

1 The Door-in-the-Face Influence Strategy: A Random-Effects Meta-Analytic Review

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A random-effects meta-analysis of research concerning the door-in-the-face (DITF) influence strategy provides evidence supporting more confident generalizations about the role of several moderator variables than that provided by previous reviews. Variations in the identity of the requester, the identity of the beneficiary, the prosocialness of the requests, the medium of communication, and the time interval between requests all appear to influence the size of DITF effects; variations in concession size do not. DITF effects are small in absolute terms (with an overall mean r of .10), but not remarkably small in the context of other effect sizes concerning social influence. However, there is substantial variability in DITF effects, even under optimal conditions. The review's findings are not easily reconciled with most proposed explanations of DITF effects, but appear consistent with a guilt-based account.

THE door-in-the-face (DITF) influence strategy is a much-studied means of social influence. Systematic research concerning the DITF strategy began more than 20 years ago, with Cialdini et al.'s (1975) classic work. Two meta-analytic reviews of the DITF literature appeared about a decade later (Dillard, Hunter, & Burgoon, 1984; Fern, Monroe, & Avila, 1986). A good deal of DITF research has appeared following those reviews, and though there has been some discussion of this research area (e.g., Dillard, 1991), no subsequent systematic review has been undertaken.

This chapter reports a meta-analytic review of the DITF research literature. Our broad purpose is to assess the current state of the literature, taking into account the studies undertaken since the last meta-analytic reviews. In doing so, we also hope to address some uncertainties arising from previous reviews,

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to shed light on possible new moderators of DITF effects, and to consider possible explanations for the observed effects.

BACKGROUND

The DITF Strategy

In the DITF strategy, a relatively large initial request is made of a person, which the person declines. Then a subsequent smaller request is made, in the hopes that the person's having declined the initial request (having metaphorically closed the door in the face of the requester) will make the person more likely to comply with the second (target) request. In experimental investigations of the strategy, researchers assess DITF success by comparing target-request compliance rates in a DITF condition with the corresponding compliance rates in a control condition in which participants receive only the target request.

For example, in Cialdini et al.'s (1975, Experiment 1) classic study, people were approached on a campus sidewalk by another student purportedly representing the "County Youth Counseling Program." In the DITF condition, a large initial request was made—that the receiver spend 2 hours a week, for a minimum of 2 years, working as an unpaid volunteer counselor at the County Juvenile Detention Center. No one agreed to this request. The smaller second request was that the receiver serve as an unpaid volunteer chaperone, spending 2 hours one afternoon or evening taking a group of juveniles from the Detention Center to visit the zoo. In the control condition, in which participants heard only the smaller (target) request, 17% consented to serve as a chaperone. In the DITF condition, in which the initial large request had been declined, 50% agreed to chaperone.

Previous Meta-Analytic Findings

The two previous meta-analytic reviews of DITF research reported that the overall observed mean DITF effect (that is, the difference between the target-request compliance in the DITF condition and in a control condition in which only the target request is received) is roughly equivalent to a correlation of .08 (Dillard et al., 1984, p. 471; Fern et al., 1986, p. 150).

These reviews also examined the effects of four specific moderator variables. First, the effects of varying time intervals between the first and second requests were examined by both extant meta-analyses. Each reported that DITF effects were larger when there was no delay between the two requests than when some time elapsed (Dillard et al., 1984, p. 478; Fern et al., 1986, p. 149). Second, the effect of variation in the identity of the requester was studied by Fern et al. (1986, p. 149), who found that DITF effects tended to

be larger when the same person made both requests than when different persons made the two requests. Third, the influence of the prosocialness of the requests was reviewed by Dillard et al. (1984, pp. 478-479). They found that DITF effects were larger when the requests came from prosocial organizations (e.g., civic or environmental groups) than when they came from nonprosocial organizations (e.g., marketing firms). Fourth, the role of the size of the concession made—that is, the size of the drop in request size from the first to the second request—was examined by Fern et al. (1986, p. 149). Their review found that variations in the magnitude of concession were not dependably associated with variations in DITF effect size.

Explaining DITF Effects

Cialdini et al. (1975) advanced what is probably the best-known explanation of DITF effects, the reciprocal-concessions explanation. This explanation proposes that the sequence of requests makes the situation appear to be a negotiation or bargaining situation, and hence a situation in which a concession by one side (the requester's making a smaller second request) is expected to be reciprocated by a concession from the other side (the person's accepting the second request). In fact, Cialdini et al. went so far as to label the procedure the "reciprocal concessions technique."

However, as several commentators have suggested, the reciprocal-concessions explanation does not appear to be entirely satisfactory (see, e.g., Dillard, 1991). In particular, the finding that concession size does not influence DITF effects appears inconsistent with the explanation; if the explanation were true, one would expect that larger concessions would yield larger effects. Additionally, this explanation obviously does not provide an explanation for the finding that DITF effects are larger with prosocial than with nonprosocial requests.

Another proposed explanation invokes perceptual-contrast effects (Miller, Seligman, Clark, & Bush, 1976). The suggestion is that the second request is perceived as smaller than it actually is because of a perceptual contrast with the larger first request. That is, the second request appears less demanding when it is seen against the backdrop of the larger initial request (and hence engenders greater compliance).

But the perceptual-contrast explanation appears incapable of accommodating the previously observed effects of moderator variables. Specifically, it does not explain why, from a perceptual-contrast standpoint, DITF effects should vary depending on whether the same person makes the requests. Nor does this explanation appear to offer any clear account of why DITF effects should be larger with prosocial than with nonprosocial requests. Moreover, direct evidence bearing specifically on the perceptual-contrast explanation gives little support to this account (Abrahams & Bell, 1994; Cantrill & Seibold, 1986; Goldman, McVeigh, & Richterkesing, 1984).

