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The Public Opinion Quarterly, Vol. 54, No. 2 (Summer, 1990), 209-218.

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ISSUE INVOLVEMENT AND RESPONSE EFFECTS IN PUBLIC OPINION SURVEYS

GEORGE F. BISHOP

Abstract This research tests the widespread assumption that response effects due to variations in question form, wording, or context will be greatest among respondents who are least involved with an issue. A meta-analysis of results from 15 split-ballot experiments conducted over a five-year period indicates that the response effects of using counterarguments or middle alternatives in survey questions are significantly larger, as would be expected, among respondents who are less involved with a given issue than among those who are highly involved with it. But the effects of question order and response order appear to be largely unrelated to how involved a respondent is with a particular issue. Issue involvement, then, appears to specify some response effects, but not others.

Public opinion researchers have long assumed that the respondents who are most susceptible to being influenced by the way in which a question is worded, the form in which it is presented, or the order or context in which it is asked are those who are least involved with an issue and whose views on the subject are therefore not well crystallized or held with much conviction (see, e.g., Cantril, 1944; Converse, 1974; Payne, 1951). But a recent investigation by Krosnick and Schuman (1988), based on nearly thirty experiments conducted over a ten-year period in various national surveys, indicates that this widespread assumption about who it is that is most susceptible to response effects may very well be wrong (cf. Stember and Hyman, 1949–50; Sudman and Bradburn, 1974; Sudman and Swensen, 1985, cited in Krosnick and Schuman, for previous evidence on the hypothesis). With the notable exception of the effects of offering or omitting a middle response

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alternative in survey questions, they found no significant relationship between such indicators of “crystallization” as attitude importance, intensity, and certainty and an individual’s susceptibility to different types of response effects. After considering alternative explanations for these counterintuitive findings, such as the possible unreliability, invalidity, or crudity of their attitude crystallization measures—all of which they find insufficient as rival hypotheses—Krosnick and Schuman have concluded that differences in attitude crystallization do not generally explain why some individuals are susceptible to response effects while others are not, and that each type of response effect is probably determined by a unique psychological moderating variable.

An independent investigation by the author has produced a similar set of results from split-ballot experiments with different topics, conducted in telephone surveys over a five-year period in a major metropolitan area of the United States. Though the results presented here extend and replicate most of those reported by Krosnick and Schuman, there is evidence that issue involvement may specify some types of response effects, but not others.

Research Design

All of the experiments were designed as part of an ongoing series of random-digit-dialed, multipurpose telephone surveys of the adult population in Greater Cincinnati.¹ Some used simple split-ballot designs, in which respondents were randomly assigned to one of two forms of the questionnaire; others were part of larger factorial designs.² The response effects of question form, wording, or context in the majority of these experiments have been reported previously (Bishop, 1987a; Bishop, Oldendick, and Tuchfarber, 1985), but the degree to which such effects depend upon a respondent’s involvement with an issue has not heretofore been analyzed. Altogether there were 15 separate experiments or replications conducted over a five-year period, involving four types of response effects: question wording, question form, question order, and response order. The final experiment on the effect of offering or omitting a middle response alternative also included a variation in the order in which the middle alternative was presented: as either the *last* or *middle* choice among three alternatives. Counting all the variations, there were a total of 18 tests of the issue involvement hypothesis.

1. The response rates for the Greater Cincinnati surveys that included the experiments with counterarguments, middle response alternatives, and the order of the questions on the abortion issue have been documented previously in Bishop, Oldendick, and Tuchfarber, 1983; Bishop, 1987a; and Bishop, Oldendick, and Tuchfarber, 1985, respectively.

2. A detailed description of these factorial designs is available from the author.

In most of these experiments issue involvement was measured by asking respondents, immediately after they gave an opinion on an issue, how important a candidate's position on the issue was to them in deciding how to vote in an election—a question adapted from previous experiments by Schuman and Presser (1981) and used by Krosnick and Schuman (1988) as one of several indicators of attitude crystallization.³ In our experiments with counterarguments, however, a respondent's involvement with an issue was measured somewhat differently. On the issue of wage and price controls, it was assessed with a follow-up question about how much the respondents had thought about the issue, whereas on the issue of a balanced-budget amendment, it was determined by asking them how interested they were in the issue; neither measure was used by Krosnick and Schuman (1988) in their comparable experiments on "question balance" with other topics. So there are some differences as well as similarities in the way in which the hypothesis was operationalized and tested in these two independent investigations.

