

# Party Over Policy: The Dominating Impact of Group Influence on Political Beliefs

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Four studies demonstrated both the power of group influence in persuasion and people's blindness to it. Even under conditions of effortful processing, attitudes toward a social policy depended almost exclusively upon the stated position of one's political party. This effect overwhelmed the impact of both the policy's objective content and participants' ideological beliefs (Studies 1–3), and it was driven by a shift in the assumed factual qualities of the policy and in its perceived moral connotations (Study 4). Nevertheless, participants denied having been influenced by their political group, although they believed that other individuals, especially their ideological adversaries, would be so influenced. The underappreciated role of social identity in persuasion is discussed.

One of the most durable lessons in social psychology is the power of group influence. Yet, that lesson commands relatively modest attention in contemporary research on attitude change. While it is acknowledged that group influence can bias responses to persuasion (Chen, Duckworth, & Chaiken, 1999; Fleming & Petty, 2000; Mackie, Worth, & Asuncion, 1990), the present article rests on the more fundamental claim that groups define the very meaning of objects in the social world.

The interpersonal transmission of meaning is underscored by the finding that as early as 12 months of age human beings interpret objects in light of the emotional expressions of others (Moses, Baldwin, Rosicky, & Tidball, 2001). In the context of persuasion, the judgments of other people in a reference group designate the *social meaning* of attitude objects. Social meaning is defined here as the perceived compatibility of an object of judgment with socially shared values (see also Johnson & Eagly, 1989; Kahan, 1999; Sears & Funk, 1991). Because social groups serve as a primary source of personal values (Bettencourt & Hume, 1999; Conover & Feldman, 1984; Heaven, 1999; Hogg & Abrams, 1988; Newcomb, 1943; Sherif, Sherif, & Nebergall, 1965; Turner, 1991), people can safely assume that other members of a self-defining reference group have a moral sensibility similar to their own, at least with respect to issues relevant to their group identity. Indeed, membership in many groups is predicated upon a commitment to common goals, and everyday experience reinforces the expectation that people who share one's social (i.e., group) identity will also

share one's values. The judgments of group representatives and leaders can thus be viewed as diagnostic, if not definitional, of social meaning.

One aspect of this definitional process involves the factual qualities ascribed to the attitude object (Asch, 1952; Ichheiser, 1970; Robinson, Keltner, Ward, & Ross, 1995). In a seminal series of studies by Solomon Asch (1948), students who were told that their peers felt negatively toward "politicians" construed that term to signify corrupt or low-status public servants such as those involved in Tammany Hall, while students told that their peers felt positively toward politicians called to mind venerable statesmen like Franklin D. Roosevelt. One interpretation is that students used the knowledge that their peers valued political integrity to interpret their judgment and to determine the consensual definition of "politicians." Once an object is so defined, attitudes shift in a manner consistent with that definition (see Allen & Wilder, 1980; Lord & Lepper, 1999; Wood, Pool, Leck, & Purvis, 1996; cf. Griffin & Buehler, 1993).

Another aspect of this definitional process involves the moral qualities highlighted in the object (see Verplanken & Holland, 2002; Wood, 1999). People invoke moral connotations congenial to the judgments of their group. Conservatives thus appeal to the sanctity of life in the context of abortion but then downplay the value of "protecting life" in favor of "vindicating justice" in the context of capital punishment. They can thus be both "pro life" and "pro death penalty," even though no inherent factual or philosophical connection exists between these two positions (Kahan, 1999). Likewise, liberals can simultaneously support abortion rights and oppose the death penalty largely because they see different values at stake for each of the two issues. When the same issue can represent different moral commitments, people tend to appeal to the one that justifies the attitude they want to have (Kristiansen & Zanna, 1994; cf. Tetlock, 1986; Tetlock, Peterson, & Lerner, 1996). And, in many cases, that attitude will be the one held by their group (see Baumeister & Leary, 1995; Haidt, 2001).

As the foregoing discussion implies, attitudes do not follow from the objective features of the object alone, for these features are, to a large extent, inferred on the basis of in-group judgments and have no intrinsic merit independent of the decision-maker's values

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(Turner, 1991). On the contrary, what is critical is social meaning—the perceived “goodness of fit” between the attitude object and socially shared values. Social meaning can be inferred, as when people are unaware of the position of their group and must estimate the attitude that someone with their values should hold. For example, political partisans might infer the liberal or conservative merit of a social program by evaluating its features in light of preexisting beliefs about the appropriate goals of such a program (see Cohen, Aronson, & Steele, 2000; Lord, Ross, & Lepper, 1979). But, more often, social meaning is not inferred but transmitted. It is defined by the judgments of other individuals who are trusted to share one’s moral allegiances—that is, individuals who share one’s social identity. For example, a Republican who learns that other party members support a government-funded job training program will regard that program as conservative by definition. He or she will construct the attitude object to fit its assigned social meaning, by ascribing it value-consistent factual qualities (e.g., “teaches important job skills”) rather than value-inconsistent ones (e.g., “gives money away”), and by highlighting moral commitments congenial to the party’s position (e.g., “we must help people to help themselves”) rather than uncongenial (e.g., “we must reduce government spending”). According to this analysis, attitudes derive from the objective features of the attitude object, and their merit in light of preexisting beliefs, primarily when information about ingroup judgments is absent and social meaning must thus be inferred. Attitudes are less responsive to these factors when information about ingroup judgments is available and social meaning directly conveyed.

Two conditions facilitate the effect of in-group judgments on attitude change. The first condition is ambiguity in the attitude object. To the extent that the merits or deficiencies of the object are unambiguous, and its moral connotations established (as with abortion and capital punishment), neither its factual qualities nor its moral qualities will be amenable to alternative interpretation and attitude change will thus be limited (see Chaiken & Maheswaran, 1994). The second condition is the issue relevance of the group. To the extent that one’s group is irrelevant to the attitude issue (e.g., as one’s political affiliation is irrelevant to choosing kitchenware), its judgments will be viewed as uninformative with respect to social meaning and attitude change will again be limited.

### Blindness to Group Influence

In spite of the posited large impact of group influence, people may be blind to it, and instead assume that their attitudes follow from an impartial assessment of relevant facts. Several phenomena are relevant to this prediction. First, people are motivated to see themselves as objective and free of bias, as research on the “bias blind spot” (Pronin, Lin, & Ross, 2002), “naïve realism” (Robinson et al., 1995; Ross & Ward, 1995; Vallone, Ross, & Lepper, 1985), the “illusion of objectivity” (Armor, 1999), and the “third-person effect” (Davison, 1983) all suggest. Second, people have limited insight into the true sources of their beliefs and behavior, and they tend to base their causal self-analysis on widely held but error-prone folk theories (Nisbett & Wilson, 1977; Petty, Wegener, & White, 1998; Wilson & Brekke, 1994). To the extent that one common folk theory in Western cultures is the belief that selfhood is autonomous in nature (Markus & Kitayama, 1991), the impact of group influence may be underestimated (cf. Miller & Ratner,

1998). Although this review is suggestive, research evidence is scant. In the present studies, group influence on political attitudes is manipulated, and its actual effect compared with its perceived effect.

### Overview of Studies

The first objective of the present studies involved testing the postulated effect of group influence on attitude. Political partisans evaluated an object germane to the values of their group—a proposed welfare reform. As involved partisans, they were expected to undertake effortful (i.e., systematic or central-route) processing of the information (see Eagly & Chaiken, 1993; Petty & Cacioppo, 1986). The studies pitted the effect of reference group information (whether Democrats or Republicans supported the policy) against prior ideological beliefs (whether participants were liberal or conservative) and policy content (whether the policy was generous or stringent). When reference group information was absent, participant ideology and policy content together were expected to determine response, with liberals supporting the generous policy and conservatives supporting the stringent one. However, when reference group information was available, the impact of participant ideology and policy content was expected to decrease, with liberals and conservatives alike assuming the stated position of their party as their own. These hypotheses were investigated in Studies 1–3. Studies 3 and 4 together investigated mediators of the effect of group information (i.e., changes in the social meaning of the policy and in its perceived factual qualities and moral connotations) and the impact of group information on behavior (i.e., the choice to take action for or against the policy).

A second objective of this research involved assessing the correspondence between the actual effect of group information and its perceived effect. All four studies thus asked participants to estimate how much they had been influenced by the position of their party.

### Study 1

Liberal and conservative college students were presented with one of two versions of a welfare policy. One version provided generous benefits, whereas the other version provided stringent benefits. Informal pilot testing confirmed both that self-identified liberals preferred the generous policy to the stringent one and that self-identified conservatives preferred the stringent policy to the generous one (effects more systematically documented in Study 2). The manipulation of policy content was crossed with a manipulation of reference group information. Half the participants were told that Democrats supported the policy; the remaining participants were told that Republicans supported it. The experiment thus featured a 2 (participant ideology: liberal or conservative)  $\times$  2 (policy content: generous or stringent)  $\times$  2 (reference group information: Democrats favor or Republicans favor) between-subjects design.

### Method

#### Participants

All but 12 participants were recruited by telephone from a list of students enrolled in an introductory psychology course; they received course credit

for their participation. The other 12 students were recruited by telephone from a separate departmental pool of participants and paid \$5.

