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# Forewarned and Forearmed? Two Meta-Analytic Syntheses of Forewarnings of Influence Appeals

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Two research syntheses evaluate the effects on attitudes of forewarning of an influence appeal. In general, warnings appeared to threaten people's attitudes or their self-images, and warning impact depended on which aspect of the self was threatened. When the topic of the appeal was involving and concerned immediate outcomes or when the appeal was actually delivered, recipients appeared to focus on the potential threat to their attitudes, and they responded defensively by cognitively bolstering their own views and resisting the appeal. However, warnings of appeals on less involving topics generated agreement before the appeal was delivered, presumably because these warnings alerted people to the self-image threat of being gullible and preemptive agreement reduced this threat.

The familiar aphorism, "forewarned is forearmed," suggests that in daily life, warnings of impending influence appeals enable people to marshal their defenses and resist the influence attempt. A familiar example is the inappropriately friendly greeting of a telephone solicitor, which can warn listeners of the caller's intent and thereby elicit a wary response to the subsequent sales pitch. The standard cover story in laboratory experiments on attitude change also assumes that warnings generate resistance. Experimental participants typically are not warned of the impending appeal, in part to maximize their attitude change to the message (Papageorgis, 1967, 1968). The idea that warnings yield resistance gains further credence from their use in public health campaigns, especially in interventions that prepare adolescents to "say no" to pressure from media and peers to use drugs, cigarettes, and alcohol and to engage in sexual activity (e.g., Botvin & Kantor, 2000; Bruvold, 1993).

Psychologists have adopted a similar perspective on warnings by considering forewarning research in relation to other factors that instigate resistance to influence (e.g., Eagly & Chaiken, 1993). Research on resistance is often traced to McGuire's (1964) classic work on inoculation, in which initial exposure to weak forms of a counterattitudinal message inoculated people in the sense that they were better able to resist subsequent appeals (see Pfau's, 1996, review). Interest in the mechanisms underlying resistance is also apparent in research on attitude strength and resilience (e.g., Bassili, 1996; Petty & Krosnick, 1995), on attitudinal selectivity in processing of persuasive appeals (Eagly, Chen, Chaiken, & Shaw-Barnes, 1999), and on training procedures to produce discriminating mindsets in message recipients (Cialdini, Sagarin, & Rice, 2001). An understanding of forewarning effects is potentially an important addition to these investigations of resistance.

The research paradigms used to study forewarning provide a rich context in which to examine not only resistance processes but also acceptance of influence. The threat presented by a warning has the potential to elicit a range of social motives, and these in turn might generate a variety of attitude outcomes. Defensive resistance is likely when a warning threatens people's existing attitudes and suggests that the upcoming appeal will challenge their views (Petty & Wegener, 1998). However, warnings also can threaten people's self-concepts and the impressions they wish to convey to others, and these kinds of threats can have surprising effects on attitudes. In particular, warnings of a highly persuasive appeal may generate preemptive agreement—that is, agreement prior to delivery of the appeal—in order to reduce the self-esteem threat of eventually being influenced (McGuire & Millman, 1965). Alternatively, warnings that sensitize people to the impressions conveyed to others may elicit attitudinal moderation when people wish to appear flexible and open minded (Cialdini & Petty, 1981). Thus, forewarnings potentially yield resistance or acquiescence depending on whether they motivate people to protect existing attitudes, to defend their self-images by avoiding being a patsy, or to create a favorable impression by holding moderate views.

In the present article, we examine forewarning impact on attitudes through two meta-analytic syntheses of prior forewarning research. To interpret warning impact, we consider people's motives when responding to warnings along with their thoughts about the issue in the impending appeal. The studies we reviewed delivered a warning of a counterattitudinal appeal and then assessed recipients' attitudes either before they received the appeal (Synthesis 1) or after the appeal (Synthesis 2). Together, the two syntheses illustrate the time course of warning impact.

## Prior Reviews of Forewarning Effects

Past narrative reviews of the experimental literature have reported that warnings have a variety of effects. McGuire (1969b)

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characterized the effects of forewarning of persuasive intent on attitudes as "frustratingly elusive" (p. 35). He concluded that "there does seem to be a relationship begging to be found, and yet it seems to be hiding out in only certain cells of our experimental design" (p. 35). Cialdini and Petty (1981) differentiated between types of warnings and concluded that forewarnings that convey just the *persuasive intent* of an appeal typically generate resistance, whereas forewarnings that specify the *message topic and stance* can generate resistance or susceptibility. Eagly and Chaiken (1993) simply noted that these two types of warnings have widely varying effects, depending on specific features of the warnings. In a discussion of how warnings can confer resistance, Petty and Wegener (1998) outlined how warnings of intent as well as warnings of topic and stance can negatively bias people's thoughts about an issue and an appeal.

In an earlier meta-analytic synthesis, Benoit (1998) examined the effects of warnings on postmessage attitudes and concluded that warnings generate resistance. In the 12 studies he reviewed, participants who were warned prior to the receipt of a message were less persuaded than participants who received the message without warning (mean $r = .18$). Furthermore, Benoit reported that warning-induced resistance was uniform across a variety of potential moderating variables.

The present syntheses expand the set of studies from Benoit's (1998) review and analyze warning impact in research that assessed attitudes either before or after receipt of an appeal. Attitudes measured prior to the appeal reveal how simply expecting an appeal influences judgments, whereas attitudes measured after the appeal reveal how initially expecting an appeal affects its persuasiveness. Prior reviews have given little reason to suppose that warning impact varies with the appeal delivery. For example, Cialdini and Petty (1981) claimed that the direction of forewarning impact "cannot be explained through an examination of whether . . . effects have been assessed in terms of pre- or postcommunication attitude change" (p. 221). Thus, we initially had few expectations that timing of assessment would influence warning impact.

## Motives Instigated by Forewarnings

Warnings are likely to generate a defensive orientation when they threaten some aspect of people's identity, their freedom, or their existing attitudes. Defensively motivated recipients evaluate the available information, including their own attitudes and beliefs, the warning, the context, the source, and the message if available, to express an attitude judgment that best meets their defensive needs (Chaiken, Giner-Sorolla, & Chen, 1996).

Allyn and Festinger's (1961) early study provided initial support for the idea that warnings induce defensive resistance. They reasoned from the cognitive dissonance notion that counterattitudinal appeals elicit dissonance and proposed that recipients warned of such appeals experience "hesitancy, suspicion, and perhaps some hostility" (p. 36). Rejection of the appeal was thought to be the easiest, most direct route to reduce the dissonance aroused by a warning.

Warnings that threaten people's existing attitudes and beliefs can generate resistance through a variety of processes. In particular, warnings can bias systematic or central processing so that people generate thoughts that support their existing views and counter the position to be advocated (Chaiken et al., 1996; Cialdini & Petty, 1981). Forewarnings are especially likely to yield resistance through biased processing when the topic is involving in the sense that it is linked to immediately important outcomes. This kind of involvement has been referred to as *outcome-relevant involvement* or as *personal relevance* (Johnson & Eagly, 1989; Petty & Wegener, 1998). On involving issues, people are already motivated to think carefully, and warnings can direct this thought to countering the impending position (Petty & Wegener, 1998). Indeed, when warned of counterattitudinal appeals on involving topics, people have been found to generate thoughts opposing the appeal and bolstering their own views, both prior to receipt of the message (Brock, 1967; Cialdini, Levy, Herman, Kozlowski, & Petty, 1976; Petty & Cacioppo, 1977) and following message presentation (Romero, Agnew, & Insko, 1996).

Defensive resistance also can emerge from minimal issue-relevant thought. In a reactance theory account, warning of a communicator's persuasive intent (e.g., "This message is designed to induce you to change your attitudes") generates resistance because it threatens recipients' freedom to hold a particular attitude position (Brehm, 1966). Recipients' rejection of the impending appeal to reestablish their personal freedom is thought to represent a "*topic-independent* resistive state" (Hass & Grady, 1975, p. 467) that does not require knowledge of or thought about the specific topic in the message. In a demonstration that warnings can generate reactance, Fukada (1986) reported that participants warned of persuasive intent expressed greater concern, at least prior to an appeal, about having their opinions manipulated than did nonwarned participants.

Warnings not only pose threats to existing attitudes and to one's freedom to hold them, they also can threaten people's identities. According to McGuire and Millman (1965), warnings of impending social pressure can make people feel vulnerable and potentially gullible. One way to retain a favorable self-view is to shift toward the appeal prior to delivery and thereby to minimize the apparent impact of the message. Although McGuire and Millman recognized that preemptive agreement can both defend the self-concept and, when broadcast publicly, secure others' favorable impressions, they emphasized self-related motives for agreement such as maximizing self-esteem and minimizing discrepancies between real and ideal selves. In this spirit, we interpret such agreement as a defensive maneuver intended to protect the self against being gullible. This preemptive agreement may vary with the topic of the appeal (McGuire, 1969a). With warnings on objective topics, recipients are unlikely to show defensively motivated anticipatory change because they can interpret influence as openmindedness and a response to facts. In this self-esteem account, then, warning-induced defensiveness elicits anticipatory agreement with the impending appeal, especially on subjective topics that involve personal preferences.

In an alternative explanation for anticipatory attitude shifts, Cialdini and his colleagues (e.g., Cialdini, Levy, Herman, & Evenbeck, 1973; Cialdini et al., 1976; see also Hass, 1975) proposed that recipients are moderating to midscale positions rather than agreeing with the impending appeal. Because warnings in this research literature typically involve counterattitudinal appeals, movement of attitudes toward midscale is in the same direction as movement toward the source's position. Thus, shifts toward the message can reflect moderation instead of agreement. These mod-

eration shifts are thought to emerge because warnings threaten recipients' public images. Shifts toward midscale convey impressions of flexibility and broad-mindedness and enable recipients to hold positions that appear reasonable and justified even to others holding an opposing position. In this analysis, then, moderation shifts are motivated by the desire to convey particular impressions to interaction partners and to achieve immediate social outcomes. Some support for this idea emerged in Johnson and Eagly's (1989) synthesis of five studies in which participants expected to state their attitudes to others with unknown positions. These researchers concluded that the impression motives instigated by such expectations yielded attitudinal moderation.

Theories of anticipatory attitude shifts typically assume that such shifts are relevant to transitory interactions with unknown others and for this reason are likely to be of short duration and to be context dependent. Thus, recipients' thinking is likely to be focused on how to manage the threat to their self-image or to achieve the desired impression rather than on enduring factors such as the attitude issue, an enduring identity, or an extended social exchange. Consistent with this view of attitude movement as strategic and context dependent, attitude judgments prior to an appeal appear to be elastic, and attitudes have been found to revert back to their initial position if the impending appeal is canceled (Cialdini et al., 1973; Cialdini et al., 1976). Similarly, anticipatory agreement to a warning does not appear to maintain when the appeal is delivered and thus has little impact on the appeal's eventual persuasiveness (McGuire & Millman, 1965).

In summary, warning-induced resistance has been explained as a defensive response to protect existing attitudes or personal freedom, whereas anticipatory change has been explained as preemptive agreement in defense of a favorable self-view or as moderation to convey desired impressions. Although the idea that warnings instigate defensiveness has the advantage of parsimoniously accounting for a range of attitudinal responses, it remains plausible that warning-induced concerns with public impressions prior to an appeal generate moderation.

## The Present Research

The present meta-analytic synthesis provides two tests of the various explanations for forewarning effects. Some studies assessed attitudes following the warning but before the appeal. To estimate forewarning impact in this preappeal paradigm, we compared participants' anticipatory attitudes with those of control participants who did not receive a warning. Other studies warned participants of an impending appeal, delivered the appeal, and then assessed their attitudes. To estimate forewarning impact in this postappeal paradigm, we compared participants' postappeal attitudes with their preappeal judgments or with the judgments of nonwarned control participants.

We used the aggregated study outcomes within each paradigm to estimate the magnitude and direction of warning effects. In addition, to evaluate the varying theoretical accounts of warning impact, we compared the predictions of each perspective against the patterning of attitude change across the studies in our review. This revealed how adequately each theory could account for the conditions under which forewarnings generated acceptance or resistance.