Findings

Table 1 summarizes the results from the various experiments and replications conducted over a five-year period.⁴ Generally speaking, the data tend to support the issue involvement hypothesis, though the magnitude of the interaction was typically not significant. Three of the four experiments with counterarguments, all nine of the middle-alternative experiments, both of the question-order experiments, and one of the three response-order manipulations—15 of the 18 variations, altogether—generated a response effect in the expected direction: greater among the less involved than among the highly involved. In only 3 of these 18 tests of the hypothesis, however, was the three-way interaction statistically significant, none of which replicated. So while

3. The term "issue involvement," as I use it here, refers to matters of social significance which are in dispute and which therefore engage the interests, thoughts, and emotions of different segments of society. In its most extreme form it refers to issues in which people become engaged and committed as in a sociopolitical cause, such as the right-to-life (or pro-choice) movement on the issue of abortion. It bears some similarity to the concept of issue involvement developed by Campbell et al. (1960: chap. 8). And it differs from Schuman and Presser's (1981) construct of "attitude strength," in that the latter is regarded as just one component of issue involvement (cf. Campbell et al., 1960:176–179), albeit a very important one. It is thus a broader theoretical construct than attitude strength, with some currency across disciplines. Issue involvement as a variable would also appear to be casually prior in that attitudes become "crystallized" or strengthened as a result of an issue becoming important to an individual for one reason or another.

4. A full set of tables on all of the experiments identified in Table 1 below are available from the author.

Table 1. Summary of Results

Experiment	Involvement Measure	Overall Response Effect	Significance of Overall Response Effect	Effect by Level of Involvement		Interaction Effect	Significance of Interaction Effect	N
				Low	High			
<i>Counterargument</i>								
Wage & price controls								
Nov.-Dec., 1978	Thought ^a	17.4%	$p< .0001$	22.8%	10.5%	12.3%	n.s.	533
May-June, 1979	Thought	16.5	$p< .001$	18.0	15.8	2.2	n.s.	499
Balanced budget								
Nov.-Dec., 1978	Interest ^b	5.9	n.s.	4.6	7.3	-2.7	n.s.	501
May-June, 1979	Interest	13.6	$p< .01$	23.6	-3.6	27.2	$p< .01$	463
<i>Middle alternative</i>								
Social Security								
May-June, 1979	Importance ^c	20.8	$p< .0001$	25.8	8.7	17.1	n.s.	501
May-June, 1980	Importance	24.4	$p< .0001$	27.3	21.6	5.7	n.s.	501
Nov.-Dec., 1983	Importance	18.6	$p< .0001$	20.1	16.6	3.5	n.s.	1166
Defense spending								
May-June, 1979	Importance	39.4	$p< .0001$	39.9	34.3	5.6	n.s.	499
May-June, 1980	Importance	23.1	$p< .0001$	37.2	14.8	22.4	n.s.	525
Nov.-Dec., 1983	Importance	22.6	$p< .0001$	30.3	15.8	14.5	$p< .05$	1176

Nuclear power								
May-June, 1980	Importance	26.2	$p<.0001$	27.5	25.4	2.1	n.s.	1004
May-June, 1982	Importance	33.7	$p<.0001$	36.5	28.3	8.2	n.s.	1023
Nov.-Dec., 1983	Importance	33.9	$p<.0001$	41.5	24.2	17.3	$p<.05$	1161
<i>Question order</i>								
Abortion								
Nov.-Dec., 1981	Importance	5.2	n.s.	6.3	0.8	5.5	n.s.	1107
May-June, 1982	Importance	14.4	$p<.0001$	15.3	10.1	5.2	n.s.	1066
<i>Response order</i>								
Social Security								
Nov.-Dec., 1983	Importance	3.3	n.s.	0.7	6.6	-5.9	n.s.	780
Defense spending								
Nov.-Dec., 1983	Importance	9.5	$p<.01$	9.8	5.9	3.9	n.s.	790
Nuclear power								
Nov.-Dec., 1983	Importance	9.6	$p<.01$	8.6	10.1	-1.5	n.s.	783

^aThe follow-up question on involvement read: "How much would you say you have thought about this issue of wage and price controls—a great deal, some, a little bit, or hardly at all?"