Liberal and conservative students were identified using a mass testing survey administered earlier in the academic term. Students indicated their political affiliation (1 = *very much a Democrat*, 9 = *very much a Republican*), their political ideology (1 = *very much liberal*, 9 = *very much conservative*), their attitude toward welfare (1 = *very much oppose welfare*, 9 = *very much favor welfare*), and how strongly they felt about their attitude toward welfare (1 = *not at all strongly*, 9 = *extremely strongly*). Students who indicated that they were extremely Democrat and liberal (a 3 or lower on each of the relevant scales) and that they favored welfare (a 7 or higher on the relevant scale) qualified as prospective liberal participants. Students who indicated that they were extremely Republican (a 7 or higher), very conservative (a 6 or higher), and generally opposed to welfare (a 5 or lower [the bottom quartile of the distribution]) qualified as prospective conservative participants. (Because of the limited number of conservative students in the participant pool, the relevant selection criteria were loosened somewhat.) To help ensure a sample of involved partisans, all participants were also required to have indicated that they felt strongly about welfare (at or above the median response of 5). The selection procedure yielded a sample of 48 liberals (67% women) and 31 conservatives (42% women).<sup>1</sup>

## Procedure

Upon arrival, participants were told that the study concerned "memory of everyday current events." They were informed that they would read a random selection of two newspaper reports and afterward complete a test assessing their recall. They were told that each report was followed by a questionnaire assessing their responses, as these might "be related to your performance on the later memory exercise." While the first report served no purpose except to reinforce the plausibility of the cover story, the second one presented a state welfare proposal. Although it was fabricated, the report was formatted to resemble an authentic newspaper article.

**Policy content manipulation.** Participants were randomly assigned to one of two versions of the welfare policy report. The "generous policy" version offered almost \$800 per month to a family with one child, an extra \$200 for every additional child, full medical insurance, \$2,000 in food stamps, extra subsidies for housing and day care, a job training program, and 2 years of paid tuition at a community college. While it limited benefits to 8 years, it guaranteed a job after benefits ended, and it reinstated aid if the family had another child. By contrast, the "stringent policy" version provided only \$250 per month and \$50 for each additional child. It offered only partial medical insurance, and imposed a lifetime limit of 1.5 years without the possibility of reinstating aid. In contrast to the generous policy, the stringent one provided no food stamps, housing, day care, job training, paid work, or college tuition. By real-world standards, the contrast between the two policies was stark. No existing program was more generous than the generous one featured here—almost all real-world programs provided less than \$700 per month and limited lifetime benefits to 5 years or fewer; none provided all the additional services supplied in the generous policy used in the present research (see State Policy Documentation Project, 2001; Wetzstein, 2002). Likewise, no existing program was more stringent than the stringent one presented here—almost all real-world programs provided more than \$250 per month, and virtually none imposed a time limit shorter than 2 years.

**Reference group information manipulation.** Policy endorsement was also manipulated in the welfare report. In the Democrats favor condition, the report noted that the policy was supported by 95% of House Democrats (and 10% of Republicans). In the Republicans favor condition, these percentages were reversed. The reference group information was buttressed by including policy-relevant rhetoric ostensibly provided by prominent Democrats and Republicans. In the Democrats favor condition, a Democrat was quoted as saying that the policy would "lighten the financial burden of

the poor," and accused Republicans of "victim blaming." A Republican was also quoted in dissent, stating that "the program was too costly," and would reward people for "having children [that they] cannot support." By contrast, in the Republicans favor condition, a Democrat was quoted as saying that the policy was "only a band-aid effort." A Republican was again quoted in dissent, stating that the program "provides sufficient coverage . . . without undermining a basic work ethic and sense of personal responsibility."

**Dependent measures.** To assess attitude, participants were asked to indicate their support of the policy on a scale ranging from 1 (*extremely opposed*) to 7 (*extremely in favor*). To assess the perceived causal sources of their attitude, they were asked to estimate the extent to which each of the following four factors contributed to their attitude toward the welfare proposal: "The specific details of the proposal"; "[Their] own personal philosophy of the role of government in social issues"; "What the typical Democrat or Republican believes"; and "[Their] own background/experience with people on welfare." Responses were made on four separate scales ranging from 1 (*doesn't contribute at all*) to 7 (*contributes a great deal*).

Next, memory of the report was assessed, and the responses served as checks on the manipulations. Participants first estimated the percentages of Democrats and Republicans who supported the policy, and then they estimated the monthly payment and the relevant time limit on benefits. Finally, participants were debriefed and thanked.

## Results and Discussion

### Preliminary Analyses

To ensure that the experimental manipulations were successful, memory of relevant report details was assessed. With a few exceptions, participants accurately remembered both policy content and reference group information. Moreover, accuracy of recall did not vary as a function of experimental condition. Ultimately, however, data from 7 students were discarded prior to analysis—5 students whose memory either of policy content or of reference group information was very inaccurate (i.e., 3 or more standard deviations away from the relevant condition mean) and an additional 2 students who guessed the experimental hypothesis. Because most participants in all studies accurately recalled the content of the manipulations, these checks receive no further discussion.

### Analyses of Attitude Data

Attitude data were examined using a 2 (participant ideology: liberal or conservative)  $\times$  2 (policy content: generous or stringent)  $\times$  2 (reference group information: Democrats favor or Republicans favor) analysis of variance (ANOVA). As predicted, reference group information was the most influential of the three independent variables. The relevant interaction involving participant ideology and reference group information proved highly significant,  $F(1, 64) = 120.81, p < .001$ . Regardless of whether

<sup>1</sup> The participants in the four studies presented in this article were drawn from introductory psychology classes and paid participant pools over several academic terms. According to the selection criteria used, the overall percentage of liberal students completing a given mass testing administration ranged from 7% to 38%, while the percentage of conservative students ranged from 4% to 17%. No study yielded evidence that the overall extremity of participants' ideology (as assessed in mass testing) moderated the effect of group information on attitude.

the policy was generous or stringent, liberal participants supported it if told that Democrats supported it ( $M = 5.46$ ) and they opposed it if told Democrats opposed it ( $M = 3.15$ ),  $t(64) = 7.67, p < .001$ . Likewise, conservative participants supported the policy if told that Republicans supported it ( $M = 5.49$ ) and they opposed it if told Republicans opposed it ( $M = 2.69$ ),  $t(64) = 7.89, p < .001$ . (See Table 1 for the relevant cell means.) By contrast, policy content had no direct effect for either partisan group; the relevant interaction involving participant ideology and policy content was not significant,  $F < 1$ .

However, there arose an unexpected interaction effect involving policy content and reference group information,  $F(1, 64) = 14.74, p < .001$ . Regardless of participant ideology, a policy was rated more favorably if it appeared contrary to the assumed ideological biases of its supporters. The generous policy was thus rated more positively ( $M = 4.94$ ) than the stringent one ( $M = 3.70$ ) in the Republicans favor condition,  $t(64) = 3.93, p < .001$ , and the stringent policy was rated marginally more positively ( $M = 4.59$ ) than the generous one ( $M = 3.88$ ) in the Democrats favor condition,  $t(64) = 1.65, p = .10$ .

### Self-Perceived Bases of Attitudes

The factors that participants claimed to have affected their attitudes diverged from the factors that actually did. They asserted that the "details of the proposal" ( $M = 5.71$ ) and their own "philosophy of government" ( $M = 5.88$ ) contributed to their attitude most, but that "what the typical Democrat or Republican believes" contributed least ( $M = 3.25$ ), paired  $t(71) = 9.59, p < .001$ ;  $t(71) = 11.00, p < .001$ , respectively. Indeed, they rated the position of their party as no more influential than their experience with people on welfare ( $M = 3.33$ ),  $t < 1$ .

### Summary

For both liberal and conservative participants, the effect of reference group information overrode that of policy content. If their party endorsed it, liberals supported even a harsh welfare program, and conservatives supported even a lavish one. The results are consistent with the contention that people base their attitudes on social meaning. Once the policy was socially defined as liberal or conservative, the persuasive impact of its objective content was reduced to nil. Participants also denied having been

influenced by the stated position of Democrats and Republicans, and instead they claimed that their beliefs followed from an apprehension of policy content (guided by their personal philosophy of government).

One unpredicted finding was that each party was more persuasive when its actual position differed from its expected one (i.e., when Democrats supported a stringent policy, and when Republicans supported a generous one). This result could reflect people's preference for moderates over extremists (see Keltner & Robinson, 1996), their tendency to consider the merits of expectation-violating messages more carefully than those of expectation-consistent messages (Maheswaran & Chaiken, 1991), or their trust of communicators who express positions contrary to their assumed biases, beliefs, or self-interests (see Eagly, Chaiken, & Wood, 1981; Walster, Aronson, & Abrahams, 1966). While future research could profitably investigate the issue, this result suggests that participants noticed the inconsistency between the stated position of their party and the expected one. Yet they conformed, even though the stated position plainly defied their party's (and their own) prevailing ideological commitments.