First, we considered how warning-induced resistance is likely to vary with warnings that specified an intent to persuade and warnings that specified the attitude topic and position to be advocated. According to reactance theory, warning-induced resistance arises from perceived threats to freedom, which may emerge with a variety of warnings but should be most pronounced with warnings that specify intentions to persuade. In addition, cognitive dissonance theory can account for resistance to a variety of types of warnings, depending on the aspect of the self that is threatened by the appeal (e.g., personal freedom, existing attitudes). In the biased thinking account, warnings may have different effects before versus after delivery of the appeal. Prior to the appeal, warnings on involving topics that specify the message position can generate cognitive bolstering and resistance. However, prior to the appeal, warnings of persuasive intent are likely to have little impact because the message topic and position have not been specified. In contrast, following the appeal, warnings on involving topics can generate resistance by specifying either the position to be advocated or persuasive intent because both types of warnings instigate biased processing of message content (Cialdini & Petty, 1981). Greater resistance with involving issues also can be explained through reactance and cognitive dissonance theories, if it is assumed that these motives emerge most strongly with threats on such topics (Cialdini & Petty, 1981).

The studies we reviewed also evaluated the informational processes underlying warning effects by varying the extent to which people were motivated or able to engage in thought about the impending appeal. These experimental variations are especially appropriate to test the biased thinking account, in which warning impact emerges from cognitive bolstering of one's own position and counterarguing of the appeal. To the extent that warning impact depends on systematic thought, warning effects should be enhanced by factors that facilitate thought production, such as instructions to list one's thoughts, and should be impaired by factors that limit thoughtful analysis, including insufficient time and distraction. However, Benoit's (1998) meta-analytic synthesis revealed that warning impact was unaffected by a time delay between the warning and the message.

We also considered how well the theories that predict warning-induced anticipatory shifts could account for the pattern of attitude change (Cialdini et al., 1976; McGuire & Millman, 1965). Anticipatory shifts toward the appeal and midscale should be found when warnings threaten people's self-esteem by making them feel gullible or raise concerns about others' impressions. These conditions are likely to hold when warnings suggest that an appeal will be difficult to resist, either because it is highly persuasive or because the source is an expert. We also anticipated that, because self-esteem and impression motives are context specific, anticipatory shifts generated by these factors would be unlikely to persist when contexts change, such as when the appeal is canceled or when the message is delivered.

To evaluate whether anticipatory change reflected self-esteem-based agreement or impression-based moderation, we considered the information provided in the warning. Agreement would be indicated if attitudes shifted only when warnings specified the source's position, because knowledge of this position is needed to agree. Warnings could give the topic of the appeal and specify a counterattitudinal position or imply this position through an intent to persuade. Alternatively, because moderation shifts to midscale

do not require knowledge of the source's position, moderation would be indicated if attitudes also changed when warnings specified only the topic and did not imply a position. To gain additional insight into whether change reflects agreement or moderation, we examined whether attitude shifts emerged primarily when participants expected to interact with the message source or a discussion partner. Impression-relevant concerns of conveying flexibility and moderation to others would be suggested if attitudes shifted primarily when these others had surveillance, because with surveillance comes social costs and benefits of attitude expressions.

Finally, we were able to evaluate two additional explanations for warning-induced anticipatory shifts to the message and midscale. If these shifts reflect conformity due to the simple knowledge that others hold dissenting views, then they should emerge in control conditions in which participants were told about an appeal but did not expect to receive it (although see Hass & Mann, 1976). On the other hand, such shifts might reflect implicit coordination between interaction partners, similar to the automatic coordination of bodily movements and speech styles that apparently smoothes interaction among conversation partners (Dijksterhuis, 2001; Dijksterhuis & Bargh, 2001). If so, such shifts should be stronger when people expected to interact with the source or discussion partner, and thus could coordinate responses, than when they expected to indicate their attitudes privately on questionnaires.

## Method

### *Identification of Articles in Sample*

Articles were identified through computerized database searches of PsycINFO (1964–1999), ERIC (1993–1999), *Dissertation Abstracts International* (1861–1999), and WorldCat (1985–1999) using one of the key words *anticipatory*, *warn*, *warning*, *forewarn*, *forewarning*, or *intent* in conjunction with one of the key words *persuade*, *persuasion*, *persuasive*, *attitude change*, *influence*, *opinion change*, *belief change*, *advocated position*, or *elastic shift*. Studies also were located from the reference lists of earlier reviews on forewarning (e.g., Benoit, 1998; Cialdini & Petty, 1981; McGuire, 1964, 1969a, 1969b; Petty & Wegener, 1998) and from the reference lists of articles included in the sample.

### *Sample Selection Criteria*

To provide a sample of studies appropriate to test our hypotheses about the defense and impression motives induced by forewarnings, we included studies that (a) delivered a warning of an impending communication (within 15 min); (b) obtained comparison-group attitudes, either from a nonforewarned control group or from a preforewarning assessment of participants' attitudes; and (c) assessed attitudes either after the warning (for the premessage sample of studies) or after receipt of the message (for the postmessage sample of studies).

These selection criteria necessitated the exclusion of a number of research paradigms. That is, because we were interested in the effects of warnings of impending appeals, we excluded studies that did not threaten participants with a specific appeal, including research on group decision (e.g., Green, 1984), studies of impression-relevant or response involvement, in which participants expected to state their positions to an audience with unknown views (see Johnson & Eagly, 1989), and studies of pretest sensitization (e.g., Lana, 1959). Because we were interested in warnings rather than multiple persuasion attempts, we did not include inoculation studies and other research in which the initial message, or warning, presented issue-relevant information (e.g., Infante, 1973; McGuire & Papageorgis, 1961). Also, because we were interested in the effects of warnings of counterattitudinal appeals on attitudes, we excluded studies of source liking that did not measure attitudes on the topic of an appeal (e.g., Berscheid, Boye, & Darley, 1968). For this reason, we also excluded studies in which the persuasive message included both pro- and counterattitudinal arguments (e.g., Hass & Linder, 1972, Experiment 1; Lundgren & Prislin, 1998; Schultz, 1963). In addition, because we were interested in warning effects prior to an appeal, we did not include studies that presented the warning after the appeal (e.g., Mills & Aronson, 1965). Also, because we were interested in the effects of preparing to receive a particular appeal, we did not include studies in which the message delivered to participants was not the one mentioned in the warning (e.g., Chen, Shechter, & Chaiken, 1996).

In addition, we included in our calculations for each study only the experimental conditions that delivered warnings and also were matched with appropriate control comparisons. For example, in Wicklund, Cooper, and Linder's (1967) "high effort" condition, warned participants engaged in effortful tasks to receive the message. Because the control group received neither a warning nor an effortful task, we did not include the high effort condition to avoid confounding forewarning impact with dissonance or effort-justification effects. Finally, we were unable to include the few studies that did not provide sufficient information to calculate effect sizes (e.g., Fitzpatrick & Eagly, 1981).

### *Sample of Studies*

In the analysis on the effects of forewarning on attitudes prior to the message, we identified 19 independent research reports that met the inclusion criteria. Several of these consisted of multiple experiments, yielding 25 separate studies in the premessage sample of studies.

In the analysis on the effects of forewarning on attitudes following the delivery of the message, we identified 18 independent research reports that met the criteria. Again, several of these consisted of multiple experiments, yielding 23 separate studies in the postmessage sample.

In two of the reviewed studies (Hollander, 1974; McGuire & Millman, 1965), the authors assessed some participants' attitudes prior to the appeal and other participants' attitudes following the appeal. Thus these studies are included in both samples. We discuss the findings of these two experiments in the *Pre- and Postappeal Attitude Assessments* section.

### *Variables Coded From Each Study*

The following data were coded from each report: (a) the information given in the forewarning (intent to persuade, topic of the appeal, position to be advocated), (b) the source's identity (peer vs. superior or expert), (c) the mode of forewarning delivery (written vs. live, audio, or video), (d) the information given to the nonforewarned control group (no information vs., when participants did not expect to receive the message, the specific or general topic), (e) the reputed persuasiveness of the anticipated appeal (high vs. ambiguous), (f) the method of establishing the persuasiveness of the appeal (claimed by experimenter vs. judged by prior audience), (g) the setting in which recipients anticipated giving their attitude judgments (private questionnaire ratings vs. public discussion of position), (h) the nature of the issue (objective factual vs. subjective preference), (i) the extent to which the topic was involving (high vs. low), (j) the form of the opinion measure (change scores vs. final status scores), (k) the raw data from which the effect size was calculated (means and standard deviations vs. the value of a statistical test such as an $F$ or a $t$), and (l) the time delay from the forewarning to the attitude measure (no delay vs. several minutes). For premessage studies, interrater agreement ranged from 71% to 92% ($M$ = 81%). For postmessage studies, interrater agreement ranged

from 74% to 100% ($M$ = 86%). Disagreements were resolved through discussion.[1]

In addition to the effect sizes included in the overall analysis, we conducted separate analyses to examine particular features of the experimental designs. In the preappeal sample of studies, we separately calculated effect sizes for the conditions in which participants were informed that the impending message had been canceled. Furthermore, in both preappeal and postappeal samples of studies, we conducted separate analyses to evaluate the importance of issue-relevant thought prior to the appeal. Specifically, we calculated separate effect sizes for the conditions in which participants were distracted between the warning and attitude measures by an unrelated task (e.g., solving verbal or arithmetic problems). Also, we calculated separate effect sizes for conditions in which participants were instructed to list their thoughts between the warning and the attitude measure. The canceled message, distraction, and thought-listing paradigms were not included in the overall calculations of warning impact.

Although the syntheses were limited to studies that presented warnings of counterattitudinal appeals, our literature search yielded studies on a variety of types of warnings. We conducted separate analyses on these other warning types because they potentially provide insight into the mechanisms through which counterattitudinal warnings affect attitudes. Specifically, warnings that mention only the topic and not the position to be advocated can reveal the effects of simply expecting new information. Proattitudinal warnings of appeals advocating positions on the same side as, but more extreme than, participants' own positions can reveal whether attitude shifts represent agreement (which would be reflected in polarization) versus moderation (which would be reflected in movement to midscale).

## *Calculation of Effect Sizes*

We calculated effect sizes for each study that provided sufficient data. Effect sizes were represented in terms of *g*, the mean difference between the forewarned and the nonforewarned groups' attitudes, divided by (for between-groups comparisons) the pooled standard deviation or (for within-groups comparisons) the standard deviation of the difference between paired observations (Hedges & Olkin, 1985). The authors each derived these calculations independently using DSTAT (Johnson, 1993), a computer program designed for meta-analytic calculations. Any discrepancies were resolved through discussion. Given that the *g* statistic overestimates the population effect size, especially for smaller sample sizes, we converted the *g*s to *d*s by correcting for bias (Hedges & Olkin, 1985).

For the preappeal sample of studies, the control group data were represented by a separate group of nonforewarned participants or, in a few cases, by participants' prewarning opinions. Two sets of controls were available for the postappeal sample. The primary analyses were conducted with control participants who received the message but not the warning. Comparisons with this no-warning message control group revealed how forewarnings affected reactions to the persuasive message. Of secondary interest were the comparisons between forewarned participants and controls who received neither the message nor the warning. Comparisons with this control group were not especially informative about the specific effects of warnings but instead provided an estimate of the impact of the full treatment (i.e., warning plus message) on participants' attitudes.

Given that the metric for the effect sizes derived from change scores in within-participant pretest–posttest designs is not the same as for effect sizes from final status scores in independent-groups designs (Morris & DeShon, 2002), when possible we calculated all effects in a single metric, final status scores. We also conducted analyses to determine whether the few effects derived from within-participant change scores differed from effects generated from final status scores. Suggesting no differences due to metric, comparable effects emerged for change scores and final status scores in both the premessage and postmessage samples.

