

Does Counter-Attitudinal Information Cause Backlash?

Results from Three Large Survey Experiments*

Andrew Guess[†]
Princeton University

Alexander Coppock[‡]
Yale University

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Abstract

Several theoretical perspectives suggest that when individuals are exposed to counter-attitudinal evidence or arguments, their preexisting opinions and beliefs are reinforced, resulting in a phenomenon sometimes known as “backlash.” We formalize the concept of backlash and specify how it can be measured. We then present results from three survey experiments — two on Mechanical Turk and one on a nationally representative sample — in which we find no evidence of backlash, even under theoretically favorable conditions. While a casual reading of the literature on information processing suggests that backlash is rampant, we conclude that it is much rarer than commonly supposed.

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[†]Assistant Professor of Politics and Public Affairs, Princeton University. Email: aguess@princeton.edu

[‡]Assistant Professor, Department of Political Science, Yale University. Email: alex.coppock@yale.edu

If people of opposing views can each find support for those views in the same body of evidence, it is small wonder that social science research ... will frequently fuel rather than calm the fires of debate.

— Lord, Ross, and Lepper (1979)

For several decades, research on public opinion and information processing has presented a challenge for believers in evidence-based decision-making. Its prognosis for the body politic is dire: Instead of prompting a reconsideration of long-held views, counter-attitudinal evidence may actually strengthen preexisting beliefs, resulting in polarization. This prediction of *backlash* is associated with numerous theoretical perspectives and has led to an emerging consensus among scholars that attempts to persuade voters, challenge opponents, or correct factual misperceptions can often lead to the opposite of the intended effect.

According to this consensus, people work — consciously or not — to protect their world-views via a series of complementary belief-preserving mechanisms (Kunda 1990). Examples include the *prior attitude effect*, the tendency to perceive evidence and arguments that support one's views as stronger and more persuasive than those that challenge them; *disconfirmation bias*, in which people exert effort to counter-argue vigorously against evidence that is not congruent with their beliefs; and various forms of selective exposure and selective attention to congenial information, sometimes referred to as *confirmation bias* (Taber and Lodge 2006). The cumulative effect of these mechanisms is polarization. People exposed to the same information may respond by strengthening their preexisting views.

The canonical explication and demonstration of these mechanisms appears in Lord, Ross and Lepper (1979), in which both pro- and anti-death-penalty college students were exposed to mixed scientific evidence on the effectiveness of capital punishment on crime deterrence. To their surprise, the authors found that the subjects did not moderate their views; rather, those who initially supported the punishment reported becoming more pro-capital punishment on average by the end of the study, and those who opposed it reported becoming less so. This study helped inspire a research agenda spanning psychology (Miller, McHoskey, Bane and Dowd 1993; Kuhn and Lao 1996), political science (Redlawsk 2002; Taber and Lodge 2006; Lau and Redlawsk 2006; Taber, Cann and Kucsova 2009; Nyhan and Reifler 2010), and other fields such as public health (Strickland, Taber and Lodge 2011; Nyhan, Reifler, Richey and Freed 2014). Backlash is one of the phenomena frequently invoked in this literature. How

prevalent is it?

Expectations of Backlash

To our knowledge, the first appearance of backlash effects in the literature occurs in Lazarsfeld, Berelson and Gaudet (1944), who observed “several boomerangs upon people who resented what they read or heard and moved in the opposite direction from that intended” (p. 154). The authors note this in passing while explaining the reasons why personal contact may be more effective — and less likely to produce such “boomerangs” — than media messages.

Since then, a number of distinct theories have accommodated the possibility of backlash during the opinion formation process. In John Zaller’s **Receive-Accept-Sample model** (1992), opinions are generally stable. Depending both on individuals’ level of political awareness and the partisan mix of elite communications, people will exhibit varying levels of receptivity to new information. Those least likely to be swayed one way or another are people with low awareness (who are not likely to be exposed to political messages at all) and high awareness (who possess sufficient knowledge and sophistication about political issues to successfully avoid or resist contrary messages). Only those in between have the opportunity to both receive *and* accept new political messages, the balance of which will depend on the level of elite consensus. When confronted with information about an issue, people then sample from the “top of the head” considerations they have accumulated over time.

Under the RAS model, new information is assimilated only under certain conditions, and even then is brought to conscious awareness only when required by a survey response or interpersonal context. When new information *can* be absorbed, the predicted result is attitude change in the direction of the overall balance of arguments made by elite political actors. The model is generally compatible with growing extremity in attitudes over time, particularly among high-awareness individuals who only seek out information that reinforces their existing considerations. At the same time, it is also consistent with limited predictions of backlash. In a survey context, confrontation with challenging information may cause highly informed individuals to bring counterarguments to mind, creating a mix of considerations more hostile to that perspective (e.g., Lord, Ross and Lepper 1979; Kuklinski, Quirk, Jerit and Rich 2001).

In contrast to Zaller’s memory-based approach, Lodge and Taber’s **John Q. Public**

model (2006; 2013) explains political evaluations as the result of motivated reasoning driven largely by unconscious processes. People make snap, emotion-laden judgments of political stimuli on the basis of affective tallies stored in memory. These tallies are in turn determined by primes and other subliminal cues accompanying the issues, candidates, or groups under consideration. Conscious processing of political information is thus little more than rationalization of the associated attitudes that elude our awareness. The JQP model predicts backlash if evidence that challenges one’s political views triggers “hot cognitions” about related topics, which in turn motivate a search (in memory or elsewhere) for confirmatory information (Redlawsk 2002).

More recently, Kahan (2012) has applied the **theory of cultural cognition** to public perceptions of risk on issues such as climate change. This perspective suggests, for example, that endorsing factual positions at odds with scientific consensus can be “expressively rational” in the sense that it reinforces one’s membership in a cultural or ideological group. Such identity-protective cognition can be both conscious and unconscious, and it could lead to predictions of backlash via mechanisms similar to JQP.

A final perspective derives from the **Bayesian learning model**. This model provides a simple, mathematically coherent mechanism, via Bayes’ rule, for updating one’s prior beliefs in light of new evidence. The predictions of the model are subtle, leading to occasional disagreements about the expected pattern of evidence under various conditions. For example, whether “unbiased” Bayesian learning implies convergence or parallel updating in evaluations of political figures has been the subject of continuing debate (Gerber and Green 1999; Bartels 2002; Bullock 2009). Bayesian rationality has often been taken to rule out polarization, but even this is possible in the presence of idiosyncratic likelihood functions, which determine the subjective probability of observing a piece of evidence given a particular state of the world. In other words, the Bayesian model is compatible with a wide range of empirical patterns, even including backlash (Benoît and Dubra 2016).