### Study 2

A second study was conducted to replicate and extend the initial findings and to address questions that were not answered in Study 1. One question involved the predicted pattern of results in a condition where reference group information was absent. As discussed in introducing the present studies, under such circumstances people are expected to infer social meaning by evaluating the policy in light of long-held ideological beliefs. Liberals should thus prefer the generous policy to the stringent one, and conservatives should prefer the stringent policy to the generous one. The effortful determination of social meaning would appear to reflect systematic or central-route processing, as one hallmark of such processing is responsiveness to message content (Eagly & Chaiken, 1993; Petty & Cacioppo, 1981). However, as in Study 1, it was predicted that this effortful processing would be bypassed, or redirected, in light of reference group information.

The addition of a no-group-information condition would also help to improve upon the test used to assess awareness of group influence. Participants in Study 1 may have been aware of the effect of "other Democrats and Republicans," but they may have reasoned that policy content was more influential because it constituted the very object of judgment. Study 2 addressed this issue by simply assessing whether participants given group information claimed to be more affected by their party's position than participants given no group information.

A third question addressed by Study 2 concerned the role of prior knowledge in moderating group influence. On the one hand, people who are knowledgeable about welfare might feel more confident in their own appraisal of the policy and thus be less influenced by the position of their party, especially if they are aware of relevant background information such as cost-of-living expenses. On the other hand, high-knowledge individuals might prove as responsive to group influence as their low-knowledge peers, if they too base their attitudes not on objective content per se, but on subjective and socially conferred meaning.

Table 1  
Attitude Toward Policy (Study 1)

Group information condition	Participant ideology			
	Liberal participants		Conservative participants	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Democrats favor				
Generous policy	5.22	0.83	2.38	0.74
Stringent policy	5.70	0.95	3.00	0.82
Republicans favor				
Generous policy	3.75	1.22	6.13	0.99
Stringent policy	2.55	1.04	4.86	0.90

Note. Scale is from 1 (*extremely opposed*) to 7 (*extremely in favor*).

## Method

### Design and Participants

The selection criteria were identical to those used in Study 1. Because of the limited number of conservative students in the participant pool, only liberal students (37 women, 17 men) took part in Study 2. They were assigned to one of four experimental conditions. In two of the conditions, either the generous policy or the stringent one was presented without further commentary. In the remaining two conditions, either the generous policy or the stringent one was presented with reference group information expected to attenuate the effect of policy content (generous policy/Republicans favor; stringent policy/Democrats favor). The other two conditions used in Study 1—the generous policy/Democrats favor condition and the stringent policy/Republicans favor condition—were conceptually unnecessary; indeed, the effect of group information in these conditions would be limited by ceiling or floor effects. Accordingly, these two conditions were dropped from Study 2. To assess the effect of prior knowledge, the mass testing questionnaire also asked students to rate their knowledge of welfare on a scale ranging from 1 (*very little knowledge*) to 9 (*a great deal of knowledge*).<sup>2</sup>

### Procedure

Except for the noted change in the featured experimental conditions, the procedure was generally identical to that used in Study 1. A minor alteration was made to the questionnaire item used to assess awareness of group influence. Rather than ask participants to rate the influence of “the typical Democrat or Republican,” the relevant item omitted the pejorative term “typical” and simply referred to “other Democrats and Republicans.”

## Results and Discussion

### Data Analytic Strategy

A median split was performed on participant knowledge, yielding a low knowledge group (with scores ranging from 1 to 5) and a high knowledge group (with scores ranging from 6 to 9). Two omnibus tests were then undertaken to analyze the attitude data. First, a 2 (policy content: generous or stringent)  $\times$  2 (reference group information: absent or available)  $\times$  2 (participant knowledge: low or high) ANOVA was performed to assess whether the effect of policy content was, as predicted, attenuated by reference group information and whether the effect of group information was, in turn, attenuated by participant knowledge. Because of the nature of the experimental design (i.e., policy content was not fully crossed with group information), a conservative omnibus test was also undertaken—a 4 (condition)  $\times$  2 (participant knowledge) ANOVA. After both analyses were performed, follow-up contrasts were conducted to clarify obtained effects. A few participants failed to fill out the questionnaire completely; as a result, degrees of freedom vary slightly for different comparisons.

### Attitude

Both the policy content  $\times$  group information interaction obtained in the 2  $\times$  2  $\times$  2 ANOVA,  $F(1, 46) = 27.66, p < .001$ , and the overall main effect of condition obtained in the 4  $\times$  2 ANOVA,  $F(3, 46) = 9.23, p < .001$ , yielded a highly significant result. In the absence of group information, participants (all of whom, as noted, were liberal) favored the generous policy more ( $M = 4.75$ ) than the stringent one ( $M = 3.31$ ),  $t(46) = 3.51, p < .002$ . However, when group information was available, these prefer-

ences reversed, such that participants favored the Democrat-supported stringent policy ( $M = 5.00$ ) more than the Republican-supported generous policy ( $M = 3.20$ ),  $t(46) = 3.92, p < .001$ . Neither analysis yielded main effects or interactions involving participant knowledge, all  $F_s < 2$ . (Using participant knowledge as a continuous variable in a regression model yielded the same null result, all  $F_s < 1$ .)

### Self-Perceived Bases of Attitudes

As in Study 1, participants attributed their attitude to policy content ( $M = 5.65$ ) and to their philosophy of government ( $M = 5.41$ ) more than to the views of “Democrats and Republicans” ( $M = 3.15$ ), paired  $t(53) = 9.38, p < .001$ ;  $t(53) = 8.54, p < .001$ , respectively. Indeed, they felt that their experience with people on welfare was marginally more important ( $M = 3.87$ ) than the stated position of their party,  $t(53) = 1.82, p = .07$ . Moreover, they did not rate the position of their party as more influential in the two group information conditions ( $M = 3.46$ ) than in the two no-group-information conditions ( $M = 2.91$ ),  $t(50) = 1.24, p > .22$ .

### Summary

In the absence of reference group information, liberal participants favored the generous policy more than the stringent one.<sup>3</sup> Without knowledge of the views of their group, participants behaved as contemporary models of persuasion would predict they would under conditions of effortful processing. They processed the policy carefully enough to discern its merits, and they chose their attitude accordingly (Eagly & Chaiken, 1993; Petty & Cacioppo, 1986). By contrast, when reference group information was available, participants gave no weight to objective policy content, and instead assumed the position of their group as their own. This effect was as strong among people who were knowledgeable about welfare as it was among people who were not. Finally, participants persisted in the belief that they had formed their attitude autonomously even in the two group information conditions where they had not.

Two alternative explanations for this last result merit brief discussion. First, participants may have denied being influenced by their party because the reference group manipulation entangled the *position* of party lawmakers with their *arguments*—that is, the policy-relevant rhetoric used to buttress the group information. To the extent that participants responded to their party’s quoted rhetoric rather than to its explicitly stated position, their causal self-assessments may have been accurate. Second, participants may have been aware of group influence on their attitudes, but they may

<sup>2</sup> While it would have been desirable to assess knowledge using a series of objective questions, evidence of the validity of the self-report measure used here was obtained. Self-ratings of welfare knowledge (as assessed by the same mass testing questionnaire item) significantly correlated with the quality of essays students wrote on the topic of welfare later in the semester (as assessed by independent coders unaware of students’ self-reported knowledge of welfare),  $r(47) = .34, p < .02$ .

<sup>3</sup> To assess the effect of policy content among conservative participants, a sufficient number were identified and recruited from the participant pool ( $N = 13$ ). As expected, they supported the stringent policy more ( $M = 4.57$ ) than the generous one ( $M = 3.17$ ),  $t(11) = 2.50, p < .05$ .

have downplayed its significance to avoid appearing foolish in the eyes of the experimenter (i.e., because of social desirability pressures). To address these two issues, an additional study was run manipulating reference group information using only percentile information, omitting the policy-relevant rhetoric. Twenty-nine liberal participants were presented with the generous policy either without further commentary or with the additional information that Democrats opposed it. To heighten anonymity, and to minimize social desirability pressures, participants were instructed at the study's outset to seal their questionnaire in a manila envelope upon completion (without providing any identifying information), and then to deposit this envelope in a drop-box (where a stack of previous participants' envelopes was visible). In all other respects, the procedure was identical to that used in Study 2. This study replicated the primary results obtained thus far. While attitudes were more favorable in the no-group-information condition ( $M = 5.20$ ) than in the Democrats oppose condition ( $M = 4.07$ ),  $t(27) = 3.27$ ,  $p < .005$ , the perceived contribution of "Democrats and Republicans" was not significantly lower in the no-group-information condition ( $M = 2.87$ ) than in the Democrats oppose condition ( $M = 3.43$ ),  $t(27) = -1.03$ ,  $p > .30$ .

### Study 3

Study 3 was designed to resolve concerns raised in the two previous studies, and to begin the effort to assess variables mediating the effect of group information on attitude. The first concern involved an alternative explanation for the observed effect on attitude. The reference group information might provide anchors for the generous and stringent endpoints of the welfare benefit scale—what Ostrom and Upshaw (1968) call "perspective." Applied to the present results, perspective theory asserts that liberal participants who learned that their party favored a stringent \$200-per-month policy defined that amount as the generous endpoint of the generous-stringent continuum of benefits, and further assumed that the stringent, Republican endpoint or alternative was less than \$200. Likewise, conservative participants who learned that Republicans favored an \$800-per-month welfare policy might have reasoned that the generous, Democratic endpoint or alternative was more than \$800. Both liberals and conservatives may thus have viewed the policy supported by their party as superior to the assumed alternative.

