

**THE IMPACT OF “NO OPINION”
RESPONSE OPTIONS ON DATA
QUALITY**
NON-ATTITUDE REDUCTION OR AN
INVITATION TO SATISFICE?

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Abstract According to many seasoned survey researchers, offering a no-opinion option should reduce the pressure to give substantive responses felt by respondents who have no true opinions. By contrast, the survey satisficing perspective suggests that no-opinion options may discourage some respondents from doing the cognitive work necessary to report the true opinions they do have. We address these arguments using data from nine experiments carried out in three household surveys. Attraction to no-opinion options was found to be greatest among respondents lowest in cognitive skills (as measured by educational attainment), among respondents answering secretly instead of orally, for questions asked later in a survey, and among respondents who devoted little effort to the reporting process. The quality of attitude reports obtained (as measured by over-time consistency and responsiveness to a question manipulation) was not compromised by the omission of no-opinion options. These results suggest that inclusion of no-opinion options in attitude measures may not enhance data quality and instead may preclude measurement of some meaningful opinions.

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Since Converse (1964, 1970) proposed that survey respondents might at times report non-attitudes, social scientists have been deeply concerned about this disquieting assertion. Converse's direct experience conducting social survey interviews led him to hypothesize that these settings exert implicit pressure on respondents to appear opinionated on a wide range of topics. When confronted with a question on a topic to which one has devoted no previous thought, the natural inclination would presumably be to acknowledge having no opinion on the matter. But when confronted with a long sequence of such questions on diverse issues, Converse suggested, most people will probably be uncomfortable repeatedly acknowledging what might appear to be wide-ranging ignorance. Therefore, he said, respondents may cope by randomly selecting responses from among the choices offered in order to appear opinionated.

Given how much impact Converse's writings have had throughout the social sciences, it is remarkable that little research has explored techniques that might be used to discourage or eliminate non-attitude reporting. In this article, we focus on one possible technique for doing so: offering a no-opinion option (see, e.g., Schuman and Presser 1981). Such options can take many different forms, and we will examine four of them: explicitly legitimizing the opportunity for respondents to say that they "haven't thought much" about the topic of an attitude question, to say that they "would not vote" on a proposed referendum, to say they "don't have enough information" to answer an attitude question, and to say they have "no opinion" about a particular issue. We begin below by reviewing the existing evidence on the impact of no-opinion options on the quality of data obtained, and then we report the results of experiments designed to assess whether these four sorts of options reduce reporting of non-attitudes.

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Existing Evidence

A number of prominent survey researchers have recommended the routine use of “don’t know” options in questionnaires, presumably in order to minimize non-attitude reporting (see, e.g., Berdie and Anderson 1974; Bogart 1972; Converse and Presser 1986, pp. 35–36; Oppenheim 1992, pp. 128–129; Payne 1950; Vaillancourt 1973). This recommendation was emphasized with regard to contingent valuation (CV) surveys, which attempt to ascertain the economic value individuals place on public goods such as wildlife sanctuaries or clean air (e.g., Mitchell and Carson 1989). Controversies about the method and an associated detailed review by a panel of distinguished social scientists led to a report recommending that CV studies adopt a referendum format asking respondents whether they would vote for or against a proposal to provide a particular public good at a stated price (Arrow et al. 1993). The panel also indicated that the response categories presented for this question should include a “not-vote” response, arguing that it offers the most neutral way to provide a “no-answer” option.

This perspective is based on a set of underlying assumptions. First, it presumes that some people have opinions on any given issue and are aware of possessing those opinions, whereas other people do not have opinions and are aware that they do not. All of the former individuals are presumed to report their opinions, regardless of whether or not a no-opinion response option is offered by a question. But the behavior of people without opinions is presumed to be contingent on question format. These individuals are presumed to report the fact that they have no opinion accurately when a no-opinion option is offered, but when no such option is offered, some or all of these people may fabricate reports of “non-attitudes” due to pressure to appear opinionated. Such non-attitude responses might be the result of the respondent choosing purely randomly among offered response alternatives (Converse 1964) or making a choice driven by the structure of the question.

Consistent with this logic, many studies have shown that many more respondents will say they have no opinion when such a response option is offered than will volunteer this response when no such option is presented (Bishop, Oldendick, and Tuchfarber 1983; Schuman and Presser 1981). However, the crucial question here is whether offering a no-opinion option attracts only respondents who would otherwise have offered meaningless responses, or whether offering a no-opinion option also attracts respondents who truly have opinions and would otherwise have reported them.

If offering a no-opinion option reduces non-attitude reporting, it should strengthen correlations between opinion reports and other variables that should, in principle, be correlated with them. If non-attitude reports are random responses, then offering a no-opinion option should reduce the amount of random variance in the attitude reports obtained (which can be estimated directly with cross-sectional data; e.g., McClendon and Alwin 1993) and

should increase the over-time consistency of opinion reports (which can be estimated with panel data). And if non-attitude reports are sometimes biased responses driven entirely by the format of the survey question, then offering a no-opinion option should reduce the magnitude of format-driven effects, such as acquiescence or response order effects, which can be estimated using split-ballot experiments (e.g., Schuman and Presser 1981). We refer to this set of proposals as the “non-attitude reduction” hypotheses. Both hypotheses propose that offering a no-opinion option will remove meaningless responses from a set of answers.

The existing evidence with regard to these hypotheses is mixed. Some studies have involved nonexperimental designs. For example, Gilljam and Granberg (1993) measured a sample’s attitudes toward building nuclear power plants, sometimes offering no-opinion options and sometimes not. Most people who said they had no opinion when offered that option reported having an opinion when the no-opinion option was omitted. And the opinions these people offered manifested sufficiently strong and sensible correlations with other variables to suggest that many of the answers given were meaningful, though not all were.

In meta-analyses of the correlates of the amount of random measurement error in numerous survey items, some of which included no-opinion options and others of which did not, Andrews (1984) found that the amount of random error was significantly less when a no-opinion option was included than when it was not, but Alwin and Krosnick (1991) found just the opposite. And Scherpenzeel and Saris (1997) and Wikman and Wärneryd (1990) found no significant effect of offering no-opinion options on reliability or validity. It is difficult to know precisely what to make of these findings because the presence or absence of no-opinion options was confounded with many other item attributes in these studies (e.g., question topic, response scale length) and this confounding was different in the four studies.

In a set of 19 experimental studies, Schuman and Presser (1981) found that offering a no-opinion option did not lead to stronger correlations between substantively related variables in the vast majority of cases. Similarly, Sanchez and Morchio (1992) found that more aggressive probing of “don’t know” answers by interviewers did not affect the strength of relations between variables. Via structural equation modeling, McClendon and Alwin (1993) showed that offering a no-opinion option did not reduce the amount of random variance in responses, contrary to the non-attitude reduction hypothesis. And McClendon (1991) found that acquiescence and response order effects were no weaker when a no-opinion option was offered, challenging the notion that this option attracts respondents who would otherwise answer in biased ways, driven by question format.

Satisficing Theory

These results are surprising if one presumes that people accurately report having no opinion when encouraged to do so. But when viewed from a different perspective, these results are not so surprising. This perspective suggests the hypothesis that no-opinion responses should not always be viewed as accurate. That is, we should not presume (as the perspective outlined above does) that every time a person chooses a no-opinion response option, he or she cannot report a meaningful response.

For example, Bradburn and Sudman (1988) suggested that respondents sometimes “use a ‘don’t know’ answer to temporize while they are searching their memories and will give an answer if the interviewer probes or simply waits a bit longer” (p. 154). Thus, some people who might be inclined to say “don’t know” may truly have opinions worth measuring. This logic presumably underlies the recommendation in the Survey Research Center’s (1976) *Interviewer’s Manual* for interviewers to challenge “don’t know” responses by repeating the question, pausing, providing a reassuring remark (e.g., “Well, we’re just interested in your general ideas about this”), or implementing a neutral probe (e.g., “What are your ideas about this?”) to “encourage the respondent to reply. . . . it is a good idea to probe all of the ‘don’t know’ responses that occur” (Survey Research Center 1976, p. 17; see also Williams 1942).

