

# In-group interest cues do not change issue attitudes

Anonymized version

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## **Abstract**

In this paper, 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.

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 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. In this paper, I ask whether people tend to change their political attitudes after learning how an issue affects members of their demographic in-groups. In this way, I aim to isolate the effects of in-group interests on opinion formation.

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.

There are both theoretical and empirical reasons to doubt self-interest as an explanation for caring about in-group interests. First, people know their own life circumstances quite well. 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). Third, Gay et al. (2014) find that linked fate works like a personality trait: individuals tend to have a sense of linked fate either with all of their social groups or with none of them. Combining this with their finding that linked fate is generally not a good predictor of political opinions or behavior, they conclude that it should probably be understood as a psychological tendency towards feeling connected with social groups.

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 both 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 the policy or politician has provided for the group—in other words, it is based on in-group interests (Kinder, 1998).

In the real world, there are many reasons why members of a demographic group might feel differently about an issue: because the demographic is correlated with ideology; because the issue is associated with a disliked out-group; because opinion leaders in the group have taken a public stance on the issue; because there is an opinion norm within the group; because acting on the issue would confer social status to the group (e.g. marriage equality);

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.

In this study, I look at gender groups (women and men), then racial groups (minorities and white people), and finally LGBT people. Among those groups, we would perhaps expect gender interests to be the least powerful opinion driver. Gender gaps in opinion are generally relatively small, even on women’s issues (Huddy et al., 2008). Since much of the evidence on in-group favoritism comes from studies of racial and ethnic groups, those groups are an obvious choice for a second experimental sample. Although a large part of the literature focuses on negative sentiment towards racial out-groups, there is also positive identification with racial in-groups, 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 2018) whose identity is already heavily politicized. Across all groups, I would expect my treatment to have stronger effects than other possible cues about in-group interests. This is because the treatment tells respondents about relative deprivation of their group compared to others—a realization that is thought to be especially motivating (Huddy et al., 2013).

Surprisingly, findings show that the effects of in-group interest cues on attitudes are minimal across all groups—even though respondents learn the information just before I measure their attitudes, and even for people who identify strongly with the group. This suggests that group thinking about political issues is not (just) about what is in the interest of the in-group, and that issue opinions are not driven by linked fate and/or in-group favoritism in these cases. Instead, in-group–issue connections are probably formed through repeated

elite discourse or personal experiences, or they may be limited to issues that map “naturally” onto people’s existing beliefs about the groups. In other words, while some real-world issue opinions may well be 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.

## **Experiment 1: Gender**

### **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 (presumably) surprising. So, as an example, one third of men were treated with information about men and obesity; one third read information about men and car accidents; and a final third were not treated at all. All of these statements are backed by real data. In each case, the respondent also learned 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 1 contains the exact wordings of all survey questions and treatment statements. Appendix section 2 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 [links redacted for anonymization].

## 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 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 2 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 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 limit of such an interval has 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 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

