Using Credible Advice to Overcome Framing Effects

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A framing effect occurs when different, but logically equivalent, words or phrases (e.g., 10% employment or 90% unemployment) cause individuals to alter their decisions. Demonstrations of framing effects challenge a fundamental tenet of rational choice theory and suggest that public opinion is so malleable that it cannot serve as a useful guide to policymakers. In this article I argue that most previous work overstates the ubiquity of framing effects because it forces experimental participants to make decisions in isolation from social contact and context. I present two experiments where I show that some widely known framing effects greatly diminish and sometimes disappear when participants are given access to credible advice about how to decide. I discuss the implications of my findings for rational choice theory, and public opinion and public policy.

1. Introduction

Over the last several decades our understanding of how people make political, social, and economic decisions has fundamentally changed. Instead of viewing people as rational expected utility maximizers, many scholars now focus on the various biases and distortions that pervade preference formation and decision making (e.g., Nisbett and Ross, 1980; Kahneman, Slovic, and Tversky, 1982; Thaler, 1991; Sunstein, 2000). One of the more celebrated examples of such biases comes in the form of *framing effects* (Tversky and Kahneman, 1981).

A framing effect occurs when two "logically equivalent (but not *transparently* equivalent) statements of a problem lead decision makers to choose different options" (Rabin, 1998:36; emphasis in original).[1] For example, people support an economic program when it is said to result in 90% employment, but then oppose the same program when it is said to result in 10% unemployment. Similarly, a framing effect occurs when, choosing between risky

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1. Recent work on political communication uses a relaxed version of this definition that does not require that the alternative frames be logically equivalent (see, e.g., Iyengar, 1991; Nelson, Clawson, and Oxley, 1997).

prospects, individuals tend to prefer risk-averse alternatives when the outcomes are framed in terms of gains (e.g., saving lives, making money), but shift to preferring risk-seeking alternatives when the equivalent outcomes are framed in terms of losses (e.g., dying, losing money). Scholars have documented numerous examples of framing effects with far-reaching implications for positive and normative theory.

On the positive side, framing effects suggest that people do not form preferences and make decisions in accordance with rational expected utility theory—which, although possessing normative roots, is a central descriptive theory of decision making in the social sciences (Quattrone and Tversky, 1988:719; Thaler, 1991:3). Specifically framing effects violate expected utility's description invariance property. This property requires that preferences not shift due to arbitrary changes in the descriptions of identical alternatives (Tversky and Kahneman, 1987; Camerer, 1995:652; Bartels, 1998:7). The violation of invariance is so fundamental that some view framing effects as sufficient evidence to dispense with rational choice models. For example, Scott (1986:339) states, "these dramatic illustrations of the influence of framing have contributed to the growing belief by many legal analysts that the traditional rational choice model should be abandoned. . ." (also see Quattrone and Tversky, 1988:734). This sentiment has, in part, motivated the new behavioral law and economics movement that seeks to move away from rational choice analyses of law by incorporating various decision-making biases and distortions into analyses (Sunstein, 2000).

On the normative side, the existence of framing effects raises serious questions about the appropriate role of citizens in the making of public policy. If citizens' preferences reflect nothing more than arbitrary changes in frames, then public officials should put little stock in public opinion as assessed through polls, voting, and referenda. For example, if citizens support an economic program when it's described in terms of employment but then oppose the same exact program described in terms of unemployment, public opinion becomes a useless guide for policy making. Entman (1993:57) explains that framing effects "raise radical doubts about democracy itself. . . How can even sincere democratic representatives respond correctly to public opinion when empirical evidence of it appears to be so malleable, so vulnerable to framing effects?" (see also, e.g., Riker, 1982:237–238; Farr, 1993; Jones, 1994:105). An example of where these types of concerns have practical implications is the recent debate about contingent valuation methods that attempt "to measure public 'willingness to pay' for non-market goods" (Bartels, 1998:10; see, e.g., Carson et al., 1994). Specifically, critics of contingent valuation worry that framing and related biases make citizens' valuations inconsistent and unreliable (e.g., Bartels, 1998:10–11).

A growing number of scholars treat framing effects as a virtually constant phenomenon that drives people's preferences and decisions (e.g., Simon, 1983:17; Kahneman and Tversky, 1984:343; Tversky and Kahneman, 1987:88; Dawes, 1988:36–37; Quattrone and Tversky, 1988:735; Hasen, 1990:393; Iyengar, 1991:13; Zaller, 1992; Entman, 1993; Bartels, 1998:23).

In this article I take a different view, arguing that much previous work overstates the extent of framing effects. If I am correct, framing effects may not have such dire implications for rational choice theory and democratic responsiveness. Specifically, I hypothesize that individuals can overcome framing effects by relying on credible advice; previous work on framing effects has ignored this possibility by forcing experimental participants to make decisions in virtual isolation from social contact and context.

