Political Preference Formation:
Competition, Deliberation, and the (Ir)relevance of Framing Effects*

by
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Abstract:
A framing effect occurs when different, but logically equivalent, words or phrases – such as 95% employment or 5% unemployment – cause individuals to alter their preferences. Framing effects challenge the foundational assumptions of much of the social sciences (e.g., the existence of coherent preferences or stable attitudes), and raise serious normative questions about democratic responsiveness. Many scholars and pundits assume that framing effects are highly robust in political contexts. Using a new theory and an experiment with more than 550 participants, I show that this is not the case – framing effects do *not* occur in many political settings. Elite competition and citizens' inter-personal conversations often vitiate and eliminate framing effects. However, I also find that when framing effects persist, they can be even more pernicious than often thought – not only do they suggest incoherent preferences but they also stimulate increased confidence in those preferences. My results have broad implications for preference formation, rational choice theory, political psychology, and experimental design.

Framing effects constitute one of the most stunning social science discoveries of the last quarter century. A framing effect occurs when different, but logically equivalent, words or phrases cause individuals to alter their preferences. For example, people reject a policy program when told it will result in 5% unemployment but prefer it when told it will result in 95% employment. In short, peoples' preferences shift due to arbitrary changes in wording (Tversky and Kahneman 1981). Framing effects challenge the foundational assumptions of much of the social sciences, particularly economics and political science (e.g., rational decision-making, stable attitudes). They also raise serious normative questions about the existence of coherent preferences that serve as the basis for democratic governance and competitive markets.

Political scientists generally either embrace the results or ignore them arguing that framing effect experiments fail to capture political reality. In this paper, I analyze these alternative perspectives. I offer a psychological theory that specifies politically relevant conditions that determine *when* framing effects occur. I test these expectations with an experiment that includes more than 550 student and non-student participants. The experiment constitutes one of the first empirical assessments of framing effects that incorporates: elite competition over frames, citizens' conversations about frames, and individual differences in expertise.

I find that framing effects matter, but in a fairly narrow sense – framing effects appear less consequential in political contexts than is often assumed. Elite competition and inter-personal conversations often vitiate and eliminate framing effects. However, I also find that when framing effects persist, they can be even more pernicious than often thought – not only do they suggest incoherent preferences but they also stimulate

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increased confidence in those preferences. I discuss the implications of the results for preference formation, rational choice theory, political psychology, and experimental design.

Framing Effects and Their Implications

Scholars from multiple disciplines employ the term "framing effect" to refer to distinct phenomena (Druckman 2001b: 226-231). I focus on one of the better known usages where, as mentioned, an effect occurs when different, but logically equivalent, phrases cause individuals to alter their preferences (Tversky and Kahneman 1981; see note 1 on alternative usages).[1] This typically involves casting the same information in either a positive or negative light (Levin et al. 1998: 150); for example, in their widely cited disease experiment (to which I will return), Tversky and Kahneman (1981, 1987) asked one group of respondents to respond to problem 1:

> Imagine that the U.S. is preparing for the outbreak of an unusual Asian disease, which is expected to kill 600 people. Two alternative programs to combat the disease have been proposed. Assume that the exact scientific estimates of the consequences of the programs are as follows:
>
> If Program A is adopted, 200 people will be saved.
>
> If Program B is adopted, there is a 1/3 probability that 600 people will be saved, and a 2/3 probability that no people will be saved.
>
> Which of the two programs would you favor?

Notice that the two programs have the same expected value of saving 200 people, but program A constitutes a risk-averse choice (i.e., the outcome is certain), while program B is a risk-seeking choice (i.e., the outcome involves a risky gamble). Tversky and

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[1] Political communication scholars use the term "framing effects" to refer to situations where by emphasizing a subset of potentially relevant considerations, a speaker leads individuals to focus on these considerations when constructing their opinions. For example, if a speaker describes a hate group rally in terms of free speech (public safety), then the audience will base its rally opinions on free speech (public safety) considerations (e.g., Nelson et al. 1997). These types of framing effects are *distinct* since they do not involve logically equivalent ways of making the same statement (Druckman 2001b). Also, while related, I do not examine survey question wording effects or preference reversals (Slovic 1995).

Kahneman find that 72% of the respondents chose program A and 28% preferred

program B. They asked another group of respondents to respond to problem 2, which

differs from problem 1 only in the choice of alternatives. This time, respondents faced the

following choice:

> If Program A is adopted, 400 people will die.

> If Program B is adopted, there is a 1/3 probability that nobody will die, and a 2/3 probability that 600 people will die.

These programs are equivalent to those offered in problem 1 except they are framed in

terms of the number of people dying (negatively) instead of the number of people being

saved (positively). Tversky and Kahneman find that, in this case, 78% of respondents

chose the risk-seeking program B, and only 22% opted for program A. Thus, individuals'

preferences change (by 50%) due to alternative frames even though the objective

outcomes and their descriptions remain equivalent. This is an example of a *risky choice*

framing effect (Levin et al. 1998).

Other work examines an analogous effect, called *attribute* framing (Levin et al.

1998). For example, Quattrone and Tversky (1988: 727) presented some respondents

with the following problem:

> Political decision-making often involves a considerable number of trade-offs. A program that benefits one segment of the population may work to the disadvantage of another segment. Policies designed to lead to higher rates of employment frequently have an adverse effect on inflation. Imagine you were faced with the decision of adopting one of two economic policies.

> If *program J* is adopted, 90% of the work force would be employed, while the rate of inflation would be 12%. If *program K* is adopted, 95% of the work force would be employed, while the rate of inflation would be 17%. The following table summarizes the alternative policies and their likely consequences:

Policy	Work Force Employed (%)	Rate of Inflation (%)
Program *J*	90	12
Program *K*	95	17

> Image you were faced with the decision of adopting program J or program K. Which would you select – program J or program K?

They find that 54% of respondents opted for program J and 46% preferred program K.

The results change, however, when the same question is posed in terms of *un*employment

(negative) rather than employment (positive) – as follows:

> …If *program J* is adopted, 10% of the work force would be unemployed, while the rate of inflation would be 12%. If *program K* is adopted, 5% of the work force would be unemployed, while the rate of inflation would be 17%. The following table summarizes the alternative policies and their likely consequences:

Policy	Work Force Unemployed (%)	Rate of Inflation (%)
Program *J*	10	12
Program *K*	5	17

In this case, 64% of respondents selected program K and only 36% opted for program J

(an 18% shift). The frame here does not affect risk attitudes; rather, the different frames –

despite being objectively identical – stimulate positive or negative associations that lead

to favorable or unfavorable evaluations (McNeil et al. 1982: 1260, Levin et al. 1998:

175).[2] Seemingly innocuous changes in the description of program attributes alter

preferences.

Evidence of Framing Effects

Evidence of framing effects appears, at first glance, overwhelming. Hundreds of

experiments – most of which follow the design of the aforementioned problems – suggest

highly robust and relevant effects.[3] Tversky and Kahneman (1987: 88) explain that the

effects are "ubiquitous [and] no adequate descriptive theory can ignore these phenomena"

(see Kahneman and Tversky 2000). Kahneman (2000: xv) adds that "Framing effects are

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[2] Levin et al. (1998) identify a third type of framing effect that affects behaviors rather than risk attitudes or evaluations.
[3] Not all the results are positive, however, and some scholars question the robustness of the effects (e.g., Jou et al. 1996, Smith and Levin 1996: 288, Fagley and Miller 1997, Druckman 2001b: 237).

less significant for their contribution to psychology than for their importance in the real world." Many political scientists agree; Bartels (2003) states that there is "little basis for supposing that even well-informed, well-thought-out opinions are likely to be immune from framing effects" (also see Simon 1983: 17, Iyengar 1991: 13, Zaller 1992, Entman 1993, Sunstein 2000). Levy (2002: 273) elaborates:

> The evidence is quite robust. Findings…have been confirmed with subjects of considerable expertise in probability and statistics, including medical professionals, and it has been supported by empirical studies of investment and insurance behavior (Kahneman and Tversky 2000). The same patterns have been confirmed by experimental economists who were determined to (and expecting to) demonstrate that the findings were artifacts of flawed experimental designs (Camerer 1995).

As *The New York Times* reported after Kahneman received the Nobel prize in economics, the results are "based primarily on work that reflects real people's choices…In that sense, the findings have an inherent validity" (Altman 2002: C6).

In addition to the number of studies, these conclusions presumably reflect two aspects of framing experiments (see, e.g., Kinder 1993: 124). First, researchers document these effects across an impressive array of domains including bargaining, economic and financial, gambling, legal, political, public health, medical, social, and other areas (for reviews, see Kühberger 1998, Levin et al. 1998). They also often withstand variations in stakes, response mode, and other problem specific characteristics (Kühberger et al. 1999). Second, the effects appear in non-student samples with so-called experts such as physicians, judges, mathematicians, engineers, individuals directly involved in the situation, gamblers, paid individuals, and others (Tversky and Kahneman 1987: 90, Kühberger 1998: 45).[4]

Implications for Political Science

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[4] Specific references for each domain and expert listed are available from the author.

Robust framing effects bring into question the foundation of much political analysis and theory (see Russell and Thaler 1991 on implications for economics). Analysts traditionally assume that citizens base their political decisions and actions (e.g., participation, voting) on their *preferences* or *attitudes*. And theorists view citizens' preferences as the basis for democratic governance (Dahl 1971: 1). Yet, framing effects challenge the very existence of preferences and attitudes.

