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Attitude Intensity, Importance, and Certainty and Susceptibility to Response Effects

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Changes in attitude question form, wording, and context have repeatedly been shown to produce change in responses. It is often assumed that such response effects are less pronounced among individuals whose attitudes are intense, personally important, or held with great certainty. We report the results of 27 experiments conducted in national surveys designed to evaluate this hypothesis. Measures of attitude intensity, importance, and certainty were found not to differentiate individuals who show response effects from those who do not. We discuss possible explanations for these counterintuitive findings.

It is now well documented that people's reports of their own attitudes are influenced by a host of factors in addition to the attitudes themselves. Variations in the wording of an attitude question, the formal properties of its structure, or the context in which it is asked all alter responses in systematic ways (Schuman & Kalton, 1985). Such variations in attitude reports are referred to in the survey methodology literature as *response effects.*

It is frequently assumed that such response effects are greatest when attitudes are uncrystallized, weakly held, or not central to a respondent. For example, Converse (1974) stated that "it is probable that extraneous factors like question form intrude most sharply on responses where attitudes are least crystallized" (p. 656). To the contrary, according to Cantril (1944), when "the respondent's mental context is solidly structured . . . the same answer is likely to be obtained irrespective of the way questions are asked" (pp. 48–49). Similarly, Payne (1951) argued that

> Where people have strong convictions, the wording of the question should not greatly change the stand they take. The question can be loaded heavily on one side or heavily on the other side, but if people feel strongly their replies should come out about the same. It is on issues where opinion is not crystallized that answers can be swayed from one side to the other by changes in the statement of the issue. (p. 179)

This assumption is echoed in the writings of many experienced survey investigators (e.g., Blankenship, 1940, p. 401; Cantril, 1944, p. 34, 35, 45; Converse, 1970, p. 177; Erikson & Luttbeg, 1973, pp. 35–39; Gallup, 1941, p. 261; Katz, 1940, p. 279; Payne, 1951, p. 135; and Rosenberg, Verba, & Converse, 1970, pp. 24–25) and therefore seems to be widely taken for granted.

Surprisingly, a clear theoretical basis for this assumption has not been developed, and the assumption itself has rarely been tested. This is probably in part because the assumption is so widely accepted. It is probably also in part because attitude crystallization is more a metaphor than a concept that can readily be operationalized. However, closely related concepts such as attitude intensity, importance, and certainty are frequently measured by means of direct questioning and are often taken to be indicators of attitude crystallization. And research on attitudes and response effects suggests a number of reasons why reports of attitudes that are intense, important, or held with great certainty might be less susceptible to response effects.

One possible reason focuses on the relation between indicators of attitude crystallization and attitude extremity. Some response effects have been viewed as resulting from slight changes in perceptions of the meanings of attitude question response options. These changes presumably lead respondents to map their attitudes differently onto the response options. For example, Hippler and Schwarz (1986) argued that fewer people say *yes* when asked if they feel speeches against democracy should be *forbidden* than say *no* when asked if they feel such speeches should be *allowed,* because *not allowing* is perceived as a less extreme stance than is *forbidding.* Such changes in perceptions of the extremity of response options are unlikely to alter reports of extreme attitudes. People who evaluate speeches against democracy extremely negatively will probably not hesitate to say that they feel they should be *forbidden* and *not allowed.* Individuals with moderately negative attitudes may be more likely to say no when asked whether they feel that such speeches should be forbidden and when asked whether they feel such speeches should be allowed. Thus, when response effects occur because of changes in the perceived extremity of response alternatives, people with moderate attitudes should evidence larger effects than people with extreme attitudes. Because intense and important attitudes and those held with great certainty are more extreme than are weak and unimportant ones and those held without great certainty (Brim, 1955; Cantril, 1946; Krosnick, 1986;

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Suchman, 1950), the former would be expected to evidence larger response effects than would the latter.

A second possible explanation for an association between the magnitude of response effects and indicators of attitude crystallization focuses on the clarity and accessibility of attitudinal cues in memory. Some attitudes are characterized by clear, univocal, and highly accessible cues in memory (e.g., Fazio, 1986), whereas others are associated with weak, ambiguous, and inaccessible cues (e.g., Bem, 1972; Converse, 1970) or with heterogeneous, conflicting cues that must be integrated into a summary evaluation in a piecemeal fashion (e.g., Fiske & Pavelchak, 1986). If a respondent is faced with ambiguous, conflicting, or inaccessible internal cues, an attitude-question wording or context that emphasizes one point of view more than others might influence the respondent's perception of what his or her own attitude is (Bishop, Oldendick, & Tuchfarber, 1984). In contrast, if one's internal cues are clear and accessible, question wording, form, and context are unlikely to distort self-perceptions (e.g., Chaiken & Baldwin, 1981). Because attitudes that are intense, important, or held with great certainty are associated with clearer and more univocal and accessible attitudinal cues (Krosnick, 1986), reports of these attitudes would be expected to be less affected by changes in question form, wording, or context.

A third possible explanation for the hypothesis focuses on response effects that seem to occur when aspects of question wording change respondents' attitudes. For example, some attitude questions ask individuals whether they favor a particular government policy and include an argument in support of it (see, e.g., Schuman & Presser, 1981, pp. 179–202). Presenting an argument in this fashion might act as a persuasive communication and might therefore sway some respondents' opinions. Attitudes involving intense feelings and those that individuals consider personally important are especially resistant to the influence of persuasive communications (intensity: Ewing, 1942; Halverson & Pallak, 1978; Knower, 1936; and Tannenbaum, 1936; importance: Ewing, 1942; Fine, 1957; Knower, 1936; Rhine & Severance, 1970; and Yankelovich, Skelly, & White, 1981), presumably for a number of reasons. First, these attitudes are bolstered by much supporting knowledge in memory (Wood, 1982), which equips individuals to counterargue against attitude-challenging information. Second, these attitudes are thought to be extensively linked to other attitudes, beliefs, values, and psychological elements in memory (Judd & Krosnick, in press; Newcomb, Turner, & Converse, 1965), and these elements exert stabilizing forces (Ostrom & Brock, 1969). Finally, these attitudes are reinforced by social support among friends and family (Byrne, 1971; Tedin, 1980) and are frequently expressed through public commitments (Krosnick, 1986; Schuman & Presser, 1981, p. 242). Social support and public commitment also increase resistance to change (e.g., Hovland, Campbell, & Brock, 1957). By analogy, then, variations in survey-question form, wording, or context that exert forces encouraging attitude change might be expected to have least effect on reports of attitudes that are intense, personally important, or held with great certainty.

Thus, there are three possible reasons why intense and important attitudes and those held with great confidence might be less susceptible to response effects. First, these attitudes are more extreme. Second, these attitudes are associated with clearer and more accessible internal attitudinal cues. And third, these attitudes are more resistant to change. All of these explanations are consistent with the commonsense notion that attitudes that represent "firm convictions" are less easily swayed by mere rhetorical variations in questions.

