

# Framing and Deliberation: How Citizens' Conversations Limit Elite Influence

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*Public opinion research demonstrates that citizens' opinions depend on elite rhetoric and interpersonal conversations. Yet, we continue to have little idea about how these two forces interact with one another. In this article, we address this issue by experimentally examining how interpersonal conversations affect (prior) elite framing effects. We find that conversations that include only common perspectives have no effect on elite framing, but conversations that include conflicting perspectives eliminate elite framing effects. We also introduce a new individual level moderator of framing effects—called “need to evaluate”—and we show that framing effects, in general, tend to be short-lived phenomena. In the end, we clarify when elites can and cannot use framing to influence public opinion and how interpersonal conversations affect this process.*

When forming political opinions, citizens often turn to others for guidance. Indeed, the last twenty years of public opinion research demonstrates that citizens base many of their opinions on what they hear from elites *and* on what they discuss with other citizens.<sup>1</sup> While some recent work recognizes the dual and often competing effects of elite rhetoric and interpersonal conversations on opinion formation,<sup>2</sup> virtually no work examines how these two forces *interact* with one another. How do interpersonal conversations affect elite influence on opinions? How does elite influence affect interpersonal conversations?

In this article, we take a step toward filling this gap by investigating how citizens' conversations affect elite influence on public opinion. We begin in the next section by discussing framing effects—one of the central means of elite influence—and how citizens' conversations

might impact elite framing. We then present an experimental test of our hypotheses. Our experiment enhances the external validity of the typical framing study by allowing participants to communicate with one another, rather than forcing them to make decisions in a social vacuum as is common in these studies. We find that, under certain conditions, citizens' conversations vitiate elite influence—elite influence via framing may not be so robust in a political world where citizens have access to alternative forms of information. As we discuss, our findings also have important implications for theoretical work on deliberation and democracy.

In addition to our focus on conversations and elite influence, we further test the robustness of framing effects by introducing a novel and fundamental individual-level moderator (i.e., need to evaluate), and by studying the longevity of the effects. We also extend the study of

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<sup>1</sup>See, for example, Iyengar and Kinder (1987), Huckfeldt and Sprague (1995), Mutz, Sniderman, and Brody (1996), Mendelberg (2002), Mutz (2002a), and Walsh (2003).

<sup>2</sup>See, for example, Berelson, Lazarsfeld, and McPhee (1954), Mendelsohn (1996), Mutz and Martin (2001), Beck et al. (2002), and McLeod, Kosicki, and McLeod (2002). These works recognize the effects of both elites and interpersonal discussions on opinions; however, unlike our study, they do not examine how conversations impact elite influence.

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framing to a new issue domain—campaign finance reform opinions (also see Grant and Rudolph 2003). In the end, we clarify when elites can and cannot use framing to influence public opinion and how interpersonal conversations affect this process.

## Framing and Deliberation

Framing effects constitute one of the primary means by which elites influence citizens' opinions; Chong describes framing as the "essence of public opinion formation" (1993, 870). A framing effect occurs when in the course of describing an issue or event, a speaker's emphasis on a subset of potentially relevant considerations causes individuals to focus on these considerations when constructing their opinions (Druckman 2001c, 226–31). For example, if a speaker describes a hate-group rally in terms of free speech, then the audience will subsequently base their opinions about the rally on free-speech considerations and, perhaps, support the right to rally. In contrast, if the speaker uses a public-safety frame, the audience will base their opinions on public-safety considerations and oppose the rally (Nelson, Clawson, and Oxley 1997).

Analysts have documented framing effects for numerous issues in various contexts (e.g., Jacoby 2000). Nearly all of this work uses surveys or laboratory experiments where individuals receive a single frame and then report their opinions, without any social interaction or access to alternative sources of information (however, see Sniderman and Theriault n.d.). Study participants thus find themselves in a social vacuum, receiving frames and reporting their opinions with no possibility to discuss the issue at hand.

We see this as a substantial limitation of previous studies, since in many political settings people have access to various forms of political information including conversations with others. Huckfeldt and Sprague explain, "[p]olitics is a social activity imbedded within structured patterns of social interaction. Political information is conveyed not only through speeches and media reports but also through a variety of informal social mechanisms—political discussions on the job or on the street. . . even casual remarks" (1987, 1197; see also, e.g., Gamson 1992, 179; Just et al. 1996; Mutz and Martin 2001; Beck et al. 2002, 61; Walsh 2003). Is elite influence via framing robust to the introduction of other common sources of political information such as interpersonal conversations? How might elite framing effects be affected by subsequent conversations between citizens?

In addressing these questions, we add to the recent trend in framing work that, in contrast to earlier research,

documents various moderators to the effects (e.g., Brewer 2001; Druckman 2001b, 2001c; Haider-Markel and Joslyn 2001; Gross and Brewer 2002; Sniderman and Theriault n.d.; however, none of this work explores the impact of interpersonal conversations). We focus on the situation where conversations *follow* an elite framing effect—that is the initial frame comes from elites. This strikes us as an important scenario since elites play a substantial role in setting the agenda for subsequent conversations between citizens.<sup>3</sup>

We derive our hypotheses from two distinct, albeit related, research programs: psychologically oriented scholarship on interpersonal conversations, and framing and theoretical work on deliberation. We begin with the former by drawing on three empirical findings. First, we build on research demonstrating that framing effects can occur via interpersonal discussions (Gamson 1992; Simon and Xenos 2000; Walsh 2001, 2003). For example, Walsh (2001, 2003) shows that people embedded in discussion networks (e.g., in voluntary associations) base various policy attitudes on their social characteristics (e.g., race, income) to a greater extent than those not in the networks. The critical point is that the frames or considerations on which people base their political opinions need not come from elites, but can in fact come from conversations with others.

Second, research on interpersonal communication shows that the composition of the discussion group affects the group's impact; of particular importance is the extent to which the group includes people with opposing views (i.e., a cross-cutting group) (see, e.g., Mutz and Martin 2001; Mutz 2002a, 2002b). For example, Mutz (2002a) finds that exposure to different viewpoints in cross-cutting groups causes individuals to have greater awareness of rationales for alternative perspectives (also see Huckfeldt, Morehouse, and Osborn n.d.).<sup>4</sup> This can result in changed attitudes or in the strengthening of existing attitudes depending on how one cognitively responds to the contrary information (Sieck and Yates 1997; Petty and Wegener 1998, 332–3). In contrast, relatively homogeneous groups lead to group *polarization* where "an initial tendency of individual group members toward a given direction is enhanced following group discussion" (Isenberg 1986, 1141; also see Paese, Bieser, and Tubbs 1993; Mendelberg 2002, 159).

Third, as mentioned, how people treat contrary information they receive from relatively cross-cutting groups

<sup>3</sup>It is beyond our scope to explore the internal dynamics of discussions such as the impact of gender.

<sup>4</sup>We generalize Mutz's (2002a) work on the composition of discussants to groups.

depends on how they cognitively respond. Sniderman and Theriault (n.d.) provide insight into how people respond in the context of framing effects. Sniderman and Theriault exposed survey respondents either to one of two frames (e.g., a free-speech or public-safety frame for a hate-group rally) or to both frames. They find a classic framing effect for participants exposed to just one frame (e.g., the free-speech frame causes increased support for the rally). However, they also find that the elite framing effect disappears among participants exposed to both frames; these individuals return to their original (unframed) opinions. This implies that relatively cross-cutting conversations, that provide people with rationales for both frames, will result in the muting of the initial elite frame. Similarly, Vinokur and Burnstein (1978) find that when two equal-sized groups with conflicting opinions interact, the groups' opinions converge toward one another (i.e., their initial conflicting opinions disappear; also see Berelson, Lazarsfeld, and McPhee 1954, 120; Cohen 1997; Huckfeldt, Morehouse, and Osborn n.d.). This is *depolarization*.

