The Presentation of a Web Survey, Nonresponse and Measurement Error among Members of Web Panel

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This study tests the idea that features of the presentation of a survey to potential respondents can affect nonresponse error, measurement error, and the relation between the two. A few weeks after they had completed one web survey, we asked members of two opt-in web panels to take part in a second web study and systematically varied our description of that survey. The description varied both the purported topic and sponsor of the second survey. The members of the sample were not aware of the connection between the two surveys. We found little evidence that the survey presentation affected response rates to the second survey or the make-up of the sample on the variables we had collected in the initial questionnaire. There were indications, however, that some answers to the questions in the second survey were affected by the framing of the survey request. For example, respondents were less favorable to gun control than they had been in the initial survey when we described the sponsor of the second survey as the "The National Coalition of Gun Owners" rather than "The National Coalition for Victims of Gun Violence" or "The National Center for the Study of Crime." We argue that the description of the survey can affect how respondents interpret the questions and what they see as a useful response. We also found evidence that attitudes toward the survey sponsor and interest in the topic were related to carelessness in completing the questions. Respondents were, for example, less likely to give the same answer to every item in a grid if they had favorable attitudes toward the sponsor than if their attitudes toward the sponsor were unfavorable.

Key words: Survey presentation; nonresponse rates; nonresponse bias; measurement error.

1. Introduction

From the respondent's viewpoint, every survey begins with a request to take part. We refer to the description of the survey in this request to participate as the *survey presentation*. The survey presentation encompasses the package of variables that shapes the sample member's perception of the survey, including the name of the survey, its sponsor, stated purpose, and topic. This information is conveyed to members through a variety of means, including advance letters and the envelopes they come in, the title printed on paper questionnaires, and the introduction used by in-person or telephone interviewer's when

they first contact members of the sample. In web surveys, it also includes information on the splash page of the survey or in the survey header that conveys information about the survey or its sponsor.

1.1. *Survey Presentation, Response Propensities, and Nonresponse Error*

It is not always clear how this information affects either the likelihood that a given member of the sample will agree to take part in the survey or the answers that they ultimately give if they do decide to participate. Both Morton-Williams (1993) and Groves and Couper (1998) show that standardized introductions that provide information about the survey in a uniform way to every member of the sample are less effective than improvised introductions that are tailored to the specific individual. Still, some features of the survey presentation do seem to have systematic effects on response rates. For example, people are more likely to respond to survey requests from government agencies than from other sponsors (e.g., Heberlein and Baumgartner 1978), when the U.S. Census Bureau is collecting the data rather than a university (National Academy of Sciences 1979), and when the topic is interesting to them than when it is not interesting (Groves, Presser, and Dipko 2004; Heberlein and Baumgartner 1978).

Groves and his colleagues have argued that nonresponse related to the topic of the survey may be especially likely to produce nonresponse bias: "Nonresponse bias in a survey estimate arises when the set of mechanisms that influence the participation decision is related to the variables involved in the estimate" (Groves et al. 2006, p. 721). Topic interest, they argue, is likely to be related both to the sample members' propensity to respond and to their values on the survey variables; covariation between the two is what produces nonresponse bias (Bethlehem 2002). Groves and his co-authors report several experiments that varied the apparent topic of a survey and one of them found significant effects for that variable. That experiment involved a mail survey sent to samples drawn from three populations – members of the American Birding Association (ABA), contributors to the World Wildlife Fund (WWF), and members of the general public. As hypothesized, the American Birding Association members responded at much higher levels than members of the other two groups when the topic of the survey was described as "Birds, Bird-Watching, and Birding," a subject likely to be of considerably more interest to ABA members than to the members of the other two samples. In addition, the World Wildlife Fund contributors who completed the questions were much more likely to report that they had engaged in bird-watching during the past year when the topic of the survey was described as birds and birding than when it was described as the design of indoor shopping malls (see Figure 8 in Groves et al. 2006). This relation between the stated topic of the questionnaire and the survey responses presumably reflects differential nonresponse error. WWF contributors with an interest in bird-watching were more likely to complete the questions when the survey was described as being about bird-watching than when it was described as being about an unrelated topic. This link between the stated topic of the survey and the survey responses was reduced when the mailing included a $2 incentive.

In summary, then, both theoretical arguments and empirical evidence show potential links between survey participation and nonresponse error (for further evidence and additional theoretical arguments, see Olson 2006, 2007).

1.2. Survey Presentation and Reporting

A second line of research suggests that the survey presentation can also affect the answers respondents give once they agree to take part. For example, Norenzayan and Schwarz (1999) conducted a study in which a sample of students read about mass murder cases and then tried to explain the murderer's motives. The experimental variable was the apparent source of the questionnaire. For one group, the questionnaire was printed on the stationery of the "Institute of Personality Research;" for the other, it was printed on the stationery of the "Institute of Social Research." When respondents saw the researchers as personality psychologists, they provided explanations for the murder that were couched in terms of the murderer's personality; when they saw the researchers as social scientists, they gave explanations of the crime in terms of the murderer's situation. (See Smith, Schwarz, Roberts, and Ubel 2006, for a similar example in a survey of a less specialized population).

There are several reasons why the framing of the survey request could affect the answers respondents provide (Galesic and Tourangeau 2007). First, the survey presentation can affect what information the respondents think the researchers are looking for, as in the Schwarz and Norenzayan study. Schwarz and Norenzayan argue that respondents try to follow the Gricean norms (Grice 1989), in this case by providing information that is relevant given the apparent purposes of the questions. According to Grice, participants in a conversation try to make each of the contributions useful in the current context. In a survey, respondents may tailor the information they provide based on what seems relevant to the survey's aims; this tendency for respondents to report what they see as relevant can easily shade over into a tendency to provide information they see as useful or even supportive of the goals of the researchers – that is, information that confirms the researchers' hypotheses or otherwise advances their cause. The same desire to help out the researchers by taking part in their study may influence the response process, affecting the answers respondents give to the questions.