In the next section I flesh out this argument in more detail. I then present two experiments designed to test the hypothesis that people use credible advice to overcome framing effects. This type of advice should be readily available in many political, social, and economic contexts, and thus my experiments may enhance the external validity of the prototypical framing study. I conclude by discussing the implications of my results for positive analyses of choice, public policy and democratic responsiveness, and institutional design. My point is not to say that framing effects are irrelevant or unimportant, but rather that they should be understood as a *conditional* phenomenon.

2. Credible Advice and Framing Effects

Do framing effects render expected utility useless as a descriptive theory, and make democratic responsiveness impossible? An increasingly common answer to these questions is yes. Indeed, many see framing effects as ubiquitous reflections of "fundamental psychological limitations" (Bartels, 1998:23). This is particularly true in political contexts where the pervasiveness of framing effects "tends to be taken for granted" (Sniderman, 2000:78).

This view is based, in large part, on experimental demonstrations of framing effects—particularly by Tversky, Kahneman, and their colleagues (e.g., Tversky and Kahneman, 1981, 1987; Kahneman and Tversky, 1984; Quattrone and Tversky, 1988; for an insightful discussion of related work, see Bartels, 1998). In a typical experiment, one group of participants responds to a choice problem using one frame (e.g., unemployment) while another group responds to a logically identical problem that uses another frame (e.g., employment). A framing effect occurs when these two groups express significantly different preferences (see Wang, 1996; Druckman, 2001). These results are impressive insofar as they span a number of political, social, and economic problems, use a variety of populations of respondents, and have been replicated extensively.[2]

On the other hand, framing effect and related experiments are not without their critics. A number of scholars have questioned the experimental designs

2. However, some replication attempts have failed. Moreover, there is a sizable literature in psychology devoted, in part, to documenting various limits to framing effects. This work suggests, consistent with the results presented below, that many of the framing results are fragile (e.g., Miller and Fagley, 1991; Bohm and Lind, 1992; Fagley and Miller, 1997; Bless, Betsch, and Franzen, 1998; Kühberger, Schulte-Mecklenbeck, and Perner, 1999:223; also see Sniderman, 2000).

and validity as well as the strength of the cognitive theory on which the results rest (relative to rational choice theory) (see, e.g., Grether and Plott, 1979; Reilly, 1982; Scott, 1986; Thaler, 1987; Schwartz, 1988:380; Riker, 1995:28–36; Wittman, 1995:41–45). One criticism—the one I take up here—is that most experiments ask respondents to make decisions with no access to outside information or advice. Riker (1995:35) recognizes this pitfall when he states, "the typical experiments (as those by Tversky and Kahneman) used to justify attacks on the rational choice model do not allow even [a] tiny bit of interaction to distribute information. So I wonder very much if these experiments have *any* relevance at all for the study of *social* science" (see also Jackman and Sniderman, 1999; Sniderman, 2000; emphasis in original). By focusing on situations where decision makers are isolated from any communication, previous framing effect experiments ignore the possibility that people may use interactions with others to overcome framing effects. Consequently, in many political, social, and economic contexts where interpersonal interactions often occur, framing effects may be much less pervasive than previous work suggests.

The question to be addressed, then, is whether providing people with certain types of additional information enables them to adapt and overcome framing effects. Here I focus on the provision of credible advice—that is, I examine the situation where in addition to facing a problem described with a specific frame, people also receive some advice on how they should decide.[3] For example, in addition to learning about alternative economic programs using an unemployment frame, respondents also may be given expert advice on which economic program is preferable. Numerous works have shown that people rely on the advice of others when forming preferences (e.g., Kuklinski and Hurley, 1994; Popkin, 1994; Lupia and McCubbins, 1998; Petty and Wegener, 1998), and thus it may be the case that people use such advice to adapt to situations where they lack coherent preferences. Moreover, the inclusion of advice seems analogous to many political, social, and economic contexts where "disputes by elites form a constant background to decision-making" (Riker, 1995:33); as a result, the external validity of the typical framing experiment should be (relatively) enhanced.

In what follows, I describe two experiments implemented to examine the extent to which people use credible advice to overcome framing effects. Both experiments expose some respondents to a classic framing problem and other respondents to the same problem *along with* a piece of ostensibly credible advice. The framing hypothesis predicts that the framing effect will be robust to the introduction of advice and preferences will thus be based on arbitrary frames. In contrast, the credible advice hypothesis predicts that when

3. Lupia and McCubbins (1998) show that an individual considers a source credible and follows the source's advice *if* the individual believes that the source shares the individual's interests and possesses knowledge about the decision (see also, e.g., Crawford and Sobel, 1982; Sobel, 1985; Farrell and Rabin, 1996).

people are given advice, they will use it regardless of the frame and preferences will thus be based on systematic information. In this case, preference formation is consistent with a conventional rational choice model where people base their preferences on their prior beliefs as well a credible signal (e.g., Lupia and McCubbins, 1998)—preference invariance is not violated because preferences are not based on arbitrary features of the problem description (see Arrow, 1982:7; Bartels, 1998:7). Moreover, from a normative perspective, preferences based on the systematic integration of credible information are much more meaningful as a public policy instrument.

