that social groups can provide logic and structure to people's political choices, without requiring unreasonable amounts of knowledge on the part of the citizen (e.g. Achen and Bartels 2016; Conover 1984; Converse 1964; Kinder 2003). My results largely do not confirm this idea. The findings go against the predictions of social identity theory, which says that people generally strive to increase the welfare of their in-groups. They also go against the idea that group norms shape opinions, since within-group consensus about a topic does not motivate people to agree with that consensus.

Instead, I find that the most powerful pathway for identities to influence opinions is the set of messages that an identity exposes a person to. We know that people's social identities are correlated with the media they consume, and the conversation partners they engage with (Stroud, 2011; Huckfeldt and Sprague, 1987). My research contributes to growing evidence that, as a result of this exposure, Americans in different social circles hear and see vastly different messages about what is happening in the world and how to interpret it. Taken together, the findings suggest that this is how members of different social groups come to have predictably different opinions—not because they are aware of the material interests of their group, or because they are motivated to adopt whichever opinion seems to be the norm in their group.

In sum, the findings in this dissertation challenge the idea that identities and opinions get connected through inevitable psychological processes, such as in-group favoritism or conformism. Instead, they encourage us to look at external causes of these connections—in particular, media elites. Applied to climate

change, this suggests that it is media content producers who caused climate concern to be associated with some group identities, and climate skepticism with others. For the purpose of climate advocacy, the next obvious question is whether this association can somehow be undone.

1.1 Why study issue-specific opinion?

Before going into detail about each empirical project in this thesis, it is worth considering why issue-specific opinion is interesting at all. After all, we have known for decades that such opinions are difficult to measure, and are neither stable over time nor (logically or ideologically) coherent in the way we might want them to be (Zaller, 1992; Campbell et al., 1960). Moreover, there is robust evidence that political elites are able to shape these opinions, at least for people who identify with a party or support a politician. This means that for most US voters, issue positions cannot be the reason why they choose one party or candidate over another (Lenz, 2013; Zaller, 2012). Party and candidate preferences seem to come before, not after, attitudes on political questions. This seems especially likely for "hard" (i.e., technical) issues such as climate change (Carmines and Stimson, 1980).

Still, there are at least three reasons why ordinary people's attitudes about political questions matter. First, politicians and interest groups seem to think it does. For example, actors with an interest in slowing down governmental climate action seem to view the creation of mass climate denial as an important goal in itself (Farrell, 2016b), and corporations are willing to spend resources spreading denial messages (Farrell, 2016a). In fact, the abundance of research on the effect of elite messaging is premised on the fact that partisan elites (including politicians and media figures) spend time sending out these messages. As a consequence, we may also want to understand whether these messages are reaching the public, and whether they are persuasive—regardless of their eventual effect on vote choice. The findings in this dissertation are relevant to that question.

Second, issue opinions could matter for behaviors other than vote choice. For instance, belief in climate change is at least somewhat connected to climate-friendly behaviors such as saving energy and recycling, especially in the United States (Hornsey et al., 2016). In other cases, opinions inform our behavior towards fellow citizens: whether we discriminate on the basis of race or gender, whether we donate our time and money to charities, where we decide to live. In places that allow for ballot initiatives or referendums, voters face choices that are explicitly about issues. While simple cues from parties and candidates can still play a role in these cases, if the issue is not clearly aligned with a partisan cleavage, there is more room for persuasion (Leduc, 2002; Vreese and Semetko, 2004).

Third, we would expect issue opinions to matter as a determinant of voting for single-issue parties. Climate opinion is an excellent example. Green parties exist in almost every developed democracy with proportional representation,

and it is hard to imagine that votes for these parties are not partly driven by people's concern about environmental problems such as climate change.

Finally, we care about issue opinions because they are the building blocks of a larger trend in American mass politics: partisan sorting (Hetherington, 2009). Sorting, or the increased predictability of people's ideology and opinions from their party affiliations, has a range of behavioral consequences (Levendusky, 2009). Some of the theories I evaluate here, would work well to explain why people's opinions would end up sorted along partisan lines.

Partisan media are an obvious candidate. If partisanship is correlated with which media a person consumes, then ideologically biased media have great potential to increase sorting. Group theories of issue-specific opinion can also explain polarization, to the extent that people are being cross-pressured by their social groups less and less. Mass cues are an alternative to elite cues in understanding why voters in one party would all end up on the same side of an issue. In the elite version of the story, it is because all partisan identifiers are listening to the same, polarized elites. In the mass version, it is because they are listening to each other. As more and more people get swayed, the persuasive force of in-party majority opinion should grow stronger and stronger. In sum, even though issue opinions do not seem to motivate electoral choices in the US, they still affect politics and society in other ways.

Chapter 2

Higher volume, same tune: How political talk radio reacts to events

> *These things have become very politicized as you know, folks. Hurricanes and hurricane forecasting is much like much else that the left has gotten its hands on [...]. The forecast and the destruction potential doom and gloom is all to heighten the belief in climate change.*
>
> —Rush Limbaugh, September 11, 2018

2.1 Introduction

More and more Americans get their political information and opinions from non-mainstream media, such as opinion blogs, podcasts and talk radio shows (Prior, 2013; Levendusky, 2013). Producers of these types of media (also known as new or alternative media) often feel less bound by journalistic norms such as accuracy, objectivity, or fairness. Instead, many have an ideological bias that informs the content they create. Moreover, the "big three" cable news channels (Fox News, CNN and MSNBC) have also developed a consistent political leaning (Ad Fontes Media, 2019; Martin and Yurukoglu, 2017; Sullivan, 2019; Van Zandt, 2019). Together, these outlets make up the landscape of so-called partisan media in the United States. And while we know more and more about the effects of such media, we have less systematic knowledge about the content they create. What kind of messaging can we actually expect to hear and see on these outlets? How exactly do they manage to report on the world in a way that is ideologically consistent?

In this paper, I investigate two strategies that media elites might use in producing partisan content—focusing on the way they talk about issues in response

to newsworthy events. The first strategy is about how much time outlets spend discussing a real-world event, and whether to make the connection between the event and a related political topic. For example, conservative outlets may be less likely to report extensively on a hurricane, and/or less likely to discuss climate change in its reporting on a storm. A second strategy is not to let real-world events change the (ideologically motivated) mix of issue positions represented on the outlet. This implies that if the outlet reports on the event, it will have to give meaning to what happened in a way that is consistent with its ideological slant. For example, if liberal shows tend to talk about climate change in a concerned way, after a hurricane hits the US, it can hold on to this issue position by portraying the event as evidence for climate change. Conservative hosts can hold on to a skeptical position by interpreting to event as irrelevant to the existence of climate change.

The analyses in this paper are based on the largest-ever collection of talk radio content. It consists of audio recordings and speech-to-text transcriptions for almost 2 million hours of talk radio. The data cover over 1,000 radio shows, with almost 220,000 episodes between them. I use natural language processing and crowd-sourcing to take full advantage of this previously unseen amount of data. Radio is a prime example of a partisan medium in the United States, but it is also an impactful medium on its own. 17% of American adults listen to terrestrial (offline) talk radio each week—the same weekly audience reach as Twitter (Pew Research Center, 2018). Because these listeners are older than the average American, they are very politically engaged (US Census Bureau, 2019; Schaffner et al., 2019). Democrats and Republicans listen to talk radio at similar rates (Pew Research Center, 2006).

In my analyses, I focus on partisan discussion of three issues (climate change, gun policy and immigration), in response to three types of events that are relevant to those topics (hurricanes, mass shootings, and immigrant family separation). I find that regardless of the show's leaning, newsworthy events always cause a spike in discussion of political issues relevant to them. In other words, downplaying events or their connections to political issues is surprisingly rare as a strategy among partisan outlets. I also show that at baseline, political talk shows are strongly ideologically sorted in the way they discuss these issues. This is the first-ever quantitative measurement of ideological consistency on talk radio in the US. Finally, events do very little to change this sorting. On both ideological sides, the balance of positions about climate change and immigration after an event is not different from the balance just before. In the case of gun policy, both sides move somewhat in the direction of an anti-gun position.

In sum, I find that partisan media producers do not selectively downplay newsworthy events or their connections to issues. Instead, the effect of events is to amplify an already ideologically consistent discussion of those issues. In other words, after an event that is related to a political issue, listeners are much more likely to hear ideologically colored discussion of that issue. This is especially important given that people, if their media habits expose them to political information at all, tend to get this information from sources on their own side of the political spectrum (e.g. Iyengar et al. 2008; Taber and Lodge

2006; Stroud 2011). For instance, Republicans who tune in to conservative radio shows are much more likely to be exposed to climate skepticism after a hurricane than they were just before. It means that events, rather than getting people on the same page about a political issue, could actually have a polarizing effect.

2.1.1 Creating partisan media content

Recently, the US has seen an increase in media options with an ideological bias. This bias can be explicit, or implicit in the content produced, so that arguably neutral observers (e.g. researchers, experts and watchdog organizations) tend to agree on their slant. We call these outlets "partisan media". This does not mean that they are aligned with a political party (Nadler et al., 2020). On the contrary, some have a fairly hostile relationship with the party that is on their side of the ideological divide (Calmes, 2015). They do, however, favor one end of the ideological spectrum.

There are three possible drivers behind the supply of partisan media content to the American public. The first would be that outlet owners or content makers prefer ideologically colored content. Evidence from newspapers suggests that outlet ownership is not a driver of slant (Gentzkow and Shapiro, 2010). In the case of radio, however, minority-owned and (to a lesser degree) female-owned talk and news stations carry more progressive and fewer conservative shows (The Center for American Progress and Free Press, 2007). As for content producers, we can expect these media elites to be excellent at fitting new information into a pre-existing worldview—since that skill is correlated with both political interest and strength of partisanship (Taber and Lodge, 2006). Different supply-side reasons are difficult to separate from one another (e.g. conservative station owners may attract hosts who are personally willing to spread a conservative message). But the evidence suggests that together, they make for a reasonable explanation of ideological media content.

The second possibility is that audiences prefer slanted content (Mullainathan and Shleifer, 2005). There are reasons to believe, however, that US media (and partisan media in particular) are more ideologically sorted than the public demands. In the case of newspapers, Gentzkow and Shapiro (2010) show that only about 20% of the ideological slant (as measured by word usage) can be explained by audience demand. Moreover, newspapers' slant is subtle compared to other media (Prior, 2013). This idea is confirmed by experiments using specially-crafted TV news fragments, where hosts take sides on an issue and give more time to guests on the same side (versus acting neutrally in a control condition). This pattern of reporting is typical for talk radio (Berry and Sobieraj, 2013). Participants dislike such news if it is incongruent with their partisan leaning, but they do not prefer congruent partisan news over non-partisan news (Bode et al., 2018).

As for talk radio, shows are far more polarized than we would expect based on the preferences of their (potential) audiences (Berry and Sobieraj, 2013, p. 74). Among people who use radio as their prime source of political news, 56% are not ideologically consistent in their opinions, according to the Pew Research

Center (2014). This is about the same proportion as in the US population. Even among people who use the highly opinionated Rush Limbaugh Show as their main source, 45% are not consistent. For local talk radio, it is 66%. According to Republican insiders, conservative media take positions on issues like climate change and immigration that actually go against societal trends, even trends among Republicans (Calmes, 2015).

A final reason for the supply of partisan content could be that it is a side product of other demands from the public: namely, a demand for outrage and/or a sense of community (Berry and Sobieraj, 2013). In that case, the audience of an outlet does not have to be nearly as consistent or polarized as the opinions that the outlet broadcasts. Instead, audiences may be attracted to a style of reporting that vilifies political opponents, or creates the feeling that the host and listeners are part of a homogeneous political in-group. In this scenario, sorted or extreme opinions on issues are not in direct demand—they are just a byproduct of a communication style that sells. It is compatible with Bode et al.'s (2018) finding that audiences are not particularly engaged by newscasts which simply give more time to "their" side's arguments. It could also explain why Americans tend to select news and information sources that match their partisan leaning—despite majorities of Democrats and Republicans saying they prefer news with no particular point of view (Pew Research Center, 2013).

Whatever the reasons behind ideological media content may be, researchers agree that there is a larger and larger offering of partisan media in the US (Prior, 2013; Levendusky, 2013). However, we do not have much systematic knowledge about what is actually being said on these media. Martin and Yurukoglu (2017) place cable news outlets on an ideological scale, based on their word content. A number of studies discuss how partisan media report on stories about parties and politicians (Groeling, 2008; Puglisi and Snyder, 2011; Baum and Groeling, 2008). Large-n studies of political talk radio are particularly rare, because audio is so challenging to process at a bigger scale. One example is Barker (2002), who counted the topics that radio host Rush Limbaugh talked about in the course of two years.

In this study, I take advantage of an unusually large talk radio data set, in order to study how partisan media talk about specific political topics—climate, gun policy, and immigration. In particular, I look at how they produce ideologically motivated content in the face of real-world events. I propose two strategies that partisan content-makers may use to make sure their response to events is in line with their ideological positions.

Partisan agendas

The first strategy I propose revolves around agenda-setting decisions—that is, choices about what to talk about in the aftermath of an event. There are two variants of this approach: strategically giving less attention to events; and strategically choosing which political topics to connect them to.

In the first variant, a media outlet spends less time on events that are less compatible with its ideological bias. This idea—that slanted media selectively

ignore inconvenient stories—is very popular in political conversations today. Groeling (2008) shows that CBS, NBC and (to a lesser extent) ABC all appeared to have a preference for reporting polls showing approval gains for Bill Clinton and approval losses for G.W Bush. Fox News had the opposite tendency. Puglisi and Snyder (2011) find that slanted newspapers tend to give more coverage to scandals involving the "other" side, and less to those involving their own. Finally, Baum and Groeling (2008) show that compared to the presumably neutral baselines of Associated Press and Reuters, both Fox News and the left-leaning blog DailyKos clearly give more attention to stories that fit their partisan narrative (so does the right-leaning blog FreeRepublic, but only under certain specifications).

A second variant of strategic agenda-setting would involve reporting on events, but being selective about which political topics to connect them to. According to Baum and Groeling (2008), the newsworthiness of a story has no significant impact on whether the story gets covered by these partisan outlets. Similarly, we could imagine that the relevance of a story to a political issue has no bearing on whether a partisan outlet connects the two. While there seem to be no studies yet of partisan issue coverage in response to events, a few pieces of evidence exist about partisan media agenda-setting in general. Puglisi (2011) reveals that during presidential campaigns with Republican incumbents, the New York Times pays particular attention to campaign topics that are "owned" by the Democratic party. While the Times can choose to cover any topic that is relevant to the campaign, its actual agenda reflects a partisan bias. Similarly, Larcinese et al. (2011) show that newspapers give more coverage some economic topics (unemployment and, to a lesser extent, trade and budget deficits) when the current trends support the partisan side that the paper is on.

Even though it is talked about less, and studied less, this second agenda-setting strategy seems to be the most feasible of the two, especially in the case of major events. Since media content producers have to fill publishing space or airtime, perhaps they cannot afford to selectively cover events. This may be especially true for radio hosts, who often talk for one or more hours a day, using limited resources. But it is quite clear that content makers need not connect events to specific political issues. In fact, studies on agenda setting in mainstream media show that this selection happens even when there are no partisan filters on content production. For example, Best (2010) shows that most of the news coverage triggered by events related to homeless people does not mention homelessness as a societal issue. Instead, coverage focuses on individual causes of the problem. Similarly, hurricanes could easily be covered without any reference to climate change.

Partisan issue positions

The second strategy that partisan media could use, is to feature only, or mostly, ideologically consistent positions on the topics they cover. They can do this both on a day-to-day basis and in response to events. The limited large-n evidence that exists on US talk radio content already suggest that partisan shows tend to

present only one side of each issue. Listening to 100 hours of political talk radio, Berry and Sobieraj's (2013) find only 72 instances of confrontation between two points of view (or "sparring"). This is because show producers almost never give airtime to guests or callers who disagree with the show's host. To compare, language painting political "others" as ideologically extreme was used 288 times. As a result, looking at specific issue-positions represented on political talk shows, I expect to see messaging that is ideologically consistent most of the time.

An open question, however, is whether partisan media are able to hold on to their ideological issue positions in the face of unusual real-world events. Perhaps newsworthy events with strong connections to political topics have the power to (at least temporarily) change the balance of positions on those topics, even in partisan media. In fact, each of the event types that I study here—hurricanes, mass shootings and family separation—has changed partisan perceptions of a political issue in one way or another. For example, Visconti and Young (2019) find that disasters such as hurricanes and floods influence Americans' climate beliefs—despite there not being strong evidence for a connection between Atlantic hurricanes and climate change (Geophysical Fluid Dynamics Laboratory, 2019). Newman and Hartman (2017) show that people who live nearby the site of a mass shooting become more likely to support stricter gun control, regardless of their partisanship. Finally, president Trump's policy of separating families at the border divided Republicans in Congress: most stayed silent, but a significant group spoke out against the policy (Phillips, 2018). Ordinary Republicans were also split in their opinions (Quinnipac University, 2018).

On the other hand, it is clear that even extreme events can be filtered through an ideological lens. The same event can be spun to support opposite issue positions, for instance by strategically pointing to different causes of the event, or by portraying the event as either good or bad. Bisgaard (2015) presents a neat example of ordinary citizens doing just that. He finds that in the UK after the Great Recession, both Conservative and Labour partisans agreed on the fact that the economy had gotten worse under a Labour government. However, Conservatives tended to say that the government was responsible for the economic situation, whereas Labour adherents said it was not. A similar logic could explain why mass shootings are followed by laws that loosen gun control in Republican-controlled state legislatures, but cause no change in Democrat-controlled states (Luca et al., 2019). While most Republicans believe that gun violence can be solved with broader gun ownership, most Democrats believe the opposite. This is true for both party elites and the public (Pew Research Center, 2019; Spitzer, 2011). These findings make clear how partisan thinkers might come away with different interpretations of the very same event.

In sum, I propose that partisan media could employ two strategies to cover events in a way that aligns with their ideological message. First, outlets could choose not to pay attention to a political topic related to the event—either by downplaying the event itself, or by not connecting it to the topic. Second, they may not change the positions they take on the topic, instead fitting the event into their existing position. In this paper, I show that the first strategy is rare, while the second is common.

2.1.2 Implications: audience effects

A key reason to care about the content of partisan media is its potential effect on the public. To understand these effects, we can lean on two existing findings about media audiences. First, as noted above, Americans prefer news outlets that share their political leaning ("selective exposure"). Both experimental (Iyengar et al., 2008; Iyengar and Hahn, 2009; Taber and Lodge, 2006) and observational studies (Stroud, 2008, 2011) confirm this. As a result, partisan position-taking implies that media consumers on each ideological side would get far more exposure to one issue position than the other.

Second, there is at least some evidence that the positions taken in partisan media can affect people's opinions about political issues. In an experimental setting, Levendusky (2013) finds lasting, polarizing effects of like-minded news programs, whereas programs from the "other side" have no effect. In the real world, Stroud (2010) shows that selective media exposure predicts future polarization of opinions about presidential candidates. Turning to radio, Barker and Knight (2000) find that Rush Limbaugh listeners develop more negative attitudes about topics and people that the radio host gives a lot of negative attention to. Not all evidence on partisan media effects points in this direction: other studies have found no effect (Arceneaux and Johnson, 2010), including from talk radio (Yanovitzky and Cappella, 2001). Nonetheless, there is a distinct possibility that partisan media can influence the issue opinions of their audience, especially in the presence of selective exposure. This is particularly important in light of the finding that talk radio content is still much more sorted than the opinions of its listeners.

Relying on this evidence, we can see that different combinations of partisan media strategies could produce very different audience effects. Table 2.1 specifies the likely effects, based on existing literature, of each combination of partisan media strategies in the face of an event. If there is no partisan filter on agendas (i.e. discussion of issues increases on both sides), and partisan position-taking decreases in the aftermath of an event, then events could lead to depolarization on both ideological sides. If agenda-setting is partisan (i.e. one side avoids discussing the issue), but the event creates a temporary decrease in partisan position-taking, then there will be a depolarizing effect concentrated on the side that gave attention to the issue. If events do nothing to change the mix of issue positions taken on each side, or if they increase the partisan gap in positions taken, then they could have a polarizing effect on opinions. This is especially true if agenda-setting is *not* partisan, because in that case, partisan messaging about the topic will be amplified on both sides.

In these predictions, I have treated agenda-setting essentially as a multiplier on the effects of position-taking. That is, when an issue appears on the agenda of a partisan outlet, then consumers of that outlet are exposed to a larger number of (potentially partisan) messages about that issue (cf. Barker 2002). This is different from the agenda-setting effects that are theorized, and found, in studies of mainstream media. In those media, attention to political issues primarily causes those issues to become more salient—that is, to be perceived

Table 2.1: Possible reactions to events in partisan media, and their likely effects on audiences.

Attention to topic	**Positions taken**	**Audience effect**
increased on one side	less partisan	depolarization on one side
increased on both sides	less partisan	depolarization on both sides
increased on one side	as or more partisan	polarization on one side
increased on both sides	as or more partisan	polarization on both sides

as more important problems (Cohen, 1963; Feezell, 2018; Iyengar et al., 1987; King et al., 2017; McCombs and Shaw, 1972; McCombs et al., 2010; Weaver et al., 1981). Partisan media are different in that when they bring up an issue, they typically take an implicit or explicit position on that issue. In fact, the message they send might well be that the issue is *not* an important societal problem (e.g. liberal coverage of immigration). As a result, there is no reason to think that equal coverage of an issue on liberal and conservative outlets would lead to equal increases in perceptions of issue salience.

2.2 Methods

2.2.1 Data and design

In this paper, I shed light on the content of partisan media by looking at one-and-a-half year's worth of US political talk radio. I define political talk radio as any radio show where one or more hosts (and possibly guests or callers) talk about current affairs. Earlier definitions of talk radio include callers as a defining characteristic (Barker and Knight, 2000; Berry and Sobieraj, 2011). This would exclude news and "in-depth" current affairs shows such as those produced by public radio networks. Since I am interested in all shows from which listeners can learn about political topics, my definition includes both of these show types. Later, I show that news and public radio shows do not behave differently from other political shows–and that the paper's results hold whether or not I include them (see section Robustness Checks).

The raw data consist of continuous recordings, and transcriptions, of the live internet streams of a large set of radio stations. They were collected by Cortico and the MIT Media Lab's Laboratory for Social Machines. Together, these stations broadcast over 1,000 unique radio shows. Supplemental Information section A.1 provides more details about this data set, including station locations, how station characteristics compare to the population of US talk radio stations, and transcription quality.

I analyze discussions of three political topics (climate change, gun policy and immigration) on each show, in the weeks before and after a relevant event happened. Shows are produced relatively independently from one another, making

them a sensible observational unit of radio content.* And because almost all shows are broadcast according to a weekly schedule (e.g. one episode per weekday), it makes sense to bundle content into weeks rather than, say, days. The unit of analysis, then, is the radio show-week. The total number of show-weeks in the analyses depends on the topic and which events it is connected to. The number of radio stations in the data set was gradually ramped up, so if an event happened later in the period, more political shows were being captured at the time. I also cover a different number of events for each topic: two for climate change, three for gun policy, and one for immigration. In total, I have 1616 show-weeks for climate, 2176 for gun policy, and 600 for immigration.

Below, I discuss my choice of topics and events. The dependent variables are the number of times a political topic was mentioned on a show, and the positions that those mentions support. The independent variables are whether we are looking at show content from before or after a major event; and whether the political talk show leans liberal or conservative. Each of these variables poses its own set of measurement challenges. Figure A.2 in Supplemental Information section A.1 shows the full project workflow.

2.2.2 Selecting issues and events

The issues I look at in this study are climate, gun policy and immigration. I choose these topics because they span different degrees of salience in current American politics. In the course of 2019, when asked to name the most important problem facing the country, 3–6% of Americans mentioned the environment, pollution or climate change (Gallup, 2019b). Among environmental issues, however, Americans tend to be more concerned about local pollution than about climate change (Gallup, 2019a). 1–8% brought up guns or gun control, and another 1–4% mentioned crime or violence. Finally, 11–27% named immigration, making it the second-most-mentioned issue. The attention given to these topics on political talk shows reflects these different levels of salience. At baseline, in the week before a relevant event happens, climate change and gun policy are mentioned less than twice on the average radio show. Immigration is mentioned nine times.

Another benefit of these topics is that each one can be clearly connected to one or more newsworthy events: hurricanes for climate change, mass shootings for gun policy; and for immigration, the outburst of attention to families being separated at the US-Mexican border. As noted above, each of these event types is known to have influenced partisan opinions on the relevant topic in one way or another. Finally, discussion of these topics can be easily identified through a small number of topic-related terms (see below).

The specific events I study here were the most newsworthy of their kind in the observed period. For the topic of climate change, I use the two hurricanes that made landfall in the continental United States: Florence and Michael. For

*The exception is that shows licensed to the same network may be subject to a common set of pressures about content, including a set of legal and moral rules called Standards and Practices.

gun policy, I look at three mass shootings that received broad attention: Santa Fe High School, Jacksonville Landing, and the Pittsburgh Synagogue shooting.[†] For immigration, I use the sharp outbreak of public attention to President Trump's policy of separating immigrant children from their parents at the US–Mexican border in June 2018. Based on Google searches for "immigration", this (and not, surprisingly, the announcement of the administration's policy two months earlier) was by far the most noteworthy immigration event in the period covered by the data.

2.2.3 Topic mentions: count and position

The key dependent variables in this study are the number of occasions where a speaker on each show mentioned a political topic, and percentage of those mentions that support a particular position. A topic mention is simply an occasion where the algorithm recognized a topic-relevant term in the speech produced by a radio show. The terms for each topic are: (1) climate change and global warming; (2) gun control, gun right(s), second amendment, gun owner(ship), anti-gun, pro-gun, and gun violence; (3) immigration, immigrant, migration, and migrant.

Shows that are broadcast on several of the recorded stations only have their mentions counted once. I make use of the transcripts from all of the broadcasts, however, in order to help deal with any errors in the transcript of any given broadcast. Supplemental Information section A.2 goes into more detail on this process.

Next, workers on Amazon's crowd-sourcing platform Mechanical Turk coded the position of the mentions. They did this by listening to a 30-second audio fragment surrounding the mention of the topic. Workers coded a sample of 25 mentions per show-week (or fewer, of course, if there were fewer than 25 mentions in that week). This amounted to about 4300 out of 4900 mentions for climate change, 5050 out of 5650 mentions for gun policy, and 7350 out of 15500 mentions for immigration.