We have been able to locate only three previous studies that evaluated the hypothesis that response effects are greater for attitudes that are intense, important, or held with great certainty. Sudman and Bradburn (1974) found slightly larger response effects for questions on topics that respondents generally did not know or care about (assessed by the researchers' judgments) than on topics dealing with respondents' important behavior or with current issues. Sudman and Swensen (1985) reported that weak attitudes and those that respondents rarely discussed evidenced no stronger response-order effects than did intense and frequently discussed attitudes. However, they found that attitudes on issues about which respondents said they had never heard or read evidenced slightly greater response-order effects than did attitudes about which respondents said they had made up their minds before the interview. Finally, Stember and Hyman (1949/1950) found no difference in the effect of a question-form variation according to interest in the topic. Thus, previous evidence evaluating the assumption offers inconsistent support for it. In this article, we report the results of a 10-year investigation designed to test the hypothesis systematically and comprehensively.

Method

The experiments that we will describe were included in telephone surveys of cross-section samples of American adults conducted by the University of Michigan's Survey Research Center between September 1974 and April 1984. All of the experiments involved essentially the same design. One half of the respondents, chosen randomly, were asked one version of an attitude question, and the other half were asked a second version of the question. All of the respondents were then asked a follow-up question intended to measure the intensity, importance, or certainty of the attitude being measured. Some of the experiments have been reported previously (Schuman & Presser, 1981) but were not fully integrated for the present purpose; others are reported here for the first time. All of the studies that we have conducted on this issue are reported here. It is useful to consider all of the available data both in aggregate and separately by the type of question variation, in order to draw generalized conclusions.

The question variations fell into three general categories. Some involved changes in the *order* in which questions were asked or in the order in which response alternatives to closed-ended questions were read to respondents. Others involved changes in the actual *wording* of questions. Still a third type involved changes in the general *form* of questions. Examples and results for each of these types of variations are described later. All but 4 of the 27 experiments we report yielded a statistically significant main effect due to question form, wording, or context. Of the 4 exceptions, 3 showed marginally significant trends in the predicted direction, and all 4 were conducted in order to replicate previously significant effects. Our interest is in whether these response effects are greater for low-intensity, low-importance, and low-certainty attitudes as compared with those characterized by high intensity, high importance, and high certainty.

In some experiments, attitude importance was measured by asking respondents how important the issue was to them when they decided how to vote in congressional elections. In other experiments, attitude intensity was measured by asking respondents how strong their feelings were on the issue. In still other experiments, attitude certainty was mea-

Table 1
Oil Supply Response Order

Survey: November and December 1982

Question 1: Used up first

Now we have some questions on other topics. Some people say that at the rate we are using our oil, it will all be used up in about 15 years. Others say we will still have plenty of oil 25 years from now. Which of these ideas would you guess is most nearly right?

Question 2: Plenty first

Now we have some questions on other topics. Some people say that we will still have plenty of oil 25 years from now. Others say that at the rate we are using our oil, it will all be used up in about 15 years. Which of these ideas would you guess is most nearly right?

	How certain do you feel that our oil will all be used up/we will still have plenty of oil? Do you feel very certain, or not very certain?			
	Very certain		Not very certain	
Response	Question 1	Question 2	Question 1	Question 2
Used up (%)	7.8	16.7	28.7	47.0
Plenty (%)	92.2	83.3	71.3	53.0
Total %	100.0	100.0	100.0	100.0
n	244	233	122	132
χ^2	$\chi^2(1, N = 477) = 9.08$, $p < .01$, $\Delta = 8.9\%$		$\chi^2(1, N = 254) = 9.07$, $p < .01$, $\Delta = 18.3\%$	
Three-way interaction	$\chi^2(1, N = 731) = 0.04$, *ns*			

sured by asking respondents how certain they were of their attitude or how easily they thought it could be changed. Although variations in type of question and measures of intensity, importance, and certainty were not completely cross-cut, there was sufficient intermixing to identify interactions that might occur. Furthermore, some experiments included two different measures of crystallization and therefore afforded two opportunities to test the hypothesis.

Results

Response Order and Question Order

Response order. Changes in the order in which response alternatives to a closed-ended question are read to respondents can affect answers to it (for a review, see Schuman & Presser, 1981; see also Krosnick & Alwin, 1987). In most such cases, recency effects occur, whereby alternatives presented late in a list are more likely to be chosen than are those read earlier; in a few cases, primacy effects have been found to occur. Regardless of direction, it seems plausible that people with intense, important, or highly certain attitudes might be less affected than others by manipulations of response order.

We tested this hypothesis using measures of beliefs about petroleum supplies and attitudes toward divorce laws. The oil supply question, adapted from Payne (1951), asked respondents whether they thought the supply of oil in the United States would be used up in 15 years. The order in which the two alternatives were read was varied across the two forms of the questionnaire, the order having a highly significant effect on responses (see Table 1).[1] Consistent with our expectations, the effect was greater in percentage terms for individuals who said they were uncertain about their beliefs on this issue in November and December 1982, when we initially conducted this experiment. This pattern of results was also obtained in May and June 1983, when we repeated the experiment. The effect sizes for the high-certainty groups in the two experiments were 8.9% and 6.0%, respectively, as against 18.3% and 12.8%, respectively, for the low-certainty groups. However, the three-way interaction (Response × Form × Certainty) did not approach statistical significance in either case: November–December 1982, $\chi^2(1, N = 731) = 0.04$; or May–June 1983, $\chi^2(1, N = 532) = 0.04$. Thus, the null hypothesis with regard to attitude certainty cannot be rejected in either of these cases.[2]

This conclusion might seem surprising because the effect sizes in percentage terms are clearly larger for the low-certainty groups. However, the chi-squares are not nonsignificant because of a lack of statistical power. Rather, the logarithmic scale used to compute the chi-square statistics weights a given between-group difference more heavily as the groups' proportions approach 0% or 100%. This is done in order to adjust for possible ceiling and floor effects. In the present case, beliefs were more skewed among the high-certainty respondents than among the low-certainty respondents. The former respondents' smaller response effects were therefore weighted more heavily than the latter respondents' larger effects in the calculation of the chi-squares. Because ceiling and floor effects can suppress the magnitude of response effects, this weighting strategy seems appropriate and therefore justifies the conclusion that attitude certainty did not determine the magnitude of this response-order effect.

In the experiments involving attitudes toward divorce laws, respondents were asked whether divorce ought to be made easier or more difficult to obtain. The question offered three response alternatives. For some respondents, the middle alternative (*stay as it is now*) was read after the other two, whereas for others, that option was read after the first and before the second. In an experiment conducted in November and December 1982, the effect of this response-order manipulation was greater among respondents who said that the issue was important to them when deciding how to vote (11.6% vs. 4.4%), but the three-way interaction was not statistically significant, $\chi^2(1, N = 720) = 1.63$. A similar pattern of results was obtained in that experiment when a measure of attitude intensity was used (10.9% vs. 6.6%), $\chi^2(1, N = 720) = 1.37$, but the direction of the difference reversed in a replication in May and June 1983—importance (5.0% vs. 7.4%), $\chi^2(1, N = 525) = 0.01$; and intensity (1.9% vs. 6.5%), $\chi^2(1, N = 525) = 0.04$. Thus, the item showed a reliable response effect, but attitude intensity and importance seemed not to specify the magnitude of this effect.