Self-presentational concerns have also been suggested as a possible explanation of DITF effects. This account proposes that rejection of the first request makes receivers concerned that they will be negatively evaluated by the requester (Pendleton & Batson, 1979). But the self-presentation explanation is also difficult to square with the research evidence in hand. As Abrahams and Bell (1994, p. 136) have noted, DF effects have repeatedly been obtained in circumstances in which self-presentational considerations should not be especially strong (as, for instance, in interaction between strangers who are unlikely ever to interact again). Moreover, the initial findings suggesting the plausibility of this account (Pendleton & Batson, 1979) have proved difficult to replicate (Reeves, Baker, Boyd, & Cialdini, 1991), and subsequent direct tests (Abrahams & Bell, 1994) have also failed to confirm expectations of the self-presentation explanation.

A final possible explanation is based on guilt (O'Keefe & Figgé, 1997). The suggestion is that DITF success comes about through a guilt-arousal-and-reduction process, in which rejecting the first request induces guilt in the receiver and accepting the second request reduces that guilt. This explanation appears to be capable of encompassing the moderator-variable effects reported in previous meta-analytic reviews. Specifically, the observed effect for prosocialness is explained as a consequence of persons feeling greater guilt when declining prosocial requests than when declining nonprosocial requests. The apparent time-interval effect is taken to occur because with increased delay between the requests, any induced guilt has greater opportunity to dissipate. The observed identical-requester effect is seen to arise because a second request that comes from a different requester does not offer the same guilt-reduction possibilities as a second request from the same person whose request was just declined.

This explanation is also consistent with current research about guilt. (This literature, addressed at some length by O'Keefe & Figgé, 1997, is only briefly summarized here.) For example, this account harmonizes with current theoretical and empirical understandings of the nature of guilt. By way of illustration: Roseman, Wiest, and Swartz (1994) found that among the reactions distinctively associated with guilt were "thinking that you were in the wrong," "thinking that you shouldn't have done what you did," "feeling like undoing what you have done," "wanting to make up for what you've done wrong," and the like (see p. 215). It is easy to imagine how (for instance) refusing to help troubled children might lead to such feelings and how the second request in the DITF sequence might offer the prospect of making up for what one has done. As another example, Baumeister, Stillwell, and Heatherton (1994) have emphasized that guilt arises from interpersonal transactions and is particularly linked to "suffering that oneself has caused" (p. 246); from such a vantage point, the DITF strategy can be seen to involve an interpersonal transaction in which the refusal of the initial request might involve the infliction of suffering on another person.

Additionally, this explanation appears consistent with extant research findings concerning guilt-based social influence. There are two areas of such research. In the first, researchers have explored the relationship between guilt and compliance with altruistic requests by inducing guilt in participants (by having them inflict harm on another) and subsequently making altruistic requests of them. A number of studies have found that guilt induction can enhance request compliance (e.g., Carlsmith & Gross, 1969; Cunningham, Steinberg, & Grev, 1980; Freedman, Wallington, & Bless, 1967; Konoske, Staple, & Graf, 1979), confirming the role that request compliance can play in guilt reduction (for a more detailed discussion of this literature, see Baumeister et al., 1994, pp. 249-251). The second area of research consists of a small body of work concerning the use of guilt appeals in persuasive messages. Such appeals are parallel to the proposed analysis of the DITF strategy: Both involve an initial arousal of guilt followed by the presentation of a course of action that might mitigate that guilt. Research on guilt-based persuasive appeals suggests that it is indeed possible for message variations to induce varying levels of guilt, that the most intense message contents do not necessarily arouse the greatest guilt, and that greater induced guilt can make for greater persuasiveness (Bozinoff & Ghingoff, 1983; Coulter & Pinto, 1995; Ghingoff & Bozinoff, 1981; Pinto & Priest, 1991; Ruth & Faber, 1988; Yinon, Bizman, Cohen, & Segev, 1976).

The Present Review

The present meta-analysis was motivated by four broad concerns. One was simply a felt need for a new, more thorough review. This need reflects our interest both in reviewing the DITF research that has appeared in the 10 years since the last meta-analytic review appeared and in including unpublished work (which both previous reviews excluded).

Second, the present review was motivated by our interest in addressing some apparent uncertainties surrounding previous reviews. These uncertainties stem primarily from methodological differences between the reviews. One such methodological difference concerns the moderating variables considered in the meta-analyses. No previous review explicitly examined all four of the moderator variables mentioned above. Dillard et al. (1984) considered only the prosocialness of the requests and the time interval between requests; they did not consider variations in requester identity or concession size.¹ Fern et al. (1986) examined the effects of concession size, time delay, and requester variation, but not the prosocialness of the requests. One can have greater confidence in claims about a given moderating variable when independent meta-analyses provide converging evidence about the variable; when those meta-analyses do not examine the same moderators, however, doubt can arise about the conclusions offered. (In a way, thus, there is a meta-analytic parallel to the importance of primary-research replication: Where two or more

independent meta-analytic reviews reach similar conclusions, confidence in those conclusions is naturally strengthened.)²

A second methodological difference involves the unit of analysis employed. As noted by Dillard and Hale (1992, p. 222), the extant reviews used different analytic units: Dillard et al. (1984) used studies as the unit of analysis, whereas Fern et al. (1986) used more fine-grained units. Apparently, when different DITF implementations were reported in a single investigation, Fern et al. analyzed each separately, whereas Dillard et al. combined them.³ On the face of the matter, Fern et al.'s procedure seems preferable. It seems unreasonable to treat two different DITF implementations separately if they appear in two different studies but combine them if they happen to appear in one.