In general, effects were given a positive sign to indicate change toward the persuasive message and a negative sign to indicate change away from the message. Thus, for warnings that threatened a counterattitudinal appeal, the sign of the effect was positive when attitudes changed toward the message position and/or the scale midpoint and was negative when attitudes changed away and became more polarized. For warnings of proattitudinal appeals, positive effects indicated movement toward the impending appeal and polarization, and negative effects indicated movement away from the appeal, reflected in moderation and/or movement toward the opposing position. For topic-only warnings, the sign of the effect was positive when attitudes changed toward the scale midpoint and/or the opposing position and negative when attitudes became more polarized.

We used both fixed-effects and random-effects models in the analyses. Fixed-effects models are appropriate when meta-analysts wish to make inferences only about the effect size parameters in the reviewed studies or about an identical set (Hedges & Vevea, 1998). In these models, the study effects estimate the population effect with the only error being from the random sampling of participants within the studies. In contrast, random-effects models are appropriate when analysts wish to make inferences that generalize beyond the specific set of reviewed studies to a broader population. These models assume that variability between effect sizes emerges from participant-level sampling error as well as from random differences between studies that are associated with variations in experimental procedures and settings.

We first conducted sensitivity analyses by calculating and comparing the overall estimates for both fixed- and random-effects models. This is advisable because these models can yield divergent results and because each is sensitive to violations of its assumptions and can yield biased results if, for example, fixed-effects models are used when random variability exists between studies (Field, 2001; Overton, 1998). In all cases, we computed mean *d*s (and models) with each effect size weighted by the reciprocal of its variance, a procedure that gives more weight to effect sizes that are more reliably estimated. The weights for the fixed-effects models reflect only sampling error, whereas the weights for the random-effects models also include a variance component that reflects the random differences among studies (Hedges & Olkin, 1985). Analyses were performed with Borenstein and Rothstein's (1999) Comprehensive Meta-Analysis and Wang and Bushman's (1999) SAS programs for meta-analysis.

To determine whether each set of *d*s shared a common population effect size, and thus differed from each other only in participant-level sampling error, we calculated the homogeneity statistic $Q$, which has an approximate chi-square distribution with $k - 1$ degrees of freedom, in which $k$ represents the number of effect size estimates. In the absence of homogeneity, we assumed that the variability could be explained by the systematic influence of between-study factors represented by the moderating variables coded from each study report. We thus accounted for the variability in heterogeneous effect sizes by calculating fixed-effect categorical models that re-

[1] We also coded the following variables that did not yield any significant effects in the analyses: the number of items in the attitude measures; whether the report appeared in a published or an unpublished outlet; whether the cover task of the experiment was to rate the presentation, to give their own opinions, or to recall the message; whether the experimental design was between-participants or within-participants; and the likelihood of participants having a preexisting attitude on the issue. In addition, in the postmessage studies, we did not obtain any significant effects for the following: the time delay from the message communication to the attitude measure; the strength of the message arguments; and the number of arguments in the appeal. These predictors are not discussed further.

lated the effect sizes to the attributes of the studies.[2] A fixed moderator effect interpretation is appropriate to draw conclusions about the specific levels of the moderating variables present in the reviewed studies (Overton, 1998). Calculation of these categorical models provided a between-classes effect, $Q_B$, and a test of the homogeneity of the effect sizes within each class, $Q_W$. We report the results of the within-class homogeneity tests only when they are significant.

We also evaluated in a random-effects model the effects of year of study publication or, for unpublished manuscripts, year of study report. The random-effects approach is especially appropriate to evaluate moderators like year because the study dates are best considered a random selection from a broader population of possible dates (Overton, 1998). To evaluate year, we conducted random-effects continuous models, which are least squares linear regressions. Each model yields a test of the significance of each predictor, along with estimates of the participant-level sampling variance and the between-studies sampling variance. Tables 1 and 2 report the data included in the review.

## Warning Effects on Attitudes Prior to the Appeal

### *Results*

The overall effect of forewarning prior to the appeal was a shift of attitudes toward the impending message position and toward midscale. As can be seen in the first row of Table 3, the various methods of calculating effect sizes yielded comparable results, including the fixed-effects estimate ($d = 0.37$; 95% confidence interval [CI] = 0.28, 0.46; $k = 20$, conditional sampling variance [$v$] = 0.002), the random-effects estimate ($d = 0.43$; 95% CI = 0.28, 0.58; unconditional sampling variance [$\tau^2$] = 0.01; Hedges & Vevea, 1998), and the unweighted estimate ($d = 0.50$). As would be expected given the diverse attributes of the studies included in this analysis, the fixed-effects model revealed that homogeneity of effects was rejected, $Q(19) = 45.88$, $p < .001$. A stem and leaf plot reporting the effect size outcomes revealed a relatively flat distribution that was reasonably symmetrically distributed around the sample mean (see Figure 1).

We conducted outlier analyses to evaluate whether the overall effect size was robust when the most deviant study outcomes were excluded from calculation of the mean effect. Homogeneity was no longer rejected after removal of four study outcomes (i.e., Deaux, 1968; Hass, 1975, Experiment 1; Hass & Mann, 1976, Experiment 2; Petty, 1977), $Q(15) = 20.68$, $p > .05$, and the recomputed fixed-effects estimate was comparable in magnitude to the estimate from the full sample ($d = 0.34$; 95% CI = 0.25, 0.43; $k = 16$). We included these four study outcomes in the analyses and used a moderator approach to account for the sample heterogeneity.

Before conducting moderator analyses, we evaluated whether the features of study design and other potential moderating variables could be interpreted independently of each other. Given the number of studies in the sample, we were not able to evaluate multiple moderators simultaneously in regression models. Individual study attributes within a given literature often are correlated in standard template designs, in part because of the common scientific practice of developing new studies from earlier researchers' experimental paradigms. Therefore, we formed contingency tables to assess the extent to which each pair of moderators was related. In general, the impending appeal tended to be described as persuasive in studies in which the source was described as an expert rather than a peer, $\chi^2(1, k = 18) = 5.73$, $p < .05$, and in studies in which the issue to be addressed was objective and factual rather than subjective and opinion based, $\chi^2(1, k = 20) = 15.93$, $p < .001$. In addition, the source tended to be described as an expert rather than a peer when the issue was objective and factual in nature rather than subjective, $\chi^2(1, k = 17) = 3.86$, $p < .05$. One additional effect emerged, the tendency for studies that used expert sources also to deliver the warning via a live, audio, or video presentation rather than in written form, $\chi^2(1, k = 19) = 9.74$, $p < .01$. In general, then, it appears that the reviewed research commonly used one of two paradigms in which warnings described (a) expert sources presenting highly persuasive messages on objective issues or (b) peer sources presenting messages of uncertain persuasiveness on subjective issues.

### *Effects of Type of Forewarnings*

The impact of different types of warning provided insight into the motives underlying anticipatory shifts. In the full sample of studies that warned of a counterattitudinal appeal, some warnings specified only the topic and stance of the impending appeal, whereas other warnings noted also that the appeal was intended to persuade recipients. Contrary to a reactance theory account of warning impact (Brehm, 1966), forewarnings that indicated an intent to persuade did not appear to generate resistance to reestablish threatened freedom. In fact, studies with warnings that specified intent to persuade, topic, and stance obtained greater shifts toward the advocated position and midscale ($d = 0.42$; 95% CI = 0.30, 0.54; $k = 8$) than did studies with warnings that specified only topic and stance ($d = 0.22$; 95% CI = 0.07, 0.36; $k = 8$), $Q_B(1) = 4.54$, $p < .05$. Four studies manipulated the type of warning and could not be included in these analyses. Homogeneity was rejected for the study grouping in which warnings specified intent, topic, and stance, $Q_W(7) = 14.66$, $p < .05$.

Analyses on types of warnings also revealed whether anticipatory shifts reflected moderation to midscale or preemptive agreement with the source position. Because the impending position was always on the opposite side of the neutral point from participants' positions, positive shifts potentially could indicate either moderation or agreement. Suggesting that these shifts reflect agreement with the specific position in the impending appeal, warning had no overall impact in the three studies (Cialdini et al.,1973; Shanbhag, 1998; Tetlock, 1983) that specified just the topic and not the position in the appeal ($d = 0.11$; 95% CI = −0.25, 0.46; $k = 3$;

(*text continues on page 129*)

[2] When possible, we conducted categorical models with a single effect size estimate for each study. This ensured that the estimates were statistically independent. An exception was the analyses on the within-study manipulations of outcome-relevant involvement, in which each study yielded effect sizes for low and high involvement. Thus, in all other categorical analyses, we excluded the few findings from studies that manipulated levels of a moderator variable. Yet, to ensure that comparable effects were obtained when these additional studies were included, we calculated multiple effect sizes for these few within-study moderators and conducted a second moderator analysis that included these effects in addition to the between-study effects. Given that the two analysis strategies yielded essentially identical results, we report only the categorical models for the statistically independent study-wide effect sizes.