3. Experiment 1

In the first experiment I used variations of what is perhaps the most widely cited framing experiment—Tversky and Kahneman's (1981, 1987) Asian disease problem.[4] Tversky and Kahneman (hereafter referred to as T&K) randomly asked one group of college students 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?
>
> _____ _____
> Program A Program B

T&K find that 72% of respondents opted for program A—the risk-averse alternative—and 28% chose Program B—the risk-seeking alternative ($N = 152$). They randomly asked another group of college students to respond to Problem 2—a problem that is identical to Problem 1 in all ways except the programs are framed in terms of people dying instead of people being saved. Specifically, the options are

> If Program C is adopted, 400 people will die.
>
> If Program D is adopted, there is a 1/3 probability that nobody will die, and a 2/3 probability that 600 people will die.

4. According to the Social Sciences Citation Index, Tversky and Kahneman's (1981, 1986, 1987; Kahneman and Tversky, 1984) articles that report this experiment were cited 2,326 times from January 1997 to July 2000. (Their 1981 paper was the most cited with 1,436 citations.)

In this case, only 22% of respondents opted for Program C and 78% of respondents chose Program D—despite the fact that Program C is equivalent to Program A (e.g., 400/600 dying = 200/600 living) and Program D is equivalent to Program B ($N = 155$). Thus preferences over *identical* programs shifted by 50% due to slight frame changes. People tend to prefer the risk-averse program (e.g., Program A) when the outcomes are framed in terms of saving lives and the risk-seeking program (e.g., Program D) when the equivalent outcomes are framed in terms of people dying. Clearly this example violates invariance by showing that arbitrary wording of the problem drives people's preferences.

My experiment introduced variations of this problem that incorporated advice in the form of program endorsements by political parties. I used political parties because a pretest suggested that partisans did in fact see their own parties as credible advice givers for this public policy problem.[5] Moreover, many scholars have documented the importance of party cues as decision-making devices (Schattschneider, 1960:138; Gerber and Jackson, 1993; Rahn, 1993; Jackman and Sniderman, 1999; Sniderman, 2000). The specific experimental design was as follows.

A total of 464 individuals who were enrolled in undergraduate classes at a large public university completed the experiment.[6] The experiment consisted of two questionnaires. The first questionnaire asked participants to respond to a variety of political and demographic questions, including a question that asked for their party identification. The second questionnaire, administered approximately three weeks after the first, randomly presented the same participants with a version of T&K's Asian disease problem. There were six versions of the problem (i.e., the experimental conditions), as described in Table 1.

The conditions described in the first two rows of Table 1 replicate the original experiment just described. The condition described in the first row is the problem framed in terms of lives saved (Problem 1), while the condition described in the second row is the identical problem framed in terms of lives lost (Problem 2). The other four conditions are variations of T&K's original questions that incorporated party endorsements. I introduced the party endorsements by altering the program labels—for example, from "Program A" and "Program B" to "Democrats' Program" and "Republicans' Program." The exact question for the condition described in the third row (Problem 3)

5. Political parties may have no inherent connection to the issue at hand (i.e., the outbreak of a disease) or the possible solutions (i.e., the programs). Some may see this as problematic because it is not entirely clear why party advice should be relevant. However, it may be exactly in this context—where individuals lack information and prior experience with the (social policy) problem—that advice is most relevant.

6. Using undergraduates should not limit the experiment's generalizability. Indeed, based on a meta-analysis of 136 framing effect studies with nearly 30,000 participants, Kühberger (1998:36) finds that the behavior of student participants does *not* significantly differ from the behavior of nonstudent participants.

Table 1. Experimental Conditions and Predictions

Problem (Condition)	Frame	Risk-Averse Alternative Label (Endorsement)	Risk-Seeking Alternative Label (Endorsement)	Framing Hypothesis Prediction	Credible Advice Hypothesis Prediction
1	Gains (Save)	Program A	Program B	Program A	n/a
2	Losses (Die)	Program A[a]	Program B	Program B	n/a
3	Gains	Democrats' Program	Republicans' Program	Democrats' Program	Partisans choose their party's program[b]
4	Losses	Democrats' Program	Republicans' Program	Republicans' Program	Partisans choose their party's program
5	Gains	Republicans' Program	Democrats' Program	Republicans' Program	Partisans choose their party's program
6	Losses	Republicans' Program	Democrats' Program	Democrats' Program	Partisans choose their party's program

[a]In Problem 2, I used the labels "Program A" and "Program B" instead of T&K's "Program C" and "Program D."
[b]I expect that the party endorsements will not significantly affect the choices of Independents (i.e., nonpartisans), and thus, the framing hypothesis will predict their behavior across all conditions.

is as follows—with the words in bold representing the only changes to the original T&K question:

> 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—**one by Democrats and one by Republicans**. Assume that the exact scientific estimates of the consequences of the programs are as follows:
>
> If **the Democrats'** Program is adopted, 200 people will be saved.
>
> If **the Republicans'** Program 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?
>
> _____ _____
> **Democrats' Program** **Republicans' Program**

The question for the condition described in the fourth row (Problem 4) differs from Problem 3 only in its description of the alternative programs. Instead of a gains frame, it uses a losses (dying) frame:

> If **the Democrats'** Program is adopted, 400 people will die.