Bradburn and Sudman (1988) also noted that “many pollsters believe that people who do not have clearly formulated opinions still lean in one direction or another. They wish to encourage people to give a substantive response by omitting an explicit ‘don’t know’ category” (p. 154). This is one reason why the substantive responses gathered from people otherwise inclined to select a no-opinion option may be meaningful and useful. Yet another possibility was suggested by Feick (1989), who speculated that some no-opinion responses may occur because respondents are not fully certain of the intended meaning of a question, and if the meaning were to be clarified, most of these respondents would be able to offer substantive answers. In line with this perspective, Fowler and Cannell (1996) viewed “don’t know” responses as indicative of inadequacies in question design. And Oppenheim (1992) noted that “it has been argued that some people give a ‘don’t know’ response in order to avoid thinking or committing themselves” (p. 129). These responses might mask real opinions as well and might therefore be best discouraged.

This latter perspective was codified and elaborated recently in Krosnick’s (1991) theory of survey satisficing. This theory is based on the presumption that answering survey questions accurately usually entails cognitive work for respondents, and this is typically true for attitude reports. Attitudes are defined as latent dispositions toward objects. These dispositions are sometimes consolidated in a single judgment that a person has stored in memory and can readily retrieve and report. But more often, attitudinal dispositions are not

consolidated in memory and instead are a set of distinct “ingredients” that, when combined together, yield an overall evaluative orientation toward an object. These ingredients have been referred to as “considerations” by Zaller (1992). Reporting an attitude therefore usually entails searching one’s memory for relevant ingredients and integrating those ingredients into a summary judgment (see, e.g., Schwarz and Bonner 2001; Strack, Schwarz, and Wänke 1991). Then, the respondent must translate that opinion into an understandable response (Tourangeau and Rasinski 1988).

If a person has a preconsolidated opinion in memory about the issue addressed in a survey question, then retrieving and reporting that opinion should happen quickly and easily. And if a person lacks a preconsolidated opinion and in fact has absolutely no relevant information in memory with which to construct an opinion, then he or she will presumably answer “don’t know.” Satisficing theory focuses on people who do have relevant considerations available in memory but must construct overall evaluations by integrating those considerations.

The cognitive demands of doing this work to “optimize” may sometimes exceed a respondent’s motivation or ability. In such situations, respondents may seek out ways to avoid doing this work while still appearing as if they are carrying on a survey interview or completing a questionnaire appropriately. Krosnick (1991) referred to this behavior as “survey satisficing,” to be contrasted with “optimizing.”

The likelihood that a respondent will satisfice is thought to be a function of three classes of factors: respondent ability, respondent motivation, and task difficulty. People who have more limited abilities to carry out the cognitive processes required for optimizing are more likely to shortcut them. People who have minimal motivation to carry out these processes are likely to shortcut them as well. And people are most likely to shortcut when the cognitive effort demanded by a question is substantial. A respondent will be inclined to implement the most extreme form of satisficing, “strong satisficing,” when ability and motivation are at their minima and task difficulty is at its maximum.

According to Krosnick (1991), a respondent inclined to implement “strong satisficing” will look for cues in the question suggesting how to offer a seemingly defensible answer with no thought about the question at all. A no-opinion response option would constitute just such a cue, and it would encourage respondents who are otherwise disposed to implement strong satisficing to do so by saying “don’t know.” If a no-opinion option is not offered and no other cue is apparent, these respondents might choose not to satisfice and might therefore be pushed to do the cognitive work necessary to search their memories for relevant information, integrate the information into a judgment, and express it, yielding a report of a genuine attitude. Thus, in the absence of a no-opinion option, useful data could be collected from these individuals.

There are other reasons to imagine that omitting no-opinion response options

might lead to reporting of genuine attitudes. For example, some “don’t know” answers may occur because respondents are not sure how to interpret the question stem or the response choices and might be reluctant to make a decision in this regard (e.g., Faulkenberry and Mason 1978). Or respondents might feel ambivalent about an object and might therefore be reluctant to take a stand one way or another (e.g., Klopfer and Madden 1980). And some respondents might be hesitant to report a potentially embarrassing or socially undesirable opinion and might prefer to say “don’t know” instead (e.g., Noelle-Neumann 1993). In all of these cases, if a respondent was encouraged instead to report an opinion, his or her response might be a meaningful representation of his or her orientation toward the object of interest.

One might imagine that if people inclined to implement strong satisficing are not offered a no-opinion option, they would choose another strong satisficing response strategy, such as answering purely randomly (see Krosnick 1991). Or, alternatively, these individuals might choose to implement “weak satisficing” instead, such as by answering “agree” in response to a question offering “agree” and “disagree” as response options, or by selecting the first response option they consider that seems plausible (see Narayan and Krosnick 1996). Such answers might appear on the surface to be substantive and meaningful, but they would not in fact represent genuine opinions (or the ingredients of opinions) held by respondents and would not manifest any signs of validity. Therefore, omitting a no-opinion option might induce people inclined toward strong satisficing to answer questions substantively, or it might lead them to answer in systematic ways driven by question structure. However, McClendon (1991) found that acquiescence response bias and response order effects were no stronger when a no-opinion option was omitted than when it was offered. This leaves open the possibility that omitting no-opinion options might induce people inclined to implement strong satisficing to answer substantively instead.

This Investigation

Our goal in conducting the studies reported here was to explore the viability of this view in comparison with the more traditional, non-attitudes-prevention perspective. If a nontrivial proportion of no-opinion responses are in fact accurate reports from people who genuinely lack opinions, and forcing these people to express substantive opinions yields reports of non-attitudes, then offering no-opinion options should increase the reliability and validity of the answers people give to a question. But if most no-opinion responses are attempts by satisficers to skirt the work required by optimizing, then encouraging these people to express substantive opinions by omitting the no-opinion option may not compromise the reliability and validity of the answers that are given.

The best way to address these issues might seem to be to identify individuals

who choose an offered no-opinion response option because they are satisficing. Unfortunately, there is no simple way to do this. One approach to doing so is to ask the same respondents the same question once with the no-opinion response option included and once with it omitted. Then the substantive answers given by people who selected the offered no-opinion option could be examined for validity. But carry-over effects from the initial question on answers to the later question can contaminate such an investigation. Therefore, we employed a more indirect approach that avoids such effects. In the three studies we conducted, some respondents were offered no-opinion response options and others were not. We compared the responses of these two groups to assess attraction to and the impact of the no-opinion option.

STUDY 1

In our first study, we used data from the 1989 National Election Study Pilot to examine the impact of offering versus omitting a no-opinion option on no-opinion responding, expecting to see that offering such an option increased the frequency of “don’t know” answers. We also tested a hypothesis derived from satisficing theory: that attraction to an offered no-opinion response option should be greatest among people with the most limited ability to optimize. For this test, we used a proxy measure of general cognitive skills: amount of formal education. Amount of formal education has been shown to be extremely strongly correlated with more direct measures of cognitive skills (for a review, see Ceci 1991).

Our first study also tested whether offering a no-opinion response option increased the quality of attitude reports obtained. In doing so, we followed Converse’s (1964) lead and examined whether offering a no-opinion option increased the over-time consistency of attitude reports. This test was predicated on the presumption that meaningless answers to attitude questions are unreliable, so eliminating them by offering a no-opinion option should increase the reliability of the opinions that are offered.

STUDY 2

For our second study, we analyzed data from a national contingent valuation (CV) survey assessing people’s values for a plan to protect the natural resources in Prince William Sound, Alaska, that were injured by the 1989 *Exxon Valdez* oil spill. This survey asked people how they would vote on a referendum proposing to prevent a future spill of this sort at a specified cost to the respondent (this survey was a replication of one done by Carson et al. [1992]). Experimental variation in the format of the referendum question allowed us to assess the impact of providing people the option not to vote at all, which we treat as similar to a no-opinion response.