Table 1
*Data in the Premessage Sample of Studies*

| Study | (Group *n*) Experimental | Control | *d* | 95% CI | Issue type | Warning delivery | Warning type | Persuasiveness of message | Delay between warning and attitude measure | Source |
|---|---|---|---|---|---|---|---|---|---|---|
| Cialdini et al. (1973)[a] | 34 | 35 | 0.19 | −0.29, 0.66 | Subjective | Written | Topic & stance | Uncertain | None | Peer |
| Cialdini et al. (1976, Exp. 1)[a,b] | 39 | 39 | −0.02 | −0.46, 0.42 | Manipulated | Written | Topic & stance | Uncertain | None | Peer |
| Cooper & Jones (1970, Exp. 1) | 139 | 139 | 0.24 | −0.00, 0.47 | Manipulated | Written | Intent to persuade, topic & stance | High | None | Manipulated |
| Cooper & Jones (1970, Exp. 2) | 96 | 12 | 0.90 | 0.29, 1.52 | Manipulated | Written | Manipulated | Manipulated | None | Expert |
| Deaux (1968)[c] | 48 | 48 | 0.86 | 0.44, 1.27 | Subjective | Live | Manipulated | Uncertain | Moderate | Expert |
| Deaux (1972, Exp. 1) | 90 | 45 | 0.40 | 0.04, 0.76 | Subjective | Audio/video | Topic & stance | Uncertain | None | Manipulated |
| Deaux (1972, Exp. 2)[a] | 150 | 150 | 0.31 | 0.08, 0.54 | Subjective | Live | Topic & stance | Uncertain | None | Manipulated |
| Dinner et al. (1972)[a] | 25 | 19 | 0.79 | 0.17, 1.40 | Subjective | Live | Intent to persuade, topic & stance | Uncertain | None | Expert |
| Hass (1975, Exp. 1) | 22 | 11 | 1.33 | 0.54, 2.12 | Objective | Live | Manipulated | High | None | Expert |
| Hass (1975, Exp. 2) | 16 | 16 | 0.91 | 0.18, 1.63 | Objective | Live | Topic & stance | High | None | Expert |
| Hass & Mann (1976, Exp. 1) | 14 | 14 | 1.01 | 0.22, 1.80 | Objective | Live | Intent to persuade, topic & stance | High | None | Expert |
| Hass & Mann (1976, Exp. 2) | 30 | 30 | 1.16 | 0.62, 1.71 | Objective | Live | Intent to persuade, topic & stance | High | None | Expert |
| Hollander (1974)[d] | 90 | 30 | 0.07 | −0.34, 0.48 | Subjective | Written | Manipulated | Uncertain | Moderate | Unspecified |
| Mays (1975) | 16 | 16 | 0.61 | −0.10, 1.32 | Subjective | Live | Intent to persuade, topic & stance | High | Moderate | Expert |
| McFarland et al. (1984, Exp. 3) | 20 | 39 | 0.53 | −0.01, 1.08 | Objective | Written | Intent to persuade, topic & stance | High | None | Expert |
| Papageorgis (1967) | 600 | 150 | 0.32 | 0.14, 0.50 | Manipulated | Other or unclear | Intent to persuade, topic & stance | High | None | Manipulated |
| Petty (1977, Exp. 5) | 38 | 38 | −0.27 | −0.73, 0.18 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| Turner (1977) | 72 | 70 | 0.59 | 0.25, 0.92 | Objective | Live | Intent to persuade, topic & stance | High | None | Expert |
| Wicklund et al. (1967, Exp. 1)[a] | 15 | 15 | 0.10 | −0.62, 0.81 | Subjective | Live | Topic & stance | Uncertain | Moderate | Expert |
| Wicklund et al. (1967, Exp. 2)[a,e] | 13 | 13 | −0.10 | −0.87, 0.67 | Subjective | Live | Topic & stance | Uncertain | Moderate | Expert |
| **Message canceled following warning** | | | | | | | | | | |
| Cialdini, et al. (1973)[a] | 34 | 42 | −0.15 | −0.60, 0.30 | Subjective | Written | Topic & stance | Uncertain | None | Peer |
| Cialdini et al. (1976, Exp. 1)[a,b] | 39 | 39 | −0.15 | −0.59, 0.30 | Manipulated | Written | Topic & stance | Uncertain | None | Peer |
| Hass & Mann (1976, Exp. 1) | 28 | 14 | 0.49 | −0.16, 1.14 | Objective | Live | Intent to persuade, topic & stance | High | None | Expert |
| Hass & Mann (1976, Exp. 2) | 45 | 15 | 0.57 | −0.02, 1.17 | Objective | Live | Intent to persuade, topic & stance | High | None | Expert |
| McFarland et al. (1984, Exp. 3) | 18 | 39 | 0.11 | −0.45, 0.67 | Objective | Written | Intent to persuade, topic & stance | High | None | Expert |
| McGuire & Millman (1965) | 96 | 96 | 0.24 | −0.05, 0.52 | Manipulated | Written | Topic & stance | High | None | Manipulated |
| Petty (1977, Exp. 5) | 38 | 38 | −0.36 | −0.82, 0.09 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| **Topic-only warnings** | | | | | | | | | | |
| Cialdini et al. (1973)[a] | 10 | 10 | 0.30 | −0.58, 1.18 | Subjective | Written | Topic | Uncertain | None | Peer |
| Shanbhag (1998)[f] | 55 | 30 | 0.11 | −0.34, 0.55 | Subjective | Written | Topic | Uncertain | None | Expert |
| Tetlock (1983) | 12 | 12 | −0.06 | −0.86, 0.74 | Subjective | Written | Topic | Uncertain | Moderate | Peer |
| **Thought listing between warning and attitude measure** | | | | | | | | | | |
| Cialdini et al. (1976, Exp. 2)[a,b] | 33 | 33 | −0.03 | −0.51, 0.45 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| Mays (1975) | 16 | 16 | 1.17 | 0.42, 1.92 | Subjective | Live | Intent to persuade, topic & stance | High | Moderate | Expert |
| Tetlock (1983) | 12 | 12 | −0.41 | −1.22, 0.40 | Subjective | Written | Manipulated | Uncertain | Moderate | Peer |
| Wu (1984)[a] | 217 | 217 | −0.07 | −0.26, 0.12 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |

(*table continues*)

Table 1 (*continued*)

| Study | (Group *n*) Experimental | Control | *d* | 95% CI | Issue type | Warning delivery | Warning type | Persuasiveness of message | Delay between warning and attitude measure | Source |
|---|---|---|---|---|---|---|---|---|---|---|
| **Distraction between warning and attitude measure** | | | | | | | | | | |
| Mays (1975)[g] | 32 | 32 | 0.96 | 0.44, 1.48 | Subjective | Live | Intent to persuade, topic & stance | High | Moderate | Expert |
| Petty (1977, Exp. 5) | 38 | 38 | 0.34 | −0.12, 0.79 | Subjective | Written | Topic & stance | Low | Moderate | Peer |
| **Within-study manipulations of outcome-relevant involvement** | | | | | | | | | | |
| Cialdini et al. (1976, Exp. 1)[a,b] | | | | | | | | | | |
| High involvement | 20 | 20 | −0.31 | −0.93, 0.32 | Manipulated | Written | Topic & stance | Uncertain | None | Peer |
| Low involvement | 19 | 19 | 0.29 | −0.35, 0.93 | Manipulated | Written | Topic & stance | Uncertain | None | Peer |
| Cialdini et al. (1976, Exp. 2)[a,b] | | | | | | | | | | |
| High involvement | 18 | 18 | −0.23 | −0.88, 0.43 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| Low involvement | 15 | 15 | 0.35 | −0.37, 1.07 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| Wu (1984)[a,h] | | | | | | | | | | |
| High involvement | 55 | 55 | −0.35 | −0.73, 0.03 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |
| Low involvement | 162 | 162 | 0.02 | −0.20, 0.24 | Subjective | Written | Topic & stance | Uncertain | Moderate | Peer |

*Note.* CI = confidence interval; Exp. = Experiment. Effect sizes (*d*s) are positive when warnings generated acquiescence and negative when warnings generated resistance.
[a] Several studies calculated warning-induced attitude change by subtracting the mean of a control group from the attitude scores of the experimental participants. Because the variance of the difference between two random variables with equal variance is twice the variance of one variable alone, we followed a conservative strategy and adjusted the pooled standard deviations by multiplying by $\sqrt{2}$. [b] Omitted "delayed discussion" conditions. [c] Omitted "information" condition because this warning provided arguments for and against the issue to be discussed in the appeal. [d] Effect size estimates are calculated using participants' attitudes toward joining the Peace Corps themselves. Both the persuasion context and the warning conditions were considered forewarnings in the present analysis. [e] Omitted "high effort" condition. [f] Effect size estimate is based on responses to the Modern Racism Scale only. We did not use data related to the "guilt" and "ambivalence" dependent measures. [g] We combined the "gullibility focus" group with the "distraction" group because the purpose of both manipulations was "eliminating opportunity for thought about the message" (Mays, 1975, p. 27). [h] Because the low-involvement manipulation was successful for the tuition but not the foreign language issue, we classified all conditions for the foreign language issue as low in involvement.

Table 2
*Data in the Postmessage Sample of Studies*

| Study | Group *n* | | *d* | 95% CI | Issue type | Warning delivery | Warning type | Persuasiveness of message | Delay between warning and message | Source |
|---|---|---|---|---|---|---|---|---|---|---|
| | Experimental | Control | | | | | | | | |
| **Warning impact compared with no-warning controls who received persuasive message** | | | | | | | | | | |
| Allyn & Festinger (1961) | 41 | 46 | −0.51 | −0.93, −0.08 | Subjective | Written | Topic & stance | Uncertain | None | Expert |
| Chen et al. (1992, Exp. 1) | 20 | 20 | −1.50 | −2.20, −0.80 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Chen et al. (1992, Exp. 2) | 40 | 40 | −1.57 | −2.07, −1.07 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Dean (1974, Exp. 1)[a] | 134 | 65 | −0.50 | −0.80, −0.20 | Objective | Written | Intent to persuade, topic & stance | Uncertain | None | Unspecified |
| Dean (1974, Exp. 2) | 45 | 23 | −0.04 | −0.54, 0.46 | Objective | Written | Intent to persuade, topic & stance | Uncertain | None | Unspecified |
| Dean et al. (1971, Exp. 1) | 44 | 44 | −0.42 | −0.84, 0.01 | Manipulated | Written | Intent to persuade, topic & stance | High | None | Manipulated |
| Fukada (1986) | 31 | 31 | −0.54 | −1.05, −0.04 | Objective | Written | Intent to persuade | High | Moderate | Unspecified |
| Hass & Grady (1975)[b] | 38 | 20 | −0.41 | −0.96, 0.14 | Subjective | Written | Manipulated | Manipulated | None | Expert |
| Hollander (1974)[c] | 90 | 30 | 0.32 | −0.09, 0.14 | Subjective | Written | Manipulated | Uncertain | Moderate | Unspecified |
| Holt & Watts (1973) | 49.5 | 49.5 | −0.40 | −0.79, 0.00 | No information | Written | Intent to persuade | High | None | Unspecified |
| Kiesler & Kiesler (1964) | 45 | 53 | −0.61 | −1.01, −0.20 | Subjective | Written | Intent to persuade | High | None | Expert |
| McGuire & Millman (1965) | 96 | 96 | 0.00 | −0.28, 0.28 | Manipulated | Written | Topic & stance | High | None | Manipulated |
| McGuire & Papageorgis (1962) | 24 | 24 | −0.02 | −0.59, 0.55 | Objective | Live | Intent to persuade | High | None | Unspecified |
| Neimeyer et al. (1991, Exp. 2) | 47 | 51 | 0.03 | −0.37, 0.43 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Petty & Cacioppo (1979) | 57 | 59 | −0.50 | −0.87, −0.13 | Subjective | Written | Intent to persuade | High | None | Peer |
| Romero et al. (1996, Exp. 1)[d] | 76 | 31 | −0.69 | −1.11, −0.26 | Subjective | Live | Topic & stance | Uncertain | Manipulated | Expert |
| Romero et al. (1996, Exp. 2)[e] | 28 | 33 | −0.34 | −0.84, 0.17 | Subjective | Live | Topic & stance | Uncertain | Moderate | Expert |
| **Warning impact compared with no-message control conditions** | | | | | | | | | | |
| Dean et al. (1971, Exp. 1) | 22 | 44 | −0.81 | −1.34, −0.28 | Manipulated | Written | Intent to persuade | High | None | Manipulated |
| Dean et al. (1971, Exp. 2)[f] | 39 | 40 | −0.30 | −0.74, 0.15 | Subjective | Written | Intent to persuade | High | None | Unspecified |
| Freedman & Sears (1965) | 121 | 121 | 0.16 | −0.09, 0.42 | Subjective | Written | Topic & stance | Uncertain | None | Expert |
| Fukada (1986) | 31 | 31 | 0.85 | 0.33, 1.37 | Objective | Written | Intent to persuade | High | Moderate | Unspecified |
| Hass & Grady (1975) | 38 | 20 | 0.78 | 0.22, 1.34 | Subjective | Written | Manipulated | Manipulated | None | Expert |
| Hollander (1974)[c] | 90 | 30 | 0.67 | 0.25, 1.09 | Subjective | Written | Manipulated | Uncertain | Moderate | Unspecified |
| Romero et al. (1996, Exp. 1)[g] | 76 | 31 | 0.29 | −0.13, 0.71 | Subjective | Live | Topic & stance | Uncertain | Manipulated | Expert |
| **Distraction between warning and message (vs. message control conditions)** | | | | | | | | | | |
| Chen et al. (1992, Exp. 1) | 20 | 20 | 0.56 | −0.08, 1.19 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Chen et al. (1992, Exp. 2) | 40 | 40 | 0.12 | −0.32, 0.56 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Romero et al. (1996, Exp. 1) | 21 | 31 | 0.06 | −0.50, 0.61 | Subjective | Live | Topic & stance | Uncertain | Moderate | Expert |
| **Distraction plus time delay between warning and message (vs. message control or, for Freedman & Sears, 1965, no-message control)** | | | | | | | | | | |
| Apsler & Sears (1968) | 40 | 40 | 0.41 | −0.04, 0.85 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Freedman & Sears (1965) | 171 | 171 | 0.09 | −0.12, 0.30 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Hass & Grady (1975) | 40 | 20 | −0.76 | −1.31, −0.20 | Subjective | Written | Manipulated | Manipulated | Moderate | Expert |

(*table continues*)

