Partisanship and the Enduring Effects of False Political Information

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Abstract

Much work on political persuasion maintains that people are influenced by information that they believe and not by information that they don’t. By this view, false beliefs have no power if they are known to be false. But research from political science and psychology suggests that this view requires modification: sometimes, false beliefs influence people’s attitudes even after they are understood to be false. In a trio of experiments, I demonstrate that the effect is present in people’s thinking about politics and amplified by party identification.

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In the nine months before the March 2003 invasion of Iraq, the Bush administration made several arguments to sway public opinion in favor of war. One loomed largest: Iraq had weapons of mass destruction and it was going to use them (Gellman and Pincus 2003). But contrary evidence was mounting before the invasion, and shortly afterward, it became overwhelming. In the months before the war, Iraq possessed no weapons of mass destruction. Nor was it attempting to make or acquire them (Duelfer 2004, ch. 1; see also Gellman 2004). As students of public opinion, we should wonder: if supporters of the war had been disabused in time—if, after months of believing that Hussein’s regime possessed WMD, they had learned that it did not—what would have happened? How would public support for the war have changed?

We don’t know the answer to that question. But we do know that the way in which citizens respond to political information is an empirical question with normative import. Empirically, we expect attitudes to bear some connection to facts and to change as relevant new facts come to light. And normatively, this is what many democrats think important: that citizens know truths about politics and use them to shape their views. It seems almost redundant to add that people should not be influenced by falsehoods. But while great effort has been spent to determine what people know about politics, little has been spent to determine the extent to which they “know”—i.e., believe—false factual claims about politics. Still less has been spent to examine the possibility that people are influenced by messages that they know to be false.

If anything, a nearly opposite assumption is made: people may be deceived but they will, if undeceived, change their views accordingly. This is why “ad watches” that fact-check political advertisements have become staples of campaign news (Ansolabehere and Iyengar 1995). And it is why studies of the connection between deliberation and attitudes typically involve not just deliberation but exposure to a wealth of factual information in a specific policy area (e.g., Luskin, Fishkin, and Jowell 2002). These efforts are not undertaken because truth is held to have intrinsic value. They are undertaken because learning the truth is supposed to affect attitudes. It may be difficult to correct people’s mistaken beliefs, but once that task is accomplished, the assumption is that attitudes immediately related to the beliefs will change accordingly.
Research from the intersection of social and cognitive psychology suggests that the assumption is not always borne out in practice. Even when old beliefs are debunked, the attitudes affected by those beliefs sometimes change little or not at all. That said, previous work on this subject has been almost wholly apolitical. In this article, I use real-world cases of political misinformation and a trio of experiments to show that the effects obtain in politics, too: false beliefs influence people’s political views even after they are understood to be false. The strength of the effects depends on the partisanship of the people whose views are at issue. These findings explain the prevalence of deceptive rhetoric in American politics, even in high-information contexts in which deception is likely to be exposed. From the standpoint of strategic communication, they emphasize the damage that can be done even by baseless slanders, thereby underscoring the importance of protecting one’s reputation and the difficulty of repairing it. Most of all, they raise new questions about elites’ ability to manipulate voters.

Motivated Reasoning Can Explain Partisan Differences in Reactions to Discredited False Information

Many studies show that people tend to resist arguments and evidence that run counter to their attitudes (e.g., Asch 1946; McGuire 1964; Lord, Ross, and Lepper 1979; Sniderman and Theriault 2004; Tetlock 2005). But these studies often sidestep the question of how much resistance is too much. Some resistance, at some times, is surely sensible, and in real-world situations it is often difficult to say how much attitudes should change in response to new evidence.

But one case does not seem difficult, at least at first glance. When new evidence completely discredits old information, attitudes once influenced by the information should no longer be influenced by it. They should become what they would have been if the information had never been encountered at all. In reality, though, people sometimes fall short of this standard. They accept the falsity of a claim but continue to be affected by it. “Belief perseverance” is the
name of the phenomenon. In politics, for example, it may occur if voters learn that an attack on a candidate is false but continue to think worse of him because of it.

Why should people be affected by what they know to be false? The cognitive mechanisms that constitute motivated reasoning (Kunda 1990; Taber, Lodge, and Glatthor 2001) may provide an answer. When people encounter a message about a politician or a policy that is not absolutely novel, they have an immediate affective reaction—sometimes strong, sometimes quite weak, but always immediate and largely shaped by prior political beliefs (Bargh et al. 1992; Lodge and Taber 2000). The encounter with the message also unconsciously primes related ideas in long-term memory, making them more accessible (Krosnick and Kinder 1990; Higgins 1996). It often sparks a conscious search for related memories, too (Anderson, New, and Speer 1985). And it spurs people not just to recall related memories but to use them to explain the information contained in the message (Kelley 1973; Anderson, Lepper, and Ross 1980). None of these tasks are typically undertaken in an evenhanded fashion. The search of memory, whether conscious or not, is often biased in favor of finding data that support one’s prior beliefs. And explanations of the new information are also constructed with an eye to supporting those beliefs.

These cognitive processes therefore tend to produce, highly available in memory, a pattern of evidence supporting one’s initial reaction to a new message. And even if one is later persuaded that the message is false, the explanations that he has constructed and the memories that he has recruited will still seem to justify that initial reaction. Consequently, the falsification of a message will not produce a corresponding change in one’s attitude toward the subject of the message—even if the attitude once depended on the message for its existence. The attitude quickly becomes independent of the information that engendered it.

This theory can explain why students who receive good scores on ability tests continue to think better of their abilities even when they learn that the scores were fabricated (Ross, Lepper, and Hubbard 1975; see also Lepper, Ross, and Lau 1986). And it can explain why people who view a negative advertisement about a candidate may think worse of the candidate even after learning that the ad is false. It also predicts partisan differences in reactions to negative
ads. To see this, consider Republicans and Democrats who view a false negative ad about a Democrat. Initially, the ad’s false claim may cause members of both parties to think worse of the Democrat. But Republicans are more likely to possess related negative ideas about the Democrat and his party, and chiefly for that reason, Republicans will be more likely to retrieve related negative ideas from long-term memory upon seeing the ad. Later, when the ad proves false to all, Republicans will be more likely think worse of the Democrat—not because they still believe the ad’s claim, but because the related negative ideas that it has summoned will now seem to justify their lower opinion. *Ceteris paribus*, this should be equally true for Democrats who view a false negative ad about a Republican.