In sum, the frames on which people base their political opinions not only come from elites but also from interpersonal conversations. When these conversations include mostly common perspectives, we expect polarization—a strengthening of the initial elite frames and thus more extreme opinions. Alternatively, relatively cross-cutting conversations that include a wider variety of views will provide individuals with an understanding of alternative frames resulting in a vitiating of the initial frames, rendering them ineffectual (i.e., depolarization).

Importantly, these hypotheses echo arguments in deliberative theory that focus on the implications of citizens' conversations for democratic governance. Mendelberg explains that a "variety of recent developments, political and academic, have sparked [an] interest in democratic deliberation. . . [and while] there is no single definition [of deliberation] on which all theorists of deliberation agree. . . deliberation is expected to produce a variety of positive democratic outcomes" (2002, 151, 153). One focus concerns the consequences of deliberation among conflicting individuals. As Mill argues, "since the general or prevailing opinion on any subject is rarely or never the whole truth, it is only by the collision of adverse opinions that the remainder of the truth has any chance of being supplied" (1859, 53; also see Habermas 1989; Benhabib 1992; Kinder and Herzog 1993, 349).<sup>5</sup>

This highlights our critical distinction between cross-cutting and noncross-cutting conversations; it also raises

an intriguing question about what constitutes the "truth" supposedly produced by cross-cutting conversations. Insofar as elite framing effects suggest manipulation (e.g., Zaller 1992, 45; Farr 1993, 386; Parenti 1999; however, see Druckman 2001c), then cross-cutting conversations might attenuate the effects, reconstituting relative truth. While this mimics our cross-cutting hypothesis, it also raises a host of questions, which we will address, about the quality of framed versus unframed opinions and the role of deliberation in producing quality opinions. Indeed, as will become clear, our study has intriguing implications for the burgeoning literature on democratic deliberation.

### Individual Moderators of Framing Effects

In addition to exploring the effects of interpersonal discussion on framing, we build on prior conflicting results to clarify which individuals exhibit more susceptibility to elite framing effects (see Druckman 2001c, 241–5). We begin by noting that framing effects, like other media effects such as priming, tend to work through memory-based processes (Nelson and Willey 2001, 255). In expressing their opinions, individuals draw on the information that comes to mind, including the frames to which they were recently exposed. For example, when asked for their opinions about the Ku Klux Klan's right to rally, individuals who just received a news-story framing the rally in terms of free speech will recall and rely on the frame, thereby attaching increased importance to free-speech considerations (Nelson, Clawson, and Oxley 1997; Nelson and Willey 2001).<sup>6</sup>

This contrasts with an on-line process where individuals access and report a previously formed opinion based on a steady stream of information over-time (instead of constructing the opinion on information in memory at the time of expression). Frames could affect such an on-line process; yet, most studies, at least implicitly, presume that the frame individuals just received affects them to the extent that it does because there is not a prior on-line opinion waiting to be expressed. Attitudes formed on-line "have a great deal of inertia. . . so new pieces of information [e.g., a recent frame] have only a small impact on them. Therefore, recent news media content [e.g., a frame] would be expected to have relatively

<sup>5</sup>We thank an anonymous reviewer for reference to this quote (also see Mutz 2002a).

<sup>6</sup>Nelson, Clawson, and Oxley (1997) show that frames mainly work by causing individuals to deliberately add more weight to the considerations emphasized in the frame, and not by increasing the temporary accessibility of those considerations.

little effect. . . [if attitudes were formed on-line instead of memory-based]” (Krosnick and Brannon 1993, 965).

Based largely on the memory-based model, Nelson, Oxley, and Clawson (1997) argue that media frames will have a greater effect on *more* knowledgeable people because only these individuals can connect the considerations put forth in the frame with their overall opinions and also weigh the importance of these considerations. Making an analogous argument about media priming, Krosnick and Brannon explain that “the more knowledge one has about politics, the more quickly and easily one can make sense of a news story and the more efficiently one can store it in, and retrieve it from, an elaborate and organized mental filing system” (1993, 966; also see Miller and Krosnick 2000, 303–4). As McGraw and Ling state, “knowledge. . . facilitates the learning and use of new information. . .” (n.d., 5).

The potential problem with this theory, however, is that some studies find the opposite—that frames have a greater impact on the *less* knowledgeable (e.g., Kinder and Sanders 1990; Haider-Markel and Joslyn 2001; also Jacoby 2000, 758, finds no effect). These authors argue that less knowledgeable people possess fewer strongly held prior opinions (and frames) and thus exhibit increased susceptibility (e.g., Kinder and Sanders 1990, 90).

We believe both arguments are correct—knowledge facilitates the use of new frames, and individuals who possess prior opinions will exhibit less susceptibility to new frames. The problem with past work is that while individuals with prior opinions may be more knowledgeable, it is not the knowledge *per se* that is at work; rather, it is the existence of prior opinions based on other information that vitiates the impact of a new frame. Thus, we assert that *elite frames will exhibit a greater impact on more knowledgeable individuals*, and a smaller impact on individuals more likely to have prior opinions. We suspect that previous conflicting results come from a failure to control for both these moderators (Krosnick and Brannon 1993).

How can we measure a tendency to have prior opinions? We have a strong theoretical basis that the construct “need to evaluate” (NE) captures this process (Jarvis and Petty 1996). High-NE individuals “more chronically [evaluate] various aspects of their lives and environments” (Bizer et al. 2000, 7). Relative to low-NE processors, high-NE processors form more opinions and base their opinions on a steady flow of information over time rather than recent salient information (Bizer et al. 2000, 21). This is analogous to the aforementioned discussion of on-line processing. Tormala and Petty explain that low-NE individuals “are relatively more dependent on the information they can recall at the time the judgment is required [e.g., information just seen in a frame]” (2001, 1609).

In short, we hypothesize that, regardless of political-knowledge levels, high-NE individuals will be more likely to possess prior opinions (and frames) based on information over time. As a result, *high-NE individuals will be less affected by new, recent information from elite frames*. By incorporating NE as an individual-level moderator, we not only expect to provide clarity to the framing literature, but we also will demonstrate the importance of a nonpolitical construct to political processing (see Bizer et al. 2000). Additionally, we introduce what we suspect to be a fundamental but never before directly examined moderator of political communication.

## The Longevity of Framing Effects

A final issue concerns the longevity of framing effects. Many studies demonstrate that elite frames can have substantial immediate effects on opinions; however, we have no idea how long these effects last (Kuklinski et al. 2000, 811).<sup>7</sup> Do these effects reflect anything more than temporary changes in reported opinions? Overall, understanding individual-level moderators and the longevity of framing effects will further inform us about the robustness of the effects.

## Campaign Finance Reform Opinions

We study framing effects by examining opinions about campaign finance reform. We do so because it is politically relevant, has received scant academic attention, involves fundamental democratic values, and is representative of a host of issues.