This methodological difference has become important because Fern et al.'s findings have been interpreted as casting doubt on Dillard et al.'s claim that prosocialness is an important moderator of DITF effects. Fern et al. did not explicitly consider prosocialness as a possible moderating factor. However, Dillard and Hale (1992, p. 222) have suggested that, because Fern et al. (1986) reported homogeneous DITF effects without partitioning cases on the basis of prosocialness, Fern et al.'s findings imply that prosocialness does not moderate DITF effects.⁴ Dillard and Hale (1992, p. 229) have noted the possibility that Dillard et al.'s means of aggregating effect sizes resulted in unrepresentative patterns of DITF effects; under this interpretation, Dillard et al.'s finding of a significant moderating role for prosocialness would be called into question. In this circumstance, it would plainly be informative to have a new meta-analytic review that uses Fern et al.'s fine-grained units of analysis and explicitly considers the possible moderating role of prosocialness. Such a review could clarify whether prosocialness is indeed an important moderator of DITF effects.

The third broad concern motivating the present review was an interest in examining the possible effects of two new moderator variables derived from the guilt-based explanation of DITF effects (O'Keefe & Figgé, 1997). The first is the identity of the beneficiary of the two requests. In a sense, when the initial request is refused, there are potentially two injured parties—the person who made the request and the potential beneficiary of the request. Being able to comply with a second request from the same requester, for the same beneficiary, presumably would provide greater guilt reduction than would (to take the opposite extreme) complying with a smaller request from a different requester, for a different beneficiary. Thus the guilt-based account leads one to expect smaller DITF effects when the beneficiary of the requests differs from the initial to the target request.⁵

The second new potential moderator is the medium of communication. Consideration of this moderator is recommended by the conjunction of two lines of research. The first is work concerning communication media, specifically the idea that "social presence" is greater in face-to-face interaction than in mediated communication. The suggestion is that in face-to-face inter-

action, the presence of the other (and hence the interpersonal relationship) is more salient, because such interaction provides more communicative modalities than does (for instance) written or telephonic interaction (Short, Williams, & Christie, 1976; for related work, see Sproull & Kiesler, 1991). The second is work emphasizing the fundamentally interpersonal character of the feeling of guilt. Baumeister et al. (1994) have underscored the close connection between guilt arousal and interpersonal transactions (noting, e.g., that a prototypical cause of guilt is the infliction of harm on a relationship partner; see p. 245). Taken together, these two lines of thought suggest that guilt-arousal processes will be stronger in face-to-face interaction than in other communication media, and thus lead to the expectation that DITF effects will be larger in face-to-face implementations than in implementations using other media.⁶

Finally, this review was motivated by our interest in examining DITF effects using a random-effects analysis rather than a fixed-effects analysis. The situation faced by a meta-analyst with multiple message replications is parallel to that faced by a researcher whose primary-research design contains multiple instantiations of message categories. A researcher can analyze such multiple-message designs by treating messages as either a fixed effect or a random effect. There has been considerable discussion of this choice in the context of primary research on messages. The relevant general principle is that replications should be treated as random when the underlying interest is in generalization. This principle reflects the fact that fixed-effects and random-effects analyses test different hypotheses. For instance, when comparing two group means while treating message replications as fixed, the hypothesis that is tested concerns whether the responses to a fixed, concrete group of messages differ from the responses to some other fixed, concrete group of messages. The parallel random-effects analysis tests whether responses to one category of messages differ from responses to another category of messages (see, e.g., Jackson, 1992, p. 110).⁷

Although perhaps not so prominently discussed, the same choice (between fixed-effects and random-effects analysis) faces meta-analysts. A meta-analysis involves a collection of replications, parallel to the message replications in a multiple-message primary-research design. Similar considerations—including whether the analyst is interested in generalization—bear on the choice between the two analyses (for some discussion, see Erez, Bloom, & Wells, 1996; Jackson, 1992, p. 123; Raudenbush, 1994; Shadish & Haddock, 1994). Most meta-analytic work, in communication and in other fields, has commonly employed fixed-effects analyses, despite the analysts' typically being interested in generalizing beyond the cases at hand. Perhaps it is unsurprising that a National Research Council panel should have concluded that meta-analytic work "would be improved by the increased use of random effects models in preference to the current default of fixed effects models" (National Research Council, 1992, p. 185).

In the present review, our interest is naturally not in the fixed sets of messages (request pairs) that happened to be studied by past investigators, but in the classes of messages of which the studied messages are instantiations, and hence a random-effects analysis was the appropriate choice. In a random-effects analysis, the confidence interval around an obtained mean effect size reflects not only the usual (human) sampling variation but also between-studies variance. This has the effect of widening the confidence interval over what it would have been in a fixed-effects analysis (see Shadish & Haddock, 1994, p. 275; for related discussion, see Raudenbush, 1994, p. 306).

Neither previous DITF meta-analysis employed random-effects analysis. Fern et al. (1986) reported mean effect sizes and associated confidence intervals based on a fixed-effects analysis (see p. 147; see also Hedges, 1982). Dillard et al. (1984) focused on the mean effect size in, and the homogeneity displayed by, a given set (or subset) of effect sizes, following procedures described by Hunter, Schmidt, and Jackson (1982) that do not consider between-studies variation in estimating means and confidence intervals.