For each topic, I asked coders to classify the mention into one of two issue positions: "skeptical" or "convinced"about climate change, "pro-gun" or "anti-gun", and "supporting immigration" or "tough on immigration". Coders could also label mentions as taking neither position. I asked two workers to code each mention. If they disagreed, I added a third. Supplemental Information section A.3 describes the coding task in more detail. Section B.1.5 lays out how I modeled the mention counts and mention positions, along with their connections to the independent variables.

[†]Another mass shooting happened in Thousand Oaks, California, just eleven days after Pittsburgh event. I did not include the Thousand Oaks shooting, as its pre-week would overlap with the Pittsburgh post-week.

2.2.4 Events: pre- and post-weeks

One of the independent variables in this study is whether a show-week happened just before, or just after an event. In the case of mass shootings, this is clear: since their timing is unpredictable, and they happen over the course of a few hours at most, their media impact starts on the day of the event. In the cases of hurricanes and immigrant family separation, on the other hand, the start of their impact is less clear-cut. For example, the strength and path of hurricanes can be predicted with more and more certainty as they approach, until they eventually make landfall.

I expect events to have media impact once they cross some threshold of social significance—for example, once a hurricane is predicted to hit a populated area. I use search indices to detect when this social threshold is reached. Supplemental Information section A.5 goes into further detail on how I use search data to define pre-event and post-event weeks.

2.2.5 Classifying shows: politics and ideology

In order to include a show in the analyses, I first need to be able to classify it as political: non-political talk shows (like cooking and gardening shows) are excluded. Second, I need to know whether it has a liberal or a conservative slant. For both decisions, I created bag-of-words classifiers based on all transcribed episodes of the show—typically 14 months' worth of data.

As a training set, I used the transcripts of 50 shows with known labels. For the training set of the political/non-political classifier, I hand-labeled 33 shows as non-political based on their titles (e.g. "Better Lawns and Gardening"), verified either by their transcript or by looking at the show's website. For the ideology classifier, I required at least two sources to confirm that a show has either a conservative or a liberal slant. This way, I gave an ideological label to 17 shows, which also served as the "political" shows for the political/non-political classifier. The classifiers were trained on the episodes of these hand-labeled shows.

When I tested the trained models on previously unseen (held-out) episodes, the political/non-political model correctly classified all 50 known shows. The conservative/liberal model successfully classified all 17 political shows. Finally, I applied the trained classifiers to all shows, including unlabeled ones. Of the shows labeled as political by the first classifier, the second classifier was able to label the vast majority (94%) as either liberal or conservative with at least fairly high certainty ($> 70\%$). This suggests that even just looking at the words used in these shows, their ideology is quite clear.

Supplemental Information section A.6 contains information on the training shows (including sources for the ideological labels) as well as further details on how the classifiers were trained, tuned and tested. Below, in the Robustness Checks section, I discuss why I treat news and public radio as potentially political and ideological shows—and I show that this decision does not affect the paper's findings. I also show the distribution of show classification probabili-

ties, and what happens to the analysis results when I vary the decision rules for labeling programs as non-political, conservative and liberal.

2.3 Results

2.3.1 Agenda-setting: connecting events and issues

The first content production strategy we are interested in, is whether shows downplay events or their connection to political topics. To find out, we can look at how the volume of talk radio discussion on a political topic changes after a relevant event. Figure 2.1 shows the total number of mentions on conservative and liberal shows, for each topic, in the weeks before and after an event. There are clear effects of events on the number of topic mentions, on both ideological sides.

To verify this, and to control for the total amount of liberal and conservative airtime, I run a negative binomial regression on the number of mentions, with an interaction between event week and ideology (see Supplemental Information section B.1.5 for justification and details). I find that on a conservative show with four hours of airtime, the estimated number of climate mentions increases from 0.8 to 2.0 ($p < .001$). On a liberal show, it increases from 2.2 to 4.1 ($p < .005$). In the case of gun policy, among conservative shows, the number of mentions changes from 2.0 to 3.3 ($p < .001$); among liberal shows, it changes from 0.7 to 1.1 ($p < .01$). The number of immigration mentions on conservative shows goes from 5.1 to 27.4 ($p < .001$); on liberal shows it goes from 8.2 to 17.7 ($p < .05$). The difference between the proportional change on the liberal and conservative sides is significant only in the case of immigration ($p < .005$). Supplemental Information section A.7 discusses the longevity of attention after it has peaked.

One concern about the immigration findings might be, that it would be difficult to report on the event (family separation) without mentioning the topic terms. For that reason, I re-do the analyses leaving out any mentions that the coders labeled "neither". These are mentions that take no position on immigration, largely because they are simply pieces of news on the topic. On both ideological sides, the proportional increase in the non-neither mentions is actually larger than the overall increases above (cons.: from 2.4 to 10.5; lib.: from 4.3 to 8.0). In other words, the immigration findings are not due to outlets being forced to report on the events themselves—instead, they are mostly due to an increase in opinionated commentary on the topic of immigration.

As these figures and model results show, baseline attention to these political topics depends on the show's leaning. Looking at pre-event weeks, I find that the difference in number of topic mentions is significant for climate change, where liberal shows have about twice as many mentions ($p < .001$). Conservative shows have about four times as many gun policy mentions as liberal shows ($p < .01$). Liberal shows have more mentions of immigration at baseline, but the difference with conservative shows is not significant.

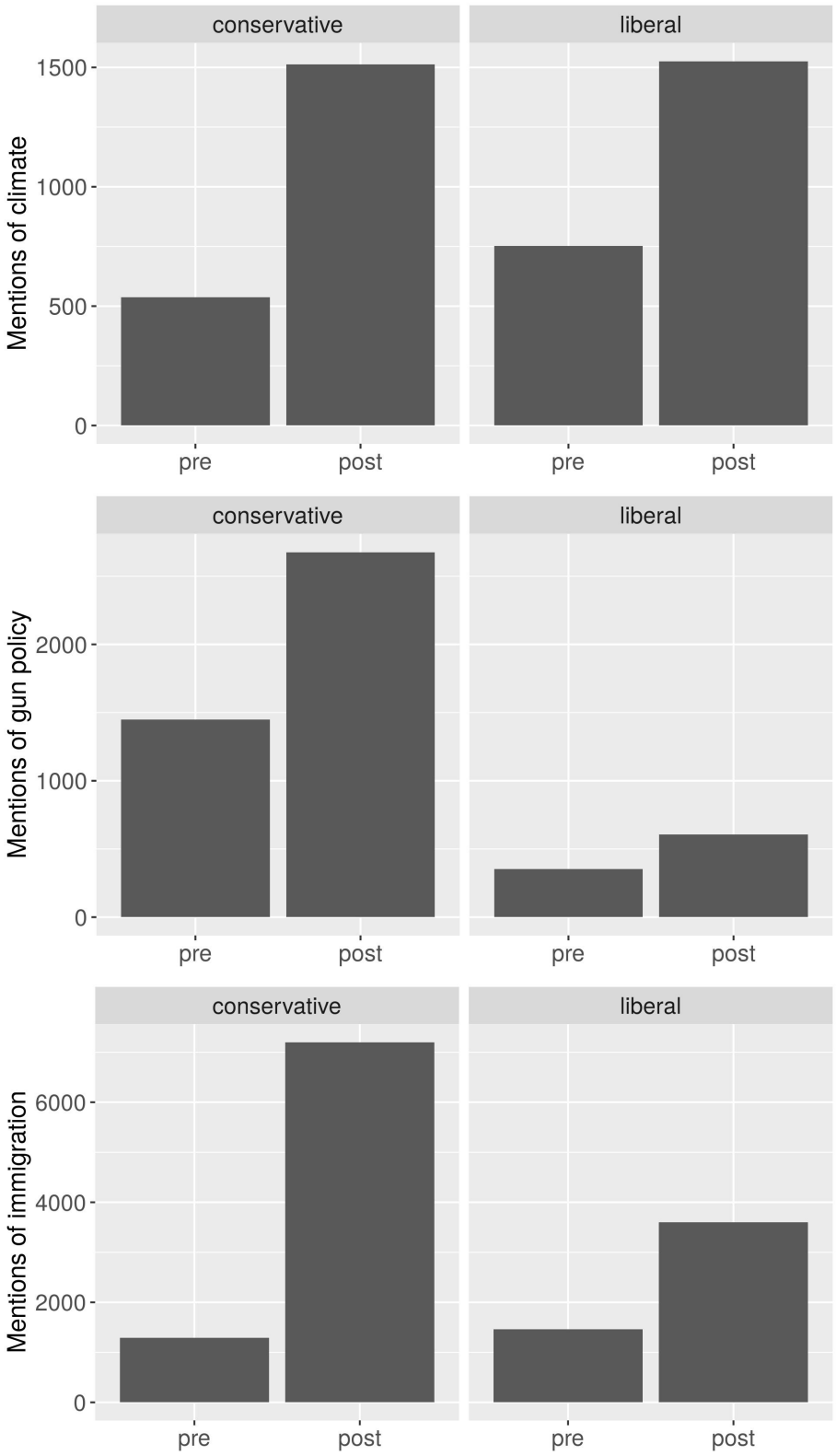

Figure 2.1: Total number of mentions of climate, gun policy and immigration on conservative and liberal radio shows, in the weeks before and after relevant events.

2.3.2 Position-taking: defending issue stances

Next, we want to know whether events change, or instead are fitted into, the mix of issue positions present on partisan outlets. Figure 2.2 shows the proportions of topic mentions supporting each (or neither) topic position. It is clear that in the week *before* an event, talk about relevant political topics is already very sorted. In the case of climate, "convinced" mentions on conservative shows are fairly uncommon, and "skeptical" mentions on liberal shows are extremely rare. For gun policy, "anti-gun" mentions on conservative shows are a small minority, and so are "pro-gun" mentions on liberal shows. Finally, mentions that are "tough on immigration" are very common on conservative shows, but are a small minority on liberal shows. There are also many "neither" mentions for immigration, both before and after the event. As noted above, most of these mentions are simply immigration-related news reports.

To verify this, I run a fractional logit regression (see again Supplemental Information section B.1.5) on the pre-event weeks only. The dependent variable is the proportion of convinced, anti-gun and "tough on immigration" mentions on each show-week (among all mentions with a position; leaving out the "neither" mentions). I regress this on the ideological leaning of the show. Results confirm that the proportions of convinced climate mentions are vastly different between conservative and liberal shows (conservative: 39%, liberal: 93%, $p < .001$). The same is true for anti-gun mentions (conservative: 22%, liberal: 68%, $p < .001$) and for "tough" immigration mentions (conservative: 70%, liberal: 30%, $p < .001$). Because both coding errors and show classification errors pull these gaps towards zero, we can read these estimates as lower-bound estimates of the real gap sizes.

The next question is whether events do anything to change this (im)balance of positions. I apply another fractional logit, now including an interaction between event week (pre or post) and ideology (liberal or conservative). I find that the proportion of convinced climate mentions increases from 39% to 41% among conservative shows, and from 93% to 94% on liberal shows. Neither change is significant. The proportion of anti-gun mentions increases from 22% to 30% among conservative shows, and from 68% to 79% on liberal shows. The change is significant for conservative shows ($p < .01$) and marginally significant for liberal ones ($p < .10$). Finally, the proportion of tough-on-immigration mentions decreases slightly from 70% to 66% among conservative shows, and from 30% to 28% on liberal shows. Neither change is significant. In all three cases, the changes on the two ideological sides are not significantly different from one another.

Finally, we might be interested in how the narratives around each issue differ depending on the speaker's position, and how speakers with different positions react to events. Supplemental Information section A.8 explores the content of radio fragments supporting different positions (climate skeptical, anti-gun and so on), before and after an event. Most interestingly, the analysis shows that in all cases, speakers on one side of the issue (respectively climate skeptical, anti-gun, and supportive of immigration) are more likely to mention details

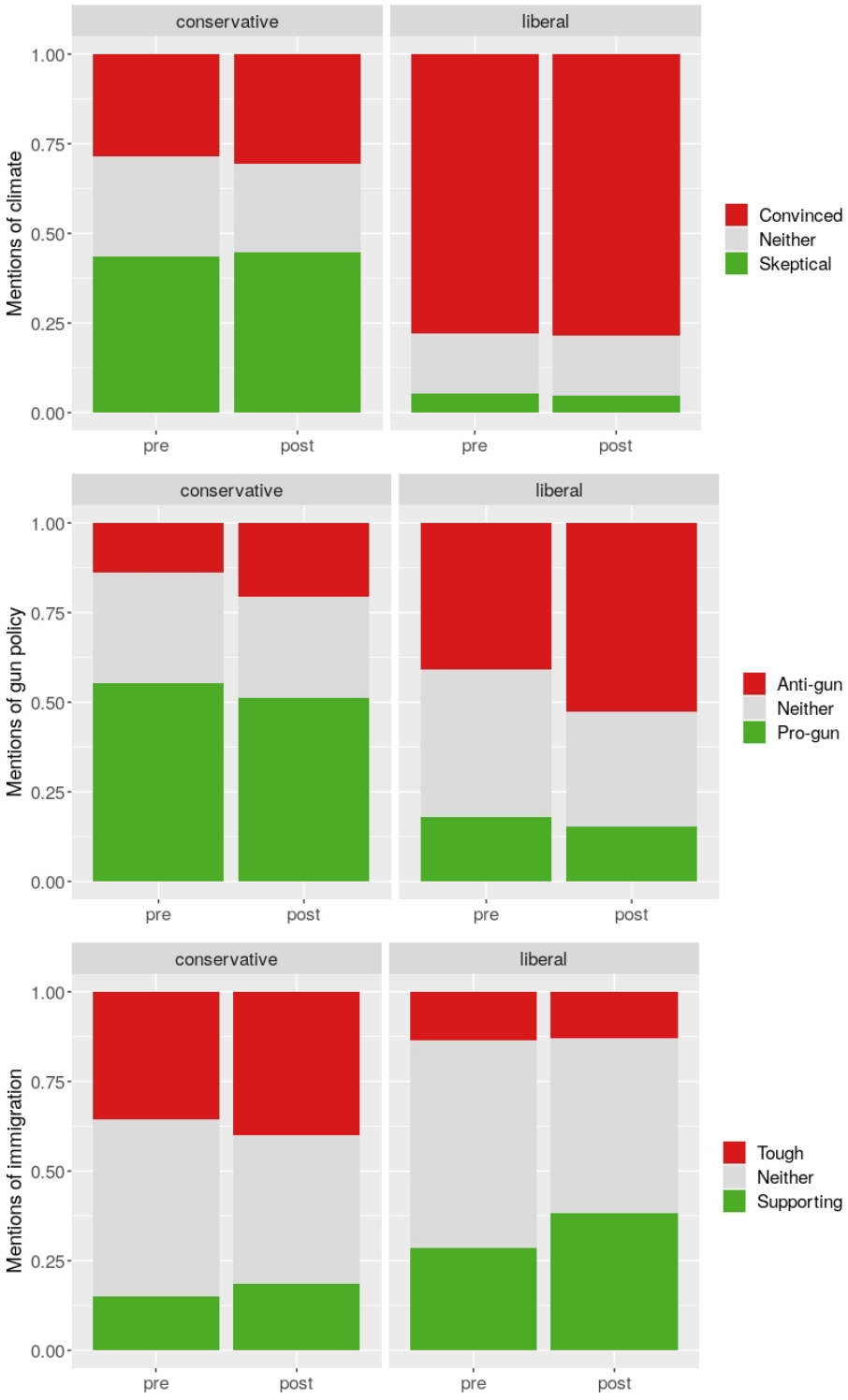

Figure 2.2: Average proportion of mentions with each position on climate, gun policy and immigration on conservative and liberal radio shows, in the weeks before and after relevant events.

related to the event. For instance, in post-event climate discussions, the word "hurricane" itself is used far more by skeptics, as they point out that intense storms are only weakly linked to climate change.

This result stands in interesting contrast with the finding that in all cases, both ideological sides react to the event by intensifying their discussion of related political topics. Perhaps both sides feel forced to speak about these topics because the mainstream media, or partisan media from the "other" side, are covering them, or because attention to any general theme is an opportunity to spread a partisan message about it. Nonetheless, the details of each event type seem to connect more naturally to one of the issue positions.

2.4 Robustness checks

2.4.1 News and public radio shows

As noted earlier, most of the existing literature has treated news and public radio shows as separate from political talk radio. In this paper, any program that treats topics similar to the political shows in the training set will be classified as political, and therefore be part of the sample. While there are conceptual reasons to include news and public radio shows (i.e., people learn about politics from them), it also turns out that empirically, they are not necessarily more "neutral" than the call-in shows that previous work has focused on. I also demonstrate below that key findings are not affected by the decision to include them.

To inspect news and public radio shows in the current data set, among shows classified as political, I find a set of 25 shows that have the word "news" in their name (e.g. "Alabama Morning News"), and 14 shows that are produced and distributed by National Public Radio (NPR). First, I look at whether these shows, when they mention a political topic, tend to take neither of the two established positions. Mentions coded as "neither" are usually presentations of facts or straightforward pieces of news about a topic. Bundling all of the observed weeks, on the topic of climate change, the average news show supports neither position in just 16% of its mentions. The same is true for NPR shows. Talking about gun policy, 33% of mentions on the average news show are neutral in this way. On NPR shows it is 34%. Immigration is the topic that invites the most neutral discussion, with 42% of news mentions and 55% of NPR mentions.

A second possibility is that these shows are neutral in the sense that they present both sides of the story equally, for instance by inviting guests with opposite points of view. However, among the topic mentions that have a position, I do not find this type of balance. In the case of climate change, the average news show dedicates more than 89% of its "positioned" mentions to one side of the issue (be it skeptical or convinced). For the NPR shows, it is 95%. On gun policy, these shows spend 73% of their non-neutral mentions arguing for the same side. For the average NPR show, that is 84%. On the topic of immigration, the average news show has 72% of its positioned mentions supporting

		without news, public				with news, public			
		counts		positions		counts		positions	
topic	ideology	pre	post	pre	post	pre	post	pre	post
climate	conservative	0.8	2.1	38	42	0.8	2.2	39	41
climate	liberal	2.2	4.1	92	94	2.0	4.1	93	94
gun policy	conservative	1.9	3.3	21	29	2.0	3.3	22	30
gun policy	liberal	0.7	1.0	71	77	0.7	1.1	68	79
immigration	conservative	4.3	25.3	70	67	5.1	27.4	70	66
immigration	liberal	7.3	14.5	31	27	8.2	17.7	30	28

Table 2.2: Predicted mention counts and positions (percentage "convinced", "anti-gun" and "tough on immigration" positions), before and after events, for each political topic, without and with NPR shows or news shows.

the same side. For NPR shows, it is 69%. Not surprisingly, all NPR shows tend to pick the same side (in particular, they overwhelmingly are convinced about climate change), whereas the group of news shows is mixed in the direction of their slant. Crucially, none of the numbers above look much different in the sample of non-news, non-NPR shows.

To check the robustness of these findings, I experiment with different definitions of news and public radio shows, based on what station(s) broadcast(s) them. All US radio stations have a self-selected format that broadly describes their programming, mostly for marketing purposes. An alternative criterion for news shows would be those shows that are broadcast at least one station with the"All News" format. An alternative criterion for public radio shows would be those shows that are broadcast on at least one station with the "Public Radio" format. These definitions lead to the same conclusion: on news and public radio shows, the discussion of political topics looks no more neutral or balanced than it does on any other political show.

Despite these conceptual and empirical arguments, we might still want to exclude news and public radio shows, in order to stay consistent with previous literature. For that reason, I repeat the analyses, leaving out shows with 'news' in the name and shows produced by NPR. I also exclude two NPR shows from the training set for the ideology classifier. This harms performance somewhat: testing the model on unseen episodes, one liberal show is now classified as conservative. Table 2.2 shows the results, alongside results *with* NPR and news shows. We can see that the basic thrust is the same.

2.4.2 Show classification thresholds

In the analyses above, shows are classified based on two thresholds. They are considered political if their episodes have an average estimated probability of being political that is greater than 50%. And they are conservative if their average episode's estimated probability of being so is 50% or more; liberal otherwise. The training set for each model is a set of shows that can reliably be labeled

as non-political, liberal or conservative. This set probably does not reflect the actual balance of show ideologies in the full sample. It is likely, then, that the models' intercept estimates are biased. Moreover, perhaps not all political shows are slanted: it is possible there are moderate shows in the sample, which I am unjustly labeling as ideological.

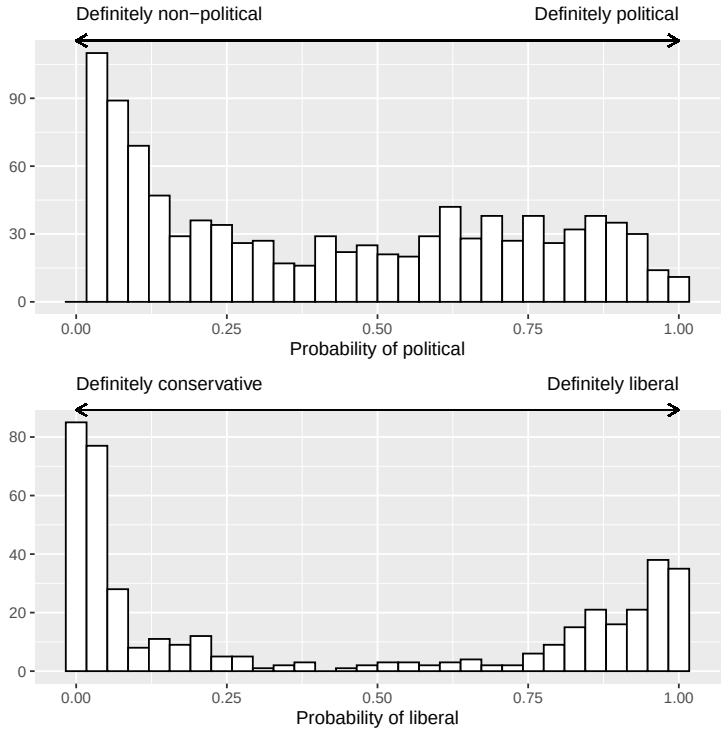

Figure 2.3: Distribution of prediction probabilities for shows from politicalness and ideology classifiers.

Figure 2.3 shows the results of the show classification effort. It looks like the choice of 'politicalness' threshold could be important, because some shows are in fact difficult to classify. Only 70% of shows can be labeled as political with at least 70% certainty. In terms of ideology, the picture looks more robust. Fully 94% of political shows get an ideological label with over 70% certainty. Nonetheless, we may be interested in how results change if we exclude shows whose ideological class is unclear.

Here, I repeat the key analyses, varying my decisions about show classes in two ways. First, I move the political decision threshold above or below 50%, biasing the model towards labeling fewer or more shows as political. Second, I create bands around the ideology threshold, excluding shows that the model is uncertain about. For example, I might only include shows for which the classifier is at least 60% certain that they are either liberal or conservative.

		climate				gun policy				immigration			
		counts		positions		counts		positions		counts		positions	
threshold	ideology	pre	post	pre	post	pre	post	pre	post	pre	post	pre	post
0.4	conservative	0.7	2.0	39	44	1.8	3.0	23	30	4.6	25.8	67	64
0.4	liberal	1.7	3.6	93	94	0.6	0.9	67	79	7.0	15.0	28	26
0.5	conservative	0.8	2.2	39	41	2.0	3.3	22	30	5.1	27.4	70	66
0.5	liberal	2.0	4.1	93	94	0.7	1.1	68	79	8.2	17.7	30	28
0.6	conservative	0.9	2.5	38	40	2.2	3.6	21	30	5.2	28.3	70	68
0.6	liberal	2.1	4.6	93	94	0.7	1.4	68	79	7.0	20.7	28	32

Table 2.3: Predicted mention counts and positions (percentage "convinced", "anti-gun" and "tough on immigration" positions), before and after events, for each political topic. Threshold indicates level of certainty we need in order to call a show "political" and include it in the data set.

		climate				gun policy				immigration			
		counts		positions		counts		positions		counts		positions	
threshold	ideology	pre	post	pre	post	pre	post	pre	post	pre	post	pre	post
0.5	conservative	0.8	2.2	39	41	2.0	3.3	22	30	5.1	27.4	70	66
0.5	liberal	2.0	4.1	93	94	0.7	1.1	68	79	8.2	17.7	30	28
0.6	conservative	0.8	2.2	37	40	2.0	3.4	21	30	5.1	27.5	71	65
0.6	liberal	2.0	4.2	94	94	0.7	1.1	68	80	8.3	17.6	30	27
0.7	conservative	0.8	2.2	36	40	2.1	3.5	21	30	5.2	27.6	71	66
0.7	liberal	2.1	4.2	96	95	0.7	1.1	69	81	8.7	17.8	29	26

Table 2.4: Predicted mention counts and positions (percentage "convinced", "anti-gun" and "tough on immigration" positions), before and after events, for each political topic. Threshold indicates level of certainty we need in order to call a show "conservative" or "liberal", and to include it in the data set.

Table 2.3 shows the results of the former analysis. Table 2.4 shows the latter. Neither decision changes the results in any significant way, except that stricter 'politicalness' thresholds lead to somewhat more topic mentions at baseline. This makes sense, since I am excluding shows that spend less time covering political topics.

2.5 Discussion

In this paper, I investigate the effects of newsworthy events on the discussion of three political topics (climate, gun policy and immigration) on US talk radio. I find that partisan radio shows do not downplay events or their connection to political issues, but they also do not usually change their positions on those issues in response to events. In other words, events tend to amplify, but not change, the messages being sent on political radio shows.

In find that in all cases, the event comes with a sharp increase in the total volume of the discussion on relevant political topics. The proportional increase in attention is not always even, but surprisingly, it is always substantial on both sides. In the case of climate change, hurricanes draw attention to climate change on both sides. Mass shootings also cause the same proportional increase in gun policy discussions on both sides. In the case of immigration, family separation outrage causes a doubling of attention for liberal shows. The increase for conservative shows is even larger, despite the fact that these shows were paying less attention to immigration before the event. In sum, going against expectations, downplaying the link between an event and a related political issue is not a typical strategy for these partisan outlets.

