
Attitude Importance and the False Consensus Effect

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Numerous studies have demonstrated that the attitudes people consider personally important have an enhanced impact on cognition and behavior. This article explores the possibility that importance may regulate the magnitude of the false consensus effect as well. The authors report on six experiments that involved a variety of subject samples (college students and the general population), data collection methods (telephone interviewing and self-administered questionnaires), and political issues (e.g., abortion, gun control, defense spending). Meta-analyses summarizing the results of 12 tests revealed a strong false consensus effect but no reliable relation between its magnitude and attitude importance. These results are inconsistent with the assumption that the false consensus effect is a result of attitudes directly or indirectly influencing perceptions of others, and they lend support to explanations of the false consensus effect that posit other mechanisms (i.e., attribution and object construal).

During the last 30 years, a great deal of research has been devoted to investigating differences between attitudes in terms of their strength (e.g., see Petty & Krosnick, 1995). One particular dimension of attitude strength that has received considerable attention is attitude importance (e.g., Byrne, London, & Griffitt, 1968; Gorn, 1975; Krosnick, 1988a, 1988b, 1989; Schuman & Presser, 1981). Attitude importance is one's perception of the degree to which one's attitude toward an object is personally important and is therefore a matter of significant personal concern, investment, and value. Research on this attitude attribute has demonstrated that the attitudes people consider personally important possess the four hallmark features of strong attitudes (see, e.g., Krosnick & Petty, 1995): They are resistant to change (e.g., Fine, 1957; Gorn, 1975; Rhine & Severance, 1970), are stable over time (e.g., Hahn, 1970; Kendall, 1954;

Krosnick, 1988b), have strong impact in guiding behavior (e.g., Krosnick, 1988a; Schuman & Presser, 1981), and have strong impact on information processing (e.g., Byrne et al., 1968; Pelham & Swann, 1989).

The starting point of the investigation reported in this article is this last attribute of important attitudes, their enhanced impact on information processing. A number of such effects of importance have been documented to date. For example, one's liking of others is based partly on similarity between oneself and others in terms of attitudes (Byrne, 1971), and attitudinal similarity has a particularly strong impact on interpersonal attraction when the attitudes involved are personally important to the perceiver (Byrne et al., 1968; Clore & Baldridge, 1968; Krosnick, 1988a). Attitudes toward objects are shaped primarily by the objects' attributes toward which perceivers have more personally important attitudes (Pelham & Swann, 1989; Rosen & Ross, 1968; Watkins & Park, 1972). Attitude importance also instigates greater between-person discrepancies in the perception of others' attitudes (Krosnick, 1988a).

In this article, we focus on another instance in which important attitudes may have an enhanced impact on

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cognition: the false consensus effect (FCE). The FCE has been observed numerous times since its first demonstration at least 5 decades ago (Wallen, 1943). When asked to estimate the proportion of others who possess a particular attitude toward an object, people who themselves hold that attitude generate higher estimates than do people who do not hold that attitude (Brown, 1982; Newcomb, 1943, chap. 7; Newcomb, Koenig, Flacks, & Warwick, 1967; Ross, Greene, & House, 1977; Spears, Eiser, & van der Pligt, 1989).¹

A variety of cognitive and social mechanisms for the FCE have been proposed (e.g., see Marks & Miller, 1987), and many of them presume that this effect occurs because individuals' attitudes either directly or indirectly shape perceptions of others' attitudes. If this is correct, then as the importance of the perceiver's attitude increases, its impact on perceptions of attitude prevalence should also increase. Thus explanations of the FCE that posit direct or indirect effects of attitudes on perceptions imply that the FCE may be greater for highly important attitudes than for relatively unimportant ones.

For example, one explanation of the FCE attributes it to the relative salience of perceivers' own attitudes when estimates of prevalence are made (e.g., see Marks & Miller, 1987). Consequently, one may focus attention on one's own attitude as a starting point and make adjustments to it according to the likely differences between oneself and others (e.g., Davis, Hoch, & Ragsdale, 1986). Because such adjustments away from anchors are seldom sufficient (Tversky & Kahneman, 1974), perceptions are likely to be biased in the direction of the perceiver's own attitude. Important attitudes are more accessible and therefore more salient in memory than unimportant ones (e.g., Krosnick, 1989); hence, important attitudes may serve as especially powerful anchors during consensus estimation.

A second possible explanation for the FCE is selective affiliation (see Marks & Miller, 1987). According to this view, people estimate the general prevalence of an attitude by assessing its prevalence among whatever sample of other individuals they can draw from their own memories. People's attitudes tend to shape their friendship selection, because people tend to associate with those who are similar to themselves (Berscheid & Walster, 1978; Newcomb, 1961), and this is especially true for attitudes that people consider personally important (Byrne et al., 1968; Tedin, 1974, 1980). Therefore, this mechanism, too, suggests a stronger FCE in the case of such attitudes.