There is thus room for justifiable concern about the degree to which previous meta-analytic findings represent conclusions about the specific DITF implementations that have been studied, as opposed to broader generalizations. For example, Fern et al.'s (1986) report that the mean DITF effect size is dependably ($p < .05$) positive when the same person makes both requests, strictly speaking, concerns only the particular DITF implementations reviewed.⁸ Of course, commonly—including earlier in this report—such meta-analytic findings have been (mis)interpreted as representing dependable generalizations beyond the cases studied. A random-effects analysis, however, provides a better basis for dependable generalization, and hence can address concerns about the evidentiary basis of previous conclusions.

METHODS

Identification of Relevant Investigations

Literature search. Relevant research reports were located in various ways, including personal knowledge of the literature, examination of previous reviews and textbooks, and inspection of reference lists in previously located reports. Additionally, we made searches through databases and document-retrieval services using "door-in-the-face," "DITF," "sequential request," and "request sequence" as search bases; these searches covered material through at least October 1, 1996, in PsycINFO, ERIC (Educational Resources Information Center), CARL/Uncover (Colorado Association of Research Libraries), *Current Contents*, and Medline, and through at least June 1, 1996, in *Dissertation Abstracts Ondisk*.

Inclusion criteria. To be included, an investigation had to meet two criteria. First, the study had to compare a DITF condition (in which, following the rejection of an initial request, a smaller target request was made) with a control condition (in which only the target request was made). Excluded by this criterion were review papers and secondary discussions and studies lacking the appropriate experimental comparisons, including studies in which the second request was not smaller than the first (Cialdini et al., 1975, Experiment 3, equivalent-request control; Miller et al., 1976, gaining-only and yielding-only conditions), those in which no explicit second request was made (Foehl & Goldman, 1983; Grace, Bell, & Sugar, 1988, spontaneous-helping conditions), those in which the target request was preceded by multiple initial requests (e.g., Comer, Kardes, & Sullivan, 1992; Goldman & Creason, 1981, two-face condition), and those lacking a relevant control condition (Goldman et al., 1984, Experiment 2; Yip & Kihoi, 1996).

Second, the investigation had to contain appropriate quantitative data pertinent to the comparison of compliance rates across experimental conditions. Excluded by this criterion were studies of effects on other dependent variables (Cantrill, 1986; Goldman, Gier, & Smith, 1981; Pendleton & Batson, 1979; Stahelski & Patch, 1993; Tybout, Sternthal, & Calder, 1983, Experiment 1) and studies for which appropriate quantitative information could not be obtained (e.g., Brechner, Shippee, & Obitz, 1976).

Dependent Variable and Effect Size Measures

Dependent variable. The dependent variable of interest was the relative success of the target request in the DITF condition and the control condition. Most studies elicited only verbal consent to the target request (as opposed, e.g., to behavioral performance of the requested action), and hence when multiple measures of compliance were available, verbal consent measures were employed to maximize consistency.

Effect size measures. Each comparison between a DITF condition and its corresponding control condition was summarized using r as the effect size measure. Effects were coded so that differences favoring DITF conditions were positive in sign and differences favoring control conditions were negative.

Most effect size estimates were derived from dichotomous indices of compliance, variously reported as frequencies (e.g., 15 of 21 participants complied) or proportions (e.g., 71%). All such reports were cast as frequencies (thus eliminating rounding errors associated with reports of proportions) and converted to r using formulas described by Johnson (1989, pp. 104-105).

In some studies, researchers retained DITF-condition participants who accepted (rather than rejected) the initial request, yielding effects based on an imperfect realization of the DITF strategy. When reports included the appropriate information, frequencies (and hence effect size computations)

ment; disagreements were resolved through discussion. The coders subsequently coded all remaining cases independently, with any further disagreements similarly resolved.

Concession size. A separate analysis was undertaken to assess the possible effects of concession size. This moderating variable is distinctive, in that coding cases for concession size is problematic. The most straightforward way of coding each case for concession size would require an assessment (on some universal metric) of the size of each request, which would then provide a basis for classifying cases based on the difference between the values for the first and second requests. But it is not clear that a suitable metric exists or that defensible assessments are obtainable. One cannot now ask the primary-research participants for their assessments of request size, and one might doubt whether request size assessments obtained today would reflect accurately the views of primary-research participants. (For an illustration of the potentially problematic nature of such procedures, see Petty, Cacioppo, Kasmer, & Haugtvedt, 1987, pp. 260-261; Petty, Kasmer, Haugtvedt, & Cacioppo, 1987, pp. 241-242; Stiff, 1986, p. 83; Stiff & Boster, 1987, pp. 252-253. For some general discussion, see Hale & Dillard, 1991, pp. 467-468.) Fern et al. (1986, p. 146) estimated relative request magnitudes in a variety of ways, including dollar ratios (where money was requested), time ratios (where time commitments were involved), and magnitude estimates obtained from marketing research students, though it is not clear whether this last procedure would yield estimates consistent with the other procedures.

An alternative approach is possible, based on studies in which manifestly different-sized initial requests were used with the same target request (or, alternatively, studies in which manifestly different-sized target requests were used with the same initial request). For example, Abrahams and Bell (1994) created two DITF conditions by varying the size of the initial request (labeled "moderate" in one condition and "large" in the other). Because these two conditions used the same target request, they represented different concession sizes: the "moderate" initial request condition involved a relatively small concession, whereas the "large" initial request condition involved a relatively large concession. Thus a comparison of the effect sizes between these two conditions provides information about the effect of concession size variations.

Examining studies that afford such within-study comparisons avoids any problems associated with post hoc assessments of request size. Moreover, this way of proceeding helps to isolate the effect of concession size: Because the evidence is derived from within-study comparisons, each obtained comparison reflects similar procedures and circumstances with respect to other possible variables.