The survey presentation can affect the interpretation of the questions in another way. Most concepts have fuzzy boundaries; that is, they include both clear, easily-classified instances and less clear, hard-to-classify cases. For example, almost everyone agrees that a paraplegic has a disability, but there is less consensus about whether a heroin addict does (Tourangeau et al. 2006). Respondents may not deal with the hard-to-classify cases correctly, reporting instances that do not meet the official definitions for the relevant concepts or omitting ones that do (Schober and Conrad 1997; Tourangeau et al. 2006). The sponsorship and other clues from the framing of the survey request may help respondents decide how to deal with the borderline cases. Government surveys, with their official trappings, may typically encourage a narrow construal of the questions. For example, Tourangeau and McNeeley (2003) argue that various features of the National Crime Victimization Survey – the survey's name, the fact that it is conducted by one federal agency (the U.S. Census Bureau) on behalf of another federal agency (the U.S. Bureau of Justice Statistics), the use of advance letters printed on official government stationery – are likely to convey to the respondents that the topic of the survey is *crimes* in some narrow official sense and this may lead them to underreport certain crimes (e.g., domestic violence or date rape) that do not fit their conceptions of the typical crime.

Besides affecting the perceived intent and scope of the questions, the survey presentation may serve as a retrieval cue that facilitates the recall of specific memories.

For example, whatever else it may have done, the questionnaire on "Birds, Bird-Watching, and Birding" used by Groves and his colleagues (which included a large picture of a bird on its cover) is likely to have triggered memories of any activities related to birds among the respondents who completed that version of the questionnaire.

Galesic and Tourangeau (2007) report a randomized experiment that provided some evidence for all three of these hypothesized mechanisms linking the survey presentation to effects on reporting. Respondents in their web survey completed a set of questions that were said to be sponsored either by a feminist organization (Women Against Sexual Harassment) or a neutral research group (the Work Environment Institute). The section of the questionnaire that included these items was entitled "Sexual Harassment Survey" for the first group of respondents or "Work Atmosphere Study" for the second group. The survey questions asked respondents how often they encountered various situations at work, how much these situations bothered them, and how likely it was that each one constituted sexual harassment. The situations ranged from ones that were quite likely to involve sexual harassment ("A co-worker sends an email of a nude person to another co-worker") to ones that were quite unlikely to involve sexual harassment ("A co-worker does not share important information with colleagues").

The results showed significant effects of the survey presentation on the answers to the questions and provided some indications of the mechanisms underlying these effects. First, situations that did not involve sexual harassment were reported more frequently under the workplace presentation of the items than under the sexual harassment presentation. It seems likely that respondents who got the sexual harassment framing of the questions tried to be cooperative by reporting only those situations that were related to the perceived intent of the survey – that is, incidents that clearly involved sexual harassment – and by omitting those that were unrelated to sexual harassment. Second, the respondents who got the sexual harassment presentation of the items said they would be more bothered on average by most of the situations than the respondents who got the workplace presentation, and they interpreted all of the situations as more likely to represent sexual harassment. Respondents seem to use the sponsorship and title of the questions in resolving ambiguities in the descriptions of the situations in the questions, inferring that even the relatively innocuous situations must have involved sexual harassment when the presentation of the survey indicated that that was the topic of interest. Third, the situations that did clearly involve sexual harassment were reported more frequently under the sexual harassment presentation. The sexual harassment presentation of the questions may have facilitated recall of actual instances of sexual harassment.

1.3. Common Causes of Nonresponse Bias and Reporting Error

More than 40 years ago, Cannell and Fowler (1963) found a relation between reluctance to participate and reporting error in a survey about hospital stays. Respondents who required more extensive follow-up to recruit were more likely to underreport the number of their hospital stays and to misreport their length than respondents who did not require so much follow-up. Their explanation for this finding was that reluctant respondents, once they were persuaded to complete the survey, were less motivated and gave less effort to the response task, providing less accurate answers than those who were cooperative from the start.

Bollinger and David (2001) report similar findings linking response propensities and measurement error. Respondents in the Survey of Income and Program Participation who subsequently dropped out of the panel provided significantly less accurate reports in the waves that they did complete than respondents who did not drop out later. Olson (2007) proposed a model of the link between response propensities and measurement error, the "common cause" model, in which the same influence affects both the likelihood of response and the level of response error. Various aspects of the survey presentation may act as common causes, affecting the likelihood of cooperation as well as measurement errors.

2. Research Design

We carried out two seemingly unconnected web surveys to evaluate the effects of the presentation of the survey request on nonresponse, measurement errors, and their potential linkages. Only respondents to the first survey were invited to participate in the second. Data were collected from January 25, 2007, to February 1, 2007, for the first survey and from March 2, 2007, to March 19, 2007, for the second. Market Strategies, Inc., conducted both surveys.

A total of 3,000 respondents completed the first survey. Half of the respondents were selected from Survey Sampling Inc.'s (SSI) Survey Spot frame, and the other half were selected from the e-Rewards web panel. Both are opt-in panels whose members had signed up online to receive survey invitations via email. The response rate (AAPOR 1) for the first survey was 4.1% among the SSI members and 14.8% among the e-Rewards members. Response rates to the second survey are discussed in detail below. We defined a respondent to a given survey as someone who completed the entire questionnaire and we discarded the data from cases who quit part way through. We chose to use volunteer Internet panels because they offer samples with a diverse demographic mix at relatively low cost. We were aware of the potential risks of studying influences on survey cooperation within a set of persons who had opted into a web survey panel, but we nonetheless believed that the experimental manipulations might affect their participation decisions. Further, our focus was on the cooperation and response behavior in the second survey among those responding to the first survey. In Section 3.1, we evaluate the representativeness of our sample by comparing the demographic characteristics of the respondents with population figures from the American Community Survey (ACS). We also compare the distribution of responses to some of the attitude questions in our survey to those obtained from similar items in the General Social Survey. The results for the two web panels were quite similar and we combined them in presenting the results.

The first survey was designed to collect potential predictors of both response propensities and answers to the second survey. The presentation of the first questionnaire was the same for all 47,078 web panel members sampled for the study. The email invitation and the first screen of the survey identified the University of Michigan as the sponsor and "attitudes and lifestyles" as the topics. Among the items in the initial questionnaire were a number of potential predictors of response propensities to the second questionnaire. These predictors included:

- Questions on how often the respondents had responded to web survey requests in the past;

- Items assessing the respondents' views about the importance of a number of social issues, including issues related to the topic of the second questionnaire (crime and violence, gun control);
- Items assessing support for or opposition to gun control policies;
- Relevant factual items (such as the presence of a gun in the household and experiences using guns);
- Rating scales on the prestige of various organizations, including the sponsors of the second questionnaire.

One month after the end of data collection for the first questionnaire, all 3,000 respondents were invited to participate in the second survey. Respondents were not told that this survey had any relationship to the first.