The FCE may also be a result of motivations such as self-esteem maintenance, need for social support, and social interaction goals (see Marks & Miller, 1987). For example, one's self-esteem may hinge partly on the belief

that one holds correct attitudes, and perceived agreement of others may help to validate those attitudes. Consequently, the desire to maintain, protect, or enhance self-esteem may lead individuals to exaggerate agreement with others. Because individuals are presumably especially invested in the correctness of the attitudes they consider most personally important, this perspective also anticipates an especially strong FCE for these attitudes.

Finally, the FCE may occur because people employ a simple, evaluation-driven cognitive heuristic in estimating others' attributes; this is called *the evaluation principle* (Sherman, Chassin, Presson, & Agostinelli, 1984). According to this perspective, people may infer that liked others tend to have good qualities, and their judgments of which qualities are good may be shaped partly by their own qualities. Thus perceivers' own attitudes may shape their judgments of which attitudes are good, and these judgments may in turn shape estimates of the prevalence of those attitudes among others. Because people are especially personally invested in the attitudes they consider most personally important, they should be especially likely to presume that such attitudes are "good" and are, therefore, particularly prevalent among others.

There are certainly other possible explanations for the FCE that do not necessarily anticipate an effect of attitude importance of its magnitude. Nonetheless, these four (regarding salience, selective affiliation, self-protection motivation, and the evaluation principle) all anticipate that the FCE will be particularly strong when it involves personally important attitudes.

Some previous studies have explored the relation of the FCE to dimensions of attitude strength related to importance, but these studies produced mixed and contradictory results. Crano (1983) found a stronger FCE among people who had vested interests in their attitudes; Campbell (1986) found a weaker FCE among individuals whose attitudes were more personally important; and Judd and Johnson (1981) found that increased issue involvement was associated with increases in estimates of the proportions of others who agree and disagree with oneself. However, Crano (1983) and Campbell (1986) used unconventional measures of the FCE in their studies, and Judd and Johnson's (1981) sample involved a highly restricted range of attitude positions (see Spears & Manstead, 1990). Using more conventional methods, van der Pligt, Ester, and van der Linden (1983) and Spears et al. (1989) found no consistent relation between the FCE and attitude extremity. Thus previous studies do not have any clear implications regarding attitude importance. We, therefore, set out to assess the relation of attitude importance to the FCE more thoroughly and directly by conducting a series of six studies.

TABLE 1: Key Methodological Features, Experiments 1 Through 6

Experiment Number	Data Collection Method	Subjects	Issues
1	Telephone survey	Students ($n = 399$)	Abortion
2	Telephone survey	Students ($n = 200$)	Central America, defense spending, abortion
3	Self-administered	Students ($n = 92$)	Defense spending, abortion, gun control
4	Telephone survey	General public ($n = 825$)	Defense spending, abortion
5	Self-administered	Students ($n = 469$)	Gun control, legal marijuana, nuclear power
6	Telephone survey	General public ($n = 178$)	Abortion

METHOD

Overview

In each study, we assessed subjects' own attitudes, their estimates of the prevalence of the attitude among others, and the personal importance of each attitude. We then assessed whether the FCE was greater for attitudes that were highly important versus attitudes that were relatively unimportant.

The studies varied in terms of a number of methodological features (see Table 1). Some studies were conducted via telephone interviews, whereas others asked subjects to complete self-administered questionnaires in our laboratory. Some experiments used representative samples drawn from the general public, whereas others used samples of college students. Across the studies, a range of different political attitudes were used to assess the FCE (for complete details on the methodology and results of these experiments, see Fabrigar, 1991).

Measures

In each experiment, subjects responded to target questions on one or more political issues. For each issue, subjects reported their own attitudes on the issue (the *self* question), their perceptions of the percentage of Americans favoring a particular position on the issue (the *other* question), and the personal importance of the issue to them.²

Experiment 1. After answering 90 questions on a range of topics including AIDS and university services, subjects indicated whether they favored or opposed legalized abortion and estimated the percentage of Americans who favored legalized abortion. Subjects then reported the amount of personal importance they attached to the abortion issue on a 5-point rating scale.

Experiment 2. After answering 20 questions on various political issues, subjects answered questions addressing U.S. involvement in Central American nations, defense spending, and abortion. Attitudes were again measured via dichotomous favor/oppose items, and estimates were made of the percentage of Americans who favored involvement in Central America, decreased defense spending, and legalized abortion. Two importance questions

on each issue, using 5-point scales, were all asked in a group immediately before all the self and other questions.

Experiment 3. After answering seven general political questions, subjects responded to questions on defense spending, abortion, and gun control. Subjects' attitudes were measured on 11-point rating scales, and estimates were made of the percentage of Americans who favored decreased defense spending, legalized abortion, and strict gun control laws.³ Two importance questions on each issue, using 5-point scales, were asked in a group immediately after all the self and other questions.

Experiment 4. After answering 10 general questions about politics, subjects responded to questions on defense spending and abortion. Subjects' attitudes were measured using dichotomous favor/oppose items, and estimates were made of the percentage of Americans who favored decreased defense spending and legalized abortion. One importance question on each issue, using a 5-point scale, was asked before all the self and other questions.