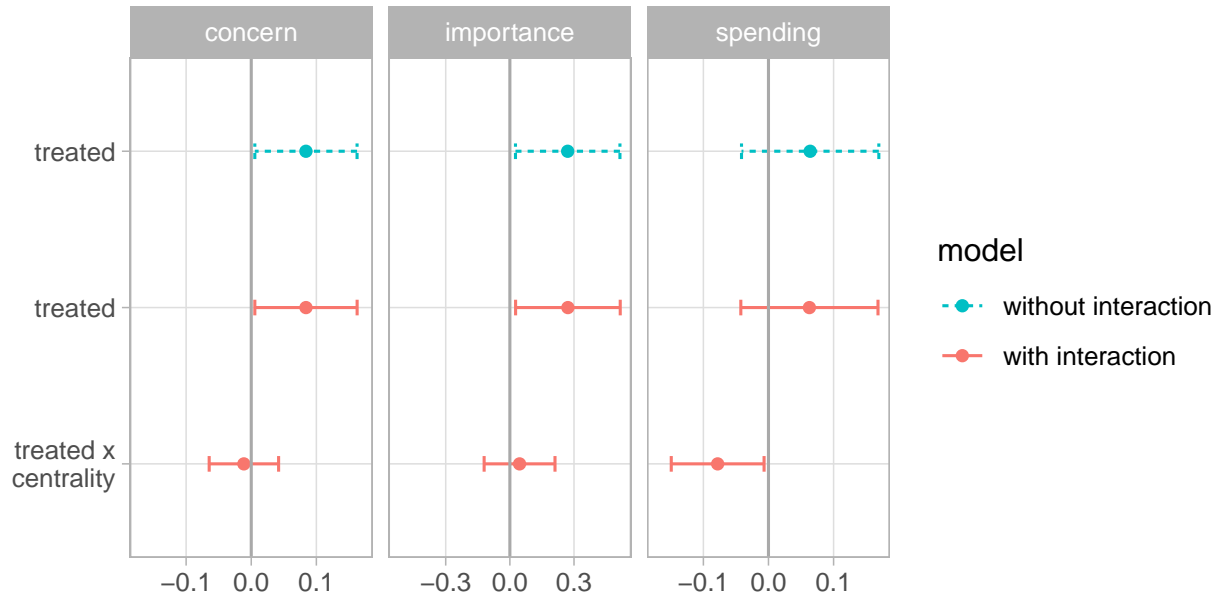


Figure 1: Effect of learning gender group interests on pre-post difference in three issue attitudes, without and with an interaction between treatment and centrality of gender identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

spending: unexpectedly, increasing centrality by one point (on a seven-point scale) *decreases* the treatment effect by about .1.

## 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 ( $n = 451$ ).

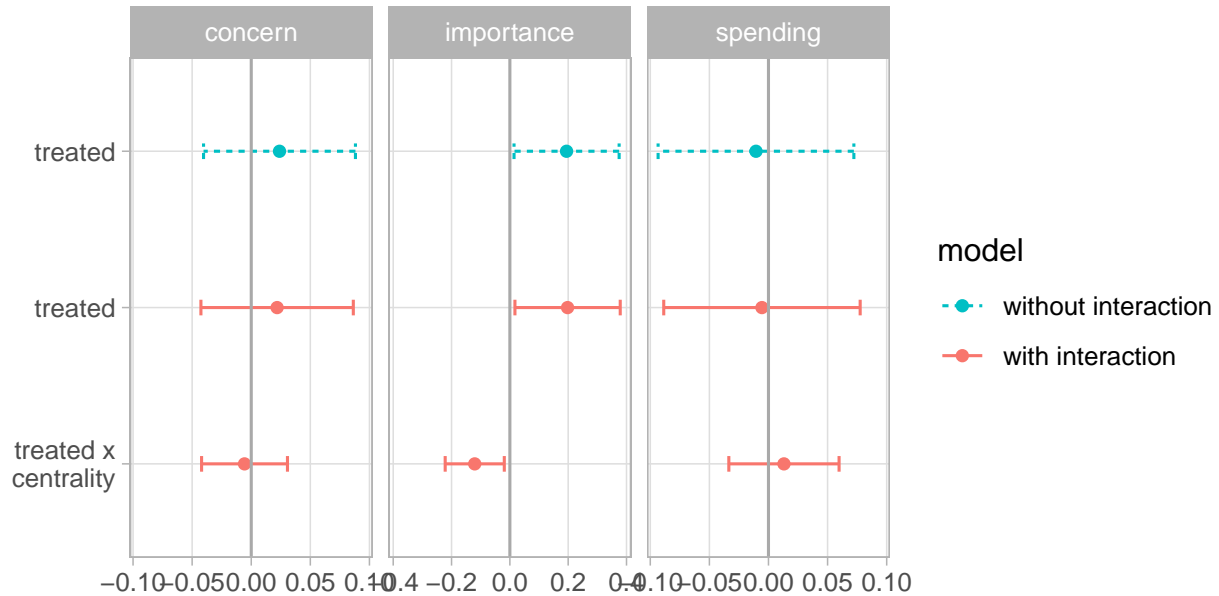


Figure 2: Effect of learning racial/ethnic group interests on pre-post difference in three issue attitudes, without and with an interaction between treatment and centrality of racial/ethnic identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

## Results

Figure 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 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.<sup>1</sup> As above, among the dependent

<sup>1</sup>This analysis was not pre-registered. See Appendix section 3.1 for more on intergroup differences in

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:  $0.01$ ,  $SE = .11$ ).