Further, I find that in the week before an event, discussion of these topics is very much ideologically sorted. That is, when left-leaning shows mention these issues, it is usually in a way that is convinced about climate change, in favor of stricter gun policy, and supportive of immigration. The opposite is true for right-leaning shows. This finding constitutes the first large-n measurement of ideological sorting on talk radio. The sizes of the opinion gaps are striking, especially in light of the fact that radio audiences themselves are not especially sorted, and do not seem to demand one-sided reporting per se.

Finally, when looking at the effect of events, I rarely find significant shifts in the positions that speakers take. The exception is shootings, which caused moderate increases in the proportion of anti-gun mentions on both sides. It is possible that many events in a row would slowly and cumulatively move positions, perhaps by gradually changing the tide of public opinion (cf. Baumgartner and Jones 2010). However, the fact that in two cases I find no detectable effect immediately after major events speaks against this hypothesis.

This paper shows that events amplify the amount of messaging about related political topics, without changing much about the mix of issue positions. As a result, in partisan media, events could have a polarizing effect, as audiences hear more ideologically motivated messaging on topics related to the event. This is illustrated by a curious finding about hurricane impacts. Usry et al. (2019)

uncover that in North Carolina, just after hurricane Florence made landfall, highly educated and partisan Republicans became *less* likely to see climate change as a threat. We also know that educated partisans are more likely to listen to like-minded partisan media (Stroud, 2011). If Republican partisans in North Carolina did so, they likely would have heard far more climate-skeptical messaging just after the hurricane than before.

Finally, partisan media may have an effect on politics whether or not they change audience attitudes—as long as party elites believe that they do. Calmes (2015) and (Hemmer, 2016, p.272-274) present convincing qualitative evidence that Republican politicians feel pressured by conservative outlets, presumably because of their effect on Republican voters. Talk radio is considered especially influential because it reaches people in rural states, including early primary states. For instance, when House majority leader Eric Cantor decided to soften his position on immigration late in his 2014 campaign, talk show host Laura Ingraham turned against him. She and other hosts are thought to have played a significant role in his primary defeat that year (Caldwell and Diamond, 2014). If politicians notice an increase in ideological messaging about a topic on partisan media, they may shift their own public positions on that topic, regardless of whether the public itself is actually influenced.

2.5.1 Future work

Having access to over a 1.5 year of continuous speech outputs from more than 1000 radio shows opens up plenty of future research possibilities. First, we might be interested to know whether there are categories of events that are simply too difficult to spin for partisan media producers. These are stories that might be very newsworthy, but that are nonetheless ignored by one side, because it is too challenging to cast them in a particular ideological light. Indeed, all the existing evidence for partisan underreporting of events is about bad news directly involving a party or candidate (Groeling, 2008; Puglisi and Snyder, 2011; Baum and Groeling, 2008). While a mass shooting can be framed as evidence for or evidence against gun control, a decrease in public approval for a president is difficult to spin as a victory for their party. Although talk radio shows are less aligned with political elites than other media (Calmes, 2015), we might still expect liberal shows to underreport bad news for Democrats, and vice versa for conservatives.

A second, related question is whether there are any less-notable event types that either conservative shows or liberal shows prefer to ignore. The events investigated in this paper are chosen to be very newsworthy–which helps us establish that position-taking does not change even in the face of extreme events. However, not reporting on these events at all would have been a challenge for a political outlet (although not connecting them to specific political issues would still seem to be an option, especially in the case of hurricanes and climate change). With slightly smaller-scale events, such as gay pride events or mine closures, disregard could be a viable strategy. As a next step, I could investigate whether this approach exists.

Third, one partisan media strategy that I did not investigate deeply in this paper, is differing baseline attention to political topics. While my analyses show how events influence the agenda, they do not look at how much attention liberal and conservative shows give to different topics in the long run. My findings from pre-event weeks contain hints that a show's average agenda is ideologically informed—for example, liberal shows mention the climate more, and conservative shows mention gun policy more. Further analyses, involving a broader range of topics, could show whether liberal and conservative shows tend to pay attention to different baskets of issues.

Finally, the data set includes hundreds of local shows, which are broadcast in only one city or state. As a result, it would be possible to look at how those shows respond to events that are strictly local.[‡] Hopkins (2018) argues that in the US, local conditions (e.g. air pollution) typically do not get translated into local public opinion about political issues (e.g. support for environmental spending). But there is one exception: when a national debate links the condition to the issue, then localities that are more affected do show more concern, spending support... for the issue. Perhaps the discourse on local outlets reflects this pattern: local events are not connected to issues, unless national media are already connecting this type of event to that issue.

2.6 Conclusion

If partisan media elites want to produce ideological content in the face of real-world events, they can take at least two different approaches. The first would be to strategically avoid connecting events to certain political topics. Surprisingly, I do not find much evidence for this strategy in political talk radio: regardless of a show's ideology, events clearly trigger a discussion of the political topics they are related to. The second approach would be to discuss topics in a way that is compatible with the outlet's slant—and fitting post-event discussions into that slant. I find that this strategy is very common. As a result, after a newsworthy event, partisan media audiences will hear a large amount of ideologically consistent discussion of political topics related to the event. On these media, events do not result in a shared narrative—instead, they give rise to two separate stories, with the potential of pushing partisans' understanding of political topics even further apart.

[‡]It would also be interesting to know how local shows react to local events that get national attention. While the hurricanes and shootings in this study fit that description, I did not collect enough events to make statistical claims about their local effects. For example, only 13 shows in the data set are local to (i.e., broadcast only in the state of) any of the mass shootings.

Chapter 3

In-group interest cues do not change issue attitudes

3.1 Introduction

Social groups are easy reference points for ordinary people trying to make sense of the political world. Compared to concepts such as ideology or values, groups are concrete, visible, and they play a large role in daily life. They are also connected to personal identities and emotions, in a way that makes them highly salient (Green et al., 2004; Mason, 2018). As a result, people's group memberships, and their attitudes toward their in- and out-groups, have the potential to be strong drivers of political opinions.

There are many reasons why members of a demographic group might feel differently about an issue: because the issue is associated with a disliked outgroup; because opinion leaders in the group have taken a public stance on the issue; because there is an opinion norm within the group; because action on the issue would confer social status to the group (e.g. marriage equality); because the demographic is correlated with ideology; and so on. In this study, I focus on the effects of in-group interests. I do this by presenting respondents with new information about how an issue affects their demographic in-group. For example, I ask whether women change their opinions about poverty after learning that women are more likely to be poor. By providing new linking information about issues that are not traditionally connected to the group, I distinguish the effect of interests from other aspects that might cause a group to care about an issue.

There are two mechanisms that could link attitudes about an issue to knowing the interests of a social in-group: self-interest and in-group favoritism. First, it is possible that information about in-groups affects opinion through people's understanding of their self-interest. Sears et al. (1980) baptized this mechanism "self-interest by proxy". Dawson (1994) called it the group utility heuristic, or "linked fate". According to this theory, group members can use the effects of

an issue or policy on their group as a whole to guess at how that issue or policy might affect them personally. For instance, a woman who learns that women are more likely to become poor, might revise her estimate of how likely she herself is to face poverty in the future.

The second mechanism is in-group favoritism: a desire to further the interest of in-group members, even if there is no benefit to oneself. Solidarity with the in-group may be a conscious choice (Tajfel et al., 1971) or an unconscious motivator (Dasgupta, 2004). According to social identity theory, in-group favoritism happens simply when someone is a member of (and feels some level of identification with) a social group. Indeed we know that in economic games, people prefer to give scarce resources to their in-group (e.g. Tajfel et al. 1971; Fowler and Kam 2007; Whitt and Wilson 2007). Similarly, Converse (1964) theorizes that social groups can influence political opinions once people are aware of their membership in the group, and of the connection between a policy (politician, party...) and that group. Often, this connection consists of material benefits that a policy or politician has provided for the group (Kinder, 1998). The treatments in this study provide precisely this type of linking information, showing that respondents' in-groups are disproportionately affected by an issue (and would therefore benefit more than others if the issue were to be prioritized).

For a few reasons, in-group favoritism may be a better explanation than self-interest for caring about the in-group's problems. First, people know their own life circumstances. The fact that members of a broad in-group suffer from a problem should have limited use for them in predicting their own experiences with the problem. Second, there is little real-world evidence that people's political opinions are informed by their self-interest (e.g. Citrin and Green 1990). Finally, in-group favoritism is thought to be strongest when the in-group is perceived to be doing worse than other groups (so-called fraternal deprivation Huddy et al. 2013)—as is the case for the issues in this study. Nonetheless, I do not rule out self-interest as a mechanism a priori.

In this study, I look at the effect of in-group interest cues on gender groups (women and men), racial groups (minorities and white people), and LGBT people. Among those groups, we would perhaps expect the interests of one's gender group to be the least powerful opinion driver. Gender gaps in opinion are generally relatively small, even on women's issues (Huddy et al., 2008). Existing research suggests that the racial/ethnic cleavage would be stronger, especially for minority members (Burns and Kinder, 2012). Although a large part of the literature focuses on negative sentiment towards racial out-groups, there is positive identification with racial in-groups as well, among Blacks but also (at least some) Latinos and Whites (Dawson, 1994; Sanchez and Vargas, 2016; Jardina, 2019). Finally, the LGBT people should be most likely to react to interest cues, since they are a relatively small social group (4.5% of the US population, Gallup 2018b) whose identity is already heavily politicized.

Surprisingly, findings show that new information about in-group interests has minimal effects on attitudes across all groups—even for people who identify strongly with the group. This suggests that in these cases, group thinking about political issues is not (just) about what is in the interest of the in-group. Issue

opinions are not driven by either linked fate or in-group favoritism. Instead, in-group–issue connections may be formed through repeated elite discourse or personal experiences, or they may be limited to issues that map "naturally" onto people's existing beliefs (stereotypes) about the groups. In sum, while some real-world issue opinions clearly are rooted in people's social identities, there seems to be no direct pathway from group interests with respect to an issue, to group-based opinions about that issue.

3.2 Experiment 1: Gender

3.2.1 Methods

This experiment had two phases: pre-treatment and treatment. In the pre-treatment phase of the experiment, respondents recruited via Mechanical Turk first indicated their gender (262 male, 222 female). I then measured their gender identity strength through Leach et al.'s (2008) Centrality subscale. Next, I asked about participants' attitudes toward four issues (poverty, depression, obesity and car accidents). I recorded three types of attitudes: concern about the issue, importance of the issue, and support for government spending to help tackle the issue. Then, for each issue, I recorded prior beliefs—asking participants whether they thought that the issue was more likely to affect women, men, or both at the same rate.

Participants were recontacted one week later for the treatment phase of the experiment. I randomly assigned each respondent to be treated on one of two issues matching their gender (poverty and depression for women; obesity and car accidents for men), or to be part of the control group. Treated respondents received information that their gender group is disproportionately affected by the issue. I chose issues whose connection to gender was real, but also little-known. So, as an example, one third of men were treated with information about men's higher obesity rates; one third read information about men being in more car accidents; and a final third were not treated at all. All of these statements are backed by data. In each case, the treatment referred to the source of the information, with a link to a web page.

Finally, all participants again indicated their attitudes toward all issues, and their beliefs about the gender imbalance in each issue. Appendix section B.3 contains the exact wordings of all survey questions and treatment statements. Appendix section B.1 has further design details, including flowcharts, sample descriptions, and justifications for my choice of dependent variables and of centrality as the identity strength measure. Pre-analysis plans for Experiments 1–3 can be found here, here and here.

3.2.2 Results

Below, I report estimates of the main effect of in-group interest cues on attitudes, and (in a second model) its interaction with identity centrality. I repeat both

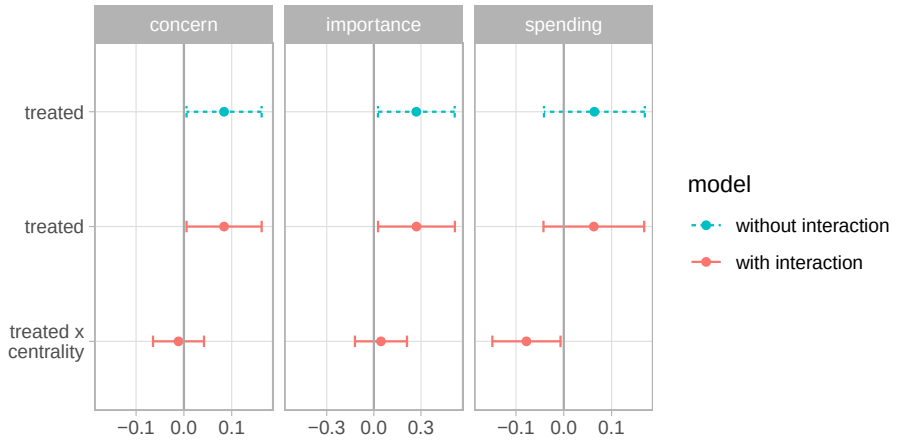

Figure 3.1: Effect of learning gender group interests on pre-post difference in three issue attitudes, without and with an interaction between treatment and centrality of gender identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

analyses for each of the three dependent variables (concern, importance, spending). The outcome variable is the difference between a participant's attitudes before treatment, and those after treatment. Appendix section B.1 contains more details about model specifications, including equations.

I find that in-group interest cues have small effects on issue attitudes, which are marginally significant at most. If a respondent learns that their group is particularly affected by an issue, their attitudes on the issue move only slightly more than those of a control respondent. The top line in Figure 3.1 shows this result. Cues increase concern by less than .1 on a four-point scale. They move an issue up by about .3 places in an eight-issue importance ranking. The effect of cues on support for spending (also a four-point scale) is equally tiny.

In order to further investigate the claim that average treatment effects are small, we can inspect 90% confidence intervals around the estimates (cf. Rainey 2014). A priori, the upper limits of such intervals have a 95% probability of being larger than the true average effect. Using this logic, we can reject effect sizes larger than .16 (concern), .51 (importance) and .17 (spending).

Finally, the interaction effect estimates in Figure 3.1 show how identity strength moderates results. The interaction is between treatment and group centrality as a measure of respondents' group identity strength. There is a small, marginally significant interactive effect on spending: unexpectedly, increasing centrality by one point (on a seven-point scale) *decreases* the treatment effect by about .1.

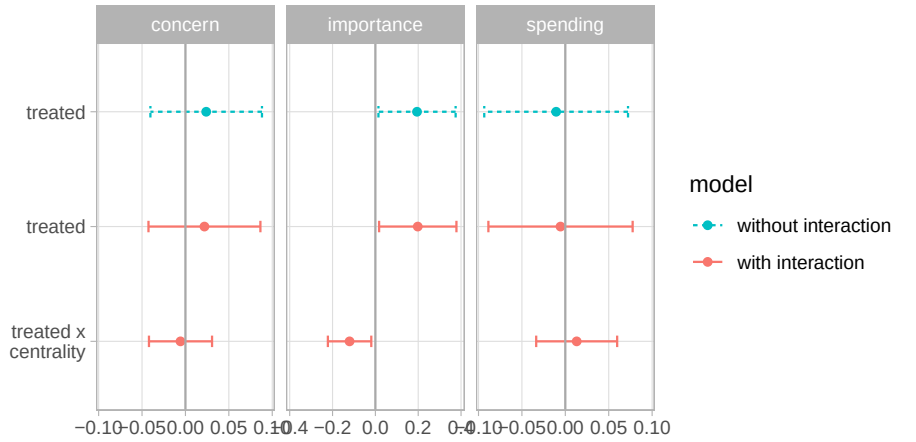

Figure 3.2: Effect of learning racial/ethnic group interests on pre-post difference in three issue attitudes, without and with an interaction between treatment and centrality of racial/ethnic identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

3.3 Experiment 2: Race/ethnicity

In this experiment, I ask whether White and minority (Black or Latino) respondents change their attitudes about an issue, after learning that the issue affects White people/minorities more than others. The design and analyses are the same as in Experiment 1, except that the groups and issues are different.

As before, respondents randomly received information about an issue that affects their racial/ethnic group disproportionately. The issues are: climate change and air pollution for Black and Latino people ($n = 267$); suicide and opioid addiction for White people ($n = 451$).

3.3.1 Results

Figure 3.2 shows the effect of the treatment on each of the three first-differenced issue attitudes. Connecting an issue to respondents' racial/ethnic in-group did not make them more concerned, or increase their support for government spending on the issue. However, it slightly increased the importance of the issue, moving it up .2 places on average in respondents' eight-place rankings. Inspecting the upper limit of the 90% confidence intervals, we can reject average effect sizes larger than .09 (concern), .37 (importance) and .07 (spending).

The one noticeable effect of in-group interest—its effect on issue importance—is in fact dependent on group centrality (identity strength). The interaction effect estimates in Figure 3.2 make this clear. Surprisingly, if centrality goes up by one (on a seven-place scale), the treatment effect is *decreased* by about .1. The interaction is significant.

Since Black, White and Latino Americans on average relate quite differently to their identity, I also explored effects for each ethnic group.* As above, among the dependent variable, only importance saw a (small, marginally significant) effect. This was only the case for Black and White people, however (Black—concern: .06, $SE = .09$; importance: .34, $SE = .24$; spending: $< .01$, $SE = .12$. White—concern: .03, $SE = .05$; importance: .22, $SE = .14$; spending: $< -.02$, $SE = .07$). For Latino people, all effects were smaller or even negative (concern: $-.06$, $SE = .09$; importance: $-.07$, $SE = .29$; spending: 0.01, $SE = .11$).

3.4 Experiment 3: Sexual orientation

In this experiment, I take the question of in-group interest cues to a third group setting: LGBT people. In the study, LGBT-identifying participants ($n = 198$) learn that two issues affect LGBT Americans more than other groups: unemployment and sexual assault.

Since the available sample of LGBT-identifying Mechanical Turk workers was relatively small, this study consisted of only one phase to avoid attrition. I did not measure the dependent variables before treatment, because participants might be hesitant to change an issue attitude they indicated just minutes ago. I also did not take pre-treatment measurements of people's belief in the connection between the issues and sexual orientation, because respondents might react differently to a treatment that felt like a correction of a belief they just stated.

Because of the between-subject experimental design, the outcomes are simply the post-treatment measurements of each issue attitude. To make up for the fact that I could not control for pre-treatment dependent variables, before treatment, I measured the personal importance of the issue for the respondent on a four-point scale, as defined by (Krosnick, 1990). In the results below, I use this personal importance as a covariate. Treatment effect estimates without the covariate are almost identical.

3.4.1 Results

Figure 3.3 shows the effect of the treatment on each issue attitude. Learning about an issue's connection with an in-group based on sexual orientation did not make respondents more concerned about the issue; it did not make the issue seem more important; and it did not increase their support for government spending on the issue. We can reject average effects larger than .17 (concern), .35 (importance) and .23 (spending).

As the interaction effect estimates in Figure 3.3 reveal, there is a marginally significant interaction effect between the treatment and group centrality when it comes to support for government spending. As before, the direction is counter-intuitive: if centrality goes up by one (on a seven-point scale), the treatment effect is decreased by about .1 on a four-point scale.

*This analysis was not pre-registered. See Appendix section B.2.1 for more on intergroup differences in identification.

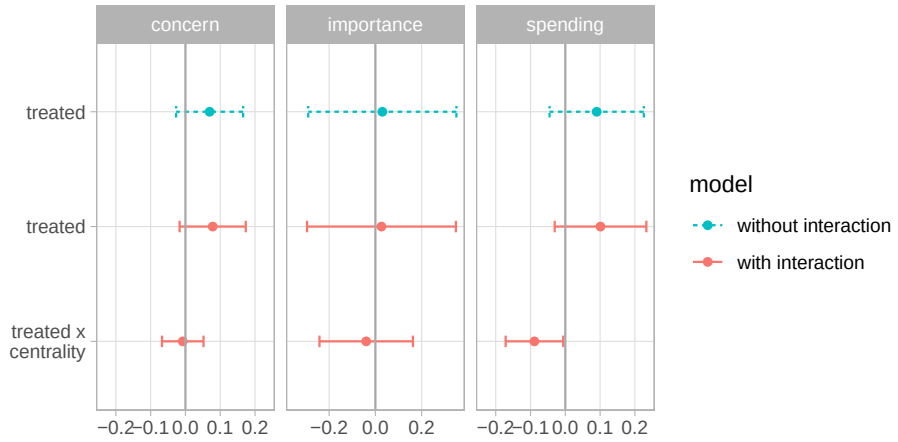

Figure 3.3: Effect of learning LGBT group interests on three issue attitudes, without and with an interaction between treatment and centrality of LGBT identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

3.5 Self-interest and linked fate

Linked fate implies that group members can use the impacts of an issue on their group as a whole as a useful indicator of how that issue might be affecting them individually. To test whether linked fate is the main mechanism behind the effects of in-group interests, Experiments 1–3 included measures of respondents' self-interest–namely, their perceived likelihood that the problem will be a threat to them in the future. Since the effects of in-group interest cues turned out so small, it would be surprising to find significant mediation of them through self-interest. Indeed, average causal mediation effects are negligible, even for the cases where we found a (marginally) statistically significant main effect (gender and concern: 0.009; gender and importance: −0.003, race and importance: −0.003; all $p > .1$). The treatment sometimes increases perceived threat to self, but the effects are small and non-significant (gender: 0.09, race: −0.02, LGBT: 0.17 on a four-point scale; all $p > .1$).

3.6 Robustness checks

Appendix section B.2 shows that the null or small effects of cues are robust to different specifications. Pooling data across experiments, I can exclude average effects larger than .11 and .15 on (four-point) concern and spending support scales, and effects larger than .40 on an (eight-place) issue importance ranking. Effect sizes are not consistently larger for any of the issue–group combinations. They are small even for respondents whose beliefs were actually changed by the

treatment, and even when I account for ceiling effects and anchoring. Finally, the section deals with alternative explanations, such as the idea that some people are simply reluctant to advantage their own group, or that respondents do not identify with the in-group members who are affected by the issues.

3.7 Discussion

In this chapter, I described three experiments testing the effect of telling people that an issue affects their social group especially. To isolate the effect of in-group interests, I chose issues that are not stereotypically connected to the group, and whose statistical connection to the group would be surprising to most people. I measured the effect of this new information on concern about the issue; its perceived importance compared to other issues; and support for government spending to tackle the issue. In all cases, I find that the in-group information has little or no effect on attitudes—even though respondents read the cues just before they indicated their attitudes. This suggests that in-group favoritism alone is not enough for people to care about these issues. Nor are their opinions moved because they use group interest as a heuristic for self-interest, as suggested by linked fate.

Any serious test of group bases for political behavior should recognize the difference between mere group membership, and identification with a social group (Huddy, 2001; Achen and Bartels, 2016). I find that, if anything, strong identifiers are slightly *less* likely to be affected by the in-group information. Robustness checks show that this cannot be chalked up to their pre-existing knowledge about group interests, but may be because they are reluctant to associate their group with a social problem. The findings suggest that identity by itself is not enough to create in-group favoritism; perhaps that requires a sense of group consciousness as well (Miller et al., 1981; Sanchez and Vargas, 2016).

The most plausible interpretation of these results is that connections between groups and issues do not flow directly from people's understandings of their own group's interests. Instead, group-based political thinking may work best for issues that either map "naturally" onto group frames, or that have been associated with groups through repeated framing efforts, in particular by elites. Winter (2008, 2006, 2005) shows that attitudes on such issues can be correlated with perceptions of groups even when people have not recently been primed with the group relevance of the issue (or have been primed only very subtly). This is true even when the connection to the group is symbolic rather than interest-based. Alternatively, issues could become connected to groups when the respondent has seen the issue affect members of their social group in real life. Group interest information may also be more effective when takes the form of a narrative rather than a statistic (cf. Betsch et al. 2011).

Related, precisely because these experiments involved giving people new information about the interests of their in-group, they did not include any issues that were firmly connected to a social group already. But perhaps political group

thinking is strongest if the group–issue link is widely known. This is because the way an issue is treated in politics can affect the social status of a group, as well as its material circumstances (cf. Sniderman et al. 2004). In turn, social identity theory suggests that people derive self-esteem from the status of their in-groups (Tajfel and Turner, 1986). But for this status effect to happen, most people (including out-group members) have to see a link between issue and group. That is not the case for the issues in this study. In sum, group–issue associations that are based on more than just group interests are likely to be less conscious or more emotionally loaded than the information-based connections in these experiment—and those kinds of associations may be more powerful.

While the pattern of results is quite clear, the conclusions from it need a few qualifications. First, there is abundant evidence on how (negative) sentiment towards out-groups, or ideological objections to helping those groups, can explain policy stances. In observational studies, attitudes toward racial or ethnic out-groups have been connected to the post-9/11 "war on terror" (Kam and Kinder, 2007), social security (Gilens, 2009; Winter, 2006), crime and drug policy (Green et al., 2006; Israel-Trummel and Shortle, 2018), and many more. In experimental studies, linking a policy to an out-group can tighten the connection between opinions about that policy and opinions about that out-group (Nelson and Kinder, 1996; Winter, 2006). Related, it is possible that the most politically relevant group-related traits are not people's group memberships or their social identities, but rather "group ideologies" such as feminism or racial conservatism (Burns and Kinder, 2012).

This study also does not contradict results on elite cueing (e.g., Nicholson 2012) or dissonant identity priming (e.g., Harrison and Michelson 2017), which suggest that in-group members are more credible as a source of political messaging. In-group favoritism is one reason to adopt the political views of fellow group members—but there are many others. It is also possible that in-group information informs behavior, but not opinions. For example, both Iyengar et al. (2008) and Bolsen and Leeper (2013) show that people seek more exposure to media content on issues that specially affect a social group they belong to. Finally, it may be that in the US, most issues are now so closely connected to political parties that they can no longer be "claimed" by other social groups—regardless of whether or not that claim is based on group interests. These experiments might have had different outcomes in less a politically polarized society.

3.8 Conclusion

Across five broad social groups and ten societal issues, I uncovered a surprisingly consistent pattern: new information connecting issues to in-group interests has little to no effect on opinions. This finding creates a new puzzle: what explains the group–issue connections that we find in the real world, for example in racial politics? Is it repeated priming by elites? Do real-world experiences create these connections? Or do some issues just naturally map onto group divisions, as Winter (2008) suggests? We know that group-based thinking about poli-

tics exists—knowing that it is not purely interest-based makes it all the more intriguing.