Question order. A second type of order variation that we examined involved changes in the context in which a question was asked. Context has generally been viewed as determined by the

[1] We present only one table for each type of form, wording, and context effect that we examined. A complete set of tables reporting the results of all of the experiments is available from either author.

[2] We tested the statistical significance of the experimental manipulations using likelihood-ratio chi-square statistics. Results using *t* tests of differences in proportions would be the same as those reported here.

content of the questions and answers that precede a target question during an interview. For example, consider a question asking whether it ought to be legal for a woman to obtain an abortion if she is married and doesn't want any more children. Fewer people say yes to this question if they have just been asked whether it ought to be legal for a woman to obtain an abortion if there is a strong chance of a serious defect in the baby (Schuman & Presser, 1981). We examined the possibility that such context effects might be smaller among people whose attitudes on a topic are important or are held with great certainty.

Five experiments were conducted testing this proposition. Four involved the abortion context effect previously described, and the fifth involved questions about U.S. and Japanese import restrictions. In this latter case, more people say that Japan should be allowed to restrict imports of U.S. goods if they were just asked whether the United States should be allowed to restrict imports of Japanese goods (Schuman & Ludwig, 1983). Measures of attitude certainty were included in four of the abortion experiments and in the Japanese–American experiment, and a measure of attitude importance was included in one of abortion experiments.

Table 2 presents the results from the first of the abortion experiments, conducted in August 1979. Consistent with our expectations, respondents who said they were very certain of their abortion attitudes were less affected by the alteration in question context than were respondents who said that they found abortion to be a difficult issue on which to reach a decision. The context alteration produced a 10.1% change in marginals for the former individuals, whereas it produced a 28.1% change for the latter; three-way interaction, $\chi^2(1, N = 610) = 4.42, p < .05$. When individuals were separated into those who said abortion was a relatively important issue when they decided how to vote and those for whom it was less important, a similar result was obtained. The context effect was greater among those who reported the issue to be relatively low in importance (7% vs. 23%), three-way interaction, $\chi^2(1, N = 610) = 5.05, p < .05$.

Table 2
Abortion Context Effect

Survey: August 1979

Question 1: Married
Would you tell me whether or not *you* think it should be possible for a pregnant woman to obtain a *legal* abortion if she is married and does not wany any more children?

Question 2: Defect
Please tell whether or not *you* think it should be possible for a pregnant woman to obtain a *legal* abortion if there is a strong chance of serious defect in the baby?

Order 1	Order 2
Married	Defect
Defect	Married

	The last question is on a different subject. Some people are very certain about their feelings about when legal abortions should be permitted. Other people see this issue as a difficult one to reach a decision on. Would you say that you are more like those who are very certain, or that you are more like those who see this issue as a difficult one to reach a decision on?			
	Very certain		A difficult issue	
Response to Question 1	Order 1	Order 2	Order 1	Order 2
Favor (%)	68.1	58.0	67.8	39.9
Oppose (%)	31.9	42.0	32.2	60.1
Total %	100.0	100.0	100.0	100.0
n	135	150	174	143
χ^2	$\chi^2(1, N = 285) = 3.15$, *ns*, $\Delta = 10.1$		$\chi^2(1, N = 317) = 25.07$, $p < .001$, $\Delta = 28.1\%$	
Three-way interaction	$\chi^2(1, N = 602) = 4.42, p < .05$			

Encouraging as this first study was, four attempts to replicate its results failed. In January 1981, October 1981, and July 1983, the abortion experiment was repeated using only the certainty measure. In all three cases, high- and low-certainty groups showed nearly equal effects of the context manipulation (15.1% vs. 17.8%, 7.6% vs. 12.1%, and 25.0% vs. 22.0%, respectively), and none of the three-way interactions even approached statistical significance, $\chi^2(1, Ns = 613, 604, 337$, respectively) = 0.17, 0.27, and 0.02, respectively. In the Japanese–American experiment, conducted in March 1983, a certainty measure again failed to specify the magnitude of the context effect. High-certainty individuals were no less affected than were low-certainty individuals (30.7% vs. 22.7%), $\chi^2(1, N = 367) = 0.26$, *ns*. Thus, although the initial study was encouraging, later ones failed to indicate that question-order effects depended on the certainty of the attitude being measured. It is tempting to look for a factor that distinguishes the first experiment from the others, but none has been identified. We are therefore inclined to treat the initial significant result as due to chance.

Question Wording

Tone of wording. We considered two types of question wording alterations. The first involved changes in the tone of the wording of a question regarding freedom of speech, taken from work by Rugg (1941). One half the respondents in a December 1983 survey were asked, "Do you think the United States should *allow* public speeches against democracy?" The other half were asked whether they thought such speeches should be *forbidden*. We found a large overall effect due to the change in wording such that more people will *not allow* than will *forbid* such speeches, but as Table 3 shows, individuals who said they were very certain of their attitudes on this issue were influenced just as much as those who were relatively uncertain of their opinions (17.0% vs. 18.9%), three-way interaction, $\chi^2(1, N = 503) = 0.14$, *ns*.

We replicated this experiment in April 1984, using a 5-point response scale and a follow-up question measuring attitude intensity. The wording effect this time was very large for respondents with more intense attitudes (21.7%), but was small for those with less intense attitudes (−0.1%), three-way interaction, $\chi^2(1, N = 435) = 5.51, p < .02$. Thus, the effect of intensity was opposite to what we expected.

Question balance. A second set of studies altered whether questions asking if respondents support a proposition offered a counter-argument to the proposition. Offering a counterargument has been shown to decrease the likelihood that respon-

Table 3
Forbid/Allow Tone of Wording

Survey: December 1983

Question 1: Allow

Do you think the United States should allow public speeches against democracy?

Question 2: Forbid

Do you think the United States should forbid public speeches against democracy?

	Some people are very certain about their feelings on this question of allowing public speeches against democracy. Other people see this issue as a difficult one to reach a decision on. Would you say that you are more like those who are very certain, or that you are more like those who see this issue as a difficult one to reach a decision on?			
	Very certain		A difficult issue	
Response	Question 1	Question 2	Question 1	Question 2
Allow, not forbid (%)	68.7	85.7	56.9	75.5
Forbid, not allow (%)	31.3	14.3	43.1	24.5
Total %	100.0	100.0	100.0	100.0
n	131	140	130	102
χ^2	$\chi^2(1, N = 271) = 11.38$, $p < .001$, $\Delta = 17.0\%$		$\chi^2(1, N = 232) = 8.84$, $p < .001$, $\Delta = 18.9\%$	
Three-way interaction	$\chi^2(1, N = 503) = 0.14$, *ns*			

dents will endorse the original proposition (Schuman & Presser, 1981). Five experiments were conducted on attitudes toward gun control laws, and four experiments examined attitudes regarding whether membership in a labor union should be required of all individuals employed by companies in which some workers are already unionized.