Campaign finance reform persists as a major political issue due in large part to the efforts of Senators McCain and Feingold. In March, 2002, Congress passed the McCain-Feingold reform bill; the law prohibits national parties from collecting or using soft money which is the unrestricted contributions by corporations, unions, and individuals. Despite the new law, debates about campaign finance will undoubtedly continue with a focus on implementation and other related regulations (Oppel 2002). While descriptive polls consistently show that the majority of Americans support campaign finance reform (72% in a February 2002 Gallup poll), there continues to be little scholarship on the origins and nature of campaign finance reform opinions (however see Gross and Brewer 2002; Grant and Rudolph 2003).

<sup>7</sup>Iyengar and Kinder (1987, 25–6, 44) examine the persistence of agenda setting and priming. There also is related work on persuasion (e.g., Hovland and Weiss 1951–52).

Aside from political relevance and the need for more research, we have an interest in campaign finance reform opinions because the issue pits two basic values against one another. Since court rulings in the 1970s, reform advocates frame the issue in terms of limiting the power of special interests (i.e., democratic equality). Opponents of reform argue that finance laws unconstitutionally restrict free speech (Grant and Rudolph 2003).<sup>8</sup> How citizens settle the clash between values is a fundamental question for public opinion scholars (Sniderman et al. 1996), and framing can be a large part of that process (Sniderman and Theriault n.d.). Moreover, from a framing perspective, campaign finance reform opinions resemble many other issues that pit basic values/considerations against one another (Nelson and Kinder 1996, 1058; Jacoby 2000, 764). We thus expect that our results generalize beyond campaign finance reform.

## Experiment

To investigate framing and interpersonal conversations in the context of campaign finance reform, we implemented a laboratory experiment—one of the central modes of inquiry in the framing literature. We next describe our experimental design and hypotheses. We then present the results.

### Participants, Design, and Procedure

A total of 261 individuals participated in the experiment in exchange for a cash payment. We recruited participants from a large public university, inviting them to take part in a study on learning from the news at the university's Political Psychology Laboratory, during the spring of 2001.<sup>9</sup> We randomly assigned participants to one of seven conditions. Those assigned to the control condi-

<sup>8</sup>References to these two values appear in nearly all media coverage and legislative debates. For example, after McCain-Feingold passed in the Senate, opponent Senator Phil Gramm stated, "We are not taking away political influence [by special interests] at all. . . We are taking it away from the people who are willing and able to use their money to enhance their free speech guaranteed by the Constitution" (Berke 2002, A30).

<sup>9</sup>The bulk of our sample consists of students. While this limits the generalizability of specific levels of reform support, we have confidence in the generalizability of any causal relationships we might uncover. A growing body of work shows that framing and analogous processes do not differ between student and nonstudent samples (Kühberger 1998, 35; Miller and Krosnick 2000, 313). Also, our main interest lies in uncovering the conditions under which elite framing does not impact individuals; and thus, the use of students who tend to be more easily influenced by persuasive arguments (Sears 1986, 522) might be a bias against our hypotheses. Finally, the participants' demographics reveal a heterogeneous and fairly rep-

resentative group that compares favorably with the 2000 National Election Study sample. Details are available from the authors.

tion neither read an article nor engaged in any discussion; they simply completed the questionnaire, described below. The treatment participants received *either* a "free-speech" framed article *or* a "special-interests" framed article. The articles, which appear in the Appendix, both describe the McCain-Feingold reform bill and state that the bill is pending in the U.S. Senate. They explain that reform supporters argue that the bill will limit special-interest influence while opponents worry about free-speech violations. The articles differ from one another in two ways. Specifically, the free-speech article uses a title that emphasizes free-speech considerations and includes a quote from a Harvard law professor who argues for the precedence of free-speech considerations (citing the Supreme Court's *Buckley v. Valeo* opinion). The special-interests article has an analogous title and quote but instead of free speech, it focuses on limiting special interests (citing Supreme Court Justice White's opinion).

We went to great lengths to make these articles appear realistic. First, in writing the articles, we drew extensively on recent similar reports. Second, we presented the articles as if they came from the *New York Times*' Web site. We copied an actual article from the site and then replaced the original text with our text—thus, the articles appeared identical to articles from the site. Third, our experiment began about a week after the Senate introduced the McCain-Feingold bill, and we finished the sessions before the Senate debate began. The articles thus accurately stated that the Senate would soon be considering McCain-Feingold. Moreover, a flurry of media coverage preceded our sessions but ended just before our first session and did not begin again until after our last session. Therefore, the articles nicely followed prior media coverage, and we did not have to worry about ongoing coverage jeopardizing comparisons across sessions. This greatly enhanced the external validity of our timing and context (Cook and Campbell 1979, 71).<sup>10</sup>

Aside from assigning treatment participants to read a free-speech or special-interests article, we also assigned them to one of three conversational conditions—a "no-discussion" group, an "unmixed" discussion group, or a "mixed" discussion group. The no-discussion group participants read the articles and did not engage in discussion, while the unmixed and mixed discussion participants took part in small group discussions *after* reading an article.

representative group that compares favorably with the 2000 National Election Study sample. Details are available from the authors.

<sup>10</sup>No participants expressed suspicion about the veracity of the articles. The style of the articles follows many other framing experiments (e.g., Nelson and Oxley 1999).

(We describe the difference between unmixed and mixed groups below.)

We put considerable time into determining how to structure these discussions. First, to maintain manageably sized groups that also had a mix of opinions, we included four participants in nearly all groups. (We used a few groups of three, but their behavior did not significantly differ from the groups of four.) Second, we attempted to balance realistic discussion settings with characteristics of deliberative settings that typically require citizens “address each other as equals and acknowledge this status by offering reasonable, morally justifiable arguments to each other” (Sanders 1997, 348). We provided a moderate degree of structure by offering (in a random order) each participant, after reading the article, an equal opportunity to state his or her view. We then allowed unfacilitated open discussion for up to six minutes (see Stasser and Titus 1985; Tindale, Sheffey, and Scott 1993). We permitted each participant to pass on stating his or her individual view and told participants that they could discuss any aspect of the article or anything else.

Participants in the discussion groups read the article and then took part in either an unmixed frame or a mixed frame discussion. In the unmixed frame discussion groups, all participants had read the same article—either the free-speech or special-interests article. The mixed frame discussions included two participants who had read the free-speech article and two participants who had read the special-interests article (see Vinokur and Burnstein 1978). For the sake of brevity, we hereafter refer to these as the unmixed and mixed discussants, respectively.

We expect that, entering the discussions, the unmixed discussants, on average, will share the same point of view to a greater extent than the mixed discussants. Indeed, these participants all received the same elite frame (and, as we will show, these frames had an impact on the no-discussion participants). Alternatively, we expect mixed discussants, on average, to be exposed to a greater variety of views since these groups include participants who received different frames. The unmixed groups were thus *relatively homogenous*, while the mixed groups were *relatively cross-cutting*.