Thus far, we have outlined four theoretical perspectives that predict, or at least allow for, the possibility of backlash effects.¹ The purpose of this paper is not to adjudicate among these

¹Our findings are distinct from those in the partisan motivated reasoning literature, in which cues can induce in- and out-partisans to update policy preferences in opposite directions (Levendusky 2013; Leeper and Slothuus 2014; Druckman, Levendusky and McLain 2018).

theories but instead to document the *prevalence* of backlash. Focusing on randomized experiments, we searched the literature for evidence of backlash effects in response to informational treatments. Within the context of correcting factual misperceptions, there are several such studies. Nyhan and Reifler (2010) discovered evidence of “backfire” effects to corrections of misinformation embedded in mock news articles about weapons of mass destruction in Iraq and funding for stem cell research, though these findings were not reproduced in a replication attempt (Wood and Porter 2018). In another study, Nyhan et al. (2014) showed that providing a correction about vaccine misperceptions can decrease vaccine-averse subjects’ reported intention to vaccinate; this finding was replicated in Nyhan and Reifler (2015). Finally, Zhou (2016) identifies so-called “boomerang” effects in framing experiments on Republicans’ responses to climate change messaging.

Alongside these findings are studies that either do not find convincing evidence of backlash or highlight alternative explanations. Redlawsk, Civettini and Emmerson (2010) examine a hypothesized “affective tipping point” or specific dose of counter-attitudinal information at which backlash stops and incorporation of the evidence begins. While the authors do not emphasize this point, the effects of small doses are too small to be distinguishable from zero.² The effects of large doses are positive and significant. A more straightforward case is Americans’ response to advances in gay rights: Bishin, Hayes, Incantalupo and Smith (2015) conclude from both experimental and over-time survey data that there is “no evidence of backlash by the public as a whole or among relevant constituent groups.” Within the context of the corrections literature, Wood and Porter (2018) execute 36 versions of the Nyhan and Reifler (2010) design over a range of different issues, finding evidence of backfire in only one. A recent study of political rumors (Berinsky 2015) found that backlash can be prevented through the use of partisan source credibility cues. Finally, an emerging literature argues that apparent factual misperceptions are at least partially an artifact of expressive responding by partisans (Prior, Sood, Khanna et al. 2015; Bullock, Gerber, Hill, Huber et al. 2015).

In this paper, we present results from three well-powered randomized experiments, each designed to identify the effect of exposure to information on the attitudes and beliefs of different subgroups. We chose three distinct issues intended to cover a range of possible

²See Redlawsk, Civettini and Emmerson (2010, fn. 15).

backlash triggers. As we detail in the next section, we operationalize the concept of “backlash” as the appearance of negative treatment effects for some subgroups — in other words, attitude change in the direction contrary to that suggested by the information presented.

Across all three studies, we find no evidence of backlash among theoretically relevant subgroups. This is most remarkable in our first study, on gun control, which was conducted on a nationally representative sample and fielded in the aftermath of what was at the time the largest mass shooting in American history. We generally find that subjects update, if at all, in the direction of the information. Each of the theoretical accounts highlighted here can accommodate backlash as well as its absence. Nevertheless, our results suggest that while backlash may occur under some conditions with some individuals, it is the exception, not the rule.

Measuring Backlash

Suppose that each individual i is endowed with three potential outcomes $Y_i(neg)$, $Y_i(control)$, and $Y_i(pos)$, corresponding to the attitude he or she would express if exposed to negative information, no information, or positive information. We define two individual-level treatment effects $\tau_{i,neg}$ and $\tau_{i,pos}$. $\tau_{i,neg}$ is defined as the difference between the negative and control potential outcomes: $Y_i(neg) - Y_i(control)$. $\tau_{i,pos}$ is defined analogously: $Y_i(pos) - Y_i(control)$. Individual i updates his or her view in the direction of evidence if $\tau_{i,neg} \leq 0$ and $\tau_{i,pos} \geq 0$. Individual i “backlashes” if $\tau_{i,neg} > 0$ or $\tau_{i,pos} < 0$.

Our expectation is that for *most* individuals and *most* treatments, $\tau_{i,neg}$ will be negative and $\tau_{i,pos}$ will be positive. Our main concern is whether there are *any* individuals for whom these signs are reversed. Unfortunately, due to the Fundamental Problem of Causal Inference (Holland 1986), we can never observe $\tau_{i,neg}$ or $\tau_{i,pos}$ for any individual. We can, however, estimate average causal effects. The Average Negative Treatment Effect (ANTE) is defined as $E[\tau_{i,neg}]$, where $E[\cdot]$ denotes the expectation operator. The Average Positive Treatment Effect (APTE) is defined analogously.

In the empirical sections below, we will present three randomized experiments in which we obtain estimates of the ANTE and the APTE. What can we conclude from these estimates? If the ANTE is estimated to be negative and the APTE is estimated to be positive, we

cannot draw strong conclusions about whether or not $\tau_{i,neg}$ and $\tau_{i,pos}$ were “correctly” signed for all individuals; that is, we cannot conclude that there is no backlash simply because on average, individual effects have the expected sign. If, however, the ANTE or the APTE are estimated have the “wrong” sign, we can indeed conclude that at least some number of subjects experienced backlash.

We will extend this logic to subgroups of subjects. The CANTE and the CAPTE are the conditional cousins of the ANTE and the APTE, i.e., they refer to the average causal effects conditional on membership in a subgroup. In particular, the majority of the theories of backlash enumerated above predict that backlash is most likely among individuals whose baseline opinions are opposed to the evidence that they see. To be specific, $Y_{baseline,i}$ is a pre-treatment characteristic of individuals. $Y_{baseline,i}$ is likely to be correlated with (but distinct from) $Y_i(control)$, the post-treatment outcome that subjects express when assigned to the control condition. We define “proponents” as those for whom $Y_{baseline,i}$ is high and “opponents” as those for whom it is low.³ Backlash theories predict that $\tau_{i,neg}$ is likely to be *positive* among proponents and that $\tau_{i,pos}$ is likely to be *negative* among opponents. If so, we are more likely to find CANTE estimates to be positive among proponents and CAPTE estimates to be negative among opponents.

Even if we fail to find “incorrectly” signed average causal effects among these subgroups, we will not be able to rule out incorrectly signed individual causal effects. We are left with something of an inferential dilemma: We are looking for evidence of backlash, but the failure to do so does not rule backlash out completely. Our empirical strategy is therefore asymmetric: We can demonstrate that backlash occurs if we can estimate incorrectly signed average causal effects with sufficient precision, but we cannot conclusively demonstrate that it never occurs.

Another approach is to consider the variances of $Y_i(neg)$, $Y_i(control)$, and $Y_i(pos)$. If it is indeed true that $\tau_{i,neg}$ is negative for most, but positive for some, the variance of $Y_i(neg)$ will be higher than the variance of $Y_i(control)$. If effects are homogeneous across subjects, then the variance of the two sets of potential outcomes will be equal. We view an inspection of the variance of outcomes as only partially informative about backlash. While backlash would be variance-increasing, so too could other patterns of treatment effects.