In the case of the gun control questions, three question wordings were used: an unbalanced version, a formally balanced version, and a counterargument version. An example of an unbalanced question is, "Would you favor a law which would require a person to obtain a police permit before he could buy a gun?" The formally balanced version of this question is, "Would you favor or oppose a law which would require a person to obtain a police permit before he could buy a gun?" Finally, one counterargument version of this question stated, "Would you favor a law which would require a person to obtain a police permit before he could buy a gun, *or* do you think such a law would interfere too much with the rights of citizens to own guns?" Previous research found that the unbalanced and formally balanced versions produced nearly identical responses (e.g., Schuman & Presser, 1981), so we combined responses to those two forms and compared them with answers to counterargument versions.

The results of the first such study we conducted in February 1975, shown in Table 4, offered some support for the strength hypothesis. The question-wording change produced essentially no change in the distribution of answers from respondents with intense attitudes (−1.0%, *ns*), whereas there was a moderate and marginally significant effect of the wording change for low-intensity respondents (6.9%), $\chi^2(1, N = 692) = 3.19$, $p < .10$. However, the three-way interaction did not approach statistical significance, $\chi^2(1, N = 1{,}326) = 1.80$, *ns*. This experiment was repeated in August 1978, but the resulting pattern was exactly opposite: The wording effect was greater for the high-intensity group (9.1%) than for the low-intensity group (−5.0%), three-way interaction, $\chi^2(1, N = 530) = 2.73$, $p < .10$. The same experiment was also repeated four times using an importance measure. The three-way interaction was statistically significant in two cases, marginally significant in a third, and nonsignificant in a fourth, but the effect was greatest for the *highest* importance group in three of these: August 1978, high = 16.2%, moderate = 11.4%, low = 8.5%, three-way interaction, $\chi^2(1, N = 530) = 51.21$, $p < .001$; Fall 1978, high = 11.9%, moderate = 1.3%, low = 11.5%, three-way interaction, $\chi^2(1, N = 1{,}398) = 3.28$, $p < .10$; and September 1979, high = 11.5%, moderate = 5.8%, low = 1.1%, three-way interaction, $\chi^2(1, N = 872) = 2.01$, *ns;* and for a moderate importance group in the fourth, February 1976, high = 4.0%, moderate = 14.0%, low = −3.1%, three-way interaction, $\chi^2(1, N = 1168) = 8.19$, $p < .01$.

Results of studies of attitudes regarding required membership in labor unions were quite similar. In four experiments, unbalanced and formally balanced versions of the question were compared with a version that included a counterargument. In three of these studies, attitude intensity was measured, and in the fourth, attitude certainty was measured. The wording effect was highly significant in all four cases, but it was larger in

Table 4
Gun Control Balance

Survey: February 1975

Question 1: Unbalanced or formal balance

Would you favor a law which would require a person to obtain a police permit before he could buy a gun?
Would you favor or oppose a law which would require a person to obtain a policy permit before he could buy a gun?

Question 2: Counterargument

Would you favor a law which would require a person to obtain a police permit before he could buy a gun, *or* do you think such a law would interfere too much with the right of citizens to own guns?

	Compared with how you feel on other public issues, are your feelings on this issue extremely strong, fairly strong, or not very strong?			
	Extremely strong		Fairly or not very strong	
Response	Question 1	Question 2	Question 1	Question 2
Favor (%)	74.9	75.9	67.6	60.7
Oppose (%)	25.1	24.1	32.4	39.3
Total %	100.0	100.0	100.0	100.0
n	447	187	450	242
χ^2	$\chi^2(1, N = 634) = 0.07$, *ns*, $\Delta = -1.0\%$		$\chi^2(1, N = 692) = 3.19$, $p < .10$, $\Delta = 6.9\%$	
Three-way interaction	$\chi^2(1, N = 1326) = 1.80$, *ns*			

the high-intensity group in two of them and larger in the low-intensity and certainty groups in the other two. All of these between-group differences were small, and none of the three-way interactions even approached statistical significance, Fall 1974 (9.2% vs. 13.0%), three-way interaction, $\chi^2(1, N = 1{,}431) = 1.42$, *ns;* September 1980 (9.9% vs. 14.9%), three-way interaction, $\chi^2(1, N = 617) = 0.81$, *ns;* December 1983 (7.0% vs. 0.3%), three-way interaction, $\chi^2(1, N = 525) = 0.72$, *ns;* and April 1984 (12.6% vs. 11.3%), three-way interaction, $\chi^2(1, N = 542) = 0.06$, *ns.*

Thus, the evidence from 11 experiments generally disconfirmed the prediction that answers to attitude questions will be less affected by wording alterations among people whose attitudes are intense, important, or held with great certainty.

Question Form

Alterations in question form involve changes in the wording of questions, but they involve more than just that. They also entail alterations in formal aspects of question structure that can be described in abstract terms. One type of form variation that we examined involved comparison of a question asking respondents to agree or disagree with a statement with a parallel forced-choice question asking respondents to choose the one of two statements that best describes their view on an issue. Presumably as a result of acquiescence bias, more people agree with a statement than express the same point of view by selecting one of two offered opinion statements (Schuman & Presser, 1981). The second type of form variation we considered contrasts a question that asked respondents to choose between two polarized response alternatives and one that also offered a middle alternative. More people select a middle alternative when it is explicitly offered than volunteer that opinion when no middle alternative is offered (Schuman & Presser, 1981).

Acquiescence. In order to assess acquiescence, we compared agree–disagree questions with ones that ask respondents to endorse one of two explicitly presented and conflicting viewpoints. The agree–disagree form of the question used in these experiments read, "Please tell me whether you agree or disagree with this statement: *Individuals* are more to blame than *social conditions* for crime and lawlessness in this country." The forced-choice version of the question read, "Which in your opinion is more to blame for crime and lawlessness in this country—individuals or social conditions?" More people typically indicate *individuals* to be the main cause of crime and lawlessness when asked the agree–disagree form of the question than when asked the forced-choice version (Schuman & Presser, 1981).

Four replications of this experiment were conducted using an intensity follow-up. The first of them, shown in Table 5, offered no support for our hypothesis. The effect of the form change was slightly greater among the high-intensity group than among the low-intensity group (16.9% vs. 9.7%), although the three-way interaction was not significant, $\chi^2(1, N = 849) = 0.85$, *ns.* In the remaining three studies, the form effect was large and statistically significant for both high- and low-intensity groups, with no difference approaching significance between the intensity levels: February 1975 (12.8% vs. 10.5%), three-way interaction, $\chi^2(1, N = 849) = 0.01$, *ns;* April, May, June 1982 (23.9% vs. 19.2%), three-way interaction, $\chi^2(1, N = 769) = 0.39$, *ns;*

Table 5
Individual/Social Conditions Acquiescence

Study: February 1975

Question 1: Agree–disagree

Please tell me whether you agree or disagree with this statement. "*Individuals* are more to blame than *social conditions* for crime and lawlessness in this country." (Do you agree or disagree?)

Question 2: Forced choice

Which in your opinion is more to blame for crime and lawlessness in this country—individuals or social conditions?