**Experimental Designs**

Owing in part to a dearth of data, the political effects of belief perseverance have almost never been studied. They received their most thorough consideration in a *Journal of Politics* article by Kuklinski et al. (2000), who sought to examine the effect of new information on people who were misinformed about welfare. In their experiment, which was embedded in a telephone survey, control-group subjects were asked factual questions about welfare. Most of them far overestimated the generosity of federal welfare programs. And because subjects were randomly assigned to the control group or to the treatment groups, we can assume that treatment-group subjects, too, were misinformed about welfare policy. But interviewers told treatment-group subjects a series of facts about welfare—that was the treatment. Those facts should have corrected their misconceptions about welfare. Later in the experiment, though, all subjects were asked to state their preferences over welfare policy—and the treatment appeared to make no difference at all. Tentatively, the authors concluded that they had discovered a case of belief

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1 Kuklinski et al. (2000) report two similar experiments. This is a description of the first one, to which the bulk of their paper is devoted.
perseverance: their subjects “absorb[ed] the facts” but nevertheless “failed to change their preferences accordingly” (Kuklinski et al. 2000, 802-803).

That may be exactly what happened. But as the authors acknowledge, it is also possible that the facts were not “absorbed” at all. They might have been rejected outright by the subjects. Consider the predicament of the typical subject in their study: without advance notice, he receives a phone call from a stranger who purports to be calling from a university but proceeds to pelt him with factual claims about welfare—claims that contradict his prior beliefs. What compelling reason does he have to accept these new claims? Probably none. It may be that Kuklinski et al. uncovered a case of belief perseverance, but it may also be that their subjects rejected the information they heard over the phone, attributing it to the partisan motivations of a misinformed or disingenuous interviewer. In this experiment, there is no decisive way to distinguish between the two possibilities.

A third possibility also cannot be distinguished from the others. It may be that subjects believed all the new information provided by the interviewer, considered it thoroughly, and deemed it irrelevant to their attitudes about welfare or insufficient reason to change those attitudes. Consider the facts that were given to treated subjects: in addition to learning the average amount of money given annually to welfare families, they learned the percentages of families on welfare, of welfare families that were African-American, of mothers on welfare for more than eight years, of welfare mothers who had less than a high school education, and of the federal budget devoted to welfare. After receiving these facts, treated subjects reported their attitudes toward proposed welfare cuts. Should we be surprised that their attitudes did not differ from those of control-group subjects? Perhaps not. There is no logical inconsistency between learning the facts about welfare and continuing to favor cuts—even if one recognizes that he has previously overestimated the generosity of welfare programs. This possibility, too, makes it difficult to determine whether belief perseverance was at work.²

² A separate experiment in Kuklinski et al. (2000) uses a different design that makes this possibility a less likely explanation for the weak effect of the treatment.
Mindful of these difficulties, psychologists embrace different experimental designs to test for belief perseverance. Typically, they begin by deceiving subjects about their ability to perform a task. The studies in Ross, Lepper, and Hubbard (1975) are instructive: at the beginning, subjects were given pairs of suicide notes and were asked, for each pair, to judge which was authentic and which was fake. After each judgment, subjects were told that they were correct or incorrect. But toward the end of the experiment, they learned that this feedback was simply made-up. They had been randomly assigned to hear feedback that was mostly positive or mostly negative. The feedback bore no relation to their actual performance at the task. The experimenter emphasized this point. The subjects affirmed that they understood. Yet, when asked how well they performed at the task and how well they would do it in the future, subjects in the “positive feedback” condition gave answers quite different from those in the “negative feedback” condition. This seems to be a case of perseverance: subjects knew that they had received false information but continued to be affected by it.

Consider the two advantages of this design. First, the discrediting of old information is probably much more believable here than in the welfare experiment. In part, this is because it seeks not to destroy a prior belief that may be deeply felt (e.g., belief in the unfairness of welfare policy), but only to convince subjects that a message they heard earlier in the experiment isn’t credible. And in part, it is because the discrediting is supplied by the same person who earlier supplied the false information. When the experimenter tells the subject that he has been lying, the subject cannot easily dismiss him as uninformed.

Second, belief in the discrediting information necessarily entails disbelief in the feedback that was provided earlier. To the extent that subjects accept the discrediting, they are logically committed to believing that the earlier feedback about their performance on the test provides no information about their abilities. (Compare this to the welfare experiment, in which subjects might have believed all that they were told by the interviewer and still—quite logically—not have changed their attitudes toward welfare at all.)
These advantages permit a simple test of perseverance. At the end of the experiment, subjects in the “positive feedback” group and those in the “negative feedback” group should not possess different beliefs about their abilities. The extent to which they do possess different beliefs—easily gauged with a $t$-test—is the extent of perseverance.

I use an experimental design that borrows elements from both of the designs already described. Following Kuklinski et al., the experiments examine political topics, and the first two do not rely on overt deception by the experimenter. Following the design of Ross, Lepper, and Hubbard, I provide false information during the experiment and then discredit it, instead of trying to disabuse subjects of beliefs that they had before the experiment began. The last experiment presented here follows their design closely, but the signal difference in the first two is that false information is neither provided nor discredited by the experimenter. Instead, I use political news media and advertisements to provide and discredit false political information.

Two hypotheses are at stake. The first is that people are affected by false political information even after it is shown to be false. The second, the motivation hypothesis, is that partisanship helps to sustain the effects of false information.

If the motivated reasoning theory described in the previous section is correct, all subjects should not be affected in the same way by a false attack on a candidate or a policy. All may initially think worse of the candidate or policy because of the attack, but the extent to which the attack prompts the retrieval of related negative beliefs should depend on party identification. Democrats who hear attacks on a Republican candidate or policy should be more likely to recall related negative beliefs, and thus be relatively unmoved when they learn that the attacks were not credible. On the other hand, Republicans should be less likely to recall related negative beliefs, and thus more likely to recover when they learn that the attacks are not credible.
Experiment 1: Newsweek and the Abuse of Prisoners at Guantánamo Bay

The United States maintains a naval base at Guantánamo Bay, Cuba. On May 1, 2005, Newsweek published a two-paragraph article detailing abuses by U.S. interrogators of Islamic prisoners held at the base.\(^3\) Six words in the article made the charge that interrogators “flushed a Qur’an down a toilet” (Isikoff and Barry 2005). Eleven days later, the claim was publicized in Afghanistan and Pakistan by Arab media outlets. It triggered several days of anti-American rioting in which at least 15 people died and 60 were injured (Kurtz 2005).

Pentagon spokesmen denied the charge. And on May 16, Newsweek editor Mark Whitaker issued a retraction: “Based on what we now know, we are retracting our original story that an internal military investigation had uncovered Qur’an abuse at Guantánamo Bay.”

Participants, Design, and Procedure

Three hundred and ten adult American citizens were recruited from a pool of participants maintained by a large private university.\(^4\) At the beginning of the experiment, all were informed that they would soon read an article about the naval base. They also read two passages containing additional information that they were told they might need to understand the article. The first was a five-sentence description of the naval base. The second was a description of the Qur’an:

The Qur’an (sometimes spelled Koran) is the sacred book of Islam. According to Islamic belief, the Qur’an was revealed by God to the Prophet Muhammad. Many Muslims believe that it is the literal word of God.