Experiment 5. Subjects immediately answered questions on gun control, legalized marijuana, and nuclear power. Subjects' attitudes were measured using dichotomous favor/oppose items, and estimates were made of the percentage of Americans who favored stricter gun control laws, legalization of marijuana, and widespread use of nuclear power. To assess whether our findings might be altered by varying the location of attitude importance measures in the questionnaires, two 5-point importance measures for each issue appeared immediately before the self and other questions for all three issues for a randomly selected half of the subjects, and the importance measures appeared immediately after the self and other questions for the remaining subjects.

Experiment 6. Following 20 questions on general political concerns and on gun control, subjects answered questions about abortion and environmental pollution laws. Subjects' attitudes were measured using dichotomous favor/oppose items, and estimates were made of the percentage of Americans who favored legalized abortion and strict environmental protection laws. Two im-

portance measures for both issues, using 5-point scales, either preceded or followed all self and other questions.

Subjects and Data Collection

Experiment 1. A representative sample of 399 Ohio State University students were interviewed by telephone by a staff of experienced interviewers. Because the order in which questions are asked can affect the magnitude of the FCE (Mullen, Driskell, & Smith, 1989; Mullen et al., 1985; Mullen & Hu, 1988), we rotated this order across subjects. A total of 202 subjects were randomly assigned to receive the self questions before the other questions (self/other order), whereas 197 received the questions in the reverse order.

Experiment 2. A representative sample of 200 Ohio State University students were interviewed by telephone by a trained and experienced staff of interviewers. Half were randomly assigned to receive the self/other question order, and the other 100 received the reverse order.

Experiment 3. A total of 92 Ohio State University students completed a self-administered questionnaire to fulfill a requirement for an introductory psychology course. Of the total, 45 subjects were randomly assigned to receive the self/other question order, and 47 received the reverse question order.

Experiment 4. A representative sample of 825 adult residents of Columbus, Ohio, were interviewed by telephone by a staff of experienced interviewers. A total of 403 were randomly assigned to the self/other question order, and 422 received the other/self order.⁴

Experiment 5. A total of 463 Ohio State University students participated to fulfill an introductory psychology course requirement. Subjects were randomly assigned to receive one of four self-administered questionnaires in which the order of the self and other questions, and the location of the attitude importance measures had been manipulated. Receiving Form A of the questionnaire (self/other order, all importance questions preceding all self and other questions) were 118 subjects; 110 received Form B (other/self, all importance questions preceding all self and other questions); 118 received Form C (self/other order, all importance questions following all self and other questions); and 117 received Form D (other/self, all importance questions following all self and other questions).

Experiment 6. A representative sample of 177 adult residents of Columbus, Ohio, were interviewed by telephone by a staff of experienced interviewers. Subjects were randomly assigned to receive one of four questionnaire forms that again varied the order of the self and other questions, and the location of the importance questions. Receiving Form A (self/other order, all im-

portance questions preceding all self and other questions) were 46 subjects; 49 received Form B (other/self, all importance questions preceding all self and other questions); 44 received Form C (self/other order, all importance questions following all self and other questions); and 38 received Form D (other/self, all importance questions following all self and other questions).

Analyses

Attitude importance indexes. In Experiments 2, 3, 5, and 6, the two items measuring the importance of each attitude were summed to create an index.

Hypothesis tests. To assess the impact of attitude importance on the FCE, we employed two analytic strategies. In our first analysis, we regressed estimates of others' attitudes on an issue on subjects' reports of their own attitudes on that issue, attitude importance, and question order.⁵ Attitude importance was coded 0 for subjects below the median and 1 for those above it, and question order was coded 1 for the self/other order and 0 for the other/self order.⁶ A significant, positive regression coefficient for subjects' own attitudes would indicate that subjects who took a favorable stand thought that a greater percentage of others would do so, as compared to the estimates provided by subjects who took an opposing stand.

The effect of attitude importance on the FCE was assessed by adding an interaction term to this regression. This term was computed by multiplying the attitude importance dummy variable by subjects' own attitudes. A significant, positive Self \times Importance interaction would indicate that the FCE was greater among subjects whose attitudes were more personally important. The impact of question order was tested by adding a Self \times Question Order interaction term and the three-way interaction of Self \times Attitude Importance \times Question Order.

In Experiments 5 and 6, the location of importance questions was dummy coded (1 when the importance measures preceded all other questions, and 0 when they followed all others). Interactions between this variable and all other independent variables were computed and added to the regressions, as was a main effect for the dummy variable.

The independent variables were entered in these regression equations in a block hierarchical fashion: All main effects were entered as a block in Step 1; all two-way interactions were added in Step 2; the three-way interactions were added in Step 3; and the four-way interaction was added in Step 4 for Experiments 5 and 6. At each step, only the highest order terms were interpreted (see Cohen & Cohen, 1983, for a description of this procedure).⁷

In our second analysis, we assessed the impact of attitude importance on the FCE by dividing subjects into high- and low-importance groups for each issue and then computing *t* tests within each group. In each *t* test, responses to the self question were treated as the independent variable, and responses to the other question as the dependent variable. A significantly higher mean percentage estimate among subjects who reported a favorable attitude than among subjects who reported an unfavorable attitude would indicate a significant FCE. Furthermore, a significantly larger *t* value among high-importance subjects than among low-importance subjects would indicate that the FCE strengthens with increasing attitude importance. Similarly, larger *t* values under either of the question orders would indicate that question order moderated the FCE.