The main independent variables involved the presentation of the request to complete the second questionnaire. We varied both the purported sponsor and topic. Respondents were randomly assigned to one of three fictitious sponsors for the second questionnaire. We intended two of the sponsors, the National Coalition of Gun Owners and the National Coalition for Victims of Gun Violence, to represent the two poles of the controversy about gun control policy. The remaining third of the sample received what we thought would be a neutral sponsor, the National Center for the Study of Crime. As we noted earlier, prior studies indicated that the sponsorship and topic of a survey can affect both the probability that sample members will take part (e.g., Groves, Presser, and Dipko 2004; Groves et al. 2006) and the answers they ultimately provide (Galesic and Tourangeau 2007; Norenzayan and Schwarz 1999). These effects on response propensities and responses are presumably a function of the valence and intensity of the respondent's feelings about the assigned sponsor, which we measured in the first questionnaire. For example, an individual highly favorable toward the Coalition of Gun Owners is presumably most likely to respond when the survey is presented under that sponsorship, less likely to respond in the Center for the Study of Crime condition, and least likely to respond in the Coalition for Victims of Gun Violence condition. Similarly, someone who is highly favorable to the Coalition of Gun Owners may answer differently when the questions come from that source than from the Coalition for Victims of Gun Violence. The topics "crime prevention" and "gun control policies" were selected to suggest contrasting frameworks that respondents might apply in interpreting potentially ambiguous questions, such as "Have you ever used a gun?"

The final factor in the experimental design was the anticipated length of the second questionnaire. Sample members were randomly assigned to one of three length conditions. The email that invited panel members to take part in the second survey told approximately 40% of them that the survey was expected to take 20 minutes to complete. Roughly equal numbers of the remainder were told that the length was five minutes or were given no indication regarding the expected length of the second questionnaire. We expected the response burden suggested by the longer questionnaire to depress overall response propensities.

To summarize our hypotheses, we expected attitudes toward the sponsor and topic to affect both the likelihood of cooperation and response behavior on questions relevant to the sponsor's likely point of view. Similarly, we expected topic interest to affect response propensities and answers to questions in the survey. We test these hypotheses examining nonresponse and measurement error in a second survey among those who had responded to an initial survey.

3. Results

Our main analyses examined three issues: (1) Did the framing of the survey request affect the probability that a given respondent would complete the second survey? (2) Did the framing of the request affect the answers the respondents provided in the second survey? (3) Was there any connection between the survey presentation and the level of care respondents gave to formulating their answers in the second survey? Prior to examining those questions, however, we address the issue of the representativeness of the web panel members who took part in our survey. We compare the demographic make-up of our respondents with population figures from the American Community Survey; we also compare the distribution of responses on several attitude items on our first web survey with responses to similar questions on the General Social Survey.

3.1. Representativeness of the Respondents

Both the SSI and e-Rewards respondents come from nonprobability panels. They are large samples of volunteers. Even if they had started as reasonably representative samples of the U.S. adult household population, the high levels of nonresponse to the first survey would raise concerns about the generality of our results. We therefore compare the members of our samples to two external benchmarks. We compare the demographic composition of our respondents to figures from the 2006 American Community Survey (ACS). The ACS is a very large sample survey carried out by the U.S. Census Bureau to provide intercensal population estimates; 2006 is the most recent year for which figures are available. We also compare the attitudes of our sample members as expressed in the first survey to figures from the 2006 General Social Survey (GSS). The GSS is an attitude survey conducted by the National Opinion Research Center. Like the ACS sample, the GSS sample is an area probability sample. In recent years, the GSS response rate has been approximately 70 percent.

Table 1 displays the distribution of demographic characteristics for the first and second survey respondents and the corresponding figures from the 2006 ACS. The respondents to our first and second surveys are quite similar in their demographic characteristics, reflecting the relatively high response rate to the second survey (67%) and the consequent overlap in the samples. The most striking difference between our respondents and the ACS respondents involves reported educational attainment. Our respondents include too many college graduates (50% and 52% in the first and second surveys) and too few who did not finish high school (1% in both of our surveys) relative to the ACS (where 25% graduated from college and 14% did not complete high school). In addition, our respondents underrepresent the youngest age group (18 to 29 years of age) by 7 to 10% and overrepresent those who are 60 or older by 6 to 10% relative to the ACS. So, although our respondents do include members of diverse subgroups, they are hardly a microcosm of the adult household population of the U.S.

Table 2 displays the distribution of the first and second survey respondents on a few variables related to political attitudes, including reported party affiliation and gun ownership. The table also displays comparable figures for the respondents to the 2006 GSS. Although the question wording in the GSS differed somewhat from that in our first web survey, the response distributions are quite similar for these three variables.

Table 1. Distribution (in Percent) of First Survey, Second Survey, and ACS Respondents, by Selected Demographic Characteristics

Demographic subgroup	Our sample		ACS sample
	First survey	Second survey	
Male	50	51	48
Female	50	49	52
	100	100	100
White	83	84	77
Black	10	9	11
Others	7	7	12
	100	100	100
Hispanic	11	10	13
Non-Hispanic	89	90	87
	100	100	100
18–29	14	11	21
30–39	17	16	19
40–49	19	19	21
50–59	22	22	17
60 +	28	32	22
	100	100	100
Less than high school	1	1	14
HS graduate	13	13	33
Some college	36	34	29
College graduate or more	50	52	25
	100	100	100

Note: Percentages from the first survey are based on approximately 3,000 respondents with minor fluctuations due to item nonresponse; percentages from the second survey are based on approximately 2,022 responding cases, again with minor fluctuations due to missing data.

In each sample, just over one-third of the respondents (34 to 35%) reported having a gun in their household. The web survey respondents are also similar to the GSS respondents with respect to political ideology (conservative versus liberal) and party affiliation (although our web respondents are somewhat more likely than the GSS respondents to be Democrats and somewhat less likely to be Independents). Despite whatever selection and nonresponse biases affect our samples, the respondents to our two surveys do not appear grossly different from a representative national sample of adults on such key characteristics as gun ownership or ideology.