If **the Republicans'** Program is adopted, there is a 1/3 probability that nobody will die, and a 2/3 probability that 600 people will die.

The questions for the conditions described in the fifth and sixth rows (Problems 5 and 6) match the two just presented, except the party endorsements are reversed (i.e., the Republicans endorsed the risk-averse alternative).

These latter four conditions (Problems 3–6) constitute the critical test between the two hypotheses. For each condition, the framing hypothesis predicts that respondents will tend to opt for the risk-averse alternative when the frame is in terms of gains and not when the frame is in terms of losses, regardless of the endorsements. In contrast, the credible advice hypothesis predicts that partisans will follow the advice of their party, regardless of if the party endorses the risk-averse or the risk-seeking alternative and regardless of the frame. As a result, party endorsements will cause framing effects to decrease or disappear among partisans. Party advice should be less helpful for Independents, since they undoubtedly view the party advice as less credible; I therefore expect that Independents will be more susceptible to framing effects when offered party endorsements.

3.1 Results

In analyzing the results, I separate the respondents based on their party identification. This facilitates analysis because predictions from the credible advice hypothesis depend on the respondent's party identification.[7] Also, in accordance with most other framing experiments, I focus on simple comparisons of the percentages of respondents who chose each program across conditions. Other analyses, such as regressing preferences on dummy variables for the experimental conditions, yield the same substantive results.

I begin with Democrats. Table 2 displays the percentage of Democrats who opted for the risk-averse alternative in each condition. (Thus, to compute the percentage that preferred the risk-seeking alternative for each condition, subtract the percentage preferring the risk-averse alternative—given in the table—from 100%.) The label of the risk-averse alternative (i.e., the endorsement) appears in the column heading, while the description of the frame appears in the row heading. The parenthetical number in each cell is the problem number listed in Table 1. Thus the cell containing a (1) displays the results for Democrats who answered Problem 1 (i.e., a gains frame with the risk-averse alternative labeled as "Program A" and the risk-seeking alternative labeled as "Program B"). The third row of the table displays the

7. Participants were separated based on their responses to the question: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, or an Independent?" (i.e., the first part of the National Election Study's party identification question; see Miller and Shanks, 1996:126–128). With this breakdown, 46% of the sample were Democrats, 28% of the sample were Republicans, and 26% of the sample were Independents. When Independent leaners are counted as partisans, there is a slight increase in the extent of framing effects among partisans (relative to what is reported here). Also, the results are substantively the same when the strength of the partisans' party identifications is incorporated.

Table 2. Percentages of Democrats Preferring Risk-Averse Alternative

	Risk-Averse Alternative Label (Endorsement)		
	Program A	Democrats' Program	Republicans' Program
Gains Frame	(1) 70% ($N = 30$)	(3) 77% ($N = 39$)	(5) 31% ($N = 39$)
Losses Frame	(2) 24% ($N = 34$)	(4) 56% ($N = 34$)	(6) 16% ($N = 37$)
Preference Reversal	46%	21%	15%

percentage of "preference reversal." This is the difference between the percentage of respondents opting for the risk-averse alternative when the frame is in terms of gains and the percentage of respondents opting for the risk-averse alternative when the frame is in terms of losses.[8] Higher values of preference reversal indicate a greater framing effect. The framing hypothesis predicts that the percentage of preference reversal will be equally high across all conditions. The credible advice hypothesis predicts that the percentage of preference reversal will be significantly lower when party endorsements are provided. Recall that in their original experiment, T&K found a 50% preference reversal.

The first column of Table 2 shows that when framed in terms of gains without party endorsements, a majority (70%) of Democratic respondents opted for the risk-averse alternative; in contrast, when framed in terms of losses without party endorsements, significantly fewer respondents chose the risk-averse alternative (24%), instead opting for the risk-seeking alternative ($z = 3.73$, $p = .000$).[9] This represents a 46% preference reversal. As in T&K's original experiment, framing had a strong effect when no endorsements were provided.

The second column of Table 2 displays results from problems where the Democrats endorsed the risk-averse alternative and the Republicans endorsed the risk-seeking alternative. Framing still had a significant effect, with 77% opting for the risk-averse alternative when given a gains frame and 56% opting for the risk-averse alternative when given a losses frame ($z = 1.91$, $p = .056$). However, the party endorsements also substantially vitiated the

8. I focus on comparisons between conditions where the parties maintain consistent positions across frames, rather than on comparisons between conditions where a party switches its endorsement (as such switches render the party advice useless).