Contingent valuation is an unusual survey methodology designed to achieve

an unusual purpose in an unusual way (Mitchell and Carson 1989). These surveys are unusual mostly because they do not simply involve asking people questions about their opinions on matters of public discussion. Rather, CV questionnaires typically begin by presenting a large amount of information to respondents about a set of circumstances with which they are probably not familiar, sometimes lasting as long as 30 minutes. Then respondents are asked to make judgments about the situation.

In typical opinion surveys, such as in the NES pilot study analyzed for our first study, a researcher cannot know whether a particular respondent has a substantive basis for forming an opinion on an issue, so it is impossible to know for sure whether a no-opinion response is the result of optimizing or satisficing. But in a CV survey, respondents are given a great deal of information, and this information set is usually designed to answer all the questions people might have when told about the situation. As a result, CV respondents are likely to have the information necessary to form attitudes on the matters in question. Therefore, a no-opinion response seems less likely to be a valid indication that a respondent has no opinion at all.

This property of the CV method makes it especially useful for examining the relation of education to selection of the no-opinion option. Because the information presentation respondents received in this study was designed to be understandable for people at all levels of education, this minimized the likelihood that less educated respondents would be less likely to have the information necessary to report meaningful opinions on the matters of interest. So if we see more no-opinion responses from less educated respondents, this context permits greater confidence that these responses reflect satisficing, rather than optimizing.¹

This study also allowed us to explore another hypothesis derived from satisficing theory: that attraction to a no-opinion option may be greatest when respondent motivation to optimize is lowest. We did so by comparing the half of the respondents in this study who announced their votes aloud to their interviewers with the other half of the respondents, who marked their votes completely secretly on paper ballots that they placed in ballot boxes. The use of a secret ballot is similar to an increasingly common procedure in face-to-face surveys, in which some sensitive questions are self-administered, on the assumption that respondents will be more honest in answering these questions privately (see Tourangeau, Rips, and Rasinski 2000, p. 296). One might imagine that secret voting would yield more accurate responses from people in this study.

However, there is a potential cost of secret voting: lack of identifiability. For decades, psychologists have explored the cognitive and behavioral con-

1. The wealth of information presented to respondents could have made them feel ambivalent, and this ambivalence may have led some respondents to select the no-opinion option. But we see no strong reason why such ambivalence should have been disproportionately concentrated among the least educated respondents.

sequences of identifiability, defined as the expectation that what a person says or does is linked to him or her personally. Many psychological studies have shown that allowing people to make decisions or take actions without being identifiable reduces the effort they invest in carrying out the task (e.g., Williams, Harkins, and Latané 1981; Zimbardo 1969). One particular consequence of identifiability is a sense of accountability, which psychologists define as “the implicit or explicit expectation that one may be called on to justify one’s beliefs, feelings, and actions to others” (Lerner and Tetlock 1999). One of the key ways social psychologists have experimentally manipulated accountability is by manipulating identifiability (for a review, see Lerner and Tetlock 1999). In many studies, research participants were asked to write their judgments on pieces of paper that did not also bear identifying information; these participants’ responses were not identifiable and were therefore considered to be made under conditions low in accountability. Participants who reported their judgments aloud to another person recognized that their judgments were linked directly to them in the eyes of others and were therefore treated as high in accountability (e.g., Lerner, Goldberg, and Tetlock 1998; Price 1987; Reicher and Levine 1994a, 1994b; Schopler et al. 1995; Tetlock and Boettger 1989; for a review, see Lerner and Tetlock 1999).

The difference between oral voting and secret voting in the survey that we analyzed maps directly onto this experimental manipulation technique. Psychologists view respondents voting orally as having a sense of identifiability-based accountability because they have to reveal their votes aloud to their interviewers. These respondents were also asked a number of open-ended questions during the survey asking them to describe their thinking process as they acquired the information, and this is likely to have further enhanced a sense of accountability for these people.² In contrast, respondents who voted secretly would have known that their responses were not linked to them and that they could not be asked to justify their votes because doing so would have compromised the secrecy of those votes. Therefore, these people voted under conditions matching those used in experimental studies of people low in accountability.

Many past studies have shown that identifiability-based accountability increases the effort people devote to making decisions, typically reducing the extent of cognitive bias manifested (for reviews, see Lerner and Tetlock 1999; Tetlock 1992). Therefore, the survey respondents who voted secretly may have had less motivation to devote cognitive effort to generating their answers than the respondents who voted orally.

Krosnick (1991) proposed that when respondents are less motivated to think carefully when answering a question, they may be especially inclined to shortcut via satisficing. If choosing an explicitly offered no-opinion response usu-

2. Although respondents who voted orally were not warned in advance, they were asked to explain the reasons for their votes after reporting them.

ally represents satisficing (by people who have the information necessary to report meaningful opinions but are inclined to skirt doing the work to report them), then a no-opinion option should attract more people when respondents vote secretly. But if, instead, no-opinion responses generally represent optimizing by people who genuinely lack opinions, there is no reason to expect that secret voting would increase attraction to explicitly offered no-opinion options.

Krosnick (1991) suggested that sources of respondent ability and motivation are likely to combine multiplicatively in instigating satisficing. So the impact of an ability variable such as cognitive skills should be most powerful under conditions of low motivation. In the present study, we tested this hypothesis by assessing whether the impact of cognitive skills was greatest when respondents answered a question using a secret ballot.

The satisficing hypothesis proposes that the least skilled respondents are most attracted to the no-opinion option because they choose not to report opinions they truly hold. But the “optimizing” view of no-opinion responses asserts that the least skilled respondents are most attracted to the no-opinion option because they have few opinions and accurately report that. If no-opinion responses do indeed represent optimizing for these respondents, however, the least skilled individuals should be more attracted to the no-opinion options when they report their votes aloud to interviewers than when they vote secretly (because oral voting presumably enhances the motivation to optimize). Thus, we could see if and how cognitive skills interact with voting method (i.e., oral or secret ballot) in order to assess whether the least skilled respondents offer more no-opinion responses because they are tending to satisfice or because they are tending to optimize.

This study also allowed us to examine whether offering a no-opinion option increased the quality of attitude reports obtained. To do so, we examined the responsiveness of answers to a manipulation that should have influenced voting if respondents were optimizing. The referendum respondents voted on involved a proposal implementing an oil spill prevention plan at a fixed cost for each respondent. Different people were told that the plan would cost different amounts of money to implement. If respondents were voting carefully, at higher prices, fewer people should have voted for the plan, assuming that as the price rises, it exceeds increasing numbers of people’s willingness to pay for the prevention plan. Therefore, the extent of care respondents devote to answering the vote question can be gauged in part by the magnitude of responsiveness to the rising price.

Proponents of no-opinion options would expect that offering a not-vote option allows respondents who truly have no opinion about the plan to say so. If a not-vote option is not omitted, these individuals might vote randomly or in a way driven by the question format and thereby reduce the sensitivity of votes to the price of the good. But opponents of no-opinion options might expect that omitting the not-vote option would inhibit satisficing and induce

respondents who would otherwise have satisficed to optimize instead, thereby producing no decline in responsiveness to price. Evidence of no decline in responsiveness to price would be inconsistent with the notion that offering no-opinion response options eliminates meaningless attitude reports.

STUDY 3

Our final study involved analysis of data from a survey conducted by the Center for Survey Research at Ohio State University, in which respondents were again randomly assigned to receive no-opinion response options in attitude questions or not. With these data, we again assessed whether attraction to the no-opinion option was greatest among respondents with little education. In addition, we examined the impact of an additional variable thought to contribute to satisficing: the location of a question in a survey.