3.2. *Response Propensity and Nonresponse Bias*

We had little reason to expect main effects on completion of the second survey for either the topic or sponsorship variables. Instead, we expected these variables to interact with the sample members' views about the topic and sponsor of the survey request they received. By contrast, we expected the reported length of the second survey to have a main effect on its response rates. In addition, we thought that the impact of the reported length might be

Table 2. Distribution (in Percent) of First Survey and GSS Respondents on Three Common Variables

	Our sample		
	First survey	Second survey	2006 General Social Survey Sample
	Do you or anyone in your household own a gun?		Do you happen to have in your home (or garage) any guns or revolvers?
Yes	34	35	34
No	66	65	64
No answer	0	0	2
Total	100	100	100
	In general, would you describe your political views as. . . very conservative, conservative, moderate, liberal, or very liberal?		We hear a lot of talk these days about liberals and conservatives. I'm going to show you a seven-point scale on which the political views that people might hold are arranged from extremely liberal – point 1 – to extremely conservative – point 7. Where would you place yourself on this scale?
Liberal	25	26	26
Moderate	43	42	39
Conservative	32	32	35
Total	100	100	100
	In politics today, do you consider your- self. . .Republican, Demo- crat, Independent, Other, No preference?		Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?
Democrat	35	35	30
Republican	27	27	27
Independent/other	38	38	43
Total	100	100	100

Note: Percentages from first survey are based on approximately 3,000 respondents, with minor fluctuations due to item nonresponse; percentages from the second survey are based on approximately 2,022 responding cases, again with minor fluctuations due to missing data. Estimates from the GSS are based on approximately 2,000 respondents.

reduced among those with highly positive attitudes toward the sponsor or among those who had high levels of interest in the topic.

In line with these expectations, the response rates for the second questionnaire were comparable under the two topic conditions. Slightly more than two-thirds of those in both the crime prevention (67.8%) and gun control policy (67.0%) conditions completed the second survey ($\chi^2(1) = 0.22$, ns). Likewise, response rates were fairly constant ($\chi^2(2) = 1.35$, ns) across the three sponsorship conditions: Center for the Study of Crime (68.0%), Coalition of Gun Owners (66.0%), and Coalition for Victims of Gun Violence (68.2%).

The expected survey length did affect decisions to complete the second questionnaire. Fully 69.9% of those anticipating a five-minute questionnaire completed the second survey, which compares to 65.4% and 67.6% among those anticipating 20 minutes or receiving no information about the survey length, respectively. The contrast between the response rates for the five- and twenty-minute conditions was significant ($z = 2.18$, $p = .030$).

Our key research hypotheses, however, concern interactions of the survey topic and sponsor with respondents' predispositions toward their assigned treatments on those variables. We computed the predisposition measures – affinity for the assigned sponsor and interest in the assigned topic – using responses from the first questionnaire. The first survey asked respondents to evaluate twelve organizations, such as Greenpeace, the Christian Coalition, and the National Science Foundation. The three fictional sponsors to be used in the second survey were also included among the twelve organizations. We asked respondents to rate on seven-point scales the value of the work done by each organization (with "very harmful" and "very valuable" as the endpoints) and their level of trust in each one (with "don't trust at all" and "trust completely" as the endpoints). We averaged these two ratings for each respondent to produce an overall rating for each sponsor. Then we took a z-score across the twelve means for each respondent so that affinity for the sponsor that the respondent actually received was measured relative to his or her ratings of the other eleven organizations. We refer to this variable as the respondent's affinity for the sponsor. The z-score approach helps us avoid overestimating (or underestimating) the level of sponsor affinity among respondents who systematically rated all twelve organizations more (or less) favorably than other respondents.

We measured interest in the topic using a similar approach. In the first questionnaire, respondents rated the importance of ten issues, including the two topics used to describe the second questionnaire, and we computed z-scores for interest in the assigned topic using these ratings.[5]

Contrary to our expectations, there was little evidence for a positive relationship between affinity for the assigned sponsor and participation in the second survey. Nor does there appear to be a positive relationship between interest in the assigned topic and participation rates. Table 3 shows the results from two logistic regression models predicting completion of the second questionnaire. The reduced model includes the effects of the three experimental variables (topic, sponsor, and burden), as well as the interactions

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[5] We also computed averages of the unstandardized ratings; the results do not differ appreciably if we use these raw scores instead of the standardized scores.

Table 3.   Logistic Regression Coefficients for Two Models Predicting Completion of Second Survey Questionnaire

Predictors of completion of second questionnaire	Reduced model Estimate	(S.E.)	Full model Estimate	(S.E.)
Predictors based on survey methodology theory				
Burden: expected 5 min.	0.13*	(0.06)	0.13*	(0.06)
Burden: expected 20 min.	−0.08*	(0.06)	−0.15*	(0.06)
Burden: not mentioned (reference)				
Sponsor: Gun Owners Coal.	−0.05	(0.07)	−0.12	(0.07)
Sponsor: Victims Coal.	0.03	(0.06)	0.03	(0.07)
Sponsor: Center for Study of Crime (reference)				
Topic: Crime prevention	0.03	(0.05)	0.04	(0.05)
Topic: Gun control policies (reference)				
Affinity for assigned sponsor	0.11	(0.07)	0.10	(0.07)
Interest in assigned topic	−0.03	(0.05)	−0.05	(0.05)
Interactions				
Sponsor: Gun Owners Coal. * Affinity	−0.02	(0.08)	0.06	(0.09)
Sponsor: Victims Coal. * Affinity	−0.12	(0.10)	−0.15	(0.10)
Topic: Crime prevention * Topic interest	0.03	(0.05)	0.03	(0.05)
Topic: Crime prevention * Sponsor: Gun Owners Coal.	0.06	(0.06)	0.07	(0.06)
Topic: Crime prevention * Sponsor: Victims Coal.	0.01	(0.06)	−0.02	(0.06)
Predictors based on behavior as a web panel member				
Panel: e-Rewards			0.55***	(0.09)
Panel: SSI (reference)				
Enrolled in five or more web panels			0.65***	(0.12)
Enrolled in four or fewer web panels (reference)				
Number of web survey invites refused in past 12 months			−0.27***	(0.04)
− 2 Log Likelihood	3,300		2,104	

Note: The interest and affinity variables are based on z-scores for the relevant items. We excluded respondents who rated all topics or all sponsors equally, since their z-scores were undefined. *$p < .05$; **$p < .01$; ***$p < .001$.

of sponsor with affinity for the assigned sponsor and of topic with interest in the assigned topic. The coefficients for affinity toward the assigned sponsor and the interaction of affinity with the sponsor condition are not significantly different from zero. Similarly, interest in the assigned topic is not related to response to the second questionnaire, nor is its interaction with the topic condition. In short, there was little support for the hypothesis that affinity for the sponsor or topic influenced the likelihood of participation in the second survey.