9. To evaluate the significance of the framing effects, I use difference of proportions tests; for comparisons between preference reversal statistics, I use difference of differences of proportions tests (Blalock, 1979:234–236). Also note that the reported p-values reflect two-tailed tests, except when the comparisons are between preference reversal statistics—where I have clear directional predictions and thus use one-tailed tests.

framing effect—particularly in the case of the losses frame, where significantly more Democratic respondents opted for the risk-averse alternative when it was endorsed by the Democrats (56%) than when it was not endorsed (24%) ($z = 2.73$, $p = .006$). (The percentage of respondents opting for the risk-averse alternative in the case of the gains frame increased slightly from 70% to 77%.) This effect is also evident in the preference reversal statistic, which significantly dropped from 46% to 21% ($z = 1.61$, $p = .054$ for a one-tailed test).

The third column of Table 2 displays results from problems where the Republicans endorsed the risk-averse alternative and the Democrats endorsed the risk-seeking alternative. In this case, the party endorsements had a substantial effect, driving the framing effect to marginal significance. Specifically, 31% opted for the risk-averse alternative when given a gains frame and 16% opted for the risk-averse alternative when given a losses frame ($z = 1.49$, $p = .136$). Thus, when the frame was in terms of gains, only 31% of the Democratic respondents opted for the risk-averse "Republicans' Program" compared to 70% when the same alternative was called "Program A" ($z = 3.23$, $p = .001$). (The percentage of respondents opting for the risk-averse alternative in the case of the losses frame dropped slightly from 24% to 16%.) The percentage of preference reversal significantly dropped to 15% ($z = 2.12$, $p = .017$ for a one-tailed test).

The preference reversal statistics decline dramatically in the party endorsement conditions, demonstrating the dampening of the framing effects. Indeed, as mentioned, the preference reversal statistics for the party endorsement conditions are both significantly lower than the preference reversal statistic for the no endorsement conditions.[10] Nonetheless, the fact that the remaining effects are above zero shows that the party endorsements did not completely eliminate the framing effects. In sum, Democratic respondents were susceptible to framing effects, however, these effects were considerably decreased by the presence of party endorsements.

Table 3 displays analogous results for the Republican respondents. The first column shows that, like the Democrats and T&K's original experiment, the Republicans displayed a significant preference reversal of 41% in the no-endorsement conditions ($z = 2.66$, $p = .008$).

The second column of Table 3 shows, however, that framing did not have a significant effect on Republican respondents when the Democrats endorsed the risk-averse alternative and the Republicans endorsed the risk-seeking alternative ($z = 0.27$, $p = .784$). In this case, only 24% of Republicans opted for the risk-averse program (i.e., the "Democrats' Program") when the problem used a gains frame. This is a significant decrease from the no endorsement problem, where 65% opted for the risk-averse program ($z = 2.52$, $p = .012$). (The percentage of respondents opting for the risk-averse alternative in the case of the losses frame dropped slightly from 24%

10. The preference reversal statistics for the party endorsement conditions are not statistically distinct ($z = 0.42$, $p = .337$).

Table 3. Percentages of Republicans Preferring Risk-Averse Alternative

	Risk-Averse Alternative Label (Endorsement)		
	Program A	Democrats' Program	Republicans' Program
Gains Frame	(1) 65% ($N = 20$)	(3) 24% ($N = 17$)	(5) 69% ($N = 26$)
Losses Frame	(2) 24% ($N = 21$)	(4) 20% ($N = 25$)	(6) 64% ($N = 22$)
Preference Reversal	41%	4%	5%

to 20%.) The lack of a framing effect also can be seen in the preference reversal statistic, which significantly dropped from 41% to 4% ($z = 1.92$, $p = .027$ for a one-tailed test).

The third column of Table 3 shows that framing also did not have a significant effect on Republican respondents when the Republicans endorsed the risk-averse alternative and the Democrats endorsed the risk-seeking alternative ($z = 0.41$, $p = .682$). When the frame was in terms of losses, 64% of the Republican respondents chose the risk-averse "Republicans' Program" compared to only 24% when the same alternative was called "Program A" ($z = 2.63$, $p = .009$). (The percentage of respondents opting for the risk-averse alternative in the case of the gains frame increased slightly from 65% to 69%.) The percentage of preference reversal significantly dropped to 5% ($z = 1.83$, $p = .034$ for a one-tailed test).

Finally, an examination of the preference reversal statistics across conditions shows that the party endorsements caused preference reversals to almost disappear. As mentioned, the preference reversal statistics for the party endorsement conditions are significantly lower than the preference reversal statistic for the no-endorsement conditions. In sum, the Republican respondents appear to be driven largely by party endorsements, with framing having virtually no effect.[11]

Table 4 presents the results from Independent respondents. In this case, party endorsements presumably do not facilitate decision making, as Independents do not firmly identify with either party (and thus, will not see either party as credible). It is thus not surprising that framing had a fairly strong effect across conditions. The first column shows another replication of T&K's original experiment with a 45% preference reversal ($z = 2.94$, $p = .003$). The

11. An interesting question is why Democrats were more susceptible to framing effects than Republicans. A partial explanation is that the Republicans' preference reversal in the generic conditions is slightly lower than the analogous statistic among Democrats. Thus the relative power of the party endorsements in diminishing the framing effect is less than it may appear at first sight. Another possible explanation is that there may be greater variation within the Democratic party that, in turn, makes the Democratic party endorsement less meaningful.