During survey interviews, respondents may become fatigued, impatient, bored, annoyed, and disinterested, decreasing their motivation to engage thoughtfully in the cognitive steps necessary to optimize. This may be particularly true for respondents with the most limited cognitive skills. As a result, offering a no-opinion option may lead to the greatest increase in no-opinion responses among the least educated respondents when they answer questions late in a survey. This is an especially interesting hypothesis because an experimental manipulation of question placement cannot affect whether or not respondents truly held opinions prior to an interview, so any effect of question placement seems appropriately attributed to impact on the process of *reporting* opinions.

We also tested one final hypothesis derived from satisficing theory: that respondents are especially attracted to no-opinion options when they devote little effort to the process of answering. At the end of this survey, respondents were asked directly about the amount of effort they had put into answering the questions. If no-opinion responding is mostly the result of optimizing, we should find reported effort to be positively related to selection of no-opinion responses. But if no-opinion responding is mostly the result of satisficing, we should find reported effort to be negatively related to selection of no-opinion responses.

Study 1

DATA

Sample. The data we examined initially were collected as part of the 1989 National Election Study Pilot. A total of 1,640 individuals constituting a national probability sample of American adults had been interviewed face-to-face twice during the year before for the 1988 National Election Study

(NES) and had provided their telephone numbers to interviewers. The response rate for the first wave of the 1988 survey was 71 percent, and the response rate for the second wave of the 1988 survey was 87 percent (Miller and National Election Studies 1988).³ A sample of 855 of these individuals was then selected to be reinterviewed twice by telephone for the 1989 NES Pilot.

If respondents who were minimally interested in politics were especially likely to drop out of the panel, the 1989 respondents would have been disproportionately politically engaged. To prevent this potential bias in sample composition, a dual sampling procedure was used in which an equal probability sample was combined with an oversample of less politically involved respondents to ensure that 1989 respondents were similar to respondents in the 1988 NES (Miller et al. 1989).

For the first wave of the pilot study, during July and August 1989, 614 individuals were successfully reinterviewed, and 494 of those people were reinterviewed during September and October. Thus, the response rate for wave 1 was 72 percent (614/855), the response rate for wave 2 was 80 percent (494/614), and the time lag between interviews varied from 1 to 3 months.⁴

Measures. Education: Respondents were categorized into four education groups: less than a high school diploma, high school diploma, more than a high school diploma but no 4-year degree, and a 4-year degree or more. Because no-opinion responding is presumed to be a form of strong satisficing, we expected it to be especially concentrated among respondents in the lowest of these education groups and about equivalently weaker among people in the higher education groups (for consistent evidence, see Narayan and Krosnick 1996). This prediction is based on Ceci’s (1991) evidence that cognitive skills rise approximately linearly with years of formal education; there are fewer years’ difference among the top education groups, so the differences between them should be smaller. Therefore, education was coded 0 for people with at least a 4-year college degree, 1 for people with more than a high school diploma but no 4-year college degree, 4 for people with a high school diploma but no further education, and 9 for people with less than a high school degree.⁵

No-opinion responding: During both waves of the 1989 interviews, respondents answered questions about their political party identifications and their attitudes on U.S. defense spending, U.S. involvement in Central America,

3. The response rate for the first wave is AAPOR’s response rate 1. The response rate for the second wave is the percent of people who had been interviewed at wave 1 who completed the wave 2 interview.

4. The demographic biases we observed in the 1988 and 1989 samples were similar to those typically observed in national surveys (e.g., men and low-education and low-income respondents were underrepresented). The demographic characteristics of respondents who were and were not offered the no-opinion option did not differ notably.

5. This was done in Studies 2 and 3 as well, except where otherwise specified. Results using a simple linear coding of the education categories resulted in similar, although slightly weaker, relations.

and gun control laws (ratings were made on 7-point scales). During both waves, half of the respondents (selected randomly) were explicitly given the option of saying that they had not thought much about each matter.⁶ The remaining respondents were not explicitly given such a response option during either wave of interviewing.

RESULTS

Abstention rates. Consistent with prior studies, offering a no-opinion response option increased the proportion of respondents who declined to report each attitude. During the first wave, a larger percentage of respondents said they had no opinion when such an option was presented than when it was omitted in the case of party identification (12.5 vs. 1.0 percent, $\Delta = 11.5$ percent, $\chi^2(1) = 31.3$, $p < .001$, one-tailed), defense spending (12.7 vs. 1.7 percent, $\Delta = 11.0$ percent, $\chi^2(1) = 27.10$, $p < .001$, one-tailed), Central America (11.3 vs. 2.4 percent, $\Delta = 8.9$ percent, $\chi^2(1) = 18.51$, $p < .001$, one-tailed), and gun control (6.1 vs. 1.7 percent, $\Delta = 4.4$ percent, $\chi^2(1) = 7.94$, $p < .01$, one-tailed).⁷ We computed the proportion of questions to which each respondent answered with a no-opinion response; the impact of offering the no-opinion option on this index was positive and highly significant ($b = .09$, $SE = .01$, $p < .01$; see col. 1 of table 1). Comparable patterns appeared in the wave 2 data (data not shown).

As expected, attraction to the no-opinion option was greatest among the respondents lowest in formal education, as evidenced by the significant and positive interaction between offering/omitting the no-opinion option and education shown in column 2 of table 1 ($b = .02$, $p < .01$). Again, comparable patterns appeared in the second-wave data (data not shown).

Over-time consistency. Next, we computed unstandardized regression coefficients estimating the consistency of people's answers to the same target question across the two 1989 interviews (see cols. 1 and 2 of table 2). The resulting consistency estimates were not significantly different for any of the items depending on whether the no-opinion option was offered or not (party identification: $t = 0.08$, n.s.; defense spending: $t = 0.90$, n.s.; Central America: $t = 1.27$, n.s.; gun control: $t = 0.47$, n.s.). Furthermore, the directions of the nonsignificant trends were not consistent across issues: the regression

6. For a randomly selected half of the respondents, the 7-point rating scales were presented in a decomposed branching format, whereby respondents reported which side of each issue they were on first and then reported how extremely they were on that side. The other half of the respondents simply selected one of seven arrayed response options in a single step (for details, see Krosnick and Berent 1993). This manipulation was crossed with the manipulation involving the no-opinion option, and no significant interactions appeared between these manipulations.

7. Reported p -values are one-tailed when we had strong theoretical justification for a directional hypothesis and the observed effect is in the expected direction; two-tailed p 's are reported for tests without strong directional hypotheses or for tests where the observed effect runs in the direction opposite to expectations.

coefficients were greater on the filtered form for defense spending and Central America, greater on the unfiltered for gun control, and just about equal on both forms for party identification. The same lack of significant differences was obtained when we examined just the data from respondents who had not graduated from high school. Thus, we cannot reject the null hypothesis that offering the no-opinion option had no impact on this indicator of data quality.

To examine consistency in a different way, we calculated a new variable for each issue, coded 1 for respondents who gave the same answer in both 1989 interviews and coded 0 for respondents who gave different answers in 1989. We then averaged these variables together across the issues to yield an index of over-time consistency. In line with our analysis of the over-time correlations for individual issues, offering or omitting the no-opinion response option was unrelated to this consistency index ($b = .03$, $SE = .03$, n.s.). This was also true when we controlled for education, age, race, gender, and income ($b = .02$, $SE = .03$, n.s.). And the effect of offering the no-opinion option on the consistency index did not vary across the education groups (education \times no-opinion filter interaction: $b = .01$, $SE = .01$, n.s.). Thus, these tests do not support the non-attitude reduction hypothesis.

One might argue that the numbers of respondents who chose the offered no-opinion option was so small that even if all these individuals had answered purely randomly if the no-opinion option had been omitted, there would have been no observable difference between the numbers in columns 1 and 2 of table 2. To test how different the numbers in columns 1 and 2 would have been if all respondents who had selected the no-opinion option when it was offered had answered randomly if the no-opinion option was omitted, we ran a simulation. For each respondent who chose the no-opinion option when it was offered, we randomly generated an answer on the response scale (i.e., the probability of each response was equal) and then reestimated the coefficients shown in column 2 of table 2. We repeated this exercise 25 times.⁸ The average coefficients from these 25 trials are shown in column 4 of table 2, and the differences between these coefficients and those in column 1 of table 2 are shown in the fifth column of the table.