First, framing effects suggest that preferences depend on context, and thus, one cannot assume fixed and exogenous preferences, as is common with rational choice models (e.g., Rabin 1998: 11). Second, framing effects in fact render standard conceptions of preferences meaningless. A preference is "a comparative evaluation of (i.e., a ranking over) a set of objects" (Druckman and Lupia 2000: 2) – as when an individual prefers Policy A to Policy B – such that the ranking is *invariant* to alternative ways of eliciting the same preference (Arrow 1982, Tversky and Kahneman 1987: 69; also see Slovic 1995). Framing effects, by definition, violate the invariance assumption since preferences depend on the frame employed during elicitation (however see Stanovich and West 1998: 290-291). Third, instead of preferences, many scholars focus on attitudes, defined as "a person's general evaluation of an object" (O'Keefe 1990: 18), such as when an individual favors or disfavors Policy A, regardless of what he or she thinks of Policy B. While attitudes need not be invariant (Kahneman et al. 1999), they typically are seen as stable constructs that do not change due to slight alterations in elicitation, as occurs with framing effects (e.g., Wilson and Hodges 1992: 38).

Pervasive framing thus means that the constructs on which people base their political actions are not what they have typically been presumed. Rather, the

psychological basis of political behavior is more volatile and less consistent (Kahneman et al. 1999). This has numerous implications, including: (1) suggesting a different approach to studying political behavior that avoids employing the constructs of preferences and stable attitudes (Zaller 1992, Kuklinski and Jerit 2001: 344); (2) questioning normative models of democratic responsiveness that assume citizens have preferences to which governors might respond (Entman 1993: 57, Bartels 2003); and (3) a re-evaluating of the role of public opinion in policy making (Kahneman et al. 1999, Levy 2002). On the other hand, if framing effects are not so widespread, then the traditional constructs may be suitable for positive, normative, and public policy concerns. Clearly, understanding framing effects has grave consequences for political science.[5]

Questioning the Robustness of Framing Effects

While many political scientists view framing effect results as sufficient to abandon traditional assumptions about coherent preferences and/or stable attitudes (e.g., Iyengar 1991, Zaller 1992, Entman 1993, Kinder and Palfrey 1993: 20, Kinder 1998, McDermott 2002: 49-50, Bartels 2003), others discount the effects, arguing that the experiments are not relevant because they do not account for aspects of politics such as elite competition (e.g., Morrow 1994: 48, Riker 1995, Wittman 1995: 41-44, Sniderman 2000). Unfortunately, these two groups of scholars rarely engage one another, and tend to rely on conjectures rather than empirical evidence about politics and framing effects.[6] I next offer and test a psychological theory that specifies *when* framing effects will occur.

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[5] While not my focus, the results also have substantially influenced international relations research, work on bargaining and organizational behavior, and research from various other disciplines including, for example, cognitive science and medical decision-making.

[6] However, see Boettcher (1995). Also see Russell and Thaler (1991) for a similar portrait of economists.

The results constitute some of the first empirical evidence that directly addresses the political relevance of framing effects.

The Psychology of Framing Effects

The obvious starting point to build a theory of framing effects is Tversky and Kahneman's (1979) prospect theory. Their theory, however, provides little insight into the processes and conditions under which the effects might occur (Jou et al. 1996: 2, Fong and McCabe 1999: 10927). Prospect theory explains risk attitudes given a particular frame – risk aversion in a gains frame and risk-seeking in a losses frame, as in the disease problem – but it does not clarify *why* (and when) the frame exerts power over decision-making. Jou et al. (1996: 9) fill this gap by showing that "framing is a form of manipulating the salience or accessibility of different aspects of information" (also see Levin et al. 1987: 52-53).[7] The frame induces individuals to think in terms of losses or gains by making the given domain accessible in their memory, which in turn, drives their risk attitudes. Accessibility involves "passive, unconscious processes that occur automatically and are uncontrolled" (Higgins and King 1981: 74).

This explains risky choice framing; however, starting with prospect theory makes little sense for attribute framing since it does not involve changes in risk attitudes (Levin et al. 1998: 166). Levin et al. (1998: 164-166), nonetheless, explain that attribute framing occurs through a similar accessibility process where "the positive labeling of an attribute leads to…favorable associations in memory [and] negative labeling of the same attribute

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[7] Accessibility is similar to but not the same as Tversky and Kahneman's (1973) availability heuristic (Higgins and King 1981: 71). Others use accessibility to explain survey question order effects and over-time response instability (Zaller 1992). These are distinct phenomena (Druckman 2001b: 231); moreover, Lacy (2001, 2003) offers a persuasive theory and evidence that these effects are due to non-separable preferences. Similarly, other types of framing effects (see note 1) might not work through accessibility (see Nelson et al. 1997).

…evokes unfavorable associations," and this in turn shapes overall evaluations. For example, in the employment problem, the term "unemployment" makes unfavorable associations relatively more accessible, and this shapes preferences.

I now turn to work on accessibility that specifies conditions under which the processes occur (see, e.g., Higgins and King 1981, Wyer and Srull 1989, Fazio 1995). I build on four lessons. First, by highlighting negative or positive information, the frame leads individuals to *subconsciously* focus on that information (e.g., lives lost or lives saved, unemployment or employment) and this leads to the given (negative or positive) evaluation/preference. Second, under certain conditions, individuals do *not* assimilate the accessible information (i.e., do not focus on the negative or positive information) – it is these conditions that will moderate and possibly eliminate a frame's impact. Third, one moderator lies with the *individual*. Accessibility research shows that individuals who possess the motivation to think more deliberatively about the problem at hand will be more likely to engage in conscious processing (e.g. Fazio 1990, Thompson et al. 1994: 475, Stapel et al. 1998).[8] If they complement this motivation with the ability to envision and consider alternative frames, then they will envision positive and negative frames, and this will temper the impact of the initial frame.[9] This is consistent with substantial research that shows how motivation and ability combine to shape performance and decision-making (e.g., Anderson and Butzin 1974, Payne et al. 1993, Petty et al. 1997, Kuklinski et al. 2001).

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[8] Fazio (1990) explains that individuals also must have the opportunity to consider alternative conceptions (i.e., more than a few seconds).
[9] The accessibility literature focuses on how motivation affects simple tasks such as interpreting an ambiguous stimulus in different ways (e.g., is someone who rarely changes his/her mind self-confident or stubborn?). In these exercises, there is no inherent ability requirement to the different interpretations. In applying this work to framing, it is important to recognize that overcoming framing also involves the ability to envision the alternative frame. This follows work on rational decision-making (e.g., Larrick et al. 1993, Payne et al. 1993), and work on systematic processing (e.g., Petty et al. 1997).

Fourth, *context* also can interrupt accessibility assimilation – if alternative information (i.e., the other frame) is provided in the context, then individuals can use it and will be less influenced by the initially accessible frame (see, e.g., Lombardi et al. 1987, Martin and Achee 1992; also see Jou et al. 1996: 8, Bless et al. 1998).[10] In this case, the context removes the onus on the individual to deliberatively reformulate the issue (i.e., motivation and ability are not necessary).

Predictions

I use this psychological portrayal to derive predictions about *when* framing effects will occur. I begin with context by considering two common political environments that shape citizens' opinions – (1) elite competition and rhetoric (e.g., Mutz et al. 1996, Kinder 1998), and (2) interpersonal conversations or deliberation among citizens (e.g., Huckfeldt and Sprague 1995, Mendelberg 2002). In terms of the former, elites have strong incentives to reframe issues that do not support their perspective, resulting in a process of framing and counter-framing (Riker 1995: 33, Wittman 1995: 43, Sniderman and Theriault n.d.). Counter-framing provides the reformulation thereby potentially eliminating the aforementioned subconscious assimilation process.[11] I predict that *individuals exposed to counter-framing (i.e., that includes the alternative frame) will be less susceptible to framing effects (from the initial frame) than those not exposed to counter-framing, all else constant*.[12] (I focus on relative comparisons with a control group, but also will examine the elimination of the framing effect.) Virtually all prior

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[10] Some work shows that when made consciously aware of the initially accessible information, individuals move in the opposite direction (e.g., Strack et al. 1993). However, in the case of counter-framing (where individuals are not simply made conscious of the initial accessible information but rather are given the other frame), there is no reason to expect such a contrast effect.

[11] In other work, I find that elites can attenuate framing effects by providing decision making cues (Druckman 2001c).

[12] Primacy or recency effects are possible, depending on the relative persuasiveness of each frame (Chong 2000); however, at a basic level with standard framing problems, I expect neither frame to dominate.

framing experiments preclude counter-framing by offering participants only a single

frame – as Riker (1995: 34) explains, "Such dueling is almost *universal* in the real world

but *unknown* in the laboratory" (emphasis added).