Every study providing such a within-study comparison was identified and used in a separate analysis.⁹ The comparison of interest concerns how DITF effects (expressed as *rs*) vary between two conditions (namely, larger and smaller concessions), and thus required a distinctive effect size index. The

were adjusted in such cases to include only participants who rejected the initial request.

When multiple control conditions were available, the effect size was based on the comparison that best isolated the effect of the DITF strategy. For example, Wang, Brownstein, and Katzev (1989) had three different "control" conditions, which varied in exactly how the target request was made; the effect size was based on the comparison of the DITF condition to the "standard control" condition, because the second request in the DITF condition best matched the request made in that control condition. Similarly, Tybout's (1978) several "control" conditions differed in various ways from the DITF condition (e.g., with respect to the timing of questionnaires), whereas the "straight persuasion" condition differed only in its lack of a DITF implementation (and hence, because it isolated the effect of the DITF strategy, it provided the appropriate comparison).

Each of us computed effect sizes independently, and we subsequently resolved any discrepancies through discussion.

Independent Variables

Requester variation. Cases were coded for whether the two requests were made by the same person.

Beneficiary variation. Cases were coded for whether the two requests had the same beneficiary, defined as the person or persons who would benefit from the request's being fulfilled.

Prosocialness of requests. Each request was classified as prosocial or nonprosocial. Prosocial requests were those that, if fulfilled, would presumably benefit society at large, directly or indirectly (such as requests for donations to charitable organizations, environmental groups, or civic organizations), or would represent favors to individuals (such as requests for favors from strangers). Nonprosocial requests were requests not meeting these criteria; the exemplary forms of nonprosocial requests were those from commercial (profit-seeking) enterprises. Based on these codings, cases were classified as either using two prosocial requests or not using two prosocial requests (this latter classification thus included cases in which one or both of the requests were nonprosocial).

Medium. The medium of communication used for each request was recorded. All cases involved either face-to-face interaction or telephone communication and were classified correspondingly.

Time interval. The length of time that elapsed between the first and second requests in the DITF condition was recorded, with cases classified as having either no delay (cases in which no more than 5 minutes elapsed between requests) or delay. Delays ranged from 2 to 14 days.

Coding reliabilities. Two coders independently classified 10 randomly selected cases for these five variables, with 96% average intercoder agree-

were combined and reported as "Cann et al. (1975) no-delay"; data from the second pilot experiment and from the main experiment in Cantrill (1985) were combined and reported as "Cantrill (1985)"; data from Experiment 1 and Experiment 3 in Cialdini et al. (1975) were combined and reported as "Cialdini et al. (1975) Experiments 1 and 3"; data from the moderate-request conditions in Experiments 1 and 2 in Even-Chen, Yinon, and Bizman (1978) were combined and reported as "Even-Chen et al. (1978) moderate"; data from Mowen and Cialdini (1980) Study 1, large-request condition, and Study 2, 15-minute-same condition, were combined and reported as "Mowen & Cialdini (1980) large"; data from Patch (1986, consulting-source condition), Patch (1988), and Dillard and Hale (1992, consulting-source condition) were combined and reported as "Patch consulting"; data from Patch (1986, nonprofit-source condition) and Dillard and Hale (1992, nonprofit-source condition) were combined and reported as "Patch nonprofit"; data from Experiments 1 and 2 in Reeves et al. (1991) were combined as appropriate and reported as "Reeves et al. (1991) extremely large" or "Reeves et al. (1991) moderately large" (see Table 1.1).

When multiple estimates were cumulated, the preferred means of cumulation was to combine the raw frequencies; for example, if the DITF compliance rates for a given request pair were 20/35 in one investigation (i.e., of 35 participants, 20 complied with the target request) and 15/25 in a second, the effect size was computed based on a DITF compliance rate of 35/60 (compared with similarly combined control group frequencies).¹⁰ We used this same procedure when collapsing across irrelevant experimental manipulations within a single investigation.

In some cases, the same primary data served as the basis for multiple reports. Whenever a given investigation was reported in more than one outlet, it was treated as a single study and analyzed accordingly. The same research was reported (in whole or part) in Bell, Abrahams, Clark, and Schlatter (1995) and Bell, Abrahams, Clark, and Schlatter (1996); in Cantrill (1985), Cantrill (1991), and Cantrill and Seibold (1986), recorded here under Cantrill (1985); in Cialdini (1975) and Cialdini and Ascani (1976), recorded here under the latter; in Hayes (1982) and Hayes, Dwyer, Greenwalt, and Coe (1984), recorded here under the latter; in Nawrat (1989) and Nawrat (1993); in Reingen (1977) and Reingen (1978); and in Schwarzwald, Raz, and Zivbel (1979) and Schwarzwald, Zivbel, and Raz (1980), recorded here under the former.

Random-effects analysis. For the primary analysis, the individual correlations (effect sizes) were initially transformed to Fisher's z s; the z s were analyzed using random-effects procedures described by Shadish and Haddock (1994), with results then transformed back to r . We employed a random-effects analysis in preference to a fixed-effects analysis because of our interest in generalizing across request pairs.

relevant effect size index is that for a comparison of two correlations; Cohen's q (the difference between the z -transformed r s) provides such an index (Cohen, 1988, p. 110). This index was computed such that positive values reflected larger DITF effects with larger concessions; negative values indicated smaller DITF effects with larger concessions.

Analysis

Unit of analysis. In our consideration of how to analyze the present collection of studies, attention to the particular requests employed was important. Every analyzed study contains a comparison of the success of a target request under two conditions (preceded, or not, by the initial rejected request), and it might be thought that one could simply straightforwardly derive an effect size measure for each study. But some studies have more than one initial/target request pair, and some initial/target request pairs are used in more than one study. If one is interested in generalizing across messages (that is, request pairs), the common meta-analytic procedure of treating each study as providing one effect size estimate is unsatisfactory.