Meta-analyses. To test the overall significance of the attitude importance effect across all six experiments, the Stouffer technique of combining significance levels was used (see Rosenthal, 1991). Tests of the homogeneity of probabilities were computed following the recommendations of Rosenthal (1991) and Wolf (1986). All calculations were performed using Mullen's (1989) Advanced BASIC Meta-Analysis program.⁸

Two meta-analytic strategies were employed. The first focused on the results of the regression analyses involving interactions. For each test of the interaction between subjects' attitudes and attitude importance, an *F* value was computed, and all such *F* values were subjected to a meta-analysis. The same approach was used to assess the impact of question order on the FCE.

A second meta-analysis was conducted using the results of the *t* tests. In this analysis, the *t* values for the FCE among high-importance subjects were combined to yield an overall effect significance level, and the *t* values for the FCE among low-importance subjects were combined to yield an overall effect significance level. A planned contrast of these two combined significance levels was conducted to determine whether the FCE was greater under high importance than low importance. A similar strategy was used to investigate the impact of question order. Additionally, to see whether question order regulated the importance effect, we used the same procedure, generating separate overall significance levels for high and low importance under the two question orders and comparing them to one another.

Data points gathered from the same subjects on multiple issues are not independent of one another and, therefore, cannot be treated as independent observations in meta-analyses of significance levels such as these. Therefore, in each experiment involving more than one political issue (i.e., Experiments 2, 3, 4, and 5), *F* values were combined across issues to yield a single overall significance level for that experiment using the mean *Z*

procedure for combining multiple dependent variables recommended by Rosenthal (1991). As a result, only one mean *Z* value for each experiment was used in each meta-analysis.⁹

RESULTS

The FCE

The FCE was strong and significant in every test of it (see column 3 and 4 of Table 2).¹⁰ Thus, regardless of the procedures used for data collection or the subject population, a clear FCE was present.

Attitude Importance

In the regression analyses, the FCE was greater among high-importance subjects in 6 of the 12 tests (i.e., 50%) shown in the fifth and sixth columns of Table 2. Only two of these differences were statistically significant ($p = .02$ and $.01$), and one difference in the opposite direction was also significant ($p = .02$). The meta-analysis suggested that the overall importance effect was not statistically significant ($Z = 0.89$, $p = .37$). Additionally, the probabilities were found to be homogeneous, and tests of planned contrasts indicated that the importance effect did not vary significantly across subject populations or data collection methods.

The *t*-test analysis yielded similar results (see Table 3). Of the 12 tests, 5 indicated a larger FCE under high importance, and 6 indicated a larger effect under low importance.¹¹ The meta-analytic contrast between the *t* tests for high-importance and low-importance attitudes was nonsignificant ($Z = .06$, $p = .95$), again suggesting that attitude importance did not influence the FCE.

Question Order

Regression analyses indicated that the FCE was not significantly affected by question order. The FCE was greater under the other/self order than the self/other order in 8 of the 12 tests, and two of these differences were statistically significant. The remaining four tests indicated a greater FCE under the self/other order, but none of these tests were significant. Meta-analysis of the regression results revealed a nonsignificant tendency for the FCE to be greater under the other/self order than under the self/other order ($Z = -1.74$, $p = .08$). The *t*-test analysis provided similar results. A meta-analytic contrast of the *t* tests of the FCE in the two orders was not significant ($Z = -1.41$, $p = .16$).¹²

Impact of Question Order on the Importance Effect

To assess whether the attitude importance effect might appear under one of the question orders, we began by examining the regression analysis results regarding the interaction between question order and

TABLE 2: Significance Tests and Effect Sizes of the False Consensus Effect (FCE) and Interaction of Attitude Importance with FCE, Experiments 1 Through 6

Experiment Number	Issue	FCE		Interaction of Attitude Importance With FCE	
		Effect Size (<i>r</i>)	Significance Test (<i>F</i>)	Effect Size (<i>r</i>)	Significance Test (<i>F</i>)
1	Abortion	.22	17.07** (1, 327)	.03	0.28 (1, 324)
2	Central America	.40	29.74** (1, 156)	-.11	1.86 (1, 153)
	Defense spending	.27	13.70** (1, 169)	-.17	5.15* (1, 166)
3	Abortion	.20	7.67** (1, 178)	-.02	0.08 (1, 175)
	Defense spending	.37	14.23** (1, 88)	-.09	0.75 (1, 85)
4	Gun control	.30	8.70** (1, 88)	-.15	1.98 (1, 85)
	Defense spending	.26	52.45** (1, 724)	-.04	0.89 (1, 721)
5	Abortion	.31	74.69** (1, 690)	.09	5.07* (1, 687)
	Gun control	.25	29.33** (1, 456)	.08	2.64 (1, 450)
	Legal marijuana	.19	17.67** (1, 457)	.02	0.19 (1, 451)
6	Nuclear power	.33	55.98** (1, 458)	.04	0.59 (1, 452)
	Abortion	.19	5.72* (1, 147)	.21	6.28** (1, 141)
Meta-analysis	All issues	.27	Z = 10.97**	.01	Z = 0.89

NOTE: Positive *r* values indicate an FCE (column 3) and a greater FCE under high importance than low importance (column 5). Degrees of freedom are in parentheses.
p* < .05; *p* < .01.

attitude importance. This interaction was significant in three cases and marginally so in three others (see column 3 of Table 4). However, it took a different form in each of these six instances, and in no case was the importance effect significantly larger under one order than the others. No meta-analysis of this interaction could be performed because of the difficulties of coding the complex and multiple directions of the interactions, but none seemed necessary given the inconsistency of the patterns.