The passage was designed to approximate the information about the Qur’an that was provided in news articles about the controversy. (Most provided at least as much information, and some provided more: e.g., Williams and Khan 2005 and Savage 2005.) It was also designed to increase

\(^3\) Although the issue was published on May 1, it was dated May 9 (Kurtz 2005).

\(^4\) This was not a sample of undergraduates. Participants hailed from 43 states. The median age was 32. 51% had graduated from college. Additional details about the sample are available from the author.
comparability between the control and treatment groups. As we shall see, the treatment probably primed treated subjects to think of the Qur’an when answering questions that appeared later in the experiment; this passage was administered to all to ensure that control-group subjects were also primed.

Each subject was then randomly assigned to read a version of the *Newsweek* article. Control-group subjects read a version identical to the one that appeared in print, save for the omission of the claim about the Qur’an and one other sentence. Treatment-group subjects read a version that included the claim about the Qur’an but was otherwise identical to the control-group version. (See Appendix B.) One hundred and fifty-seven subjects were assigned to the control group while 153 were assigned to the treatment. The success of randomization was gauged by testing it against the subjects’ self-reported party identification; using a chi-square test, the null hypothesis of independence cannot be rejected ($p = .54$).

After completing a series of unrelated tasks lasting between 8 and 16 minutes, treatment-group subjects learned that *Newsweek* had retracted part of the article. They then read the full text of the *Newsweek* retraction. All but 13 reported understanding the correction; those 13 were excluded from the analyses that follow.

All subjects were asked to state whether they approved, disapproved, or neither approved nor disapproved of the treatment of detainees at Guantánamo Bay. Control subjects were asked after reading the article; treatment subjects, after reading the article and after the discrediting. At the end of the experiment, all subjects were also asked to state whether they thought Congress should investigate the reported abuse of prisoners (to which the possible responses were “Yes,” “No,” and “Unsure”) and their party identification (“Democrat,” “Republican,” “Neither”).

After finishing the experiment, each subject was told about its purpose. Belief perseverance was described, and subjects were given links to the actual *Newsweek* article (including the retraction) and to a *Newsweek* article about the controversy.
Results

What should treated subjects think of the treatment of detainees? After they read their version of the article, but before they read the retraction, their attitudes differ from those of control-group subjects. (51% of control-group subjects disapprove of the treatment of detainees. 60% of treatment-group subjects do the same. The difference is significant at $p = .08$.) This is unsurprising, for they have read a different article.

But consider their attitudes after they have read the retraction. That retraction applies solely to the claim that a military investigation uncovered abuse of the Qur’an by U.S. interrogators. That is, it applies to the only information received by the treatment group but not by the control group. Consequently, the attitudes of treated subjects should revert to their pre-treatment state, which is to say that they should match the distribution of attitudes in the control group. And so they will if there is no belief perseverance. In other words, the test of perseverance is whether the treatment, which consists of the provision of false information and the subsequent discrediting of that information, affects attitudes. If it does, there is a perseverance effect. If it doesn’t, there isn’t.

The top panel of Figure 1 presents the overall results. It suggests no perseverance. At the end of the experiment, 51% of control-group subjects disapproved of the treatment of detainees, as did 51% of treated subjects. But the bottom panels of Figure 1 reveal a strong perseverance effect conditional on partisanship. In the control condition, 56% of Democratic subjects disapproved of the treatment of detainees. After treated Democrats read the retraction, 68% of them disapproved ($p = .05$). On this issue, treated Democrats exhibited a perseverance effect.

Treated Republicans did, too—in the opposite direction. After reading the claim about the Qur’an and later reading about its retraction, only 8% of them disapproved. Control-group Republicans, who never read the claim about the Qur’an at all, had a 36% disapproval rate—more than four times as high, and significant at $p = .01$. In other words, reading that U.S. interrogators flushed a Qur’an down a toilet made Republican subjects less disapproving of the treatment of
**Figure 1: Partisanship Moderates Perseverance in the Guantánamo Bay Experiment.** Each panel depicts estimated densities for mean percentages of subjects disapproving of the treatment of detainees. Dashed lines represent control-group subjects; solid lines represent treated subjects. Light grey areas are 95% highest density regions for the treatment group. Dark grey areas are 50% HDRs for the treatment group. In all cases, the treatment consists of reading that U.S. interrogators had flushed a Qur’an down a toilet and later learning about the retraction of that claim.

The lines in the top panel overlap almost perfectly, suggesting that there was no perseverance effect. But the bottom panels reveal that the effect was large. Treated Democrats and Republicans were moved in opposite directions by the treatment. Both groups differed from their control-group counterparts, even though the retraction deprived them of any informational basis for doing so. (The difference between control- and treatment-group attitudes is significant at $p = .05$ for Democrats, .01 for Republicans.)

The reason for the apparent absence of perseverance in the top panel of Figure 1 is now clear. We see no overall effect because the treatment caused *two* effects—one for Republicans, another for Democrats—in opposite directions. Averaged together, they cancel each other out.\(^5\)

\(^5\) An alternative inference is that the claim about the Qur’an had little effect on treatment-group Republicans but that its retraction caused them to differ from their control-group counterparts. But this seems unlikely *a priori*, and the data refute it. Treatment-group Republicans’ attitudes were first measured just after they read the claim about the Qur’an. Those measurements show that the claim about the Qur’an made them much more approving of detainee treatment ($p = .001$). The retraction, by contrast, had a small effect (and $p = .14$).

\(^6\) Treated subjects’ approval ratings were first measured just after they read their version of the *Newsweek* article. It is not known whether this made their later ratings of the candidate “sticky,” i.e., less responsive to the retraction of the claim about the Qur’an. Note, though, that a long series of distractor tasks intervened between the two approval measurements. To the extent that “stickiness” was at work, its effect should have been weakened by the distractors. Moreover, stickiness-based accounts of perseverance cannot explain the results of other studies in which the question of interest was asked only once of each subject. Nor, for that matter, can they explain the responses to the Congressional hearings question in this experiment, which are described below. (That question, too, was asked only once of each subject.)
Table 1: Belief Perseverance in the Guantánamo Bay Experiment. Cell entries in the first two columns are parameter estimates and standard errors from ordered logistic regressions. The dependent variable is approval of the treatment of detainees, ranging from “disapprove” to “neither disapprove nor approve” to “approve.” The treatment, described in the text, consists of reading about the abuse of the Qur’an at Guantánamo Bay and then learning about the retraction of that claim. Interesting coefficients statistically significant at 95% using a one-sided test for $H_A > 0$ are denoted by *; interesting coefficients significant at 90% are denoted by +.

The results suggest substantial belief perseverance among Democrats and Republicans: treated partisans differed from their control-group counterparts, even though the treatment gave them no informational basis for doing so. The difference between the treatment effects for the two groups is also significant, indicating that partisanship moderated the extent of perseverance.