Table 2 (*continued*)

| Study | Group *n* | | *d* | 95% CI | Issue type | Warning delivery | Warning type | Persuasiveness of message | Delay between warning and message | Source |
|---|---|---|---|---|---|---|---|---|---|---|
| | Experimental | Control | | | | | | | | |
| **Thought listing between warning and message (vs. message control or no-message control)** | | | | | | | | | | |
| Hass & Linder (1972, Exp. 2)[h] | 14 | 14 | −0.16 | −0.90, 0.59 | Subjective | Written | Topic & stance | Uncertain | Moderate | Unspecified |
| Petty & Cacioppo (1977, Exp. 1) | 80 | 40 | −0.74 | −1.13, −0.34 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Petty & Cacioppo (1977, Exp. 2)[i] | 30 | 15 | −0.75 | −1.39, −0.11 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| **Within-study manipulations of outcome-relevant involvement (vs. message control)** | | | | | | | | | | |
| Allyn & Festinger (1961) | | | | | | | | | | |
| High involvement | 16 | 16 | −0.90 | −1.63, −0.17 | Subjective | Written | Topic & stance | Uncertain | None | Expert |
| Low involvement | 25 | 30 | −0.23 | −0.76, 0.30 | Subjective | Written | Topic & stance | Uncertain | None | Expert |
| Apsler & Sears (1968) | | | | | | | | | | |
| High involvement | 20 | 20 | −0.17 | −0.79, 0.45 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Low involvement | 20 | 20 | 1.13 | 0.46, 1.80 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Chen et al. (1992, Exp. 1) | | | | | | | | | | |
| High involvement | 10 | 10 | −2.31 | −3.44, −1.17 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Low involvement | 10 | 10 | −0.03 | −0.90, 0.85 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Chen et al. (1992, Exp. 2) | | | | | | | | | | |
| High involvement | 20 | 20 | −2.70 | −3.56, −1.85 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Low involvement | 20 | 20 | 0.26 | −0.36, 0.88 | Subjective | Written | Topic & stance | Uncertain | Moderate | Expert |
| Dean et al. (1971, Exp. 1) | | | | | | | | | | |
| High involvement | 22 | 22 | −0.23 | −0.82, 0.36 | Manipulated | Written | Intent to persuade, topic & stance | High | None | Manipulated |
| Low involvement | 22 | 22 | −0.62 | −1.23, −.02 | Manipulated | Written | Intent to persuade, topic & stance | High | None | Manipulated |
| Petty & Cacioppo (1979) | | | | | | | | | | |
| High involvement | 20 | 21 | −1.32 | −1.99, −0.64 | Subjective | Written | Topic & stance | High | None | Peer |
| Low involvement | 37 | 38 | −0.17 | −0.63, 0.28 | Subjective | Written | Topic & stance | High | None | Peer |

*Note.* CI = confidence interval; Exp. = Experiment. Effect sizes (*d*s) are positive when warnings generated acquiescence and negative when warnings generated resistance.
[a] Effect size is based on the composite results across all samples that used the standard x-ray message (see Dean, 1974, p. 44). The attitude measures obtained after a 2-week period were not included because these were the only such measures in the sample of studies that provided sufficient data to calculate effects. [b] Omitted conditions in which participants were warned of the topic of the impending appeal only. These topic-only conditions could not be analyzed separately (as they were in the premessage analyses) because no other postmessage topic-only studies were located. [c] Effect size estimates are calculated using participants' attitudes toward joining the Peace Corps themselves. The persuasion context and warning conditions were considered forewarnings in the present analysis. [d] Mean attitude change is based on two conditions: optimal forewarning and no delay. The low-involvement condition was not included because the message was not comparable to that in the message control group (i.e., the controls received a high-involvement message). [e] Omitted the "prior thoughts" condition. [f] Effect size estimate is based on the attitude change scores for the low-involvement condition only. No data were provided for high involvement. [g] Mean attitude change is based on three conditions: optimal forewarning, low involvement, and no delay. [h] Omitted "refutation" condition. [i] Omitted "unwarned, topic thoughts" condition.

Table 3
*Effect Size Estimates for Preappeal and Postappeal Samples of Studies*

| Sample of studies | *k* | Mean sample size | Fixed-effects | | Random-effects | | Mean unweighted *d* | Effect sizes indicating resistance (%) |
|---|---|---|---|---|---|---|---|---|
| | | | Mean weighted *d* | 95% CI | Mean weighted *d* | 95% CI | | |
| Premessage | 20 | 125 | 0.37 | 0.28, 0.46 | 0.43 | 0.28, 0.58 | 0.50 | 20 |
| Postmessage vs. message control | 17 | 95 | −0.38 | −0.48, −0.28 | −0.42 | −0.63, −0.22 | −0.45 | 94 |
| Postmessage vs. no-message control | 7 | 105 | 0.21 | 0.06, 0.37 | 0.23 | −0.15, 0.61 | 0.24 | 29 |

*Note.* *k* = the number of studies, and thus the number of effect sizes, in each mean effect size calculation; CI = confidence interval. Effect sizes (*d*s) are positive when warnings generated acquiescence and negative when warnings generated resistance.

see Table 1).[3] Thus, the attitude shifts observed to counterattitudinal warnings likely reflect agreement with the impending appeal rather than moderation to midscale, which was anticipated in impression accounts of warning impact (e.g., Cialdini et al., 1973).

Additional insight into whether warnings induce agreement or moderation shifts can be gained from warnings of extreme proattitudinal messages, in which influence emerges in adoption of extreme views but moderation emerges in movement to midscale. However, impending proattitudinal appeals had no significant impact on participants' attitudes ($d = -0.10$; 95% CI = −0.45, 0.26; $k = 4$). Thus, these findings are not discussed further.

### *Determinants of Warning-Induced Agreement*

*Likelihood of being persuaded.* In general, anticipatory shifts toward the impending appeal were greatest when warnings suggested that recipients would eventually be influenced. Specifically, anticipatory agreement was more pronounced when the source was reputed to be an expert ($d = 0.71$; 95% CI = 0.56, 0.89; $k = 12$) rather than a peer ($d = -0.04$; 95% CI = −0.30, 0.23; $k = 3$), $Q_B(1) = 22.63$, $p < .001$. Five studies were not included in this analysis; one of these did not specify the source, and four manipulated source identity.

In addition, agreement was greater when participants were informed that the message was highly persuasive ($d = 0.44$; 95% CI = 0.32, 0.56; $k = 9$) than when message persuasiveness was not clearly specified ($d = 0.27$; 95% CI = 0.14, 0.40; $k = 10$), $Q_B(1) = 3.72$, $p < 05$. One study was not included in this analysis because it manipulated the anticipated persuasiveness of the message. Homogeneity was rejected for the studies with highly persuasive messages, $Q_W(8) = 19.53$, $p < .05$, and for studies with messages of uncertain persuasiveness, $Q_W(9) = 19.79$, $p < .05$. It is interesting to note that, of the studies with highly persuasive messages, greater agreement emerged when participants were told that previous audiences had found the message persuasive ($d = 0.88$; 95% CI = 0.63, 1.13; $k = 7$) than when the experimenter claimed only message persuasiveness ($d = 0.34$; 95% CI = 0.20, 0.47; $k = 3$), $Q_B(1) = 14.49$, $p < .001$. Perhaps previous audience ratings were especially credible and increased the perceived threat of the impending appeal.

Agreement also was greater when the topic of the impending appeal involved an objective issue with a single, factual answer ($d = 0.78$; 95% CI = 0.56, 1.00; $k = 6$) rather than a subjective issue for which diverse positions were likely to be acceptable ($d = 0.30$; 95% CI = 0.17, 0.44; $k = 10$), $Q_B(1) = 13.06$, $p < .001$. Homogeneity was rejected for studies with subjective topics, $Q_W(9) = 18.71$, $p < .05$. Four studies were not included in this analysis because they included multiple types of issues. These findings could be taken as evidence against McGuire and Millman's (1965) claim that preemptive agreement is unnecessary with objective topics because persuasion can be excused as openmindedness to the facts in the appeal. Alternatively, the findings may not stem from the type of issue but instead could reflect other features of the research paradigms. In particular, studies with objective issues tended also to use expert sources and to describe appeals as highly persuasive—two factors that themselves generated preemptive agreement.

*Modality of presenting the warning.* Greater change emerged when the forewarning was presented live or via audio or video ($d = 0.54$; 95% CI = 0.41, 0.67; $k = 12$) than when the warning was presented in written form ($d = 0.19$; 95% CI = 0.04, 0.34; $k = 7$), $Q_B(1) = 12.04$, $p < .001$. This analysis excluded one study for which mode of presentation of the warning was unclear. Homogeneity was rejected for the group of studies with live, audio, or video presentations, $Q_W(11) = 21.64$, $p < .05$. It is unclear how to interpret the modality findings given that studies with live, audio, or video warnings also tended to have expert sources and that source expertise generated agreement. Thus, the modality effects may have emerged because modality was associated with other aspects of experimental designs.

*Surveillance by the source.* Anticipatory agreement might be greatest in public settings in which social coordination pressures are likely to be most pronounced (Dijksterhuis, 2001) and in which recipients can reap the social benefits of the favorable impressions

[3] These analyses do not include Hass's (1975) "topic-only" warning condition because participants in this experimental condition most likely experienced an intent to persuade. The experimental cover story was to investigate "what makes a successful persuasive communication" (p. 1157), and participants were told further that they would receive a highly persuasive message. Indeed, the warning effect in this experimental condition ($d = 1.32$) diverged sharply from the warning effects in the three topic-only studies without persuasive intent ($d = 0.11$), while being highly similar to the effect Hass obtained when the warning specified the topic and counterattitudinal stance of the appeal ($d = 1.39$). As a point of information, our coding did not identify another study in which the cover story specified an intent to persuade while the warning did not mention such an intent.

| *Premessage (appeal anticipated)* | | *Postmessage (versus no-warning message control)* | |
|---|---|---|---|
| 1.3 | 3 | | |
| 1.2 | | 0.3 | 2 |
| 1.1 | 6 | 0.2 | |
| 1.0 | 1 | 0.1 | |
| 0.9 | 01 | 0.0 | 03 |
| 0.8 | 6 | -0.0 | 24 |
| 0.7 | 9 | -0.1 | |
| 0.6 | 1 | -0.2 | |
| 0.5 | 39 | -0.3 | 4 |
| 0.4 | 0 | -0.4 | 012 |
| 0.3 | 12 | -0.5 | 0014 |
| 0.2 | 4 | -0.6 | 19 |
| 0.1 | 09 | | |
| 0.0 | 7 | | |
| -0.0 | 2 | | |
| -0.1 | 0 | -1.5 | 07 |
| -0.2 | 7 | | |

*Figure 1.* Stem and leaf plot of effect size outcomes (*d*s) for pre- and postmessage samples.

conveyed through any moderation shifts (Cialdini & Petty, 1981). Contrary to these expectations, attitudes actually changed less in public settings in which participants expected to discuss their attitudes with a partner or defend their attitudes to the message source ($d = -0.14$; 95% CI = −0.46, 0.18; $k = 2$) than in private settings in which participants expected to indicate their judgments on a questionnaire ($d = 0.40$; 95% CI = 0.31, 0.49; $k = 16$), $Q_B(1) = 10.31$, $p < .01$. Two studies (Cialdini et al., 1973; Deaux, 1968) were not included in this analysis because they manipulated whether attitudes were to be expressed publicly or privately after the impending appeal (the findings of these studies are evaluated in detail in the General Discussion section). Homogeneity was rejected for the private study grouping, $Q_W(15) = 29.21$, $p < .05$. Thus, the surveillance findings provided little support for the social coordination or impression-motivated accounts of warning impact.

### *Variations in Recipients' Motivation and Ability to Think About Message Topic*

*Topic involvement.* According to the biased-processing account, warnings induce resistance on involving topics that implicate personal outcomes because they orient thoughts in support of initial opinions. Three of the experiments (Cialdini et al., 1976, Experiments 1 and 2; Wu, 1984) directly varied involvement in the topic, and we conducted separate within-study analyses on these data to compare warning impact for highly involving versus less involving topics (see Table 1). Involvement was varied in these studies either through the description of the issue—a university policy (e.g., senior comprehensive exams, tuition increase) that would be effective immediately versus in several years (Cialdini et al., 1976, Experiment 2; Wu, 1984)—or through the grouping of participants according to their reports of the personal importance of the topic (e.g., for female high school students, waiting a year after high school before marriage; Cialdini et al., 1976, Experiment 1).