To test for a question order effect more directly, we used the *t*-test method. Specifically, we computed *t* tests estimating the magnitude of the FCE separately among high- and low-importance subjects for each question order. These *t* statistics were then subjected to planned contrasts, which indicated that the attitude importance effect was not significant under either of the two question orders (self/other order: *Z* = .34, *p* = .73; other/self order: *Z* = .28, *p* = .78).

Impact of Attitude Importance Measure Location

In the questionnaires of Experiments 5 and 6, the manipulations of the location of the attitude importance

measures had no statistically significant interactions with attitude importance (see column 4 of Table 4).¹³ Again, no single pattern was apparent in the relevant interactions yielded by the regression analyses, so no meta-analyses could be performed. We also conducted a meta-analysis using the *t*-test approach, combining both Experiments 5 and 6 with the earlier experiments (which involved between-study variation in the location of the importance questions). This meta-analysis again indicated no significant importance effect under either location (importance questions first: *Z* = 0.70, *p* = .48; importance questions second: *Z* = 1.28, *p* = .20).¹⁴

Question Order and Importance Measure Location

One last set of tests explored the possibility that the question order and importance measure location might interact in regulating an importance effect on the FCE. One of the four interactions testing this in our regressions was marginally significant (see column 5 of Table 4), indicating the expected importance effect only under the other/self order when the importance measures were last. However, very different patterns were observed in each of the other three instances, so no meta-analysis

TABLE 3: Significance Tests and Effect Sizes of the False Consensus Effect (FCE) Under High and Low Importance, Experiments 1 Through 6

Experiment Number	Issue	FCE Under High Importance		FCE Under Low Importance	
		Effect Size (r)	Significance Test (t)	Effect Size (r)	Significance Test (t)
1	Abortion	.23	3.02** (1, 161)	.21	2.82** (1, 166)
2	Central America	.31	3.09** (1, 88)	.53	5.20** (1, 68)
	Defense spending	.10	0.85 (1, 72)	.37	3.97** (1, 97)
	Abortion	.18	1.60 (1, 75)	.22	2.25* (1, 103)
3	Defense spending	.39	2.54* (1, 36)	.43	3.04** (1, 40)
	Gun control	.19	1.21 (1, 38)	.30	1.99* (1, 39)
4	Defense spending	.19	2.55** (1, 180)	.28	6.90** (1, 544)
	Abortion	.45	6.27** (1, 157)	.27	6.48** (1, 533)
5	Gun control	.29	4.62** (1, 233)	.21	3.24** (1, 224)
	Legal marijuana	.22	3.73** (1, 264)	.15	2.12* (1, 194)
	Nuclear power	.30	5.13** (1, 263)	.36	5.33** (1, 196)
6	Abortion	.37	3.49** (1, 76)	-.01	0.05 (1, 72)
Meta-analysis	All issues	.27	$Z = 7.57^{**}$.25	$Z = 7.65^{**}$
Contrast between high and low importance:			$Z = .06, p = .95$		

NOTE: Positive r values indicate an FCE. Degrees of freedom are in parentheses.

* $p < .05$; ** $p < .01$.

was possible. Furthermore, meta-analyses of t tests from all studies again indicated no significant importance effect in any of the four cells of a Question Order \times Location matrix (self/other order, importance questions first: $Z = 0.11, p = .91$; self/other order, importance questions second: $Z = 1.09, p = .28$; other/self order, importance questions first: $Z = 0.68, p = .50$; other/self order, importance questions second: $Z = 0.73, p = .46$).

DISCUSSION

Although we found a consistent FCE across a wide range of political issues, subject populations, and data collection methods, we found no indication that attitude importance moderated its size. This is surprising in light of explanations for the FCE that propose it results from the direct or indirect impact of individuals' attitudes on perceptions of others (i.e., focus of attention, selective affiliation, motivation, and the evaluation principle). Because important attitudes have an especially powerful impact on other attitude-related cognitive processes, this should presumably have been the case for the FCE as

well. As Marks and Miller (1987) have made clear, definitive and direct tests of these explanations for the FCE have not yet been conducted, but there is at least some evidence consistent with each of them.