In accordance with our prior discussion, we predict that the unmixed (relatively homogenous) group will cause polarization such that the initial elite frames will be exaggerated, while the mixed (relatively cross-cutting) groups will depolarize, and the impact of the elite frames will be squelched.<sup>11</sup>

<sup>11</sup>We emphasize that our unmixed groups were *relatively* more homogenous, while our mixed groups were *relatively* more cross-

To summarize, we assigned each participant to either the control group or one of six treatment conditions—which varied the elite frame received (free speech or special interests) and the type of subsequent discussion (no-discussion, unmixed discussion, or mixed discussion). After completing this part of the study, each participant received a questionnaire. In addition to common demographic queries—including political knowledge and need to evaluate (NE) measures—we included three items to measure elite framing effects (Nelson and Oxley 1999).<sup>12</sup>

First, we asked participants if they support passage of the McCain-Feingold campaign finance reform bill, which was the main focus of the article and discussion. We measured this *overall opinion* question on a seven-point scale with higher scores indicating increased support. Second, participants rated how important the key ideas of “protecting free speech rights of individuals and groups” and “protecting government from excessive influence by special interests” were for them when they thought about McCain-Feingold.<sup>13</sup> We measured these *belief-importance* items on seven-point scales with higher scores indicating increased perceived importance. Third, we included *belief-content* measures that asked participants if they thought the impact of reform would have a positive or negative effect on “free-speech rights” and “limiting special-interest influence.” Higher scores on a one-to-seven scale indicate a more positive effect from reform.

cutting. Some participants undoubtedly came into the experiment with strong prior opinions; were consequently unaffected by the article’s frame; and, as a result, may have entered the discussion with an attitude counter to the article they read—thereby rendering the particular group not entirely unmixed/homogenous or perfectly mixed/heterogeneous. This is *not* a problem for our analyses. Random assignment means that the average participant in each condition was basically the same, and our focus is on comparing these averages across experimental conditions. Moreover, within each particular discussion group, the average participant prior to the discussion presumably resembled the average participant from the no-discussion groups—since they had had exactly the same experience of reading the framed article. If we find that the frames in the no-discussion groups had the expected effects (e.g., the special-interests frame caused increased reform support), then we can assume that the average discussion group participant, prior to discussion but after reading the article, reflects the article they just read. This means that, on average, the groups will differ from one another in terms of being relatively homogenous or cross-cutting, and our focus is on the effects of these different groups, on average.

<sup>12</sup>Participants were paid, thanked, and debriefed after completing the questionnaire. Details on all measures are available from the authors.

<sup>13</sup>The articles explicitly mention these two considerations, and they are most relevant to reform debates (see Nelson, Clawson, and Oxley 1997, for a similar approach).

TABLE 1 Experimental Conditions and Predictions

Measure	No-Discussion		Unmixed Discussion		Mixed Discussion	
	Free Speech Frame	Special Interests Frame	Free Speech Frame	Special Interests Frame	Free Speech Frame	Special Interests Frame
Overall Support for McCain-Feingold	Lower <sup>A</sup>	Higher <sup>A</sup>	Lower <sup>B</sup>	Higher <sup>B</sup>	No difference <sup>C</sup>	
Importance of Free-Speech Rights	Higher <sup>A</sup>	Lower <sup>A</sup>	Higher <sup>B</sup>	Lower <sup>B</sup>	No difference <sup>C</sup>	
Importance of Influence by Special Interests	Lower <sup>A</sup>	Higher <sup>A</sup>	Lower <sup>B</sup>	Higher <sup>B</sup>	No difference <sup>C</sup>	
Impact of Reform on Free-Speech Rights (Belief Content)	No difference <sup>A</sup>		No difference <sup>B</sup>		No difference <sup>C</sup>	
Impact of Reform on Influence by Special Interests (Belief Content)	No difference <sup>A</sup>		No difference <sup>B</sup>		No difference <sup>C</sup>	

<sup>A</sup>These comparisons are between the no-discussion free-speech participants and the no-discussion special-interests participants.

<sup>B</sup>These comparisons are between the unmixed discussion free-speech participants and the unmixed discussion special-interests participants. We also predict that these differences will be greater than those among the no-discussion participants (i.e., polarization).

<sup>C</sup>These comparisons are between the mixed discussion free-speech participants and the mixed discussion special-interests participants. We also predict that these participants will not significantly differ from the control group participants (see Sniderman and Theriault n.d.).

In evaluating elite framing effects, we follow Nelson and his colleagues by focusing on between frame comparisons of the overall opinion and belief importance measures (e.g., Nelson, Clawson, and Oxley 1997; Nelson and Oxley 1999; Nelson and Willey 2001). We say an *elite framing effect* occurs if, within a discussion condition (i.e., no-discussion, unmixed, or mixed), compared to participants who read the free-speech article, participants who read the special-interests article exhibit significantly greater support for reform; rate influence by special interests as significantly more important; and rate free-speech rights as significantly less important. Moreover, we expect the effect of the frames on overall opinion to be mediated by the belief-importance measures. Finally, consistent with Nelson and his colleagues, we predict that elite framing effects will have no or minimal effect on the belief-content measures (Nelson and Oxley 1999, 1043; Nelson and Willey 2001, 260); we include content measures to demonstrate that framing works through a mechanism that is distinct from persuasion.<sup>14</sup>

We thus expect an elite framing effect for the no-discussion participants (i.e., this would replicate prior

framing effect experiments), an even greater elite framing effect for the unmixed discussion participants (i.e., the conversations exaggerate the initial elite frames), and no elite framing effect for the mixed discussion participants. We summarize these predictions, by treatment condition, in Table 1.<sup>15</sup> Recall that we also will examine the mediating role of belief importance, the moderating effects of knowledge and NE, and the longevity of the effects.

## Deliberation Results

In Table 2, we report the results for overall opinion, belief importance, and belief content (i.e., the impact measures) for each condition. Within each discussion (treatment) condition, we use asterisks to indicate the significance levels between statistically significant results.<sup>16</sup>

We also include the control group results as a point of comparison. Our main analyses do not involve comparisons with the control group, however, since our focus—like that of virtually all prior research—is on how alternative elite communications

<sup>14</sup>Nelson, Oxley, and Clawson (1997, 223) explain that framing “differs both theoretically and empirically from . . . *persuasion*” (emphasis in original). Framing works by altering the considerations seen as important (belief importance; e.g., when thinking of a new housing project, are economic or environmental considerations more important?) whereas persuasion involves changes in the evaluations based on those considerations (belief content; e.g., will the economic impact be positive or negative?).

<sup>15</sup>Due to our focus on how conversations affect elite framing effects, we do not include conditions that involve conversations without prior framing. We expect that such conditions would resemble our discussion conditions, depending on the extent of conflicting perspectives. These conditions would be normatively intriguing since they would reveal the impact of deliberation on unaffected opinions.

<sup>16</sup>We use one-tailed tests since we have directional predictions (Blalock 1979, 163). When appropriate, we also carried out planned comparisons and found consistent results.

TABLE 2 Overall Support Opinion, Belief Importance, and Belief Content Measures by Condition

Mean (Standard Deviation) for:	No-Discussion		Unmixed Discussion		Mixed Discussion		Control Group (N = 49)
	Free Speech Frame (N = 38)	Special Interests Frame (N = 40)	Free Speech Frame (N = 35)	Special Interests Frame (N = 34)	Free Speech Frame (N = 32)	Special Interests Frame (N = 32)	
Overall Support for McCain-Feingold <sup>a</sup>	4.53** (1.94)	5.28** (1.30)	4.57* (1.70)	5.24* (1.76)	4.66 (1.72)	4.81 (1.86)	4.52 (.92)
Importance of Free-Speech Rights <sup>b</sup>	5.26** (1.67)	4.58** (1.50)	4.34 (1.70)	4.68 (1.63)	4.69 (1.67)	4.69 (1.55)	4.84 (1.45)
Importance of Influence by Special Interests <sup>b</sup>	5.24* (1.78)	5.75* (1.15)	5.06** (1.71)	5.74** (1.64)	5.34 (1.56)	5.28 (1.75)	5.46 (1.50)
Impact of Reform on Free-Speech Rights <sup>c</sup> (Belief Content)	3.74** (1.74)	4.33** (1.29)	4.20 (1.57)	4.38 (1.60)	4.44 (1.54)	4.56 (1.63)	4.33 (1.41)
Impact of Reform on Influence by Special Interests <sup>c</sup> (Belief Content)	5.05 (1.69)	4.93 (1.29)	4.71 (1.60)	5.21 (1.55)	4.59 (1.01)	4.59 (1.74)	4.94 (1.31)

Note: Table entries are mean 7-point scores. \*\* $p < .05$ ; \* $p < .10$ .