In support of the motivated-processing idea, greater involvement yielded greater resistance. Specifically, warnings on highly involving issues resulted in a boomerang away from the impending appeal ($d = -0.31$; 95% CI = −0.61, −0.02; $k = 3$), whereas warnings on less involving issues yielded a slight tendency toward acquiescence ($d = 0.07$; 95% CI = −0.13, 0.27; $k = 3$), $Q_B(1) = 4.60$, $p < .05$.

In the studies that did not vary outcome relevance of the topic, we categorized the topics of the persuasive appeals as being of high or low involvement. As anticipated, greater resistance emerged in the single study (Petty, 1977, Experiment 5) that used an involving topic relevant to participants' immediate outcomes ($d = -0.27$; 95% CI = −0.73, 0.19; $k = 1$) than in studies with less involving issues ($d = 0.41$; 95% CI = 0.32, 0.50; $k = 18$), $Q_B(1) = 8.38$, $p < .001$. One study (Cialdini et al., 1976, Experiment 1) that manipulated outcome relevance was not included in this analysis, although it did appear in the within-study analysis of involvement (along with two other studies that did not appear in the overall data set because participants listed their thoughts prior to indicating attitudes). Thus, comparable results emerged in the between-study and within-study analysis on involvement, and these support the idea that warning-induced threats bias recipients' thoughts about an impending appeal on an involving topic.

*Time delay and thought listing between forewarning and attitude measure.* A time delay between the warning and attitude measure, which presumably would enable recipients to generate cognitive defenses, was not associated with increased resistance. That is, comparable levels of attitude change emerged in studies with no delay between the forewarning and the appeal ($d = 0.40$; 95% CI = 0.30, 0.49; $k = 14$) and in those with a delay from 2 to 10 min ($d = 0.23$; 95% CI = 0.02, 0.45; $k = 6$). Homogeneity was rejected for the studies with no time delay, $Q_W(13) = 28.39$, $p < .01$, and for studies with a delay of a few minutes, $Q_W(5) = 15.58$, $p < .05$.

Although the opportunity for cognitive bolstering provided by the time delay was not sufficient to generate resistance, a direct instruction to list thoughts apparently provided the necessary motivation and opportunity to do so. Four studies (Cialdini et al., 1976, Experiment 2; Mays, 1975; Tetlock, 1983; Wu, 1984) in the review included conditions in which participants listed their thoughts during the interval between warning and attitude assessment, and we conducted separate analyses on these studies (see Table 1). As would be anticipated from the biased-processing perspective, participants who listed their thoughts were more op-

posed to the appeal ($d = -0.02$; 95% CI = −0.19, 0.15; $k = 4$) than participants in non-thought-listing studies (i.e., the full-sample effect reported above; $d = 0.37$; 95% CI = 0.28, 0.46; $k = 20$), $Q_B(1) = 16.58$, $p < .001$. Homogeneity was rejected for the thought-listing studies, $Q_W(3) = 10.19$, $p < .05$.

*Distraction between warning and attitude measure.* To examine whether preemptive agreement emerged from a relatively superficial or more thoughtful analysis of the issue in the impending appeal, we examined two studies (Mays, 1975; Petty, 1977, Experiment 5) in which participants were distracted between the warning and the attitude measure (see Table 1). Because distracted recipients expressed high levels of agreement with the impending appeal ($d = 0.60$; 95% CI = 0.26, 0.95; $k = 2$), it appears that impairing people's ability to think had minimal impact on attitude change. Indeed, the level of agreement did not differ from that obtained in the studies that did not use distraction (i.e., the full-sample effect reported above; $d = 0.37$; 95% CI = 0.28, 0.46; $k = 20$, *ns*). Thus, preemptive agreement with the impending appeal seemed to represent a superficial, minimally thoughtful analysis of the attitude issue.

### *Cancellation of Appeal Following Warning*

To examine whether anticipatory agreement effects were context dependent and maintained only as long as participants expected to receive the persuasive message, we conducted separate analyses on the seven studies (Cialdini et al., 1973; Cialdini et al., 1976, Experiment 1; Hass & Mann, 1976, Experiments 1 and 2; McFarland et al, 1984, Experiment 3; McGuire & Millman, 1965; Petty, 1977, Experiment 5) in the review that included conditions in which the impending appeal was supposedly canceled (see Table 1). Consistent with the view of preemptive agreement as context dependent, forewarned and nonforewarned participants' attitudes did not differ significantly upon cancellation ($d = 0.08$; 95% CI = −0.09, 0.25; $k = 7$). Furthermore, the attitude shifts after cancellation were significantly smaller than the shifts obtained in the full-sample analysis on warned participants who were expecting a persuasive appeal ($d = 0.37$; 95% CI = 0.28, 0.46; $k = 20$), $Q_B(1) = 9.2$, $p < .01$.

## *Discussion*

When attitudes were assessed prior to the appeal, forewarnings generated shifts toward the source's impending position. Furthermore, the pattern of these anticipatory shifts suggests that attitudes changed as a defensive response to the threat of an impending counterattitudinal appeal.

The effects that emerged for type of warning are informative about whether warnings instigated agreement or impression-motivated moderation to midscale. That is, marked attitude shifts were generated by warnings that specified an intent to persuade as well as by warnings that specified the topic and stance of the appeal. Despite the superficial differences between warnings of intent and warnings of topic and stance, both alerted recipients to an impending counterattitudinal appeal. In contrast, warnings had little impact when they failed to suggest a challenging position and specified only that participants would hear a message on a particular topic. Because attitudes shifted only when the message position was implied or stated in the warning, this change appears to represent agreement rather than moderation to a midscale position in an attempt to convey a defensible, broad-minded orientation (see Cialdini et al., 1973).

Additionally casting doubt on the idea that attitude change represented moderation to achieve immediate social goals, agreement was not especially marked in public contexts, in which participants should have been especially concerned about the impressions they conveyed to others. It is interesting that surveillance also had no significant effect on influence in other research paradigms, including conformity in the Asch line-judging paradigm (Bond & Smith, 1996) and agreement with minority group sources (Wood, Lundgren, Ouellette, Busceme, & Blackstone, 1994). Influence in these paradigms likely emerged for a variety of reasons in addition to concerns with immediate impressions. Many social motives, like the self-defensive responses that appear to underlie warning effects, are not limited to public settings with surveillance but also emerge in private contexts (see Wood, 2000).

The overall pattern of attitude change findings is consistent with McGuire and Millman's (1965) self-esteem account in which preemptive agreement is a defensive strategy to reduce the possible threat to self of eventually capitulating to the appeal. Anticipatory attitude shifts would have allowed recipients to avoid feeling gullible when they eventually were influenced. Indeed, consistent with recipients' apparent defensiveness, preappeal agreement was especially likely when warnings threatened participants with a message that they would be unlikely to resist. That is, in comparison with nonwarned control participants, agreement was most marked when warnings specified that the source was especially expert or the message was likely to be highly persuasive. Presumably, these anticipatory shifts to highly persuasive appeals from expert sources emerged because the threat of eventually being influenced was strongest under these conditions.

The synthesis also is informative about the extent of message-relevant thought instigated by these defensive motives. The two studies (Mays, 1975; Petty, 1977, Experiment 5) in the premessage sample that provided a direct test of the role of thoughts by instituting a distraction task between the warning and attitude assessment obtained significant anticipatory shifts toward the appeal. Because this level of agreement did not differ from that found in studies without distraction, recipients' ability to think carefully about the issue appears to have had little impact on agreement. This pattern of findings suggests that the anticipatory attitude shifts in this paradigm did not involve much systematic thought about the message topic.

Given this minimal thought, anticipatory agreement plausibly reflects heuristic processing and recipients' use of such rules as "expert sources can be trusted" (Chaiken et al., 1996, p. 553). Regardless of whether participants used such a rule, attitude change appears to have been motivated by defensive goals within a particular context. The strategic nature of such shifts is apparent in that attitudes tended to revert to baseline when participants were informed that the impending appeal had been canceled. Presumably, if recipients' processing had been motivated by other factors such as the desire to hold a valid, unbiased position, cancellation of the appeal would not have negated attitude change, and it might even have increased change because of the reminder of the message and the associated source identity or reputed message persuasiveness. Additional evidence that attitude change did not emerge simply from informational processes is provided by studies that included an additional control condition in which participants were informed of the impending appeal but did not expect to be

exposed to it themselves (i.e., Cooper & Jones, 1970; Hass & Mann, 1976). Although we were unable to calculate exact effect sizes because sufficient data were not available for these conditions, both studies reported that simple knowledge of the existence of a counterattitudinal appeal did not generate significant attitude change. In fact, information about the appeal was not sufficient to change attitudes even when it was described as highly persuasive and the source was described as expert (Hass & Mann, 1976). Thus, the findings of the present review converge to suggest that recipients changed their attitudes toward the appeal in strategic self-defense.

The preemptive agreement based on minimal thought that emerged across the full set of findings contrasts with the resistance generated in the studies in which recipients were motivated to think about the appeal. When the topic of the impending appeal was involving, and presumably recipients were already motivated to think carefully about the message (Johnson & Eagly, 1989), then the warning appeared to negatively bias this thought. In fact, this cognitive bolstering appeared to have been so persuasive that it convinced participants to shift their attitudes away from the message in a boomerang effect ($d = -0.32$; 95% CI = $-0.61$, $-0.03$; $k = 3$). Additional evidence of resistance is found in the attitude stability (rather than preemptive agreement) that emerged when participants were instructed to write down their thoughts immediately after receiving the warning. Thus, in studies with experimental variations that increased thought about the appeal—either through involving message topics or instructions to list thoughts—attitudinal resistance was obtained, rather than the preemptive agreement obtained in the overall set of studies.

It is interesting to note that resistance did not emerge in studies that provided a time delay between the warning and preappeal attitude measure. Presumably, time enabled generation of cognitive defenses, but because it did not ensure that participants were motivated to generate such thought, time delay was not reliably associated with resistance. We discuss the effects of delay further, after presenting the findings from the second synthesis on studies that assessed attitudes following presentation of the persuasive message.

## Warning Effects on Attitudes Following Appeal

### *Results*

The second synthesis evaluated the postmessage effects of warnings. Contrary to the overall anticipatory agreement apparent in the preappeal paradigm, in the postappeal studies, warned recipients changed their attitudes less than control participants who received the appeal without warning. As can be seen in the second row of Table 3, the various methods of calculating effect sizes yielded comparable results, including the fixed-effects estimate ($d = -0.38$; 95% CI = $-0.48$, $-0.27$; $k = 17$, $v = 0.003$), the random-effects estimate ($d = -0.42$; 95% CI = $-0.63$, $-0.22$; $\tau^2 = 0.01$), and the unweighted estimate ($d = -0.45$). Thus, forewarning impact after receipt of the appeal emerged in resistance. The test for homogeneity was significant, $Q(16) = 60.16$, $p < .001$. The stem and leaf plot of study outcomes revealed a tendency toward a normal distribution (see Figure 1).

We conducted outlier analyses to evaluate whether the overall effect size was robust when the most deviant study outcomes were excluded from calculation of the mean effect. Homogeneity was no longer rejected after removal of three study outcomes (i.e., Chen, Reardon, Rea, & Moore, 1992, Experiments 1 and 2; Hollander, 1974), $Q(13) = 18.88$, $p > .05$. The fixed-effect estimate calculated after removal of these studies was comparable to the estimate from the full sample ($d = -0.34$; 95% CI = $-0.45$, $-0.24$; $k = 14$). We included these three study outcomes in the analyses and used a moderator approach to account for the sample heterogeneity.