Interpersonal communication also could moderate framing effects, in a similar

way as counter-framing, when participants in the conversations introduce alternative

frames (Morrow 1994: 48, Riker 1995: 33-35, Wittman 1995: 43). I expect that this will

occur when the discussion group is heterogeneous, where it includes participants initially

exposed to different frames, as this will likely lead to the introduction of alternative

perspectives (Druckman and Nelson 2003, Visser and Mirabile 2003). I thus predict that

individuals who engage in conversations with a heterogeneous group will be less

susceptible to framing effects than those who do not engage in conversations, all else

constant.[13]

I also investigate the impact of homogenous discussion groups that include only

those exposed to the same frames.[14] On one hand, homogenous discussions will be less

likely to lead to the introduction of alternative frames, and thus, will not have the same

moderating impact as heterogeneous discussions (Visser and Mirabile 2003). In fact,

discussions among like-minded people might reiterate the frames, resulting in an

exaggerated effect (see Isenberg 1986: 1141, Mendelberg 2002: 159). On the other hand,

homogenous discussions tend to stimulate individuals to provide justifications for their

preferences (e.g., Mendelberg 2002: 153), and prior work suggests that the thought

generated from justifying one's preferences can limit framing effects (e.g., in some sense,

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[13] This is consistent with the finding that heterogeneous groups cause a depolarization of preferences such that initial preferences disappear as individuals' preferences move towards one another (see Vinokur and Burnstein 1978, Mendelberg 2002: 159).

[14] Recent work highlights the varying consequences of homogenous and heterogeneous groups (Mutz 2002, Visser and Mirabile 2003).

it produces expertise; see Miller and Fagley 1991, Takemura 1994, Sieck and Yates

1997). I thus do not have clear expectations about the impact of the homogenous groups.

When it comes to individual level variables, the two key constructs have clear

empirical analogs. For motivation to think deliberatively, Cacioppo and Petty's (1982)

need for cognition measure (NC) gauges an individual's "tendency to engage in and

enjoy thought… high levels of NC … more likely to process information in a careful,

elaborate fashion…" (Smith and Levin 1996: 284; also see Bizer et al. 2000). For ability

to visualize alternative frames, Larrick and his colleagues (1990, 1993; Nisbett 1993,

Riker 1995) show that individuals with training in economics and related areas engage in

more normatively rational decision-making and exhibit increased familiarity with

expected utility, which is the underlying structure of many framing problems (also

Ericsson and Charness 1994). I combine NC with training to differentiate "experts" with

high NC and high training from "non-experts." I predict that, *in contexts where framing*

effects occur, experts will be less susceptible to framing effects than non-experts, all else

constant.[15] This prediction does not apply to counter-framing or heterogeneous discussion

contexts where there will be either no effects to moderate or such small effects that there

will not be sufficient variance. In these contexts, the situation replaces the need for

expertise (see Kuklinski et al. 2001).[16]

[15] Smith and Levin (1996) examine NC as a moderator of framing effects (using different problems than I use below); they find framing effects only among low NC individuals. Other work shows that framing effects disappear when participants are induced to engage in more deliberative thinking (Miller and Fagley 1991, Takemura 1994, Sieck and Yates 1997).

[16] Expertise constitutes a distinct construct from age, student status, and profession (e.g., a financial planner, a physician). Thus, despite claims to the contrary (e.g., McNeil et al. 1982, Thaler 1991: 158), that prior work shows that these latter variables do not moderate framing effects is not surprising (see Riker 1995: 32-33, Wittman 1995: 44). Other research examines the impact of variables such as gender (Fagley and Miller 1990, 1997, Druckman 2001a), cognitive ability (Stanovich and West 1998), personal involvement (Levin et al. 1998: 160), and personality variables (Fagley and Miller 1990, Kowert and

Experiment

Participants, Design, and Procedure

To test these expectations, I implemented an experiment. A total of 580 individuals participated in the experiment in exchange for a cash payment and a snack. I recruited participants from a large, public University and the surrounding urban community by taking out newspaper advertisements, advertising in classes, sending e-mails, posting flyers, and contacting local community organizations. I invited participants to take part in a preference formation study at the University's Political Psychology Laboratory. While students constituted a majority of the sample, a substantial numbers of non-students also participated (approximately 28%).[17]

Each participant responded to four randomly ordered framing problems and a background questionnaire. Like other studies that include multiple problems (e.g., Miller and Fagley 1991, Jou et al. 1996, Wang 1996, Fagley and Miller 1997, Kowert and Hermann 1997), I instructed participants to treat the problems independently and imagine that they were being confronted with each scenario. To ensure robustness, I selected four widely cited problems that varied across domains and type of framing effect (i.e., attribute and risky choice framing; see Levin et al. 2002 on using multiple problems). Each problem could be framed in either a negative or positive light.

Specifically, I used two attribute framing problems: (1) the aforementioned *employment* problem (Quattrone and Tversky 1988: 727-728), and (2) a problem focusing

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Hermann 1997). These results are mixed. There also is work that varies problem specific details such as specific stakes (see, e.g., Kühberger et al. 1999).

[17] The average age was about 27. Other sample statistics include: 51% females, 87% Caucasian, 13% political science majors (of the students), 50% self-identified Democrats, and 24% self-identified Republicans. There also was reasonable variance on political variables such as trust in government and political interest. Details about the sample, and all other analyses presented below are available from the author.

on programs to combat youth *crime* (Quattrone and Tversky 1988: 728-729). The crime problem appears in Appendix A, and in short, it asked respondents to choose between two programs that distribute varying amounts of money to combat youth crime to two communities. One frame describes the communities in terms of the percentage of youth with no criminal records (positive frame) while the other reports the percentage with criminal records (negative frame).

I also used two risky choice framing problems: (3) a problem that focuses on how to *invest* a community grant (Tversky and Kahneman 1987: 74-75)[18], and (4) the previously described *disease* problem (Tversky and Kahneman 1981: 453). For the investment problem, which also appears in Appendix A, respondents voted on how to invest a community grant – it can either be invested in a risk-averse definite return or a risk-seeking probabilistic return. The frames involve gaining money from a base amount (positive) or losing money from a base amount (negative).

For all four problems, participants expressed a preference for one of two alternatives. A framing effect occurs when, compared to individuals who receive a positive frame, individuals who receive a negative frame are significantly more likely to express a "negative preference" (on a particular problem or across problems) – that is, a preference for the alternative predicted by the negative frame. For example, for the original disease problem, those who received the negative (dying) frame were, as expected, significantly more likely to express a negative preference for the risk-seeking program B (78%; n = 155), than those who received the positive (saved) frame (28%; n = 152; for a two-tailed difference of proportions test, $z = 8.78$; $p \leq .01$). This difference in the percentages is called a preference shift (e.g., a 50% preference shift). For the

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[18] I use a variation of the Tversky and Kahneman's (1987: 74-75) investment problem.

employment problem, program K constitutes the negative preference; the predicted

preferences for the other two problems are described in Appendix A.[19] Prior research on

all four problems shows significant framing effects; the question is if the effects

withstand variations in context and expertise.

I randomly assigned each participant to one of eight conditions that varied the

frame – positive or negative – *and* the context – control, counter-framing, homogenous

group, heterogeneous group. As in prior work, participants assigned to a positive framing

condition received all problems using a positive frame while those in a negative framing

condition received all problems with negative frames (Fagley and Miller 1997).

Participants dealt with one problem at a time and expressed a preference for each

problem by checking an alternative on a separate page that followed the particular

problem. For each problem, participants also recorded how confident they were that their

"choice is the best possible choice" (Sieck and Yates 1997, Kuklinski et al. 2001).

Participants assigned to the control group simply answered the problems and

background questionnaire.[20] Participants assigned to the counter-framing condition

received – for each problem – not only the original problem but also a reframing of the

problem that uses the opposite frame (e.g., for participants in the positive frame

condition, the re-framing always uses the negative frame; notice that the framing

condition refers exclusively to the initial frame). In addition to re-casting the problem in

terms of the alternative frame, the counter-frames, to some extent, draw out the

connection between frames (e.g., for the employment problem, it states that 90%

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[19] This is a conventional framing effect measure (Tversky and Kahneman 1987); an alternative approach focuses on if the frames cause significantly more (or less) than 50% of participants to choose a given program (see Wang 1996, Druckman 2001a).
[20] Because I expected expertise to moderate the control group, I over-sampled the control group.

employment implies 10% unemployment) (see Jou et al. 1996 on motivation for this approach). The re-framing occurs prior to the participants being asked to formally express their preference.[21] Appendix B provides the specific counter-framing problems.[22]

Participants assigned to one of the group conditions received the original problems (only), after which they engaged in a conversation with three other participants (a few groups included two or four other participants, but these differences proved insignificant). They were told that the other members of the group faced the same situation, and that they should not share their questionnaires. Moreover, I instructed them to read the problem and then have each person in the group speak about the problem, in whatever order they would like. (A participant could pass on speaking.) They next had five minutes of open discussion during which time they could discuss anything. After the discussion, participants expressed their preference for the particular problem. I emphasized that they should state their individual preference and not the group's sentiment, and also reiterated that their questionnaires and preferences were private.[23]

In the homogenous group conditions, all participants received the same (positive or negative) frame. In the heterogeneous group conditions, two participants received each

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[21] Dawes (1988: 37) reports the persistence of framing effects after individuals express their preferences and are told about the effect (also see Levin et al. 1987, Loke 1989). However, this confounds preference formation processes with dissonance and experimental demand issues. My design also differs from prior work that offers participants both frames simultaneously in a mixed format (e.g., McNeil et al. 1988, Druckman 2001a). For relevant discussion, see Jou et al. (1996: 6).

[22] I also assigned a small number of participants to other conditions that mimicked the counter-framing conditions, except that instead of providing the alternative frames, the initial frames were reiterated. That the results from these conditions were generally consistent with the control group and not the counter-framing condition is evidence that it was the content of the counter-framing conditions that mattered and not simply its length.