	How strongly do you feel about that: quite strongly, or not so strongly?			
	Quite strongly		Not so strongly	
Response	Question 1	Question 2	Question 1	Question 2
Individuals (%)	59.6	42.7	46.9	37.2
Social conditions (%)	40.4	57.3	53.1	62.8
Total %	100.0	100.0	100.0	100.0
n	307	300	113	129
χ^2	$\chi^2(1, N = 607) = 17.52$, $p < .001$, $\Delta = 16.9\%$		$\chi^2(1, N = 242) = 2.33$, *ns*, $\Delta = 9.7\%$	
Three-way interaction	$\chi^2(1, N = 849) = 0.85$, *ns*			

and October, November, December 1982 (28.6% vs. 20.9%), three-way interaction, $\chi^2(1, N = 774) = 1.15$, *ns.* Thus, intensity did not specify the magnitude of acquiescence-response bias.

Middle alternatives. The second type of question-form alteration we examined involves middle alternatives. Consider, for example, a question about legalization of marijuana: "In your opinion, should the penalties for using marijuana be *more* strict or *less* strict than they are now?" In this form of the question, the middle alternative is omitted, although interviewers are typically instructed to accept and record a middle-alternative response if it is offered spontaneously. A comparable question including a middle alternative might read, "In your opinion, should the penalties for using marijuana be *more* strict, *less* strict, or *about the same* as they are now?" More individuals say *the same as they are now* when that option is explicitly offered than when it is not (Schuman & Presser, 1981).

In three experiments, we examined whether this effect was greater among people with low-intensity or low-importance attitudes. The first two involved the marijuana question described earlier, and the third addressed U.S. involvement in the war in Vietnam. An intensity follow-up was used in the Vietnam experiment and in one of the marijuana experiments, and an importance follow-up was asked in the other marijuana experiment. In the first experiment, shown in Table 6, the question-form effect was much smaller for the high-intensity group (9.5%) than for the low-intensity group (23.2%). However, the effect was statistically significant in both groups, and the three-way interaction did not approach significance, $\chi^2(1, N = 1{,}195) = 0.93$, *ns.* The same pattern of differences appeared for the other two replications (5.6% vs. 26.7% and 5.9% vs. 22.8%,

Table 6
Marijuana Middle Alternative

Survey: February 1976

Question 1: Middle alternative omitted

In your opinion, should the penalties for using marijuana be *more* strict, or *less* strict than they are now?

Question 2: Middle alternative offered

In your opinion, should the penalties for using marijuana be *more* strict, *less* strict, or *about the same* as they are now?

Response alternative	How strongly do you feel about this issue: quite strongly, or not so strongly?			
	Quite strongly		Not so strongly	
	Question 1	Question 2	Question 1	Question 2
Polar (%)	92.3	82.8	85.1	61.9
Middle (%)	7.7	17.2	14.9	38.1
Total %	100.0	100.0	100.0	100.0
n	444	430	161	160
χ^2	$\chi^2(1, N = 874) = 18.76$, $p < .001$, $\Delta = 9.5\%$		$\chi^2(1, N = 321) = 22.80$, $p < .001$, $\Delta = 23.2\%$	
Three-way interaction	$\chi^2(1, N = 1195) = 0.93$, *ns*			

respectively), and the three-way interactions in these cases were marginally significant: August 1977, $\chi^2(1, N = 1{,}109) = 3.24$, $p < .10$; and Fall 1974, $\chi^2(1, N = 1{,}291) = 3.03$, $p < .10$. Thus, for these middle-alternative experiments, there seemed to be some consistent though weak support for the hypothesis that attitude intensity or importance specified the effect of offering a middle alternative.

Meta-Analysis of Individual Studies

Surprisingly, the 31 tests reported earlier produced very little support for the hypothesis that response effects would be smaller for attitudes that are intense, important, or held with great certainty (for a summary of the results, see Table 7). Of the four response-order experiments, three produced effects in the expected direction and one in the reverse direction; none of the differences were statistically significant. Of the five question-order experiments, three produced results in the expected direction, one of them significant, and two were in the reverse direction, neither one statistically significant. Of the two tone-of-wording experiments, one produced an effect in the expected direction, and the other was in the opposite direction; the latter was statistically significant. Of the nine balance experiments, three produced effects in the expected direction and six in the opposite direction; one of the latter was statistically significant. The four acquiescence experiments produced results in the opposite direction; none were significant. Among these experiments, then, 15 of the tests we performed were in the expected direction and 16 were in the opposite direction. This near 50–50 proportion is certainly consistent with the conclusion of no effect. Therefore, it seems that measures of attitude importance, intensity, and certainty are not useful for identifying which individuals are susceptible to these response effects. The only response effect that provided any confirmation of the hypothesis was the middle-alternative experiments. In all three experiments we conducted, low-intensity and importance respondents were found to flock to the offered middle alternative more than did high-intensity and importance respondents.

These conclusions are reinforced by a meta-analysis of the experiments reported earlier. The meta-analysis was conducted using d′, the difference between proportions, as the measure of effect size and by treating each study as generating two experiments: one for a high-intensity, importance, or certainty group, and one for a low-intensity, importance, or certainty group (see, e.g., Rosenthal, 1984, pp. 86–87).[3] For the total set of experiments, a test of heterogeneity indicated significant differences in the magnitude of effects across studies, $\chi^2 = 134.74$, $p < .0001$. However, as the first row of Table 8 illustrates, a planned comparison of the high-intensity, importance, and certainty effect sizes with the low-intensity, importance, and certainty effect sizes revealed no overall difference ($z = 0.24$, $p = .82$).

We thought it worthwhile to examine whether the measures of intensity, importance, and certainty differ from one another in their effectiveness to identify respondents susceptible to response effects. As the second, third, and fourth rows of Table 8 illustrate, meta-analyses conducted separately for the measures of intensity, importance, and certainty revealed that one was no more likely than another to regulate the magnitude of response effects. The same conclusion is supported by examination of the individual studies separately for measures of attitude intensity, importance, and certainty (intensity, 6 of 14 in the expected direction; importance, 3 of 8 in the expected direction; and certainty, 6 of 9 in the expected direction).

The last six rows of Table 8 display the results of comparisons done separately by type of response effect. The question and response-order experiments evidence no discernible trend in terms of effect size. And, as expected, the middle-alternative experiments evidence a large and highly significant effect size. The effects for the tone of wording, balance, and acquiescence experiments are almost marginally significant ($.10 < p < .16$), but they are all in the direction opposite to the hypothesis. The high-intensity, importance, and certainty groups showed slightly *larger* response effects than did the low-intensity, importance, and certainty groups. However, because two of these three effect sizes are relatively small and all are nonsignificant by conventional criteria, we are reluctant to view them as important. Also, we view the relatively large figure in the case of the tone of wording experiments with some skepticism because of the dramatic discrepancy between the results of two studies on which it is based. It is worth noting, however, that a meta-analysis combining the tone of wording, balance, and acquiescence experiments revealed a significant effect of intensity, importance, and certainty (average effect size = 4.5%, $z = 2.14$, $p = .032$), although it was in the opposite direction from the original hypothesis. In sum, then, the only apparently reliable effect of

[3] To satisfy the assumption of independence between studies, we used only one follow-up measure for each experiment to compute the statistics shown in the first row of Table 8. Therefore, the 4th, 6th, 8th, and 18th data points listed in Table 7 were not included in that meta-analysis. Equivalent results to those in the text were obtained, though, when the 3rd, 5th, 7th, and 17th data points were omitted instead.