To determine whether the features of study design can be interpreted independently of each other, we formed contingency tables for each pair of moderators. Only two significant effects emerged, suggesting that the attributes of the studies did not cohere in discrete research paradigms. Specifically, the reputed persuasiveness of the message (as described in the forewarning) was related to the type of warning, $\chi^2(1, k = 15) = 8.88$, $p < .05$, such that studies with uncertain persuasiveness tended to use warnings of topic and stance, whereas studies with messages reputed to be persuasive tended to use warnings of persuasive intent. In addition, the objective or subjective nature of the topic in the appeal was related to the type of forewarning, $\chi^2(1, k = 12) = 7.50$, $p < .05$, such that studies with objective topics used a variety of types of warnings (e.g., ones that specified intent, topic, and stance; ones that specified intent only), whereas studies with subjective topics tended to use warnings that specified intent, topic, and stance.

### *Type of Forewarning*

According to the biased-processing analysis, a variety of types of warning can instigate the defensively motivated thought and message processing that result in attitudinal resistance. In support of this reasoning, resistance emerged regardless of whether warnings noted simply the intent to persuade ($d = -0.45$; 95% CI = $-0.64$, $-0.26$; $k = 5$); mentioned the topic and message position but not persuasive intent ($d = -0.42$; 95% CI = $-0.58$, $-0.26$; $k = 7$); or specified all three components of topic, stance, and intent ($d = -0.39$; 95% CI = $-0.61$, $-0.17$; $k = 3$). Two studies that manipulated type of warning were not included in this analysis. Homogeneity was rejected for studies that mentioned topic and stance, $Q_W(6) = 43.18$, $p < .001$. These findings challenge a reactance account of warning effects because resistance was not heightened when warnings limited freedom by specifying an intent to persuade.

### *Motivation and Ability to Think About the Appeal*

*Topic involvement.* Six experiments varied outcome-relevant involvement in the message topic: One of these (Allyn & Festinger, 1961) compared participants who reported that the issue was high versus low in personal relevance; four studies (Apsler & Sears, 1968; Chen et al., 1992, Experiments 1 and 2; Petty & Cacioppo, 1979) informed college students of a new university policy that would be implemented in the immediate versus distant future (e.g., replacing professors with teaching assistants, implementing senior comprehensive exams); and one study (Dean, Austin, & Watts, 1971, Experiment 1) presented an involving issue (the election of a disliked political candidate) versus a less involving one (annual X rays for tuberculosis). We calculated two effect size comparisons for each study to represent warning impact for highly involving and for less involving topics (see Table 2).

Replicating the involvement effects from studies in which attitudes were assessed prior to the appeal, warnings instigated more

resistance with highly involving topics ($d = -0.92$; 95% CI = –1.22, −0.63; $k = 6$) than with less involving topics ($d = -0.01$; 95% CI = −0.25, 0.23; $k = 6$), $Q_B(1) = 22.08$, $p < .001$. Homogeneity was rejected for the high-involvement effects, $Q_W(5) = 31.96$, $p < .001$, and for the low-involvement ones, $Q_W(5) = 16.23$, $p < .01$.

For the studies that did not experimentally vary outcome-relevant involvement, we classified the overall level of involvement of the influence topics. However, only two studies (Hass & Grady, 1975; Romero et al., 1996, Experiment 2) used highly involving topics, and these reported slightly but not significantly greater resistance ($d = -0.37$; 95% CI = −0.74, 0.01; $k = 2$) than studies with less involving topics ($d = -0.23$; 95% CI = −0.39, −0.08; $k = 7$). Two studies were not included in this analysis because the level of involvement was unclear, and six were not included because they manipulated involvement. Homogeneity was rejected in the low-involvement group, $Q_W(6) = 16.50$, $p < .05$. Given that few studies were coded as having highly involving topics in either synthesis, the overall positive impact of warning on attitudes in the premessage sample and the overall resistance effect in the postmessage sample cannot be attributed to the postsample using more highly involving topics.

*Listing thoughts between warning and appeal.* To evaluate whether warning effects (and presumably cognitive bolstering) were enhanced by listing thoughts between the warning and the appeal, we conducted separate analyses on the three studies that directed participants to list their thoughts (Hass & Linder, 1972, Experiment 2; Petty & Cacioppo, 1977, Experiments 1 and 2; see Table 2). Consistent with the premessage analyses, participants who listed thoughts evidenced high levels of resistance ($d = -0.64$; 95% CI = −0.94, −0.33; $k = 3$). One of the studies (Hass & Linder, 1972, Experiment 2) in this analysis used a no-warning message control, and two studies (Petty & Cacioppo, 1977, Experiments 1 and 2) used a no-warning, no-message control. For these two studies, the negative effect represents a true boomerang away from the message position. Furthermore, the resistance after listing thoughts was marginally greater than the resistance effect observed among participants who did not list thoughts following the warning (i.e., the full-sample effect reported above; $d = -0.38$; 95% CI = −0.48, −0.28; $k = 17$), $Q_B(1) = 2.56$, $p < .11$.

*Distraction between warning and appeal.* To examine the extent to which resistance reflected a thoughtful analysis of the message position, we conducted separate analyses on the three studies (Chen et al., 1992, Experiments 1 and 2; Romero et al., 1996, Experiment 1) that distracted participants with filler tasks between the warning and the delivery of the influence appeal (see Table 2). The distraction task reduced the warning impact to nonsignificance; warned participants did not differ from control participants who received a message with no warning ($d = 0.20$; 95% CI = −0.11, 0.50; $k = 3$), which suggests that resistance emerged from careful scrutiny. Furthermore, this effect differed significantly from the resistance observed among participants who were not distracted (i.e., the full sample reported above; $d = -0.38$; 95% CI = −0.48, −0.28; $k = 17$), $Q_B(1) = 12.51$, $p < .001$.

We also conducted separate analyses on the three studies (Apsler & Sears, 1968; Freedman & Sears, 1965; Hass & Grady, 1975) that distracted participants by a brief 1–2-min questionnaire following the warning and then delayed delivering the appeal for a total of 5–10 min (see Table 2). Similar to the distraction-alone studies reported in the prior paragraph, warnings had little impact after distraction and delay ($d = 0.05$; 95% CI = −0.13, 0.23; $k = 3$). Furthermore, this effect differed significantly from the resistance observed among participants who were not distracted (i.e., the full sample reported above; $d = -0.38$; 95% CI = −0.48, −0.28; $k = 17$), $Q_B(1) = 16.43$, $p < .001$.

### *Other Moderators*

*Attributes of the impending appeal.* Consistent with our claim that the anticipatory change in the premessage analysis was temporary and context dependent, the apparent likelihood that the appeal would prevail over participants' opinions had no effect on postappeal attitudes. Thus, no significant effects emerged for whether the warning implied that the message was reputed to be highly or moderately persuasive or whether the source was described as a peer versus an expert.

*Mode of message delivery.* Contrary to the preappeal synthesis, the modality through which warnings were delivered had no impact, although the modality of message delivery proved to affect attitudes. Warnings generated more resistance when the persuasive messages that followed the warning were presented via audio or video ($d = -0.59$; 95% CI = −0.77, −0.40; $k = 6$) than when presented in written form ($d = -0.29$; 95% CI = −0.41, −0.17; $k = 11$), $Q_B(1) = 6.78$, $p < .01$. Homogeneity was rejected for both study groupings: $Q_W(5) = 30.32$, $p < .001$, for written presentations and, $Q_W(10) = 23.07$, $p < .05$, for audio and video presentations. Because the modality effects were not consistent across the two syntheses, they are not discussed further.

*Comparison with no-warning–no-message control group.* Seven studies (Dean et al., 1971, Experiments 1 and 2; Freedman & Sears, 1965; Fukada, 1986; Hass & Grady, 1975; Hollander, 1974; Romero et al., 1996, Experiment 1) provided data to evaluate the effects of forewarning in comparison with a no-treatment control group that received neither warning nor message (see Table 2). These comparisons are not especially informative about the effects of warnings but instead provide an estimate of the impact of the full treatment of the warning plus message. For these comparisons, a positive effect emerged when participants who received the warning were compared with control participants who were simply asked for their opinion ($d = 0.21$; 95% CI = 0.06, 0.37; $k = 7$). The positive effect indicates that the combined warning plus message treatment had an overall persuasive effect on participants' attitudes. This finding sheds light on the resistance effect that emerged from the comparison of warned participants with nonwarned controls who received a persuasive message. That is, although warnings generated resistance, they did not completely nullify the persuasive impact of the appeal (see Table 3). Homogeneity was rejected, $Q(6) = 33.14$, $p < .001$.

### *Pre- and Postappeal Attitude Assessments*

Two studies (Hollander, 1974; McGuire & Millman, 1965) assessed participants' attitudes immediately following the warning as well as after receipt of the persuasive appeal. We consider these in some detail because of their potential to provide a within-study perspective on the effects of warnings over time.

Because the message topics in these studies were minimally involving, we did not anticipate that participants would evidence strong resistance through biased processing at either the preappeal

or postappeal assessments. McGuire and Millman (1965) used general social and political issues, such as the difficulty of developing a cure for cancer, and Hollander (1974) used the topic of joining the Peace Corps, which he noted had little relevance for his college freshman participants.

We expected that McGuire and Millman (1965) would find preemptive agreement prior to the appeal because they highlighted the "persuasive effectiveness" (p. 475) of the message source and thereby implied that recipients would ultimately experience attitude change. In support of this idea, their participants shifted toward the appeal prior to receipt of the message ($d = 0.24$; 95% CI $= -0.05, 0.52$; $k = 1$). Also as expected, this agreement was limited to the preappeal context and was not apparent in assessments of postappeal attitudes. That is, warned recipients did not differ from participants who read the appeal without warning ($d = 0.00$; 95% CI $= -0.28, 0.28$; $k = 1$).

In contrast, we expected that Hollander (1974) would find minimal anticipatory shifts prior to the appeal because his warning did not suggest to recipients that they would eventually be persuaded. Indeed, this experiment generated little evidence of anticipatory change ($d = 0.07$; 95% CI $= -0.34, 0.48$; $k = 1$). Contrary to our expectations, however, following the appeal, participants demonstrated positive change toward the message in comparison to controls who received the message without warning ($d = 0.32$; 95% CI $= -0.07, 0.74$; $k = 1$). As can be seen in Table 2, Hollander's study generated the largest postappeal shift toward the message of any in our sample. He attributed the positive postmessage effect to the minimally involving topic, for which the warning did not instigate defensive resistance, and further noted that this effect was statistically significant only for part of his sample (i.e., for women and not men).

### *Publication Year*

We constructed random-effects regression models to predict effect size outcomes from the year in which a study appeared in the literature. Separate models were calculated for the preappeal and postappeal samples of studies. The only significant effect was in the postappeal analyses, in which a negative regression coefficient revealed that more recently reported studies obtained larger negative effect sizes ($\beta = -.015$, $SE = .008$; 95% CI $= -.31, -.0003$; $k = 17$). Thus, more recent studies obtained greater warning-induced resistance. Because this effect emerged only in the postappeal and not preappeal data set, it does not appear to characterize the warning literature in general. However, it is interesting to note that Bond and Smith's (1996) synthesis of Asch-type line-judging conformity experiments also obtained greater resistance in more recent studies.

## *Discussion*

Warning recipients of an impending appeal proved to be an effective resistance technique when attitudes were assessed following the appeal's delivery. That is, in comparison with participants who received an appeal with no warning, warned participants were less convinced by the persuasive message. Because this effect is a difference score and because treatments were delivered both to the experimental conditions (i.e., warning plus message) and to the no-warning control conditions (i.e., message only), the exact nature of the warning effect is somewhat ambiguous. To clarify warning impact, we also calculated effect sizes that compared the attitudes of warned participants with those of no-treatment control groups (i.e., those that received neither appeal nor warning). This comparison revealed that the postappeal attitudes of warned participants were somewhat more favorable than those of the nonwarned, no-message control group. Thus, it seems that forewarnings inhibited the change that nonwarned participants demonstrated toward the appeal but did not completely nullify the appeal's impact.