[23] I structured the discussions in this way because it ensured manageably sized groups that potentially had a mix of opinions. Also, providing each participant an opportunity to speak followed by open discussion balances realistic discussion settings with characteristics of deliberative settings (Sanders 1997: 348). Also note that Stasser and Titus (1985), Pease et al. (1993), and Tindale et al. (1993) study framing and group discussion, but their focus is on the collective group decision and not on how the group discussion directly affects individuals' preferences.

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respective frame. While it may be the case that individual participants were not affected by the initial frame they received, I can confidently say that the heterogeneous groups will, on average, contain a greater mix of opinions than the homogenous groups and thus be more likely to include a counter-framing scenario.[24]

In all conditions, once participants finished the four problems, they completed a questionnaire that asked a variety of demographic, social, and political questions. This included a two item NC measure, also used by Bizer et al. (2000) (see Appendix C), where higher scores indicate greater NC. It also asked participants to report the number of economics and statistics courses they had taken (see Larrick et al. 1993, Nisbitt 1993). I classified participants as experts if they were above the median for both NC and the number of classes since I expect a moderating effect only for individuals who possess both motivation and ability (e.g., Payne et al. 1993: 112).[25]

In the analyses that follow, I also will control for if the participant is an adult or not. Prior work finds robust effects across students and non-students (i.e., adults; see Kühberger 1998: 36); however, a common criticism of experiments is that they include only student samples (Sears 1986). Here, I can directly assess if being an adult mattered.[26]

[Table 1 About Here]

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[24] Finding significant effects in the control condition would confirm this – that is, that the initial frames do, on average, shape participants' preferences (see Druckman and Nelson 2003: 733 for additional discussion).

[25] I use median splits to minimize measurement error. Despite fairly reliable alpha scores, I expect that the measures (especially NC) contain error such that slight changes on the respective scales may not accurately capture real differences. In contrast, a median split allows me to focus on what are more likely to be qualitatively distinct groups (e.g., Smith and Levin 1996, Miller and Krosnick 2000).

[26] Sears (1986: 521) explains that the potential problem with using students concerns their stage of development rather than their student status *per se*. Thus, rather than focus on school enrollment, I classify anyone over the age of 23 as an adult and anyone 23 years old or younger as a non-adult. Results are robust to using a strict student variable or other nearby age cutoffs.

In Table 1, I summarize the experimental conditions (and number of participants), problems, and predictions. The top two rows list the eight randomly assigned conditions. The first column includes the four problems to which participants responded (in a random order). The predictions, which account for expertise in the control and homogenous groups, are invariant across problems. In addition to examining if the alternative contexts moderate the effects relative to the control group, I also will see if they eliminate the framing effects.

Overall Framing Results

I begin with an analysis of overall framing effects by combining responses to the four problems. For each participant, I add up the number of negative preferences expressed – that is, the number of preferences (0 through 4) that were consistent with what would be expected if they had been given a negative frame (e.g., program B for the disease problem, program K for the employment problem). An overall framing effect occurs if those who received the negative frames express significantly more negative preferences than those who received the positive frames (for the same approach, see Fagley and Miller 1990, Miller and Fagley 1991, Jou et al. 1994, Fagley and Miller 1997; also see Sieck and Yates 1997).

[Table 2 About Here]

In Table 2, I report the distribution of negative preferences broken down by frame received, but not by contextual condition. Only 14% of respondents exhibited all negative or all positive preferences. More importantly, the table reveals an overall framing effect with twice as many negatively framed, as opposed to positively framed, participants

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opting for a majority (3 or 4) of negative preferences (34% versus 17%; $\chi^2_4 = 50.63$; $p \leq$ .01)

To examine the robustness of the effects across conditions, I run a series of ordered probit regressions with the number of negative preferences as the dependent variable. I present the results in Table 3.[27] The first regression shows, again, that across conditions, those who received the negative frame (0 = positive frame, 1 = negative frame) express significantly more negative preferences. In model 2, I add dummy variables for each contextual condition (0 = not in the given condition, 1 = in the condition), and interactions between each condition and the frame so as to see if the context moderates the impact of the frame.

[Table 3 About Here]

The results strongly support my hypotheses. Specifically, the significant negative interactions between counter-framing and frame, and heterogeneous group and frame suggest that the framing effects are significantly smaller in these contexts than in the control group. Also, the significant interaction between homogenous group and frame implies that the group does in fact moderate the effect, although the interaction is only marginally significant ($p \leq .1$).[28] I assess the substantive impact for each condition in Figure 1, by plotting the predicted probabilities of expressing a majority of negative preferences, for each frame.[29]

[Figure 1 About Here]

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[27] A version of Table 2 that presents negative preferences separately for each condition mimics the ordered probit results. Also, five participants failed to express preferences on all of the problems and thus are excluded from the analyses.

[28] The significant main effect for counter-framing suggests that that counter-framing caused respondents to express more negative preferences, regardless of the frame received.

[29] I do this using *Clarify* (Tomz et al. 1999) based on model 2. I do not plot standard deviations because *Clarify* provides probabilities for each dependent variable value (0 through 4), and I sum the probabilities for 3 and 4. The results are consistent using different breakdowns.

The figure shows a substantial (and statistically significant) framing effect for the

control group, with those receiving the negative frame having a .47 predicted probability

of expressing a majority of negative preferences compared to just .12 for those receiving

the positive frame. In contrast, the counter-framing condition eliminates the framing

effect, revealing a non-significant .7 shift between frames (also see Kuklinski et al. 2000:

811, Sniderman and Theriault n.d.).[30] The group results show that those conditions,

especially the heterogeneous groups, significantly minimize the framing effects;

however, the effect is not entirely eliminated.[31]

In model 3, I add main and interactive variables for adult status and expertise.[32] I

find no significant main or interactive effects. Framing effects apparently do not differ

between non-adults and adults (also see Kühberger 1998: 36), and, counter to my

expectations, experts are as susceptible to framing effects as non-experts. (This is

confirmed with an examination of the substantive effects.)

Recall that I also expected expertise to moderate framing effects in the

homogenous group (where the effects are relatively smaller, but still significant). I

investigate this possibility with model 4 where I add a three-way interaction between

expertise, homogenous group, and frame. The highly significant negative coefficient

suggests that in the homogenous groups, framing effects were significantly greater for the

non-experts. Also, the two-way homogenous-frame interaction is no longer significant.

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[30] Using *Clarify*, I find that none of the probabilities for the five values of the dependent variables are significantly different (at the .05 level) across frames. I also find that the counter-framing framing effect is significantly relatively smaller than the control group's effect (using a difference of differences of proportions test).
[31] For both group conditions, four out of the five predicted probabilities for the five dependent variable values are significantly different across frames. I also find that for both groups the framing effects are significantly relatively smaller than the control group's effect (at least at the .1 level).
[32] The number of observations drops slightly because some participants did not respond to the need for cognition questions.

[Figure 2 About Here]

In Figure 2, I plot the predicted probabilities of expressing a majority of negative preferences, by frame, for experts and non-experts in the control and homogenous discussion conditions.[33] Clearly, expertise plays no role in the control condition, but substantially impacts framing in the homogenous condition with only non-experts exhibiting the effect (i.e., there are no apparent framing effects for the experts, and the effect for experts is significantly smaller than that for non-experts). Expertise thus depends on context. Deliberative theorists emphasize how conversations stimulate engagement, thought, and understanding (Mendelberg 2002: 153), and in this case, it appears that this extra stimulation is necessary and sufficient to induce experts to think through the framing problems.[34]

In sum, in sharp contrast to wide-spread claims, framing effects are *not* robust across political contexts. Both counter-framing and heterogeneous discussions minimize framing effects, and in the former case, the effects are eliminated. Homogenous discussions also reduce and eliminate the effects for experts, showing that individual level moderators depend on context (i.e., individual variables do not moderate the effects in the control group).

Problem-Specific Framing Results

I next examine the results for each problem separately. In Table 4, I report the predicted probabilities of expressing a negative preference for each problem, controlling

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[33] I exclude the adult variable for the regression on which the graph is based; however, results are robust to including it and setting it at a variety of values.

[34] While I did not expect expertise to matter in the other conditions, I ran other regressions that included expertise interactions with counter-framing and/or heterogeneous groups (where small framing effects occurred). None of these interactions approached significance. I also find no significant interactions between adult status and the conditions. Finally, the results are robust to including a variety of other controls including the order in which respondents received the problems (also see Miller and Fagley 1991).

for preferences on the other problems.[35] A significantly greater probability for the negative frame than the positive frame indicates a framing effect.[36] The results reinforce the overall analyses.

[Table 4 About Here]

First, all problems exhibit significant framing effects in the control group; moreover the preference shifts (negative frame probability – positive frame probability) closely resemble those found in the original employment (18% in original, 15% here), crime (30% in both), and investment (36% in original, 41% here) experiments. The disease problem shift of 18% is substantially smaller than Tversky and Kahneman's (1981) original 50%; however, it matches other replications (e.g., Miller and Fagley 1991, Bohm and Lind 1992, Druckman 2001a). The relatively smaller disease result also is counter to the expectation that larger effects occur when the problem involves lives rather than money (Fagley and Miller 1997).