Chapter 4

The party bandwagon: Effects of opinion cues from ordinary partisans

4.1 Introduction

Citizens do not arrive at their political attitudes alone: they are, among other things, influenced by the opinions of people around them. There has been extensive evidence that the opinions of partisan elites are particularly influential. For example, experiments have shown that partisan respondents adapt their opinions on social security programs to the view associated with their party (Cohen, 2003). Levendusky (2009) uses observational data to demonstrate how this happens in the real word: in the US, elite signaling has caused more and more ordinary Democrats and Republicans to align their ideology with their party. Lenz (2013) similarly uses panel data and a survey experiment to show how often citizens look to their political leaders for cues on what issue positions to take. Finally, Broockman and Butler (2017) do the same in an ambitious field experiment (but see Butler and Hassell 2018 for a similar experiment with null findings).

So, it is surprising that there has been very little recent work on the effect of a very similar type of cue: information about the popularity of an opinion in a broad social group. I will call this type of information a "mass cue". Building on a 1980s tradition of research on the effect of polls, Mutz (1998) was the first to formulate a theory of so-called impersonal influence, describing the way ordinary people are impacted by their perceptions of large groups, such as fellow party members or the nation as a whole. However, different from elite cues, the effects of mass cues appear to be either small, or to show up under very particular circumstances (e.g. Mutz 1998; Lang and Lang 1984). Moreover, the very same cues seem to have a positive effect on some respondents, while having

a negative ("boomerang") effect on others (Marsh, 1985; Cloutier et al., 1989).

One possible reason for this discrepancy is that most studies on mass cues present information about nation-wide opinion, whereas studies of elites cues usually refer to elites that represent a party, or some other politically relevant group. However, what little evidence exists on mass cues about the in-group (as opposed to cues about opinion in the nation as a whole) suggests that those cues, too, are not particularly powerful. For example, when Mutz (1998) studied the effects on Democrats of other Democrats' evaluations of primary candidates, she still found that this in-party cue had no effect on average. And in an experiment by Kaplowitz et al. (1983), college students changed their opinions somewhat to conform with the majority of fellow students at their own university, but only on low-commitment issues (and also on high-commitment issues when their answers would be made public).

In this chapter, I use two pre-registered survey experiments to compare the effects of mass and elite cues directly. This comparison is interesting for four reasons. First, existing findings on the weakness of mass cues are curious in light of the recent "group turn" in political behavior. Theorists have long argued that groups play a key role in structuring political behavior (e.g., Madison 1787; Campbell et al. 1960), but recently, this idea is seeing a revival among political theorists (Achen and Bartels, 2016; Mason, 2018). Second, given the current low public trust in politicians (Gallup, 2018a), we might wonder why people would take their cues from distrusted political elites rather than from people like themselves. Third, as I explain below, mass cues help us better understand elite cues, because they are especially compatible with one of the two main competing explanations for why elite cues work. Finally, by comparing the strength of cues about the in-party versus the out-party, this chapter also contributes to the literature on affective polarization (Iyengar and Westwood, 2015; Mason, 2018). If respondents are more likely to move away from out-party views, than they are to move towards in-party views, this is compatible with the idea that negative feelings toward the out-party are a key political motivator in the United States today.

4.1.1 Mass cues to reveal mechanisms

In addition to reviving the literature on mass cues, this study can also help resolve the debate between informational and normative mechanisms for (elite) cue-taking. There is a lot of literature, including experimental studies, documenting the effect of elite cues on people's opinions. In general, people tend to move toward positions that are associated with the elites of their party, and away from positions connected to elite of the other party. However, there are a number of possible mechanisms for the effect (Leeper and Slothuus, 2014). On the one hand, there could be an informational explanation: people take the side of the majority because they infer that the majority must be correct. This is sometimes called the "consensus heuristic" (Axsom et al., 1987). Correctness does not have to be objective or universal: it can also mean that an opinion is "right" in the sense of being compatible with a person's values and/or inter-

ests (Downs, 1957; Lupia and McCubbins, 1998; Lavine et al., 2012). On the normative side, respondents can use cues to learn which opinions are seen as acceptable in their party, which is also a type of social group (Campbell et al., 1960). This can change their (expressed) opinions even if no other group member is monitoring their behavior. For example, in a survey setting, respondents may prefer to portray themselves as a good or typical member of their party for the benefit of the interviewer, or for their own psychological comfort.

Tentative evidence exists for both mechanisms. On the one hand, people do not only take opinion cues from their in-groups: for example, Lupia (1994) finds that Californians are more likely to take the "right" stance on a ballot proposition if they know which interest groups favor which position. These people are not following group cues because they want to belong—the cues are coming from groups that they are not even part of. Instead, they are presumably using the cues to work out which opinions match their own values or needs. And Broockman and Butler (2017) find small but significant effects of opinion cues from state legislators, even when those cues are communicated without any reference to the legislator's party. Here, information makes for a better explanation than group norms. Finally, Brader et al. (2013) find that in-party cues are more effective if they come from parties with greater ideological consistency, suggesting that supporters of those parties have more faith that each of those parties' stances will be aligned with their own values.

Other findings are more compatible with norm-based explanations. When we prime people's partisan identities before asking them about their opinions, those opinions become more polarized along partisan lines (Shani, 2009; Bartels, 2002; Conover et al., 1987; Gerber and Huber, 2010; Jerit and Barabas, 2012). In other words, party cues are also effective when they contain absolutely no new information. Just reminding people of their identity as a party member seems to motivate them to give answers that are consistent with that identity. Similarly, Gerber et al. (2010) encourage voters to register with a party (in order to participate in a primary), and find that encouraged citizens conform more with "their" party in their opinions and behaviors. This suggests that registering reinforced their partisan identities, which in turn made them look to their party for political guidance.

Also in support of the norm-setting mechanism, Levitan and Verhulst (2016) show that student participants tend to move their opinions toward the average opinion of small groups of other students—but that the effect is much larger when they learn the group's opinion from a short personal interaction rather than a computer message. The effect of the interaction seems to go beyond just revealing information. Finally, Petersen et al. (2013) establish that cues from a liked party tend to lead to longer response times on opinion questions for those respondents who end up disagreeing with the cue. The authors conclude that respondents are expending effort, because they are either trying (and failing) to justify adapting their opinions to the party cue, or trying to justify not following their party's lead. This would suggest that party view is a norm that people try to adhere to, rather than a heuristic that saves them thinking time.

Both of these elite cueing mechanisms—information and norms—apply to

mass cues as well. However, we can make clear predictions about which mechanism should be stronger in each case. Presumably, people think of their fellow party members as less knowledgeable on average than party elites (though our results below show that the difference is small). Therefore, they should be less valuable as a source of informational cues. On the other hand, the mass party is the social group that ordinary partisans are a member of. So, from a normative point of view, they should care more about what their fellow partisans think. In other words, if people use party cues primarily as heuristics, then elite cues should have an effect that is stronger than or equal to that of mass cues. If cues are instead used as social signals about group norms, then mass cues should be more effective. So, a side-by-side comparison of the effects of opinion cues coming from either ordinary or elite party members, helps us address the mechanism behind party cues.

In this chapter, I report on two survey experiments that compare the effects of mass and elite cues. In the first experiment, I use a nationally diverse sample, and I focus on cues coming from the in-party. In the second experiment, I use a Mechanical Turk sample, and I investigate the effect of out-party cues side-by-side with in-party cues. Previous research has sometimes found that respondents move away from opinions associated with their out-party (Nicholson 2012; Bechtel et al. 2015; Broockman and Butler 2017, but see Gelpi 2010 for more mixed findings). While both information and norms could explain this effect, out-party cues are somewhat more compatible with norms as a mechanism. Even if a respondents concludes from the out-party cue that a particular opinion must be incompatible with his or her values or interest, that may not be a very strong signal that the opposite position *is* compatible with them. However, if people are motivated to distinguish themselves from their out-groups, then going against an out-group norm might be rewarding in itself.

4.2 Study 1: national sample

In this experiment, I compare the effect of mass cues to the effect of elite cues on a range of political issue statements. Mass cues are presented as statistics about the opinions of ordinary Americans who identify with the respondent's in-party. Elite cues are statistics about the opinions of in-party politicians. The pre-analysis plan for the design can be found here.

4.2.1 Methods

The sample for this study consists of a nationally diverse group of 1000 respondents recruited by Survey Sampling International. Partisan "leaners" who identify as independent but closer to one party are counted as partisans. Pure independents, who feel close to neither party, are not included in the analyses.

The study covers eight issues: climate change, Medicare, taxes, the UN, immigration, affirmative action, marijuana, and same-sex marriage. Opinions about each issue are represented by agreement or disagreement with an issue

statement. For instance, climate change is represented by a statement about the EPA regulating greenhouse gases. Democratic and Republican respondents see different versions of the same issue statement, reworded in such a way that the position associated with their own party would be to agree with the statement. For example, for Republicans, the climate change issue statement is: "the EPA should be prohibited from regulating greenhouse gas emissions". For Democrats, the statement is "The EPA should regulate greenhouse gas emissions".

Each respondent receives cues about four randomly selected issues. For two of the issue statements, the respondent receives a cue that suggests a strong consensus in their in-party (around 90% agreement). For the other two statements, the respondent receives a cue that suggests their in-party is divided on the issue (around 50% agreement). These cues include pie charts showing the (supposed) level of agreement about the cue within the party. The respondent is not cued about the remaining four statements. A small subset of respondents (25%, the "pure control" group) receives no cues at all. That way, I can verify whether respondents who receive cues also change their answers on non-cued issues—that is, I can check for spillover.

After reading the cue (if there is one), respondents indicate on a 7-point scale how much they personally agree or disagree with each issue statement. Finally, I debrief respondents with the actual distribution of opinions on the statements they were cued about, using data from Broockman (2016).

4.2.2 Results

To calculate and compare the effects of mass and elite party cues on opinions, I pool the data on all eight issue statements for all respondents. The unit of analysis here is the person-issue, and n is 852 (8×1000, minus independents). A preliminary analysis suggests that non-treated issues for treated participants are not noticeably different from issues in the pure control group ($\overline{y}_{notreat} - \overline{y}_{control} = -0.08, CI = [-0.20, 0.04]$). For that reason, as specified in the PAP, we will use them as control observations.

Figure 4.1 shows the mean level of agreement with issue statements in the consensus (90% agreement) and division (50% agreement) cue conditions, compared to issue statements that were not cued. At first glance, it looks like all cue types have very small effects. I use regression analysis to investigate the effect sizes and their uncertainty.

Im this analysis, I regress the outcome variable (Issue Opinion) on two treatment variables: an indicator of whether respondent received a consensus cue, a division cue, or no cue on this issue (Cue Content), and its interaction with an indicator of whether respondent received mass cues or elite cues (Cue Level). Moreover, the regression includes person-level fixed effects (i.e., dummy indicators for each respondent). This means that Cue Level does not need to be included as main effects here, because it only varies between respondents. So, its influence is absorbed by the person-level fixed effects. This means that observations from the pure control group will not contribute to estimating any of the treatment effects, as the effect of being in the pure control group will be

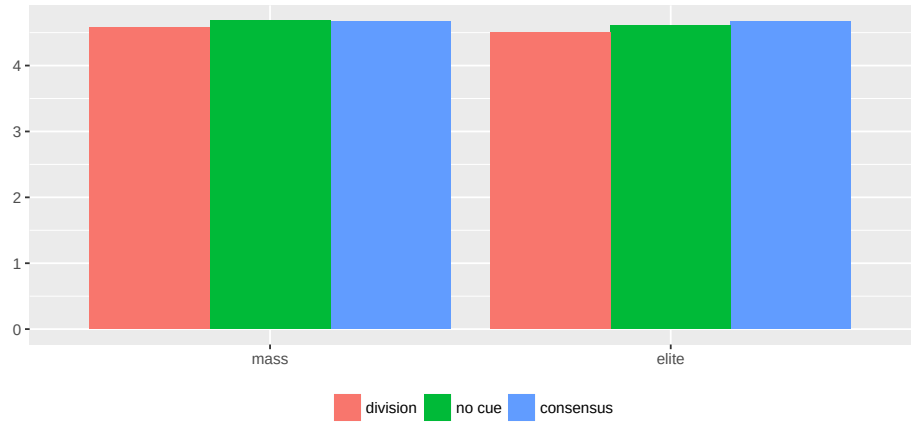

Figure 4.1: Mean agreement (on a seven-point scale) with issue statements after either a division cue (50% of in-party agrees), no cue, or a consensus cue (90% of in-party agrees). Cues are either about the mass party ("citizens"), or about party elites ("politicians").

absorbed by the dummies. I cluster standard errors at the person level.

Note that Republicans and Democrats see opposite versions of each statement, worded in such a way that the typical member of their party would agree with the statement (e.g. "prohibit" versus "allow" same-sex marriage). So, a higher score on this variable can be seen as stronger agreement with the position that is associated with one's own party. To keep observations from different issues comparable, the analysis includes an indicator for the issue addressed in the question, interacted with the respondent's partisan identity (Democrat or Republican). The main effect of partisan identity is absorbed by the person-level fixed effects. The remaining variation in the dependent variable can be thought of as variation in how strongly each respondent agrees with the issue statement, given which statement it is (e.g., the Republican version of the statement on climate change, or the Democrat version of the statement on immigration).

Figure 4.2 shows the results of this regression analysis: estimates of the effect of each cue type (mass and elite; consensus and no cue compared to division) as well as a comparison between elite and mass cue effects. As expected from the condition means, the effects are all minimal, and not significantly different from zero. Only the elite party cue comes close to statistical significance, So, participants who were cued about a consensus in their party seem to feel no differently about the issue at hand than participants who are cued about their party being divided. This is the case regardless of whether the cue is about the mass party or about partisan elites.

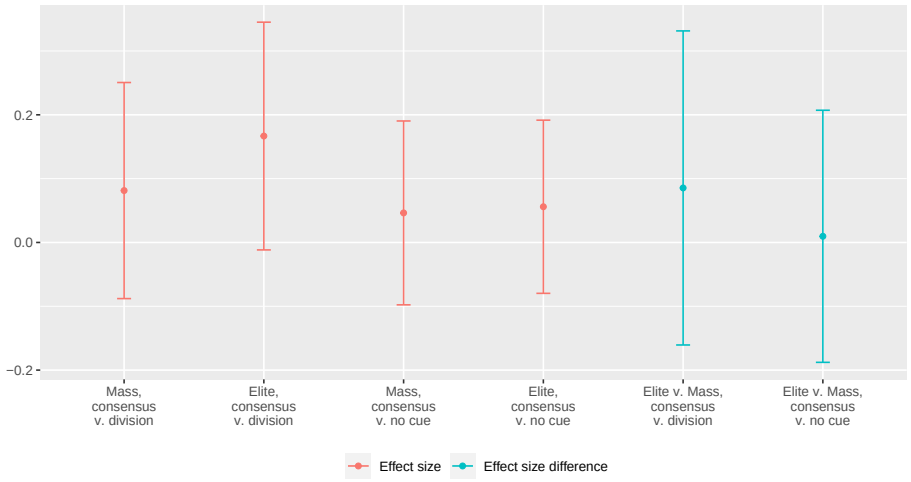

Figure 4.2: Effect sizes of a consensus cue (90% of in-party agrees) compared to either a division cue (50% agrees) or no cue. Cues are either about the mass party ("citizens"), or about party elites ("politicians"). Effect size differences show how elite and mass cues compare. Bands are 95% confidence intervals. Outcome variable is a seven-point agree-disagree scale.

4.3 Study 2: Mechanical Turk sample

While the null results from Study 1 are quite clear statistically, the study has two shortcomings that could be blamed for the lack of cueing effects. First, the information in the cues may not have been new or surprising to some participants. In Study 2, I control for this possibility by adding a pre-treatment phase to the design, where I ask about people's prior beliefs. Second, Study 1 only presents in-party cues. However, the desire to take distance from the out-party may actually be stronger than the desire to follow the in-party. For that reason, Study 2 also involves out-party cues. Finally, I add a number of covariate questions (e.g. trust in politicians and party members) to help uncover the mechanism behind cue-taking (or the lack thereof).

The pre-analysis plan for the study can be found here, and a number of minor deviations from the plan are marked with a † symbol and footnote.

4.3.1 Methods

Sample

The sample for this study consists of adult American respondents recruited in July and August 2018 through the Amazon Mechanical Turk crowdsourcing platform. After collecting a few demographics, I filter participants based on their partisan identities. I drop all "true independents" who report feeling close

to neither party, as our theory makes no predictions about their reactions to cues. True independents make up 14% of the starting sample. Moreover, since Democrats are strongly overrepresented on Mechanical Turk, I randomly drop 50% of Democrat respondents. Some participants drop out between phases: the retention rate is 71%. As a result, from the 1495 respondents I filtered in initially, I obtained a final sample size of 1072.

Experimental Procedure

In this study' participants' experiences vary on two dimensions. First, respondents are randomly assigned to either the mass or the elite version of the survey. In the mass cue condition, cues and covariate questions are about the opinions of ordinary Americans who identify with a party. In the elite cue condition, cues and covariate questions are about the opinions of partisan politicians. Respondents are also assigned to either the in-party or the out-party version. This means cues and questions will be either about their in-party (Democrats for Democrat identifiers, Republicans for Republican identifiers) or their out-party.

The experiment has two phases. In phase 1, I measure partisan identity—once again, partisan "leaners" who see themselves as independent but closer to one party than another are treated the same way as partisans. Next, I use a feeling thermometer to gauge the warmth or coldness of their feelings towards either mass or elite members or either their in- or out-party, depending on condition. Finally, I probe participants' prior beliefs about the partisan distribution of opinions in the US.

Asking respondents to estimate opinion distributions pre-treatment has two purposes in this set-up. First, it allows us to see how much the cue differs from respondents' prior beliefs. Cues that are further removed from a person's priors may have larger effects. Second, it gives us some insight in the real-world effectiveness of cues. If there is barely a connection between respondents' answers and the correct answers, then we know that respondents are not receiving, or not remembering, these particular types of mass and/or elite cues in their daily lives. I leave two to three weeks' time between this measurement and the actual treatment, to make sure probing priors does not affect respondents' reaction to the treatment.

Two to three weeks later, respondents are recontacted for phase 2, which includes the experimental manipulation (cue) and measurement of the dependent variable (opinion). Respondents first receive a cue suggesting there is a consensus (around 90% agreement) about an issue among either their in-party or their out-party. Cued issues are randomly selected from a set of eight issues. As noted above, the content of the cue depends on which condition the respondent is in: in-party or out-party, and mass or elite. After the cue, we ask respondents for their own opinion about all eight issues, including the issue they were cued about. Next, I ask participants to remember which issue they just read a cue about. I also ask how much they trust public opinion polls such as the one that was mentioned in the cue. At the end, I debrief respondents about the actual level of agreement about the statement in the cue, once again using data from

Broockman (2016).

Treatment: Cue

The issues covered in the study are climate change, medicare, taxes, and the UN, immigration, affirmative action, marijuana, and gay rights. Opinions about each issue are represented by agreement or disagreement with a policy statement related to that issue. For instance, cues and questions about climate change involve a statement about the EPA regulating greenhouse gases; gay rights cues and questions are about allowing same-sex marriage.

In the cueing step, each of the respondents receives the following message about one of the issues:

> These days, there are a number of political topics that almost all [Democrat/Republican] [Americans/politicians] feel the same way about. In particular, about 90% of [Democrat/Republican] [Americans/politicians] agree that:

Underneath this introduction is an issue statement. Each issue statement is worded such that we would expect the party in the cue to agree with the statement. For example, if the cue is about Republicans' opinion on climate change, the issue statement is: "the EPA should be prohibited from regulating greenhouse gas emissions". If the cue is about Democrats, the statement is "the EPA should regulate greenhouse gas emissions". In other words, we aim to convince respondents that intra-party consensus about the issue is high—not that the majority opinion in the party is the opposite of what they might have thought.

Dependent variable: Opinion questions

Finally, for the opinion questions, the respondent indicates on a 7-point scale how much they personally agree or disagree with each of the eight issue statements. These include the statement that the respondent was just cued about. In order to make sure that all respondents are faced with the same wording in their cue and question steps, *all* statements are worded such that we would expect the party from the cue to agree with them. So, if a respondent was just told that Republicans believe "the EPA should be prohibited from regulating greenhouse gas emissions", he or she will then be asked about his or her agreement with the same statement. Moreover, all of the other opinion questions will also be phrased so that "agree" corresponds to the typical Republican position. The opposite is true for a respondent who just received a cue about Democrats.

4.3.2 Results

Trust, knowledge and feelings

Figure 4.3 illustrates how participants perceive in-party and out-party elites, and ordinary partisans. It shows much participants trust these groups, as well

as how knowledgeable they find these groups of people, "when it comes to making judgements about the issues facing our country". Of course, the largest perceived differences are between the in-part and the out-party. Out-party members are trusted much less, and they are perceived as less knowledgeable. Within these groups, the differences between elites and normal partisans are surprisingly small. Elites are trusted slightly less, but are seen as slightly more knowledgeable than mass party members.

As Figure 4.4 shows, the results from the feeling thermometer reflect those of the trust question, except the in/out-group difference is even starker. That is, people are far colder toward out-party members than in-party members, and somewhat colder towards elites than towards mass members.

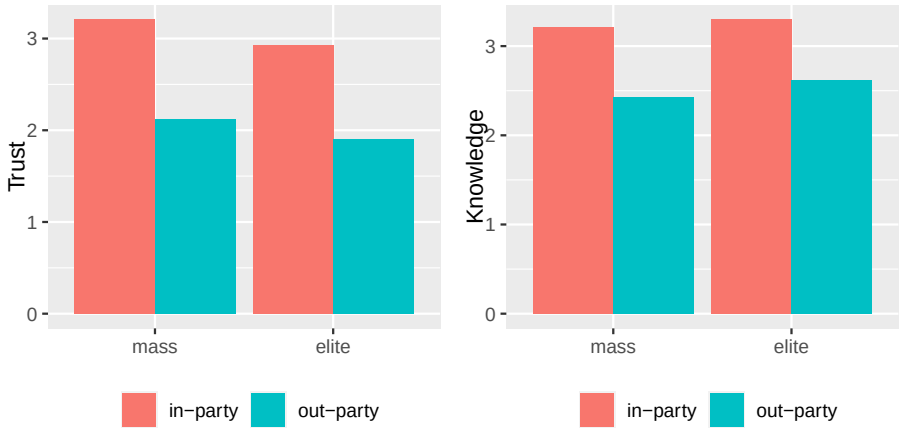

Figure 4.3: Trust in, and perceived knowledge of, in-party and out-party elites ("politicians") and mass ("citizens") when it comes to making judgments about the issues facing our country.

Priors

Next, it is worth inspecting participants' prior beliefs about the distribution of opinions in each party. These are participants' estimates of the levels of agreement with each issue statement, for each partisan group (Republican elites, Republican mass members, and so on). First, we can look the standard deviations of participants' priors. Averaged over levels (mass and elite) and partisan groups (Democrat and Republican), participants' guesses have a standard deviation of about 25 percentage points. In other words, there is a large amount of disagreement between respondents about the level of consensus in the parties about each of the issues.

Second, we can check the difference between participants' estimates and the true numbers, based on Broockman (2016). Table 4.1 shows the average (absolute) distances and average (signed) differences for in-party and out-party

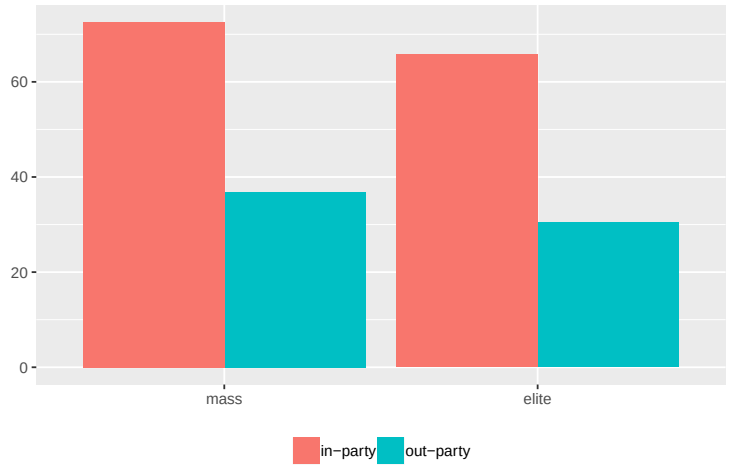

Figure 4.4: Feeling thermometer towards in-party and out-party elites ("politicians") and mass ("citizens").

	distance		difference	
	in-party	out-party	in-party	out-party
citizens	21	23	-2	-5
politicians	23	24	15	14

Table 4.1: Average distances and differences between respondents' estimates of elite and citizen consensus (within parties). Difference is the respondent's guess minus the true poll result. Distance is the absolute value of difference.

elites and mass members. Note that the ground truth for elite opinion is itself only based on a non-representative, 200-person sample of state legislators, while the ground truth for mass opinion is based on a representative, 1000-person sample. The distance metric shows that on average, people make very wrong guesses about the level of consensus in both their in-party and their out-party. The (signed) difference metric shows that these errors do not cancel out, at least for elites: on average, people underestimate the level of consensus among politicians by 13-15%.

Finally, there is a correlation of only 30% between the real level of consensus about an issue in a party, and participants' guesses. This would suggest that in real life, people only rarely encounter and absorb correct consensus cues—or at least that their perceptions of the parties are driven more by noise than by real information.[†]

[†]I did not pre-register this correlation check.