Table 7
Summary of Results

Experiment	Crystallization measure	Overall response effect (%)	Significance of overall effect	Effect of crystallization (%)	Significance of crystallization effect	*N*
Response order						
Oil supply						
November, December 1982	Certainty	12.9	$p < .001$	9.4	*ns*	731
May, June 1983	Certainty	9.5	$p < .01$	6.8	*ns*	532
Divorce						
November, December 1982	Importance	7.8	$p < .04$	−7.2	*ns*	720
	Intensity	7.8	$p < .04$	−4.3	*ns*	720
May, June 1983	Importance	6.9	$p = .11$	2.4	*ns*	525
	Intensity	6.9	$p = .11$	4.6	*ns*	525
Question order						
Abortion						
August 1979	Certainty	18.9	$p < .001$	18.0	$p < .05$	610
	Importance	18.9	$p < .001$	16.0	$p < .05$	610
January 1981	Certainty	16.3	$p < .001$	2.7	*ns*	613
October 1981	Certainty	9.4	$p < .02$	4.5	*ns*	604
July 1983	Certainty	24.1	$p < .001$	−3.0	*ns*	337
Japanese/American						
March 1983	Certainty	26.9	$p < .001$	−8.0	*ns*	367
Tone of wording						
Forbid/allow						
December 1983	Certainty	18.6	$p < .001$	1.9	*ns*	503
April 1984	Intensity	10.7	$p < .03$	−21.8	$p < .02$	435
Balance						
Gun control						
February 1975	Intensity	3.8	$p = .15$	7.9	*ns*	1,326
February 1976	Importance	6.2	$p < .02$	−7.1	$p < .01$	1,168
August 1978	Intensity	0.7	*ns*	−14.1	$p < .10$	530
	Importance	0.7	*ns*	−7.7	*ns*	530
Fall 1978	Importance	8.2	$p < .01$	−0.4	$p < .10$	1,398
September 1979	Importance	5.4	$p = .10$	−12.6	*ns*	872
Union						
Fall 1974	Intensity	9.2	$p < .001$	3.8	*ns*	1,431
September 1980	Intensity	11.5	$p < .01$	5.0	*ns*	617
December 1983	Certainty	5.9	$p < .08$	−6.7	*ns*	525
April 1984	Intensity	12.4	$p < .01$	−1.3	*ns*	542
Acquiescence						
Individuals/social conditions						
February 1975	Intensity	15.2	$p < .001$	−7.2	*ns*	849
February 1975	Intensity	12.4	$p < .001$	−2.3	*ns*	849
April, May, June 1982	Intensity	21.6	$p < .001$	−4.7	*ns*	769
October, November, December 1982	Intensity	24.3	$p < .001$	−8.3	*ns*	774
Middle alternative						
Marijuana						
February 1976	Intensity	13.3	$p < .001$	13.7	*ns*	1,195
August 1977	Importance	19.8	$p < .001$	21.1	$p < .10$	1,109
Vietnam						
Fall 1974	Intensity	11.5	$p < .001$	16.9	$p < .10$	1,291

Note. The percentage in the Effect of crystallization column is positive when the low crystallization group had a larger effect than the high crystallization group. That percentage is negative when the effect is larger in the high crystallization group.

intensity, importance, or certainty on response effects in the expected direction according to the meta-analysis appeared in the case of the middle-alternative experiments.

Consistency of Attitude Intensity, Importance, and Certainty Measures Across Experimental Conditions

The design used in the experiments described earlier involved asking two halves of national samples different versions of target questions and then asking a question measuring attitude intensity, importance, or certainty. The analyses described earlier are predicated on the assumption that, even though different versions of the attitude question were asked of the two samples, the follow-up measures assessed the intensity, importance, or certainty of the same attitude on both forms of the questionnaire. For example, the high-intensity group in one half-sample was assumed to be comparable with the high-intensity group in the other half-sample, and so too for the low-intensity half-samples. Compari-

Table 8
Meta-Analysis Results

Experiment set	No. of experiments	Average effect of intensity, importance, or certainty (%)	*z*-score testing average effect	*p*
All experiments	27	−0.4	0.24	.82
Type of crystallization measure				
Intensity	14	−1.0	0.46	.65
Importance	8	−0.6	0.20	.84
Certainty	9	2.8	0.92	.36
Type of response order effect				
Response-order	4	2.8	0.68	.50
Question-order	5	2.8	0.70	.49
Tone-of-wording	2	−10.0	1.54	.12
Balance	9	−2.8	1.04	.15
Acquiescence	4	−5.4	1.49	.14
Middle-alternative	3	17.2	4.80	.00

sons of high- or low-intensity groups across conditions are obviously not meaningful unless group membership was assigned in equivalent manners in both samples.

Given the designs of these experiments, it is impossible to be sure that this assumption was met. However, it is possible to test it in part by examining the equivalence of distributions of responses to the intensity, importance, and certainty measures across questionnaire forms. If these distributions were not identical across forms, that would indicate that the intensities of different attitudes were assessed, or that the initial question affected the intensity follow-up in different ways. If there was no variation in marginal distributions across forms, that does not demonstrate conclusively that the intensity measures tapped the same latent construct in both cases. 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 potential problem seem relatively less likely.

We found few statistically significant differences in intensity, importance, or certainty distributions across forms. In all, 28 comparisons yielded no differences, and 4 yielded significant ones. Two such differences would be expected by chance alone given a confidence level of .05, but, interestingly, all 4 of the significant ones involved the experiments on balance. This might lead one to suspect the equivalence of the intensity measures across question wordings in those cases. However, the other 7 balance experiments showed no significant differences in distributions of responses to the follow-up measures across forms. Furthermore, among the four studies that showed significant differences, the question including the counterargument was associated with reports of greater intensity or importance (as compared with the noncounterargument forms) for two studies and lesser intensity or importance for the other two studies. The average percentage difference in intensity, importance, and certainty between the counterargument forms and the other forms across the nine balance experiments is a mere 0.1%, a figure indistinguishable from zero. Given no indication that including a counterargument either increases or decreases reports of attitude intensity, importance, and certainty, we are reluctant to reject the balance experiments as invalid.

One way to be sure that the follow-up questions address the same attitude is to use a within-subjects experimental design. The same subject could be asked two different versions of a question on two different occasions, and a measure of attitude importance, intensity, or certainty could be asked during one of the interviews. We used this design in two experiments, one dealing with the effects of counterarguments (involving attitudes toward required union membership) and the other dealing with acquiescence (involving beliefs about the causes of crime).