³What “high” and “low” mean in any specific context is a matter of judgment, and in one empirical application, we also estimate conditional effects among “moderates,” those whose values of $Y_{baseline,i}$ are middling.

Research Approach

Our three studies share important design features, so we describe them together here for convenience. All three studies employ a within-and-between-subjects experimental design. First, respondents were invited to complete a pre-treatment (T1) survey in which we collect baseline demographic information, importantly including measures of $Y_{baseline,i}$. Second, respondents were invited back to a main survey (T2) in which treatments were allocated and post-treatment outcomes were collected.

We conducted these studies on two platforms, a nationally representative sample administered by GfK and Amazon’s Mechanical Turk (MTurk). In recent years, social scientists have recognized the utility of MTurk as a tool for recruiting subjects (e.g., Buhrmester, Kwang and Gosling 2011). While opt-in samples collected via MTurk should not be considered representative of the U.S. population, they have been shown to outperform laboratory-based convenience samples in ensuring adequate variation across sociodemographic and political characteristics of interest (Berinsky, Huber and Lenz 2012). Typically, MTurk samples tend to skew younger, less wealthy, better educated, more male, whiter, and more liberal than the population as whole. We opted for this approach in Study 2 and Study 3 in order to boost the representativeness relative to student populations (Sears 1986; Clifford and Jerit 2014), and to better ensure generalizability of results via similarity of subjects, treatments, contexts, and outcome measures across domains (Coppock and Green 2015).

Some scholars have worried about selection bias in the case of conservatives on Mechanical Turk (Kahan 2013). Further, MTurk workers may seek out the “right” answer or to exhibit other types of demand effects, especially given that members of that population tend to participate in many social science studies (Chandler, Mueller and Paolacci 2014). While we share these concerns, we also note that the evidence to date indicates that estimates obtained from MTurk samples match national samples well (Mullinix, Leeper, Druckman and Freese 2015; Coppock 2017). Most critically, the utility of MTurk samples for drawing inferences about the causal effects of information treatments depends on treatment effect heterogeneity. If the treatment effects for these subjects are substantially different from the effects for others, then MTurk is a poor guide to effects more generally. For this reason, Study 1 is fielded on a nationally representative sample.

In all three studies, we estimate average treatment effects (both the CANTE and CAPTE) of information separately for “proponents” and “opponents” as defined by pretreatment measures of our dependent variables. In Appendix A, we reproduce our analyses splitting our sample by ideology, partisanship, attitude extremity, attitude consistency, and issue importance.

Study 1: Gun Control

Study 1: Procedure

We fielded Study 1 on a nationally representative sample ($N = 2,122$) administered by the survey firm GfK from June 22-28, 2016, roughly 10 days after the mass shooting in Orlando, Florida, and in the midst of a heated debate about terrorism and the regulation of firearms. In a preliminary wave of the survey, administered 3-10 days after the shooting, we determined whether subjects support (“proponents”) or oppose (“opponents”) stricter gun control laws. We also asked subjects four questions about their preferred gun control policies. We combine all four dependent variables into a composite index using factor analysis in order to improve power (Ansolabehere, Rodden and Snyder 2008).

In the experiment, subjects were randomly assigned to one of three conditions: no information (control), pro-gun-control information (positive), or anti-gun-control information (negative). The treatments we employed were modeled on those of Lord, Ross and Lepper (1979). Subjects were shown graphical evidence of a relationship between gun control policies and four outcome variables: gun homicides, gun suicides, gun accidental deaths, and gun assaults. The evidence was presented as if it were the central finding in “Kramer and Perry (2014)”, a fictitious academic article. (See Appendix C for questionnaire and stimuli.) We then collected our dependent variables and asked again about subjects’ “proponent” or “opponent” status.

We assigned subjects to treatment conditions using block random assignment. Using the R package `BlockTools`, we created matched trios, matching on U.S. region, age, education level, Hispanic ethnicity, gender, income category, marital status, employment, party identification, and ideology. Within each trio, we used the R package `randomizr` to assign one unit to each

of the treatment conditions. Means and standard deviations for each treatment condition are shown in Table 1, along with the standard error of both our estimate of the mean and the standard deviation. While it is somewhat unusual to report uncertainty estimates for the estimated standard deviation of an outcome, we do so to facilitate comparisons of the variability of outcomes by treatment condition. As it happens, the standard deviations of the outcomes do not appear to vary by treatment condition and are very precisely estimated.

Table 1: Study 1 Gun Control : Treatment Conditions

Condition	N	T2 Attitude		T2 Belief	
		Mean	SD	Mean	SD
Control	730	-0.03 (0.04)	0.04 (0.00)	0.69 (0.02)	0.02 (0.00)
Positive Information	702	0.06 (0.04)	0.04 (0.00)	0.72 (0.02)	0.02 (0.00)
Negative Information	690	0.00 (0.04)	0.04 (0.00)	0.62 (0.02)	0.02 (0.00)

Bootstrapped standard errors are in parentheses.

Study 1: Analytic Strategy

We present results that we pre-registered in planned regression specifications. We use OLS with HC2 robust standard errors, separately by subject type. We employ survey weights (provided by GfK) for all models.

Study 1: Results

In Table 2, the coefficients on the positive information treatment are estimates of the CAPTE. Likewise, the coefficients on negative information correspond to the CANTE. The estimated effects of the information treatments are small and, in most cases, with standard errors as large or larger than the estimated coefficients. Whether as a result of the issue itself, the nature of the sample, or the timing of the experiment, we do not find substantial effects of information on our composite measure of gun control policy preferences. Importantly, however, these negligible persuasive effects are similar across subgroups: Pro-gun-control information has positive coefficients for both opponents and proponents of gun regulation, indicating a lack

Table 2: Effects of Information on Gun Control Composite Scale

	Dependent Variable: Composite Scale			
	Among Opponents		Among Proponents	
Positive Information	0.05 (0.09)	0.05 (0.08)	0.06 (0.06)	0.04 (0.06)
Negative Information	0.04 (0.08)	-0.003 (0.08)	0.04 (0.06)	0.04 (0.06)
Constant	-0.91 (0.05)	-1.69 (0.28)	0.41 (0.05)	0.08 (0.24)
Covariates	No	Yes	No	Yes
N	718	718	1,359	1,359
R ²	0.001	0.14	0.001	0.14

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

Covariates include age, registration, education, Hispanic ethnicity, gender, income, marital status, employment status, party ID, and ideology.

of evidence for backlash. Negative information similarly has positive coefficients for both subgroups in the unadjusted models (although for opponents with covariate adjustment, the coefficient is just below zero).