The numbers in column 4 would have appeared in column 1 if all no-opinion option selectors had answered randomly. But in fact the numbers in column 4 are notably lower than those in column 1, by .11 units on average, a statistically significant difference ($p < .05$; see the bottom of the last column in table 2). In contrast, the average difference between columns 1 and 2 of table 2 was much smaller, .03 units (n.s.). This suggests that many respondents who chose an offered no-opinion option did not answer randomly when a no-opinion option was not offered.

8. We examined whether implementing more trials affected the conclusions we drew from these simulations and found no notable differences regardless of whether we implemented 25, 50, 75, or 100.

Table 1. Regressions Predicting “No-Opinion” Responding

Predictors	Study 1		Study 2					Study 3						
	1	2	3	4	5	6	7	8	9	10 ^a	11 ^b	12	13	14
“No-opinion” option offered vs. omitted	.09** (.01)	.03** (.02)	1.43** (.22)	.95** (.27)	1.10** (.27)	.66* (.31)	.76** (.34)	.23** (.02)	.19** (.02)	.41** (.06)	.38** (.06)	.49** (.08)	.45** (.09)	.46** (.11)
Education		.00 (.00)		-.00 (.02)		.00 (.02)	-.01 (.03)		.00 (.01)	.02 (.01)	-.01 (.01)		.00 (.00)	.00 (.02)
Secret ballot					-.99* (.45)	-.99* (.45)	-2.79** (1.04)							
Effort												-.11 (.08)	-.11 (.08)	-.12 (.10)
“No-opinion” option offered vs. omitted × education		.02** (.01)		.15** (.04)		.14** (.04)	.11* (.06)		.02* (.01)	-.01 (.02)	.08** (.02)		.02* (.01)	.01 (.03)
“No-opinion” option offered vs. omitted × secret ballot					.91* (.50)	.86* (.50)	2.36* (1.09)							

Table 2. Unstandardized Regression Coefficients Estimating the Consistency Over Time of Reports of the Same Attitudes During Two Consecutive Interviews in 1989

Attitude	"Haven't Thought" Option		Difference between Cols. 1 and 2	"Haven't Thought" Option Offered; "No- Opinion" Responses Replaced with Ran- dom Responses	Difference between Cols. 1 and 4
	Not Offered	Offered			
Party identification	.88** (223)	.89** (211)	.01	.72** (253)	.16*
Defense spending	.62** (226)	.67** (218)	.05	.49** (254)	.13*
Central America	.58** (223)	.68** (214)	.09	.51** (253)	.07
Gun control	.72** (228)	.69** (236)	-.03	.64** (252)	.08
Average	.70	.73	.03	.59	.11*

NOTE.—Numbers of cases appear in parentheses below the coefficients. None of the differences in the third column of this table are statistically significant. Coefficients in the fourth column were obtained by averaging the results from 25 iterations in which random responses replaced "no opinion" responses in the data that yielded the numbers in column 2. Significance tests for the values in column 4 were obtained by dividing the average coefficient over the 25 iterations by the average standard error for the coefficients in those 25 iterations.

* $p < .05$.

** $p < .01$.

Summary. This study suggests that offering a no-opinion option did not increase the consistency of reports of the same attitude over time. This finding is in line with the notion that people who selected the no-opinion option when offered would not have answered purely randomly (as Converse suggested) if the no-opinion option had not been offered. In principle, these individuals could have answered nonsubstantively based on a content-free response bias, which could have yielded an artifactual appearance of stability. Past research suggests that such response biases are not likely to be stable over a period as long as a month at the level of the individual item (e.g., Hui and Triandis 1985), so this may not be a problem here. Nonetheless, our next study investigated this issue in a way in which systematic measurement error is unlikely to create the appearance of validity.

Attraction to the no-opinion option was greater among respondents with less formal education, which is consistent with satisficing theory's predictions. However, this latter finding is also consistent with the notion that no-opinion responses represent optimizing, because less educated citizens may be less likely to form opinions on political issues. Our subsequent studies further investigated the relation between education and no-opinion responding to assess whether it is most likely to reflect optimizing or satisficing.

Study 2

DATA

Sample. For this study, the National Opinion Research Center interviewed a probability sample of 1,182 American adults living in 12 primary sampling units (selected from the NORC master sampling frame on the basis of their having sufficient numbers of interviewers to complete the work without hiring new staff).⁹ Interviews were conducted face-to-face in respondents' homes during May, June, and July of 1993. Of dwelling units that were occupied by English-speaking households, the response rate for the survey was 73 percent (for more detail about this survey, see the report available at <http://www.rff.org/~kopp/Reports.htm>).¹⁰

Measures. The questionnaire presented to respondents a very detailed scenario describing the condition of Prince William Sound before and after the *Exxon Valdez* spill. Interviewers presented this information to respondents orally by reading a lengthy script, which was accompanied by visual displays of maps, diagrams, drawings, and photographs. Extensive pretesting (via focus groups and one-on-one interviews) was conducted to assure that the information presentation provided respondents with all the information they would want to make subsequent decisions and presented that information in an easily digestible fashion. On average, the presentation lasted about 20 minutes.

After the spill was described in detail, a proposed plan to prevent further spills was outlined using numerous visual aids. The plan would involve setting up a fleet of escort ships to guide oil tankers into and out of the Sound and to contain oil if some were to be spilled. Respondents were told that implementing the plan would cost their households a specified dollar amount, to be paid as a one-time federal income tax. Respondents were then asked

9. These included Baltimore, MD; Birmingham, AL; Boston, MA; Charleston, SC; Harrisburg, PA; Ft. Wayne, IN; Manchester, NY; Nicholas County, KY; Portland, OR; Richmond, VA; Seattle, WA; and Tampa, FL.

10. This response rate corresponds to AAPOR's response rate 1. The small demographic biases in this sample were similar to those observed in Study 1. The respondents offered and not offered the no-opinion option and the respondents who voted secretly and orally did not differ notably in terms of demographics.

whether, if an election were being held and this plan were proposed on a referendum, they would vote for or against the proposal.

Respondents were randomly assigned to two different versions of the potential referendum question. Half of the respondents were asked, "If the program cost your household a total of [\$x], would you vote for the program or against the program?" The particular dollar amount included in the question (\$x) was one of four values (\$10, \$30, \$60, and \$120), varied randomly across respondents. When individuals said they would not vote at all or that they didn't know how they would vote, interviewers were instructed to accept those answers rather than pressing respondents to say they would vote either for or against the program. The other half of the respondents were asked instead, "If the program cost your household a total of [\$x], would you vote for the program, against the program, or would you not vote?"

Crossed with the manipulations of the presence of the not-vote option and the cost amount was the manipulation of voting method. Half of the respondents (again, selected randomly) answered the voting question orally. The other half of respondents were handed a paper ballot. The ballot for the group without a not-vote option displayed the choices "Vote For" and "Vote Against." The ballot for the group with a not-vote option had a third choice in addition: "I Would Not Vote." The respondents were also given an envelope in which to place the ballot and a wooden box with a small opening on the top, and they were told, "Because elections in the United States use secret ballots, I will ask you to record your vote on a secret ballot. Please don't tell me how you vote. This is the ballot that you will use. Once you have voted, please seal the ballot in the envelope and drop it in this ballot box. Your ballot will be sent, unopened, to the National Opinion Research Center. I will not know how you voted." These individuals knew the interviewer would not ask them to justify their answers to the voting question, because doing so would reveal a respondent's vote choice. The variable we call "voting method" was coded 1 for respondents who voted secretly and 0 for respondents who voted orally. For the analyses reported below, we combined respondents who selected the not-vote option, who were coded as saying "not sure," and who refused to answer into a single group.¹¹

Respondents were also asked to report how much formal education they had, and responses were coded as in Study 1. All other variables were coded as in Study 1.