### 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.

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identification.



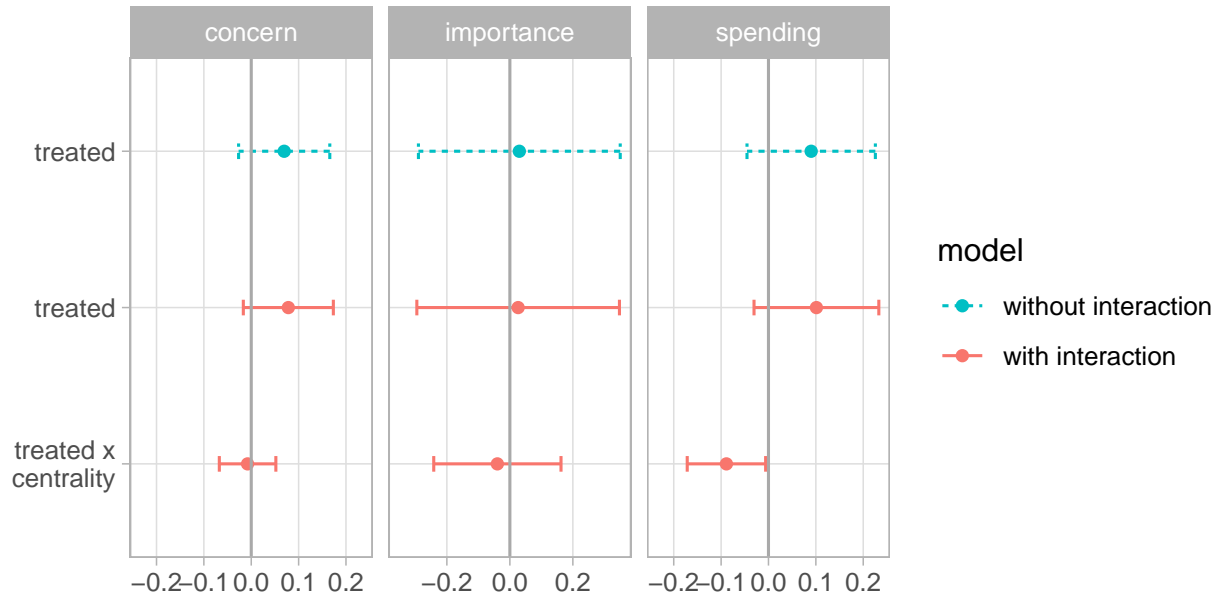


Figure 3: Effect of learning LGBT group interests on three issue attitudes, without and with an interaction between treatment and centrality of LGBT identity, with 90% confidence intervals. Concern and spending are 4-point scales, importance is an 8-issue ranking.

## Results

Figure 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 0.17 (concern), 0.35 (importance) and .23 (spending).

As the interaction effect estimates in Figure 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.

## Robustness checks, linked fate and self-interest

Appendix section 3 shows that the null or small effect findings 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.

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:  $est = 0.009$ ,  $p = 0.12$ ; gender and importance:  $est = -0.003$ ,  $p = 0.88$ , race and importance:  $est = -0.003$ ,  $p = 0.44$ ). The treatment increases perceived threat to self, but the effects are small (gender:  $est = 0.09$ ,  $p = .07$ , race:  $est = -0.02$ ,  $p = .72$ , LGBT:  $est = 0.17$ ,  $p = .12$  on a four-point scale).

## Discussion

In this article, I described three experiments testing the effect of telling people that an issue affects their social group especially. I measured the effect of this 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. This cannot be chalked up to their pre-existing knowledge about group interests, because before the treatment (in Experiment 1 and 2), these respondents were no more likely to believe that the treated issues were related to their gender or race. One possible explanation is that strong identifiers are reluctant to connect their own group to a societal problem—as most of the issues in this study have a negative connotation. 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 real-life experience with the issue affecting (a member of) their social group. 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).

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 all kinds of political messaging. In-group favoritism could be 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.

## 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 some issues just naturally map onto group divisions, as Winter (2008) suggests? Or do we need both? We know that group-based thinking about politics exists—knowing that it is not purely interest-based makes it all the more intriguing.

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