Study 3 addressed this problem by holding the range of benefits—the perspective—constant. Participants were presented both with a generous policy and with a stringent one. In the no-group-information condition, they were provided with no additional information. In the group information condition, they were told that Republicans supported the generous policy and that Democrats supported the stringent one. Because the two policies were presented side-by-side, the generous-stringent perspective was fixed, and the policy alternative held constant.

Study 3 also attempted to assess variables mediating the effect of reference group information on attitude. It is possible that the effect is driven not by a shift in social meaning, but by heuristic or peripheral-route processing. The results would be perfectly understandable if participants read the message in such a low-effort fashion that they failed to comprehend the content of the policy. One result that casts doubt on this possibility involves participants in the no-group-information conditions in Study 2. Because those

participants were responsive to policy content, they clearly processed effortfully enough to determine the merit of the policy. Nevertheless, it is possible that the reference group information elicited heuristic or peripheral-route processing, causing participants to scrutinize the message less than they otherwise would (Mackie, Gastardo-Conaco, & Skelly, 1992; but see also Mackie et al., 1990). Assuming that their party's position was correct, people may have seen little reason to evaluate the policy carefully. In contrast to this conceptualization of group influence, the one offered here asserts that people who are provided with group information continue to process persuasive information in a systematic manner. However, that processing is biased or redirected to yield a representation of the object consistent with its assigned social meaning (see also Alvaro & Crano, 1996; Asch, 1952; Chaiken & Maheswaran, 1994; Chen et al., 1999; Fleming & Petty, 2000).

Several new measures were included to test whether reference group information (a) elicits heuristic or peripheral-route processing and/or (b) biases the direction of systematic processing. First, *cognitive effort* was measured by asking participants to indicate how closely they read the report (Cacioppo, Petty, Kao, & Rodriguez, 1986). Second, *cognitive responses* were assessed by asking participants to list their thoughts (positive, negative, and neutral) about the policies (Cacioppo et al., 1986; Chaiken & Maheswaran, 1994). Third, *recall* was assessed more thoroughly in Study 3 than in the previous two studies by using an open-ended memory exercise (Cacioppo et al., 1986).

To the extent that group information elicits heuristic processing, participants should report reading the message less closely, generate fewer thoughts, and remember less in the group information condition than in the no-group-information condition (Fleming & Petty, 2000). To the extent that group information biases systematic processing, the number of positive versus negative thoughts generated about either policy (i.e., the *direction* or *valence* of processing) should vary as a function of condition (Chaiken & Maheswaran, 1994).

The cognitive response measure could also yield more evidence in support of the contention that processing of a message containing reference group information is effortful and systematic in nature. If attitudes arise from systematic or central-route processing rather than from heuristic or peripheral-route processing, then they should follow from thoughts—positivity of attitudes should correlate with positivity of thoughts (Cacioppo et al., 1986; Chaiken & Maheswaran, 1994). If attitudes are equally elaborated in the group information condition and in the no-group-information condition, the relevant correlations should be comparable.

## Method

### Design and Participants

To generalize beyond the liberal sample used in the prior study, Study 3 featured conservative participants (11 women, 20 men). In the control condition, they were presented with both a generous policy and a stringent one without further commentary. In the group information condition, they were presented with the same two policies, but informed that Republicans supported the generous policy and that Democrats supported the stringent one.

## Procedure

The procedure was similar to that used in the previous studies. However, the content of the two policies was altered slightly in order to shorten and simplify the news report and to maintain its verisimilitude.<sup>4</sup> Once again, the main concern was whether policy content would determine attitudes when reference group information was absent, and whether the effect of policy content would be attenuated (or overturned) when reference group information was available.

With two exceptions, the dependent measure questionnaire was identical to that used in the previous studies. The first exception was that attitudes toward each of two policies (rather than only one) were assessed. The second exception involved a refinement in the item used to assess awareness of group influence. Participants in Studies 1 and 2 may have failed to construe "Democrats and Republicans" as referring to Democratic and Republican lawmakers (as opposed, for example, to registered voters). This ambiguity was remedied by changing the relevant item to "What Democratic versus Republican lawmakers apparently believe."

In addition, three new sets of measures were introduced to assess cognitive processing (similar to those used by Cacioppo et al., 1986, and by Chaiken & Maheswaran, 1994). The first set of measures assessed *cognitive effort*. Participants were asked "how closely did you read the article" (1 = *not at all closely*, 7 = *extremely closely*) and "how much did you try to carefully read the details about the welfare proposals" (1 = *very little*, 7 = *a great deal*). The second measure assessed *cognitive responses* by providing participants with 3 min to "list all your thoughts about the article and the two welfare proposals it described." The third measure assessed *recall* by asking participants to record "all that you can remember about the two welfare proposals."

Two evaluators independently evaluated the cognitive response exercises. They recorded the total number of thoughts, the referent of each thought (generous policy or stringent policy), and the valence of that thought (positive, negative, or neutral). They also counted the number of program details listed in the recall exercise. Both evaluators were unaware of participants' experimental condition. Interrater agreement proved satisfactory (ranging from  $r = .80$  to  $r = 1.0$ , with a mean  $r = .92$ ), and ratings were thus averaged to provide a single score for each dimension.

## Results

Two participants failed to answer several questionnaire items, again resulting in slightly variable degrees of freedom for different comparisons. In addition, 3 participants suspected the authenticity of the report—2 in the control condition and 1 in the group information condition—and hence their data were discarded prior to analysis.

### Attitude

The attitude data were examined using a 2 (policy: generous or stringent)  $\times$  2 (reference group information: absent or available) ANOVA with policy serving as a repeated measure. The interaction between policy and group information was highly significant,  $F(1, 26) = 10.43$ ,  $p < .005$ . In the no-group-information condition, participants (all of whom, as noted, were conservative) favored the stringent policy more ( $M = 4.71$ ) than the generous one ( $M = 3.00$ ),  $t(26) = 2.49$ ,  $p < .02$ . This difference was eliminated (indeed, fully reversed) in the group information condition, where participants favored the Republican-supported generous policy more ( $M = 5.00$ ) than the Democrat-supported stringent one ( $M = 3.57$ ),  $t(26) = 2.08$ ,  $p < .05$ .

## Measures of Cognitive Processing

*Cognitive effort.* Participants' responses to the two items assessing the effort they invested in reading the report were highly correlated,  $r(26) = .83$ ,  $p < .001$ ; accordingly, they were averaged into a single composite. The mean response was near the maximal value of the 7-point scale ( $M = 6.07$ ), and no difference was found between the group information condition ( $M = 6.29$ ) and the no-group-information condition ( $M = 5.85$ ),  $F < 2$ . Processing in both conditions thus appeared equally effortful.

*Cognitive responses and recall.* This assertion is further supported by results along the cognitive response and recall measures. Participants in the group information condition neither generated fewer thoughts than did participants in the no-group-information condition ( $M_s = 6.46$  and  $7.50$ , respectively),  $F < 2$ , nor recalled fewer policy details ( $M_s = 9.82$  and  $10.14$ , respectively),  $F < 1$ .

If group information biased the direction or valence of processing, it should have caused thoughts about the stringent policy to become more negative and thoughts about the generous policy to become more positive. Contrary to predictions, it did not. The number of negative thoughts concerning the stringent policy was subtracted from the number of positive thoughts concerning that policy (see Chaiken & Maheswaran, 1994). The same procedure was repeated for the generous policy. Neither index was affected by condition,  $F_s < 1$ .

<sup>4</sup> While the generous policy used in Studies 1 and 2 set monthly benefits at about \$800 for 8 years, the generous policy used in Study 3 set them at \$1,000 for 7 years; it also stipulated that after 1 year beneficiaries would participate in a "government work-fare program." Moreover, while the stringent policy in Studies 1 and 2 fixed monthly benefits at \$250 for 1.5 years, the stringent policy in Study 3 fixed them at \$400 for 3 years. As in Studies 1 and 2, the generous policy included complete medical coverage, food stamps, housing, and day care; the stringent policy did not. While these alterations, as noted, served to shorten and simplify the report, they also helped to maintain its credibility by reducing, slightly, the contrast between the two proposals. Like Studies 1 and 2, Study 3 aimed to maximize the difference between the generous policy and the stringent one without undermining the verisimilitude of the report and thus introducing threats to internal validity (i.e., participant suspicion and experimental demand). Presenting the policies side-by-side, however, obviously lowered the threshold of plausibility somewhat, and it was thus necessary to introduce some of the alterations noted above. To confirm that the perceived difference between the two policies remained intact, they were presented to a sample of 39 college undergraduates (presented as Policy A and Policy B, with order of presentation counterbalanced). Thirty-five respondents (90%) correctly identified the generous policy as generous and the stringent policy as stringent. They also indicated that they felt confident in this assessment (1 = *not at all confident*, 9 = *extremely confident*:  $M = 6.85$ ,  $SD = 2.06$ ).