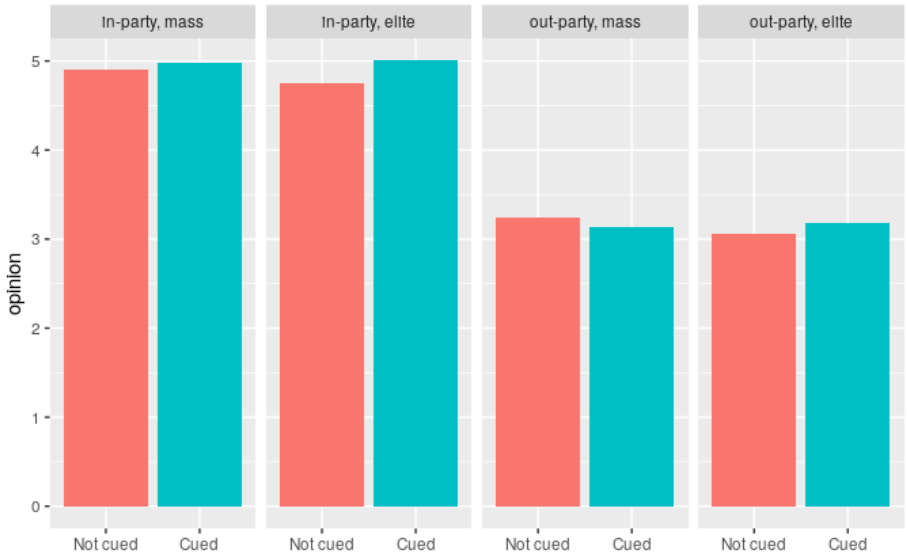

Figure 4.5: Mean agreement (on a seven-point scale) with issue statements with and without a consensus cue (90% of party agrees). Cues are either about the mass party ("citizens") or about party elites ("politicians"), and either about the in-party or the out-party. Respondents in in-party conditions are asked about statements that match their own party's position; respondents in out-party conditions are asked about statements that go against their own party's position.

Effect of party cues

To assess and compare the effects of mass and elite cues from in-party and out-party sources, I run a regression analysis pooling the data on all eight issue statements for all respondents. I do not analyze the 8% of respondents who could not recall which issue they were cued about. Results are not substantively different if these respondents are left in. Thus, the unit of analysis is the person-issue, and n is 7848 (8 issues $\times$ 981 respondents).

Figure 4.5 shows the mean level of agreement with both cued and non-cued issue statements in each of the conditions. Only in-party elite cues seem to have a noticeable effect on opinions; and even there, the effect is rather small. As before, I use regression analysis to estimate effect sizes and the uncertainty around them.

To get at treatment effects, I regress the outcome variable (Issue Opinion) on the two- and three-way interactions between indicators for whether respondent received a cue on this issue (Cued); whether the cue was about mass members or elites (Mass/Elite); and whether it was about the in-party or out-party. The regression includes person-level fixed effects (i.e., dummy indicators for each respondent). As a result main effects for Cued Party, Mass/Elite and

In/Out (but not Cued) can be left out, because these factors only vary between respondents. The same is true for the respondent's own party identity. Standard errors are also clustered at the person level.

Recall that respondents see opposite versions of each statement, depending on their condition. Republicans in the in-party condition, and Democrats in the out-party condition, see wordings that the typical Republican would agree with (e.g. "prohibit" versus "allow" same-sex marriage). The reverse is true for Democrats in the in-party, and Republicans in the out-party condition. So, a higher score on the outcome variable means stronger agreement with the position that is being cued.

To absorb variation coming from the fact that some respondents face questions about flipped issue statements, I also include covariates that indicate which issue the question was about (climate change, medicare, etc.), whether the wording of the question was Democrat or Republican in its direction, and the interaction between the two. The remaining variation in the dependent variable can be thought of as variation in how strongly each respondent agrees with the issue statement, given which statement it is (e.g., the Republican version of the statement on climate change, or the Democrat version of the statement on immigration).

Figure 4.2 shows the effect sizes of each cue type, and the interaction terms that capture their differences. Only in-party elite cues have a small effect on opinion: cued issues in that condition see about a .25 point increase (on a seven-point scale) in agreement.[†] Mass cues about the in-party, and both types of cue about the out-party, have no effect.

Moderating priors

Cues may be more impactful when they are far away from a person's prior belief. If the consensus cue of 90% agreement is much higher than the respondent's prior, we expect a larger effect, as the new information is more surprising. Of course, we could also theorize a smaller effect, if a larger differences makes the cue less believable, perhaps because participants do not trust the poll on which the cue is based (cf. Lord et al. 1979, Kuru et al. 2017). However, I find no systematic relationship between a person's prior on an issue and their response to a manipulation check about their level of trust in the cue. For that reason, I expect a surprising cue to have a larger or same-sized effect as a less surprising cue.

To investigate this possibility, I check whether priors moderate the effect of the cue. As specified in the pre-analysis, I only conduct this analysis for the condition where the cue actually had a statistically significant effect—that is, the in-party elites condition. However, the interaction effects between all treatments and the respondent's prior in percentage points are tiny (effect sizes are all

[†]In the pre-analysis plan, I planned only barplots for issue statement agreement by Cued, Mass/Elite, and In/Out status, but showing the effect sizes and their confidence intervals directly turned out to be clearer.

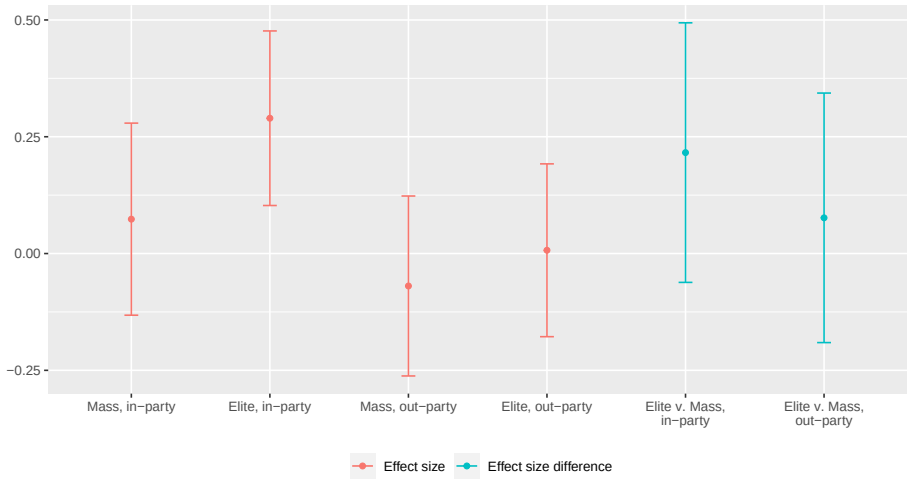

Figure 4.6: Effect sizes of each cue type, and difference between elite and mass cue effects within the in- or out-party conditions. Outcome variable is a seven-point measure of agreement with the (cued) issue statement.

< 0.01), meaning that surprising cues are no more effective than unsurprising ones.

4.4 Discussion

In this study, I investigated whether people tend to follow the opinions of ordinary members of their political in-groups ("mass cues"). Existing work mostly shows effects that are either small, or show up under very particular circumstances (e.g. Cloutier et al. 1989; Kaplowitz et al. 1983; Lang and Lang 1984; Marsh 1985; Mutz 1998). My findings suggest opinion cues coming from ordinary party members are not particularly strong in changing people's political views. This is true even when the cues are very different from what people believed to be true about their party (or the out-party). As a whole, these results are curious in light of the recent "group turn" in political behavior. Since parties are arguably the most politically relevant social groups in the United States, group-based theories of political behavior leads us naturally to the conclusion that parties-as-groups should be able to shape people's opinions.

Most of the time, I also found no effect of elite cues on opinions. Insofar as mass and elite cues both had non-effects, it is difficult to draw any conclusions about how and why they differ. As a result, I cannot make the anticipated contribution to the debate on the mechanisms behind cueing effects. Descriptively, it turns out that elites are perceived as slightly more knowledgeable than mass members, but that they are trusted less and people feel less warmly towards them. In other words, in the literature as a whole, elite cues outperform mass cues notwithstanding the fact that politicians seem somewhat less well-placed than citizens to set and enforce partisan group norms.

My null findings about elite cues stand in contrast to the host of studies, including experimental ones, documenting their effect (e.g., Cohen 2003; Levendusky 2009; Lenz 2013; Broockman and Butler 2017). However, a closer look at the literature shows that experimental studies of elite cue effects so far have actually found tremendously variable effect sizes. There is no consensus at all on the conditions that make the effects smaller or larger. Moreover, this amount of variability suggests that publication bias could be hiding a collection of non-findings in this area. Two recent field experiments on elite opinion leadership also lead to opposite conclusions (Broockman and Butler, 2017; Butler and Hassell, 2018).

To further investigate this situation, I conducted an exploratory meta-analysis of 18 experimental elite cueing studies, encompassing 45 elite cue treatments (see Appendix C for an overview). I found that Republicans tend to be slightly more sensitive to elite cues than Democrats. Cues that are unexpected given the party positions of the cue-giver are a little more influential than expected cues (cf. Druckman and Leeper 2012). Nonetheless, a large amount of the effect size variation remained unexplained. A large experiment, allowing side-by-side comparisons of elite cue effect sizes under all the possible key design choices, could help us solve this puzzle. The power of political elites to shape opinions raises big questions about the democratic ideal, so it is important to measure this power well—and to understand when it is strongest.

Of course, the conclusions from this study are bound by a number of limitations. First, since it is not a perfect replication of any existing elite cueing

study, it is hard to tell why its (largely insignificant) effect sizes differ from the literature. Second, in the real world, people are not likely to learn about mass or elite opinion through statistics. Rather, they learn what the average Democrat or Republican thinks through personal interactions. Elite opinions, too, come to citizens one by one, perhaps through quotes in the media or social media posts. Some elites have a much wider audience than others. This also explains why respondents did such a poor job of estimating opinion consensus among both citizens and elites. Norms and information that are transmitted through interactions or individual stories could well be more influential than a poll result (Aarøe, 2011; Levitan and Verhulst, 2016).

4.5 Conclusion

One way in which people can form opinions about complicated political issues, is by learning which stance on the issue is associated with "their" party, and which one with the other party. This can mean at least two things: the opinion that is most prevalent among ordinary partisans like themselves, or the opinion held by politicians in their party. In this chapter, I set out to measure which type of partisan cue is more influential. I found no effect at all of learning that there is strong (or weak) consensus about an issue among members of either party. I also generally did not any effect of cues about politicians' opinions. These findings are surprising both theoretically (as social groups are generally thought to be strong drivers of political thinking) and in light of the empirical literature on elite cues. The findings highlight how design choices could play a large role in the size of cueing effects. They reveal a need for meta-analyses and large-scale experiments investigating the heterogeneity in the effect of opinion cues.

Acknowledgements

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Finally, my gratitude goes out to everyone who provided comments and advice on the different chapters of this dissertation: Martin Bisgaard, Michael Bossetta, Malte Dahl, Peter Thisted Dinesen, Matt Grossman, Camilla Bjarnøe Jensen, Patrick Kraft, Jonathan Ladd, Asmus Leth Olsen, Michael Bang Petersen, Arthur Spirling, Rune Slothuus, Dominik Stecuła and Michael Wagner.

Appendix A

Higher volume, same tune: supplementary material

A.1 Data set

Between May 2018 and today, the number of stations followed went from 72 to around 160. Of the initial stations, 50 were chosen randomly from the population of US talk radio stations, and more stations were added and dropped from the data set in the course of the next few months according to the geographical interests of the team at the Laboratory for Social Machines. Figure A.1 shows where in the United States the transcribed stations are located. Table A.1 describes the distributions of these stations in terms of content and station subtype, compared to the population of all talk radio stations. Underrepresented station types include stations from the Midwest, college stations, and public radio stations.

	Sample %	Population %	t-test p-value
region: Midwest	16	28	0.00
region: Northeast	22	16	0.04
region: South	33	30	0.35
region: West	29	27	0.46
format: Business News	0	1	0.09
format: College	2	13	0.00
format: News	2	2	0.91
format: News/Talk	49	27	0.00
format: Public Radio	32	45	0.00
format: Talk	15	12	0.18

Table A.1: Balance table comparing region and format for radio stations in our sample, to the population of US talk radio stations.

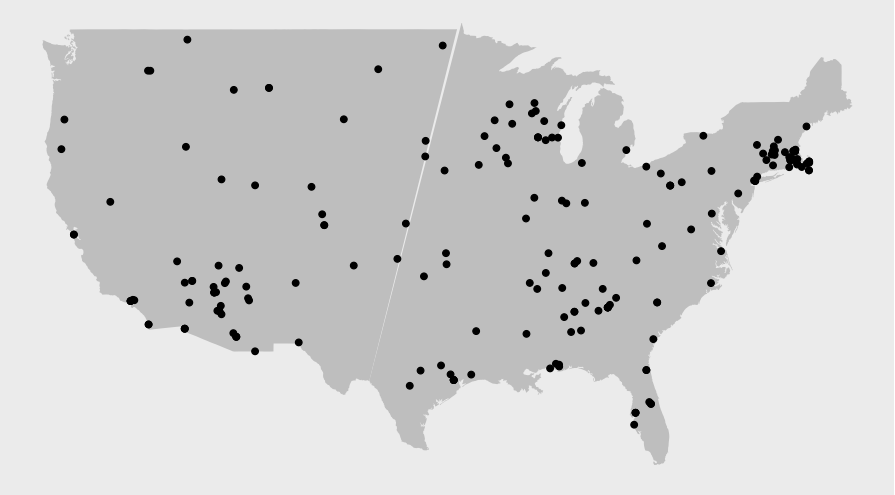

Figure A.1: Locations of the continental radio stations in the data set. Two more stations are in Alaska, and one is on Hawaii.

The speech transcription algorithm was gradually improved in the course of data collection, with the error rate going from 27% in April 2018 to 13% in November 2018, and staying stable since. This error rate was measured using existing transcripts from NPR and Rush Limbaugh, whose audio is likely a little easier to transcribe than the average show. Beeferman et al. (2019) describe the features of (a subset of) this data set in more detail.

Audio and transcriptions from all radio stations, supplemented with information about the station's weekly schedules and with Google trends data, were used to measure both dependent and independent variables in this project. Figure A.2 illustrates the full workflow.

Figure A.2: The project workflow, from data over dependent variable (topic mentions and positions) and independent variables (pre or post-event week and show ideology), to regression models.

A.2 Ascribing mentions to shows

In principle, ascribing a topic mention to a show should be easy. A topic term counts as being part of a show if it was mentioned on a radio station, during a time slot when we know that show is being broadcast on that station. There are two issues with that approach, however. First, the speech-to-text algorithm creating the transcriptions is not perfect. Some topic term mentions are missed, whereas others are false positives. Second, the radio schedule data comes from a range of sources, some more reliable than others. An hour of audio coming from a particular station could have the wrong show label if we do not have a correct, up-to-date schedule for the station.

Both problems can be remedied when there is more than one "airing" of the show–that is, when the show is broadcast on more than one of the stations that were recorded that day. I followed the following procedure to decide how many (and which) topic mentions should be ascribed to each show.

1. For each topic mention, create a "slice" of content that includes up to ten words that coming before and after the topic term. For example: "years from now, fifty years, there isn't any evidence of climate change" (aired on KBTK, April 25th 2018).

2. In the entire set of transcripts coming from a particular day, search for clusters of similar mentions (low string distance).

3. For each cluster, do the following:

   (a) Take all mentions in the cluster, and check which show labels they have, based on station scheduling data. For example, the first mention above comes from audio labeled as The Glenn Beck Program, but it is in a cluster together with five (very similar) mentions that are all labeled as being part of the Rush Limbaugh Show.

   (b) For each of the shows that occur at least once as labels in the cluster, calculate confidence that it is the correct show label for this cluster of mentions. Calculations take into account how many mentions in the cluster have this show label, and also how many times each show was aired on different stations that day, without containing a similar mention.

   (c) Compare the show label likelihoods, and choose the one with the highest confidence. For example, the mention cluster above was ascribed to The Rush Limbaugh Show with a confidence of .6. This relatively low confidence mostly comes from the fact that there are many airings, labeled in the scheduling data as broadcasts of The Rush Limbaugh Show on April 25th 2018, that did not include any mentions with a content slice similar to this one.

4. Treat each cluster, with its most likely show label, as a single unique mention that happened on that show–but only if its show label confidence is greater than .5. Otherwise, discard the cluster.

A.3 Mechanical Turk task

Each time one of the political topics was mentioned, the position taken was coded by workers on Amazon's crowdsourcing platform, Mechanical Turk. Workers were allowed to code as many mentions as they wanted, for a payment of $0.14 per mention.

In the case of climate change, for 71% of mentions, the first two coders agreed on the classification. In another 25% of the cases, a third coder broke the tie, and I used the majority opinion as the code for that mention. In the final 4%, all three coders disagreed, and I labeled the fragment "neither". In the case of gun policy, the distribution was: 61% two-coder agreement; 32% two-out-of three majority; 6% no agreement. Immigration fragments were the most difficult to code: the percentages were 54%, 37%, and 9%. This is largely because coders differed on whether mentions supported any position, or should go into the "neither" category instead.

Human coders listened short audio fragments surrounding each mention. This meant that transcription errors were not an issue, and also that the ratings are based on vocal as well are verbal cues. We know from previous work that tone of voice confers unique information (Dietrich et al., 2019). Audio fragments started 10 seconds before the topic-related phrase (e.g. "global warming") was said, and ended 20 seconds after. After extensive pre-testing, I found that longer fragments very rarely provided information that would change one's initial judgment. Next, the coder was asked to choose between two positions (e.g., "skeptical" or "convinced" about climate change), or "neither" position

Instructions received minor tweaks during the coding process, in order to account for common mistakes. Right next to the audio player, coders always saw the following brief instructions on their screen:

Climate:

- Skeptical: climate change evidence is false or unclear, climate change is not an important problem, it is too costly to fight against climate change.
- Concerned: climate change evidence is solid, climate change caused by humans, it is a threat and we need action.
- Neutral: no clear opinion about climate change, and no mention of evidence for or against climate change.

Gun policy:

- Pro-gun: right to own guns, looser gun control laws, guns protect people, second amendment
- Anti-gun: stricter gun control laws, guns cause violence/mass shootings
- Neither: no opinion about gun rights/ gun control, no hints whether the speaker is pro- or anti-gun.

Immigration:

- Supporting immigration: we don't need a wall or more deportations, families should stay together, immigration is good for our country
- Tough on immigration: border needs protection, illegal immigration should be stopped, immigration is bad for our country
- Neither: just news, no opinion about immigration, no hints whether the speaker is supportive or tough.

Finally, coders were encouraged to click through to the longer instructions ("code book") if they were doing the task for the first time, or had not done the task in the past day. The sections below contain the descriptions of each topic position in the final code books.

In the code books, coders were encouraged to classify mentions as "neither" if there was not enough context to classify them, if they did not fall into any of the other categories, or if the audio fragment was not about climate change. For example, a piece of news (with no negative or positive tone) about a law that was passed in Congress, or a commercial about climate-proofing your windows. However, the code books also explained that topic mentions can support positions even if the speaker is not giving their own opinion. For example, a news item about new evidence for (or against) climate change would still count as concerned (or skeptical) and not neutral, because its effect could be to make a listener more concerned (or skeptical).

Climate, Skeptical - "The evidence for climate change is false or not certain; predictions did not come true." "Scientists are hiding evidence against global warming." "The climate is always changing." "Humans did not cause global warming." "Problems we see today (e.g. wildfires) are not caused by climate change." "Even if global warming exists, the effects are not so bad, or they are positive." "Climate change is not important compared to other problems." "It is too expensive or risky to take action, it would cost too many jobs, it is too soon to take action, it is not our responsibility."

Climate, Concerned - "The evidence for climate change is clear." "Humans and their greenhouse gas (CO_2) emissions cause global warming." "Climate change will have negative effects (e.g. sea levels rising, plants or animals dying) now or later." "Problems we see today (e.g. storms, droughts) are due to global warming." "We need to act on it (e.g. by using less energy or clean energy)." "People who are looking for solutions or who are passing climate laws are doing the right thing."

Gun policy, Pro-gun - "People have the right to own guns, protected by the second amendment." "The government should not take our guns away." "There should be fewer laws and rules about owning or buying guns and ammo (e.g. bullets)." "Gun control does not prevent crimes." "People who own guns prevent crimes from happening, because they can defend themselves, their family, and others." "People need guns to protect themselves if the government turns against the citizens." "Mass shootings are a mental health problem." "The US does not have more gun violence because it has more guns."

Gun policy, Anti-gun - "There should be stricter rules about who can own and buy guns." "People who want to buy a gun should have to pass a background check or get a license." "Some types of guns, like assault rifles, should be banned." "We need stronger measures to prevent teenagers, or people with mental health problems from having guns." "The United States has more gun violence than other countries because it has more guns." "Mass shootings would happen less often if it was harder to get a gun."
Immigration, supportive - There should not be a wall on the border with Mexico, and we should deport fewer people. Unauthorized immigrants are often running from violence in their home country. They should be treated well and families should stay together. The rules for legal immigration should not be made stricter. People who were brought into the country as children should be allowed to stay. Immigrants are hard-working, and they contribute to our society. America is a nation of immigrants.
Immigration, tough - We should invest more money and manpower into protecting the border and deporting unauthorized immigrants. If immigrants come or stay here illegally, they broke the law. Immigrants they raise crime rates, they do not pay taxes and should not get government help. We also need stricter policies on legal immigration. Many immigrants don't speak English well, don't adopt American culture, or take jobs from Americans. American-born citizens should come first.

A.4 Modeling mention proportions and counts

For each week on each radio show, I am interested in two outcomes: the number of mentions of a topic; and the proportion of mentions that advocate different positions.

The number of mentions of each topic across shows-weeks has a very skewed distribution. Given that, a linear model of mention counts would have large uncertainty around its coefficients. Moreover, conclusions would be heavily dominated by a handful of shows that have far more mentions than the others. Instead, I use a negative binomial model. The coefficients in this model tell us about the *proportional* change in the outcome variable associated with a change in the predictors.

The full model, which tells us about the differential effect of events on shows with different ideologies, is:

$$\mathbb{E}[Y_{iw}^{count}|T_w, I_s] = exp(\beta_0 + \beta_1 T_w + \beta_2 I_s + \beta_3 T_w I_s + \beta_4 A_s)$$

where Y_{iw}^{count} is the number of topic mentions on show s in week w. It has a negative binomial distribution. A_s is the show's airtime in minutes per week. I control for total airtime because shows with more content obviously have more opportunities to mention a topic.

To model position proportions, a so-called fractional response, I use a generalized linear model (GLM) with a logit link function and a quasi-binomial prob-

ability mass function (PMF) for the outcome (Papke and Wooldridge, 1996).* This is also known as a fractional logit.

The full model is:

$$\mathbb{E}[Y_{sw}^{prop}|T_w, I_s] = logit^{-1}(\beta_0 + \beta_1 T_w + \beta_2 I_s + \beta_3 T_w I_s)$$

where Y_{sw}^{prop} is the fraction of topic mentions on show s in week w, that support a particular position (e.g. climate skepticism). It has a quasi-binomial distribution. T_w indicates whether the week is a pre-event or post-event week, and I_s is the ideological leaning of show s. In all cases, when estimating these models, I cluster standard errors at the show level.

A.5 Google trends and topic mentions

To define "pre-event" and "post-event" weeks, I use Google Trends data. They give a day-by-day index of the number of Internet searches for a search term describing the event (e.g., "hurricane Florence" or "hurricane Michael"). On each day, I compare search activity to the peak-activity day for that event. By my definition, pre-event weeks end on the last day where search activity was less than 5% compared to the peak. Post-event weeks start on the first day where search activity was at least 20% compared to the peak.

Figure A.3 shows these periods, along with the number of topic mentions per day on all talk radio shows.† While these decision rules do not always line up perfectly with the "before" and "after" of talk radio attention, they do a reasonable job of capturing the baseline and the peak. In the figures, we can also see that the "pre-event" weeks are acceptable baselines, even though no week is ever completely free of (at least local) events that are relevant to these political topics.

*Using a quasi-binomial PMF instead of a binomial one does not change the estimates but gives us more robust standard errors (Papke).

†These are unique mentions, not double-counting mentions on radio shows that are broadcast on more than one station. See Appendix A.2 for more on this.

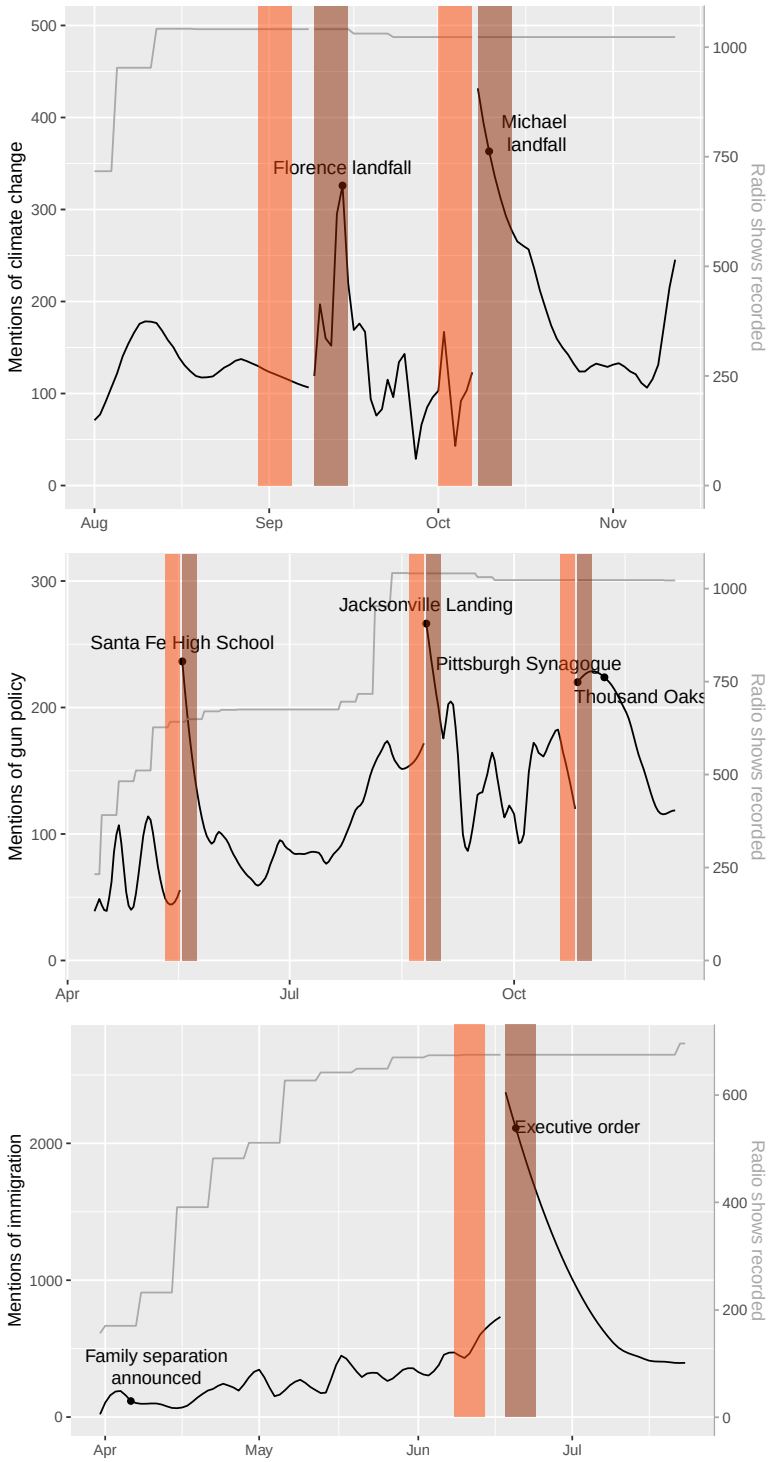

Figure A.3: Daily number of talk radio mentions of climate, gun policy and immigration. Dots are significant events. Bands show the pre- (orange) and post- (brown) event weeks as defined by Google Trends. Mention trends smoothed using Loess regression, allowing for discontinuities at the beginning of each event's "post" week. Light grey lines show the number of talk radio shows recorded on each date.