<sup>a</sup>Higher scores indicate increased support for McCain-Feingold.

<sup>b</sup>Higher scores indicate increased perceived importance.

<sup>c</sup>Higher scores indicate a more positive impact from reform.

affect opinions (e.g., Tversky and Kahneman 1981; Kinder and Sanders 1990; Nelson and Kinder 1996; Nelson, Clawson, and Oxley 1997, 578–9). We will, nevertheless, note comparisons with the control group as it serves as an alternative and underappreciated evaluative standard that reveals the impact of frames on unadulterated opinions (Druckman 2001a, 2001b, 1048).

As expected, we find strong evidence for an elite framing effect for the no-discussion group participants—compared to free-speech participants, special-interests participants displayed significantly more support for McCain-Feingold, and rated special interests as a significantly more important and free speech as a significantly less important consideration ( $t_{76} = 2.01$ ,  $p < .03$ ;  $t_{76} = 1.53$ ,  $p < .07$ ;  $t_{76} = 1.92$ ,  $p < .03$ , respectively). The free-speech article also persuaded participants to believe the impact on free speech would be significantly more negative; this belief-content effect does not mitigate the more substantial framing effects, but rather it suggests that the articles have multiple effects (i.e., framing and persuasion; see Nelson and Oxley 1999).

We also see an elite framing effect for the unmixed discussion group with the special-interests participants expressing more support for McCain-Feingold and rating special interests as more important, than the free-speech participants ( $t_{67} = 1.59$ ,  $p < .06$ ;  $t_{67} = 1.68$ ,  $p < .05$ , respectively). We find no significant effects on the free-speech consideration or the belief-content measures.

Counter to the polarization hypothesis, we find no evidence of an exaggerated elite framing effect—an ANOVA shows no significant frame  $\times$  discussion interaction (in the expected direction) that would indicate an exaggerated effect.<sup>17</sup> This analysis also reveals that the discussions themselves did not have significant effects on how the elite frames were processed (i.e., there are no significant discussion main effects). In short, elite framing persists largely unchanged in the face of discussions that include mostly common perspectives.

We suspect that the lack of a polarization effect stems from the fact that, as discussed (see note 11), while these groups were presumably homogenous relative to the mixed groups, they may not have been perfectly homogenous. The occasional participant could have been unaffected by the article and entered the discussion with

<sup>17</sup>For both McCain-Feingold opinion and special-interests importance, the frame main effect is significant ( $F_{1,143} = 6.43$ ,  $p < .02$ ;  $F_{1,143} = 5.20$ ,  $p < .03$ , respectively) but both the discussion main effect ( $F_{1,143} = 0$ ,  $p < .99$ ;  $F_{1,143} = .14$ ,  $p < .72$ ) and the interaction ( $F_{1,143} = .02$ ,  $p < .88$ ;  $F_{1,143} = .10$ ,  $p < .76$ ) are not significant. For the importance of free speech, the main effects are not significant ( $F_{1,143} = .44$ ,  $p < .51$  for frame;  $F_{1,143} = 2.33$ ,  $p < .13$  for discussion). The interaction is marginally significant ( $F_{1,143} = 3.63$ ,  $p < .06$ ), however, the means in Table 2 reveal that this is not in the direction predicted by the polarization hypothesis. Indeed, for the importance of free speech, we find a larger elite framing effect for the no-discussion group than for the unmixed-discussion group. ANOVA analyses for the belief content measures show no significant effects.



an opposing view that was sufficient to prevent further polarization, but not sufficient to generate the depolarization predicted with the relatively more cross-cutting mixed groups. It also may be that the measurement delay due to the discussions even caused a slight waning of a short-lived framing effect.

The story changes when we look at the mixed discussion participants. Table 2 shows no significant framing effects for these participants; as predicted, these cross-cutting conversations eliminate elite framing effects.<sup>18</sup> In fact, the mixed discussion participants' level of support for McCain-Feingold and their belief-importance responses do *not* differ significantly from the control groups' responses.<sup>19</sup> This corroborates Sniderman and Theriault's (n.d.) claim that, when exposed to conflicting frames, people return to their original or unaffected positions as reflected by the control group (also see Huckfeldt, Morehouse, and Osborn n.d.).<sup>20</sup> This finding also resonates with the aforementioned theoretical work on deliberation. The "collision of adverse opinions" in the mixed group altered opinions by eliminating the elite framing effect. The question of whether this is akin to restoring "truth" (Mill 1859) is something to which we will return.

Our results extend the study of framing effects into a new issue domain by showing the conventional effect in the no-discussion group. We also find that conver-

sations that include common perspectives (with people who were exposed to the same frame) do little to the initial elite effect. However, conversations that include conflicting perspectives (with people who received different frames) eliminate elite influence via framing. In sum, in the political world where people receive and then discuss elite information, conversations can limit elite influence—but only if those conversations involve cross-cutting groups or individuals exposed to alternative arguments.<sup>21</sup>

We next examine the mediational process of elite framing by using the same path-analytic approach as Nelson and his colleagues (Nelson, Clawson, and Oxley 1997; Nelson and Oxley 1999). In Figure 1, we present separate path analyses for the no-discussion, unmixed, and mixed discussion groups.<sup>22</sup> For both the no-discussion and unmixed discussion groups, the frame shaped the belief importance ratings, which in turn substantially affected overall opinions. In contrast, the frames had minimal impact on the content measures and even when they did (in the no-discussion condition), this effect did not carry through to overall opinions. In both groups, the frame had no direct effect on overall opinions suggesting that elite framing works by affecting belief importance (and not content) that in turn drives overall opinions.

Figure 1c shows that for the mixed group, the frame had no effect on any of the measures; overall opinion was shaped by the belief importance and content measures, but the frame played no role. These results corroborate Nelson and his colleagues' research that shows that framing works through belief importance and as such constitutes a unique psychological process (see note 14). The results also confirm our finding that cross-cutting conversations limit the impact of elite frames, highlighting the importance of moving the study of elite influence out of a social vacuum and into settings with informational competition.

In so doing, future work would benefit from examining information flows more acutely. For example, Figure 1 shows that in the no-discussion conditions,

<sup>18</sup>We focus on within-group comparisons because our prediction is that there should be no framing effect among mixed discussion participants, and, as discussed, the conventional way to measure elite framing effects involves comparisons between frames. In contrast to our polarization hypothesis, our predictions here do not involve relative comparisons across discussion conditions—for example, we do not predict a relatively smaller or a reverse framing effect in this group (see Blalock 1979, 347–8).

<sup>19</sup>This is true regardless of if we compare the control group to each framed mixed-discussion group or to all mixed-discussion participants (averaged). The relevant average statistics for all mixed-discussion participants, for McCain-Feingold, speech importance, and special-interests importance, respectively, are: 4.73 (1.78, 64; compared with the control group,  $t_{110} = .76$ ,  $p < .25$ ); 4.69 (1.60, 64; compared with the control group,  $t_{110} = .51$ ,  $p < .35$ ); and 5.31 (1.64, 64; compared with the control group,  $t_{110} = .48$ ,  $p < .35$ ).