^bThe follow-up read: "How interested would you say you are in this issue of balancing the federal budget—very interested, somewhat interested, slightly interested, or not at all interested?"

^cThe follow-up read: "How important is a candidate's position on social security benefits when you decide how to vote in an election—is it one of the important factors you would consider, a very important factor, somewhat important, or not too important?" The same type of importance question was used to measure involvement on the issues of defense spending, nuclear power, and abortion.

Table 2. Meta-Analysis of Response Effects

	Number of Experiments	Average Effect of Involvement	Z Score Testing Avg. Effect	<i>P</i> value
All experiments ^a	15	9.7%	4.87	.00
Type of effect				
Counterargument	4	9.8	2.15	.02
Middle alternative	9	10.7	4.35	.00
Question order	2	5.4	1.17	.12
Response order ^b	3	-1.2	.28	n.s.

^a Does not include the three response-order experiments which were part of the three middle-alternative experiments conducted in November/December 1983.

^b Effects for response order were calculated and tested as three separate experiments.

there was some clear evidence of an involvement effect in these experiments as a whole, the interaction effect in any single experiment was typically too weak to be detected reliably.⁵

Meta-Analysis of Experiments

The meta-analysis in Table 2, combining the results of the multiple experiments, tends to support this proposition. Following Krosnick and Schuman (1988), the analysis uses the difference between proportions, d' , as the measure of the effect size and treats each investigation as generating two experiments: one for the high-involvement group and one for the low-involvement group (see Rosenthal, 1984: chap. 4). As in Krosnick and Schuman's study, a test of heterogeneity showed a significant variation in the size of the response effect across the 15 separate experiments ($X^2 = 150.59$, $p < .0001$). Unlike Krosnick and Schuman, however, a comparison across all experiments of the effect sizes in the high-involvement group with those in the low-involvement group indicated that the overall effect of involvement was highly significant ($z = 4.87$, $p < .0001$). But this discrepancy is due, in large

5. In addition to the analysis presented here, I examined whether there was a curvilinear relation between the follow-up measures of involvement and the response effects in the 15 experiments we conducted, but I could find no reliable evidence of such a relation. We also checked whether the distribution of responses to the follow-up measures differed significantly across question forms in each of the experiments. None was found to be significantly different.

part, to the greater number of middle-alternative experiments in the present meta-analysis (9 of 15 experiments)—all of which generated an effect in the expected direction—whereas the middle-alternative experiments in Krosnick and Schuman's meta-analysis, which generated the only reliable evidence of an interaction effect due to "crystallization," were much smaller in number (3 of 27 experiments).

Furthermore, the influence of involvement appears to depend upon the type of response effect. Like Krosnick and Schuman, we found that the effect sizes for the question-order and response-order experiments were not significantly larger in the low-involvement groups than in the high-involvement groups. But it is also important to note here that the overall response effects for the question-order and response-order experiments were relatively small, particularly for the latter (see Table 1). So there is less possibility of significant variation by level of involvement. We also found, as they did, some of the strongest evidence for the hypothesis in the middle-alternative experiments, in which the effect sizes were consistently and significantly larger among those who were less involved with the issues than among those who were highly involved with them (average effect size = 10.7%). Our results for question order, response order, and middle alternatives thus extend and replicate Krosnick and Schuman's findings, but with different issues, and in a different time and setting.

In contrast, however, we discovered that the effect sizes in the counterargument experiments were significantly higher for the less-involved respondents than for those who were highly involved (average effect size = 9.8%).⁶ Krosnick and Schuman's meta-analysis of experiments on counterarguments (question balance) found inconsistent and insignificant differences in effect sizes between high- and low-involvement groups. This discrepancy in the results of the two investigations may well be due to a difference in the issues selected for experimentation, as well as a difference in the follow-up questions used to measure involvement or crystallization. Krosnick and Schuman used the issues of gun control and the question of whether workers should be required to be union members, both of which would probably be familiar topics for most respondents, whereas the present inves-