These findings are affirmed by the ordered logistic regression models reported in Table 1, which also account for the trichotomous nature of the approval rating. Both models—one for Democratic subjects, the other for Republican subjects—simply posit that the approval rating is a function of the treatment. Estimates from the models reveal that the marginal effect of the treatment is significant for Democrats ($p = .06$) and Republicans ($p < .001$) alike. For Democrats, the treatment increased the estimated probability of disapproving by 11%; the probabilities of remaining neutral and approving declined by 7% and 4%. For Republicans, the treatment caused estimated probabilities of disapproving or taking a neutral stance to fall by 22% and 6%; the probability of approving rose 28%.

These results show that the treatment substantially affected both Democrats and Republicans. But what of the hypothesis that the treatment affected them in different ways—that partisanship moderated belief perseverance? Evidence in its favor is provided by the third column of Table 1, which shows that the difference between the treatment effects on the logit scale is
nearly four times its standard error ($p < .001$). The difference is large, too: its size can be gauged by looking at the bottom panels of Figure 1, which show that the gap in disapproval rates between Democrats and Republicans grew from 20% in the control condition to 60% in the treatment condition.

Additional perseverance can be found in subjects’ end-of-experiment attitudes toward Congressional hearings about the abuse of detainees. In the control group, 69% of subjects called for hearings. In the treatment group, 80% did. The finding was significant overall ($p = .02$) and for Democrats (control 78%, treatment 86%, $p = .09$) but not for Republicans (control 65%, treatment 69%, $p = .35$).

Both hypotheses, then, were borne out. Even after learning that Newsweek disavowed its own claim, professing to understand that disavowal and seeming to accept it, subjects who were exposed to the claim continued to be affected by it. And the direction in which they were affected depended on their party identification.

But consider an important qualification. Although Newsweek’s retraction is clear and total, it is not inconceivable that subjects might have viewed it as ambivalent. This is especially true in light of widely publicized criticisms of Newsweek from the Pentagon and the White House. If subjects were aware of those criticisms, they might have concluded that the magazine was pressured into issuing an insincere retraction. That would prevent us from inferring either that subjects were affected by information that they knew to be false or that they were unreasonably resisting the discrediting. What happens when subjects cannot reasonably draw such a conclusion?

**Experiment 2: John Roberts and the Abortion Clinic Bombing**

On August 8, 2005, NARAL Pro-Choice America (formerly the National Abortion Rights Action League) released a television advertisement that accused Supreme Court nominee John Roberts
of “supporting violent fringe groups and a convicted clinic bomber.” The ad begin by depicting an abortion clinic in ruins, segued to injured women in wheelchairs, and ended with a voiceover admonition that “America can’t afford a Justice whose ideology leads him to excuse violence against other Americans” (NARAL Pro-Choice America 2005).

Opponents and supporters of abortion rights were quick to criticize the ad as “blatantly untrue” and “deceptive.” They noted that Bray v. Alexandria, the case in which Roberts was accused of supporting violent anti-abortion protesters, was not about clinic bombing. (It was about nonviolent blockades of clinics.) The only connection to bombing seemed tenuous: six years before the Supreme Court heard Bray, one of its defendants had been convicted for his role in the bombing of several clinics. As Solicitor General, Roberts argued the Bush administration’s position before the Court, which, in a 6-3 decision, agreed with much of that position (Barge 2005, Keenan 2005).

After little more than 24 hours, NARAL Pro-Choice America pulled the ad from the air. By then, it had played 200 times—almost entirely in the small television markets of Maine and Rhode Island, the home states of senators whom the organization hoped to sway (Nielsen Media Research 2005).

**Participants, Design, and Procedure**

Four hundred and fifty adult American citizens were recruited from two pools of participants—one maintained by Survey Sampling International, the other by a large private university—to participate in a “study of social attitudes.” At the beginning of the experiment, all read a passage explaining what the Supreme Court is, that John Roberts had been nominated to fill a vacancy, that Roberts had argued the Bush administration’s position in Bray, and that six Justices had sided with him. At this point, subjects were randomly assigned to the control or treatment group.

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7 Most of the questions in the study were about issues unrelated to the experiment. Subjects were also told that “no knowledge of politics or interest in politics is necessary,” and the open-ended comments that many made at the end of the experiment provide no indication that they were unusually knowledgeable or interested.
The success of randomization was gauged by testing it against the subjects’ self-reported party identification; using a chi-square test, the null hypothesis of independence cannot be rejected ($p = .49$).

Treatment-group subjects were then asked to read a transcript of the ad. After answering a long series of unrelated questions, they were informed that NARAL had withdrawn the ad. They were also told about criticisms of the ad by two people: Walter Dellinger, identified as “an ally of the group that aired the ad and an important attorney in the Clinton administration,” and Arlen Specter, identified as “a Republican senator and supporter of abortion rights.” These criticisms introduced no new material information about the ad. Instead, they repeated the information that had been provided at the beginning of the experiment.

At the end of the experiment, subjects were debriefed as in the previous experiment and given links to a Factcheck.org criticism of the NARAL ad (Barge 2005) and to the NARAL rebuttal of that criticism (Keenan 2005).

All subjects were asked to report their attitude toward Roberts “as a Supreme Court Justice” on a seven-point scale ranging from “disapprove strongly” to “neither approve nor disapprove” to “approve strongly.” Control-group subjects were asked these questions after reading the introductory text; treatment-group subjects were asked after reading the transcript of the ad and after the discrediting. Subjects were also asked to report how sure they were of their position on a five-point scale ranging from “not at all sure” to “extremely sure” and to report their party identification on a three-category scale as in the previous experiment.

**Results**

Immediately after reading the ad transcript, 50% of treatment-group subjects disapproved of Roberts, while only 33% of control-group subjects did the same. The top panel of Figure 2 reveals that the discrediting reduced the difference between the groups but did not eliminate it: after receiving the discrediting, 42% of treated subjects disapproved. (The difference from the 33% in the control group is significant at $p = .01$.) Even after reading of the ad’s withdrawal and
a bipartisan evisceration of its insinuations, treated subjects were more disapproving of Roberts than their control-group counterparts.

And just as in the previous experiment, the effect was moderated by partisanship. John Roberts was a Republican, a prominent official in a previous Republican administration, and a Bush nominee. The motivation hypothesis led me to expect that Democrats would be affected by the advertisement even after it was comprehensively refuted. And it did: 56% of control-group Democrats disapproved of Roberts, but among treated Democrats, the disapproval rate rose to 72% ($p = .01$).

Republicans, by contrast, have no obvious motivation to think poorly of John Roberts. Quite the opposite. The motivation hypothesis therefore predicts only a weak perseverance effect among Republicans after they understand that the advertisement is false. And that is what we find: although the ad initially doubled Republican disapproval rates from 11% to 22% ($p = .01$),

**Figure 2: Partisanship Moderates Perseverance in the John Roberts Experiment.** Each panel depicts estimated densities for mean percentages of subjects disapproving of John Roberts. Dashed lines represent control-group subjects; solid lines represent treated subjects. Light grey areas are 95% highest density regions for the treatment group. Dark grey areas are 50% HDRs for the treatment group. In all cases, the treatment consists of reading the transcript of an advertisement critical of John Roberts and later learning about both the ad’s retraction and sharp bipartisan criticism of its factual merits.