The claim that the alterations made to the policies were of minor conceptual significance is further buttressed by a replication of Study 2. In this study, liberal participants reviewed the original generous policy and stringent policy featured in Studies 1 and 2, but the two policies were presented side-by-side as in Study 3. While participants had to be replaced because of suspicion somewhat more often in the group information condition ( $n = 6$ ) than in the no-group-information condition ( $n = 1$ ), this study replicated the results obtained in Study 2. Once again, the critical policy  $\times$  group information interaction was significant,  $F(1, 20) = 7.60$ ,  $p = .01$ .

The results thus far suggest that attitudes were cognitively elaborated, that is, based on systematic or central-route processing. Participants in both conditions asserted that they read the report carefully, and in the no-group-information condition they based their attitudes on policy content. Another indicator of relatively systematic processing is whether positivity of thought correlates with positivity of attitude (Cacioppo et al., 1986; Chaiken & Maheswaran, 1994). In fact, the extent to which thoughts were biased in favor of the stringent policy over the generous one predicted the extent to which attitudes favored the stringent policy over the generous one,  $r(27) = .38, p < .05$ ; and this correlation did not differ by experimental condition,  $F < 1$ . There was thus no evidence that attitudes were less cognitively elaborated in the group information condition than in the no-group-information condition.

### *Self-Perceived Bases of Attitudes*

Once again, participants ascribed their attitude to policy details ( $M = 5.41$ ), to their philosophy of government ( $M = 5.48$ ), and, to a lesser extent, to their experience with people on welfare ( $M = 3.93$ ), each of which was rated as more influential than the stated position of party lawmakers ( $M = 3.11$ ), paired  $t(26) = 5.56, p < .001$ ;  $t(26) = 5.68, p < .001$ ;  $t(26) = 1.65, p = .11$ , respectively. In fact, they did not rate the position of party lawmakers as more influential in the group information condition ( $M = 3.36$ ) than in the no-group-information condition ( $M = 2.85$ ),  $t < 1$ .

### *Discussion*

The results of Study 3 clarify and extend upon those obtained in Studies 1 and 2. Participants again based their attitude on policy content if reference group information was absent, but they seemingly defied that content if reference group information was available. This effect occurred even though the report presented the generous policy and the stringent one side-by-side. The effect of group information on attitude thus cannot reflect a shift in scale perspective (Ostrom & Upshaw, 1968), because both the generous-stringent range of the benefit scale and the policy alternative were held constant across conditions. In addition, participants again appeared to be blind to the effect of group information on their attitudes.

The measures assessing cognitive processing yield two conclusions. First, group information had a large effect on attitude without decreasing depth of processing or otherwise eliciting heuristic or peripheral-route processing. In both conditions, participants reported reading the report with equal care, generated an equal number of thoughts, and showed equally satisfactory recall. In both conditions, participants exhibited the same correlation between positivity of thought and positivity of attitude—a hallmark of cognitive elaboration (Petty & Wegener, 1998). Second, contrary to predictions, no evidence was found to suggest that group information biased systematic processing. One simple methodological explanation for this null effect, however, involves the possibility that participants had insufficient time to provide a thorough account of their positive, negative, and neutral thoughts toward each of the two policies (a suspicion reinforced in post-debriefing interviews, and a problem subsequently remedied in Study 4).

Two important questions remain unresolved. First, what psychological processes mediate the effect of group information on attitude? Second, would group information affect behavioral outcomes more consequential than the paper-and-pencil ones assessed thus far?

### Study 4

Groups affect attitudes by changing perceptions of the social world. Objects are socially defined as compatible or incompatible with long-held values, and they are then perceived in a manner consistent with their assigned social meaning. This account was tested in Study 4. Liberal participants were presented with a federally funded job training program—one that pilot testing had confirmed to be attractive among politically liberal undergraduates. In the no-group-information condition, the program was described without further commentary. In the group information condition, it was also noted that Democrats opposed the program.

Three measurement strategies were used to assess hypothesized mediating processes and perceptions. First, participants appraised the social meaning of the program by rating it along the liberal-conservative dimension. Second, participants completed the same cognitive response measure used in Study 3, but they were provided with more time to finish it. In addition, their cognitive responses were coded to assess two possible shifts in cognitive processing. On the one hand, group influence could affect the valence of processing but not the content processed. Thoughts might refer to the same factual qualities in both conditions (e.g., “puts people in jobs”), but those qualities might be evaluated more negatively in the Democrats oppose condition than in the no-group-information condition. On the other hand, group influence could define the very content to be processed (Asch, 1952). Thoughts might refer to more stringent factual qualities in the Democrats oppose condition (e.g., “imposes menial labor”) and to more generous factual qualities in the no-group-information condition (e.g., “offers high-paying jobs”).

The second strategy used to assess mediating processes involved asking participants to express and explain their attitude in the context of a written editorial. Like the cognitive response measure, the editorials tapped the perceived factual qualities of the attitude object. In addition, the editorials also tapped its perceived moral connotations. As research in political psychology suggests, liberals are invested in two values that sometimes conflict—individualism and humanitarianism (see Skitka, Mullen, Griffin, Hutchinson, & Chamberlin, 2002; Tetlock, 1986; Tetlock et al., 1996). As interviews conducted in the context of our pilot testing suggested, one reason that liberals supported the job training program used in Study 4 was that it reconciled these two values. Encouraging personal responsibility could be viewed as a means to achieving a humanitarian end (see Kristiansen & Zanna, 1994). To the extent, however, that participants downplay the importance and efficacy of cultivating personal responsibility, and highlight the importance and urgency of providing direct humanitarian relief, they ought to support the program less, and support more the existing welfare system and similar program alternatives that allocate resources immediately without the requirement of work. Participants in the no-group-information condition were thus expected to write editorials appealing both to individualism and to humanitarianism, whereas participants in the Democrats oppose condition were



expected to appeal to individualism less and to humanitarianism more (see also Kristiansen & Zanna, 1994).

Beyond assessing relevant mediational processes, Study 4 also had a pair of secondary objectives. The first involved assessing the effect of group information on consequential behavior. Rather than only indicate their attitude on an anonymous questionnaire, participants could write an editorial either in support of the program or against it, and they were led to believe that their response would affect the decisions of real policy makers. The other objective involved assessing attributions made for the attitudes of others. Although participants were expected to be blind to the effect of group information on themselves, they were expected to be perfectly able to recognize its influence on others (see also Kenworthy & Miller, 2002; Pronin et al., 2002). They may even go a step further and assume that their beliefs are more objective than those of other people (Armor, 1999; Ross & Ward, 1995). To explain their own beliefs, people will naturally cite the most available causal candidates—their thoughts and feelings toward the object (see Malle, 1999), failing to realize that those thoughts and feelings have been shaped, to a large extent, by their group. When explaining the beliefs of others, however, people have no direct access to such rich subjective information. As a result, they are unlikely to discount the causal significance of salient external forces like group influence. As Epley and Dunning (2000) write, perceptions of others “may be more accurate not because of the information people possess about others, but rather because of the information they lack” (p. 868).

### Method

#### Participants

Because of their greater availability in the participant pool, liberal students were again recruited (30 women, 19 men).

#### Procedure

Upon arrival, participants were told that the purpose of the study involved surveying student opinion about social-political issues for the on-campus “Institute of Social Policy Studies.” To that end, they were asked to review a social program aimed at helping poor families to achieve economic self-sufficiency. Participants were given a report (actually fabricated), formatted to resemble an article from the *Economist*. The report described a government-funded program that “would guarantee both job training and gainful employment . . . to poor people who receive welfare benefits and to those whose benefits have ended.” Beyond assisting the poor, the program also had three specific features—a tax incentive to encourage businesses to hire disadvantaged people, mandatory participation for welfare recipients, and projected economic benefits such as higher employment rates. In the no-group-information condition, the report presented the program and evidence of its efficacy without further commentary. In the Democrats oppose condition, the report further noted that Democrats opposed the program and that Republicans supported it.<sup>5</sup> As in Study 3, the report also specified the policy alternative—welfare benefits of “\$750 per month . . . along with food stamps and medical insurance.” Like Study 3, Study 4 thus removed the potential confound of a shift in scale perspective.

The dependent measures were generally the same as the ones used in the prior studies. In contrast to Study 3, however, thought valence on the cognitive response measure was assessed by asking participants to write either a “+”, “-”, or “0” next to each thought depending on whether it was positive, negative, or neutral (see Fleming & Petty, 2000). Participants

were also given 4 min (rather than 3 min) to complete the thought-listing exercise.

Three new dependent measures were also included. The first new dependent measure tapped the social meaning of the program. Participants rated the program along the liberal-conservative dimension using a single 7-point scale (1 = *extremely liberal*, 7 = *extremely conservative*).

The second new set of dependent measures consisted of several items assessing the perceived causes of other people’s attitudes. Participants were again presented with each of the four factors that they had previously rated in terms of its contribution to their own attitude. However, they now rated the contribution of each factor first to the attitude of the average Democrat at their school and then to the attitude of the average Republican at their school (see Robinson et al., 1995, for a similar procedure). Participants thus estimated the contribution of policy details, philosophy of government, party lawmakers, and experience with people on welfare, to their own attitude, to that of the average Democrat, and to that of the average Republican on 12 separate 7-point scales. Participants also rated the objectivity of their own evaluation of the program, that of the average Democrat, and that of the average Republican.