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[35] I focus on between frame comparisons within contextual conditions. I do so because my focus is on preference invariance – that is, "different representations [descriptions] of the same choice problem should yield the same preference" (Tversky and Kahneman 1987: 69). If I find between frame differences within a contextual condition, it is a clear violation of invariance. This is not the case if I find differences across conditions. Cross-condition comparisons between the control condition and another condition are inappropriate because I do not expect invariance to hold in the control group, and thus there may well be differences with other conditions. Cross-condition comparisons between either of the discussion conditions and another condition are inappropriate because preferences in the discussion conditions may have been influenced by the discussions themselves. If so, this is not a violation of invariance. Thus, differences across conditions (comparing either frame) do not indicate an invariance violation.

[36] For each problem in each condition, I ran probit regressions of preference on frame and preferences on the other three problems. This ensures that the problems can be treated independently (see Frisch 1993: 419 and Levin et al. 2002 for evidence that problems are typically seen as independent). I then used *Clarify* to produced predicted probabilities of selecting the negative program (setting other problem preferences at their mean). The results are robust to the inclusion of a host of other controls (including the order in which problems were received), and they also mimic the basic percentages of participants opting for each alternative for each problem.

I focus here on the elimination of framing effects rather than on relative comparisons with the control group. I do so because, in several cases, the control framing effect is small enough that a no-framing effect finding in another condition would not be significantly smaller than the control group's effect (and, moreover, a reverse framing effect in the other condition could significantly differ from the control even though this would be contrary to my prediction; see Takemura 1994, Smith and Levin 1996). Analyses of relative comparisons are generally consistent.

Second, I find no significant effects in the counter-framing condition. Third, for the homogenous group, I find significant effects for all but the crime problem, and the effects are even a bit larger than the control group for the employment and disease problems. In other analyses – consistent with the prior results – when I separate experts from non-experts, I find significant (substantive) effects only for non-experts for the crime, investment, and disease problems.[37] Otherwise, expertise as well as adult status does not moderate any of the results.

Finally, the heterogonous group exhibits just one significant framing effect – on the investment problem. This is not surprising since the two versions of the investment problem are not transparently equivalent – while the expected values of the outcomes are the same, the problems differ insofar as there are different initial endowments (see Appendix A). It seems quite plausible that the groups were unable to communicate the alternative frames to one another, in contrast to the counter-framing condition where the alternatives are explicitly provided. Indeed, Frisch (1993) finds that a majority of respondents in this type of problem do not think they should be treated as equivalent.[38] This explains the small framing effect found in the overall analyses for the heterogeneous groups. In sum, the problem specific results not only match the overall results, but they also explain the overall heterogonous finding (i.e., that the effect is not entirely eliminated).[39]

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[37] The overall insignificance of framing for the crime problem in the homogenous group stems from experts exhibiting a reverse framing effect with 36% of experts who received the negative frame expressing a negative preference compared to 67% of experts who received the positive frame. In contrast, the non-experts demonstrated a substantial framing effect.

[38] Informal conversations with participants are consistent with this interpretation.

[39] The counter-framing and heterogeneous discussion results may reflect extreme sensitivity to frames. For example, the initial frames in each condition might push participants in one direction and then the introduction of the other frame induces them back. This would not, however, contradict my argument that different contexts prevent violations of preference invariance and limit manipulation by single frames (also

One other noteworthy finding concerns the risky choice framing problems. Since these problems involve risk, I measured participants' standing risk attitudes. Using four items from Zuckerman's (1994; see Bromiley and Curley 1992: 123) sensation seeking scale, I created a measure where higher scores indicate a divergence from risk-neutrality (either towards being more risk-averse or more risk-seeking) (see Fagley and Miller 1990). I expect that risk-neutral oriented participants will be more susceptible to framing effects since their prior risk attitudes are not strong in either direction and therefore are more open to risk manipulation (see Kowert and Hermann 1997).

For the control group, I find a large framing effect for the disease problem among risk-neutral individuals (a 43% shift) and no framing effect for maximum risk-seeking or risk-averse individuals (a -1% shift). I do not find analogous results for the investment problem, however. This finding is intriguing insofar as participants cannot possess prior beliefs about these hypothetical problems *per se*; however, the risk-attitude measure resembles a prior attitude for the risky choice problems. The implication is that less hypothetical framing problems might be moderated by individuals' prior beliefs.[40]

Confidence Results

I extend the analyses by examining the confidence participants expressed in their preferences. Increased confidence causes individuals to: deepen their commitment to their preference, take action based on their preference, ignore and not pursue additional information, and resist persuasion (Sieck and Yates 1997: 218; also see Cobb and

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see Jerit and Barabas 2003, Sniderman and Theriault n.d.). (Of course, another question concerns the ultimate basis of citizens' preferences). An alternative design would involve measuring preferences after exposure to the initial frame, and then again after exposure to the counter-frame or the discussions. The potential problem, however, is that participants may adhere to their initial preferences so as not to appear inconsistent within the experiment (i.e., the design conflates preference formation processes with demand and dissonance processes). Indeed, this is what Levin et al. (1987) and Loke (1989) find.

[40] Other political and demographic variables do not moderate the results.

Kuklinski 1997, Kuklinski et al. 2000). Confidence is clearly consequential, and how framing affects confidence has gone unexplored.

I focus on whether individuals who are susceptible to framing effects – and thus lack coherent preferences – express increased confidence in their judgments.[41] I expect this could be the case because individuals susceptible to framing are less likely to recognize conflicting ways of thinking about the problem (e.g., in terms of losses or gains). As a result, they avoid conflicted impressions that often work to decrease confidence (Tetlock 1986).[42] Over-confidence in incoherent preferences also would be consistent with a well documented over-confidence bias (e.g., Payne et al. 1993: 209, Russo and Schoemaker 1992, Kuklinski et al. 2000).

[Table 5 About Here]

As mentioned, participants rated the confidence they had in each of their preferences on a 7-point scale ranging from a low score of "not confident at all" to "moderately confident" to a high score of "very confident." I report the average and median confidence scores for each problem in Table 5. I used ordered probits to analyze how various factors impact confidence scores. My key variable measures if a participant's preference on a given problem matches the preference that would be predicted from the frame the participant received. For example, in the disease problem, agreement occurs if a participant who received the negative (positive) frame expressed a preference for the risk-seeking (risk-averse) program. I call this "frame agreement," and it equals 1 if the participant's preference agreed with the frame and 0 if not. While a score

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[41] A few framing studies explore confidence in a variety of ways (e.g., Levin et al. 1987, 1988, Loke 1989, Tindale et al. 1993, Sieck and Yates 1997); however, none examine how being affected by a frame impacts preference confidence.

[42] Framed participants also might see their preferences as following more naturally from the frame (e.g., Fischhoff 1983, Tversky and Kahneman 1987: 90).

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of 1 is not sufficient evidence that the frame had a causal impact, it is a necessary condition and is the most direct measure available.

I include dummy variables that measure if the participant (randomly) received the particular problem second, third, or fourth. Repetition, even absent actual increases in skill, can increase confidence (Oskamp 1965), and, as a result, receiving a problem later in the order might enhance confidence. I also capture this by including the adult variable since it may relate to experience.

I include expertise, following prior work that shows that systematic information processing often produces increased confidence in judgments (Eagly and Chaiken 1993, Sieck and Yates 1997; however, also see Thaler 1991: 158). Additionally, I include the experimental condition variables. Experimental evidence suggests that discussion stimulates confidence (Tindale et al. 1993); significant discussion condition effects also would be consistent with the argument that deliberation increases thoughtfulness and opinion justification – both of which can increase confidence (see Kinder and Herzog 1993, Mendelberg 2002: 153). Finally, I add an interaction between frame agreement and the homogenous discussion group. I do so because framing effects occurred in this condition, and it also may be the case that agreeing with the frame along with supportive discussion could boost confidence.[43]

[Table 6 About Here]

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[43] I do not expect interactions with the other conditions since framing effects themselves are limited in those conditions, and thus, the frame agreement variable will be less salient. Nonetheless, in other analyses, I explored interactions with the other conditions and found no significant results. I also examined a variety of other interactions such as between expertise and frame agreement; expertise and condition; expertise, frame agreement and condition (for each condition); and problem order with various other variables. I find no significant results.

I present the results in Table 6; I analyze each problem separately to account for problem order and frame agreement. There is clear evidence that having preferences consistent with a framing effect bolsters an individual's confidence in those preferences. In the case of the employment problem, this occurs as a main effect; for the other three problems, agreement complemented by homogenous discussions causes participants to increase their confidence (and they are substantively significant). In Figure 3, I plot the probability of an individual expressing above median confidence scores, for each problem, differentiating those whose preference agreed with the frame from those whose preference disagreed.[44] The effects are non-trivial, ranging from a 7% shift to a 30% shift.[45]

[Figure 3 About Here]

While the causal mechanisms remain unclear (as mentioned), the results suggest that individuals who might base their preferences on frames – those who hold incoherent preferences – express increased confidence in those preferences. An added twist is that discussion sometimes works to generate over-confidence. The message for deliberative theorists is that homogenous discussions act as a double-edged sword – they work to eliminate framing effects among experts; however, when the effects persist, they stimulate and reinforce an over-confidence in framed preferences (e.g., groupthink). Deliberation might lead to justification for preferences but this is not a positive outcome if the preferences are baseless.