The top panel presents results from all participants and suggests that there was a substantial perseverance effect. But the bottom panels reveal that aggregating over parties masks substantial differences between partisans. Among Democratic participants, the perseverance effect was very large (control = 56%, treatment = 72%, $p = .01$). Among Republicans, it was quite weak (control = 11%, treatment = 14%, $p = .25$).
it had only a weak effect after it was discredited (control disapproval rate = 11%, treatment disapproval rate = 14%, $p = .25$).

As in the previous experiment, the moderating effect of partisanship is affirmed by ordered logistic regression models in which approval ratings are regressed on the treatment. Estimates from the model, reported in Table 2, account for the polychotomous nature of the approval rating and thereby offer finer tests of perseverance effects. They reveal that the difference between the Republican and Democratic treatment effects is significant ($p = .06$) and quite substantial. The magnitude of the difference is indicated by the bottom panels of Figure 2, in which the gap between Democratic and Republican disapproval rates grows from 45% in the control condition to 58% in the treatment condition.

A similar phenomenon is at work in subjects’ reports of the confidence they have in their ratings of Roberts. The treatment provides no factual basis for a different level of confidence—it provides, in total, neither more nor less credible information than the control-group subjects receive. Still, we might expect that the experience of having apparent information about Roberts given and then discredited would shake the confidence that treated subjects repose in their assessment of Roberts. In reality, it has the opposite effect: treated subjects are more sure of their attitude. There are no apparent partisan differences (nor any obvious reason to expect them), but there is a plain treatment effect. 75% of treated subjects were “very sure” or “extremely sure” of their rating of Roberts, against only 64% of control-group subjects ($p = .02$). This is not quite a belief perseverance effect: at issue is not a belief but the confidence with which the belief is held. But it is disconcerting, especially when coupled with evidence about perseverance in subjects’ ratings of Roberts. We have already seen that treated subjects differ from the control group even when the advertisement’s insinuations are discredited. To the extent that this is disturbing, the finding might be ameliorated by the finding that treated subjects were at least less confident of their more disapproving views. But they are not—they are more confident of their views, rather than less.
Table 2: Belief Perseverance in the John Roberts Experiment. Cell entries in the first two columns are parameter estimates and standard errors from ordered logistic regressions. The dependent variable is approval of the John Roberts as a Supreme Court Justice, a seven-category variable ranging from “disapprove” to “neither disapprove nor approve” to “approve.” The treatment, described in the text, consists of reading the transcript of a television advertisement critical of John Roberts and later learning about the ad’s retraction following sharp bipartisan criticism of its factual merits. Interesting coefficients statistically significant at 95% using a one-sided test for $H_A > 0$ are denoted by *; interesting coefficients significant at 90% are denoted by +.

The results suggest substantial belief perseverance among Democrats: treated Democrats disapproved of Roberts more than their control-group counterparts, even though the treatment left them no informational basis for doing so. The difference between the treatment effects for Democrats and Republicans is also substantial, indicating that partisanship moderated the extent of perseverance.

As in the Guantánamo Bay study, it appears that political information affected subjects’ attitudes even after it was discredited. Alternatively, it is possible that treatment subjects differed from their control-group subjects because they alone learned that Dellinger and Specter disliked the ad. But this seems unlikely. Outside the world of political elites, Dellinger and Specter are almost entirely unknown, and their quotations from Dellinger and Specter only repeated information that was given to all subjects at the beginning of the experiment.8

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8 Subjects were not asked whether they recognized either man’s name. But consider data from the 2000 ANES, in which respondents were asked to identify four much more famous political figures: Tony Blair, Trent Lott, William Rehnquist, and Janet Reno. Lott was the least well-known of the four; only 9% could name his office, which was
No, if perseverance in this experiment is an artifact, the problem is likely the sharpness of the discrediting. It was not at all ambiguous. Still, it was not as decisive as the discreditings in the early belief perseverance experiments, in which the very experimenters who provided new information to subjects later explained that they had simply fabricated the information (e.g., Ross, Lepper, and Hubbard 1975). Despite the discrediting in this experiment, it is possible that a participant might have imagined that the ad was truthful and that NARAL was pressured into withdrawing a truthful but unpopular ad from the air. With that in mind, a third experiment was designed to confront treatment subjects with a wholly unequivocal discrediting.

**Experiment 3: Candidate Evaluation in a U.S. Senate Election**

Three hundred and thirty-four adult American citizens were recruited from two pools of participants—one maintained by Survey Sampling International, the other by a large private university. Upon arriving at the experiment’s web site, subjects were told that much of the study was about “voter guides” published by influential newspapers before major elections. They were led to believe that they would read about a randomly selected candidate in a 2004 election for the U.S. Senate. In fact, all were presented with a voter guide about a fictional Republican candidate for an open Senate seat in Wyoming. It contained biographical information and the candidate’s stances on political issues. It was formatted to resemble an authentic guide, and the information in it was adapted (and occasionally taken verbatim) from the positions of real candidates that were published in real voter guides.9

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9 The majority of voter guides in American newspapers follow the template of the Voter Guide Toolkit published by e-ThePeople (http://www.vgt2004.org). To ensure realism, the guides used in this experiment followed the same template.
Control-group subjects received the short version of the guide, which presented the candidate’s fairly centrist views on taxes and economic growth, the Patriot Act, immigration, and payment of dues to the United Nations. Treatment-group subjects received the long version, which added information about other stances: a call to eliminate the Department of Education and most federal involvement in education, and a call to create a market in pollution complete with commercial trade in “pollution credits.” These stances, typically associated with dedicated conservatives, were included because they are relatively unpopular even among Republicans. But they are by no means unheard-of: the former stance was part of the national Republican Party platform until 2000 (American Presidency Project 2006), and the latter is a hallmark of the Bush Administration’s “Clear Skies” initiative (White House 2002).

One hundred and seventy-one subjects were assigned to the control group; 163 were assigned to the treatment. The success of randomization was gauged by testing it against the subjects’ self-reported party identification; using a chi-square test, the null hypothesis of independence cannot be rejected ($p = .52$).

After completing a series of unrelated tasks, treatment-group subjects were debriefed about the information in the voter guide. They were told that the study was actually about people’s responses to different politicians’ stances on education and the environment and that it had therefore been necessary to mislead subjects by inserting fabricated stances on those issues into an otherwise accurate guide. They were also told that the experimenters did not really know the candidate’s stances on those issues. (See Appendix D.) All subjects were asked whether they understood this debriefing. Sixteen reported that they did not, and their answers have been excluded from the analyses that follow.