RESULTS

Abstention rates. As in Study 1, the proportion of abstainers increased from 4.7 percent on the unfiltered form to 17.2 percent on the filtered form, an increase of 12.5 percentage points ($\chi^2 = 46.17, p < .001$; probit coefficient

11. Results did not differ when we excluded refusals from the analyses.

= 1.43, SE = .22, $p < .01$, see col. 3 of table 1). The propensity to select the not-vote option when offered was also related to education as expected, as indicated by the significant interaction between education and offering/omitting the not-vote option (probit coefficient = .15, SE = .04, $p < .01$, see col. 4 of table 1). The least educated respondents manifested the largest effect (20.0 percentage points), and the most educated manifested the smallest effect (4.4 percentage points).

As column 5 of table 1 shows, voting method affected the magnitude of this response effect significantly and as expected (probit coefficient = .91, SE = .50, $p < .05$). Offering the not-vote option attracted 11.0 percent of respondents who voted orally and 14.0 percent of the respondents who voted secretly. This effect remained significant when controlling for the effects of education (probit coefficient = .14, SE = .04, $p < .01$) and voting method (probit coefficient = .86, SE = .50, $p < .05$) simultaneously (see col. 6 of table 1).

Also as expected, the effect of offering the not-vote option was greatest among the least educated respondents who voted secretly ($\Delta = 25.5$ percent) and smallest among the most educated respondents who voted orally ($\Delta = 1.9$ percent). The impact of education on the response effect was strong and significant among respondents who voted secretly (probit coefficient = .18, SE = .05, $p < .01$) and was weaker among respondents who reported their votes orally (probit coefficient = .11, SE = .06, $p < .01$), and the three-way interaction between voting method, education, and the inclusion of the not-vote option was statistically significant (probit coefficient = $-.27$, SE = .15, $p < .05$; see col. 7 of table 1).¹²

Data quality. Next we looked at the impact of the not-vote option on responsiveness of people’s votes to the manipulation of the cost of the proposed program. A centerpiece of economic theory is the notion that willingness to pay is elastic with regard to price: as the price of a good increases from zero to a large number, the proportion of people willing to buy the good usually decreases (e.g., Samuelson 2001). If respondents in this survey approached their task in a thoughtful manner, then they should have been responsive to price as well: fewer people should have voted for the program as its price increased.

12. After reporting how they would vote, respondents were asked four “quiz” questions about information they had been given: the amount of damage that would occur if the plan were not implemented, where the proposed plan would prevent oil spills, how effective the proposed plan would be at preventing oil spills, and the number of years they would have to pay extra taxes to pay for the proposed program. Each respondent was given a score reflecting the number of correct answers he or she gave to these four questions, and we repeated all analyses in table 1 controlling for this measure of information about the proposal. Less educated respondents were less likely to answer these questions correctly ($r = .12$, $p < .01$, $N = 1,160$), but controlling for this index did not alter the effects of education apparent in table 1. This suggests that less educated respondents were not more attracted to the not-vote option when offered because they were less likely to understand the information they had been given.

If respondents who would have chosen the not-vote option if offered answered randomly or otherwise arbitrarily when the not-vote option was not offered, there should be more random or systematic error in responses from people not offered the not-vote option. And more such error would yield the appearance of less responsiveness of answers to the program cost because these respondents would be answering the vote question without regard to the price offered.

The effect of cost on support for the plan was significant when the not-vote option was offered (probit coefficient = $-.99$, $SE = .27$, $p < .01$) and even stronger when the not-vote option was omitted (probit coefficient = -1.30 , $SE = .26$, $p < .01$). These figures were not significantly different from one another ($z = .84$, n.s.). All this suggests that including a not-vote option did not change measured responsiveness to cost. If the magnitude of this responsiveness is an indicator of data quality, then a not-vote option did not increase quality.

To see whether our design could detect a decrease in data quality due to omitting the not-vote option, we examined what the effect of cost on votes would have been if respondents who selected the not-vote option had not been given this option and had instead randomly chosen a response. We did so by conducting 25 trials generating random responses for respondents who chose the not-vote option when it was offered. The average effect of cost on voting across these trials (average probit coefficient = $-.80$, $SE = .24$, $p < .01$) was nearly 40 percent smaller than the average effect of cost among people not offered the not-vote option (coefficient difference = $.50$, $z = 1.32$, $p < .10$). This difference did not appear in the real survey data, which suggests that omitting the not-vote option could in principle have decreased the responsiveness of vote choices to cost but did not do so in practice.

Our interpretations of the results thus far are based on the premise that secret voting led to a lowered sense of accountability and reduced the effort people devoted to generating optimal answers. If this is so, then responsiveness to cost should have been higher under oral voting conditions than under secret voting conditions. Consistent with this presumption, people who voted orally manifested a significant, negative relation of cost to voting in favor of the program (probit coefficient = -1.39 , $SE = .26$, $p < .001$), and this relation was only 60 percent as strong among people who voted secretly (probit coefficient = $-.88$, $SE = .26$, $p < .01$; difference: $z = 1.38$, $p < .08$).

Furthermore, the relation of cost to voting was strongest among people who were not offered a not-vote option and who voted orally (probit coefficient = -1.47 , $SE = .37$, $p < .001$) and weakest among people who were offered the not-vote option and who voted secretly (probit coefficient = $-.62$, $SE = .38$, $p < .05$). The relation of cost to voting was significant and of intermediate magnitude among the other two groups of respondents (not-vote option omitted, secret voting: probit coefficient = -1.12 , $SE = .37$, $p < .01$; not-vote option offered, oral voting: probit coefficient = -1.33 ,

SE = .37, $p < .001$). Because respondents were randomly assigned to these four groups, this pattern is consistent with the conclusions that offering a not-vote option and secret voting both compromised response quality individually and interactively.

Study 3

DATA

Sample. For this study, the Center for Survey Research at Ohio State University conducted computer-assisted telephone interviews with a national probability sample of adults ($N = 300$) and a probability sample of adults from Franklin County, Ohio ($N = 217$). These samples were combined for our analyses. Interviewing of the national sample began on August 4, 1999, and ended on August 19, 1999; interviewing of the Franklin County sample began on August 19, 1999, and ended on August 30, 1999. Of all known-eligible households, the response rate for the national sample was 21.2 percent; for the Franklin County sample, it was 23.7 percent.¹³

Measures. No-opinion responding: During a long interview in which a wide range of political topics were addressed, all respondents answered questions about the harshness with which courts deal with criminals, the power of the government in Washington, federally provided day-care, and allowing a book by a communist author to be in a public library. All these questions required respondents to choose one of two substantive response options (e.g., the government “should provide day-care” or “shouldn’t provide day-care”). Five questions were asked before the first target question. The four target questions were each separated from one another by other questions, with at least eight questions between each neighboring pair of the target questions.

Eight versions of the questionnaire were used (which we’ll call A, B, C, D, E, F, G, and H), and respondents were randomly assigned to receive one of these versions. Respondents who received questionnaires A, B, C, or D were explicitly offered the option to say they had no opinion for the questions about government power, day-care, and the communist author and to say they

13. These are AAPOR’s response rate 1’s. The small demographic biases in this sample were similar to those observed in Studies 1 and 2. There were no notable demographic differences between the Ohio and national samples. Only one significant difference appeared between respondents who were offered the no-opinion option and those who were not: people who were offered this option had slightly but significantly more education than respondents who were not explicitly offered this option. This one significant difference would be expected based on chance alone, given random assignment to experimental condition and the number of such tests we conducted with various demographics across the three studies. To correct for this, we controlled for this difference in the analyses to follow. The Ohio and national samples mirrored the state and the nation, respectively, in terms of age, race, and most categories of education and income. Both samples overrepresented males, people with a college education, and people with annual family incomes of \$150,000 or more.

did not have enough information to answer the question about courts. Respondents who received questionnaires E, F, G, or H were not offered explicit no-opinion response options.