Thus, in the present analysis, the fundamental unit of analysis was the request pair (that is, the pair composed of a target request and its corresponding initial request). A measure of effect size was recorded for each distinguishing request pair found in the body of studies. For example, a study reporting separate comparisons between DITF and control conditions for multiple different initial/target request pairs contributed multiple observations (e.g., Miller, 1974), whereas a study with a single request pair contributed only one. Similarly, a study with a single target request contributed multiple observations if comparisons were available for different initial requests (e.g., Wang et al., 1989).

Where a study employed an experimental manipulation corresponding to an independent variable in the current report, effect sizes were computed separately for the relevant conditions. For example, Patch (1986) ascribed the requests to either a nonprofit (prosocial) organization or a consulting company (nonprosocial); separate effect sizes were recorded for these two conditions. When an experimental manipulation not germane to this review was employed (e.g., formal versus informal clothing; Williams & Williams, 1989), effect sizes were computed collapsed across such variations.

Usually, a given request pair was used only in a single investigation, and hence only one effect size estimate was associated with the pair. But when a request pair was used in more than one study, there would be several estimates of the effect size associated with that request pair. These multiple estimates were cumulated into a single summary estimate before inclusion in the analysis.

Such cumulation occurred in the following cases: data from Experiment 1 (no-delay condition) and Experiment 2 in Cann, Sherman, and Elkes (1975)

TABLE 1.1
Continued

Study	r	n	Codings ^a
Mowen & Cialdini, 1980			
Study 1 very large	.127	128	1/1/2/1/1
Study 2 10-minute same	.000	72	1/1/2/1/1
Study 2 10-minute different	-.143	72	1/1/2/1/1
Study 2 15-minute different	-.030	72	1/1/2/1/1
large	.152	200	1/1/2/1/1
Nawrat, 1989, 1993			
sweater	.286	70	1/1/1/1/1
fiancée	.059	76	1/1/1/1/1
Oliver, 1984			
prosocial low-cost	.000	40	1/1/1/2/1
prosocial high-cost	-.109	40	1/1/1/2/1
commercial low-cost	-.152	40	1/1/2/2/1
commercial high-cost	-.066	40	1/1/2/2/1
Patch consulting	.044	324	1/1/2/2/1
Patch nonprofit	.202	231	1/1/1/2/1
Reeves et al., 1991			
extremely large	.285	120	1/1/1/1/1
moderately large	.220	120	1/1/1/1/1
Reingen, 1977, 1978	.065	128	1/1/1/1/1
Reingen & Kernan, 1977	-.147	84	2/2/2/2/2
Reingen & Kernan, 1979	-.139	196	1/1/2/2/1
Rogers, 1976			
Experiment 1 one-requester	-.051	40	1/1/1/2/1
Experiment 1 two-requester	-.168	40	2/2/1/2/1
Experiment 2	.168	120	1/1/2/2/1
Experiment 3 one-requester	-.168	80	1/1/1/2/1
Experiment 3 two-requester	-.132	80	2/2/1/2/1
Schwarzwald et al., 1979			
20-10	.101	40	1/1/1/1/1
30-10	.436	40	1/1/1/1/1
40-10	.436	40	1/1/1/1/1
50-10	.314	40	1/1/1/1/1
60-10	.000	40	1/1/1/1/1
100-10	-.152	40	1/1/1/1/1
20-15	.420	40	1/1/1/1/1
30-15	.420	40	1/1/1/1/1
40-15	.329	40	1/1/1/1/1
50-15	-.253	40	1/1/1/1/1
60-15	-.451	40	1/1/1/1/1
100-15	-.503	40	1/1/1/1/1
30-20	.408	40	1/1/1/1/1
40-20	.302	40	1/1/1/1/1
50-20	.101	40	1/1/1/1/1
60-20	.000	40	1/1/1/1/1
100-20	-.218	40	1/1/1/1/1

TABLE 1.1
Cases Analyzed

Study	r	n	Codings ^a
Abrahams & Bell, 1994	.362	91	1/1/1/1/1
moderate	.441	91	1/1/1/1/1
large	.000	112	1/1/1/1/1
Bell et al., 1995, 1996	.093	48	1/1/1/1/1
Brownstein & Katzev, 1985	.091	38	1/1/1/1/1
Burger, 1986, Experiment 7			
Cann et al., 1975			
delay	-.218	47	1/1/1/2/2
no-delay	.366	87	1/1/1/2/1
Cantrill, 1985			
donation-effort	.378	121	1/1/1/1/1
donation-novelty	.438	83	1/1/1/1/1
elderly-effort	.399	117	1/1/1/1/1
elderly-novelty	.164	114	1/1/1/1/1
Cialdini & Ascani, 1976	.178	126	1/1/1/1/1
Cialdini et al., 1975			
Experiment 2 rejection-moderation	.236	39	1/2/1/1/1
Experiment 2 two-requester	-.258	38	2/2/1/1/1
Experiments 1 and 3	.278	96	1/1/1/1/1
Collins & Brady, 1994	-.083	240	1/1/2/1/1
Crano & Sivacek, 1982	-.120	61	2/2/1/2/2
Dillard & Hale, 1992, lobbying	.148	160	1/1/2/2/1
Even-Chen et al., 1978			
Experiment 1 large	.143	116	1/1/1/1/1
Experiment 2 large	.253	67	1/1/1/1/1
moderate	-.190	183	1/1/1/1/1
Foss & Dempsey, 1979	.127	59	2/1/1/2/2
Experiment 2	.002	62	2/1/1/1/2
Experiment 3			
Goldman, 1986			
moderate	.172	152	1/1/1/2/1
target	.211	152	1/1/1/2/1
Goldman & Creason, 1981	.315	64	1/1/2/2/1
Goldman et al., 1984, Experiment 1	.207	100	1/1/1/1/1
Grace et al., 1988	.186	30	1/2/1/1/1
Harari et al., 1980	.227	94	1/1/1/2/1
Hayes et al., 1984	-.123	607	1/1/1/2/1
Katzev & Brownstein, 1989			
math test	-.167	60	1/1/1/1/1
survey	.064	57	1/1/2/2/1
Miller, 1974			
time-time	.319	44	1/1/1/2/1
money-time	.047	44	1/1/1/2/1
money-money	.321	44	1/1/1/2/1
time-money	.189	44	1/1/1/2/1
Miller et al., 1976	.478	38	1/1/1/2/1