If web panel members do not decide whether to complete the survey on the basis of its purported topic or sponsor, what factors do they consider? We identified a set of variables reflecting the respondent's experiences as a web panel member. The full model in Table 3 shows the large effects on response propensity associated with these web panel behavior measures. Membership in numerous (five or more) web panels and belonging to the e-Rewards panel rather than the SSI panel were both strong predictors of response ($b = 0.65, p < .001$, and $b = 0.55, p < .001$, respectively). The odds of responding to the second questionnaire were 1.7 times greater for e-Rewards members than for the SSI members, holding the other factors constant. In addition, the number of survey invitations refused in the past twelve months has a powerful negative relationship with response propensity ($b = -0.27, p < .001$). The inclusion of these variables leads to a sharp improvement in the fit of the model (the -2 Log Likelihood statistic drops from 3,300 to 2,104).[6] It is clear that the experimental manipulations pale in comparison to the characteristics of the panels and how they use their members. This may be evidence of distinct influences on participation likelihood that are specific to volunteer web survey panelists.

Although topic and sponsor had no effect on the proportion of the respondents to the first survey who completed the second one as well, it is still possible that these variables affected the level of nonresponse bias in the estimates from the second survey. For example, the response rates may be the same in the Coalition for Victims of Gun Violence and the Coalition of Gun Owners conditions, but the characteristics of these two responding samples may be quite different. The design used for this study (in which some of the same questions appear in both surveys) allows us to carry out a fairly straightforward evaluation of nonresponse bias due to attrition. Using data from the first survey, for each of six key items we assess the potential for attrition bias by comparing estimates based on the respondents to the second questionnaire ($n = 2,022$) with estimates based on nonrespondents to that questionnaire ($n = 978$). If a feature of the survey presentation, such as the reported survey sponsor, has an effect on who elects to participate, then estimates based on those responding under that sponsor may differ from estimates based on the sample that responds under a different sponsor. In this study, we

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[6] It is not clear why the members of the two panels differ so sharply. One possible explanation is the difference in the incentives offered. The e-Rewards panel uses a points system, in which members are credited with a certain amount of "e-Rewards currency" for each survey that they complete. The first and second questionnaires in this study were each worth $3.50 in e-Rewards currency. The SSI Survey Spot panel, by contrast, features a $25,000 sweepstakes as well as an "instant win" game for which members are eligible when they complete the questionnaire. In light of these different incentive structures, it is perhaps not surprising that the e-Rewards members (who receive a more tangible per-survey reward) were significantly more likely to respond to the second questionnaire than the Survey Spot members.

expected that the Coalition of Gun Owners sponsor condition might disproportionately attract respondents from the first survey who oppose gun control generally. Likewise, we expected that the Coalition of Victims of Gun Violence condition would disproportionately attract respondents to the first survey who support gun control. With respect to the topic manipulation, we anticipated that the "gun control policies" manipulation might draw in respondents with more extreme views on the issue relative to the "crime prevention" topic.

Table 4 displays results for six key outcome variables measured in the first survey. These items are whether a gun is present in the respondent's household, the respondent's experience using a gun, an index of support for various gun control measures based on a battery of six items, a question on support for gun control versus gun owner rights, political ideology, and an index of political engagement based on the response to six items. A priori, we would expect larger differences for variables that are highly correlated with the sponsorship variables (e.g., those related to gun control attitudes). For each item, the comparisons of the results for the respondents and nonrespondents are computed overall and within each experimental condition.

In a few instances, the experimental variables appear to have the effects we anticipated. For example, those responding to the second questionnaire under the Coalition of Gun Owners sponsorship were significantly less likely to describe themselves as political "moderates" than those who did not respond to the second questionnaire under this sponsor ($\chi^2(1) = 8.72$, $p = .003$). Those responding under the Center for the Study of Crime sponsor were significantly less likely to report that they or another member of their household had ever used a gun compared to those not responding under this sponsorship ($\chi^2(1) = 8.30$, $p = .004$). Still, the modest magnitude (at most 12 percentage points) and general inconsistency of these effects across related measures suggest that the levels of nonresponse bias due to attrition in estimates from the second questionnaire are quite low. Furthermore, six of the ten significant results in Table 4 disappear when we control for demographic correlates of response propensity (age, race, and education). Differences between respondent and nonrespondent means that remain significant after we introduce demographic controls are denoted in the table by the ^ symbol. The four significant differences after adjustment are only slightly larger than the 2.7 expected by chance alone. All in all, the experimental variables seem to have little impact on either the response rates or nonresponse bias in estimates in the second survey.

3.3. *Impact of Topic and Sponsor on Measurement Properties of the Second Survey*

Although the purported topic and sponsor of the second survey did not appear to have much effect on its response rates or on the composition of the respondent pool, it is still possible that these variables affected the answers respondents gave to the second survey. The second survey questionnaire included a large number of items related to the topic and sponsor. To reduce the number of variables to a manageable size, we grouped them into three composites – one measuring concerns about personal safety (based on four items in the second questionnaire), one measuring attitudes toward gun control (based on 11 items), and one assessing involvement in the issue of gun control (based on three items). We examined responses to these composites as a function of the topic and sponsorship,

Table 4. Estimates for Six Key Items from First Survey, By Response Status in Second Survey

			Gun in household	Ever used a gun	Index of support for gun control	Favor gun control over gun rights	"Moderate" political views	Political engagement index	N
Total		All 1st Survey respondents	34%	25%	31.8	60%	43%	2.9	3,000
		2nd Survey nonrespondents	31%	27%	31.9	61%	47%	2.9	978
		2nd Survey respondents	35%*	24%	31.8	60%	42%*	2.9	2,022
		Estimates computed for each sponsor condition							
Sponsor	Center for Study of Crime	2nd Survey nonrespondents	34%	30%	31.7	60%	42%	2.9	320
		2nd Survey respondents	35%	22%**^	31.8	61%	43%	2.9	680
	Gun Owners Coalition	2nd Survey nonrespondents	30%	27%	32.3	62%	52%	3.0	340
		2nd Survey respondents	36%	24%	32.0	60%	42%**^	2.8	660
	Victims Coalition	2nd Survey nonrespondents	31%	23%	31.6	62%	45%	2.9	318
		2nd Survey respondents	34%	27%	31.8	59%	40%	2.8	682