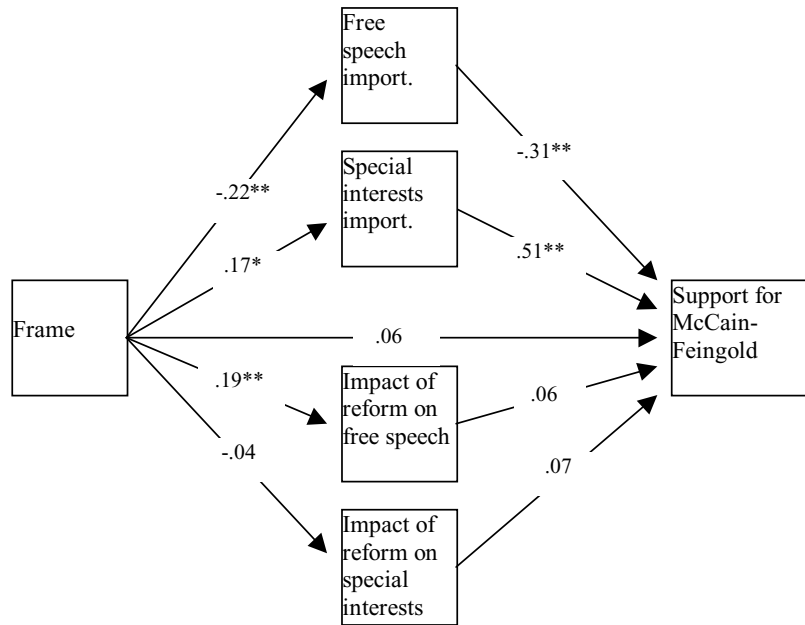
<sup>20</sup>We also find that the control group significantly differs from the following conditions: for McCain-Feingold, no-discussion and unmixed discussion special interests frame participants ( $p < .05$  for both); and for speech importance, no-discussion and unmixed discussion free-speech frame participants ( $p < .10$  for both). (Details are available from the authors.) Many widely cited framing effects do not include comparisons with a control group, and it is unclear if such comparisons would be significant. While we do not question the relevance of comparisons between framed conditions, we emphasize the importance of recognizing what is and is not being measured (e.g., the impact of frames on unadulterated opinions requires comparisons with a control group).

<sup>21</sup>Examining the absolute differences between opinions across frames accentuates the distinction between the no-discussion and unmixed-discussion conditions, and the mixed-discussion conditions. For example, for overall McCain-Feingold opinions, the frames caused total shifts in opinion of .75 and .67 for the no-discussion and unmixed-discussion conditions, respectively, and only .15 for the mixed discussion conditions. The analogous shifts for the importance of special interests are .51, .68, and .06.

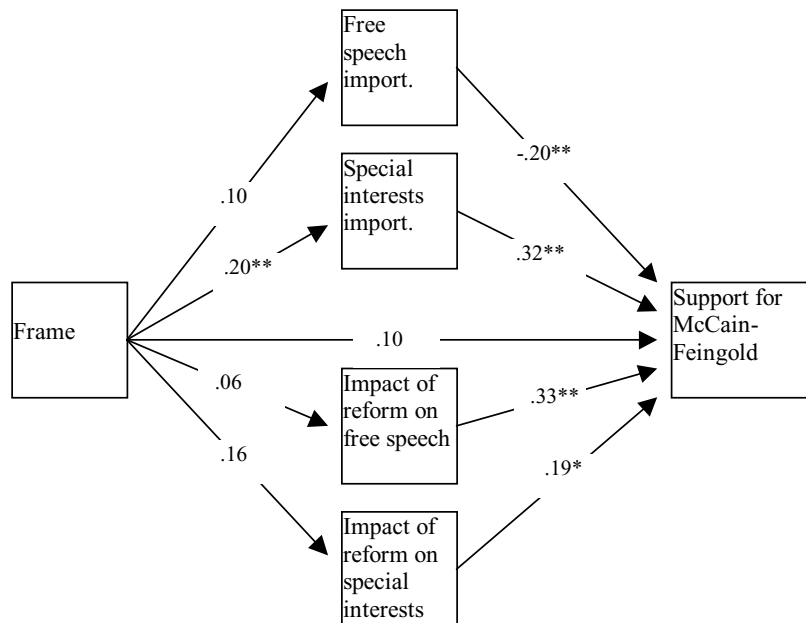
<sup>22</sup>We first regress the belief importance and content measures on the frame, and then we regress overall opinion on the frame, and the importance and content measures.

**FIGURE 1** Mediation Analyses

**A. No-Discussion Conditions**



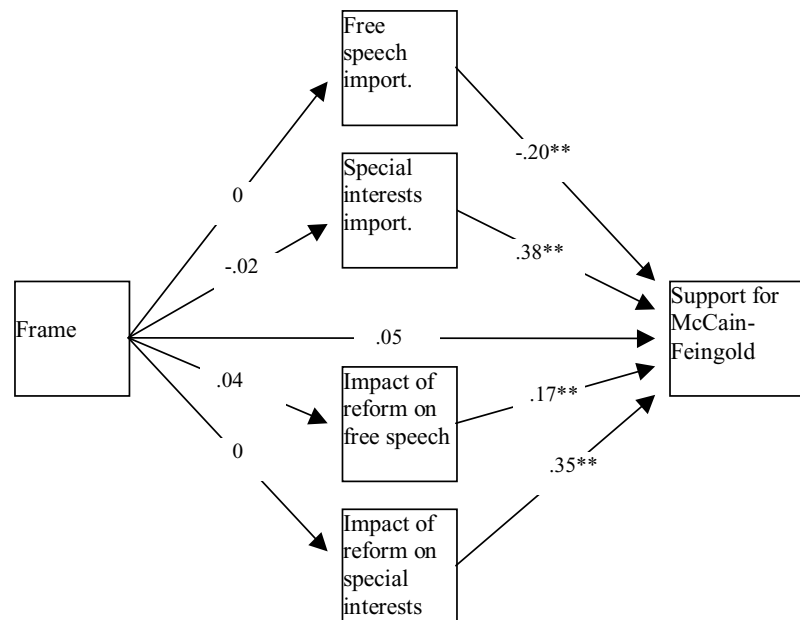
**B. Unmixed Discussion Conditions**



the frames affected belief content, which in turn had no impact on McCain-Feingold opinions. In contrast, in both discussion groups, the frames did not influence belief content but belief content shaped McCain-

Feingold opinions. While this does not have consequences for our framing results, it suggests that the conversations introduced distinct dynamics. It may also further explain our failure to find polarization—that is, the

FIGURE 1 (Continued)

*C. Mixed Discussion Conditions*

Note: As in Nelson, Clawson, and Oxley (1997) and Nelson and Oxley (1999), coefficients are standardized ordinary least-squares (beta) coefficients. \*\* $p \leq .05$ ; \* $p \leq .1$ . Frame is coded so that 0 = Free speech and 1 = Special interests. The importance items are coded so that higher values indicate increased perceived importance. The belief content items are coded so that higher values indicate a more positive effect. Support for McCain-Feingold is coded so that higher values indicate increased support.

conversations did not appear to simply reiterate the elite frames.<sup>23</sup>

### Individual Moderator Results

We next examine individual moderators of framing effects. As discussed, we introduce a new and potentially

<sup>23</sup>Reform advocates often express incredulity about equating money to free speech, seeing the link as nonsensical (e.g., Stevens' opinion in *Nixon v. Shrink Missouri Government PAC*, January 24, 2000; Silverstein 2003). If uninitiated individuals experience similar difficulty in grasping the money and speech connection, then they may be less swayed by the speech argument and more critical of those who espouse it. Our results support both these conjectures. First, in contrast to the special-interests frame, for no- and unmixed discussion participants, the speech frame did little to change McCain-Feingold opinions from what they would have been absent the frame (i.e., the control group). Second, participants who read the speech article—which links the Supreme Court to the speech argument—displayed relatively less faith, trust, and approval of the Court (see Nicholson and Howard n.d.). Details are available from the authors.

fundamental moderator—need to evaluate (NE); we expect high-NE individuals to be less susceptible to framing effects. We also examine the role of political knowledge, hypothesizing that increased knowledge facilitates framing effects (once controlling for NE).