The warning-induced resistance apparent in the postappeal sample of studies might appear surprising given the anticipatory agreement that emerged in our synthesis on the preappeal sample. Yet both attitudinal outcomes can be understood as responses to the threat posed by the warning. Prior to the appeal, people appeared to alleviate the warning-induced threat through strategic preemptive agreement, at least when the topic of the impending message was not relevant to personal outcomes. Such attitude shifts enabled them to appear uninfluenced when they actually received the appeal. However, when the appeal was actually delivered, people's response to the initial warning-induced threat was rejection of the message.

In general, the resistance apparent when attitudes were assessed following the appeal appeared to be a thoughtful reaction to the message topic. The thoughtful nature of warning-induced resistance was suggested by the minimal impact of warning in the few studies in which recipients were distracted immediately after the warning and prior to the appeal. Because distraction inhibits thought, distracted recipients presumably were unable to elaborate on the warning-induced threat prior to message delivery, and as a result, warning had no impact. This pattern contrasts with the preappeal synthesis, in which distraction failed to inhibit recipients' relatively superficial agreement.

Additional evidence of the cognitive processes underlying resistance emerged in studies with experimental procedures known to promote thought generation; that is, with direct instructions to list thoughts and with involving message topics (Eagly & Chaiken, 1993). Especially strong resistance was found in these studies, presumably because recipients' already extensive thought was directed toward defense by the warning. Involvement and thought listing procedures also were associated with resistance in the preappeal synthesis, suggesting that these experimental variations instigated defensively biased cognitive responding in both data sets.

The one surprising finding concerning thought generation was the failure of a time delay between the warning and the appeal to increase warning-induced resistance in either the preappeal synthesis or in the postappeal synthesis, in which delay was combined with distraction. It appears that delay provided the opportunity to think carefully about the appeal but did not provide the motivation to do so. Given that past reviewers of the warning literature have sometimes concluded that delay is important to warning-induced resistance (e.g., Cialdini & Petty, 1981), we decided to examine in detail the three postappeal studies that manipulated whether a delay existed between the warning and the appeal (Freedman & Sears, 1965; Hass & Grady, 1975; Petty & Cacioppo, 1977, Experiment 1). A noteworthy feature of all of these studies is that they used involving topics (e.g., for high school students, an appeal to limit teenage driving; for college students, an appeal to increase subway and bus fares or to institute senior comprehensive exams). Thus, participants presumably were already motivated to think carefully about the appeal. To evaluate the impact of warnings on

attitude change in these studies, we calculated for each study an effect size comparing warning impact on attitudes given no time delay versus warning impact given a 2- to 10-min delay (i.e., effect sizes were calculated as mean attitude for warning-with-delay minus warning-without-delay conditions, divided by the pooled standard deviation). In this subset of studies, significantly greater resistance emerged with the delay ($d = -0.34$; 95% CI = $-0.53$, $-0.14$; $k = 3$). These findings are consistent with our argument that a time delay enhances resistance primarily when participants are already motivated to cognitively bolster their positions; they can then use the delay to do so.

## General Discussion

In summary, the findings of the two syntheses reveal the sequence of events by which forewarning of an impending appeal affects attitudes. Prior to presentation of appeals, warnings on involving topics threatened attitudes, biased thinking about the topic, and generated resistance. Alternatively, when people were not motivated to think about a topic, warnings threatened their self-images by making them feel gullible, and in response to this, people preemptively agreed. This preemptive agreement was strategic, involved minimal issue-relevant thought, and was limited to the preappeal context. When the message was eventually delivered, the wariness induced by a forewarning appeared to have only one effect on attitudes. That is, warnings generated resistance by biasing people's thoughts about the issue in the appeal, in contrast to the reactions of those who received a message with no warning.

We were able to identify the extent to which warnings instigated issue-relevant thought because the reviewed studies used a variety of experimental manipulations designed to evaluate information processing. In a few studies, participants were instructed to list their thoughts before the appeal, in several other studies participants were distracted between the warning and the appeal, and in still other studies the message topic was involving in the sense that it was relevant to recipients' immediate goals. The effects of these various experimental manipulations converge to suggest that resistance emerged from careful scrutiny of relevant information whereas agreement emerged from more superficial analysis.

The question of how much people are aware of their strategic reactions to forewarning was not addressed in the reviewed research. Certainly, warning-induced resistance involved conscious thought about the issue to bolster existing attitudes and refute the advocated position. However, this analysis might be a deliberate defensive response or it might be a relatively automatic reaction to a communicator's persuasive intent, with little explicit recognition of the need for defense. In contrast, the finding that preemptive agreement was not disrupted by distraction suggests that this response to warnings requires minimal cognitive processing and might operate autonomously when warnings threaten people's identities. Our explanation of such anticipatory shifts as undertaken to preserve self-esteem further implies that the strategy operates outside of consciousness. Awareness of one's vulnerability to a persuasive attack and the defensive response to avert it would likely undermine the effectiveness of this strategy.

Despite the evidence for warning effects on the extensiveness with which participants processed information about an issue, the reviewed research was less informative concerning the specific motives generated by warnings. Few of the studies in our review obtained manipulation checks or used experimental manipulations to directly tap the motivational states elicited by the warning. Thus, we as reviewers were functionally in the same position as the original researchers, forced to infer motivation from the pattern of attitude change outcomes. Because this leaves open the possibility that warning impact was not motivational and emerged just from knowledge of the existence of a counterattitudinal message, we also examined conditions in which participants were informed of messages that they did not expect to receive. Supporting a motivational account of warning impact, this information appeared to have little effect on attitudes. Additional evidence that attitude expressions were strategically motivated to achieve particular goals in the influence setting is provided in the preappeal synthesis by the elimination of warning-induced attitude change when the message was canceled. In future research on forewarnings it will be important to use manipulation checks to clearly delineate the nature and magnitude of the defensive motives elicited by threat of an impending appeal.

### *Motives and Modes of Processing*

Across the two research paradigms we reviewed, then, warnings produced either strategic agreement based on minimal issue-relevant thought or produced thoughtful resistance. Resistance potentially also could have emerged from less thoughtful processes, such as those specified in reactance theory (Brehm, 1966). Reactance should have been especially likely when warnings threatened recipients' freedom by, for example, noting that the message was intended to change their views. However, in the preappeal synthesis, warnings that specified an intent to persuade generated the same acquiescence as those that only noted the impending position. In the postappeal sample of studies, all counterattitudinal warnings appeared to establish a wariness among participants that instigated critical evaluation of the message. In general, the reactance perspective on warnings, like the cognitive dissonance account, proved difficult to evaluate because it is unclear whether the original research established the appropriate experimental conditions to induce the relevant psychological states (although see Fukada, 1986). For this reason, definitive evidence of the reactance-inducing aspects of warnings awaits further primary research.

In addition to the possibility that warning-induced resistance could emerge from highly thoughtful as well as less thoughtful information-processing strategies, warning-induced agreement also could result from various modes of processing. In the reviewed studies, preemptive agreement emerged from heuristic reasoning and other strategies that involved minimal issue-relevant thought. Yet, if a warning induced a self-image threat of sufficient magnitude, people presumably would be motivated to carefully scrutinize information relevant to defending their self-concept, and the resulting attitude judgments might be durable across such changes in context as cancellation of the appeal (Chaiken et al., 1996). Thus, even though the studies in our review yielded evidence of thoughtful resistance and superficial agreement, warnings plausibly also could induce resistance based on shallow processing and agreement from thoughtful scrutiny of information.

Our claim that warning impact is based on threats to existing attitudes and to one's self-image can be located within the long history of research on defensive motives in persuasion and social influence. Early functional theorists postulated that attitudes can serve ego-defensive functions by helping people to cope with

emotional conflicts and defend their self-images (Katz, 1960; Smith, Bruner, & White, 1956). For example, prejudice can serve a function whereby people bolster their own ego by projecting feelings of inferiority onto derogated out-groups such as ethnic minorities. A closely related notion is Katz's value-expressive function, under which attitudes reflect personal values and core aspects of the self-concept (see also Herek, 1986; Prentice, 1987). A motive to preserve the self-concept and associated world views also emerges in Johnson and Eagly's (1989) discussion of value-relevant involvement, Cialdini and Trost's (1998) analysis of goals to manage the self-concept, Kunda's (1990) discussion of motivated reasoning, and self-concept versions of dissonance theory (e.g., Aronson, 1992). Central to all of these views is the assumption that people hold and change attitudes to defend, maintain, and enhance aspects of the self. These traditions of theorizing and empirical evidence are consistent with the present finding that self-threats have a conservative impact by preserving existing attitudes and self-concepts. In the present syntheses, this conservative impact paradoxically emerged in attitudinal resistance or in attitude change, depending on the specific aspect of the self that was threatened.

Although forewarnings of impending appeals appeared to instigate defensive reactions, it remains plausible that in certain contexts knowledge of another's views can heighten concerns about the impressions conveyed to others and can yield moderation. Cialdini et al. (1973) argued that these impression concerns are most likely to emerge in a specific context, that being when participants expect an immediate discussion on an uninvolving topic with a person who holds opposing views. Two of the preappeal studies (Cialdini et al., 1973; Deaux, 1968) established these conditions in certain cells of their experimental designs. In these conditions, considerable attitude change was apparent toward midscale and the impending partner's position ($d = 0.87$; 95% CI = 0.42, 1.33; $k = 2$). Important for interpreting these attitude shifts as attempts to convey a favorable impression to the discussion partner, the shifts were somewhat smaller in "private" conditions in these studies, in which participants were forewarned and expected to indicate their attitudes privately ($d = 0.29$; 95% CI = −0.15, 0.73; $k = 2$), $Q_B(1) = 3.28$, $p < .08$. Thus, although warning of a counterattitudinal appeal did not appear to motivate attitude change through impression concerns, the expectation of a discussion with another person on an uninvolving topic apparently was successful at doing so.

Another context in which impression motives have been found to direct attitude change is in impression-relevant involvement studies, also called *response involvement* studies (Johnson & Eagly, 1989). Impression-relevant studies have design features that render them inappropriate to test the present hypotheses (e.g., in many studies, the partner's position was not specified and thus would not necessarily challenge recipients' attitudes), and for this reason they were not included in the present review. Yet, given our finding in the prior paragraph that impression motives can be elicited by the anticipation of discussion, additional research is needed to identify the specific conditions under which warnings instigate defense versus impression concerns. Chen et al. (1996) offered the interesting speculation that in everyday life these motives can have complementary effects. That is, impression motives may reinforce the information-processing biases created by defense motives—as likely occurs when people state their opinions on involving social issues to like-minded friends. In general, although impression motives in the forewarning literature have been linked to moderation to easily defensible positions, such motives can conceivably yield a variety of attitudinal outcomes (see Schlenker & Pontari, 2000).

### *Conclusion*

The present review offers guidance for researchers and practitioners interested in resistance techniques. Research on resistance to influence has typically taken a back seat to psychologists' driving concern with attitude change (see Eagly & Chaiken, 1993). As Pfau (1996) complained, "It is ironic, given the central role of persuasion in the public sphere of contemporary society, that there has been so little emphasis on protecting people against influence" (p. 142). The present findings demonstrate how the study of forewarnings can contribute to theoretical understanding of the general motivational forces and cognitive mechanisms by which people maintain and change their attitudes.

The practical importance of warnings as a resistance technique is apparent in the coherent, moderately sized effects that emerged almost uniformly when attitudes were assessed following the appeal. However, warnings did not always convey immunity to the message, and the pattern of warning impact can be used to design warnings to maximize their resistance effect.

The ideal warning to induce resistance would be delivered when people are focused on the topic of the impending appeal, rather than on self-image threats such as appearing gullible, and when people are motivated and able to engage in careful thought about the issue. Although in the present review such careful scrutiny was instigated by factors unrelated to the warning itself (e.g., involving topics), it remains possible that warnings could be framed to achieve these outcomes. For example, warnings could be fashioned not only to alert people to an impending appeal but also to exhort them to think carefully about the issue, much as participants in some of the experiments we reviewed were instructed to list their thoughts. Such warnings that actively engage people with the topic of the appeal would have the added benefit of encouraging the kinds of issue-relevant thought that provide sustained resistance over more than a single occasion.

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