Table 4. Continued

			Gun in household	Ever used a gun	Index of support for gun control	Favor gun control over gun rights	"Moderate" political views	Political engagement index	N
			Estimates computed for each topic condition						
Topic	Crime prevention	2nd Survey nonrespondents	33%	30%	31.4	61%	48%	3.0	482
		2nd Survey respondents	35%	25%	31.9	61%	41%**	2.9	1,017
	Gun control policies	2nd Survey nonrespondents	30%	24%	32.3	61%	45%	2.9	495
		2nd Survey respondents	35%*	23%	31.7	59%	42%	2.8	1,005
			Estimates computed for each expected burden condition						
Burden	5 minutes	2nd Survey nonrespondents	32%	29%	32.0	64%	52%	2.8	271
		2nd Survey respondents	33%	23%*	31.9	60%	42%**^	2.8	629
	No mention	2nd Survey nonrespondents	33%	31%	31.6	58%	44%	3.1	292
		2nd Survey respondents	35%	25%	31.5	60%	42%	2.9*^	608
	20 minutes	2nd Survey nonrespondents	30%	22%	32.0	62%	45%	2.9	415
		2nd Survey respondents	37%*	26%	32.0	59%	42%	2.9	785

Note: $*p < .05$; $**p < .01$; ^ indicates difference remains significant after controlling age, education, and race.

controlling for gun control attitudes reported in the first survey by the same respondent. For each composite, we carried out three-way analyses, classifying respondents as above or below the median on their gun control attitudes reported in the first survey and by the purported topic and sponsor of the second survey.

We found effects of the experimental variables on two of the composites, but not on the third (the one based on the four items on personal safety). For the issue involvement variable, the purported topic of the second survey interacted significantly with gun control attitudes – $F(1, 2009) = 5.76, p < .05$. The topic variable seemed to have opposite effects for those with different views on gun control. Those who were below the first survey median in their support for gun control measures reported greater involvement in the issue of gun control when the topic was described as crime prevention (mean of 2.47) than when it was described as gun control (mean of 2.36); those who were above the median in their support of gun control reported greater involvement in this issue when the topic was gun control rather than crime prevention (2.48 vs. 2.44). For the gun control composite, there was a significant main effect for topic – $F(1, 2009) = 4.45, p < .05$. Respondents reported more support for gun control when the topic was described as crime prevention than when it was described as gun control (the means are 0.03 and −0.03, respectively). There was also a marginal sponsor by attitudes interaction $F(2, 2009) = 2.96, p < .06$.[7] Figure 1 displays the means on the gun control attitudes composite for each combination of second survey sponsor and first survey attitudes. With the two sponsors likely to have strong views about gun control (the National Coalition of Gun Owners and the National Coalition for Victims of Gun Violence), respondents seem to become polarized in their reported gun control attitudes. Those who were relatively favorable to gun control in the first survey became even more favorable in the second; by contrast, those who were relatively unfavorable to gun control in the first survey became even less favorable in the second. This polarization seemed especially marked when the second survey sponsor was the National Coalition for Victims of Gun Violence.

We also examined responses to a set of questions asking respondents how often they encountered seven potentially threatening situations (such as walking alone after dark or driving through an unfamiliar neighborhood). A repeated measures analysis of variance on these items revealed one significant effect – an item by topic interaction $(F(6, 11742) = 2.22, p < .05)$. Table 5 shows the difference between the mean frequency ratings under the two topic conditions, by item. Positive numbers indicate the frequency ratings were higher on average when the topic was described as crime prevention; negative numbers indicate that the frequency ratings were higher on average when the topic was described as gun control. The items reported as more frequent when the topic was crime prevention were clearly crime-related (e.g., hearing about a crime in the neighborhood), whereas the items reported as more frequent when the topic was gun control were not directly related to crime (e.g., being home alone after dark).

The first and second survey questionnaires included a number of common items, and we looked at how answers to these items differed between the two surveys. We examined the

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[7] In addition, for the gun control composite, there was (as expected) a highly significant main effect of gun control attitudes reported in the first survey – $F(1, 2009) = 1,679.4, p < .001$.
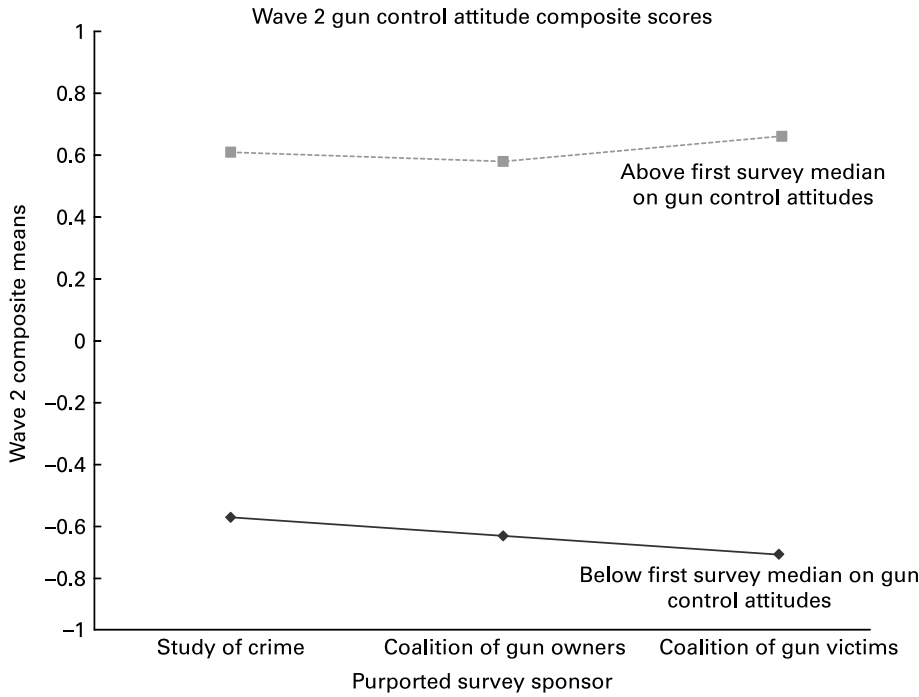
Fig. 1. Mean attitudes on gun control composite in second survey by purported survey sponsor and first survey gun control attitudes

hypothesis that respondents might tailor their answers in a direction that was favorable to the sponsor, particularly when they reported high levels of affinity for the sponsor they received. For example, we determined whether each respondent reported more or less support for gun control measures in the second survey than in the first. For each respondent, we counted up the number of such changes in a pro-gun direction and analyzed this variable as a function of the purported topic and sponsor of the second survey and of attitudes toward the purported sponsor reported in the first survey (with respondents