The third new dependent variable was behavioral support. Participants were asked to write an editorial in response to the program. The experimenter explained that their editorial would be forwarded to the on-campus Institute of Social Policy Studies, where it would be used to “assess the views of the student body and . . . to inform public policy.” The experimenter gave participants two forms—one designated for use if they opposed the program and the other designated for use if they supported it. Each form was printed on bond paper, and stamped with the Institute’s letterhead. Participants were told to put their signed editorial in an envelope (addressed to the Institute) upon completion, and then to insert this envelope in a drop-box. Beyond providing a measure of behavioral support, the editorials (as noted) were also coded to assess both the assumed factual qualities of the program and its perceived moral connotations. After completing their editorial, participants were debriefed and thanked.

#### Assessments of Cognitive Responses

Two coders independently assessed each participant’s cognitive response exercise. Neither was aware of participants’ experimental condition. The exercises were evaluated along three dimensions—overall valence of thoughts about the program, overall content of thoughts about the jobs and training component of the program, and valence of thoughts about specific (i.e., unambiguous) program features. To assess overall valence of thought, the coders counted the number of positive, negative, and neutral

<sup>5</sup> For exploratory purposes, this manipulation had also been crossed with a manipulation of report ambiguity. One version of the report used ambiguous arguments (e.g., invocations of desirable economic and ethical principles), while the other version used unambiguous arguments (e.g., scientific evidence of the efficacy of the program along economic indices). Because source cues tend to have a stronger impact on responses to ambiguous information than on responses to unambiguous information (Chaiken & Maheswaran, 1994), it seemed plausible that the effect of group information would be larger for the ambiguous report than for the unambiguous one. This expectation was confirmed in analyses of perceived argument quality (assessed using a series of Likert scales). The arguments in the ambiguous report were rated as strong in the no-group-information condition but weak in the Democrats oppose condition. By contrast, the arguments in the unambiguous report were rated as equally strong in both conditions. However, in analyses of the primary attitudinal, behavioral, and mediational measures, neither main effects nor interactions involving report ambiguity were found. These null effects again attest to the power of group influence. Participants ignored or discounted recognizably strong arguments if they contradicted the judgment of their group.

thoughts directed at the program (omitting thoughts irrelevant to the program, and correcting obvious errors in the identified valence of thoughts). To assess overall content of thoughts, the coders counted the number of thoughts that referred to generous program qualities (e.g., "provides effective job training" or "gives good wages") and the number of thoughts that referred to stringent program qualities (e.g., "provides only menial labor" or "pays only minimum wage"). The final set of assessments was made to test the prediction that group information would have little effect on thought valence toward features that were less ambiguous than the jobs and training component of the program, and hence less amenable to alternative factual interpretation (see Chaiken & Maheswaran, 1994). To assess valence of thought toward these unambiguous program features, the coders counted the number of positive, negative, and neutral thoughts directed at the program's tax incentive, its mandatory component, and its putative economic benefits. Although participants might evaluate the tax incentive or mandatory component positively or negatively, or view the putative economic benefits as likely or unlikely, little if any ambiguity surrounded the factual definition of these features. Interrater reliability proved satisfactory for each dimension (ranging from  $r = .70$  to  $r = 1.0$ , with a mean  $r = .91$ ), and the scores of the two coders were subsequently averaged to provide a single rating for each dimension.

### Assessments of Written Editorials

The coders first assessed each editorial along dimensions similar to those used to evaluate the cognitive response exercises. They counted the number of references made to generous program qualities and the number of references made to stringent program qualities. They also counted both the number of positive references and the number of negative references to the tax incentive, the mandatory component, and the putative economic benefits.

Next, the coders assessed the moral connotations highlighted in the attitude object. They evaluated each editorial along four dimensions tapping the intensity of appeal to individualistic values relative to humanitarian ones: (a) expressed *desirability of cultivating personal responsibility*, as signified by statements such as "it is important to encourage self-reliance"; (b) expressed *danger of providing too much humanitarian aid*, as signified by statements such as "too much help perpetuates laziness, fraud, or poverty"; (c) expressed *danger of providing insufficient humanitarian aid*, as signified by statements such as "without help, people in poverty will go without food." Assessments for these three dimensions were made using separate 5-point scales ranging from 1 (*not at all*) to 5 (*a great deal*). The coders also assessed the *conception of poverty* expressed in each editorial along the individualism-humanitarianism dimension—that is, the extent to which the causes of poverty were implied to rest in the individual (e.g., "poor people lack a work ethic") or in the environment (e.g., "poor people lack resources"). The relevant scale ranged from 1 (*very much inside the individual*) to 5 (*very much in the environment*). Interrater reliability proved satisfactory for each dimension assessed in the editorials (ranging from  $r = .72$  to  $r = 1.0$ , with a mean  $r = .86$ ), and the scores of the two coders were again averaged to provide single ratings for each dimension.

The *danger of providing insufficient aid* dimension and the *conception of poverty* dimension were reverse coded so that higher values for each of the four moral connotation items signified more emphasis on individualism relative to humanitarianism. A factor analysis confirmed that all four dimensions loaded on a single factor (with eigenvalues ranging from .78 to .91). The items were subsequently averaged into a composite index of individualism (Cronbach's  $\alpha = .85$ ).<sup>6</sup> Evidence of its convergent validity was provided by a significant correlation with participant liberalism (as assessed by averaging the party affiliation and political ideology scores obtained for each participant in mass testing 3–6 weeks prior to participation in the study). Even within the restricted sample of Democrats used in this study, participants who had endorsed liberalism more wrote editorials that appealed to individualism less,  $r(48) = -.41$ ,  $p < .005$ —a

finding consistent with past research showing that liberals value individualism less than do conservatives (Skitka et al., 2002).

## Results

### Attitude

Participants supported the program more in the no-group-information condition ( $M = 4.97$ ) than in the Democrats oppose condition ( $M = 3.67$ ),  $F(1, 45) = 12.52$ ,  $p = .001$ .

### Behavioral Support

The majority of participants in the no-group-information condition wrote editorials in favor of the program (76% did so), whereas the majority of participants in the Democrats oppose condition wrote editorials opposed to that program (71% did so),  $\chi^2(1, N = 49) = 10.78$ ,  $p < .005$ .

### Perceived Social Meaning

As predicted, participants rated the program as less compatible with the values of their group (i.e., more conservative) in the Democrats oppose condition ( $M = 5.00$ ) than in the no-group-information condition ( $M = 3.80$ ),  $F(1, 45) = 15.27$ ,  $p < .001$ .

### Measures of Cognitive Processing

*Cognitive responses and recall.* As in Study 3, group information did not elicit heuristic processing. Participants in the Democrats oppose condition and in the no-group-information condition generated an equal number of thoughts about the program ( $M_s = 7.71$  and  $7.51$ , respectively), and recalled an equal number of program details ( $M_s = 10.67$ ,  $10.13$ ),  $F_s < 1$ . Once again, thought valence (i.e., the number of positive minus negative thoughts about the program) correlated with attitude positivity, yielding further evidence that attitudes were cognitively elaborated,  $r(48) = .67$ ,  $p < .001$ . If anything, attitudes were more elaborated in the Democrats oppose condition,  $r(23) = .74$ ,  $p < .001$ , than in the no-group-information condition,  $r(24) = .44$ ,  $p < .03$ .

In contrast to Study 3, Study 4 yielded clear evidence that group information biased the valence (or direction) of systematic processing. Overall, thought valence was more negative in the Democrats oppose condition ( $M = -1.50$ ) than in the no-group-information condition ( $M = 0.75$ ),  $F(1, 45) = 7.03$ ,  $p = .01$ . Moreover, thought valence constituted a significant covariate in the analysis involving attitude,  $F(1, 44) = 28.20$ ,  $p < .001$ . While controlling for thought valence did not eliminate the effect of group information on attitude, it did reduce that effect to  $F(1, 44) = 4.91$ ,  $p = .03$ . The modified Sobel test (Baron & Kenny,

<sup>6</sup> The composite index used in between-condition significance tests was computed after each dimension was first standardized to equate its variance with the other dimensions. Because the composite index was positively skewed, a square-root transformation was also performed (after a value of +5 was added to each score to avoid zero values). For ease of interpretation, however, the reported means are derived from an untransformed composite of scores along the original (unstandardized) dimensions. Analyses using this latter composite yield the same (i.e., statistically significant) basic result.

1986) confirmed that this reduction was significant,  $Z = -2.34$ ,  $p < .02$ . Biased systematic processing accounted for almost 50% of the effect of reference group information on attitude.

However, group information did not bias the valence of processing so much as it defined the very program content to be processed (cf. Asch, 1948, 1952). The number of thoughts referring to stringent program qualities was subtracted from the number of thoughts referring to generous program qualities. As predicted, the assumed factual qualities of the program were less generous in the Democrats oppose condition ( $M = -0.40$ ) than in the no-group-information condition ( $M = 0.20$ ),  $F(1, 45) = 4.92$ ,  $p = .03$ . A secondary finding indicates that group information affected not only the interpretation of ambiguous program content, but the salience of unambiguous program content. While participants in both conditions assigned a negative valence to the mandatory component ( $M = -0.91$ ), the mean number of thoughts about this component was higher in the Democrats oppose condition ( $M = 0.94$ ) than in the no-group-information condition ( $M = 0.53$ ),  $F(1, 45) = 3.97$ ,  $p = .05$ .