Table 3: Effects of Information on Gun Control Support

	Dependent Variable: Support Gun Control			
	Among Opponents		Among Proponents	
Positive Information	0.03 (0.04)	0.02 (0.04)	0.01 (0.02)	0.01 (0.02)
Negative Information	-0.02 (0.04)	-0.03 (0.04)	-0.08*** (0.02)	-0.07*** (0.02)
Constant	0.19 (0.03)	-0.03 (0.16)	0.93 (0.01)	0.77 (0.07)
Covariates	No	Yes	No	Yes
N	724	724	1,375	1,375
R ²	0.002	0.10	0.02	0.10

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

Covariates include age, registration, education, Hispanic ethnicity, gender, income, marital status, employment status, party ID, and ideology.

Turning to the binary gun control support dependent variable, which asks whether respondents support “stricter gun control laws in the United States,” we similarly estimate small but positive coefficients for positive information across subgroups.⁴ Unlike with the composite

⁴We note that this particular analysis — testing the effect on this question — was not preregistered.

index, however, we find negative effects of anti-gun-control information on both opponents and proponents. These estimates rise to conventional levels of significance for *proponents* of gun control, suggesting, contrary to predictions of backlash, that it is those in favor of gun control who are most receptive to evidence questioning its effectiveness. Together, these findings show a more robust persuasive effect on a generalized measure of support compared to specific, policy-related opinions.

We can also use the standard deviations reported in Table 1 to perform a test of whether the treatments polarized opinion. If the treatments were polarizing, the standard deviation in the successively more pro or more con treatment groups should be larger, but we do not observe this pattern. Formal statistical tests also reveal that the treatment groups do not differ with respect to the standard deviations of the outcomes.

Finally, in Appendix A, we show that there is no backlash in a preregistered analysis of the effect of the information treatments by party identification. This is significant because of its commonly theorized role as a perceptual filter that could, in the RAS or JQP accounts, promote polarization or backlash.

The results of Study 1 indicate that gun control attitudes do not consistently move in response to either positive or negative information. This is somewhat surprising given the well-powered nature of the experiment, but we acknowledge that the timing of the study, at the height of a national debate on gun control, may explain why the attitudes were difficult to move. The relatively equivocal findings also show, however, that backlash was unlikely to have occurred as a result of these treatments.

Study 2: Minimum Wage

Study 2: Procedure

A large number ($N = 2,979$) of survey respondents on Mechanical Turk were recruited to participate in a pre-treatment survey measuring demographic characteristics (age, gender, race/ethnicity, education, partisan affiliation, and ideological leaning) as well as baseline attitudes toward the minimum wage. From this large pool of survey respondents, 1,500 were invited to take part in the main survey testing the effect of videos on attitudes toward the

minimum wage. Invitations to take part in the main survey were offered on a random basis, though more slots were offered to younger respondents and those with stronger views (pro or con) about the minimum wage. Of the 1,500 recruited to the main survey, 1,170 participated.

Subjects were exposed to two videos on the subject of the minimum wage. Two of the videos were in favor of minimum wage increases, one presented by John Green, a popular video blogger, and the other presented by Robert Reich, former U.S. Secretary of Labor and established left-leaning public intellectual. On the “con” side of the debate, one video was presented by an actor, and the other by economics professor Antony Davies. Within each side, one video featured a relatively young presenter and the other a relatively old presenter. Finally, two videos were included as placebos, addressing mundane requirements of state minimum wage laws. Links to all six videos are available in Appendix C, as well as screenshots that convey the production quality and mood of the videos.

Subjects were randomized into one of thirteen conditions: placebo, or one of the twelve possible orderings of the four persuasive videos. Subjects answered intermediate questions relating to how well-made and persuasive they found each video, then at the end of the survey, they answered two questions which serve as our main dependent variables. The *Amount* question asked, “What do you think the federal minimum wage should be? Please enter an amount between \$0.00 and \$25.00 in the text box below.” The interpretation of this dependent variable may be colored by anchoring considerations (Tversky and Kahneman 1974), since specific wage numbers were mentioned in some of the treatment videos. The second dependent variable avoids this concern. The *Favor* question asked, “The federal minimum wage is currently \$7.25 per hour. Do you favor or oppose raising the federal minimum wage?” The response options ranged from 1 (Very much opposed to raising the federal minimum wage) to 7 (Very much in favor of raising the federal minimum wage).

Study 2: Analytic Strategy

We order the treatment conditions according to the amount of pro-minimum-wage video content. The information content of the *Con Con* conditions is scored -1, the *Pro Con* and *Placebo* conditions 0, and the *Pro Pro* conditions 1, as shown in Table 4. We will estimate separate regressions as written in Equation 1 for opponents, moderates, and proponents.

Opponents are defined as subjects whose pre-treatment *Favor* response was 4 or lower and whose *Amount* response was below the median response (\$10.00). Those with *Favor* responses of 4 or higher and *Amount* responses above the median are defined as proponents. All others are defined as moderates.

$$Y_i = \beta_0 + \beta_1(POS_i) + \beta_2(NEG_i) + \beta_3(PLACEBO_i) + \epsilon_i \quad (1)$$

Table 4 presents the means and standard deviations by experimental group. Here again, we see an indication that the treatments had average effects in their intended directions. The means of the *Con Young* / *Con Old* condition are lower than the means of the mixed conditions, which are themselves lower than the means of the *Pro Young* / *Pro Old* conditions. These differences are all statistically significant. Turning to the differences-in-standard-deviations, formal tests under the sharp null of no effect lend some support to the hypothesis that the treatments lead to increases in the polarization of opinion — the differences between the placebo condition and the *Pro Old*, *Con Old* condition are statistically significant for both dependent variables at the $p < 0.01$ level. However, while increases in the standard deviations of outcomes would be a consequence of backlash, these increases could also result from some individuals having larger treatment effects than others, but with all effects still correctly signed.

Study 2: Results

The results of Study 2 are presented in Tables 5 and 6. Focusing on the covariate-adjusted estimates, the video treatments had powerful effects for all three subject types. In contrast to Study 1, positive information had positive and statistically significant effects on subjects' preferred minimum wage amount; negative information had strongly negative effects.

A similar pattern of response is evident in Table 6. All coefficients are correctly signed. With the exception of negative information among opponents, all these coefficients are statistically significant.