Why might our results have disconfirmed the implication of these explanations regarding attitude importance? One possibility is that our tests were flawed by measurement problems or other methodological inadequacies. For example, one might imagine that our studies did not effectively measure attitude importance. However, in many previous studies using operationalizations equivalent to ours, self-reports of attitude importance have succeeded in specifying the susceptibility of attitudes to persuasion (Ewing, 1942; Fine, 1957; Knowler, 1936), the stability of attitudes over time (Krosnick, 1988a; Schuman & Presser, 1981), the accessibility of attitudes in memory (Krosnick, 1988b, 1989), the extremity of attitudes (Borgida & Howard-Pitney, 1983; Brent & Granberg, 1982), the level of evaluative consistency between attitudes (Jackman, 1977; Judd & Krosnick, 1989), the impact of attitudes on social evaluations (Krosnick, 1988b), and the strength of the attitude-

TABLE 4: Significance Tests of Interactions, Experiments 1 Through 6

Experiment Number	Issue	Importance × Question Order	Importance × Location	Importance × Question Order × Location
1	Abortion	$F(1, 323) < .01$ $p = .95$		
2	Central America	$F(1, 152) = 3.48$ $p = .06$		
	Defense spending	$F(1, 165) = 3.59$ $p = .06$		
	Abortion	$F(1, 174) = 3.06$ $p = .08$		
3	Defense spending	$F(1, 84) = 1.31$ $p = .26$		
	Gun control	$F(1, 84) = 0.06$ $p = .80$		
4	Defense spending	$F(1, 720) = 4.73$ $p = .03$		
	Abortion	$F(1, 686) = 4.45$ $p = .04$		
5	Gun control	$F(1, 446) = 2.40$ $p = .12$	$F(1, 446) = 0.38$ $p = .54$	$F(1, 445) = 0.57$ $p = .45$
	Legal marijuana	$F(1, 447) = 5.86$ $p = .02$	$F(1, 447) = 0.08$ $p = .78$	$F(1, 446) = 0.54$ $p = .46$
	Nuclear power	$F(1, 448) = 0.45$ $p = .50$	$F(1, 448) = 2.33$ $p = .13$	$F(1, 447) = 3.38$ $p = .07$
6	Abortion	$F(1, 137) = 0.04$ $p = .85$	$F(1, 137) = 0.24$ $p = .63$	$F(1, 136) = 0.04$ $p = .84$

behavior relation (Krosnick, 1988b; Schuman & Presser, 1981). Because self-reports of attitude importance have consistently worked effectively in the past, it seems very unlikely that they failed here.

A second potential explanation for our failure to find importance effects is that there may not have been much variance in the importance of the examined political attitudes to our subjects. Specifically, one might argue that these issues are likely to be highly important to all our subjects or that they are likely to be unimportant to all our subjects. Such restriction of range would depress associations between attitude importance and any other variable, including the size of the FCE. However, this is also unlikely to have occurred here, for two reasons. First, there was substantial variance in all the importance measures we collected. Second, comparable importance measures have yielded sufficient variance to detect strong effects in the many studies using comparable subject populations reviewed above.

An alternative approach to assessing importance effects is to compare attitudes that differ in the consensual importance attached to them by subjects. So, for example, false consensus might have been measured for an attitude that is presumably highly important to all subjects and one that is minimally important to all subjects (e.g., see Lastovicka & Gardner, 1979; Rhine & Severance,

1970). However, as Petty and Cacioppo (1986, p. 83) and Kiesler, Collins, and Miller (1969) have articulated, this approach confounds importance with other differences between objects. Consequently, it is difficult to be confident about what is responsible for any observed effects. Furthermore, the only way to be sure that the objects actually differ in consensual importance is to measure perceived importance using the same methodology we used here. Ultimately, then, one must rely on the validity of this method for a manipulation check. We therefore do not consider this alternative approach to be an especially useful one with unique advantages.

Our hypotheses regarding attitude importance were based on the assumption that as compared to attitudes people consider personally unimportant, important attitudes are more accessible and salient, lead to more attitude-based attraction, and involve more personal investment and relevance to the self. Do our failures to uncover importance effects here raise doubts about the validity of these assumptions? Given the large existing body of evidence consistent with them, we think not.

Instead, we view our results as suggesting two alternative views, one of the FCE and the other of attitude importance. With regard to the FCE, our results raise the possibility that it does not result from direct causal impact of people's own attitudes on their perceptions of

others. If this is so, an attitude's accessibility, salience, immediate social support, and degree of personal investment would be irrelevant to the process.

Indeed, there are at least two possible explanations for the FCE that are not predicated on the assumption that attitudes directly or indirectly shape perceptions of prevalence, and they seem all the more plausible in light of our evidence. One such explanation emphasizes the role of attributional reasoning (see Marks & Miller, 1987). This perspective is based on the well-established tendency of people to attribute their own attitudes to situational rather than dispositional factors (e.g., Jones & Nisbett, 1971). A rational attributional analysis would, therefore, presumably lead people to infer that whatever situational factors determined their own attitudes will also determine most others' attitudes. Consequently, when individuals are asked to judge the prevalence of their own attitudes among others, they may, therefore, infer them to be relatively common. Thus the FCE would not be a result of attitudes directly affecting perceptions. Rather, individuals make the same assumption about the causes of their own attitudes and others' attitudes, and thus the two resemble one another.