Question order manipulation: Respondents who received questionnaires A or H were asked the target questions in the order listed above. Respondents who received questionnaires B or G were asked the target questions in the reverse order. Respondents who received questionnaires C or F were asked the question about day-care, the communist author question, the courts question, and then the government-power question. Respondents who received questionnaires D or E were asked the government-power question, the courts question, the communist-author question, and then the day-care question. This question order manipulation was accomplished by rearranging chunks of questions. Target questions asked in the first half of the interview were coded as “early”; questions asked in the last half were coded as “late.”

Effort: Near the end of the interview, respondents were asked three questions gauging how much effort they put into answering the questions in the survey. One question asked how carefully respondents had thought about their answers to the questions in the survey (response choices: extremely carefully, very carefully, somewhat carefully, not too carefully, or not carefully at all); a second question asked how thoroughly respondents had searched their memories for relevant information before answering each question in the survey (response choices: extremely thoroughly, very thoroughly, somewhat thoroughly, not too thoroughly, or not thoroughly at all); the third question asked how much effort respondents spent making sure the answers they gave to the survey questions best reflected their opinions (response choices: a great deal, a lot, some, a little, or no effort at all). These three items were coded to range from 0 to 1, with higher numbers indicating greater care, thoroughness, and effort. The three values were then averaged to yield an index of effort.

Education: Education was measured and coded as it was in Studies 1 and 2.

RESULTS

Abstention rates. As expected, offering the no-opinion option increased the rates of abstention substantially: by 24.2 percentage points for the courts question ($\chi^2 = 45.03, p < .001$), 32.0 percentage points for the government-power question ($\chi^2 = 64.55, p < .001$), 13.5 percentage points for the day-care question ($\chi^2 = 23.33, p < .001$), and 22.6 percentage points for the communist author question ($\chi^2 = 57.20, p < .001$). As expected, the percent of the target items that a person answered by selecting or volunteering a no-opinion response increased from 6 to 29 percent (an increase of 23 percentage points) as a result of offering no-opinion response options ($b = .23, SE = .02, p < .01$; see col. 8 of table 1).

Education. This effect was moderated by education as expected. Among the least educated respondents, offering the no-opinion options increased no-

opinion responding from 11 to 46 percent (an increase of 35 percentage points). Among the most educated respondents, the inclusion of an explicit no-opinion response option increased no-opinion responding by only 16 percentage points. The interaction between offering or omitting a no-opinion option and education was significant ($b = .02$, $SE = .01$, $p < .05$; see col. 9 of table 1).

Question placement. To assess whether the location of a question in the survey influenced no-opinion responding, we calculated two measures of no-opinion responding for each respondent. One was the proportion of no-opinion responses to the first two target questions the respondent was asked, and the other was the proportion of no-opinion responses to the last two target questions the respondent was asked. The increase in the propensity to respond “no opinion” due to offering this option was related to survey location as expected. Questions asked late in the survey manifested a larger effect (26 percent) than questions asked early in the survey (20 percent). This difference was statistically significant ($F(1,488) = 3.74$, $p = .05$).¹⁴

The effect of education on attraction to the offered no-opinion option was large and significant for questions asked late in the survey ($b = .08$, $SE = .02$, $p < .01$; see col. 11 of table 1). But for questions asked early, presumably before fatigue had set in, the education groups did not differ significantly in terms of attraction to the offered no-opinion options ($b = -.01$, $SE = .02$, n.s.; see col. 10 of table 1). The three-way interaction between offering a no-opinion option, education, and question placement was highly significant ($F(3,478) = 3.91$, $p < .005$), indicating that offering the no-opinion option increased no-opinion response selection especially powerfully among low-education respondents late in the questionnaire. This constitutes the expected interaction between ability and motivation in regulating satisficing.

Effort. In order to gauge the role of effort, we began by examining whether effort was related to selecting an offered “no-opinion” option. The interaction between offering a “no-opinion” option and effort was highly significant ($b = -.34$, $SE = .11$, $p < .01$; see col. 12 of table 1), such that attraction to the no-opinion option was greater among people who exerted less effort. This is some of the most direct evidence consistent with the satisficing claim that no-opinion responding is the result of satisficing rather than optimizing.

When we simultaneously assessed the moderating effects of education ($b = .02$, $SE = .01$, $p < .05$, see row 5 of col. 13 in table 1) and effort ($b = -.33$, $SE = .11$, $p < .01$; see row 7 of col. 13 in table 1) on the impact of explicitly offering the “no-opinion” response option, both were significant. The three-way interaction between the inclusion of a “no-opinion” option, educational attainment, and effort was not significant ($b = .01$, $SE = .04$, n.s.; see col. 14 of table 1), so effort and education did not interact in influencing no-opinion responding.

14. The analyses including question placement as a variable were MANOVAs treating question placement as a within-subjects variable.

General Discussion

In writing this article, we sought to reexplore an alternative perspective on the meaning of no-opinion responses that has been acknowledged at times in the scholarly literature but has received little attention empirically to date. Below we review and integrate our findings and outline why they suggest that future research should continue to consider the possibility that no-opinion responses may not always identify people who completely lack opinions on an issue.

NO-OPINION RESPONSES AND DATA QUALITY

Inclusion of a no-opinion option did not reliably improve the quality of the data obtained: the over-time consistency of attitudes did not increase, nor did the statistical predictability of obtained responses. These findings therefore question the notion that no-opinion options discourage respondents from providing meaningless answers to survey questions. Instead, this evidence is consistent with the possibility that the respondents attracted by no-opinion options would have provided substantive answers of the same reliability and validity as were provided by people not attracted to those options.

SELECTION OF THE NO-OPINION OPTION

A number of our findings are consistent with hypotheses derived from the satisficing perspective. Attraction to no-opinion response options was most common among respondents low in education, when respondents voted secretly, when questions were asked late in a survey, and when respondents devoted little effort to answering questions. Furthermore, in Studies 2 and 3, a source of ability and a source of motivation appeared to combine multiplicatively, such that attraction to the no-opinion option was greatest when both ability and motivation were low. And in Study 3, reported effort was negatively related to no-opinion reporting, suggesting that choosing a no-opinion response option was more likely the result of satisficing than of optimizing.

A plausible interpretation of the negative association between education and attraction to the no-opinion option is optimizing. Less educated respondents may be the least likely to form opinions on a range of issues, so they may be accurate in saying “don’t know” most often when that option is offered.

But two considerations suggest that this may not be true. First, if this were true, we would expect to see more measurement error in attitude reports made when the no-opinion option is omitted, but this did not occur. And, more importantly, in Study 3, the relation of education to no-opinion option attraction was strong when respondents were fatigued toward the end of their interviews but was zero when respondents were fresh at the start of the

interviews. This suggests that when a negative relation between these two variables appears, it may be due to satisficing.

Reinforcing this conclusion is evidence from other studies that attraction to no-opinion options is greater when items are placed later in a questionnaire (Culpepper, Smith, and Krosnick 1992; Dickinson and Kirzner 1985; Ferber 1966; Ying 1989), when items are more difficult to process (Converse 1976; Klare 1950; Nuckols 1949), when the purpose and sponsor of a study are described in less motivating ways (Houston and Nevin 1977), and when respondents are not induced to work hard at the reporting task (Cannell, Oksenberg, and Converse 1979). These demonstrations suggest that no-opinion responses may be at least partly reflections of momentary task difficulty and respondent motivation (as the satisficing perspective anticipates) rather than simply reflecting a true lack of opinions.

Our findings also resonate with research exploring the impact of “don’t know” options on reports of factual matters. Poe et al. (1988) showed that offering a “don’t know” option significantly decreased the number of substantive answers respondents gave to factual questions. But the test-retest reliability of answers given to these questions was no different among respondents offered a “don’t know” option and among respondents not offered that option. Thus, the quality of substantive responses obtained from people who would have selected the “don’t know” option had it been offered was just as high as among the people who offered substantive responses to these questions when the “don’t know” option was offered. Thus, the processes we see underlying answers to opinion questions may underlie answers to factual questions as well.