Table 4: Study 2 (Minimum Wage): Treatment Conditions

Condition	N	Positive Information	Negative Information	Favor		Amount	
				Mean	SD	Mean	SD
Placebo	93	0	0	5.34 (0.18)	1.64 (0.12)	9.23 (0.29)	2.77 (0.33)
Con Young / Con Old	162	0	1	4.77 (0.15)	1.84 (0.08)	8.67 (0.21)	2.65 (0.24)
Pro Old / Con Old	165	0	0	4.99 (0.16)	2.09 (0.09)	9.99 (0.33)	4.28 (0.28)
Pro Old / Con Young	195	0	0	4.87 (0.14)	1.93 (0.07)	9.85 (0.24)	3.32 (0.24)
Pro Young / Con Old	169	0	0	5.01 (0.15)	1.90 (0.09)	9.30 (0.24)	3.03 (0.27)
Pro Young / Con Young	192	0	0	5.18 (0.12)	1.64 (0.09)	9.38 (0.21)	2.94 (0.32)
Pro Young / Pro Old	193	1	0	5.59 (0.12)	1.67 (0.11)	10.93 (0.28)	3.91 (0.28)

Table 5: Effects of Information on Preferred Minimum Wage Amount

	Dependent Variable: T2 Amount					
	Among Opponents		Among Moderates		Among Proponents	
Pos. Info (0 to 1)	0.43 (0.51)	0.60* (0.33)	1.88*** (0.37)	1.51*** (0.30)	1.31*** (0.37)	1.40*** (0.30)
Neg. Info (0 to 1)	-0.61 (0.50)	-0.70** (0.31)	-0.83*** (0.18)	-0.93*** (0.19)	-1.93*** (0.31)	-1.70*** (0.30)
Condition: Placebo	-0.12 (0.53)	-0.60** (0.25)	-0.35 (0.27)	-0.46* (0.25)	-0.79* (0.42)	-0.49** (0.24)
Constant	6.89 (0.21)	2.49 (0.86)	9.37 (0.10)	5.64 (2.66)	11.99 (0.18)	1.39 (1.19)
Covariates	No	Yes	No	Yes	No	Yes
N	343	343	356	356	470	470
R ²	0.01	0.66	0.19	0.33	0.10	0.44

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

The information content of the Placebo condition is coded 0.

Covariates include T1 Amount, T1 Favor, age, gender, ideology, party ID, and education.

Table 6: Effects of Information on Favoring Minimum Wage Raise

	Dependent Variable: T2 Favor					
	Among Opponents		Among Moderates		Among Proponents	
Pos. Info (0 to 1)	0.86*** (0.25)	0.77*** (0.21)	0.56*** (0.16)	0.55*** (0.14)	0.29*** (0.09)	0.35*** (0.07)
Neg. Info (0 to 1)	-0.02 (0.25)	-0.13 (0.19)	-0.63*** (0.21)	-0.66*** (0.22)	-0.56*** (0.18)	-0.48*** (0.16)
Condition: Placebo	0.70** (0.27)	0.34 (0.25)	0.28 (0.24)	0.20 (0.21)	0.01 (0.19)	0.07 (0.17)
Constant	2.97 (0.11)	1.72 (0.53)	5.34 (0.08)	3.11 (1.22)	6.34 (0.05)	2.64 (0.53)
Covariates	No	Yes	No	Yes	No	Yes
N	343	343	356	356	470	470
R ²	0.05	0.47	0.07	0.18	0.06	0.33

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

The information content of the Placebo condition is coded 0.

Covariates include T1 Amount, T1 Favor, age, gender, ideology, party ID, and education.

Study 3: Capital Punishment

The treatments used in Study 3 are adapted from those of Lord, Ross and Lepper (1979).⁵ In that study, subjects were presented sequentially with apparent scientific evidence both challenging and affirming the notion that the death penalty deters crime, what we would refer to as a “mixed evidence” condition. To this single condition, we added various combinations of pro-capital punishment, anti-capital punishment, and inconclusive evidence. We additionally made minor updates to the original text (changing the publication date of the fictitious research articles from 1977 to 2012, for example) and to the graphical and tabular display of the fabricated data using modern statistical software.

We recruited 1,659 MTurk subjects to take the T1 survey in which we gathered a series of pre-treatment covariates (age, race, gender, and political ideology) and two items concerning capital punishment: attitude toward the death penalty and belief in its deterrent effect. From the pool of 1,659, 933 subjects’ pre-survey responses indicated clear and consistent support or opposition to capital punishment. These subjects were invited to participate in the main survey. Among these, proponents were defined as subjects whose answers to the pre-treatment

⁵ Authors who performed an earlier replication of the original design (Kuhn and Lao 1996) generously shared the original experimental materials, so we were able to use the identical wordings of the study summaries and descriptions.

attitude and belief questions were between 5 and 7 on a 7-point scale. Opponents were defined as subjects whose answers to these questions were between 1 and 3. In total, 683 subjects participated in the main survey (287 proponents and 396 opponents).

The two main dependent variables measured subjects' attitudes and beliefs about capital punishment. The *Attitude* question asked, "Which view of capital punishment best summarizes your own?" The response options ranged from 1 (I am very much against capital punishment) to 7 (I am very much in favor of capital punishment). The *Belief* question asked, "Does capital punishment reduce crime? Please select the view that best summarizes your own." Responses ranged from 1 (I am very certain that capital punishment does not reduce crime) to 7 (I am very certain that capital punishment reduces crime).

Study 3: Procedure

Subjects were randomly assigned to one of six conditions, as shown in Table 7 below. All subjects were presented with two research reports on the relationship between crime rates and capital punishment that varied in their findings: *Pro* reports presented findings that capital punishment appears to decrease crime rates, *Con* reports showed that it appears to increase crime rates, and *Null* reports showed that no conclusive pattern could be discerned from the data. The reports used one of two methodologies:⁶ either cross-sectional (a comparison of 10 pairs of neighboring states, with and without capital punishment) or time-series (a comparison of the crime rates before and after the adoption of capital punishment in 14 states).

As in Study 2, the statistical models will operationalize the "information content" of the pair of reports seen by subjects in a linear fashion. The positive information content of two *Pro* reports is coded as 2, one *Pro* and one *Null* as 1, and so on. In order to allow the coefficient on information content to vary depending on whether the information is pro- or counter-attitudinal, we will split the information content variable into positive information and negative information, as shown in Table 7. We view the information content parameterization as a convenient way to summarize the overall pattern of results, not as an assertion that the effects are strictly linear. As before, we present the means and standard deviations of both outcome variables by treatment group in Table 7. These estimates provide an indication that

⁶See Online Appendix C for details on the study design and experimental materials.

the treatments had average effects in the “correct” direction.

Relative to the *Null Null* condition, the differences-in-standard-deviations across groups are generally not statistically significant at the 5% level, according to a randomization inference test conducted under the sharp null hypothesis of no effect for any unit. The only exception is the difference-in-standard-deviations between the *Null Null* and *Pro Null* conditions for the *Support* dependent variable ($p = 0.04$). This inference does not survive common multiple comparisons corrections, including the Bonferonni, Holm, and Benjamini-Hochberg corrections. We conclude from these tests that the treatments do not polarize opinion in the sense of increasing its variance.