Evidence of a pure shift in evaluation would be provided if individuals who thought about an unambiguous program feature were found to evaluate that feature more negatively in the Democrats oppose condition than in the no-group-information condition. Analyses were thus undertaken to assess the effect of group information on thought valence toward each of the three specific program features, with each analysis confined to participants who had generated at least one thought about the relevant feature. With one exception, no effects emerged,  $F_s < 2$ . The exception was that participants evaluated the tax incentive more negatively in the Democrats oppose condition ( $M = -0.11$ ) than in the no-group-information condition ( $M = 0.70$ ),  $F(1, 27) = 6.22$ ,  $p < .02$ .

*Written editorials.* Analyses of the written editorials reinforce the claim that group information did not bias processing as much as it defined the object to be processed. The number of references to stringent program features was subtracted from the number of references to generous program features. As predicted, the editorials presented the program as less generous in the Democrats oppose condition ( $M = -0.35$ ) than in the no-group-information condition ( $M = 0.55$ ),  $F(1, 45) = 5.13$ ,  $p < .03$ . On balance, participants envisioned a stringent program more than three times as often in the Democrats oppose condition (54% did so) than in the no-group-information condition (16% did so),  $\chi^2(1, N = 49) = 7.87$ ,  $p < .01$ . With one exception, there were no significant effects involving either the frequency or the valence of references to the three unambiguous program features,  $F_s < 2$ . The exception was that marginally more negative references to the mandatory feature were made in the Democrats oppose condition ( $M = 0.94$ ) than in the no-group-information condition ( $M = 0.53$ ),  $F(1, 45) = 2.93$ ,  $p < .10$ .<sup>7</sup>

While the results thus far indicate that reference group information changed the perceived factual qualities of the object of judgment, the next set of results indicate that it also changed its perceived moral connotations. An analysis of covariance (ANCOVA) was performed on the individualism scores assigned to the editorials, using participant liberalism (assessed in mass testing) as a covariate. The analysis yielded a significant effect of condition,  $F(1, 45) = 4.95$ ,  $p = .03$ . In the no-group-information condition, liberals struck a balance between humanitarianism and individualism; the mean score (adjusted  $M = 2.68$ ) did not differ

from the scale midpoint (1 = *extremely humanitarian*, 5 = *extremely individualistic*),  $t(44) = -1.64$ ,  $p > .10$ . By contrast, the mean score in the Democrats oppose condition favored humanitarianism over individualism (adjusted  $M = 2.09$ ),  $t(44) = -4.57$ ,  $p < .001$ .

*Overall statistical test of mediation.* When an ANCOVA was performed on attitude using social meaning, biased processing, and emphasis on individualism (with participant liberalism statistically partialled out), all three proved significant covariates,  $F(1, 42) = 17.91$ ,  $p < .001$ ;  $F(1, 42) = 16.04$ ,  $p < .001$ ;  $F(1, 42) = 13.54$ ,  $p = .001$ , respectively; and the effect of reference group information was reduced to nil,  $F < .01$ .<sup>8</sup>

### *Perceived Causal Bases of Attitudes for Self*

Once again, participants claimed that the details of the program ( $M = 5.58$ ) and their own philosophy of government ( $M = 5.10$ ) contributed to their attitude most, and that the stated position of party lawmakers contributed least ( $M = 3.43$ ), paired  $t(48) = 7.44$ ,  $p < .001$ ;  $t(48) = 5.03$ ,  $p < .001$ , respectively. They viewed the position of party lawmakers as no more important than their experience with people on welfare ( $M = 3.47$ ),  $t < 1$ . There were two other noteworthy results. First, the position of lawmakers was rated as no more influential in the group information condition ( $M = 3.69$ ) than in the no-group-information condition ( $M = 3.17$ ),  $F < 1$ , again suggesting little if any awareness of group influence. Second, program details were rated as more influential in the group information condition ( $M = 6.00$ ) than in the no-

<sup>7</sup> It seemed possible that group information affected attitudes by causing participants to focus on the mandatory component and to use it as the primary rationale for construing the program as stringent in nature. Contrary to this hypothesis, however, controlling for the number of thoughts directed at the mandatory component did not reduce the effect of group information on attitude; indeed, it did not constitute a significant covariate in this analysis,  $F < 1$ . Moreover, group information exerted a stronger effect among participants who had no thoughts about the mandatory component,  $t(45) = 3.79$ ,  $p < .01$ , than among participants who did,  $t(45) = 1.42$ ,  $p < .18$ . Likewise, controlling for the number of negative editorial references to the mandatory component left intact the main effect of group information,  $F(1, 44) = 8.88$ ,  $p = .005$ ; and the effect of group information did not vary as a function of whether the editorial mentioned the mandatory component or not,  $F < 1$ . These analyses indicate that attention to the mandatory component was neither necessary nor sufficient to produce the effect of group information.

<sup>8</sup> It seemed possible that the mechanisms mediating the effect of reference group information might vary as a function of individuals' knowledge of welfare. Party judgments might serve as a heuristic cue for participants low in knowledge but bias processing for participants high in knowledge (see Fleming & Petty, 2000). However, the results yielded no support for this conjecture. As in Study 2, knowledge (assessed in mass testing) did not moderate the effect of group information on attitude,  $F < 1$ , suggesting that low-knowledge and high-knowledge participants conformed to comparable degrees. In addition, knowledge moderated neither the effect of group information on the three mediators, all  $F_s < 1$ , nor the relationship between these mediators and attitude, all  $F_s < 2$ . It seems likely that participants in all studies undertook high effort processing regardless of their level of knowledge because they believed that their memory would be tested in Studies 1–3 and that their responses would inform policy decision makers in Study 4. Accordingly, they had little opportunity to use party judgments as a mere heuristic cue (cf. Maheswaran & Chaiken, 1991).

group-information condition ( $M = 5.19$ ),  $F(1, 45) = 8.69$ ,  $p = .005$ . Participants thus asserted that they had based their attitude on an autonomous and rational consideration of the facts, and, if anything, they made this assertion more rather than less in the group information condition.

### *Perceived Causal Bases of Attitudes of Others*

As predicted, participants viewed their peers as more conformist and less rational than themselves. They claimed that the position of party lawmakers had a far greater effect on Democratic allies ( $M = 5.24$ ) than on themselves ( $M = 3.43$ ), paired  $t(48) = 6.40$ ,  $p < .001$ ; and that it had an even larger effect on Republican adversaries ( $M = 5.61$ ) than on Democratic allies, paired  $t(48) = 2.71$ ,  $p < .01$ . Participants also asserted that the details of the program contributed both to the attitude of Democrats ( $M = 5.02$ ) and to the attitude of Republicans ( $M = 4.78$ ) less than it contributed to their own attitude ( $M = 5.58$ ), paired  $t(48) = -2.83$ ,  $p < .01$ ;  $t(48) = -4.11$ ,  $p < .001$ . Furthermore, they claimed that one's "background with people on welfare" informed the attitudes of Democratic allies marginally less ( $M = 2.98$ ) than their own attitude ( $M = 3.47$ ), paired  $t(47) = -1.84$ ,  $p = .07$ , and that it informed the attitudes of Republican adversaries even less ( $M = 2.39$ ) than those of Democratic allies,  $t(47) = -3.30$ ,  $p = .002$ . In short, participants ascribed their own beliefs to relevant facts and real-world experience, but they ascribed the beliefs of their allies, and especially those of their adversaries, to group influence. More generally, participants felt that their own beliefs were more "objective" ( $M = 4.39$ ) than those of fellow Democrats ( $M = 3.74$ ),  $t(43) = 3.21$ ,  $p < .005$ , and that the beliefs of Democrats were more objective than those of their Republican rivals ( $M = 3.37$ ),  $t(48) = 1.98$ ,  $p = .05$ .

### *Discussion*

Seventy-six percent of students in the no-group-information condition responded to the program by submitting positive editorials to a policy institute. But if they were informed that their party opposed that program, almost the same percentage of students submitted negative editorials, even though program content—and program alternatives—were held constant across the two conditions. Moreover, although participants were unaware of the effect of group information on themselves, they readily perceived its effect on their political adversaries and even on their potential allies (see also Kenworthy & Miller, 2002; Robinson et al., 1995; cf. Miller & Ratner, 1998).

The cognitive processing measures yield two major conclusions. First, the effect of group information on attitude was mediated, in part, by biased systematic processing. Second, this biased processing did not so much skew evaluations of an invariant object, but rather redefined the very object to be evaluated (Asch, 1948, 1952). Analyses of the written editorials further supported this claim. They also indicated that group information affected not only the assumed factual qualities of the object, but its perceived moral connotations. Participants in the no-group-information condition highlighted the value of personal responsibility as much as the value of humanitarianism, and they presumably saw support of the program as an expression of those twin moral commitments. By contrast, participants in the Democrats oppose condition high-

lighted personal responsibility less than humanitarianism, and they evidently saw opposition as logically following from the overriding importance of direct humanitarian relief.