TABLE 1.1
Continued

Study	r	n	Codings ^a
Shanab & Isonio, 1980	.000	80	1/1/1/1/2
Shanab & O'Neill, 1979	.249	80	1/1/1/1/1
Shanab & O'Neill, 1982			
large	.053	80	1/1/1/1/1
very large	.425	80	1/1/1/1/1
Snyder & Cunningham, 1975	-.243	54	2/2/1/2/2
Tybout, 1978, Experiment 1	-.158	120	1/2/2/1/1
Tybout et al., 1983, Experiment 4	.000	105	1/1/2/2/1
Wang et al., 1989			
\$10	.009	75	1/1/1/1/1
\$25	.105	74	1/1/1/1/1
\$50	-.098	77	1/1/1/1/1
Williams & Williams, 1989	.000	136	1/1/1/1/1

a. The coding judgments, in order, are requester variation (1 = same requester, 2 = different requesters), beneficiary variation (1 = same beneficiary, 2 = different beneficiaries), prosocialness (1 = prosocial, 2 = nonprosocial), communication medium (1 = face-to-face, 2 = telephone), and time interval (1 = no delay; 2 = delay).

RESULTS

Overall DITF Effects

Effect sizes were available for 88 distinct request pairs. The number of participants was 7,780, with study sample sizes ranging from 30 to 607. Details on each included case are contained in Table 1.1.

Across all 88 cases, the random-effects weighted mean correlation was .097. The 95% confidence interval for this mean was .049, .144, indicating a dependably positive overall DITF effect.

Moderating Factors

Table 1.2 provides a summary of the results concerning the effects of the five main moderating variables, considered individually. For each variable, the results indicate an important moderating role. The mean effects are dependably positive if the same person makes both requests, if the requests have the same beneficiary, if the requests are prosocial, if the requests are made face-to-face, or if there is no delay between the requests. The mean effects are not dependably positive, however, if different persons make the requests, if the requests have different beneficiaries, if the requests are not prosocial, if the requests are made over the telephone, or if there is a delay between the requests. Indeed, effect sizes are significantly ($p < .05$) larger when the same person makes both requests (as opposed to when different persons make the requests), when the requests have the same beneficiary (as

TABLE 1.2
Summary of Results

	k	r	95% CI	Q (df)
All cases	88	.097	.049, .144	311.3 (87)**
Same requester	80	.116	.067, .166	287.2 (79)**
Different requesters	8	-.110	-.202, -.018	6.0 (7)
Same beneficiary	79	.117	.068, .167	279.2 (78)**
Different beneficiaries	9	-.110	-.223, .003	9.2 (8)
Prosocial requests	71	.120	.064, .176	265.0 (70)**
Nonprosocial requests	17	.012	-.052, .077	33.1 (16)*
Face-to-face	57	.120	.057, .182	202.8 (56)**
Telephone	31	.055	-.015, .125	96.3 (30)**
No time interval	81	.112	.062, .161	293.2 (80)**
Time interval	7	-.080	-.179, .018	6.1 (6)
Optimal moderator values	45	.152	.078, .226	164.0 (44)**
Suboptimal moderator values	43	.039	-.016, .094	115.6 (42)**

* $p < .01$; ** $p < .001$.

opposed to having different beneficiaries), and when there is no delay between the two requests (as opposed to some time interval between them).

As a way of summarizing the effects of these five moderating factors, we classified cases into two categories: cases in which the moderating variables had values that would be expected to maximize the effect size (namely, two prosocial requests having the same beneficiary, made face-to-face by the same requester with no delay between the requests) and cases in which one or more of the moderating variables had a less-than-optimal value.

As indicated in Table 1.2, in 45 cases, all five moderating factors had optimal values, with a mean r across these cases of .152; this mean correlation was dependably ($p < .001$) positive. In 43 cases, at least one of the five moderating factors had a less-than-optimal value; the mean r across these cases was .039 and was not significantly different from zero. Thus, although the 95% confidence intervals for these two means overlap, only under optimal conditions is the mean effect dependably positive.

With respect to concession size, 15 distinguishable comparisons were available between DITF effect sizes obtained with relatively smaller and relatively larger initial requests (see Table 1.3). The n -weighted mean q (difference between z -transformed r s) was .005. Because meta-analytic techniques for handling effect size indices such as q appear not well developed, confidence intervals were not constructed. However, the obtained mean q is

literally nearly zero and is obviously substantially smaller than the .10 q value conventionally labeled "small" by Cohen (1988). Of the 15 comparisons, 7 were positive (indicating larger DITF effects with larger concessions) and 8 were negative (indicating smaller DITF effects with larger concessions).