In other analyses, I find no interactive effects for expertise on confidence. Moreover, counter to my expectations, the results show that expertise directly influenced

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[44] I use *Clarify*, setting other variables at their means.
[45] Examining the predicted probabilities for all 7 values of the dependent variables, I find significant differences across values for the investment and disease problems, and marginally for the crime problem.

confidence only for the employment problem, and adults moved in the opposite direction than predicted for the disease problem. Otherwise, I find that order mattered in the expected direction (see Fong and McCabe 1999: 10931), but only for the attribute framing problems. The experimental conditions have no direct effects on confidence; deliberation by itself is not sufficient to increase confidence, and counter-framing also has no effect.

In sum, the evidence points to a disturbing over-confidence bias (also see Kuklinski et al. 2000). Experts – who perhaps should have more confidence – are generally not more confident. On some problems, confidence grows from experience with prior problems (the order result) even though it is not clear that learning actually occurs (e.g., there is no feedback). And in certain contexts – particularly homogenous deliberative situations[46] – those who might be vulnerable to framing effects exhibit increased confidence. These individuals might be relatively committed to incoherent preferences. Overall, while framing appears much less relevant to political contexts than often presumed, when it does occur, it might be even more deleterious than previously thought.

Conclusion

Social scientists initially viewed framing effects as an intriguing empirical anomaly. While it was immediately evident that framing violated the first premises on

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[46] I find marginal evidence that increased agreement among discussion group members (i.e., the more group members agree with one another) enhances confidence. This occurred for the employment and disease problems in the homogenous groups and for the crime problem in the heterogeneous groups. Also, for the homogenous group, the interaction between frame agreement and group agreement enhances confidence only for the investment problem. I suspect that the lack of stronger interactive effects stems from low variance in group agreement. (Also, there is a high amount of agreement in general; across problems for the homogenous group, 72% of other group members shared an average individual's preference). The general homogenous environment was agreeable and reiterative, and small changes in that environment do not seem to matter.

which a considerable amount of social science is built (Russell and Thaler 1991), scholars did not readily abandon the fundamental concepts of preferences and stable attitudes. For example, in one of their initial articles, Tversky and Kahneman (1981: 457) acknowledge that the effects are "systematic, although by no means universal." Over time, however, this sentiment changed such that nearly twenty years later, the authors describe framing as "ubiquitous" and "prevalent" (e.g., Kahneman and Tversky 2000). Many social scientists – including political scientists – have come to agree, and consequently, they have moved away from assuming that individuals maintain invariant preferences and/or stable attitudes (e.g., Zaller 1992). This has tremendous consequences not only for the study of political, social, and economic behavior, but also for theories of democratic responsiveness and market competition (Russell and Thaler 1991, Bartels 2003).

I argue that it is premature to abandon the concepts of preferences and attitudes, and to accept the concomitant implications. My results show that framing effects depend in critical ways on context – as a result, framing effects appear to be neither robust nor particularly pervasive. Elite competition and heterogeneous discussions limit and often eliminate framing effects. Homogenous discussions do the same among experts. These are meaningful contexts insofar as elite rhetoric and debate, and interpersonal conversations define many political environments (e.g., Riker 1995, Mutz et al. 1996, Huckfeldt and Sprague 1995, Mendelberg 2002) – in fact, they presumably better mimic political (and economic) situations than the traditional (control group) experiments (Morrow 1994, Riker 1995, Wittman 1995).

It also is important, however, to acknowledge that in the case of interpersonal discussions, homogenous groups may be more common (Mutz and Martin 2001; c.f.,

Huckfeldt 2001, Huckfeldt, Morehouse, and Osborn n.d). Moreover, my findings show that homogenous discussions not only fail to limit framing effects among non-experts, but they also induce an over-confidence bias. The results therefore should not be taken as indication that framing effects never occur or do not matter – indeed, when they occur, they can induce over-confidence in incoherent preferences. The point is simply that they are not as pervasive as often assumed.

I conclude by discussing implications for rational choice, political psychology, and experimental design. In applying rational choice models to politics and, assuming invariant preferences, scholars often dismiss framing effects citing a lack of experimental realism, inadequate participant incentives, or other fairly *ad hoc* reasons (e.g., Kreps 1990: 19-22, Morrow 1994: 48, Riker 1995, Lupia and McCubbins 1998: 23; also see Thaler 1991). For example, Wittman (1995: 44) states, "the framing effect is unlikely to be so powerful in real life… I have never run any of these experiments… subjects are fooled by frames only when they are inexperienced." A more constructive approach – than asserting the application of invariant preferences – is to undertake empirical work that explores the robustness of rational choice assumptions in different contexts with various individuals. This would not only provide an assessment of which theories are appropriate in different situations, but it also would provide insight into the meaning of individuals' preferences in varying contexts. Psychological work on decision-making has progressed to a point where deductive models of choice need not always rely on traditional rational choice assumptions, and this opens an opportunity for an enhanced theory of choice and strategy with stronger psychological foundations (Satz and Ferejohn 1994, Camerer 1999, 2003, Turner 2001).

Just as rational choice approaches might benefit from a deeper appreciation of context, so would political psychological approaches. Extant framing effect results are not sufficient to indiscriminately dismiss preferences and attitudes, as many political psychologists have done (see Kuklinski and Jerit 2001: 344 for discussion). More importantly, in recent years, political psychologists have focused on individual heterogeneity in reasoning, particularly sophistication; Sniderman et al. (1991: 8) refer to this as the heterogeneity hypothesis (also see Sniderman 1993: 223). While such individual heterogeneity matters, variations in context are of equal consequence. Lewin (1936) made this point over sixty years ago, but with few recent exceptions[47], there has been little attention to contextual influences and their interaction with individual level variables (see Kuklinski 2002: 6, 8, Kuklinski and Quirk 2003: 2). This is particularly significant given the amount of political psychological research that uses data from the unique and typically constant context of conventional public opinion surveys. As Sniderman et al. (1991: 264-265) explain, "the conventional survey interview, though well equipped to assess variation among individuals, is poorly equipped to assess variation across situations" (also see Brady 2000). The expertise results suggest that individual heterogeneity and contextual variations also interact with one another.

A final implication concerns experimental design and inference. I argue that the widespread impact of framing effect results – despite the possibility that they are not particularly robust – reflects two inferential problems. First, scholars often focus on experiments with statistically significant framing effects (Kühberger et al. 1999: 223), and such experiments may be over-represented in journals (due to a publication bias

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[47] See Sniderman et al. (1991: 264-266), Lupia and McCubbins (1998), Sniderman (2000), Kuklinski et al. (2001), Lau and Redlawsk (2001), Baum (2002), and Sniderman and Bullock (n.d.).

towards positive findings; however, see, e.g., Bless et al. 1998). This can lead to a "file-drawer" problem such that skewed attention to positive findings results in an overestimation of an effect and its prevalence (Scargle 2000). Moreover, a lack of attention to negative findings inhibits an understanding of the determinants of framing effects (see King et al. 1994: 129 on a similar problem of selecting on the dependent variable).

The second inferential problem concerns external validity or the generalization of the results. The initial framing experiments involved testing alternative theories of choice (i.e., expected utility and prospect theory), and this minimized the salience of external validity concerns (Cook and Campbell 1979: 83). As the results gained notoriety, the effects and experiments became more applied to public policy and other areas (Kahneman 2000: xv). This enhanced the relevance of the work's external validity (Cook and Campbell 1979: 83), and, as discussed, confidence in the generalizability of the results stemmed from replications across issues with non-student subjects and so-called experts.

Experimental samples receive an inordinate amount of attention and criticism when it comes to the external validity of experiments in political science (e.g., Sears 1986), and many basically equate sample representativeness with external validity (see Kuklinski and Quirk 2003: 2 for discussion).[48] Caporaso (1995: 460) explains that external validity is "often reduced to a sampling instability issues" (see, e.g. McDermott 2002: 37). However, external validity involves much more than the sample – it also includes generalizing across settings, times, and stimuli (Cook and Campbell 1979: 71).

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[48] Others focus on the artificial settings in many laboratory experiments (e.g., Kinder and Palfrey 1993). The flip side of this criticism is that experimenters have more leeway in varying the setting than is available with other methodologies such as public opinion surveys.

When making assumptions about how people reason and when making inferences from empirical research, scholars need to carefully consider the context under study – perhaps, to an even greater extent than the population. In the case of framing, invariance in individuals' preferences depends on variations across settings.

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Appendix A

Crime Framing Problem: Positive Frame

The country of Delta is interested in reducing the crime rate among its immigrant groups. The Department of Justice has been allocated $100 million ($100M) for establishing a crime prevention program aimed at immigrant youth. The program would provide the youth with job opportunities and recreational facilities, inasmuch as criminal acts tend to be committed by unemployed youth who have little to do with their time. A decision must be made between two programs currently being considered.

The programs differ from each other primarily in how the $100M would be distributed between Delta's two largest immigrant communities, the Alphans and the Betans. There are roughly the same number of Alphans and Betans in Delta. *Statistics have shown that by the age of 25, 96.3% of all Alphans have no criminal record, whereas 98.8% of all Betans have no criminal record.*

The following two programs are being considered. *Program S* would allocate $55M to the Alphan community and $45M to the Betan community. *Program T* would allocate $65M to the Alphan community and $35M to the Betan community. The following table summarizes these alternative programs:

Program	To Alphan Community	To Betan Community
Program *S*	$55M	$45M
Program *T*	$65M	$35M

Imagine you were faced with the decision between program S and program T. In light of the available crime statistics, which would you select – program S or program T?