A.6 Classifying radio shows

Table A.2 contains all non-political shows that I used to train the political/non-political classifier. Table A.3 lists all political shows. For each show, the table includes at least two sources backing its ideological label. A source is considered to confirm an ideological label if it names the program, its host, or another closely associated entity as either "conservative" or "right-wing"; or as "liberal", "left-wing" or "progressive".

Requiring more than one source hedges against the possible ideological bias in ideological bias judgments themselves. For example, the Center for American Progress is itself classified as left-wing by the Media Bias/Fact Check group. I had to leave a number of potentially ideological shows out of this training set because I could find only one source to confirm its slant. I never came across two sources that contradicted each other in their judgments of any given show's bias; when two sources existed, they always agreed.

Note that the liberal show training set includes two NPR programs: All Things Considered, and Morning Edition. Section 2.4.1 in the body of this thesis justifies this choice, but also provides a robustness check that leaves NPR shows out of the training set.

In total, the labeled shows had almost 8500 episodes, of which almost 5800 were political. Before training the model on these shows, I held out 10% of each show's episodes, to be used for model testing. To transform the show transcripts into data, I counted and normalized the number of occurrences of 5000 word pairs in each transcript. In other words, the features fed to the model are term frequency–inverse document frequency (TF-IDF) vectors for 5000 bigrams. I left out any features whose TF-IDF score was correlated too strongly with any particular show label–for example, hosts' verbal tics, their names or shows sponsors.

Existing literature on categorical ideology classification at the phrase (Iyyer et al., 2014) or document (Yan et al., 2017) level suggests that regularized logistic regression (LR) works well with this amount of data. I tried both LR (with L2 regularization) and Support-Vector Machines (SVMs). I decided between these two models, and tuned both the show-specific feature correlation threshold and the shrinkage parameter c, via blocked k-fold cross-validation. That is, I left out all episodes from the same show at once, and then tried to predict their label with a model trained on the other shows. LR slightly outperformed SVM for the political/non-political model, and SVM slightly outperformed LR for the ideology model.

Once tuned, I tested the final models' performance by using them to label the hold-out episodes of each show, which were all completely new to the model. For each show, when trying to classify its hold-out episodes, I trained a model on all *other* shows. This way, I avoided rewarding the model for making predictions based on show-specific features. The political/non-political LR correctly classified all 50 shows based on their hold-out episodes. The conservative/liberal SVM successfully classified all 17 political shows.

After tuning and testing, I trained the SVMs on the full labeled data set

Show	Topic
Food Friday Vox Pop	food
WMT Cooking Show	food
Better Lawns and Gardens	gardening
Classic Gardens and Landscape	gardening
GardenLine w/ Randy Lemmon	gardening
Dr. Bob Martin	health
Purity Products	health
Your Health with Dr. Joe Galati	health
At Home with Gary Sullivan	home
House Talk with Ray Trimble	home
Sturdy Home Improvement	home
Texas Home Improvement	home
Handel on the Law	legal
The Legal Exchange	legal
Your Legal Rights	legal
Financial Advisors with Aubrey Morrow	money advice
Money Matters with Ken Moraif	money advice
The Dave Ramsey Show	money advice
The Financial Exchange	money advice
Afropop Worldwide	music
Afternoon Jazz	music
Classic Jazz with Michele Robins	music
Classical 24 with Andrea Blain	music
Classical 24 with Bob Christiansen	music
Homegrown Music	music
Jesus Christ Show (PRN)	religious
Lutheran Hour	religious
St. John's Lutheran Church	religious
Ben Maller	sports
Buckey Sportsman with Dan Armitage	sports
FOX Sports Radio	sports
Fox Sports Weekends	sports
The Big Sports Show	sports

Table A.2: Non-political shows in training set, with their hand-coded topic.

(training and hold-out). I applied the political/non-political classifier to all 1005 shows. Of those, 576 shows were labeled as non-political. Next, I applied the ideology classifier. It labeled 249 of the political shows as conservative, and 180 shows as liberal. This number of liberal shows is somewhat surprising, as talk radio has a reputation for being overwhelmingly conservative (The Center for American Progress and Free Press, 2007; Berry and Sobieraj, 2013). The answer lies partly in the fact that conservative shows reach far larger audiences: of the ten most-listened-to syndicated talk radio hosts in 2019, only one (Thom

Conservative

Show	Sources
Ben Shapiro	Media bias/fact check (as The Daily Wire), Politifact, Wikipedia
Glenn Beck	CAP, Pew, Wikipedia
Hugh Hewitt	CAP, Media bias/fact check (as Salem Radio Network News), Wikipedia
Joe Pags	CAP, Wikipedia
Laura Ingraham	CAP, Politifact, Wikipedia
Mark Levin	CAP, Media bias/fact check (as Conservative Review)
Mike Gallagher	CAP, Wikipedia
Rush Limbaugh	CAP, Pew, Politifact, Wikipedia
Sean Hannity	CAP, Pew, Politifact, Wikipedia
The Savage Nation	CAP, Politifact, Wikipedia

Liberal

Show	Sources
All Things Considered	Media bias/fact check (as NPR), Pew (as NPR)
Democracy Now!	Media bias/fact check, Wikipedia
Mike Malloy	CAP, Wikipedia, Liberal Talk Radio Wiki
Morning Edition	Media bias/fact check (as NPR), Pew (as NPR)
Ring of Fire Radio	Media bias/fact check, Wikipedia
Stephanie Miller	Media bias/fact check (as Fstv), Politifact, Wikipedia, Liberal Talk Radio Wiki
Thom Hartmann	CAP, Media bias/fact check (as Fstv), Politifact, Wikipedia, Liberal Talk Radio Wiki

Table A.3: Political shows in training set, with their ideology label and sources. Sources: CAP (Center for American Progress and Free Press, The Structural Imbalance of Talk Radio, 2007, ampr.gs/2UegLbP); Liberal Talk Radio Wiki (ltradio.fandom.com/wiki/List_of_personalities); Media bias/fact check (mediabiasfactcheck.com); Pew Research Center (journalism.org/interactives/media-polarization); Politifact (politifact.com/personalities/); Wikipedia (wikipedia.com). The entity labeled by the source is in parentheses, if it is something other than the show or its host.

Hartmann) can be labeled liberal (Talkers, 2019). Liberal shows are more small-scale and/or local (Berry and Sobieraj, 2013, p.129). For instance, they may be podcasts that are also carried on one or a handful of terrestrial stations.

There are a few reasons to treat show ideology as binary, rather than continuous. First, existing evidence suggests that radio shows are ideologically sorted, suggesting that it is reasonable to divide shows into a liberal and a conservative group. Second, having two ideology categories is a common choice in stud-

ies of talk radio (cf. Yanovitzky and Cappella 2001, Sobieraj and Berry 2011, The Center for American Progress and Free Press 2007, Jamieson and Cappella 2008, p. 86). The classification results support this–most political shows can be classified with fairly high confidence as either liberal or conservative, suggesting it is not as important for a model to cover the ideological "middle ground". Finally, it is more believable to classify the training data into ideological bins. Sources that designate radio shows as right- or left-leaning usually give categorical labels. In a study on TV news, Martin and Yurukoglu (2017) solved this by training a classifier on Congressional speech, with continuous DW-NOMINATE scores as the ideology outcome variable. I found that a domain-adapted binary classifier trained on speeches in the 114th Congress misclassified 3 out of 17 political training shows. Switching to a Congress-based model could thus lead to a significant drop in prediction quality.

A.7 Long-term effects: agenda half-lives

Studies of the agenda-setting power of events in mainstream media often find effects that last for months (Lawrence, 2000; Birkland, 2004; Zhang et al., 2017). In this Appendix, I describe how trends in topic mentions tend to evolve, once they have peaked after an event. In other words, I analyze how quickly attention to a topic dissipates. The graphs in A.5 already give us some visual cues about how long the effects of events can last. Here, I fit a model to the post-event trends.

For each topic, I look at total topic mention counts in the month after each event, day by day. Because I am interested in the downward trend, the start of this month is not the start of the post-event week (which I defined earlier as the first day on which the event reaches some level of social significance). Instead, it is the peak of talk radio attention to the topic: the day with the most mentions.

To avoid catching the beginning of attention to the next event, I leave out any days that fall in the post-week of the next event. This results in six dropped observations for climate change, and six for mass shootings. Finally, for the Pittsburgh Synagogue shooting, I only include the first nine days. This is because ten days after the shooting, there was another mass shooting in Thousand Oaks, California.

After pooling the data across events within topics, I estimate the following simple model:

$$\mathbb{E}[Y_{d,e}^{pct}] = 2^{-\beta d}$$

$Y_{d,e}^{pct}$ is the total number of mentions of the topic on day d after the peak for event e. It is measured as a percentage of peak attention–i.e., attention on day $d = 0$. I did not include an intercept, as $Y_{d=0,e}^{pct} = 1$ by definition. Using base 2 for the exponential decay conveniently allows us to interpret the inverse of β as the half-life of attention; the number of days it takes for mention counts to halve. I estimate the model using non-linear least squares.

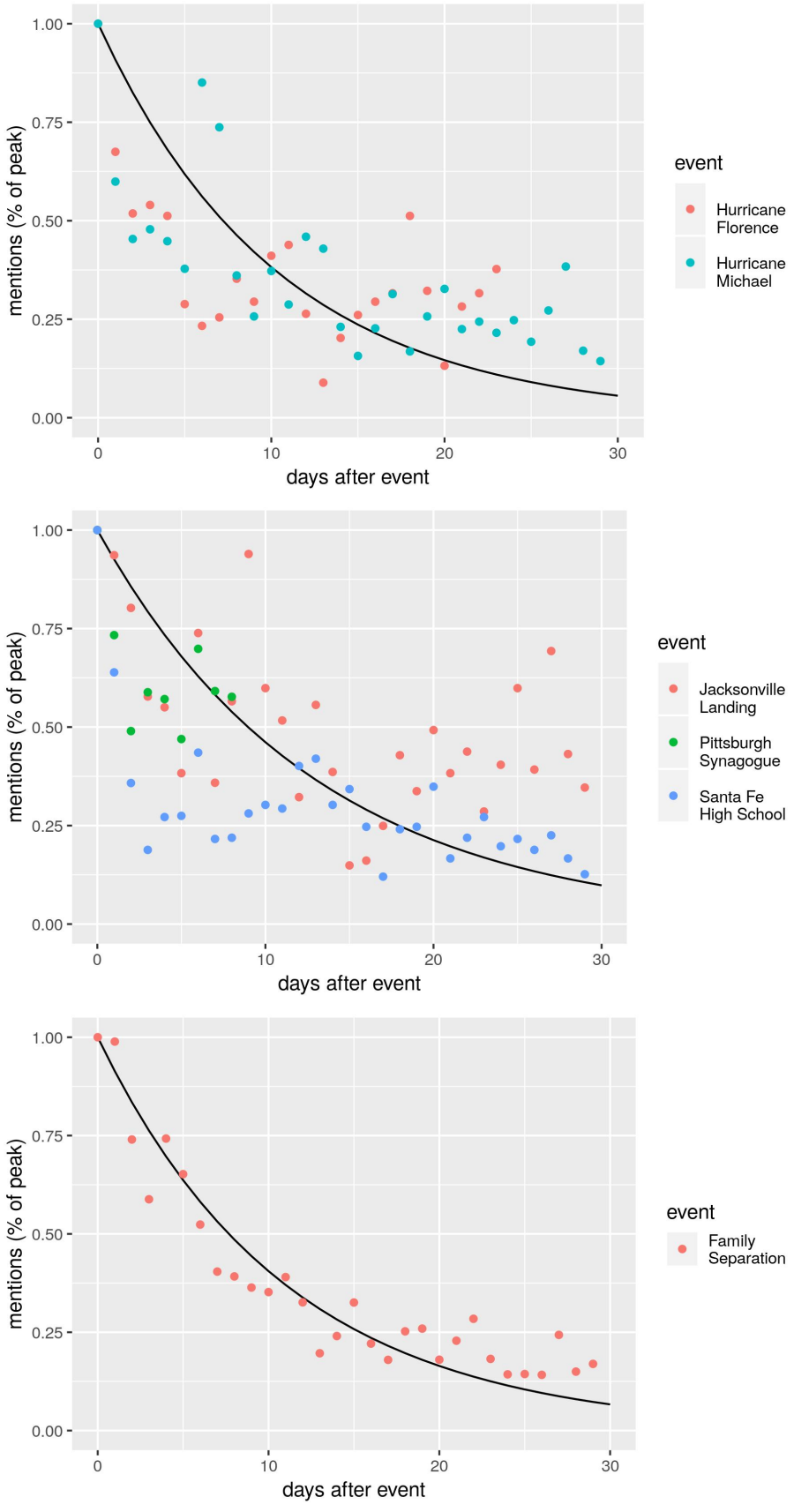

Figure A.4: Decline in attention (number of topic mentions) after the peak for

This model is not perfect–for instance, attention likely returns to some baseline level in the long run, rather than eventually going to zero. However, it fits the time trend in the attention data reasonably well.

Figure A.4 shows the predicted post-peak attention trend for each topic, alongside the data. The estimated beta coefficients are 0.14 for hurricanes and climate change, 0.11 for mass shootings and gun policy, and 0.13 for family separation and immigration. This means that the half-life of attention to these events is 7–9 days.

A.8 Common phrases for issue positions

Since I have thousands of hand-coded mentions for each topic position, it is possible to look at which phrases are most predictive of each topic position. These are the n-grams whose occurrence has the strongest connection with a topic mention having one label or the other (measured through the chi-square statistic, leaving out all mentions that were coded as having neither position). The phrases are stemmed, meaning they are reduced to their root form–for instance, "illegal immigrant" and "illegal immigration" will be counted as the same phrase. I present the phrases that are most predictive in the pre-event weeks, as well as those that are most predictive in the post-event weeks; phrases are split by which label they are positively correlated with.

Table A.4 shows us the most convinced and most skeptical climate phrases. Convinced terms tend to refer to the evidence for, or consequences of, climate change (e.g. extreme weather, sea level rise). Skeptical mentions of the climate tend to use the phrases "global warming" and "man-made" (likely to contest man-made climate change). They also use more political words (Democrat, Trump, complicit), and in the post-event weeks, they are more likely to use the storm-related words "category" and "hurricane".

Table A.5 lists anti-gun and pro-gun phrases. Typical for anti-gun speech is the use of the term "gun violence", and references to high schoolers' fifty-mile march to gun manufacturer Smith & Wesson's headquarters in Springfield, MA (which happened just before the Jacksonville shooting). After a mass shooting, anti-gun speakers are far more likely to talk about specifics of the shooting, including locations. Pro-gun speakers, both before and after a shooting, talk about gun rights and self-defense. They also refer listeners to the websites of firearm suppliers and gun advocacy groups (hence the phrases "go", "dot", "com").

Finally, as Table A.6 shows, tough-on-immigration mentions are strongly characterized by the term "illegal immigration". Other common phrases also refer to problems connected to immigration, such as gangs and chain migration. On the supportive side, terms tend to portray immigrants as people, and in the post-event week, refer to elements of family separation policy.

While it is difficult to summarize the many possible conclusions from this analysis, one finding stands out. In each case, only one side seems to react to events by bringing in specific phrases related to the events. In the case of

Convinced, pre		Convinced, post	
feature	chisq	feature	chisq
impact	22.779	report	60.338
water	16.495	energi	41.262
extrem	16.343	degre	37.942
us	15.047	impact	35.839
rise	11.625	um	34.771
address	10.920	issu	30.768
lead	9.934	state	30.684
level	9.825	california	27.967
sea	9.559	world	27.561
effect	9.468	action	22.794
summer	9.122	term	22.238
plan	8.742	govern	21.030
extrem weather	8.742	new	20.920
twenti	8.186	research	20.726
grow	7.981	governor	20.641

Skeptical, pre		Skeptical, post	
feature	chisq	feature	chisq
global warm	100.227	global warm	169.761
warm	91.292	warm	142.212
global	74.828	global	98.865
proof	32.539	hurrican	94.953
guy	29.652	blame	59.744
die	29.421	trump	56.241
caus	28.976	whole	42.804
go	28.967	complicit	42.004
written	28.942	warm climat	40.812
man	27.028	got	40.475
manmad	24.939	categori	40.212
manmad global	24.786	manmad global	39.050
democrat	24.786	manmad	34.559
mass	24.729	know	33.843
die evolut	23.680	laughter	32.555
mass die	23.680	noth	31.062

Table A.4: N-grams associated with convinced and skeptical mentions of climate change.

climate change, the terms “hurricane” and “blame” surface on the skeptical side after a storm. Listening to these speech fragments, it becomes clear that the key narrative here is that Atlantic hurricanes should not be blamed on climate change (an argument that is supported by climate scientists; Geophysical Fluid Dynamics Laboratory 2019). In the case of gun policy, the anti-gun side is

Anti-gun, pre		Anti-gun, post	
feature	chisq	feature	chisq
violenc	411.329	violenc	487.724
gun violenc	397.632	gun violenc	467.703
student	81.617	school	221.417
smith	68.159	shoot	200.547
march	63.227	student	136.666
springfield	61.983	florida	77.679
mile	59.833	gun control	74.266
fifti mile	54.630	mass	73.911
children	54.406	texa	72.804
shoot	45.973	control	72.066
ralli	45.222	school shoot	68.454
teenag	45.075	news	63.878
archiv	44.224	legisl	62.724
violenc archiv	44.224	high	60.864
school	43.905	high school	60.654

Pro-gun, pre		Pro-gun, post	
feature	chisq	feature	chisq
amend	116.781	amend	265.884
second amend	108.440	second amend	237.058
second	107.960	second	233.030
right	77.661	right	56.059
gun owner	40.602	defend	47.026
owner	40.517	pro	43.414
believ	37.465	amend right	36.107
amend right	34.451	dot com	29.935
defend	29.964	com	29.935
dot com	28.813	gun right	25.791
com	28.813	dot	24.124
constitut	23.940	pro second	23.042
go	23.411	foundat	22.111
conserv	22.680	free	20.689
respons	20.424	amend foundat	19.669
gun right	19.520	go	19.384

Table A.5: N-grams associated with anti-gun and pro-gun mentions of gun policy.

far more likely to refer to the details of a mass shooting. Finally, speakers supportive of immigration are much more likely to bring up specifics about family separation policy, such as children's detention centers. These findings might suggest that hurricanes, shootings and family separation are, in fact, easier to fit into one position than into another.

Tough on immigration, pre		Tough on immigration, post	
feature	chisq	feature	chisq
illeg immigr	58.550	illeg	161.476
illeg	49.103	illeg immigr	137.891
chain migrat	21.960	want	46.638
chain	20.364	alien	35.052
gang	19.890	law	34.165
secur	18.421	immigr law	29.531
attorney general	18.176	whi	25.504
whi	17.752	open border	22.332
general	17.036	presid	22.054
gang violenc	15.336	illeg alien	21.699
wall	13.192	enforc	19.842
session	12.885	bomber	16.691
big	11.814	act	15.857
amnesti	11.585	border	15.419
suspect	11.585	citizen	14.395

Supporting immigration, pre		Supporting immigration, post	
feature	chisq	feature	chisq
worker	26.498	children	66.550
famili	18.941	detent	49.414
women	18.898	texa	44.657
parent	16.997	protest	42.129
separ	16.522	parent	37.459
togeth	15.749	polici separ	33.115
son	13.649	immigr detent	28.618
recent	12.949	immigr children	26.689
protest	11.588	first ladi	24.529
near	10.591	um	24.130
immigr communiti	10.499	separ	23.561
immigr famili	9.857	shelter	23.416
better	9.449	group	22.790
young	8.698	outsid	22.094
colleg	8.616	visit	21.880
communiti	8.600	center	21.273

Table A.6: N-grams associated with tough and supportive mentions of immigration.

Appendix B

In-group interest cues: supplementary material

B.1 Design details and justifications

B.1.1 Procedure

Figure B.1 shows how participants moved through the different steps in Experiment 1 (on gender) and 3 (on LGBT issues), including which questions were asked before and after treatment. The design for Experiment 2 (on race) is analogous to Experiment 1, with different groups and issues.

Experiment 1 and 2 occur in two phases: a pre-treatment and (one week later) a treatment phase. There are a few advantages to this set-up. First, the measurement of group identities is separated from the treatment, which avoids making those identities salient for all participants (including the control group). Second, prior beliefs about the effect of the issue on one's group can be measured before the treatment, without the treatment coming across as a correction. Finally, being able to control for pre-treatment measures of the dependent variables (in this case by first-differencing the dependent variables) makes the treatment effect estimates more precise. In Experiment 3 and 4, because the pool of potential LGBT participants was small, these advantages did not weigh up against the possibility of a high drop-out rate and an undersized final sample.

In all experiments, participants can be assigned to learn about one of two group-related issues (or to be in the control group). I include two issues per group to diminish (though of course not eliminate) the possibility that any inter-group differences in effect sizes can be ascribed to differences in the issues they were linked to. Covering a total of ten issues in all experiments combined also helps me find any patterns (or a lack of them) in the kinds of issues that are most sensitive to treatment. Section B.2.5 below investigates a few of these patterns.

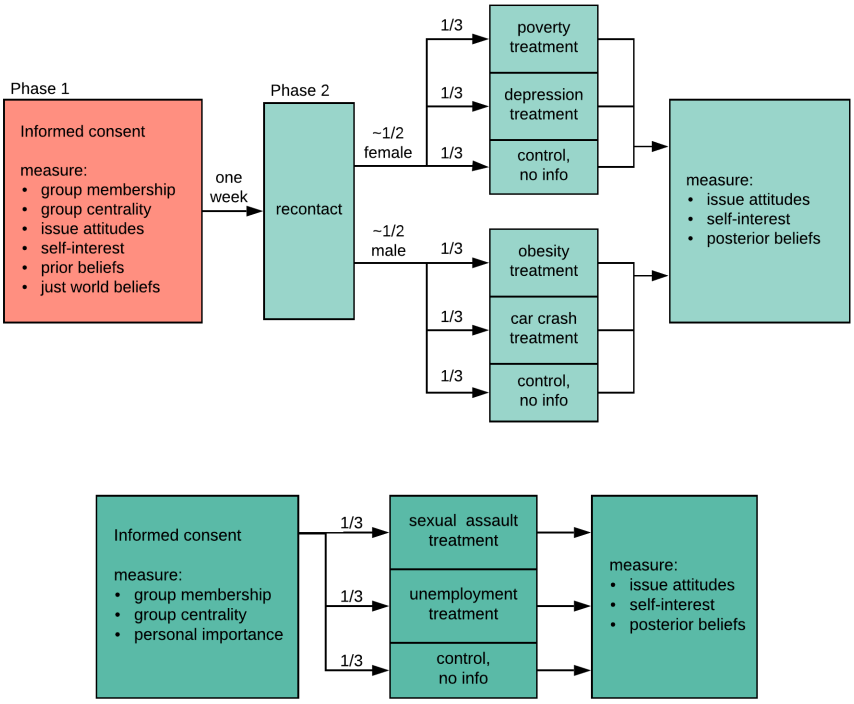

Figure B.1: Designs for Experiment 1 (gender) and Experiment 3 (LGBT). Participants were treated (or not) with information about gendered issues or LGBT issues. Then, they answered post-treatment measures. In Experiment 1, there was a pre-treatment phase where filled out pre-treatment measures.

As the design flowcharts show, the experiment include two measures that are not featured in the paper's main analyses: prior/posterior beliefs, and just world beliefs. These measures are used in sections B.2.3 and B.2.5 of this Appendix.

B.1.2 Samples and recruitment

Below, I describe the sample characteristics and recruitment strategies for each experiment. All of the respondents are adults from the United States. They were recruited on Amazon's Mechanical Turk platform. Mechanical Turk workers are younger, lower-income, and more likely to be unemployed than the average American (Levay et al., 2016). However, there is more and more evidence that this recruitment strategy does not affect experimental results (Berinsky et al., 2012; Coppock, 2018; Mullinix et al., 2015). We might worry that workers on crowd-sourcing platform pay less attention to textual treatments. However, the instrumental variables analyses in section B.2.3 show that respondents' beliefs

are indeed systematically shifted by the treatment.

Experiment 1: Gender

The sample for this study consists of 484 respondents recruited in September 2018. 262 are male, and 222 are female. Some participants dropped out between phases—the retention rate from the original sample was 65%.

Experiment 2: Race/ethnicity

The sample for this study consists of 451 White and 267 Black or Latino participants, recruited in September–October 2018. Most respondents were newly recruited, as in Experiment 1. In addition, I engaged Black and Latino participants from a pool of workers who had completed another survey task on Mechanical Turk at least one month, and up to 18 months, prior to being recruited for this survey. Respondents who identified as neither White, Black nor Latino were filtered out of the sample.

I grouped into the Latino category all participants who identified as Hispanic/Latino, including those who also identified as White or Black. Black and Latino participants were pooled into one group and received the same treatments, though the identity centrality measure applied to their group only (e.g., "I often think about the fact that I am Black").

Experiment 3: LGBT

The 198 participants for this study (126 female, 66 male, 6 other or unknown gender) were recruited in November 2018. These workers had completed another survey task one month before being recruited for this survey. All participants had previously indicated that they identified as LGBT and were willing to be recontacted for another survey. In addition, I filtered participants by including an LGBT identification question at the beginning of this survey. I found no detectable difference between LGBT women and LGBT men in the strength of their LGBT identity.