Table 4. Percentages of Independents Preferring Risk-Averse Alternative

	Risk-Averse Alternative Label (Endorsement)		
	Program A	Democrats' Program	Republicans' Program
Gains Frame	(1) 68% ($N = 19$)	(3) 58% ($N = 26$)	(5) 57% ($N = 14$)
Losses Frame	(2) 23% ($N = 22$)	(4) 28% ($N = 18$)	(6) 29% ($N = 21$)
Preference Reversal	45%	30%	28%

second and third columns show that framing effects remained significant, or near significant, regardless of the party endorsements (for the conditions described in the second and third columns, respectively, $z = 1.96$, $p = .050$, and $z = 1.69$, $p = .091$). Indeed, the preference reversal statistics also show the strength of the gains and losses framing effects among Independents relative to the partisans. The preference reversal statistics for the party endorsement conditions are *not* statistically different from the preference reversal statistic for the no-endorsement conditions ($z = 0.75$, $p = .227$ for a comparison with the Democrats as the risk-averse alternative conditions, and $z = 0.79$, $p = .215$ for a comparison with the Republicans as the risk-averse alternative conditions; p-values are one-tailed). In sum, party labels did less to vitiate framing effects among Independents.[12]

Overall, the results show that, when party advice was provided, framing significantly affected the preferences of Independents, had a mild effect on Democrats' preferences, and had a slight, if any, effect on Republicans' preferences. The party advice clearly reduced framing effects—in some cases, causing them to completely disappear.[13] The results highlight the conditional nature of framing effects and suggest that when provided systematic information, people tend to use it in a rational and reasonable fashion.[14]

12. The party endorsements vitiated the framing effect a bit among Independents. This is probably due to the specific distribution of Independent leaners across conditions—some of whom undoubtedly used the party endorsements as cues (e.g., 64% of Independents in the condition with a gains frame where the risk-averse alternative was endorsed by the Republicans were *Democratic* leaners).

13. An alternative test of the two hypotheses is provided by a probit analysis with the preference as the dependent variable and all but one of the conditions as dummy independent variables. This analysis reveals that, among partisans, in each instance where the two hypotheses offered different predictions, the data support the credible advice hypothesis. Detailed results are available from the author.

14. Notice that partisans' preferences tend to change with the party endorsements; for example, a Democrat favors the risk-averse alternative when the Democrats endorse it, but then switches to the risk-seeking alternative when the endorsement changes. Some may see this as a different type of framing effect because the actual outcomes, as described, remain logically

4. Experiment 2

In the second experiment I used a different but equally well-known framing problem from McNeil et al. (1982; see also, e.g., Arrow, 1982:7; Kahneman and Tversky, 1984:346; McNeil, Pauker, and Tversky, 1988; Camerer, 1995:652–653; Rabin, 1998:36–37). McNeil et al. (1982) provided respondents with the following scenario:

> Imagine that you have lung cancer and you must choose between two therapies: surgery and radiation.
>
> Surgery for lung cancer involves an operation on the lungs. Most patients are in the hospital for two or three weeks and have some pain around their incisions; they spend a month or so recuperating at home. After that, they generally feel fine.
>
> Radiation therapy for lung cancer involves the use of radiation to kill the tumor and requires coming to the hospital about four times a week for six weeks. Each treatment takes a few minutes and during the treatment, patients lie on a table as if they were having an x-ray. During the course of the treatment, some patients develop nausea and vomiting, but by the end of the six weeks they also generally feel fine.
>
> Thus, after the initial six or so weeks, patients treated with either surgery or radiation therapy feel about the same.

One group of respondents (randomly) received a description of the procedure outcomes framed in terms of dying. Specifically, they were told that

> Of 100 people having surgery, 10 die during surgery or the postoperative period, 32 die by the end of one year and 66 die by the end of five years.
>
> Of 100 people having radiation therapy, none die during treatment, 23 die by the end of one year and 78 die by the end of five years.

McNeil et al. found that 44% of the respondents preferred radiation therapy. Another group of respondents (randomly) received the *same exact* outcome description except it was framed in terms of people living:

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equivalent. The problem with this interpretation is that following a party endorsement is analogous to accepting a credible signal where the recipient has some uncertainty about which outcome is better (e.g., the recipient may be uncertain about side effects of the programs, the validity of the outcome estimates, or a variety of other contextual features), and thus relies on a signal. This is not a violation of invariance where preferences depend on "arbitrary features of the context, formulation, or procedure used to elicit those preferences" (Bartels, 1998:7). In short, party cues are not arbitrary (see, e.g., Kuklinski and Hurley, 1994:749); rather, they serve as a systematic decision-making tool (e.g., Bartels, 2000).

> Of 100 people having surgery, 90 live through the postoperative period, 68 are alive at the end of one year and 34 are alive at the end of five years.
>
> Of 100 people having radiation therapy, all live through treatment, 77 are alive at the end of one year and 22 are alive at the end of five years.