Table 7: Study 3 (Capital Punishment) : Treatment Conditions

Condition	N	Positive Information	Negative Information	T2 Attitude		T2 Belief	
				Mean	SD	Mean	SD
Con Con	117	0	2	3.52 (0.19)	2.07 (0.07)	3.27 (0.15)	1.65 (0.09)
Con Null	116	0	1	3.15 (0.20)	2.12 (0.09)	3.11 (0.15)	1.58 (0.09)
Null Null	112	0	0	3.18 (0.20)	2.06 (0.09)	3.13 (0.15)	1.56 (0.08)
Pro Con	118	0	0	3.59 (0.21)	2.27 (0.08)	3.69 (0.17)	1.75 (0.09)
Pro Null	121	1	0	3.62 (0.21)	2.28 (0.07)	3.81 (0.15)	1.66 (0.09)
Pro Pro	102	2	0	3.69 (0.22)	2.22 (0.08)	4.13 (0.16)	1.61 (0.10)

Bootstrapped standard errors in parentheses.

Study 3: Analytic Strategy

The relatively complicated design described above can support many alternative analytic strategies. Each report is associated with seven intermediate dependent variables in addition to the two endline dependent variables. Subjects could have been assigned to eighteen different combinations of research reports. Reducing this complexity requires averaging over some conditions and making choices about which dependent variables to present. We present here our preferred analysis. We will focus on the separate effects of positive and negative

information on subjects' T2 responses to the *Attitude* and *Belief* questions. Because the *Null Null* and *Pro Con* conditions are both scored 0 on both the positive information and negative information scales, we include an intercept shift for the *Null Null* condition,⁷ as shown in Equation 2:

$$Y_i = \beta_0 + \beta_1(POS_i) + \beta_2(NEG_i) + \beta_3(Condition_i = \text{Null Null}) + \epsilon_i \quad (2)$$

We will estimate Equation 2 using OLS for proponents and opponents separately. $\hat{\beta}_1$ forms our estimate of the CAPTE and $\hat{\beta}_2$ our estimate of the CANTE.

Study 3: Results

Tables 8 and 9 present estimates of the effects of information on attitudes and beliefs about capital punishment. Focusing on the covariate-adjusted models, we estimate that a unit increase in positive information causes an average increase in support for capital punishment of 0.015 scale points among proponents and 0.068 scale points among opponents, neither of which is statistically significant. Negative information has a strong negative effect among proponents (-0.326, $p < 0.01$), and a weakly negative effect among opponents (-0.042, $p = 0.43$). These estimates imply that moving from the *Con Con* condition to the *Pro Pro* condition would cause proponents to move $2 \cdot 0.015 + 2 \cdot 0.326 = 0.682$ scale points and opponents to move $2 \cdot 0.068 + 2 \cdot 0.042 = 0.220$ scale points. While the treatment effects do appear to differ by subject type ($p < 0.05$), we do not observe the “incorrectly” signed treatment effects that backlash would produce.

Turning to Table 9, we observe that the effects of the information treatments on belief in the deterrent efficacy of capital punishment are nearly identical for proponents and opponents. For both groups, moving from *Con Con* to *Pro Pro* results in an entire scale point's worth of movement. Study 3 again does not provide any direct evidence of backlash. For both proponents and opponents, treatment effects were always correctly signed.

⁷This specification treats the *Pro/Con* condition as the baseline condition. Following the original study's design, we did not include a pure control condition. In Online Appendix B, we re-estimate the models for this study and for Study 2 with *Null Null* as the reference category instead.

Table 8: Effects of Information on Support for Capital Punishment

	Dependent Variable: T2 Attitude Toward Capital Punishment			
	Among Proponents		Among Opponents	
Positive Information (0 to 2)	0.05 (0.10)	0.01 (0.09)	0.12 (0.09)	0.07 (0.06)
Negative Information (0 to 2)	-0.27*** (0.10)	-0.33*** (0.08)	0.01 (0.08)	-0.04 (0.05)
Condition: Null Null	-0.23 (0.20)	-0.19 (0.16)	-0.002 (0.14)	-0.06 (0.10)
Constant	5.86 (0.13)	0.44 (0.56)	1.76 (0.10)	0.65 (0.23)
Covariates	No	Yes	No	Yes
N	287	287	395	395
R ²	0.04	0.41	0.01	0.47

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

The information content of the Null Null condition is coded 0.

Covariates include T1 Attitude, T1 Belief, age, gender, ideology, race, and education.

Table 9: Effects of Information on Belief in Deterrent Efficacy

	Dependent Variable: T2 Belief in Deterrent Effect			
	Among Proponents		Among Opponents	
Positive Information (0 to 2)	0.15 (0.11)	0.12 (0.10)	0.35*** (0.11)	0.31*** (0.09)
Negative Information (0 to 2)	-0.35*** (0.11)	-0.36*** (0.10)	-0.16 (0.10)	-0.20** (0.09)
Condition: Null Null	-0.32 (0.21)	-0.28 (0.19)	-0.27* (0.16)	-0.30** (0.14)
Constant	5.08 (0.14)	1.69 (0.68)	2.47 (0.12)	1.44 (0.33)
Covariates	No	Yes	No	Yes
N	287	287	395	395
R ²	0.08	0.25	0.09	0.34

*p < .1; **p < .05; ***p < .01

Robust standard errors are in parentheses.

The information content of the Null Null condition is coded 0.

Covariates include T1 Attitude, T1 Belief, age, gender, ideology, race, and education.

Summary of Results

Figure 1 summarizes the results of all three experiments, plotting the covariate-adjusted treatment effect estimates of positive and negative information from Tables 2, 3, 5, 6, 8, and 9. Out of 24 opportunities, 23 estimates are correctly signed. Needless to say, this pattern is unlikely to occur by chance; a formal binomial test roundly rejects the null hypothesis that the treatment effects were equally likely to be correctly or incorrectly signed ($p < 0.001$). Eleven of the 24 treatment effect estimates are statistically significant at $p < 0.05$ or better. While some individual estimates are less precise than we would like, the overall pattern of evidence is strongly in favor of individuals updating in the direction of the information presented rather than resisting it or otherwise exhibiting backlash.

Exploring Treatment Effect Heterogeneity

Thus far, we have considered whether proponents and opponents (as defined by pre-treatment measures of support) experience differently signed treatment effects in response to the same information. However, the proponent/opponent covariate is not the only potential moderator that could be associated with treatment effect heterogeneity. The theoretical accounts we outlined earlier hypothesize a few factors in particular. For example, cultural cognition would predict boomerang effects in response to factual claims that threaten one’s cultural or ideological worldview. The John Q. Public and related motivated reasoning accounts suggest a greater likelihood of backlash among the most politically knowledgeable and aware individuals as well as the strongest partisans. We were able to measure several factors in our studies: attitude extremity, issue importance, attitude consistency, ideology, and partisanship. We present analyses by these variables in Appendix A, again finding mild evidence of treatment effect heterogeneity but no evidence of backlash.