There is considerable empirical support for this explanation. For example, several experiments have shown that the FCE is stronger when people are told to focus on the situational causes rather than on the dispositional causes of their choices (Gilovich, Jennings, & Jennings, 1983; Zuckerman & Mann, 1979; Zuckerman, Mann, & Bernieri, 1982). Additionally, the FCE has been found to be stronger for hypothetical behaviors rated as likely to be influenced by situational determinants, and weaker for behaviors rated as likely to be influenced by dispositional determinants (Gilovich et al., 1983).

The attributional explanation provides no basis for expecting to find a relation between attitude importance and the FCE. It is certainly plausible that individuals might engage in more attributional reasoning when an attitude is important, but this would not be sufficient to produce an association between importance and the FCE. Such a relation would require individuals to attribute their important attitudes more to situational causes than to dispositional causes. Although this is an interesting possibility, we see no compelling reason why importance would be associated with a particular kind of attribution. Similarly, there are no empirical data to support such an assumption. Thus the failure to find a relation between importance and the magnitude of the FCE does not appear to be particularly problematic for the attributional explanation of the FCE.

A second potential mechanism of the FCE that does not assume that attitudes influence perceptions of prevalence involves differential object construal (Gilovich, 1990). According to this view, any verbal statement of an

attitude object, such as "legalized abortion," is ambiguous to at least some extent and is, therefore, open to interpretation. Some people may assume that this means legalized abortion under any circumstances, whereas other people may assume it means legalized abortion under specific conditions (e.g., in cases of rape or incest). The former individuals' broad interpretation may lead them to express less positive attitudes and to assume that others would also be relatively negative toward the concept. In contrast, the latter individuals' more narrow interpretation might lead them to express more positive attitudes and to assume that others would be relatively positive. The difference between these two groups' estimates of others' attitudes would constitute what is traditionally viewed as an FCE. Thus the similarity between people's own attitudes and perceptions of others' attitudes is a function of both being based on a common object construal. People's own attitudes do not have any direct impact on their perceptions of others' attitudes.

Empirical evidence supporting the differential construal explanation has been provided in a series of studies (Gilovich, 1990). For example, questions that were rated as being open to wide latitudes of construal showed stronger FCE than questions that were rated as permitting only narrow latitudes of construal. Similarly, experimental manipulations of the specificity of questions have revealed that the FCE is greater when subjects receive a general version of a question (e.g., "Do you prefer the color tan or aqua?") than when subjects receive a specific version of a question (e.g., "Do you prefer a tan or aqua swatch?").

As with the attributional explanation, the differential construal explanation does not appear to anticipate an association between importance and the FCE. Although a relation between latitude of construal and attitude importance is possible, we see no compelling reason why individuals should systematically have either broader or narrower latitudes of construal when an attitude is important. Additionally, there is no empirical evidence supporting such a relation. Thus the lack of an association between attitude importance and the FCE does not appear to threaten the validity of the differential construal explanation.

Along very different lines, it is conceivable that our failure to find an association between attitude importance and the FCE is not due to the FCE's being caused by attributional reasoning or differential construal. Instead, it is possible that attitude importance may influence the magnitude of the FCE in two opposing ways. Greater attitude importance may enhance the impact of an attitude on perceptions of others, via mechanisms such as salience or selective affiliation. At the same time, attitude importance may also encourage more extensive gathering and processing of attitude-relevant informa-

tion (Berent & Krosnick, 1993a, 1993b). This may, in turn, produce better memory for attitude-relevant information (Berent & Krosnick, 1993a). Such information might well include knowledge concerning the prevalence of a particular attitude position among others (e.g., via news media reports of polling results). This link between importance and knowledge is supported by Krosnick (1990), who has found that the accuracy of individuals' perceptions of where presidential candidates stand on issues increases as the importance of the attitude increases. Similarly, other research has found that individuals are capable of generating more pieces of information about an issue in open-ended thought listings when the issue is personally important (Krosnick, Boninger, Chuang, Berent, & Carnot, 1993). Thus the enhanced impact of important attitudes on perceptions of others may be counterbalanced by greater knowledge concerning the actual prevalence of an attitude position among others. This could yield a null association between attitude importance and the FCE.

In closing, it is interesting to note that our failure to find an importance effect here parallels the findings reported by Krosnick and Schuman (1988). In their study, 27 separate experiments were conducted and meta-analyzed to explore the impact of attitude importance, intensity, and certainty on susceptibility to changes in attitude reports because of question form, wording, and context alterations. Despite a strong theoretical rationale for expecting effects of intensity, importance, and certainty, Krosnick and Schuman (1988) found none. Their results challenged a number of popular explanations of response effects and served to stimulate new theories concerning the nature of response effects (e.g., Krosnick, 1991; Narayan & Krosnick, 1993). We hope that the present studies will serve a similar function in the false consensus and attitude importance literatures.

NOTES

1. Marks and Miller (1987) presumed that the FCE is revealed by (a) higher estimations of the proportion of a group that shares one's own attributes relative to estimates made by those who do not share the attribute, and (b) higher estimations of the similarity between one's attributes and one's perceptions of the attributes of any single other person relative to estimates made by those who do not share the attribute. We are reluctant to assume that these two types of effects are produced by the same cognitive mechanisms. Clearly, at the very least, methodological complications plaguing assessments of beliefs about individuals do not interfere with assessments of beliefs about members of a group (e.g., see Judd, Kenny, & Krosnick, 1983). Therefore, we confine our attention here to estimates of the proportions of groups.