We took three steps to investigate the impact of these moderators. First, we merged data from the no-discussion and unmixed discussion groups since the aforementioned (ANOVA) results suggest no significant differences between the two groups (see note 17). Moreover, when we separate out these conditions and run the analyses (below) separately, we find that the results for each condition generally mimic the results for the conditions run together. We do not include the mixed discussion groups since we neither expected nor found (in analyses not reported here) moderating effects—indeed, there were no framing effects to moderate.

Second, we created measures of political knowledge and NE. In creating a knowledge measure, we followed Miller and Krosnick's (2000, 304–5) approach

of combining five general-knowledge items with five domain-specific knowledge questions about campaign finance reform. We summed correct answers to these questions and then used a median split to assign respondents to a low (0) or a high (1) knowledge group. Our NE measure comes from Bizer et al.'s (2000, 14–5) three-item scale.<sup>24</sup> We aggregated the three questions and then used a median split to create a low (0) and a high (1) NE group.<sup>25</sup>

Third, to analyze the moderating effects, we focused on our three key framing variables—overall opinion about McCain-Feingold, free-speech importance, and special-interests importance. We regressed each measure on a dummy variable indicating if the participants received the free-speech article (0) or the special-interests article (1). We then added dummy variables for knowledge and NE; these coefficients reveal if those with high knowledge or high NE differed in their opinions (regardless of the frame) from those in the low groups. More importantly, we added interactions between the frame and the knowledge and NE measures. Significant coefficients here indicate if the framing effect differed based on levels of knowledge and/or NE. We present the results in Table 3.

Consistent with our expectations, the results show that it is those with higher levels of knowledge who exhibit relatively more susceptibility to elite framing effects. Specifically, significant and positive frame  $\times$  political knowledge interactions for McCain-Feingold and special-interests importance show that the frame exercised its effect particularly on those with high knowledge. This supports the argument that political knowledge facilitates the use of frames by allowing individuals to make sense of and connect the framed information to their opinions. It also suggests that, although elites may attempt to use framing to manipulate an unknowing and naïve populace, it is knowledgeable people who are relatively more apt to use the frames (see Miller and Krosnick 2000, 312).

<sup>24</sup>While our knowledge and NE measures are correlated ( $r = .23$ ,  $p < .01$ ,  $n = 257$ ), the moderate level of the correlation suggests distinct constructs (see Bizer et al. 2000).

<sup>25</sup>Following similar research (e.g., Kinder and Sanders 1990; Krosnick and Brannon 1993; Nelson, Oxley, and Clawson 1997), we use median splits to minimize measurement error. Despite reliable alpha scores (of approximately .60 for both knowledge and NE, which exceeds Bizer et al.'s 2000 finding), we expect that the measures contain error such that slight changes on the respective scales may not accurately capture real differences. In contrast, a median split allows us to focus on what are more likely to be qualitatively distinct groups. With continuous measures, we find consistent, but weaker, results (as do Miller and Krosnick 2000, 305, in their priming study).

TABLE 3 Framing Effect Moderators (OLS)

Independent Variable	Dependent Variable		
	McCain-Feingold Support	Free-Speech Importance	Special-Inter. Importance
Frame	1.11** (.47)	-.40 (.49)	.76* (.45)
Political Knowledge	.18 (.38)	.39 (.39)	.17 (.36)
Need to Evaluate (NE)	1.18** (.38)	-.36 (.39)	.86** (.36)
Frame $\times$ Pol. Know.	.93** (.55)	-.48 (.57)	.78* (.53)
Frame $\times$ NE	-1.65** (.55)	.89* (.57)	-1.14** (.53)
Constant	3.79** (.34)	4.80** (.35)	4.57** (.33)
R <sup>2</sup>	.15	.04	.12
Number of Observations	145	145	145

Note: Entries are unstandardized regression coefficients with standard errors in parentheses. \*\* $p < .05$ ; \* $p < .10$ .

More importantly, we find strong evidence that NE plays a substantial role in moderating framing effects. The frame  $\times$  NE interaction is significant across all three regressions, indicating, in each case, that high-NE individuals move (relatively) against the elite framing effect (i.e., recall that the frame variable is coded 1 for special interests). Elite frames have considerably more influence on low-NE individuals; that is, individuals who engage in less chronic evaluation and who are less likely to possess prior opinions (and frames) display relatively more reliance on the recently received frames.

We do not see our NE result as adding yet another variable to the list of potential moderators; rather, we believe NE constitutes a fundamental moderator of framing and media effects more generally. High-NE individuals who engage in frequent evaluation will form more opinions on which they can draw. As a result, new information contained in elite messages will not have as large an effect on their opinions—relative to low-NE individuals who will be more reliant on the messages in constructing their opinions. We also believe that previous conflicting results on the role of knowledge stem, in large part, from a failure to measure NE.<sup>26</sup> We hope that future work will

<sup>26</sup>When we exclude NE and the frame  $\times$  NE interaction from the McCain-Feingold regression, we find no significant knowledge effect.

**TABLE 4** Longevity of Framing Effects

Mean (Standard Deviation) for:	Free-Speech Frame (N = 36)	Special-Interests Frame (N = 34)
Overall Support for McCain-Feingold <sup>a</sup>	4.94 (1.71)	5.35 (1.30)
Importance of Free-Speech Rights <sup>b</sup>	4.61 (1.76)	4.24 (1.56)
Importance of Influence by Special Interests <sup>b</sup>	5.03 (1.86)	5.32 (1.45)

<sup>a</sup>Higher scores indicate increased support for McCain-Feingold.

<sup>b</sup>Higher scores indicate increased perceived importance.

include the NE measure, examining it along with knowledge across various issues and types of effects.

### Longevity Results

We also examine the longevity of framing effects. We implemented a follow-up survey ten days after the initial experiment—over which time the major local newspapers made no mention of campaign finance reform. We focus exclusively on the no-discussion and unmixed-discussion participants since these participants exhibited initial effects—do these initial effects endure? We again combine these two groups, as treating them separately yields the same results.

In Table 4, we display the results for our three main framing measures for the participants who responded to the follow-up.<sup>27</sup> The results for each measure move in the direction of a framing effect, but none are significant. The elite framing effect disappeared after ten days even in the virtual absence of other elite information. This is intriguing insofar as no other study has examined the longevity of framing effects, and these results suggest that the effects—at least when it comes to certain issues—may be fleeting. Not only does this bring the robustness of framing effects into question, but it also adds an interesting dynamic to debates about the nature of attitudes. Do short-lived framing effects suggest slight blips in enduring initial attitudes, or do the effects reflect extreme instability?