Crime Framing Problem: Negative Frame
Equivalent to positive frame except for the following passage:

…Statistics have shown that by the age of 25, 3.7% of all Alphans have a criminal record, whereas 1.2% of all Betans have a criminal record.

- Program *T* is the negative preference. (In the original experiment, Quattrone and Tversky 1988: 728-729 find 59% of respondents opt for Program *T* when given the negative frame and 29% do so when given the positive frame.)

Investment Framing Problem: Positive Frame

Imagine that the community where you live was given a $3,000 government grant for future community development. The community *must* however immediately invest the grant in one of two programs, and everyone agrees that the estimated impact of each program is as follows:

If *program Y* is adopted, your community will gain $1,000.

If *program Z* is adopted, there is a 50% chance that your community will gain $2,000 and a 50% chance that your community will gain nothing.

Which program would you vote for – program Y or program Z?

Investment Framing Problem: Negative Frame

Imagine that the community where you live was given a $5,000 government grant for future community development. The community *must* however immediately invest the grant in one of two programs, and everyone agrees that the estimated impact of each program is as follows:

If *program Y* is adopted, your community will lose $1,000.

If *program Z* is adopted, there is a 50% chance that your community will lose nothing and a 50% chance that your community will lose $2,000.

Which program would you vote for – program Y or program Z?

- Program *Z* is the negative preference. (In the original experiment, Tversky and Kahneman 1987: 74-75 find 64% of respondents opt for Program *Z* when given the negative frame and 28% do so when given the positive frame.)

Appendix B

All counter-framing problems begin with the control group version of the problem. In what follows, I provide the re-framing for each problem for the case where the initial frame was positive (i.e., the re-framing presents the negative frame). The re-framing of initial negative frames is analogous.

Employment Framing Problem: Re-framing of Initial Positive Frame

…Image you were faced with the decision of adopting program J or program K. However, before deciding which program to adopt, consider another way of looking at the same programs. Specifically:

- If *program J* is adopted then:
  – employment would be 90% – meaning that 10% would be unemployed, and
  – inflation would be 12%.

- If *program K* is adopted then:
  – employment would be 95% – meaning that 5% would be unemployed, and
  – inflation would be 17%.

Which would you select – program J or program K?

Crime Framing Problem: Re-framing of Initial Positive Frame

…Imagine you were faced with the decision of selecting, in light of the available crime statistics, between program S and program T. However, before deciding which program to select, consider another way of looking at the same programs. Specifically:

- In the Alphan community, 96.3% of youth have no criminal record – meaning that 3.7% of Alphan youth have a criminal record.

- In the Betan community, 98.8% of youth have no criminal record – meaning that 1.2% of Betan youth have a criminal record.

- Both programs allocate more money to the Alphan community, but *program T* favors the Alphan community to a greater extent than *program S* (i.e., *program T* gives the Alphans $65M of the $100M compared to *program S* which gives the Alphans $55M of the $100M).

Which would you select – program S or program T?

Investment Framing Problem: Re-framing of Initial Positive Frame

…Imagine that you had to vote for program Y or program Z. However, before deciding for which program to vote, consider another way of looking at the same programs. Specifically:

- If *program Y* is adopted, your community will gain $1,000 in addition to the original $3,000 – meaning that your community ends up with $4,000.

- If *program Z* is adopted, there is:
  – a 50% chance that your community will gain $2,000 in addition to the original $3,000 – that is, your community ends up with $5,000, and
  – a 50% chance that your community will gain nothing in addition to the original $3,000 – that is, your community ends up with $3,000.

Which program would you vote for – program Y or program Z?

Disease Framing Problem: Re-framing of Initial Positive Frame

…Imagine you were faced with the decision of selecting program A or program B. However, before deciding which program to select, consider another way of looking at the same programs. Specifically:

- If no program is adopted, then 600 people will die for sure.

- If _program A_ is adopted, then 200 people will be saved – meaning that instead of 600 people dying, only 400 people will die.

- If _program B_ is adopted, there is:
  – a 1/3 probability that all 600 people will be saved – that is, instead of 600 people dying, nobody will die, and
  – a 2/3 probability that no people will be saved – that is, all 600 people will die.

Which of the two programs would you favor – program A or program B?

Appendix C

Motivation: Need For Cognition Measure

Some people like to have responsibility for handling situations that require a lot of thinking, and other people don't like to have responsibility for situations like that. What about you? Do you dislike having responsibility for handling situations that require a lot of thinking very much, do you dislike it somewhat, do you neither dislike nor like it, do you like it somewhat, or do you like it very much?

| dislike very much | dislike somewhat | neither dislike nor like | like somewhat | like very much |

Some people prefer to solve simple problems instead of complex ones, whereas other people prefer to solve more complex problems. Which type of problem do you prefer to solve: simple or complex?

simple   complex

Following Bizer et al. (2000), I coded the first question such that 0 = dislike very much; .25 = dislike somewhat; .50 = neither dislike nor like; .75 = like somewhat; 1 = like very much. I coded the second question such that 0 = simple; 1 = complex. I then took the average of these two scores.

Ability: Training in Economics and Statistics Measures

Have you ever taken a course on statistics and/or probability, and if so, how many courses?

never taken one   1   2   3   4   5 or more

Have you ever taken an economics course, and if so, how many courses?

never taken one   1   2   3   4   5 or more

I summed these two measures (see Larrick et al. 1993: 335, Nisbett 1993).

I classified individuals as experts if they were above the median on both the motivation measure and the ability measure.

Figure 1
Predicted Probability of Expressing a Majority of Negative Preferences

Figure 2
Predicted Probability of Expressing a Majority of Negative Preferences
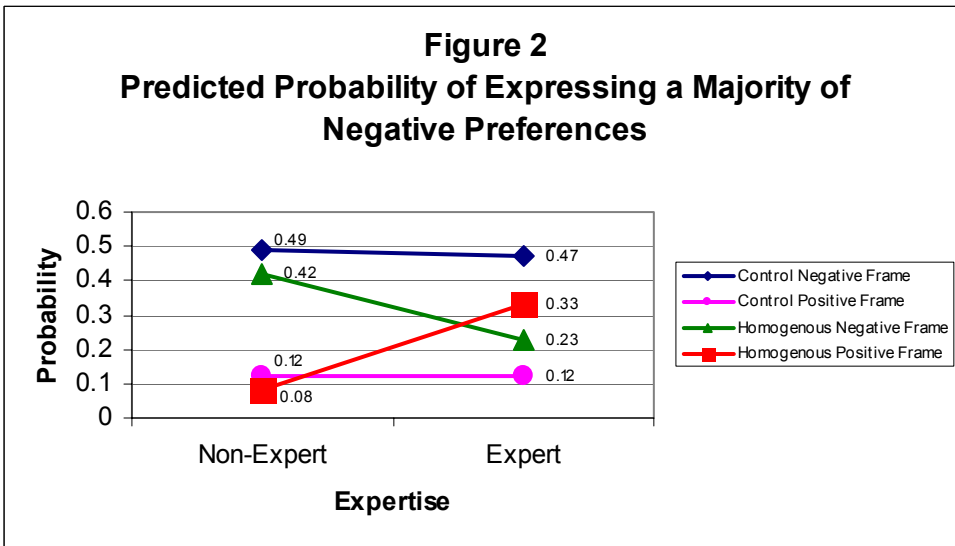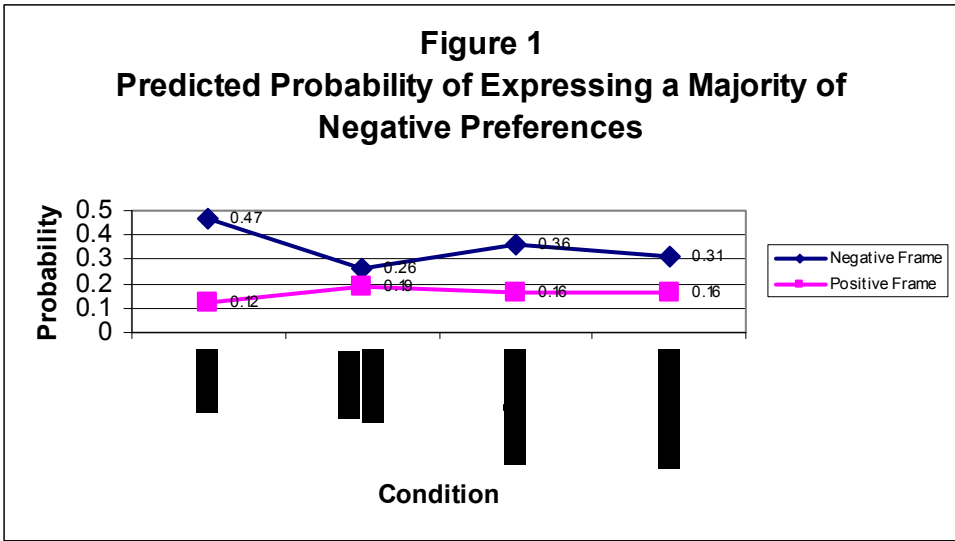
Figure 3
Predicted Probability of Being Above the Median in Confidence