Only 18% of these respondents preferred radiation therapy. Thus there is a 26% preference reversal due to a change of frames (from a dying frame to a living frame). This framing effect comes about because "the impact of perioperative mortality on the comparison between the two treatments [is] greater when it [is] framed as a difference between mortality rates of 0 per cent and 10 per cent, than when it [is] framed as a difference between survival rates of 100 per cent and 90 per cent" (McNeil et al., 1982:1260). This result is another example of an invariance violation, suggesting that people cannot form coherent preferences.[15]

My experiment involved 313 individuals who did not take part in the first experiment (and who were again enrolled in undergraduate classes at a large public university). Each participant randomly received one of six variations of McNeil et al.'s scenario. Two of the conditions mimicked McNeil et al.'s original experiment (i.e., one offered the dying frame and the other offered the living frame). The other four conditions differed from the original experiment by offering credible advice about which therapy is preferable. Specifically participants received either a dying frame or a living frame that included an endorsement for either surgery or radiation. The endorsement read:

> When asked which treatment they think would be preferable, a group of lung cancer specialists from ____ recommended [surgery/radiation].

In the experiment, the specialists were said to be from two nationally prominent (and locally well-known) medical research organizations. A separate test with representative participants (who did not take part in the experiment) demonstrated that the specialists from these organizations are perceived to be credible.

In sum, participants received a description that (1) used either a dying frame or a living frame and (2) offered no endorsement, a credible endorsement for surgery, or a credible endorsement for radiation.[16] The framing

15. Levin, Schneider, and Gaeth (1998) distinguish this type of framing effect (what they call attribute framing) from the type of framing effect reflected in the Asian disease problem (what they call risky choice framing).

16. A small number of participants participated in conditions where they received advice from a noncredible source (i.e., an insurance executive with no knowledge of lung cancer therapies). In this case, the advice did little to limit the framing effect—the results were similar to the results for the no endorsement conditions.

Table 5. Percentages of Participants Preferring Radiation Therapy

	No Endorsement	Surgery Endorsement	Radiation Endorsement
Dying Frame	44% ($N = 46$)	22% ($N = 50$)	44% ($N = 50$)
Living Frame	13% ($N = 53$)	13% ($N = 55$)	38% ($N = 55$)
Preference Reversal	31%	9%	6%

hypothesis predicts that participants will be significantly more likely to prefer radiation when a dying frame is given than when a living frame is given, regardless of whether or not advice is provided. The credible advice hypothesis predicts that in the conditions where an endorsement is provided, participants will follow the advice, regardless of the frame.

4.1 Results

Table 5, shows the percentage of participants who preferred radiation therapy, by condition. (Thus to compute the percentage that preferred surgery for each condition, subtract the percentage preferring radiation—given in the table—from 100%.) The column headings state the endorsement, while the row describes the frame. The third row displays the percentage of preference reversal. As before, higher values of preference reversal indicate a greater framing effect.

The first column of Table 5 shows that, when no endorsement was provided, participants were significantly more likely to prefer radiation when given a dying frame (44%) than when given a living frame (13%) ($z = 3.45$, $p = .001$).[17] This represents a 31% preference reversal, closely matching the original experiment.

The second column of Table 5 displays results from each frame when surgery was endorsed. The endorsement had no effect on participants who received a living frame; however, among those who received a dying frame, it drastically reduced the number of participants who preferred radiation—from 44% to 22% ($z = 2.30$, $p = .022$). The preference reversal statistic shrunk to 9%, which is a significant drop from 31% ($z = 1.84$, $p = .033$ for a one-tailed test). The framing effect is no longer significant ($z = 1.22$, $p = .223$).

The third column reports results for participants who received a radiation endorsement. In this case, 38% of participants who received a living frame opted for radiation—this is a 25% increase from the living frame condition without an endorsement ($z = 2.97$, $p = .003$). Moreover, the endorsement

17. Four of the participants refused to choose one of the alternatives. One of these four—who received the living frame without an endorsement—did not express a preference, and instead wrote, "I would ask my doctor for her/his opinion."

rendered the framing effect insignificant ($z = 0.62$, $p = .532$) and caused the preference reversal statistic to shrink to 6% ($z = 1.89$, $p = .029$ for a one-tailed test).

In sum, participants who did not receive credible advice were susceptible to a substantial framing effect. In contrast, when participants were offered credible advice, they often used it to overcome the framing effect. While not all participants followed the endorsement, the credible advice nonetheless sufficiently counteracted the framing effect.[18] This is corroborative evidence that people can and do adapt to make consistent choices even when they initially lack coherent preferences.

5. Conclusion

The experimental results demonstrate that the availability of credible advice dramatically decreases, and sometimes eliminates, framing effects. Instead of basing their preferences on arbitrary question wording, people tend to rely on what they believe is credible information. Moreover, it seems plausible that outside of the laboratory, people do in fact access and use advice from others, especially in situations where they have ill-formed preferences. Many previous framing effect experiments ignore this possibility, and as a result, overstate the pervasiveness of framing effects. I conclude by discussing the implications of my results for positive analyses of choice, democratic responsiveness and public policy, and institutional design. I also touch on some possible extensions.