Table 5. Difference in Mean Frequency Ratings across Topic Conditions (Crime Prevention − Gun Control), by Item

Item	Difference in means
You encounter a stranger who appears mentally unstable	0.08
You hear about a crime that occurred in your neighborhood	0.05
You drive or ride through an unfamiliar neighborhood	0.04
Someone at your workplace becomes noticeably angry or nervous	0.02
A stranger knocks on the door to your home	0.01
You are home alone after dark	− 0.12
You walk outside alone after dark	− 0.19

Note: Sample sizes are approximately 1,000 per topic group. A positive difference indicates that the situation was seen as more frequent when crime prevention was the topic; a negative difference indicates that the situation was seen as more frequent when gun control was the topic.

classified by quartiles on their affinity for the sponsor); this analysis used raw scores rather than *z*-scores. A three-way analysis of variance revealed a significant overall effect for sponsor attitudes on our index of changed responses ($F(3, 1890) = 3.58$, $p < .05$). Examination of the relevant means for the change variable shows only one clear pattern (see Table 6). There was a clear increase in the number of pro-gun changes with greater affinity for the sponsor when the sponsor was said to be the National Coalition of Gun Owners; within this condition (the second column of means in the table), there is a significant linear trend by affinity quartile ($F(1, 620) = 12.1$, $p < .01$). When the sponsor is supposedly a gun owners group, the more favorable the respondents are to that group, the more likely they are to change their answers in a progun direction.

Table 6.   *Differences in Responses (Second Survey – First Survey), by Purported Sponsor of Second Survey and Affinity toward that Sponsor*

Affinity for the survey sponsor	National Center for the Study of Crime	National Coalition of Gun Owners	National Coalition for Victims of Gun Violence
Lowest affinity quartile	1.30	1.11	1.12
Second quartile	1.31	1.30	1.31
Third quartile	1.32	1.45	1.33
Highest affinity quartile	1.40	1.50	1.20

Note: Each mean based on approximately 150 respondents from the second survey.

3.4.   *Relation Between Presentation and Sloppy Responding*

Our final set of analyses examined whether there was any relationship between interest in the survey topic and affinity for the sponsor and various indicators of sloppy responding or measurement error in the second survey. Our hypothesis was that liking for the sponsor and interest in the topic would lead to more careful processing of the questions. We examined several behaviors that we thought were likely to reflect careless processing of the questions; these included item nonresponse, straight-lining responses to items in grids (giving the same answer to every item in the grid), fast completion of the survey (defined as completion times in the fastest decile), failing to check a button on an item ostensibly included "for quality control purposes," and inconsistent responses across the two surveys. According to Krosnick (1991, 1999), item nonresponse and straight-lining are manifestations of satisficing in the survey response process. Completion times are a rough measure of the overall amount of effort and attention spent on completing the web questionnaire. Similarly, failing to follow instructions on an item that asked respondents to click a check box and inconsistent responses to the same questions across the two surveys could reflect careless or superficial processing of the survey questions. (Because of the relatively short interval between the two surveys, we thought most changes in response were likely to reflect measurement error rather true change.) Each of these behaviors can thus be seen as indirect indicators of measurement error.

Sponsor affinity had a significant negative effect on respondents' straight-lining behavior in the second survey. We defined straight-liners as respondents who gave identical answers to every question in at least one grid. As shown in Figure 2, those with
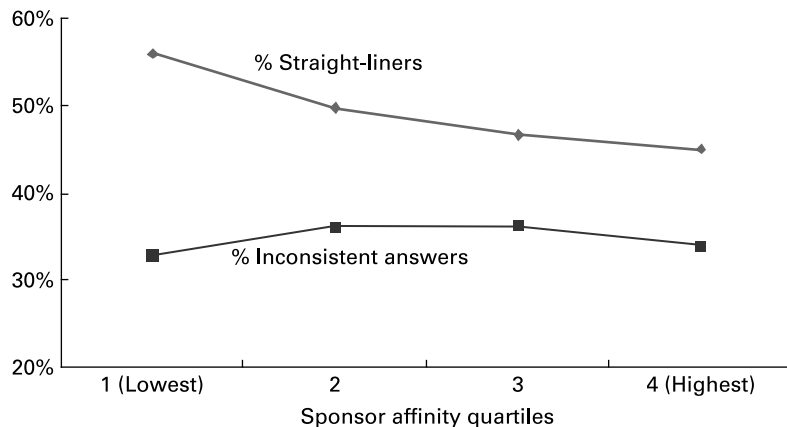
Fig. 2. Sponsor affinity and the proportion of respondents who gave straight-lining responses in at least one grid and the mean percentage of inconsistent answers between the two surveys

greater affinity for the sponsor were less likely to straight-line than those with less affinity ($\chi^2(3) = 13.7$, $p = .003$). However, there seems to be a curvilinear relation between sponsor affinity and the mean percentage of inconsistent responses to the same questions in the two different surveys. Respondents at the extremes (those in the lowest and highest affinity quartiles) gave fewer inconsistent responses on average than those in the middle two quartiles ($F(3, 1910) = 6.74$, $p = .0002$). There were no statistically significant relations between sponsor affinity and the other measures of sloppy responding. By contrast, topic interest affected only one of the indicators – item nonresponse in the second survey. To our surprise, those in the higher topic interest quartiles were more likely to have missing data than those in the lowest interest quartiles ($\chi^2(3) = 12.3$, $p = .006$); the percentage of cases with at least one missing item ranged from 21.7% in the second-lowest interest quartile to 30.7% in the highest interest quartile.[8] The other measures of sloppy responding did not seem to vary significantly across topic interest quartiles.

4. Discussion

4.1. Summary

We varied several features of the presentation of the request to take part in a web survey, including the description of the topic, sponsor, and length of the survey, and we examined the effects of these variables on response rates, nonresponse bias, the answers respondents provided, and the level of care respondents took in completing the questions. Although previous work has demonstrated the effects of the framing of the survey request, particularly the survey topic, on response propensities and nonresponse bias, we find little

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[8] In general, item nonresponse rates in the second survey were quite low. For most items, the missing data rate was less than 1%. Only three items had missing data rates higher than 2% – an item asking for the respondent's year of birth (2.9%), one asking which Constitutional amendment addresses gun ownership (4.7%), and an item asking the respondent when he or she had last gotten a request to do an Internet survey before ours (2.3%). There was no indication that item nonresponse was higher for gun-related items than for other items in the survey.

evidence that the topic or sponsor had much effect either on the overall response rate or on survey estimates (see Tables 3 and 4). The stated length of the survey did affect response rates, but none of the presentation variables seemed to alter the make-up of the responding sample.