Probability

0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0

0.52
0.38
0.33
0.25

0.68
0.66
0.46
0.4

No Agreement   Agreement

Frame Agreement
(preference matches that predicted by frame)

employment
(all conditions)
crime
(homogenous group)
investment
(homogenous group)
disease
(homogenous group)
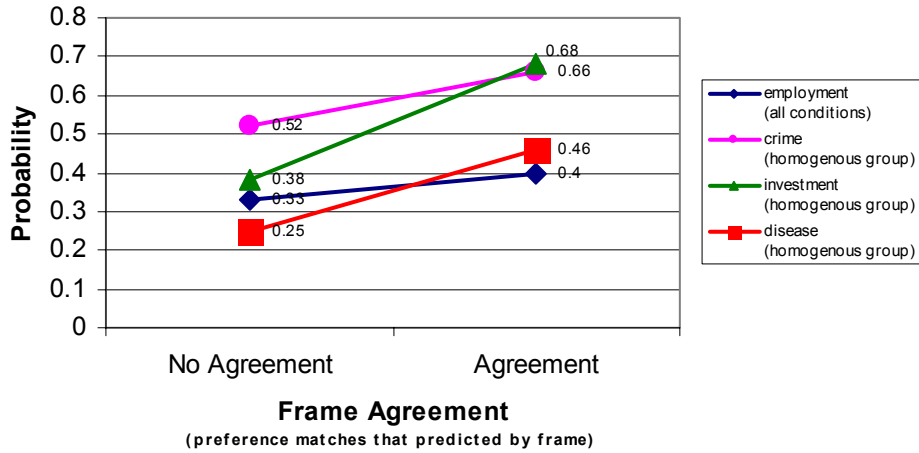
Table 1
Experimental Conditions and Predictions

Problem:	Control		Counter-Framing		Homogenous Group		Heterogeneous Group	
	Positive Frame (N=102)	Negative Frame (N=113)	Positive Frame (N=94)	Negative Frame (N=99)	Positive Frame (N=44)	Negative Frame (N=44)	Positive Frame (N=42)	Negative Frame (N=42)
Employment (attribute) Crime (attribute) Investment (risky) Disease (risky)	• Significant framing effects (replication of original experiments) • Sig. smaller (and/or no) effects for experts		• Relative to control, sig. smaller (and/or no) effects		• No clear prediction • If effects, sig. smaller (and/or no) effects for experts		• Relative to control, sig. smaller (and/or no) effects	

Table 2
Distribution of Negative Preferences Across Contexts

Number of Negative Preferences*	Positive Frame	Negative Frame	Total
0	16% (44)	4% (12)	10% (56)
1	35% (98)	21% (63)	28% (161)
2	32% (89)	40% (119)	36% (208)
3	15% (43)	28% (83)	22% (126)
4	2% (5)	6% (19)	4% (24)
Total	100% (279)	100% (296)	100% (575)

*$\chi^2_4 = 50.63$; $p \leq .01$

Table 3
Determinants of Negative Preferences

Independent Variable	Model 1	Model 2	Model 3	Model 4
Frame	.64***	1.10***	1.24***	1.15***
	(.09)	(.15)	(.18)	(.18)
Counter-Framing	–	.32**	.31**	.32**
		(.15)	(.15)	(.15)
Homogenous Group	–	.15	.17	-.27
		(.19)	(.20)	(.25)
Heterogeneous Group	–	.16	.24	.23
		(.19)	(.20)	(.20)
Counter x Frame	–	-.89***	-.91***	-.92***
		(.21)	(.22)	(.22)
Homo. x Frame	–	-.45*	-.47*	.12
		(.27)	(.28)	(.34)
Hetero. x Frame	–	-.57**	-.64**	-.63**
		(.27)	(.28)	(.28)
Adult	–	–	.21	.20
			(.13)	(.14)
Adult x Frame	–	–	-.03	-.01
			(.19)	(.19)
Expertise	–	–	.12	-.05
			(.13)	(.15)
Expert x Frame	–	–	-.24	-.003
			(.19)	(.20)
Expert x Homo.	–	–	–	1.04***
				(.36)
Expert x Homo. x Frame	–	–	–	-1.59***
				(.54)
τ_1	-1.04	-.91	-.79	-.86
	(.08)	(.12)	(.14)	(.14)
τ_2	.004	.16	.29	.23
	(.07)	(.11)	(.13)	(.14)
τ_3	1.00	1.18	1.31	1.26
	(.08)	(.12)	(.14)	(.14)
τ_4	2.14	2.33	2.48	2.44
	(.11)	(.14)	(.17)	(.17)
Log likelihood	-788.93	-778.69	-755.18	-750.10
Number of Observations	575	575	560	560

Note: The dependent variable is the number of negative preferences on the four problems, ranging from 0 to 4. Table entries are ordered probit coefficients with standard errors in parentheses. ***$p \leq .01$; **$p \leq .05$; *$p \leq .1$ for two-tailed tests. Twice the difference in log-likelihoods is distributed as a chi-square with the difference in the number of parameters as the degrees of freedom. Model 1 compared with model 2 results in $\chi^2_6 \geq 20.48$; $p \leq .01$, and model 3 compared with model 4 results in $\chi^2_2 \geq 10.16$; $p \leq .01$. Thus, models 2 and 4 constitute significant improvements over models 1 and 3, respectively.

Table 4
Probability of Expressing a Negative Preference

Problem:	Control (n = 213)		Counter-Framing (n = 192)		Homogenous Group (n = 86)		Heterogeneous Group (n = 84)		Overall (n = 575)	
	Positive Frame	Negative Frame	Positive Frame	Negative Frame	Positive Frame	Negative Frame	Positive Frame	Negative Frame	Positive Frame	Negative Frame
Employment	.39** (.05)	.54** (.05)	.47 (.05)	.57 (.05)	.39** (.07)	.63** (.07)	.57 (.08)	.55 (.08)	.44*** (.03)	.56*** (.03)
Crime	.25*** (.04)	.55*** (.05)	.36 (.05)	.37 (.05)	.39 (.08)	.31 (.08)	.19 (.06)	.21 (.07)	.30*** (.03)	.40*** (.03)
Investment	.26*** (.05)	.67*** (.05)	.35 (.05)	.36 (.05)	.22** (.06)	.44** (.08)	.32** (.08)	.56** (.08)	.30*** (.03)	.51*** (.03)
Disease	.47*** (.05)	.65*** (.05)	.49 (.05)	.56 (.05)	.48*** (.08)	.77*** (.07)	.48 (.08)	.62 (.07)	.48*** (.03)	.64*** (.03)

Note: Table entries are probabilities of expressing a negative preference, as produced by *Clarify*, based on probit regressions for each problem, controlling for preferences on the other problems (and nothing else; although results are robust to incorporating other controls). Standard deviations are in parentheses. ***$p \leq .01$; **$p \leq .05$; *$p \leq .1$ for two-tailed difference of proportions tests. The results are substantively and statistically the same either using the 95% confidence intervals produced by *Clarify* (e.g., examining if the negative frame probability falls within the positive frame confidence interval), or examining the basic percentages of participants opting for each alternative for each problem (not controlling for other preferences; the predicted probabilities closely mimic these percentages).

Table 5
Preference Confidence Scores

Problem:	Average (Standard Deviation)	Median (1st quartile; 3rd quartile)
Employment (n = 579)	4.90 (1.34)	5 (4; 6)
Crime (n = 579)	5.34 (1.23)	5 (5; 6)
Investment (n = 579)	5.31 (1.25)	5 (5; 6)
Disease (n = 580)	4.73 (1.51)	5 (4; 6)

Table 6
Determinants of Preference Confidence

Independent Variable	*Problem:*			
	Employ.	Crime	Invest.	Disease
Frame Agreement	.20**	-.05	.04	.09
	(.10)	(.10)	(.10)	(.10)
Second Problem	.30**	.15	.11	-.12
	(.12)	(.13)	(.13)	(.13)
Third Problem	.28**	.63***	-.002	-.06
	(.13)	(.13)	(.12)	(.13)
Fourth Problem	.36***	.55***	-.06	-.06
	(.12)	(.13)	(.13)	(.12)
Counter-Framing	-.03	-.02	.05	-.09
	(.11)	(.11)	(.11)	(.11)
Homogenous Group	-.10	.07	-.15	-.16
	(.21)	(.19)	(.20)	(.20)
Heterogeneous Group	-.16	.18	.18	.06
	(.14)	(.14)	(.14)	(.14)
Adult	-.04	-.08	.06	-.25***
	(.09)	(.10)	(.10)	(.09)
Expertise	.20**	.08	.003	.11
	(.09)	(.09)	(.09)	(.09)
Agreement x Homo.	-.21	.41*	.73***	.49**
	(.25)	(.25)	(.26)	(.25)
τ_1	-1.84	-2.06	-2.25	-2.05
	(.18)	(.21)	(.21)	(.17)
τ_2	-1.38	-1.68	-2.00	-1.50
	(.15)	(.17)	(.18)	(.14)
τ_3	-.77	-1.13	-1.34	-.91
	(.13)	(.15)	(.15)	(.13)
τ_4	-.04	-.46	-.59	-.30
	(.13)	(.14)	(.14)	(.13)
τ_5	.72	.37	.20	.41
	(.13)	(.14)	(.14)	(.13)
τ_6	1.70	1.34	1.06	1.05
	(.14)	(.15)	(.14)	(.13)
Log likelihood	-908.78	-845.13	-860.32	-973.56
Number of Observations	556	556	555	556

Note: The dependent variable is the confidence rating for each problem, ranging from 1 to 7. Table entries are ordered probit coefficients with standard errors in parentheses. ***$p \leq .01$; **$p \leq .05$; *$p \leq .1$ for two-tailed tests.