The covariate-by-covariate search for backlash is fraught with the potential for making false discoveries (Kerr 1998; Humphreys, Sanchez de la Sierra and van der Windt 2013; Gelman and Loken 2016). In order to guard against these pitfalls, we conduct a holistic analysis of treatment effect heterogeneity using a method that considers all measured moderators in a single model. Bayesian Additive Regression Trees (BART) is a recent advance in statistical

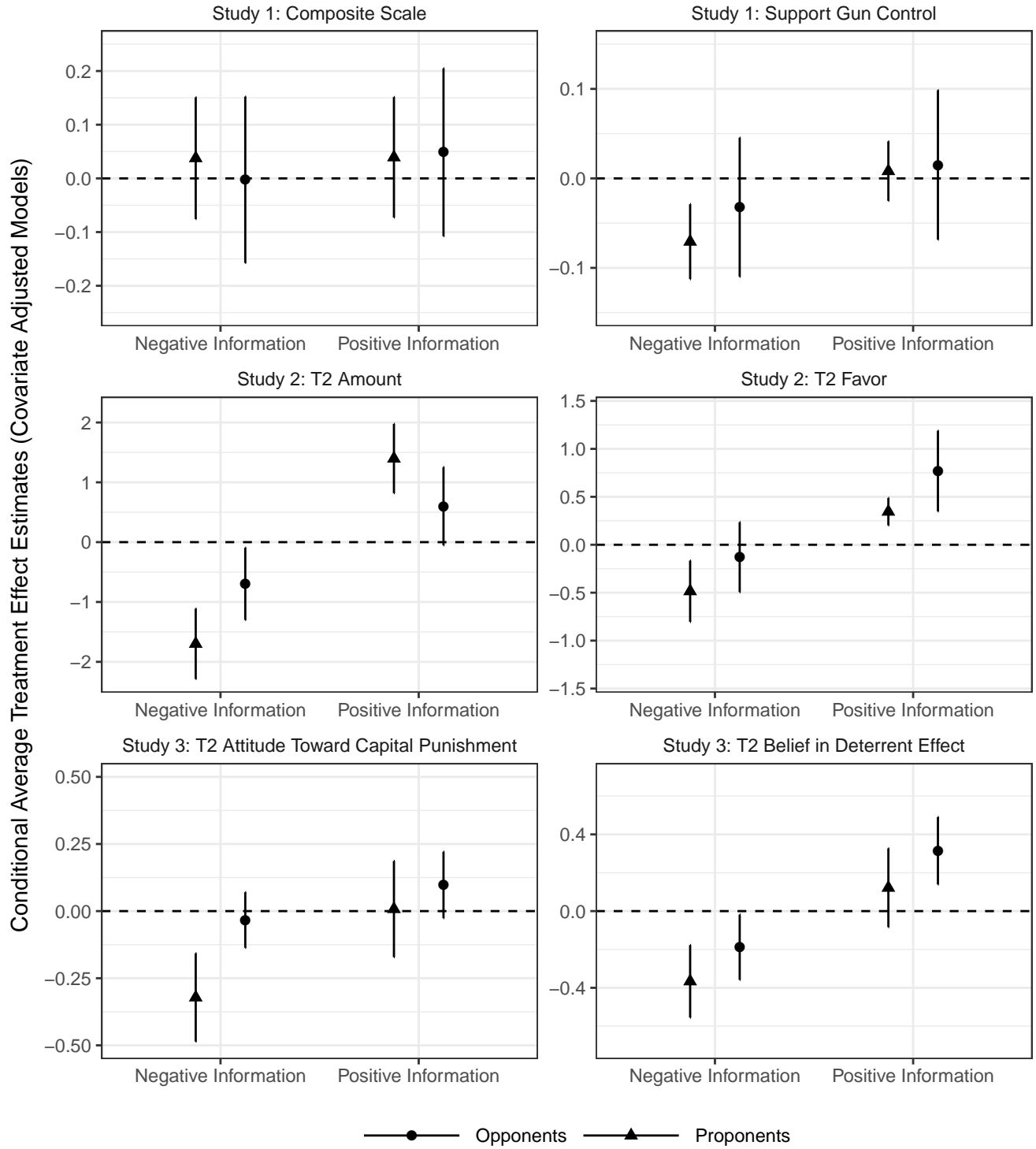


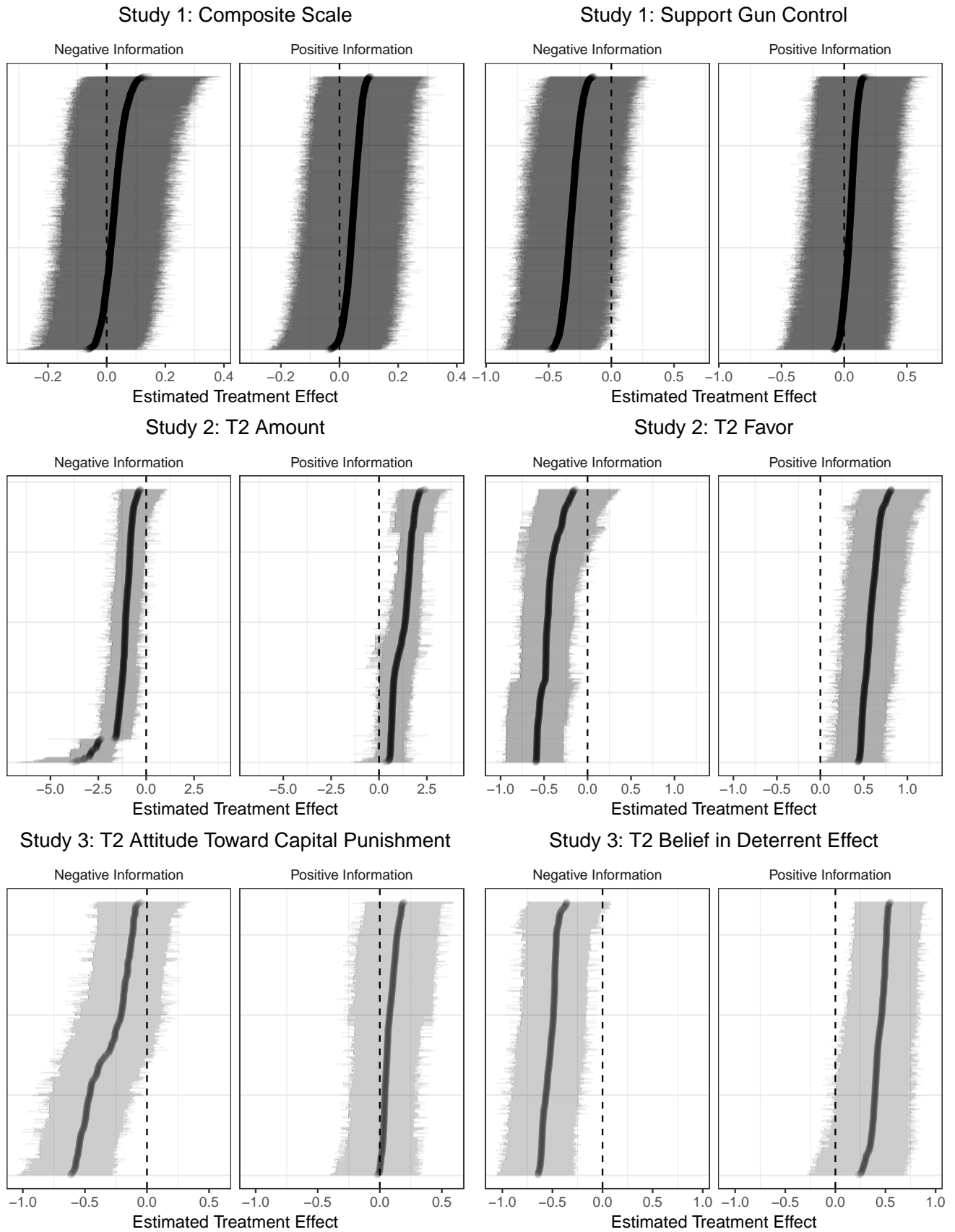
Figure 1: Regression coefficients from models (with controls) reported for all three studies, with 95% confidence intervals.