Although the evidence reported here appears to be inconsistent with the non-attitude reduction hypothesis and consistent with the satisficing perspective, we could not directly identify respondents’ reasons for choosing the no-opinion option. Our analyses at the aggregate level suggest that many or even most respondents who choose an explicitly offered no-opinion response option may have meaningful attitudes, but we cannot rule out the possibility that some people do so because they truly do not have attitudes.

STUDIES OF OBSCURE AND FICTITIOUS ISSUES

One finding that might appear to challenge this conclusion is evidence from studies of obscure and fictitious issues (Bishop, Oldendick, and Tuchfarber 1986; Schuman and Presser 1981). In these studies, respondents were asked about their opinions on issues on which they almost certainly did not have preconsolidated opinions. Yet as many as 30 percent of respondents in regional and national samples offered opinions on these issues during survey interviews. This result seemed troubling to observers because these reports seemed almost certain not to represent real attitudes. Offering no-opinion response options seemed to be an effective antidote to such reporting tendencies because

offering such options reduced the rates of opinion reporting in response to such questions.¹⁵

However, even this matter is not straightforward. Many of the people who reported opinions on these obscure and fictitious issues appeared to be offering what might be considered real opinions, in the sense that they were meaningful reflections of respondents' predispositions. By asking respondents a question, a researcher communicates the expectation to respondents that they are capable of interpreting the question and drawing on information they have in memory to answer it in some reasonable way, so it seems sensible that respondents would do their best to comply with that unstated expectation (e.g., Schwarz 1996). Consistent with this logic, Schuman and Presser (1981) found that attitudes toward the Monetary Control Bill were correlated with concern about inflation, some respondents made remarks suggesting that they inferred the bill's meaning along these lines (e.g., presuming that it was an anti-inflation measure), and attitudes toward this bill were fairly stable over time. Regardless of whether respondents were correct in their inferences about these issues, their answers to the survey questions may have represented genuine reactions to their interpretation of the question's meaning.

In this light, it is not obvious that offering no-opinion response options and reducing the rate of opinion offering in response to these questions is desirable. It could be that in those studies, offering a no-opinion response option reduced rates of reporting real attitudes based on the issue labels presented to respondents. And if respondents had found themselves in a voting booth, asked to cast a vote on the Monetary Control Bill, their inferences about its purpose may well have shaped their behavior. If so, encouraging respondents to abstain from reporting their opinions would have forgone the opportunity to measure these attitudes.

PRACTICAL RECOMMENDATIONS

Thus our findings suggest a possible practical recommendation to survey designers that seems worth further testing. As appealing as offering a no-opinion response may be, doing so may lead researchers to collect less valid and informative data than could be done by omitting it. Because our estimates of response quality did not change notably depending on whether a no-opinion option is offered or omitted, it might seem that the decision about whether to offer one is inconsequential. But, in fact, offering this option will most likely reduce a researcher's effective sample size (by encouraging some respondents to say "don't know"), yielding reduced statistical power at the very least. Furthermore, a no-opinion option apparently systematically encourages

15. No-opinion responses were more common among more educated respondents in these studies, which is consistent with the claim that low-education respondents were most likely to manufacture meaningless opinions on these issues.

low-education respondents to avoid the effort of deciding how to answer the question, thereby reducing the impact they have on survey results. If researchers want to collect as many valid opinions as exist, it appears that doing so may best be done by omitting no-opinion options and measuring attitude strength directly instead.

For researchers who prefer to offer no-opinion response options to respondents or at least to accept those responses when volunteered, it may still be possible to maximize substantive opinion reporting. Respondents who say they have no opinion can then be asked if they lean toward one of the substantive response options. This may encourage some respondents who initially say “no opinion” because of satisficing to report their opinions.

No research to date has compared data quality across questions with the no-opinion response option omitted, questions with the no-opinion response option offered, and questions that probe no-opinion responses in this fashion. But one study is indirectly related to this matter and offers further reinforcement for the conclusion we reach here. Visser et al. (2000) explored the accuracy of preelection polls by comparing their measurements with subsequent actual election outcomes. Respondents in these surveys were asked for whom they would vote in each of a variety of races, and respondents who volunteered “don’t know” answers were then asked whether they leaned toward a candidate, and if so, which. Treating responses to these leaning follow-ups as valid candidate preferences increased the accuracy of the poll forecasts of the election outcomes, which suggests there was some validity to responses that were initially “hidden” by “don’t know” answers. Therefore, there may be value in following up “don’t know” responses to all sorts of questions by asking about leaning. We look forward to further research examining the use of no-opinion options and follow-up questions.

SECRET VERSUS ORAL VOTING

Regardless of whether accountability was at work or not in Study 2, that study has an interesting implication regarding the use of secret ballots and self-administration more generally. In order to permit respondents to comfortably report beliefs, opinions, preferences, or behaviors that they might prefer to keep confidential, many researchers have expressed keen interest in the use of secret ballots. For example, Perry (1979) strongly advocated their use, though he acknowledged that the aggregate results he obtained from secret ballot measurements were very rarely different from those obtained from conventional oral reporting. Later studies have shown larger effects, with self-administration often increasing reporting of socially undesirable behaviors and decreasing reporting of socially desirable behaviors (Nederhof 1984; Presser and Stinson 1998; Wiseman 1972; see also Nederhof 1985; Tourangeau, Rips, and Rasinski 2000). But these studies have always focused on aggregate comparisons of reporting rates in groups of individuals. We know of no

evidence showing that self-administration yields more accurate responses at the level of the individual respondent, as gauged by comparisons with benchmarks of accuracy.

Our results suggest that there may be a disadvantage to this technique that Perry failed to notice: reduced individual-level data quality due to enhanced satisficing. We found that the relation of the proposed cost of the oil spill prevention program to voting at the level of the individual was only 60 percent as strong among respondents who voted secretly as compared to respondents who voted orally. This is consistent with the claim that self-administration does not reduce measurement error and may even increase it.

If this sort of compromise in data quality has occurred in past studies of socially desirable or undesirable behaviors, observed changes in rates of reporting such behaviors might not have constituted increases in the accuracy of people's answers. More random measurement error would in and of itself increase reporting of rarely reported undesirable behaviors (e.g., committing crimes) and would decrease reporting of frequently reported socially desirable behaviors (e.g., voting) simply because adding random responses will make any distribution flatter.¹⁶ While causing an increase in the appearance of accuracy at the aggregate level (e.g., because socially undesirable behaviors or opinions would be reported more often), adding random responding would not of course yield increased accuracy at the individual level. Therefore, it may be worthwhile to reconsider whether in fact secret voting and self-administration have truly improved individual-level reporting accuracy in all instances where that has been presumed.

CONCLUSION

It is certainly tempting to believe that whenever a respondent claims to have no opinion on an issue, this is genuinely true for him or her. But the work reported here suggests that this conclusion may not be warranted, reinforcing the views of Bradburn and Sudman (1988), Feick (1989), Fowler and Cannell (1996), and others. Many respondents who claimed to have no opinion (when encouraged to do so) appear to have been capable of generating substantive responses with the same reliability and validity of the responses provided by people who readily offer substantive opinions even when a no-opinion answer was legitimized. Thus, no-opinion answers may be due more to satisficing rather than optimizing and might therefore be best discouraged rather than encouraged. We look forward to much-needed future studies continuing to explore these issues and shedding light on the meaning of no-opinion responses and their desirability in surveys.

16. Random error would, of course, decrease the appearance of aggregate accuracy for behaviors that are desirable but reported by a minority of respondents (e.g., regular church attendance; Presser and Stinson 1998) or behaviors that are undesirable but reported by a majority of respondents.

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