B.1.3 Dependent variables: concern, importance, spending support

I took pre- and post-treatment measurements of three outcome variables: concern about the issue (i.e. whether the issue is seen as a problem), importance of the issue (when ranked with other issues), and support for government spending to help tackle the issue. These measures are somewhat dependent on each other (Wlezien, 2005), but they should be progressively harder to change.

Self-reported concern is the most "costless" attitude in this study—people could change their response only to show their allegiance with the in-group (cf. Bullock et al. 2015; Prior et al. 2015). Importance is, to a point, conditional on concern: for an issue to be an important problem, it first has to be seen as

a problem. In addition, upgrading the importance ranking of one issue comes at the price of downgrading another. Finally, even if a respondents thinks of an issue as important, they may still not feel that government action is the best way of addressing it. Moreover, a blanket spending increase might seem costly as a way to address the problem for in-group members, specifically. Indeed, in a review on self-interest in politics, (Kinder, 1998, p. 802) finds that interests change perceived issue importance more often than they change positions on policy. Changes in support for government spending should be the most demanding test.

In the importance ranking question, the treatment effect size may depend somewhat on the alternative issues among which subjects needed to rank the issue of interest. Section B.3.5 shows, for each experiment, which other issues were present in the ranking. I discuss the consequences of these choices, along with other concerns about the sensitivity of the dependent variable measures, in section B.2 below.

Figures B.2–B.4 show the distribution of the dependent variables for each issue.

B.1.4 Group identification

Centrality

To measure group identification, I used the Centrality subscale developed by Leach et al. (2008). It has three items, which I average into an identity centrality score:

- I often think about the fact that I am a [man/woman/...]
- The fact that I am a [man/woman/...] is an important part of my identity
- Being a [man/woman/...] is an important part of how I see myself.

The Centrality scale measures the overlap between a person's self-concept and their concept of the group. This matches Conover's (1984) definition of group identification as the group schema becoming a self-schema. Greenwald et al. (2002) also speak about group identity as an association between the self and a social category. Similarly, Deaux (1996) writes that a person identifies with a group if he or she accepts that membership of the group defines him or her in some way.

Figure B.5 shows the distribution of identity centrality scores by group.

Of course, there are other in-group attitudes that could moderate the connection between group membership and political opinions. One of them is group consciousness—a set of political beliefs including the idea that collective action is needed to improve the group's social standing (McClain et al., 2009). Group consciousness is more demanding than group identity: identifying with a group would seem to be a necessary, but not a sufficient condition for group

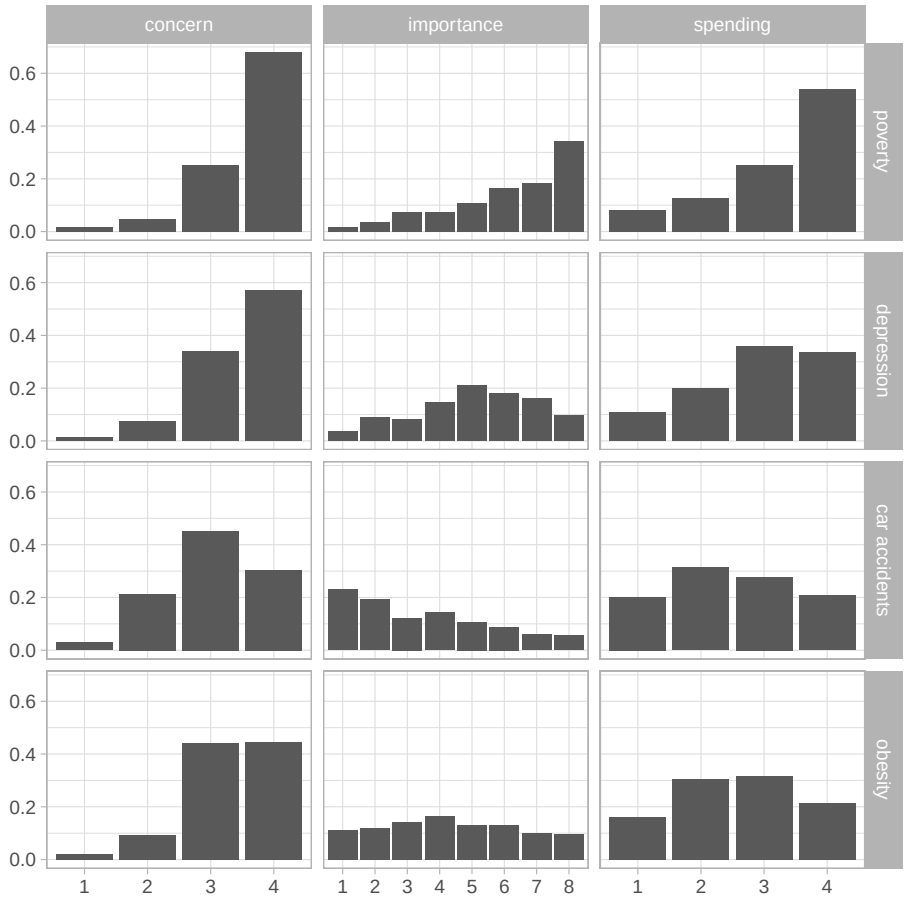

Figure B.2: Distribution across respondents of concern, importance and spending scores by issue in Experiment 1 (gender), pre-treatment.

consciousness (cf. McClain et al. 2009; Miller et al. 1981). In fact, group-conscious members should act on in-group interests almost by definition. Complicating the situation, however, is the fact that three key components of group consciousness—closeness to the group, perceived discrimination and collective action—are empirically quite distinct (Sanchez and Vargas, 2016). Future research might investigate whether only group members with group consciousness are likely to translate in-group interests into political opinions.

B.1.5 Model specifications

In Experiment 1 and 2, the basic model for each of the issue attitude types is:

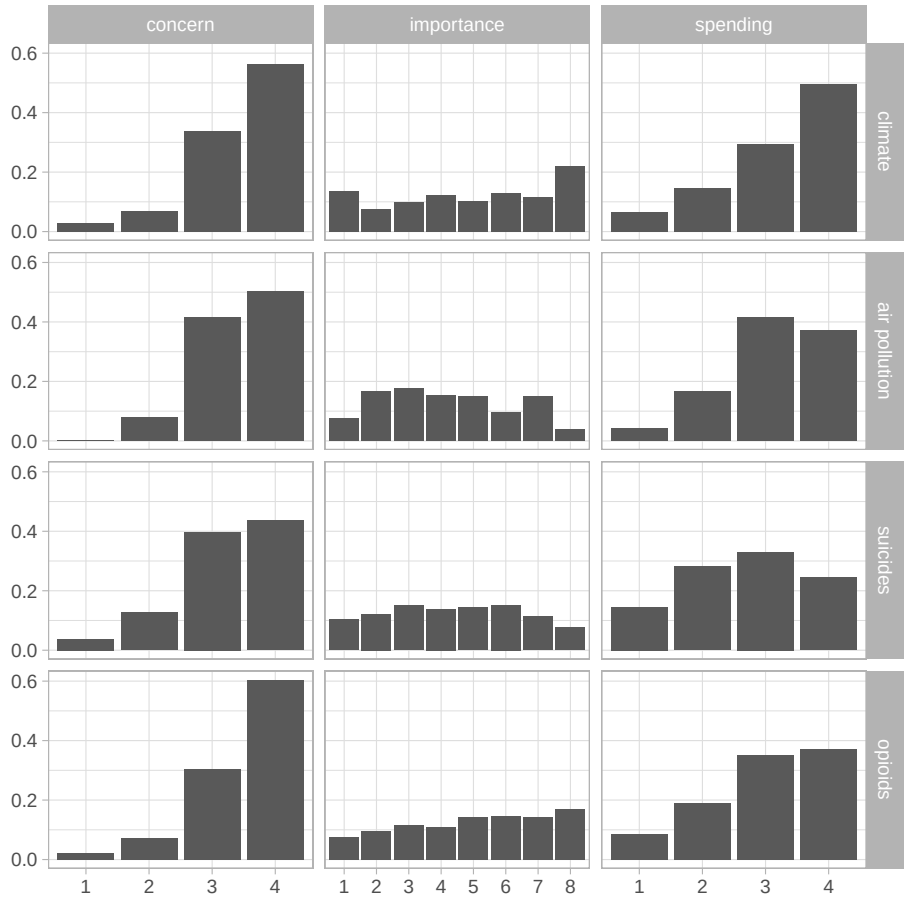

Figure B.3: Distribution across respondents of concern, importance and spending scores by issue in Experiment 2 (race), pre-treatment.

$$Y_{ij,t=2} - Y_{ij,t=1} = \alpha_j + T_{ij} + \epsilon_{ij}$$

where Y_{ijt} is respondent i's attitude on issue j at time t, T_{ij} indicates whether the respondent got treated on this issue, and α_j is an issue-specific intercept. The error terms ϵ_{ij} are clustered at the level of the respondent, i.

In Experiment 3, since pre-treatment dependent variables were not available, they could not be used as controls in the analyses. To make up for this, before treatment, I measured the personal importance of the issue for the respondent on a four-point scale, as defined by Krosnick (1990). The baseline estimating equation for each of the issue attitude types becomes:

$$Y_{ij} = \alpha_j + T_{ij} + P_{ij} + \epsilon_{ij}$$

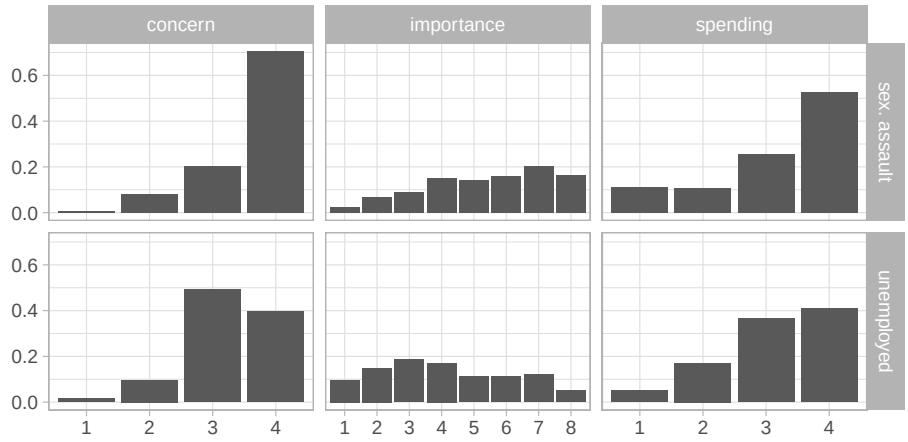

Figure B.4: Distribution across respondents of concern, importance and spending scores by issue in Experiment 3, in control group.

where Y_{ij} is respondent i's attitude on issue j, and P_{ij} is the personal importance of issue j to respondent i.

For treated participants, I leave non-treated issues out of the analyses, in case the treatment spills over into other issue attitudes. For control participants, I include their responses on both of the issues related to their group.* The unit of analysis is the person-issue. All analyses are OLS regressions with issue fixed effects. Standard errors are clustered at the person level in all cases.

*This means that there are equally many control and treatment person-issues in the final sample: there are twice as many treated as control respondents, but control respondents contribute two issues each.

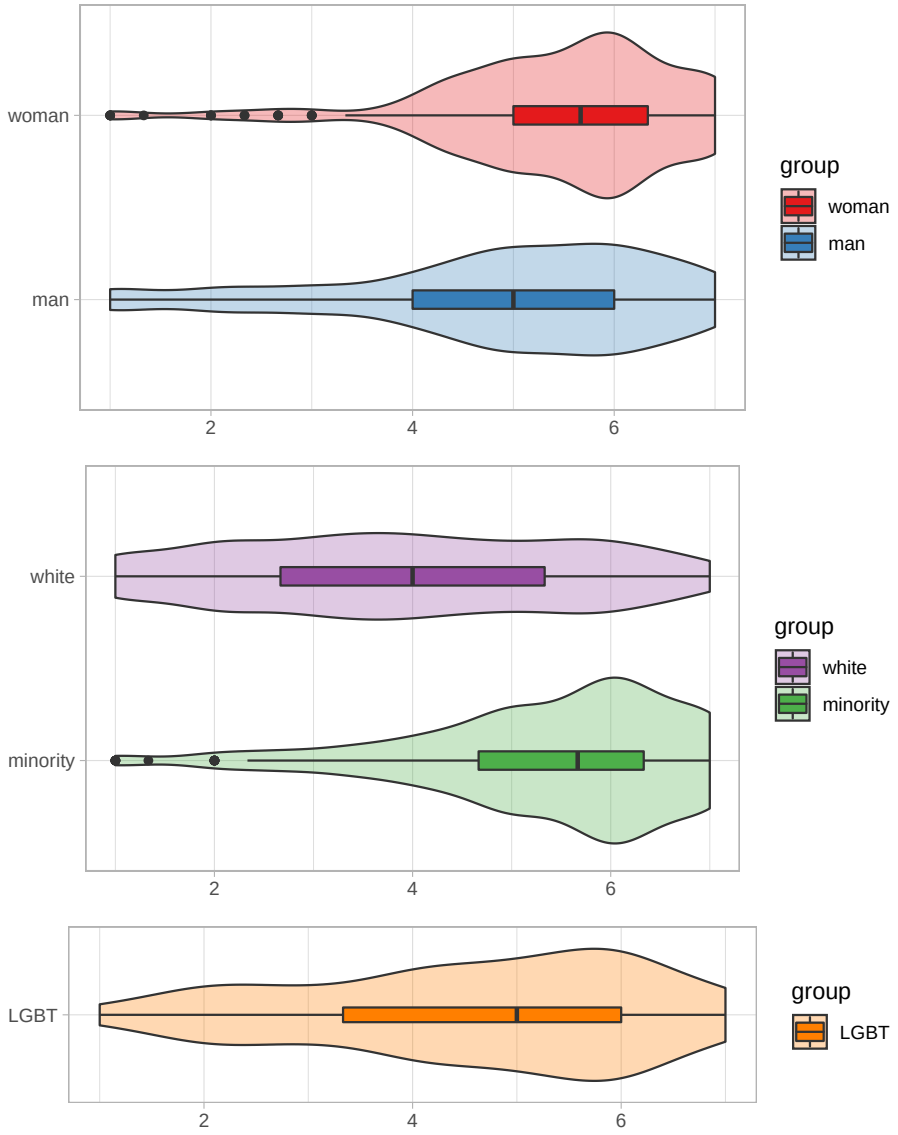

Figure B.5: Violin plots (smoothed distributions plus boxplots) of scores on identity centrality scale (average of three 7-point items), by group.

B.2 Robustness checks

Throughout Experiment 1–3, I find that group interest cues have little to no effect on issue attitudes. In this section, I solidify this finding by showing that it holds even when I pool data across Experiments; when I examine the group–issue combinations one by one; and when I control for the fact that the treatment may not have affected every respondent's beliefs about interests (e.g., because the information was not new). Finally, I examine the possibility of ceiling and anchoring effects, and discuss more substantive explanations for the null results.

B.2.1 Pooled analysis

Since all the experiments have similar designs, I can pool their data to get a more general estimate of the effect of in-group interest cues.[†] The LGBT experiment has no pre-treatment measurements, so I combine the data in two ways: using the gender and race data only ($n = 1179$); and using all data, but ignoring pre-treatment measurements for the gender and race studies ($n = 1376$). Figure B.6 reports treatment effect estimates for both pools, on each of the three issue attitudes, using a model with an interaction between the treatment and identity centrality. Estimates are almost identical without the interaction. The only statistically significant effect is that of issue importance, with respondents moving treated issues up by about .25 of a place on average in their rankings. The estimated effect on concern is .06 or less; the effect on spending support is .07 or less. Both pools allow us to reject effect sizes greater than .11 (concern), .40 (importance) and .15 (spending).

There is almost no evidence for an interaction between the treatment and identity centrality. To further investigate this, I split up identity centrality into within-group centrality (how strongly the person identifies with their group, compared to other members of that group) and between-group centrality (the group's average identification level).[‡] Pooling data from all experiments, within-group centrality has very small negative interaction effects with the treatment (concern: $-.01$, $SE = .03$; importance: $-.01$, $SE = .09$; spending: $-.05$, $SE = .04$). Between-group centrality has very small positive interaction effects (concern: .01, $SE = .05$; importance: .12, $SE = .13$; spending: .05, $SE = .06$). None are significant.

B.2.2 Issue-by-issue analysis

We might be interested to know whether any issue/group combinations were more effective than others at moving attitudes. Figure B.7 shows the issue-by-issue effect sizes from Experiment 1–3, botained by adding issue-treatment interactions to the models specified in section B.1.5. There are no obvious patterns across all three dependent variables. Three issue/group/dependent

[†]Note that here, the issue fixed effects absorb any differences between experiments in the average levels of the dependent variables without treatment.

[‡]This analysis was not pre-registered.

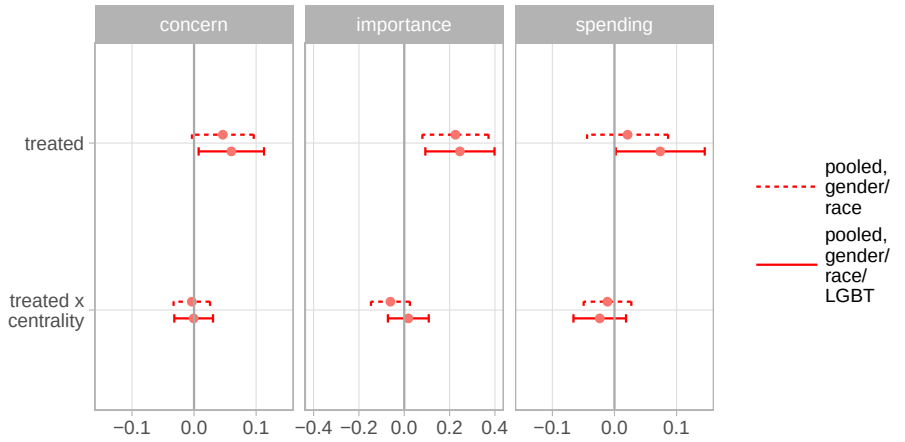

Figure B.6: Effect of connecting an issue to in-group interests, and its interaction with group identity centrality, on three attitudes, with 90 % confidence intervals. Data pooled either across gender and racial groups ($n = 1179$); or across gender, racial and LGBT groups ($n = 1376$).

variable combinations narrowly cross the threshold of marginal significance (car accidents/men/concern, air pollution/minorities/importance, and unemployment/LGBT/concern, $p < .10$). However, with 30 (10 issues x 3 attitudes) comparisons, this is precisely what we would expect if all of the true effects are zero.

B.2.3 Change in beliefs

Treatment as a belief change instrument

We may be concerned that the treatment did not actually change respondents' belief in the connection between their group and the issue at hand. Some respondents could have been aware of the connection beforehand; their beliefs would not have been moved by the treatment, because they were already aligned with the treatment to begin with. Other respondents perhaps did not believe the information provided in the treatment. While I cited a source for each treatment, perhaps not all respondents trusted these sources (or my interpretation of them). Finally, some participants may not have paid attention to the treatment. Here, I conduct instrumental variables analyses to show that the treatment is ineffective even when it actually changes people's beliefs.[§]

All experiments included a two-part question on respondents' beliefs about

[§]The pre-analysis plans for Experiment 1 and 2 specified that I would only conduct this instrumental variables analysis if I found a statistically significant main effect of the treatment. However, I now believe that the instrumental variables analysis is also, and even especially, useful for making sure I am interpreting the null findings correctly.

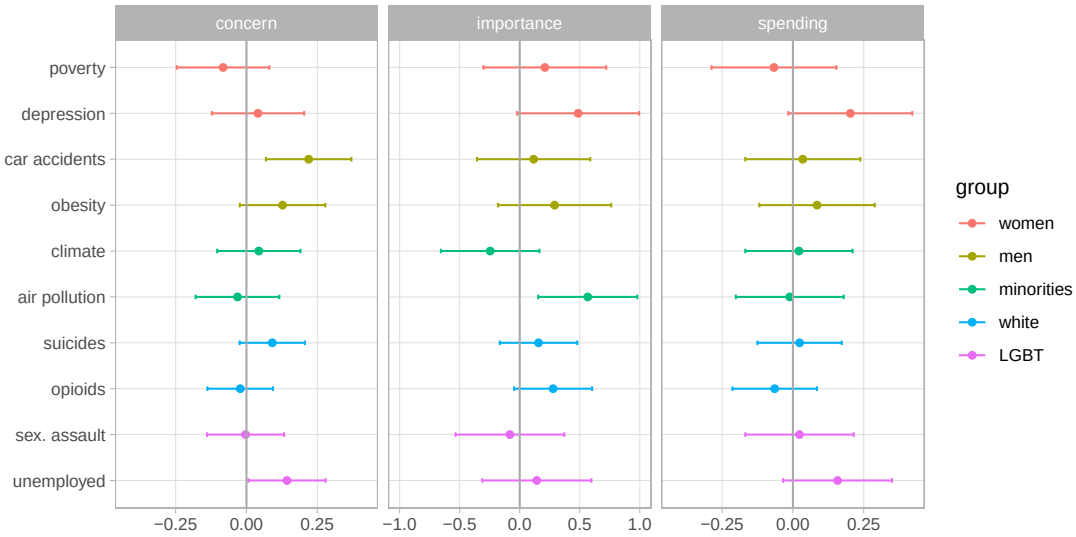

Figure B.7: Effect of connecting an issue to in-group interests, by issue–group combination, on three attitudes, with 90 % confidence intervals.

group disparities in each issue. First, respondents indicated whether they believed the issue happens more to their in-group, more to their our-group, or whether it is about the same. Next, they specified whether they had a lot of confidence, a moderate amount, or only a little confidence in their answer. I used these questions to create a seven-point scale. Respondents who answered "don't know" or "about the same" are at the midpoint. The other respondents are on either side of the midpoint, with the most confident respondents sitting at the ends. The top half of the scale represents a correct answer (e.g., poverty happens more to women), and the bottom half represents an incorrect one. In Experiment 1 and 2, I used the difference between pre- and post-treatment measurements of belief. In Experiment 3, I only took a single, post-treatment measure of belief.

The treatment is a strong instrument for belief (change) in all cases (gender: $F(1, 641) = 188.7, p < .001$, race: $F(1, 938) = 148.1, p < .001$, LGBT: $F(1, 393) = 21.9, p < .001$). Figures B.8–B.10 illustrate the effect of treatment on beliefs about each issue. It shows that in Experiment 1 and 2, untreated respondents rarely move their beliefs between phases, whereas treated respondents move towards the correct, high-confidence end of the scale. In Experiment 3, treated respondents are far more likely to give high-confidence correct answers than untreated respondents.

Figure B.11 shows the results of two-stage least squares regressions for each experiment. The only effects that reach marginal significance are on issue importance, when in-groups are based on gender or race/ethnicity. A respondent who went up one point on the in-group interest belief scale, would bump an

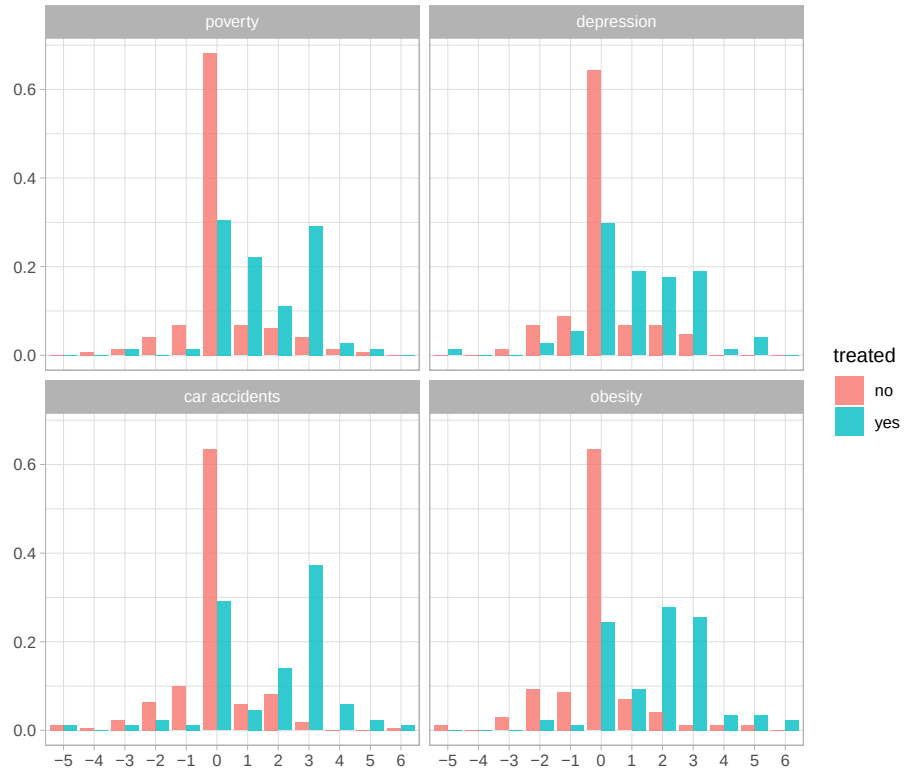

Figure B.8: Distribution across respondents of change in beliefs about gender-issue connections between phases in Experiment 1 (gender), for treated and untreated respondents. Belief is a 7-point scale.

issue by less than .2 places on average in his or her importance ranking. In other words, the treatment generally does not change attitudes even for those respondents whose beliefs were moved. This rules out the explanation that the treatment effect is being suppressed by, for example, respondents who already knew their group is more affected by the issue.

The instrumental variables approach leads to consistent estimates only if the so-called exclusion restriction holds. That is, the treatment can only affect issue attitudes through people's beliefs about their in-group's interests. It is possible that the information in the treatment has non-informational effects—for example, being treated might increase the salience of the social group and its interests, even for respondents who already knew about the issue-group connection. However, this and other plausible violations of the exclusion restriction would cause an *upward* bias in the belief change effect estimates. This means that if anything, these estimates should be read as upper bound estimates.