learning (Chipman, George and McCulloch 2007, 2010) that has been recommended by social scientists (Hill 2011; Green and Kern 2012) as a method for flexibly and automatically de-

testing treatment effect heterogeneity. BART is a sum-of-trees model that makes predictions of the conditional mean of the outcome variable while minimizing overfitting. A principal benefit of using BART over other machine learning algorithms is that it is robust to the choice of tuning parameters; we use the default settings implemented in the `dbarts` package for R. Standard statistical methods such as OLS have the advantage of providing relatively simple data summaries. The effect of treatment can be summarized as the coefficient on the treatment indicator and heterogeneity can be characterized by the coefficients on interaction terms. BART models, by contrast, cannot easily be summarized by a series of coefficients, so we rely on graphical presentations.

Figure 2 plots the estimated treatment effect for each subject as a function of their individual covariate profile, along with a 95% credible interval. According to this analysis, 93% of subjects have a positive treatment effect estimate when exposed to positive information and 77% have a negative treatment effect estimate when exposed to negative information. In none of the cases where a subject was predicted to have an incorrectly signed treatment effect does the 95% confidence interval exclude 0. This analysis reveals a remarkable level of treatment effect homogeneity. Most subjects appear to update in the direction of information, by approximately the same amount.

Figure 2: BART Estimated Treatment Effects



Discussion

How common is backlash? Across three studies, we find little evidence of the phenomenon. In the formulation introduced earlier, our estimates of CANTE are not positive, and our estimates of CAPTE are not negative. Evidence about gun control does not polarize opinion on the subject and appears to make proponents less likely to support gun regulation. Arguments about the minimum wage move respondents in the direction of evidence — toward supporting a higher or a lower dollar amount according to the slant of the evidence presented in the video treatments. Finally, pro-capital-punishment evidence tends to make subjects more supportive of the death penalty and to strengthen their beliefs in its deterrent efficacy, while evidence that the death penalty increases crime does the opposite.

The studies reported here were designed to encompass a variety of different types of information: scientific evidence described in tables and graphs in addition to more colloquial video appeals. The issues covered vary along numerous dimensions: both “hot” (gun violence) and “cold” (minimum wage), easily mapped to the partisan divide or not, and of varying degrees of salience. The results do not depend on the particular issue, whether arguments were presented in a one- or two-sided fashion, or idiosyncratic features of the topics chosen. Additionally, in results not reported here, backlash effects do not materialize over time. In two studies in which we collected follow-up responses (Study 2 and Study 3), the initial findings persist at least 10 days after the initial experiment, although the magnitudes are somewhat attenuated.

We noted above that our research design is asymmetric. If we had found significant evidence of incorrectly signed effects, we could have concluded that backlash did indeed occur in our experiments. On the whole, we did not find incorrectly signed effects — but we cannot conclude that backlash did not occur because there may have been some individuals (not exclusively defined by “opponent” or “proponent” status or partisan identity) that did in fact have an adverse reaction to the treatments. One intriguing possibility is that this very asymmetry contributes to the relatively widespread contention that presenting individuals with counter-attitudinal information is counterproductive: We can draw sharp inferences when backlash occurs but are left wondering when it does not. This imbalance may carry over into the visibility and novelty of published research findings.

These experiments show that when people are exposed to information, they update their

views in the expected or “correct” direction on average. However, one way in which these findings might not generalize to non-experimental contexts is if people selectively avoid counter-attitudinal information. Prior (2007) and Arceneaux and Johnson (2013) find that many individuals, if given the choice, simply consume entertainment rather than news information, thereby selecting out of both pro- and counter-attitudinal information in one stroke. On the other hand, Bakshy, Messing and Adamic (2015) show that while partisan Facebook users do appear to prefer pro-attitudinal news stories, they are exposed to and do consume a large amount of counter-attitudinal information. Other recent work shows evidence that selectivity in media consumption is limited to relatively small subgroups (Barberá 2014; Guess N.d.; Guess, Nyhan, Lyons and Reifler 2018). Future research should consider the conditions under which individuals could be induced to seek out larger or smaller doses of information with which they disagree.

A reasonable objection to these findings is that while individuals may not exhibit backlash when reading relatively sterile descriptions of academic studies, they may do so when actually arguing about some proposition with an opponent. This is partially a concern about demand effects: Were subjects, especially eager-to-please MTurk respondents, responding with stronger accuracy motivations than we would expect to find in a more naturalistic setting (e.g., Hauser and Schwarz 2016)? Since we did not explicitly incorporate motivational primes into our designs, we cannot completely rule out the possibility.⁸ However, we note that our dependent variables generally asked about subjects’ opinions on issues rather than factual perceptions, a feature illustrated most vividly in the case of gun control, in which arguments about the appropriate policy response are heavily contested (especially while our Study 1 survey was fielded). Beyond these particular experiments, scholars have found surprisingly little evidence of demand effects when information about researchers or their expectations are given to online study participants (Leeper and Thorson 2015; White, Strezhnev, Lucas, Kruszewska and Huff 2016; Mummolo and Peterson 2017).

Of course, many political disputes linger and do not easily resolve when new information comes to light. We speculate that in truly contentious political environments, in which opposing sides routinely insult the other (or much worse), the introduction of evidence could

⁸For more on accuracy motivations and external validity, see Druckman (2012).

induce a divergence in attitudes (Berry and Sobieraj 2013). Perhaps in such antagonistic contexts, individuals become distrustful of counter-attitudinal arguments. We leave the search for backlash effects in such contentious environments to future research.

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