All subjects were asked to state whether they approved, disapproved, or neither disapproved nor approved of the candidate. Control-group subjects were asked after reading the voter guide; treated subjects, after reading the guide and after the debriefing. In addition, all subjects answered the party ID and attitude strength questions described in the previous section.
**Figure 3: Partisanship Moderates Perseverance in the Candidate Evaluation Experiment.** Each panel depicts estimated densities for mean percentages of subjects disapproving of the candidate. Dashed lines represent control-group subjects; solid lines represent treated subjects. Light grey areas are 95% highest density regions for the treatment group. Dark grey areas are 50% HDRs for the treatment group. In all cases, the treatment consists of receiving information about the candidate’s stances on education and the environment and later learning that those stances were fabricated by the experimenter.

The top panel suggests a moderate overall perseverance effect. But in the by-now-familiar pattern, the overall results obscure sharp partisan differences. As in the previous experiment, the bottom panels reveal that the effect is extremely strong among Democrats but modest for Republicans.

**Results**

Consider the predicament of treatment-group subjects just after they read the voter guide. They alone have been exposed to the candidate’s very unpopular stances on education and pollution credit trading. We might expect that they would think less of the candidate at this point in the experiment. And they did: 52% of treatment-group subjects initially disapproved of the candidate, against only 19% of control-group subjects.

But consider now their attitudes after they read the retraction. The retraction was unambiguous and it could not have come from a more authoritative source. If subjects believed the false information in the voter guide, they certainly should have believed the information in the discrediting to at least the same degree. And it is highly likely that they did. Even so, the top panel of Figure 3 reveals a sharp difference between the control and treatment groups. At the end of the experiment, 31% of treated subjects disapproved of the candidate, against only
19% of control-group subjects ($p = .01$). Treated subjects continued to be affected by the false information.

Democrats and Republicans alike thought worse of the candidate immediately after reading his stances on education and the environment. But only Democrats had an obvious prior motivation to think poorly of the candidate. The motivation hypothesis therefore leads us to expect that their attitudes toward the candidate were less affected by the discrediting. And that is what the bottom panels of Figure 3 reveal. At the end of the experiment, Republicans were moderately affected by the treatment: the disapproval rate of 12% among control-group Republicans rose to 20% among their treatment-group counterparts ($p = .16$). But Democratic subjects exhibited a perseverance effect more than twice as strong. In the control condition, 20% of Democratic subjects disapproved of the candidate; in the treated condition, 42% did ($p < .01$). This is just the pattern of results that we should expect if the motivation hypothesis is true.

### Table 3: Belief Perseverance in the Candidate Evaluation Experiment.

<table>
<thead>
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<td>-.60 .45</td>
<td>.98* .55</td>
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<tr>
<td>$\tau_1$</td>
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<td>$\tau_2$</td>
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Cell entries in the first two columns are parameter estimates and standard errors from ordered logistic regressions. The dependent variable is approval of the U.S. Senate candidate, ranging from “disapprove” to “neither disapprove nor approve” to “approve.” The treatment, described in the text, consists of reading a voter guide about the candidate and later learning that some information in the guide was false. Interesting coefficients statistically significant at 95% using a one-sided test for $H_A > 0$ are denoted by *.

As in the previous experiment, the results suggest substantial belief perseverance among Democrats. Treated Democrats disapproved of Roberts more than their control-group counterparts, even though the treatment left them no informational basis for doing so. The difference between the treatment effects for Democrats and Republicans is also substantial, indicating that partisanship moderated the extent of perseverance.
As in the previous experiments, the results are affirmed by ordered logistic regression models in which the approval rating is regressed on the treatment. The model estimates, reported in Table 3, show that partisanship moderates belief perseverance even when the trichotomous nature of the dependent variable is taken into account. For Republicans, the treatment was modest and shy of statistical significance by conventional standards (albeit slightly: $p = .11$). It lowered their estimated probability of approval by 15%, increasing their estimated probability of disapproval by 8% and of taking a neutral stance by 7%. For Democrats, though, the marginal effect of the treatment was substantial (and $p < .01$). The treatment decreased their estimated probability of approval by 37%, increasing the probabilities of approving and taking a neutral stance by 31% and 6%.

The third column of Table 3 confirms that the difference between the treatment’s effects on Republicans and Democrats was unlikely to occur by chance ($p = .04$). The magnitude of the difference can be judged by looking to Figure 3, in which the gap between percentages of Democratic and Republican subjects disapproving of the candidate expands from 31% in the control group to 42% in the treatment group.

As in the John Roberts experiment, something like belief perseverance was at work in the confidence that subjects repose in their ratings of the candidate. Once again, the treatment provided no obvious basis for a greater level of confidence in the measured attitude. But once again, treated subjects were more confident than control-group subjects at the end of the experiment: 26% of control-group subjects reported that they were “very sure” or “extremely sure” of their attitude toward the candidate, against 32% of the treated subjects. At $p = .12$, the difference is quite unlikely to have occurred by chance.

Pause a moment to consider these findings. Treated subjects knew nothing about the candidate before the experiment began. They apparently believed, and certainly were affected by, the candidate’s fake stances on two issues. They then read an unambiguous and unequivocal retraction establishing the falsity of those stances. But they either put more stock in the false
claims than in the retraction or—more likely—accepted the retraction but continued to be affected by the false claims.

Is there a benign explanation for these results? They cannot be explained by reference to what subjects might have known prior to entering the experiment, for it is not possible that they had prior information about the candidate or the retraction. (In this respect, the current experiment is distinct from the previous two.) The retraction was not muddled; if anything, it was less ambiguous than the false information that had previously been provided. Its source was not obviously partisan and, more importantly, it had been believed at an earlier point in the experiment. In short, it seems difficult to explain the results by reference to anything like a reasonable dismissal of the discrediting.

**Discussion**

It never pays for our government to give false impressions to the American public with a view to enlisting its support for short-term purposes, because this always revenges itself later when it becomes necessary to overcome the wrong impression one has created (Kennan 1997, 38).

Kennan’s conclusion is beyond the purview of this article. But his premise—that it is difficult in politics to defeat wrong impressions—is not. The experiments described here suggest that his premise is correct. We already knew that prior beliefs can be difficult to change under ordinary circumstances (McGuire 1964; Abelson 1986). What these experiments add is an understanding that even under very favorable circumstances—indeed, even when a person is, in all likelihood, made to understand that his belief is false—his related attitudes will not always change accordingly. False beliefs are not just hard to kill. They have an afterlife, too.

These experiments also show, for the first time, that the extent of belief perseverance depends on differences between individuals. In doing so, they offer strong support for the idea that motivated reasoning underpins perseverance. Other explanations—e.g., those rooted in online processing, or in the anchoring-and-adjustment heuristic—can account for the finding
of perseverance. But no theory that fails to account for motivation can explain the partisan
differences that are manifest here.