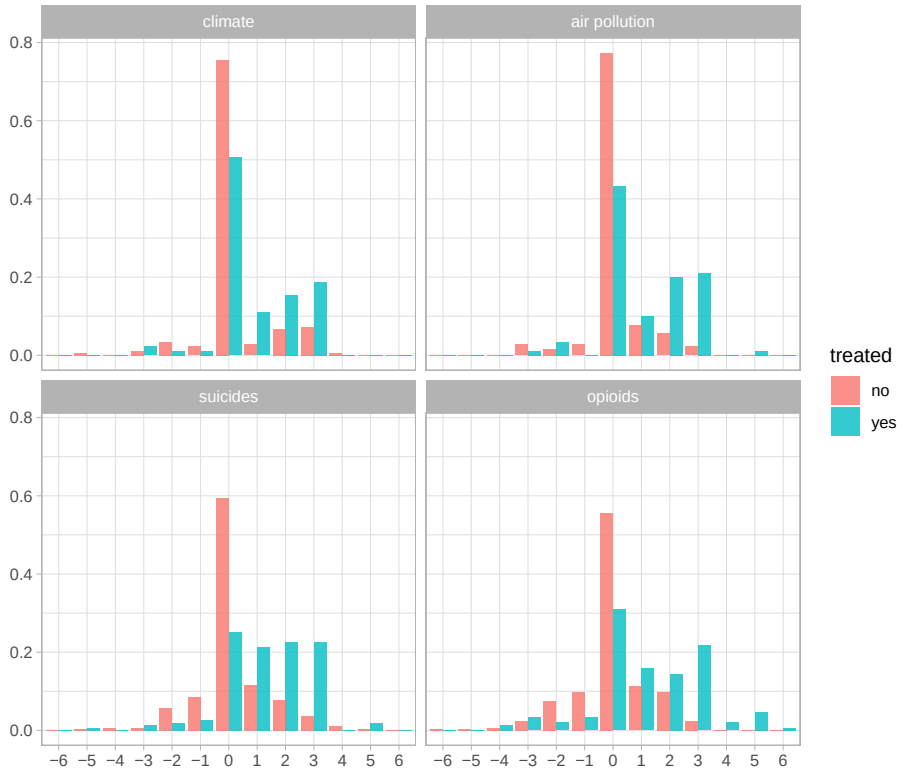

Figure B.9: Distribution across respondents of change in beliefs about race-issue connections between phases in Experiment 2, for treated and untreated respondents.

Moderation by prior beliefs

One of the problems that the instrumental variables analysis above addresses, is that an informational treatment should not have any effect on respondents who already know the information. This section presents results for a different (not pre-registered) approach, where prior beliefs are used as a moderator.

Here, I re-analyze the data from Experiments 1 and 2, using respondents' pre-treatment beliefs as a moderator for the treatment effect. Pre-treatment beliefs are measured on a seven-point scale, from high confidence in the wrong answer (meaning there is a lot of room for the treatment to change beliefs) to high confidence in the right answer (meaning the treatment will likely not change beliefs).

Tables B.1 and B.2 show that pre-treatment belief is never a substantively or statistically significant moderator of the treatment effect. Including this moderator does not change our conclusions about the (usually negligible) effect of the treatment. Like the instrumental variables analyses, this suggests that small

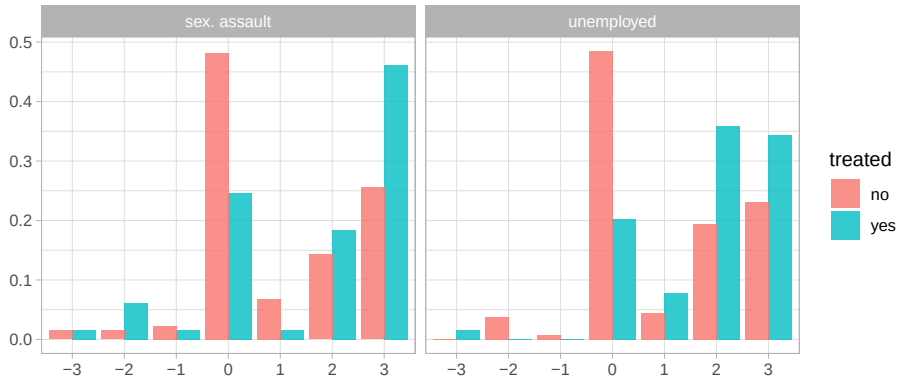

Figure B.10: Distribution across respondents of belief about LGBT-issue connections in Experiment 3, for treated and untreated respondents.

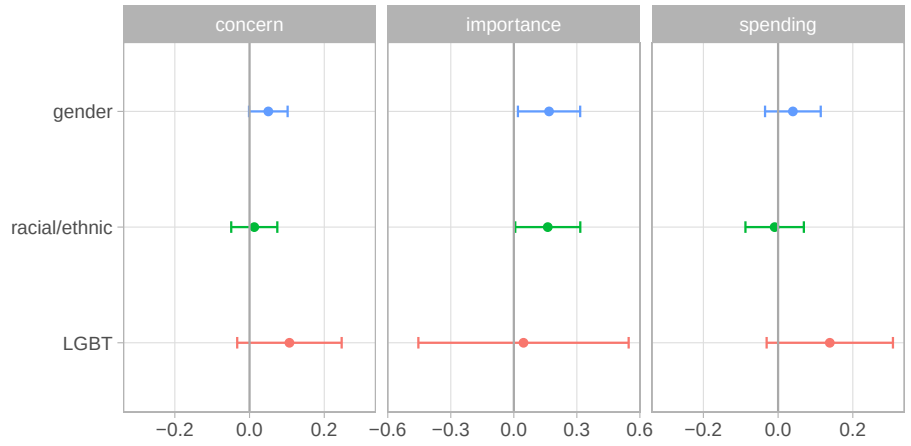

Figure B.11: Effect of changing beliefs about in-group interests, as instrumented by an information treatment, on three issue attitudes, with 90% confidence intervals. Belief is on a 7-point scale. Concern and spending are 4-point scales, importance is an 8-issue ranking.

treatment effects are not due to respondents already having the information beforehand.

Group centrality and beliefs

Finally, beliefs could help explain the null or negative effects of group centrality. For example, people for whom the group is central might know more about the group's interests ahead of time, and therefore be less affected by the treatment. Alternatively, strong identifiers could be reluctant to connect their own group

Table B.1: Effect of learning about gender in-group interests on three issue attitudes, interacted with prior beliefs.

	Dependent variable:		
	concern	importance	spending
treated	0.08*	0.26*	0.06
	(0.05)	(0.15)	(0.07)
belief	0.02	0.09	−0.002
	(0.02)	(0.07)	(0.03)
treated:belief	0.01	0.06	0.02
	(0.03)	(0.11)	(0.05)
Observations	641	638	637
R^2	0.02	0.02	0.01
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table B.2: Effect of learning about racial/ethnic in-group interests on three issue attitudes, interacted with prior beliefs.

	Dependent variable:		
	concern	importance	spending
treated	0.03	0.22*	−0.03
	(0.04)	(0.12)	(0.06)
belief	0.01	−0.07	−0.02
	(0.02)	(0.06)	(0.03)
treated:belief	−0.01	−0.07	0.03
	(0.03)	(0.09)	(0.04)
Observations	962	948	950
R^2	0.01	0.03	0.01
Note:		*p<0.1; **p<0.05; ***p<0.01	

to a societal problem—as most of the issues in this study have a negative connotation. The (non-pre-registered) analyses below investigate these possibilities.

For Experiment 1 and 2, I start by regressing the pre-post difference in beliefs on centrality, treatment status, and their interaction (plus issue fixed effects). In the gender case, I find a very small and non-significant interaction ($-.06$, SE: .08). For racial groups, the interaction is small but statistically significant, meaning that strong identifiers are slightly less moved in their beliefs by the treatment (-0.11, SE: 0.05). In Experiment 3, running the same regression using just post-treatment beliefs, I once again find a very small, non-significant interaction (0.05, SE: 0.1).

Further analyses show that it is unlikely that prior knowledge puts a ceiling on belief change for strong identifiers. Before treatment, on a belief scale from -3 (high confidence in the wrong answer) to 3 (high confidence in the right answer), even a person with the highest possible identity centrality is only predicted to score 0.5 (for gender groups), 0.4 (race) and 1.1 (LGBT). In fact, in the only case where centrality actually seems to attenuate the treatment (Experiment 2, race), the correlation between centrality and prior belief in the issue–group connection is actually negative ($-.1$). This gives some tentative support to the idea that in the case of racial groups, strong identifiers prefer not to associate their social groups with societal problems.

B.2.4 Ceiling effects and anchoring

Figures B.2–B.4 above illustrate the distribution of concern, importance and spending scores of the treated issues among untreated respondents. It shows that we might be concerned about ceiling effects, where respondents would have picked the top option even without treatment—especially for the concern variable, but also for importance and spending on issues such as poverty, climate change and sexual assault.

For that reason, I repeat the analyses pooling Experiment 1 and 2. For each dependent variable, I exclude those observations (in both treatment and control group) where a respondent already chose the top option for that issue on that variable in the pre-treatment phase. Estimated effects increase slightly (concern: 0.10, SE: 0.05; importance: .25, SE: 0.10; spending: .03, SE: 0.05). However, they remain substantively small.

A more subtle variant of a ceiling effect could affect the issue importance ranking. Even if a respondent places the issue of interest in the middle of the scale, the issues ranked above it could be so important that it could never displace them. While I cannot identify respondents for whom this might be the case, it is reassuring that in Experiment 1 and 2, control group respondents commonly switch the issues of interest around in their rankings between phases (the issue with the smallest average movement in importance-rank is poverty with 1.0). Moreover, in all experiments, untreated respondents are quite spread out in their importance rankings of the treated issues (see Figures B.2–B.4; the issue with the lowest standard deviation for importance rank is again poverty with 1.8). At least some respondents found it reasonable to rank each issue in

each of the eight available places.

A final concern to do with dependent variable changeability, is that the pre-treatment measures may have anchored people's attitudes. The experimental designs partly address this worry. There is at least one week between pre-treatment and post-treatment measures; anchoring is less likely for the eight-place issue importance ranking (in fact, the numbers above show that untreated issues are commonly moved around); and the effects in the LGBT experiment are still null, even though there were no pre-treatment measurements.

B.2.5 Alternative explanations

Finally, there are a number of alternative interpretations for the non-effects of interest cues in this set-up. Some of these are easily ruled out; for others, the counter-evidence is more tentative. The analyses in this section are exploratory and not pre-registered.

First, in-group bias is likely to be less socially acceptable when the in-group is not traditionally seen as being discriminated against. Members of those groups may be less likely to take an opportunity to openly favor their in-group (cf. White 2007). To test this, I pool respondents from groups seen as less (men, White people) or more (women, minorities, LGBT) disadvantaged. Treatment effects do not differ significantly from each other, and the effect sizes for disadvantaged groups are almost identical to the effects in the overall pool (concern: .06, $SE = .04$; importance: .27, $SE = .13$; spending: .12, $SE = .06$).

A second possible objection is that the treatment might only make an impression on respondents if the group's relative risk is high enough. For example, Black and Latino people only have a 15% higher risk of dying from heat-related diseases, but White people are three times more at risk of committing suicide. For that reason, I do an analysis where I interact the treatment with the group's relative risk for the issue being asked about (e.g. 1.15 or 3). Interaction effects are small, and in fact, negative (concern: $-.01$, $SE = .04$; importance: $-.05$, $SE = .71$; spending: $-.11$, $SE = .06$).[¶] This suggests that, if anything, communicating larger relative risks has a smaller effect.

Similarly, it is possible that information about the relative risk of an in-group member being affected by an issue is not sufficient. Instead, issue attitudes may only move if the in-group is especially affected, *and* the risk is seen as significant. For example, white respondents may have disregarded the information about opioid overdose deaths, on the basis that opioid overdoses only represent a tiny fraction (about 2%) of deaths in the United States. This explanation is somewhat weakened by the fact that the issue-by-issue analyses do not show particularly large effects for issues that most people know to be large-scale, such as obesity (which affects around 40% of Americans) and depression (around 7.1%). The same is true for issues that were ranked as more important in society, such as poverty (median rank: 2 out of 8 pre-treatment) and sexual

[¶]From a model without an interaction with identity centrality; adding this interaction does not change results noticeably.

assault (median rank: 3 out of 8 in the control group). Still, future iterations might use with a treatment that communicates both relative and absolute risk to the group, for instance, by showing the number of group members that are affected by the problem yearly.

Another less convincing story is Lerner's (1980) concept of belief in a just world. Perhaps some (in particular conservative) respondents believe that people are, and should be, rewarded based on their individual efforts rather than their group memberships (cf. Carney and Enos 2017). To investigate this, I included a just world belief scale (four items on seven-point scales) at the end of the gender and race experiments. I found no significant negative interaction between just world belief (averaged over items) and the treatment in either study, or when pooling the data from both studies. Interaction effects were tiny and positive (pooled estimate for concern: .00, $SE = .02$; importance: .09, $SE = .06$; spending: .01, $SE = .02$).

A related explanation would be that respondents do not identify with fellow group members who are affected by the issues, for one of two reasons. First, Marques et al. (1988) suggest that in-group members who deviate from a norm are judged more harshly than out-group members who do the same (the so-called "black sheep effect"). If respondents believe that being affected by a problem is a result of a personal choice or failure of the in-group member (for example, that being in a car accident is due to being a careless driver), then they may not be inclined to help. This idea is related to respectability politics, where members of a minority believe that their in-group will benefit if its members behave according to dominant out-group norms (Higginbotham, 1993). However, the issue-by-issue analysis above contains hints against this explanation. Issues that seem less influenced by personal choices, like climate change, air pollution or sexual assault, are no more affected than issues that can more easily be connected to individual actions, like car accidents or suicide.

A second reason is that respondents may think of the affected members as part of a different subgroup. For example, older men may assume that car accidents happen more to *young* men, a group that they are not part of. While difficult to falsify (there will almost always be a subgroup that is more affected by a problem), this explanation would still have important consequences for group-based thinking in politics. Many group theories refer to large social categories such as gender, race or even American citizens. If only subgroup interests are relevant, that would be an important constraint on the power of group-based political opinions. It would also support those researchers who have been arguing for the relevance of cross-cutting identities and disadvantaged subgroups (e.g. Cohen 1999; Harris 2014; Strolovitch 2006).

Finally, it is possible that the dependent variables did not pick up any effect, because they are do not make any explicit reference to the social group at hand. For example, women are not asked for their concern about poverty *among women*, or about their approval of government spending to combat poverty *among women*. It is possible that the group interest cues only affected attitudes about the issue as it applies to the in-group, and not about the issue as a whole. This explanation, too, is quite difficult to test, because referring to groups as

part of the dependent variable measurement would make those groups salient to the control group as well. On the other hand, similar to the subgroup story above, accepting this explanation would place strong limits on the relevance of group thinking in politics. Essentially, it implies that in-group members favor policies that benefit their in-group, but not if those policies also benefit some out-group members. This makes the concept of social groups less useful for explaining political attitudes in general.

B.3 Questionnaire

This section details all of the questions that were asked of participants in all three experiments, as well as the wording of the information treatment. Section B.1 below explains the order in which these questions were asked (as some question were asked both pre- and post-treatment in Experiment 1 and 2), and contains justifications for the key design and measurement choices.

B.3.1 Informed consent

I agree to participate in a research study conducted by Clara Vandeweerdt at the Massachusetts Institute of Technology (MIT). In order to analyze responses to the questionnaire, my answers will be recorded. Researchers will have no access to any personal information about me, except for my MTurker ID, the time at which I took the survey and the answers I filled out. No identifying information about me will be made public and any views I express will be kept completely confidential.

Findings from this study will be reported in scholarly journals, at academic seminars, and at research association meetings. The data will be stored at a secured location and retained indefinitely. My participation is voluntary. I am free to withdraw from the study at any time.

By participating in this survey, I confirm that I am 18 or older. I also give the researchers permission to invite me for a (paid) follow-up study.

[in Experiment 3, LGBT:] Please note that this survey touches on the topic of sexual assault, and that you are free to skip questions on that topic if you need to.

Please select one of the following options. If you choose not to participate, the survey will end immediately and no data will be recorded. Should you have questions, please send an e-mail to claravdw@mit.edu .

- I agree to participate
- I do not agree to participate

B.3.2 Group membership

First, we would like to ask a little more about you.

Experiment 1, 3 and 4: What is your gender?

- Male
- Female
- Another gender
- Prefer not to say

Experiment 2 and 4: Which of these group(s) would you say you belong to?

- White/Caucasian
- Black/African American
- Hispanic/Latino
- Asian
- Native American
- Pacific Islander
- Other

Experiment 3 and 4: Do you identify as LGBT (Lesbian, Gay, Bisexual or Transgender)?

- Yes
- No

B.3.3 Partisanship

[Experiment 4] Generally speaking, do you consider yourself a...

- Democrat
- Republican
- Independent
- Other Party

[If Democrat or Republican] Would you call yourself a strong [Democrat/Republican] or a not very strong [Democrat/Republican]?

- Strong
- Not very strong

[If Independent or Other Party] Do you think of yourself as closer to the Republican Party or to the Democratic Party?

- Closer to the Republican Party
- Closer to the Democratic Party
- Neither

B.3.4 Group identity

Centrality

[Experiment 1–3] How much would you say you agree or disagree with the following statements?

- The fact that I am [a man / a woman / White/ Black / Latino / LGBT] is an important part of my identity.
- I often think about the fact that I am [a man / a woman / White / Black / Latino / LGBT].
- Being [a man / a woman / White / Black / Latino / LGBT] is an important part of how I see myself.

answer options: Strongly disagree – Disagree – Somewhat disagree – Neither agree nor disagree – Somewhat agree – Agree – Strongly agree

Interconnection

[Experiment 4] How much would you say you agree or disagree with the following statements?

- When I talk about [men / women / White people / Black people / Latinos / LGBT people], I often say "we" rather than "they".
- When someone criticizes [men / women / ...], it feels like a personal insult.

answer options: Strongly disagree – Disagree – Somewhat disagree – Neither agree nor disagree – Somewhat agree – Agree – Strongly agree

Linked fate

[Experiment 4] Do you think that what happens to [men / women / ...] in this country will have something to do with what happens in your life?

- yes
- no

[If yes, follow up with:] Will it affect you: a lot, some or not very much?

B.3.5 Issue attitudes

[issue presentation order always randomized]

Concern

Next, we would like to ask your opinion about a few social issues. For each of the issues below, please tell us how serious of a problem you think this issue is for our society.

Experiment 1 and gender version of Experiment 4:

- Poverty
- Depression
- Obesity (being seriously overweight)
- Car accidents

Experiment 2 and race version of Experiment 4:

- Climate change
- Air pollution
- Suicides
- Addiction to opioids (strong painkillers)

Experiment 3:

- Unemployment
- Sexual assault
- Poverty
- Climate change

answer options: Not at all serious/not a problem – Not very serious – Somewhat serious – Very serious

Importance

Please rank the issues below by how important you think they are as problems in our society. You can drag and drop issues to change their order.

Experiment 1 and gender version of Experiment 4:

- Poverty
- Depression
- Obesity (being seriously overweight)
- Car accidents
- Smoking
- Unemployment
- Air pollution
- Climate change

Experiment 2 and race version of Experiment 4:

- Poverty
- Suicides
- Addiction to opioids (strong painkillers)
- Car accidents
- Smoking
- Unemployment
- Air pollution
- Climate change

Experiment 3 and LGBT version of Experiment 4:

- Poverty
- Suicides
- Addiction to opioids (strong painkillers)
- Car accidents
- Sexual assault
- Unemployment
- Air pollution
- Climate change

Spending support

For each of the issues below, how much would you favor extra government spending to tackle them?

Experiment 1 and gender version of Experiment 4:

- Poverty
- Depression
- Obesity (being seriously overweight)
- Car accidents

Experiment 2 and race version of Experiment 4:

- Climate change
- Air pollution
- Suicides
- Addiction to opioids (strong painkillers)

Experiment 3 and LGBT version of Experiment 4:

- Unemployment
- Sexual assault
- Poverty
- Air pollution

answer options: Do not favor – Favor a little – Favor moderately – Favor very much

B.3.6 Self-interest

Experiment 1, gender:
Now, we would like you to think about whether some issues could happen to you personally. For each of the issues below, do you think this is something that will happen to you in the future?

- Poverty
- Depression
- Obesity (being seriously overweight)
- Car accidents

Experiment 2, race:
Now, we would like you to think about whether some issues could affect you personally. For each of the issues below, do you think this is something that could affect you in the future?

- Climate change
- Air pollution

And for each of the issues below, do you think this is something that could happen to **you or someone close to you** in the future?

- A suicide attempt
- Addiction to opioids (strong painkillers)

Experiment 3, LGBT:
For each of the issues below, do you think this is something that could happen to **you or someone close to you** in the future?

- Unemployment
- Sexual assault

answer options:

- Will probably not [happen/affect me]
- May or may not [happen/affect me]
- Will probably [happen/affect me]
- [Has/is] already [happened/affecting me]
- Prefer not to answer

B.3.7 Prior/Posterior Beliefs

Next, we would like to ask you about a few issues in the United States, and whether they happen:

- more often to [men / White people / LGBT people],
- more often to [women / Black and Latino people / heterosexual (non-LGBT) people], or

- about as often to [men / White people / LGBT people] as to [women / Black and Latino people / heterosexual (non-LGBT) people].

If you are unsure about an answer, please don't look up more information—instead, just give us your best guess.

Experiment 1 and gender version of Experiment 4:	Experiment 2 and race version of Experiment 4:	Experiment 3 and LGBT version of Experiment 4:
In your opinion, [does poverty/does depression/does obesityÂ (being seriously overweight)/do car accidents] happen more to men, more to women, or is it about the same?	In your opinion, [do suicides/does addiction to opioids (strong painkillers)] happen more to White people, more to Black and Latino people, or is it about the same? In your opinion, [does climate change/does air pollution] affect White people, does it affect Black and Latino people more, or is it about the same?	In your opinion, does [unemployment/sexual assault] happen more to LGBT people, more to heterosexual people, or is it about the same?

answer options:

- More to [men / White people / LGBT people]
- More to [women / Black and Latino people / heterosexual (non-LGBT) people]
- About the same
- Don't know

[after every belief question, unless respondent said "Don't know"]: How much confidence do you have in your answer?

- A lot
- A moderate amount
- A little

B.3.8 Just world beliefs

[Experiment 1–3] Finally, we would like to know a bit more about how you view the world. Please tell us how much you agree or disagree with each of the statements below.

- I feel that people get what they are entitled to have

- I feel that a person's efforts are noticed and rewarded
- I feel that people who meet with misfortune have brought it on themselves
- I basically feel that the world is a fair place

answer options: Strongly disagree – Disagree – Somewhat disagree – Neither agree nor disagree – Somewhat agree – Agree – Strongly agree

B.3.9 Treatment

We would like to share with you a piece of information about a social issue in the United States. Please take a moment to read it.

Gender

women:

- In the US, poverty happens more to women than to men. Women are 30% more likely to be living in poverty than men.
- In the US, depression happens more to women than to men. Women are twice as likely as men to be have depression.

men:

- In the US, car accidents happen more to men than to women. Men are twice as likely as women to die in a car crash.
- In the US, obesity happens more to men than to women. Men are 20% more likely than women to be seriously overweight.

This info comes from [the US Census Bureau, Gallup, the Centers for Disease Control and Prevention, the Kaiser Foundation].

Race

minorities:

- In the US, climate change affects minorities more than white people. Black and Latino people are already 15% times more likely than white people to die from causes related to very hot weather.
- In the US, air pollution affects minorities more than white people. Black and Latino people live in places where the air has 40% more of the harmful chemical NO_2 compared to white people.

white people:

- In the US, suicide affects white people more than minorities. White people are three times more likely to commit suicide than Black or Latino people.

- In the US, addiction to opioids (strong painkillers) affects white people more than minorities. White people are twice as likely to die from an opioid overdose than Black or Latino people.

This info comes from [the American Journal of Epidemiology, Environmental Health Perspectives, the Suicide Prevention Resource Center, the Kaiser Foundation].

LGBT

- In the US, unemployment affects LGBT people more than straight people. LGBT Americans are 50% more likely to be jobless than straight Americans.

- In the US, sexual assault affects LGBT people more than straight people. LGBT Americans are three times more likely to have been sexually assaulted than straight Americans.

This info comes from [the Williams Institute, the American Journal of Public Health].

Appendix C

The party bandwagon: supplementary material

Table C.1: Studies compared in a meta-analysis of elite cue effect sizes.

reference	topic	cue source	cue content	source type	context	issue polarization
Bechtel et al. (2015)	immigration, housing	in-party OR out-party	actual party position	collective	Switzerland	medium
Benegal and Scruggs (2018)	climate	politician or scientist	consensus on climate change	individual	US	high
Berinsky (2017)	health care	legislator	rumor rejection	individual	US	high
Bolsen et al. (2014)	energy	Congress Republicans AND/OR Democrats	majority supports	collective	US	high
Brader et al. (2013)	many	in-party	party proposed policy	collective	Europe	mixed
Broockman and Butler (2017)	many	legislator	legislator's own opinion	individual	US	mixed
Bullock (2011)	health care	House Republicans AND Democrats	90% of one party supports and 90% of other party opposes	collective	US	medium
Butler and Hassell (2018)	many	elected official	own opinion	individual	US	mixed
Cohen (2003)	welfare	House Republicans OR Democrats	95% of one party supports, 10% of the other	collective	US	high
Druckman et al. (2013)	immigration, drilling	Congress Republicans AND Democrats	actual party positions (both)	collective	US	low + high
Gelpi (2010)	Iraq	president Bush	real positive, fake negative quote	individual	US	high
Nicholson (2012)	immigration, housing	president(ial candidates)	politician supports	individual	US	low + high

Table C.1: Studies compared in a meta-analysis of elite cue effect sizes.

reference	topic	cue source	cue content	source type	context	issue polarization
Nicholson (2011)	welfare	House Republicans AND Democrats	"imagine" 95% of one party supports, 10% of other party	collective	US	high
Tesler (2018)	climate	Congress Republicans AND Democrats	more than ever believe in climate change	collective	US	high
Zhou (2016)	climate	fictional former US congressman	pro-climate quote, framed in article	individual	US	high
Petersen et al. (2013)	many	in-party OR out-party	party proposed policy	collective	Denmark	mixed
Boudreau and MacKenzie (2014)	many	in-party	party supports	collective	California	mixed
Levendusky (2010)	many	Congress Republicans AND Democrats	opinion distribution by party	collective	US	low

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