In the fall of 1974, respondents were asked either the unbalanced or the formally balanced version of a question about required union membership, as well as a measure of attitude intensity. In February 1975, the same respondents were asked a version of the question including a counterargument. Using the intensity measure from the first interview, the effect of the counterargument was 8.5% for high-intensity respondents and 7.6% for low-intensity respondents; this difference is not statistically significant, $\chi^2(1, N = 678) = 0.00$. Given that the percentage differences reveal a small trend in the direction opposite to the hypothesis, these data disconfirm the prediction that the effect of the counterargument would be greater for low-intensity respondents.

In October, November, and December 1982, one group of respondents was asked the agree–disagree version of the question about the causes of crime shown in Table 5 and a question assessing the intensity of those beliefs. During a second interview 6 months later, these respondents were asked the forced-choice version of the causes-of-crime question. The effect of question form was 16.5% for the low-intensity group and 13.5% for the high-intensity group; this difference is in the direction predicted by the hypothesis, although it is nonsignificant, $\chi^2(1, N = 305) = 0.11$, *ns*. However, opposite results were obtained from another group of respondents who were asked the forced-choice version of the question during the first interview and the agree–disagree version during the second. The form effect was greater among the high-intensity group (21.2%) than among the low-intensity group (15.9%), although this difference was also not significant, $\chi^2(1, N = 330) = 0.50$, *ns*. Thus, the results of these within-subject experiments are consistent with those from the between-subjects experiments, because all of them indicate no reliable relation between response effects and attitude intensity, importance, and certainty.

This evidence does not completely rule out the possibility that the follow-up measures were not equivalent across experimental conditions in all of the studies we conducted. Perhaps a preferable experimental design would be to measure attitude intensity, importance, or certainty *before* measuring the attitude, rather than after. For example, respondents could be asked, "How important is the issue of gun control laws to you personally?" before being asked whether they favor or oppose such laws. One problem with such a design is that the attitude-importance measure could affect responses to the subsequent attitude measure. If the magnitude of the effect varies depend-

ing on the wording or form of the subsequent attitude questions or depending on the questions preceding the importance measure, interpretation of the results of such experiments might be complicated. Nonetheless, studies using this design ought to be conducted, although we would expect them to produce findings comparable with those reported here.

One justification for this expectation is provided by previous studies of attitude intensity and importance and susceptibility to the influence of persuasive communications. Some of these studies measured intensity or importance before subjects were exposed to a persuasive communication (Halverson & Pallak, 1978; Tannenbaum, 1936; Yankelovich et al., 1981), and others measured these variables after exposure (Ewing, 1942; Fine, 1957; Knower, 1936). Despite this variation in procedures, all of these studies found intense and important attitudes to be more resistant to change than weak and unimportant ones. This variation seems not to have muted a real association in that case, so we doubt that it would in the present studies.

Discussion

We undertook this investigation in order to evaluate the widely held belief that changes in survey-question form, wording, and context have more impact on responses from individuals with attitudes that are weak, unimportant, or held with uncertainty. To test this hypothesis, we examined two types of form effects, two types of wording effects, and two types of order effects. Thus, our methodology used a number of diverse approaches to operationalizing response effects.

The only evidence of a reliable negative relation between attitude intensity and importance and response effects appeared in comparisons between questions that offer middle alternatives and those that do not. People who consider an attitude unimportant or whose attitudes are not intense flock to a middle alternative disproportionately often when it is offered. This result is consistent with previous research: Attitudes that people consider personally important tend to be the subjects of frequent conscious thought (e.g., Wood, 1982), and mere thought about an attitude object tends to polarize attitudes (e.g., Tesser, 1978). As a result, important attitudes are typically more extreme than unimportant ones (e.g., Borgida & Howard-Pitney, 1983; Brent & Granberg, 1982), so people whose attitudes are unimportant would be expected to select a "middle alternative" more often than those with important attitudes. Because nearly all respondents choose from among the response alternatives explicitly offered, relatively few low-importance respondents generate it on their own when no middle alternative is included in a question. When offered a middle alternative, however, low-importance respondents flock to it much more often than do high-importance respondents. Similar reasoning holds in the case of attitude intensity because intense attitudes tend to be extreme (Cantril, 1946; Suchman, 1950). We therefore regard the evidence that reports of important and intense attitudes are less affected by middle-alternative manipulations as constituting a special and limited effect of those variables, rather than an indication of a more general effect of attitude crystallization on all response effects.

We failed to detect a reliable negative relation between attitude intensity, importance, and certainty and susceptibility to the other types of response effects we examined. Therefore, we are faced with trying to explain these counterintuitive results. One possible explanation for these findings is that the measures of attitude intensity, importance, and certainty we used lack either reliability, validity, or both. These and similar measures are, in fact, only moderately reliable (Krosnick, 1986; Schuman & Presser, 1981, p. 261). If limited reliability were the only problem, however, we would expect to see fairly consistent but weak relations between these measures and response effects. The evidence of much inconsistency in direction across experiments in terms of the effects of intensity, importance, and certainty casts doubt on the unreliability explanation.

Although the present data do not permit direct assessment of the validity of the follow-up measures, evidence from other studies attests to their validity. Measures of attitude importance, intensity, and certainty have been shown to specify the susceptibility of attitudes to persuasion (e.g., Ewing, 1942; Fine, 1957; Knower, 1936; Yankelovich et al., 1981), the stability of attitudes over time (e.g., Krosnick, in press-a; Schuman & Presser, 1981), the accessibility of attitudes in memory (Krosnick, 1986), the extremity of attitudes (Borgida & Howard-Pitney, 1983; Brent & Granberg, 1982; Knower, 1936), the level of evaluative consistency between attitudes (Jackman, 1977; Judd & Krosnick, in press; Schuman & Presser, 1981; Smith, 1982), the impact of attitudes on social evaluations (Krosnick, in press-b), and the strength of the attitude–behavior relation (Krosnick, in press-b; Schuman & Presser, 1981). Furthermore, the measures of attitude intensity and importance used here succeeded in identifying respondents susceptible to the response effect illuminated by the middle-alternative experiments. All this suggests that the measures used here to assess attitude intensity, importance, and certainty do not lack validity, although continued efforts to improve the measurement of these attitude attributes are certainly in order.

A related possible explanation for the present results is that, even though our measures of attitude intensity, importance, and certainty may be sufficiently reliable and valid, they may be too crude to identify those few individuals at the extreme low end of the dimensions whose attitude reports evidence response effects. We cannot evaluate this possibility directly with the present data. However, if it were true, we would again expect to have seen some consistent, albeit weak, relation between our measures and the magnitude of response effects. Because no such relation appeared, this explanation is questioned. Furthermore, we found no systematic difference between the results of experiments in which the crystallization measures isolated small groups of low-crystallization respondents and the results of experiments in which the measures isolated relatively large groups of low-crystallization respondents. And again, the evidence that our intensity and importance measures successfully identified individuals who were especially susceptible to response effects in the middle-alternative experiments attests to the precision of those measures.