One virtue of experiments is that they permit decisive challenges to beliefs that are
relatively weak. The beliefs are weak because they have only been instilled at an earlier point in
the experiment itself. The challenges are strong because they come from authoritative sources
who cannot easily be dismissed as biased or uninformed. But in ordinary political life, beliefs are
often stronger than the ones instilled in these experiments. And challenges to those beliefs are
weaker because they come from sources who can be dismissed as biased or uninformed. In this
sense, the experiments described here produce conservative tests of the perseverance hypotheses.
If there are any political situations in which we should not observe belief perseverance, they are
the situations created by the experiments described here.

There is a second reason why perseverance in the outside world is likely to be stronger
than these results suggest. Even when the political world does produce decisive challenges to
particular beliefs, selective exposure to congenial news sources is likely to shield people from
them (Ross, Lepper, and Hubbard 1975, 891; Taber and Lodge 2006). This view was challenged
in the past by those who argued that people do not consciously seek to reinforce their views
through selective exposure (Sears and Freedman 1967; Frey 1986). But selective exposure does
not require a reinforcement motive (Katz 1968), and there is reason to believe that heightened
sorting of the electorate (Levendusky 2006) and the splintering of the market for news into
specialized niches (Prior 2007, Chapter 4) have made it increasingly common (though see
Webster 2005).

**Benign Explanations?**

In recent years, the idea that manipulation is a serious problem for democratic polities has been
challenged on many fronts. Lupia and McCubbins (1998) argue that manipulation of voters
through deception is more difficult than commonly supposed. Druckman and Nelson (2003)
and Sniderman and Theriault (2004) argue a similar point about manipulation through issue
framing. Druckman (2004) finds the same for classical (“equivalency”) framing. Brader (2006, 190-98) cedes the potential danger of manipulation through “fear appeals” in advertisements but suggests that these dangers may be offset by advantages, e.g., the ability of such appeals to increase participation. And Lupia (2005) suggests that politicians’ ability to manipulate citizens through such appeals is probably weaker than we often imagine. To be sure, none of this work suggests that manipulation cannot occur. But all of it suggests that either the ease of manipulation or its normative importance has been overstated.

And all of it may be correct. But the results presented here may seem to tend in the opposite direction—to suggest that citizens’ views can be manipulated by false information in a way that resists correction. Is this right? Or is a more benign interpretation in order?

At a glance, one benign explanation may seem especially attractive: Republicans and Democrats in these experiments differ simply because they have different preferences. Unfortunately, this is not a cogent explanation. The evidence of perseverance lies not in the differences between Republicans and Democrats but in the differences between control-and treatment-group subjects. At the end of the experiment, treated Republicans differ from control-group Republicans, even though the two groups have, by virtue of random assignment, approximately identical preference distributions. The same, of course, is true for Democrats who participated in the experiments. Something else must explain why treated subjects differed from their control-group counterparts.

The results may also seem relatively harmless if subjects believed the false information but failed to believe the discreditings. There is no dispositive way to demonstrate that this explanation is wrong. But it seems unlikely. In open-ended comments at the end of the experiment, many subjects wrote extensively, but not one expressed doubt about a discrediting. There were no noticeable differences between the relatively educated and the relatively uneducated (however one wishes to make the distinction); to the extent that the less educated should have been more accepting, or the more educated the more suspicious, the results appear to be unaffected. Moreover, the discreditings were like the false information in three
ways—they came from the same source, were at least as unambiguous (indeed, probably more unambiguous), and were presented in the same manner—all of which makes it unlikely that the discrediting would be less trusted. And for the same reasons, subjects should—from a normative standpoint—have believed the discrediting. The appeal here is to a normative intuition: ceteris paribus, it should not be more difficult to discredit information than to make it believable in the first place. It should not require more authority, more repetition, more volume—or, for that matter, more of anything.¹⁰

The most interesting attempt at a benign interpretation starts with the mechanisms of motivated reasoning. Those mechanisms suggest, consistent with these experiments, that false negative information caused treated subjects to summon related beliefs and attitudes from long-term memory. When the false information was discredited, the retrieved memories still seemed to justify their more negative attitudes. In short, they used other, remembered information to justify their attitudes. What is wrong with bringing more information to bear, rather than less, when justifying a stance toward a politician or a policy?

Ceteris paribus, nothing is wrong with it. Priming and remembering are not inherently pernicious. Nor is the instinctive tendency to explain what we observe. But when these phenomena are activated by false information and directed by the desire to shore up our partisan convictions, something is amiss. In that case, the stock of considerations that partisans draw on to justify their attitudes is nothing like the one that would result from evenhanded contemplation. Nor is it like the one that they would have if they had never been exposed to the false information at all. False information succeeds in manipulating partisans’ views by causing them to draw on a stock of considerations different from, and more biased than, the one that they would otherwise use. And this manner of manipulation is robust because it succeeds even if the false

¹⁰ If this normative contention seems doubtful, imagine that the Ross, Lepper, and Hubbard (1975) discrediting failed. That is, imagine that subjects continued to believe their suicide-note test feedback even after they heard that it had been fabricated. Even in this case, a modified version of the discrediting might have succeeded. For example, the discrediting might have been offered by a different experimenter—one who was more attractive (Chaiken 1979) and had better posture (Tiedens and Fragale 2003). Both modifications might have made the discrediting more believable. But neither should have.
information is discredited. Slanderous advertisements, for example, may achieve their purposes even if they are quickly exposed as false. And the safeguards of political competition and a free speech marketplace may be less helpful than we imagine. Reconciling these facts with a benign explanation is not at all easy.
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## Appendix A: Sample Sizes

<table>
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**Table A1: Sample Sizes for Each Experiment.** Cell entries are numbers of subjects. Entries in the “Total” row include independents. The numbers provided here are the numbers used in the analyses. As explained in the text, a few subjects initially assigned to the treatment conditions were excluded from the analyses (and thus from the counts of treatment-group subjects in this table) because they did not understand the discrediting of the false information.
Appendix B: Guantánamo Bay Experiment

All subjects read a version of this article:

Investigators probing interrogation abuses at the U.S. detention center at Guantánamo Bay have confirmed some infractions alleged in internal FBI e-mails that surfaced late last year. Among the previously unreported cases, sources tell Newsweek: interrogators, in an attempt to rattle suspects, [flushed a Qur’an down a toilet and] led a detainee around with a collar and dog leash. An Army spokesman confirms that 10 Gitmo interrogators have already been disciplined for mistreating prisoners. (New details of abuse are also in a new book to be published this week by a former Gitmo translator.)