As in Study 3, group information did not discourage systematic, central-route processing. The number of thoughts, and the amount recalled, did not differ as a function of condition. Likewise, the cognitive elaboration of attitudes (i.e., the correlation between thought positivity and attitude positivity) did not decrease in the Democrats oppose condition; if anything, it increased (see also Alvaro & Crano, 1996). One inconsistency between the results obtained in the present study and those obtained in Study 3, however, is noteworthy. Although biased systematic processing did not mediate the effect of group information in Study 3, it partly mediated that effect in Study 4. It seems likely that providing a thorough and accurate account of thoughts in response to two attitude objects (as participants did in Study 3) is more difficult than providing such an account in response to one attitude object (as participants did in Study 4). The shorter amount of time allotted to complete the relevant measure in Study 3 may have exacerbated this problem.

### *General Discussion*

Four studies demonstrated the impact of group influence on attitude change. If information about the position of their party was absent, liberal and conservative undergraduates based their attitude on the objective content of the policy and its merit in light of long-held ideological beliefs. If information about the position of their party was available, however, participants assumed that position as their own regardless of the content of the policy. The effect of group information was evident not only on attitude, but on behavior (Study 4). It was as apparent among participants who were knowledgeable about welfare as it was among participants who were not (Study 2). Important alternative explanations for the obtained results, such as effects of heuristic processing and shifts in scale perspective, were ruled out (Studies 3 and 4).

### *Considerations of Underlying Process*

Attitude change did not result from mindless conformity. No evidence was found that message scrutiny or attitude elaboration was lower in the group information condition than in the no-group-information condition. In absolute terms, scores along many of the depth-of-processing indices proved high. That participants in the no-group-information condition based their attitudes on message content attests further to the effortful nature of their processing (Petty & Wegener, 1998). Participants in some of the group information conditions, moreover, might have expended additional cognitive effort to figure out why the actual position of their party differed from the expected one (Maheswaran & Chaiken, 1991).

Groups affect attitudes by shaping perceptions of objects in the social world. Study 4 illustrated this process. Participants who were presented with Democratic opposition to a policy defined that policy as "not liberal," and they perceived the object in a manner consistent with its assigned social meaning. While individuals in the no-group-information condition thus envisioned a job training program that would help poor people to "find employment and support themselves," many participants in the Democrats oppose condition called to mind a program that would "dump beneficiaries

into menial labor." Not only did they ascribe different factual qualities to the object of judgment (see Asch, 1952; Ichheiser, 1970), but they also highlighted different moral qualities. In the no-group-information condition, the job training program stood for a commitment both to personal responsibility and to humanitarian relief. Indeed, the cultivation of personal responsibility may have been viewed as a means to achieving a humanitarian end (see Kristiansen & Zanna, 1994). Support of the program followed naturally from its fulfillment of both moral goals. By contrast, in the Democrats oppose condition, the same program represented the abandonment of humanitarian ideals for rugged individualism. Conservatives in Studies 1 and 3 may have overcome their resistance to a generous but Republican-supported welfare policy through a similar process. By viewing humanitarian relief as a strategy to foster self-reliance, they could support a program that they would have otherwise rejected. In this sense, group influence redefines the very relevance of socially shared values and the methods appropriate to achieve them.

### *Relevance to Dual-Process Models of Persuasion*

Both the heuristic-systematic model of persuasion and the elaboration likelihood model of persuasion informed the analysis of attitude change presented here. As these models suggest, people may undertake biased "systematic processing" or "message elaboration," evaluating the object in a manner congenial to the judgments of their group (Chaiken & Maheswaran, 1994; Chen et al., 1999; Fleming & Petty, 2000; see also Lord et al., 1979). Consistent with this claim, Study 4 found that biased processing accounted for approximately half the effect of group influence on attitudes. However, group influence did not bias information processing as much as it posited the very information to be processed.

### *Implications for Self-Deception*

The results suggest that self-deception can arise from the failure to apply a valid social theory to oneself (see also Epley & Dunning, 2000; Nisbett & Wilson, 1977). Participants in the present research realized the power of group influence in a general sense; thus, they could accurately estimate its impact on the attitudes of others. In assessing the causes of their own attitudes, however, they found reason to believe that they were exempt from its effect (see Pronin et al., 2002). In cases where people are aware of a general social process but perceive themselves to be immune to it, they may paradoxically have better insight into the causes of others' behavior than into the causes of their own (see Epley & Dunning, 2000).

### *Clarifications, Limitations, and Unanswered Questions*

The present studies were not intended to suggest that group influence always predominates over message content. After all, the impact of each variable depends not only on its conceptual significance but on the manner in which it is operationalized. Clearly, with the right procedures, one could show that group influence failed to override message content. For example, liberals might resist Democratic support of a stringent program that cut all state and federal assistance. This criticism is relevant to the present research to the extent that the manipulation of message content lacked magnitude or salience relative to the manipulation of group influence.

By statistical criteria, however, the message content manipulation lacked neither magnitude nor salience. Studies 2 and 3 demonstrate that the magnitude of the effect of message content in the no-group-information conditions was as large as the independent effect of reference group information. Group information did not override the effect of message content by decreasing its psychological salience. As the various depth-of-processing measures indicate, processing of message content was equally attentive regardless of whether group information was available or not. Judged by relevant real-world criteria, moreover, the manipulation of policy content was large. As previously noted, the two policies were more extreme than the most generous policy and the most stringent one available in much of the United States. By contrast, the manipulation of reference group information was arguably small. It consisted only of a few references to the position of Democrats and Republicans, and it was not atypical of political discourse in the real world. Studies can be informative of the relative power of independent variables, if the effect of a manipulation that is small by real-world standards is shown to override the effect of a manipulation that is large by real-world standards (see Ross & Nisbett, 1991).

Three limitations in the present research are noteworthy. First, the manipulation of group information confounded the judgments of the ingroup with those of the outgroup. Whether the results reflect conformity to the ingroup position, or "reactive devaluation" of the outgroup position (Ross & Ward, 1995), thus remains a question for future research. However, given that political identification tends to be dichotomized in American culture (Brady & Sniderman, 1985), knowledge of one party's position may imply knowledge of the other. Second, the studies concerned only political decision making. To the extent that political attitudes are symbolic in nature (Sears & Funk, 1991), they may be especially responsive to social influence. This problem is balanced by the recognition that arguably few decision domains are as important as the political. A third limitation concerns the participant sample. Although an effort was made to select students who cared about politics in general and welfare in particular, the reliance on college undergraduates could limit the generality of the results. On the one hand, college students' political beliefs may be more malleable than those of older adults (see Sears, 1986), artificially inflating the reference group effects found here. On the other hand, college students' group loyalties may be less crystallized than those of older adults (again see Sears, 1986), artificially depressing the reference group effects found here. In short, it would be desirable to attempt to replicate these results with a more diverse sample and in different decision domains.

One unresolved question concerns whether a reference group less relevant to the decision domain could produce attitude change. For example, would the judgments of one's classmates affect attitudes toward a welfare policy? Although past research finds that such issue-irrelevant groups can produce attitude change (e.g., Asch, 1948; Griffin & Buehler, 1993; Mackie et al., 1990; Wood et al., 1996), whether they produce as much attitude change as issue-relevant ones remains unclear. On the one hand, issue-irrelevant groups might have smaller effects because the values motivating their judgments may be unclear. While participants in the present research could attribute the judgments of lawmakers to shared ideological values, they may have found the judgments of classmates on the same topic difficult to explain. On the other

hand, however, issue-irrelevant groups might produce large attitude change effects through processes beyond the ones considered here. For example, they may elicit heuristic processing, and thus produce conformity of a more mindless nature (Mackie et al., 1990, 1992). While future research will help to resolve these issues, it should be borne in mind that people consult with groups relevant rather than irrelevant to the decisions they face (Katz, 1957).

A final question concerns the role of dissonance reduction processes in mediating group influence. In certain experimental conditions in the present research, participants may have experienced dissonance between the cognition "I approve of this policy" and the cognition "My group opposes it." To reduce dissonance, they could either bring their attitude in line with that of their group or reduce the psychological importance of their group. Insofar as people invest effort and identity in a group membership, however, its psychological importance may be relatively fixed. Attitude change may constitute a less painful route to dissonance reduction. Investigators could test this account by measuring dissonance arousal (Elliot & Devine, 1994) and statistically assessing its role in mediating group influence. On the other hand, they could test whether the effect of group influence on attitudes is attenuated by manipulations known to forestall dissonance-motivated rationalization, such as the opportunity either to misattribute dissonance arousal to a decision-irrelevant cause (Zanna & Cooper, 1974) or to affirm an alternative source of self-identity (Steele, 1988; see also Cohen et al., 2000; Sherman & Cohen, 2002).

Beyond these conceptual issues, practical concerns are also raised by the present research. The process of attitude formation and change described here can exacerbate intergroup conflict. Once an issue represents socially shared values, it constitutes "a point on which people do not so much form opinions as choose sides" (Ellsworth & Gross, 1994, p. 23). To the extent, moreover, that people remain blind to group influence on themselves, they may feel that they alone have based their beliefs on a rational assessment of the facts, while their adversaries, and even their allies, are biased. The transmission and maintenance of social meaning is thus central not only to persuasion, but also to social dispute.

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