### Conclusion

Most previous work on framing effects involves exposing experimental or survey participants to a single frame

<sup>27</sup>Approximately 50% of the participants responded in exchange for entry in a lottery. More than 75% of the respondents reported being exposed to reform information fewer than three times since the experiment (and we suspect some counted the experiment).

and then immediately gauging their response with no alternative forms of information available. Using this approach, scholars have documented numerous framing effects, leading some to view the effects as highly robust and pervasive (e.g., Zaller 1992; Entman and Herbst 2001). In this article, we have enhanced the external validity of the typical framing study by incorporating three dynamics common to many political settings: (1) interpersonal conversations, (2) individual heterogeneity, and (3) over-time change. We also have extended the study of framing effects to the hotly contested issue of campaign finance reform.

Our results highlight the conditional and potentially short-lived impact of elite framing. Our most important finding concerns the role of cross-cutting interpersonal discussions in limiting framing effects. Interpersonal conversations permeate the political world, and a failure to consider their moderating impact can lead to misleading conclusions about unilateral elite influence (also see Mendelsohn 1996). We also document individual heterogeneity in elite framing effects by introducing a new and potentially powerful moderator—need to evaluate. Low-NE individuals and those who are highly knowledgeable appear more susceptible to elite framing effects. Finally, we offer evidence about the short time horizon of elite influence. *All of this suggests a more limited impact for elites than is often thought.*

We believe, however, that elite framing effects do regularly occur and have important consequences—as we have shown, they are simply conditional. We find that conversations that include mostly common perspectives do not moderate elite framing effects, and individuals tend to engage in such conversations to a greater extent than cross-cutting discussions (Mutz and Martin 2001; Mutz 2002a, 115; Walsh 2003; however, also see Huckfeldt, Morehouse, and Osborn n.d.). Moreover, we continue to have little idea about the impact of alternative discussion formats—including more informal discussions, discussions that include various mixes of opinions, and discussions that both precede and then follow elite framing attempts on different issues (see Huckfeldt, Johnson, and Sprague 2002). We need a deeper understanding of the processes by which different forms of influence work (e.g., how adept are individuals at judging the credibility of elites and other citizens?) (see Huckfeldt 2001; Lupia 2002b).<sup>28</sup> The important point is that future work on preference formation should consider the simultaneous

<sup>28</sup>It also is presumably not the case that competing frames will always cancel out—it might depend on the strength of the arguments as well as other factors. Moreover, this will not always be predictable (e.g., Chong 200, 123–4).

and competing effects of elite rhetoric and interpersonal discussions.

Finally, what do our results reveal about deliberation and democracy? As discussed, theorists often emphasize the salubrious effects of cross-cutting deliberation (e.g., Mill 1859, 53). Deliberation is said, for example, to increase engagement, tolerance, and justification for individuals' opinions (Mendelberg 2002, 153). Ultimately, opinions formed via deliberation with conflicting perspectives should better capture the "will of the people" by ensuring quality opinions that approximate truth, reasonableness, and rationality (Mill 1859, 23; Dewey 1927, 208; Kinder and Herzog 1993, 349; Benhabib 1996, 71; Bohman 1998, 401; Fishkin 1999, 283; Dryzek 2000, 55; Mendelberg 2002, 180; however, also see, e.g., Sanders 1997).

While our deliberative setting may not have been ideal, our mixed discussion results could be construed as showing that deliberation enhances opinion quality—it eliminates elite framing influence that some see as akin to manipulation (Zaller 1992, 45; Farr 1993; Parenti 1999; Entman and Herbst 2001, 207). In our case, then, deliberation enhances opinion quality if opinions affected by elite frames are indeed of lower quality than unaffected opinions (i.e., since the mixed discussion opinions resemble unaffected control-group opinions). However, we have no basis for assuming relatively higher quality of unaffected opinions, especially since they seem so easily moved by elite frames (Kuklinski et al. 2000, 811). On the flip side, one could argue that deliberation has negative consequences with the framed opinions possessing higher quality (Druckman 2001c); yet, this begs the question of why these opinions also do not last (i.e., our longevity analysis; although is stability even desirable?).

A deliberative theorist might argue that the appropriate standard would have been the opinions of participants who deliberated without prior exposure to elite frames. While we agree future work should include this condition, we also recognize the circularity of arguing that deliberative processes enhance opinion quality where opinion quality is defined as the product of deliberation (Bohman 1998).<sup>29</sup> In sum, our results accentuate the importance of developing an independent standard by which to evaluate the quality or truthfulness of opinions. While this is certainly challenging, it also is necessary if we are to assess the democratic consequences of elite influence and deliberation.

<sup>29</sup>Criteria that deliberation may promote include more information, rational preferences, increased justification for one's opinions, and reflection upon a greater number of considerations (Benhabib 1996; Mansbridge 1999). While these serve as useful starting points, there is no consensus on their desirability (e.g., Lupia 2002a on information).

## **Appendix** **Free-Speech and Special-Interests** **Experimental Articles**

### **Senate to consider free speech implications of campaign finance reform**

by Jonathan Pratt

WASHINGTON, Jan. 25 – Next week the U.S. Senate will consider the bi-partisan McCain-Feingold campaign finance reform bill which would ban soft money contributions to political parties. Under current law, there is no limit to these "soft" contributions made by individuals, businesses, and labor unions to political parties. The parties typically use soft money during federal elections to purchase issue ads and fund "get-out-the-vote" activities. A similar version of the McCain-Feingold reform bill failed in the last Congress, and its prospects for passage this time are unclear.

In last year's election, the two parties raised over \$400 million in soft money, much of it coming from special interests. Supporters of the reform bill say it would go a long way toward limiting the influence of special interests and lobbyists in Washington. Opinion about the bill, however, is mixed. In addition to political opposition, the reform bill faces a serious constitutional challenge as well. Since money given to the political parties is used for advertising and to promote different political ideas and policies, opponents of the bill argue that limiting these contributions violates free speech rights guaranteed by the First Amendment to the U.S. Constitution.

While many people worry about the power of special interests, they also do not want to limit free speech. Dr. Susan Baker, a law Professor from Harvard University, points to the Supreme Court's ruling in *Buckley v. Valeo* that "[Financial] advocacy of the election or defeat of candidates for federal office is no less entitled to protection under the First Amendment than the discussion of political policy generally. . . ." Dr. Baker herself adds that "protecting free speech takes precedence over limiting special interests because the First Amendment ensures that citizens can spend money to promote their political views. . . any spending limits create a dangerous slippery slope with regard to our fundamental First Amendment rights."

### **Senate to consider campaign finance reform that could limit influence of special interests**

by Jonathan Pratt

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finance reform bill which would ban soft money contributions to political parties. Under current law, there is no limit to these “soft” contributions made by individuals, businesses, and labor unions to political parties. The parties typically use soft money during federal elections to purchase issue ads and fund “get-out-the vote” activities. A similar version of the McCain-Feingold reform bill failed in the last Congress, and its prospects for passage this time are unclear.

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While many people worry about limiting free speech, they also want to limit the power of special interests. Dr. Susan Baker, a law Professor from Harvard University, points to former Supreme Court Justice Byron White’s argument that “the evils of unlimited contributions are sufficiently threatening to warrant restriction regardless of the impact of the limits on the contributor’s opportunity for effective speech.” Dr. Baker herself adds that “limiting special interests takes precedence over free speech because the possibility of corruption from special interests can directly erode the political process and also undermine the public’s confidence in it.”

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