These findings, expected in an upcoming report by the U.S. Southern Command in Miami, could put former Gitmo commander Maj. Gen. Geoffrey Miller in the hot seat. Two months ago a more senior general, Air Force Lt. Gen. Randall Schmidt, was placed in charge of the SouthCom probe, in part, so Miller could be questioned. The FBI e-mails indicate that FBI agents quarreled repeatedly with military commanders, including Miller and his predecessor, retired Gen. Michael Dunleavy, over the military’s more aggressive techniques. “Both agreed the bureau has their way of doing business and DOD has their marching orders from the SecDef,” one e-mail stated, referring to Secretary of Defense Donald Rumsfeld. Sources familiar with the SouthCom probe say investigators didn’t find that Miller authorized abusive treatment. But given the complaints that were being raised, sources say, the report will provoke questions about whether Miller should have known what was happening—and acted to try to prevent it. An Army spokesman declined to comment.

Treatment-group subjects read the article without the text in brackets. Treatment-group subjects read the article with the text in brackets.

This version is very similar to the original Newsweek article, which can be found, with retraction, at http://www.msnbc.msn.com/id/7693014/site/newsweek/.
Appendix C: John Roberts Experiment

At the beginning of the experiment, all subjects were told:

The Supreme Court is the most powerful court in the country. At any time, it has nine members.

From 1994 through 2004, the Supreme Court had a period of unprecedented stability. The same nine justices served on it for that entire time.

But in 2005, two justices left the Supreme Court and two new ones were nominated by President Bush. One of the new nominees was John Roberts.

In 1991, John Roberts appeared before the Supreme Court to argue the Bush administration’s position in *Bray v. Alexandria*. He argued that a federal anti-discrimination law couldn’t be used against protesters blockading abortion clinics. He won the case: six justices agreed with him.

Roberts’ nomination to the Supreme Court was opposed by NARAL Pro-Choice America and several other organizations. Even so, Roberts was confirmed by the Senate in September. 22 Democrats voted against him, but 78 Senators (including another 22 Democrats) voted for him.

Treatment-group subjects then read this transcript of the NARAL Pro-Choice America ad:

**Speaking Out**

Announcer: Seven years ago, a bomb destroyed a women’s health clinic in Birmingham, Alabama.

(Text on screen: New Woman/All Women Health Clinic; January 28, 1998)

(On screen: Pictures of fire trucks. Pictures of the destroyed clinic. Picture of a hospitalized woman in a wheelchair.)

Emily Lyons: When a bomb ripped through my clinic, I almost lost my life.
Announcer: Supreme Court nominee John Roberts filed court briefs supporting violent fringe groups and a convicted clinic bomber.

(On screen: Video footage of John Roberts. Picture of a legal brief from Bray v. Alexandria.)

(Text on screen: Roberts filed court brief supporting clinic protestors)

Emily Lyons: I’m determined to stop this violence so I’m speaking out.

Announcer: Call your Senators. Tell them to oppose John Roberts. America can’t afford a Justice whose ideology leads him to excuse violence against other Americans.

Near the end of the experiment, treatment-group subjects received this information:

Recall the ad transcript you read about earlier in this survey. The ad was strongly criticized by many people, some of whom were prominent supporters of abortion rights.

Walter Dellinger, an ally of the group that aired the ad and an important attorney in the Clinton administration, called the ad “unfair and unwarranted.” He added that “it is unfair to suggest that John Roberts, in advancing a somewhat narrow interpretation of [the anti-discrimination law], was supporting ‘violent fringe groups and a convicted clinic bomber’—as unfair as it would be to suggest that the six Justices who were part of the majority in Bray joined a decision supporting violent fringe groups.”

Arlen Specter, a Republican senator and supporter of abortion rights, called the ad “blatantly untrue and unfair.”

Stung by these criticisms and many others, NARAL Pro-Choice America withdrew the ad from television.
Appendix D: Candidate Evaluation Experiment

All subjects received the following information about the candidate’s stances on issues:

**Economy.** With the costs of food, gas and health care rising, how can you make your constituents’ lives better? Two simple words: tax relief. You can’t create jobs, and you can’t create real opportunity, by taxing your way to a stronger economy. I’m a tax cutter. And I’m proud of it. I believe the people of Wyoming should be able to keep more of their hard-earned money and spend it as they see fit.

To be a tax cutter, you’ve got to have discipline on the other side. You’ve got to restrain spending. This is common sense. Everyone from Wyoming must live within a budget. When times get tough, you have to adjust your expenses to live within your income.

**Patriot Act.** Would you vote to reauthorize the Patriot Act? I understand the central role that intelligence gathering plays in law enforcement and national security. The Patriot Act increases law enforcement’s ability to collect intelligence. But it goes too far in compromising cherished civil rights for uncertain benefits. Therefore, I support the efforts of a bipartisan group of Senators who are promoting the the SAFE (Security and Freedom Ensured) Act. The SAFE Act makes appropriate amendments to the Patriot Act, such as requiring increased judicial review and higher standards of evidence before law enforcement agencies can obtain secret subpoenas to review medical, business and library records.

**Immigration.** Immigration is one of the most challenging issues we face today. I support the bipartisan initiative to establish a guest worker program for immigrants working in the U.S. I believe that legal immigration is good for our country and good for our economy.
But we must be tough on illegal immigration. The issue reaches well beyond the strain on our economy to matters of homeland security.

United Nations dues. Should the United States fully pay the membership dues recently assessed by the United Nations? Not until the U.N. undertakes a program of reform. The U.S. already pays more dues than most members of the U.N.—combined. And yet we continue to hear of corruption and rampant misuse of our funds. The misuse of the oil-for-food funds is only the latest example. We need to demand more for our money from the U.N. before we give more money.

Treatment group subjects also read about the candidate’s much less moderate stances:

Education. Decisions about our children’s education are too important to be left to federal bureaucrats. As Senator, I will work to eliminate the Department of Education, all federal intervention into education, and all federal subsidies of education, except those that support veterans. My role as a Senator will be to get decisionmaking about education out of the hands of the federal government and into the hands of families and neighborhoods.

Environment. Thirty years of competition between undue alarmism and unthinking skepticism have confused environmental issues in the minds of most Americans.

The first thing that we have to realize is that property rights and free markets are essential protectors of a clean, sustainable environment. The phenomenon was most pronounced in Eastern Europe during the heyday of the Soviet Union, but it is also discernible in America: Government is the biggest polluter and the biggest facilitator of pollution.

If we are going to preserve and redeem our environment, we must begin a commercial trade in “pollution credits”—a quantified, qualified “right to pollute.”
Pollution, properly understood, is an offense against the property rights of those whom it affects, and should be treated as an actionable tort to be adjudicated by the legal system.

At the end of the experiment, treated subjects read that the candidate’s stances on education and the environment were not really his. Instead, they had been fabricated:

At the beginning of this survey, you read a voter guide about a candidate. Not all of the information in that guide is known to be true.

We—researchers at Stanford—made up information about the candidate’s stances on education and the environment. We do not really know where he stood on those two issues. Our intention is to see how different positions on those issues affect people’s views of different candidates. To do this, we present a lot of true information from a voter guide, adding a few made-up stances on several issues.

In this case, only the candidate’s stances on education and the environment were fabricated. All of the other information in the voter guide was accurate, and was presented to you as it initially appeared in print.