6. As one referee pointed out, the three-way interaction reached significance in only one of the four counterargument experiments, and the size of the three-way interaction was negligible in two of them. A similar pattern, however, occurred in the middle-alternative experiments: the three-way interaction reached significance in only two of the nine middle-alternative experiments (less than one out of four), and the size of the three-way interaction was very small or negligible in several of them (cf. Krosnick and Schuman, 1988:947). Furthermore, the average effect of involvement in the middle-alternative experiments (10.7%) was about the same size as that observed in the counterargument experiments (9.8%). Both effects thus behave much the same way, fluctuating in magnitude from one experiment to the next, but the overall effect detected by the meta-analysis is clearly significant in each case.

tigation used the subjects of a constitutional amendment to balance the federal budget and the use of wage and price controls by the federal government, both of which would probably not be that familiar to many respondents. A respondent's familiarity with, or knowledge of, an issue may thus determine his or her susceptibility to the influence of a counterargument on an issue, a variable that is tapped only partly, if at all, by the different measures of involvement or crystallization used in both Krosnick and Schuman's and the author's investigation of counterargument effects. But the measures of involvement used in the present study—*thought* and *interest*—may capture the familiarity dimension somewhat better than the intensity, importance, and certainty indicators used by Krosnick and Schuman and thus explain part of the discrepancy. Further experiments with more direct measures of issue familiarity might therefore yield even stronger evidence for the hypothesis.

To summarize, the meta-analysis indicates that the response effects of using counterarguments or middle alternatives in survey questions were significantly larger, as would be expected, among respondents who were less involved with the issues than among those who were highly involved with them. But the effects of question order and response order were largely unrelated to how involved a respondent was with a given issue. Issue involvement, then, appears to specify some response effects but not others; in the case of counterargument effects, in one investigation but not the other.

Conclusion

The results clearly suggest that some types of response effects in public opinion surveys may well be a function of issue involvement. The strongest evidence for the hypothesis, in this investigation as well as in Krosnick and Schuman's study, comes from experiments with the middle response alternative. In addition, using different issues and different measures of involvement or crystallization, the meta-analysis presented here supports the hypothesis that counterargument effects are significantly greater for those who are less involved with an issue, suggesting that different response effects may be specified by different measures of involvement. Furthermore, the effects of involvement in these (and in Krosnick and Schuman's) experiments may be masked, in part, by a possible context effect. Because of the usual sequence of the questions (ask about opinion, then involvement), respondents may infer that they are interested in an issue, or that they have thought about it, or that it is important to them because they have just ex-

pressed an opinion on it.⁷ This hypothesis seems plausible given what we know from previous research on context effects on self-reports of interest in political affairs (Bishop, 1987b; Bishop, Oldendick, and Tuchfarber, 1984). Only by conducting a controlled split-ballot experiment, in which respondents are asked how involved they are with various issues either *before* or *after* they have given an opinion on them, can we put this hypothesis to rest. Even in the absence of such follow-up experiments, the evidence presented here leads us to conclude that, with admittedly imperfect measures, issue involvement does appear to specify at least some response effects. With improved measures of issue familiarity and involvement and experimental control of a plausible context effect, the influence of issue involvement might well be greater.

On the other hand, given the widespread assumption among survey researchers that involvement effects are to be expected if not already “self-evident,” perhaps the really surprising finding in these investigations is the relatively weak and inconsistent effects of involvement, and not the fact that some do occur. No matter how involvement is measured, then, such effects might be modest at best.

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7. As another referee noted, however, if there were context effects on the involvement measures, we would expect to see that experimental manipulations that altered responses to the opinion questions would alter responses to the involvement measures as well. But neither Krosnick and Schuman nor I (see footnote 5) have found evidence of such context effects. But as Krosnick and Schuman acknowledge, “a variation in an attitude question could systematically alter subsequent reports of attitude intensity in opposite directions for different individuals, leaving the aggregate distribution of responses to the follow-up measure identical in both samples. Nonetheless, showing that the overall distributions are equivalent across attitude-question variations makes this problem seem relatively less likely” (Krosnick and Schuman, 1988:948). However, I maintain that only a controlled set of experiments, as proposed here, will put this context hypothesis to the required test.

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