A third possible explanation for the present findings is that the relations of attitude intensity, importance, and certainty to response effects are not monotonic but rather are curvilinear. Perhaps individuals who care a great deal about an issue are uninfluenced by response effects because their attitudes are strong, and individuals who don't care at all about an issue are uninfluenced because they pay little attention to the details of relevant questions. Perhaps only individuals who care at least

moderately about an issue both devote careful processing to an attitude question and have sufficiently uncrystallized attitudes so as to evidence response effects (see, e.g., Schuman & Presser, 1981, p. 197; Stember & Hyman, 1949/1950). Plausible as this explanation seems, it turns out not to be supported by our data. Many of our follow-up measures allowed us to divide respondents into only two groups, but some allowed finer-grained divisions. In none of these cases did we find any reliable indications of curvilinear relations between response effects and intensity, importance, or certainty.

Given the apparent untenability of these alternative possibilities, we are inclined to believe that attitude intensity, importance, and certainty do not determine the magnitude of most of the response effects we examined. This suggests that the theoretical justification for our hypothesis is incorrect. Our expectation that intense and important attitudes and those held with great confidence would show smaller response effects was based on two sets of beliefs, one about the characteristics of these attitudes and one about the mechanisms of response effects. We assumed that these attitudes are more extreme, are associated with less ambiguous and more accessible internal cues, and are more resistant to the influence of persuasive arguments. We assumed further that some response effects occur because either (a) attitude-question wording, form, or context influences the perceived extremity of response alternatives, (b) attitude-question wording, form, or context influences respondents' self-perceptions regarding their attitudes, or (c) persuasive arguments within questions change respondents' attitudes. A great deal of evidence (reviewed earlier) supports our assumptions about the characteristics of intense and important attitudes. They have been shown repeatedly to be more extreme, associated with clearer and more accessible internal cues, and more resistant to the influence of persuasive communications. We are therefore reluctant to believe that the present evidence calls those assumptions into question.

Instead, we are inclined to interpret the present evidence as indicating that the mechanisms of the response effects we examined are not what we thought they were. That is, these response effects may not result from changes in perceived extremity of response options, changes in self-perceptions, or changes in attitudes. If they result instead from other mechanisms, there may be little reason to expect that intense or important attitudes or those held with great certainty would be less susceptible to them.

Although a great many studies document response order, question order, tone of wording, and balance effects, few studies have been conducted examining the psychological mechanisms of those effects. The little work that has been done in these areas is hardly conclusive, but it suggests that the mechanisms of these effects may not involve changes in the perceived extremity of response options, changes in self-perceptions, or changes in attitudes. Response-order effects seem likely to be produced by interactions of memory, elaborative thought, and framing effects and seem most likely to occur among people for whom the cognitive and motivational costs of elaborate cognitive processing are high (Krosnick & Alwin, 1987). Question-order effects may reflect the impact of belief priming, self-presentational concerns, assimilation and contrast effects, or changes in the objects about which respondents perceive the question to be asking (Nakamura, 1959; Schuman & Ludwig, 1983; Schuman & Presser, 1981). And acquiescence and balance effects may result from self-presentational concerns (Lenski & Leggett, 1960) or personality dispositions (Couch & Keniston, 1960). If the response effects we examined result from these psychological processes, the factors that regulate each are likely to vary from effect to effect. Furthermore, high intensity, importance, or certainty attitudes would not necessarily be expected to be less responsive to these effects than low intensity, importance, or certainty ones.

Attitude intensity, importance, and certainty are not the first general variables to fail to identify the individuals who are most responsive to changes in question form, wording, and context. In an extensive investigation, Schuman and Presser (1981) tested the claim that response effects are smaller among individuals with extensive formal education as compared with those with relatively little formal education. This hypothesis was based on the belief that the former individuals are more cognitively sophisticated and are therefore more practiced at and better able to deal with multiple choice-type questions and to recognize and avoid being influenced by biased wordings. Contrary to Schuman and Presser's expectations, more educated individuals did not show consistently smaller response effects than less educated individuals. However, education has been found to identify highly susceptible individuals for particular response effects (see, e.g., Krosnick & Alwin, 1987, in press). This evidence therefore reinforces the conclusions that a single variable (such as education or attitude intensity, importance, or certainty) is unlikely to moderate all response effects and that, instead, each response effect is likely to have unique moderators.

As we noted earlier, it is widely taken for granted that the quality distinguishing attitudes that resist response effects from those that do not is crystallization. Our experiments examined only three measures of attitude crystallization: intensity, importance, and certainty. During the last 30 years, however, attitude research has examined a number of other variables that might be viewed as reflecting attitude crystallization, including direct behavioral experience with an attitude object (e.g., Fazio, 1986), vested interest in an attitude object (e.g., Sivacek & Crano, 1982), and affective–cognitive consistency (e.g., Chaiken & Baldwin, 1981). It is tempting to think that these characteristics of attitudes all reflect a single attribute (i.e., attitude crystallization) and to expect that they all have identical effects and correlates. Surprisingly, though, these dimensions are only weakly correlated with each other and with measures of attitude intensity, importance, and certainty (Budd, 1986; Budd & Spencer, 1984; Chaiken & Yates, 1985; Fazio & Zanna, 1978; Norman, 1975; Sivacek & Crano, 1982; see also Raden, 1985). It therefore seems appropriate to view these dimensions as relatively independent of one another, to view the present findings as applying only to intensity, importance, and certainty, and to leave open the possibility that different results might have been obtained if different aspects of attitude crystallization had been measured.

In this light, it is useful to consider a study by Chaiken and Baldwin (1981). They found that the phrasing of questions asking individuals about their past behavior toward an object can affect reports of attitudes. Furthermore, the effect of the behavior questions was found to be greater for individuals whose attitudes were characterized by low affective–cognitive consistency

than for individuals whose attitudes were characterized by high affective–cognitive consistency. This result might seem at first to conflict with the present findings because a measure of attitude crystallization was found to regulate the magnitude of a response effect. However, both the measure of attitude crystallization and the response effect Chaiken and Baldwin examined were different from those we examined here. We view their finding as we do the results of the present experiments on middle alternatives: The effect observed seems likely to be limited to the particular attitude attribute measure examined due to the unique mechanism of the response effect examined.

We take a similar view of Hippler and Schwarz's (1986) evidence regarding tone of wording effects. They showed that the asymmetry between responses to questions using the words *forbid* and *allow* occurred primarily among respondents who said they were *indifferent* about whether salt ought to be used to melt snow on streets. Indifference may be sufficiently distinct from intensity and certainty so that both their evidence and ours is valid. However, it may also be that their finding is a result of a methodological artifact that they point out (Hippler & Schwarz, 1986, p. 94). We look forward to further research attempting to resolve this discrepancy.

Obviously, our explanations for the results reported here are largely speculative and do not fully satisfy our intuition that attitude intensity, importance, and certainty *should* moderate response effects. However, if the problem is to be solved, it is essential to recognize and confront the array of negative results reported in this article. What seems needed at this point is more definitive research aimed at discovering the psychological mechanisms of the many reliable response effects thus far documented. On the basis of such evidence, it may be much easier to specify the interacting variables that identify the individuals who are most susceptible to each effect.

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