Positive analyses of choice have benefited from the discovery of framing effects and related biases. People do not always act as rational expected utility maximizers. However, they also do not always display vulnerability to framing effects and related biases. As discussed, many scholars treat framing effect evidence as sufficient to abandon rational choice models (e.g., Scott, 1986:339). My results and related findings suggest this is premature—when given access to advice, people can overcome framing effects and act in accordance with the requisites of rational choice theory (e.g., basing their decisions on credible signals). An ideal positive model of choice would include a statement of the conditions under which expected utility models do and do not apply and the conditions under which framing effects can be expected (see, e.g., Fazio, 1990; Payne, Bettman, and Johnson, 1993). As the behavioral law and economics movement expands, it will be important to recognize the conditional nature of framing effects and related biases.

The implications for democratic responsiveness and public policy are similar. In recent years, scholars and pundits increasingly point to framing effects as evidence for the futility of coherent public opinion (e.g., Entman, 1993).

18. Of interest, at least 56% of the participants always preferred surgery and at least 13% of the participants always preferred radiation, regardless of the frame and/or endorsement. This suggests a bound on the number of people who are susceptible to framing effects and/or persuasion (see, e.g., Kowert and Hermann, 1997).

If peoples' preferences reflect arbitrary question wording, then there is no reason to expect public policy to correspond with public opinion, and in fact, it would be ill advised for government officials to look to public opinion for guidance (or to use contingent valuation methods, for example; Bartels, 1998). But just how often do people's preferences reflect seemingly arbitrarily frames? The results presented here, combined with similar results (e.g., Bless, Betsch, and Franzen, 1998; Kühberger, Schulte-Mecklenbeck, and Perner, 1999:223), suggest that (arbitrary) frames may not play such a prominent role in shaping preferences. Indeed, when given the opportunity, people appear to adapt in a reasonable way to situations where they lack coherent preferences. When people adjust and base their preferences on what they believe is high-quality information, public opinion can function as a relatively useful guide for public policy. In short, while framing effects and similar biases may distort public opinion in some contexts, it is presumptuous to dismiss public opinion as meaningless because these framing effects sometimes occur.

The experimental results suggest that the quality of public opinion improves when people have access to advice from well-known, credible sources. A number of steps can be taken to enhance the availability of such information. Lupia and McCubbins (1998) demonstrate how a variety of institutions can structure incentives so as to make credible advice transparent. For example, they (1998:209) show that requiring political candidates or supporters to reveal how much they spend on a campaign can facilitate the transmission of credible advice. This also speaks to the importance of campaign finance reform that requires secretive tax-exempt groups that engage in political (election-related) activities to publicly reveal their expenditures and donors. The ability of citizens to form coherent preferences increases as they gain access to information about who is endorsing whom. As others have argued (e.g., Popkin, 1994:236; Lupia and McCubbins, 1998), it may be more important to provide voters with clear decision-making cues rather than overwhelming them with detailed information.

All of these implications demand further investigation. Moreover, a number of directions can be taken to refine, extend, and generalize the results presented here. First, it remains unclear if people generally form accurate perceptions of advice givers; for example, it may be the case that people misperceive credibility and are thus misled (e.g., Kuklinski and Hurley, 1994, 1996; Lupia and McCubbins, 1998:70–74; Kuklinski and Quirk, 2000). Alternatively, it may be the case that the expert advice givers themselves exhibit susceptibility to framing effects. Second, in my experiments, respondents received the credible advice either concomitantly with the frames (in the first experiment) or after the frames (in the second experiment). An interesting question is if people use credible advice in the same way when it precedes the frames. Related research in social psychology on primacy and recency persuasion effects suggests that the answer to this question may be complex. This work shows that, under some circumstances, the influence of the first relevant piece of information overwhelms later information whereas,

under other circumstances, the reverse effect occurs. Which effect takes place depends, in part, on the amount of time elapsed between exposures to the two pieces of information and between exposure and preference revelation (see Eagly and Chaiken, 1993:264–267). Of course, I did not manipulate these time variables; however, doing so may be a useful extension.

A third issue concerns the specific nature of the problems. Like much previous framing work, I used hypothetical framing problems. While the use of such problems facilitates the controlled testing of framing effects (Quattrone and Tversky, 1988:720), future work should examine the interaction of framing effects and credible advice with actual, ongoing political problems. Finally, most framing effects work, including the experiments presented here, ignores the strategic interplay that undoubtedly goes on between parties and other elites. That is, in nearly all experiments, individuals are exposed to one frame or the other. Strategic actors, however, undoubtedly opt for the frame that favors their side, and thus individuals typically may be exposed to different frames from different sources rather than just a single frame. This may vitiate framing effects even further (Sniderman and Theriault, 1999).

In sum, demonstrations of framing effects represent a great advance in our understanding of preference formation and decision making. A danger, however, is that some scholars focus only on successful framing attempts, leading them to confound the existence of framing effects with their ubiquity. This has resulted in an overstatement of the positive and normative implications of framing effects.

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