2. Throughout this article, we refer to questions gauging one's own attitude as the *self* question and questions gauging estimates of others' attitudes as the *other* question. This terminology differs from that used by Mullen and his colleagues (Mullen et al., 1985, 1989; Mullen & Hu, 1988), who referred to reports of one's own attitude as *choices* and estimates of others' attitudes as *estimates*. Because these terms have not

been widely adopted in the false consensus literature (see Marks & Miller, 1987), we prefer to use terms that differentiate more clearly the two sorts of questions being asked of people.

3. Because only one low-importance subject fell on the negative side of the attitude continuum for the abortion issue, we did not include this issue in the analyses reported in this article. However, when we performed the analyses again including abortion data from Study 3 when possible, our conclusions were unaltered.

4. In this experiment, respondents receiving the other/self order were all interviewed before any interviewing of the remaining respondents began.

5. In Experiments 1, 2, 4, 5, and 6, attitude self-reports were coded 1 = favor and 0 = oppose. In Experiment 3, attitude self-reports on the 11-point scale were treated as continuous variables rather than dichotomous measures.

6. Although importance was measured on a continuous scale, it was treated as a categorical variable for the purposes of the analyses in this experiment and all subsequent experiments. This was done because past experience has suggested that importance effects tend to be more powerful when treated as a categorical variable (Krosnick, 1986, 1988b). This may indicate that importance is categorical in nature. Alternatively, measurement error in the importance measures may attenuate any advantages derived from the additional information provided by the continuous measure data. Parallel analyses treating importance as a continuous variable were conducted for all tests in each of the six experiments. These analyses yielded results that were very similar to but weaker than the results of analyses using importance as a categorical variable.

Previous studies of attitude importance have indicated that it is sometimes most effective to place only subjects selecting the highest importance category into the high-importance group and to place all other subjects in the low-importance group (e.g., see Krosnick, 1988b). Only Experiment 4 had a sufficiently large sample to permit employing this approach while still having a sufficiently large high-importance group. Therefore, this was the one case when we departed from the approach of dividing subjects into the high- and low-importance groups based on the median of the importance distribution.

7. This analytic approach required that subjects provide complete data on all measures involved in the analysis for each issue. Because of incomplete data, 68 subjects were dropped from the final analysis in Experiment 1. In Experiment 2, 40 subjects were dropped from the analysis for Central America; 27, for defense spending; and 18, for abortion. No subjects were dropped from the analysis in Experiment 3. In Experiment 4, 97 subjects were dropped from the defense spending analysis, and 131 were dropped from the abortion analysis. In Experiment 5, 10 subjects were dropped from the analysis for gun control; 9, for marijuana; and 8, for nuclear power. And in Experiment 6, 25 subjects were dropped.

8. Throughout the text, we report results of analyses of significance levels. We conducted parallel analyses of effect sizes and found the results to be very similar to those of significance levels.

9. A possible alternative approach to analyzing our data would have been to treat each issue's data as if it were independent of all other issues, even when the same subjects were involved. However, because such an approach would violate the statistical assumption of independent tests, we report only the more defensible analyses in which each experiment contributed a single set of summary statistics to the meta-analysis (see Mullen, 1989; Rosenthal, 1991).

In the studies in which subjects were asked about more than one political issue, it is conceivable that later attitude and perception reports were contaminated by earlier ones. Specifically, having answered questions about one issue, subjects might have then been able to anticipate the order in which questions about the other issues would be asked. This might have then clouded any question order effects. We therefore conducted an additional meta-analysis, using only the results for the first issue asked about in each study. However, the results of this analysis were comparable to those reported in the text.

10. The results for environmental laws in Experiment 6 are not reported because only four subjects reported opposing such laws. Thus it was not possible to compute regressions, because there was no meaningful variance in the key independent variable.

11. One test could not be computed, because there was insufficient variance in the attitude score for the low-importance subjects to compute a *t* test.

12. These results contradict two meta-analyses and two experiments reported by Mullen and his colleagues (Mullen et al., 1985, 1989; Mullen & Hu, 1988). Given that the meta-analyses did not involve random assignment to question order, these order effects were possibly a result of unknown confounds across studies. It is more difficult to reconcile our results with Mullen's experiments (Mullen et al., 1989). In these experiments, a significant question order effect was found when subjects were randomly assigned to respond to questions on preferences and risky choice dilemmas in either a self/other or an other/self order. Our failure to find reliable question order effects even when pooling results across more than 1,900 subjects raises some doubts concerning the generality and/or reliability of this order effect.

13. Because the interaction may be configured in numerous different ways, the valences of the *F* values in the table do not reflect the directions of the interaction.

14. Following Campbell (1986), we also assessed the effect of attitude importance on the FCE, controlling for attitude extremity in the one study that permitted this: Experiment 3. Doing so did not alter our conclusions at all: The importance effect remained nonsignificant.

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