We did observe effects of the presentation of the second survey on the answers to the questions. For example, the purported topic of the survey appeared to affect responses to questions about how involved respondents were in the issue of gun control, but this effect depended on the respondent's attitude toward gun control (as reported in the first survey). The respondents also reported greater support for a series of gun control measures when the topic was described as crime prevention than when it was described as gun control. This main effect of the topic variable is likely to represent a priming effect (in which the crime prevention framing of the survey brings to mind arguments that favor gun control). Still, the effect is quite small; the difference across topic conditions is about .07 of the overall standard deviation in scores on the gun control composite we created.

We also found some reporting effects like those described by Galesic and Tourangeau (2007): Crime-related situations seemed to be rated as more frequent when the topic was crime prevention than when it was gun control, and the reverse was true for items that were less clearly related to crime (but that nonetheless evoked a sense of personal threat). These reporting differences could reflect the respondents' attempts to report the incidents they saw as most relevant to the researcher's aims (as Norenzayen and Schwarz (1999) have argued). The most striking reporting effect we observed involved changes in the answers to items that were in both surveys. Among respondents who thought the second survey was sponsored by the National Coalition of Gun Owners, the more favorable their attitudes to that organization (as measured in the first survey) the more likely they were to change their answers in a pro-gun owner direction (see the second column of means in Table 4). Respondents who liked that sponsor seemed to shade their answers in a direction the sponsor was likely to approve. This effect goes beyond reporting what seems relevant, because it depends on the respondents' attitudes toward the sponsor. It seems likely that respondents were trying to "help out" a sponsor they were sympathetic to by providing answers that supported the sponsor's aims.

Our final set of analyses explored the relation between several measures of careless responding, on the one hand, and affinity for the sponsor and interest in the topic, on the other. Neither liking for the sponsor nor interest in the topic seemed to have consistent effects on careless responding.

4.2. Failure to Affect Response Rates

Our study is one of several attempts to manufacture nonresponse bias by manipulating the description of the survey to potential respondents. These attempts have had a somewhat mixed track record. For three of the populations they examined, Groves, Presser, and Dipko (2004) found large topic effects on response rates (reflecting presumed differences in levels of interest in the topics), but for a fourth population – contributors to fringe political candidates – they did not observe such effects. Groves and his colleagues (Groves et al. 2006) report several attempts to replicate and extend the findings of the earlier study, but only one of their three efforts to affect response rates (the birding study

discussed earlier) was successful. It may be that descriptions of the topic and sponsor of the survey, as they are commonly presented to sample persons, tend to be minor influences on participation decisions. In our study, although the topic and sponsor of the second questionnaire were mentioned in the text of the email invitation and displayed prominently in boldface on the survey splash page, the email invitations came, as is customary, from the web panel providers, not from the purported survey sponsors themselves. This may have reduced the effect of the sponsorship. The degree of salience of the sponsor and topic in the survey request that is required to make these factors important influences on response propensities clearly deserves more study.

Another possibility is that the mode of data collection and our use of web panel members may have limited the effects of the survey presentation variables. Respondents who have volunteered to do surveys and are subsequently contacted via email to complete an on-line questionnaire may take less notice of details about the survey than would those who had not volunteered or who were asked to answer the questions in a different mode. Groves and his colleagues (Groves et al. 2006) make a similar argument that the impact of the *description* of the topic may be smaller in mail surveys (where respondents can see all of the questions before deciding whether to take part) than in telephone surveys, although the effect of the actual content of the questions may be larger in a mail survey. As in a mail survey, prospective web respondents can scroll through a questionnaire beforehand, and this may reduce the effects of descriptions of the topic; in our study, everyone got the same questions regardless of how we characterized them.

A related problem is that Internet panels may not be the best vehicle for studying the effect of the survey presentation on nonresponse. The respondents to the first questionnaire consisted of 1,500 panel members from the e-Rewards panel and an equal number from the SSI Survey Spot panel. Members of both panels have committed to responding to surveys sent by those companies, regardless of their content. Members of both panels can receive multiple survey requests during a single week – or even during a single day. Based on administrative data, we found that the median number of survey requests per week is 1.5 among the e-Rewards panel members who responded in the first survey. The SSI panel members report that they receive even more web survey invitations (although no administrative data were available to verify these reports). Faced with a steady barrage of solicitations, it is quite possible that web panel members, over time, come to ignore certain aspects of the survey requests, such as the ones we tried to manipulate. Although Tables 1 and 2 suggest that our sample respondents do not differ markedly from the general population in terms of their demographic characteristics or political attitudes, they clearly differ in the number of surveys they complete.

4.3. Presentation Effects on Measurement Error

Despite these weak effects on response propensities, the survey presentation variables, in particular our descriptions of the topic and sponsorship of the second survey, did have significant (if small) effects on the answers respondents provided. We anticipated two different effects of the framing of the survey request – systematic changes in the answers and changes in the level of effort respondents gave in formulating their answers. The first type of change was likely to produce bias in survey estimates and the second type to

increase their variance. We found some evidence for both types of effects. For example, respondents reported crime-related situations as occurring more frequently when the stated topic of the survey was crime prevention than when it was gun control but reported other ambiguously threatening situations as more frequent when the purported topic was gun control. And the more favorable respondents were to the National Coalition of Gun Owners, the more likely they were to change their answers in a progun direction when they thought that that organization sponsored the second survey. In addition, we found some evidence that carelessness in responding was related to relatively unfavorable attitudes toward the sponsor (Figure 2).

Even though our sample is limited to members of two web panels, we believe that these measurement effects may be inevitable consequences of the framing of the survey request to potential respondents. Every survey has to be presented to the sample members in some way and the description of the survey is likely to serve as a contextual cue, shaping the respondents' understanding of the questions and triggering the recall of some incidents, possibly at the expense of others. Even neutral-sounding but official sponsors (like the U.S. National Center for Health Statistics or the U.S. Bureau of Justice Statistics) may encourage respondents to interpret the questions in a particular way – for example, to adopt narrow definitions of the intended concepts. It is clearly important to understand how the survey presentation affects who takes part in the survey and how the respondents formulate their answers to the questions – and what steps can be taken to minimize these effects of the framing of the survey request.

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