Homogeneity of Effects

There is substantial heterogeneity in the observed effects. Even under the very specific circumstance identified by the conjunction of optimal values for the five key moderator variables, there is significant variability in the set of effect sizes. Homogeneous effects do appear to obtain within some categories; for example, the set of effect sizes involving delay between the two requests is apparently homogeneous. However, the categories within which the null hypothesis of homogeneity is not rejected are also categories that contain relatively few cases, and hence may have low power for detecting heterogeneity.

DISCUSSION

What emerges from this review is a somewhat more complex but also better-evidenced picture of DITF effects than that afforded by previous reviews. We discuss, in turn, the findings concerning moderators of DITF effects, the magnitude and robustness of DITF effects, the explanation of DITF effects, directions for future DITF research, and some methodological aspects of the current review.

Moderator Variables

Individual moderator variables. Previous meta-analytic reviews suggested that three particular moderator variables—requester identity variations, prosocialness of requests, and time interval between requests—play significant roles in influencing DITF effects. However, no previous review offered an appropriate (random-effects) analysis that might underwrite generalizations about these moderators. The current findings, however, do provide a more sound evidentiary basis for these previous suggestions. Specifically, when different persons make the two requests, when the requests are not prosocial, or when a time interval intervenes between the two requests, the mean DITF effect is not significantly different from zero; by contrast, when the same person makes both requests, when the requests are prosocial, or when no time interval separates the requests, the mean DITF effect is dependably positive.

The current evidence for the role of prosocialness is especially noteworthy, given the doubts that Dillard and Hale (1992) have raised. In the present

TABLE 1.3
Concession Size Cases

	q	n
Abrahams & Bell, 1994	.094	182
smaller concession ("moderate") $r = .362$		
larger concession ("large") $r = .441$		
Even-Chen et al., 1978, Experiment 1	.311	232
smaller concession ("moderate") $r = -.165$		
larger concession ("large") $r = .143$		
Even-Chen et al., 1978, Experiment 2	.494	134
smaller concession ("moderate") $r = -.231$		
larger concession ("large") $r = .253$		
Goldman, 1986	-.040	304
smaller concession ("target") $r = .211$		
larger concession ("moderate") $r = .172$		
Mowen & Cialdini, 1980, Study 1	.031	256
smaller concession ("large") $r = .096$		
larger concession ("very large") $r = .127$		
Mowen & Cialdini, 1980, Study 2 same	-.256	144
smaller concession ("15 minute") $r = .251$		
larger concession ("10 minute") $r = .000$		
Mowen & Cialdini, 1980, Study 2 different	-.114	144
smaller concession ("15 minute") $r = -.030$		
larger concession ("10 minute") $r = -.143$		
Oliver, 1984, prosocial	.109	80
smaller concession ("high-cost") $r = -.109$		
larger concession ("low-cost") $r = .000$		
Oliver, 1984, commercial	-.087	80
smaller concession ("high-cost") $r = -.066$		
larger concession ("low-cost") $r = -.152$		
Reeves et al., 1991	.069	240
smaller concession ("moderately large") $r = .220$		
larger concession ("extremely large") $r = .285$		
Schwarz et al., 1979, 20-10 vs. 100-10	-.255	80
smaller concession ("20-10") $r = .101$		
larger concession ("100-10") $r = -.152$		
Schwarz et al., 1979, 20-15 vs. 100-15	-1.001	80
smaller concession ("20-15") $r = .420$		
larger concession ("100-15") $r = -.503$		
Schwarz et al., 1979, 30-20 vs. 100-20	-.655	80
smaller concession ("30-20") $r = .408$		
larger concession ("100-20") $r = -.218$		
Shanab & O'Neill, 1982	.401	160
smaller concession ("large") $r = .053$		
larger concession ("very large") $r = .425$		
Wang et al., 1989	-.107	152
smaller concession ("10") $r = .009$		
larger concession ("50") $r = -.098$		

review, the unit of analysis was the request pair, quite unlike Dillard et al.'s procedure in which the study was the unit of analysis. Despite this methodological difference, the present results indicate that DITF effects are indeed significantly positive when prosocial requests are made and not dependably positive otherwise.

These results also provide evidence consistent with Fern et al.'s (1986) failure to find any relationship between concession size and DITF effect size. Notably, this confirmation was obtained in a manner quite different from that employed by Fern et al.; such diversity in method provides even greater confidence in this null result.

Additionally, this analysis has identified two new moderating factors: the identity of the beneficiaries of the request and the medium of communication. When the two requests have different beneficiaries or when the requests are made over the telephone rather than face-to-face, the mean DITF effect is not significantly different from zero, whereas DITF effects are dependably positive when the requests have the same beneficiary or when requests are made face-to-face.

However, a complication arises in the consideration of beneficiary-variation and requester-variation effects, namely, that beneficiary variations and requester variations are largely confounded. In 83 of the 88 cases analyzed here, the requester and beneficiary were either both constant across the requests ($k = 77$) or both varied ($k = 6$). In only 5 cases was one varied while the other was constant: In 3 cases the same person made the two requests but the beneficiaries varied, and in 2 cases the beneficiary was identical but different persons made the two requests.

This confounding naturally makes for interpretive uncertainties concerning the distinctive effects of these two moderating factors. It may be that beneficiary variations are solely responsible for the observed effects, and hence that the effects previously attributed to requester variations are in fact due to beneficiary variations; it may be that requester variations are solely responsible for the observed effects; or it may be that both factors play a role.

The possibility that both factors play a role is supported by an examination of the contrast between cases in which both the requester and the beneficiary differ from the first request to the second ($k = 6$) and cases in which one varies but the other does not ($k = 5$). When both the requester and the beneficiary are different in the two requests, the mean r is $-.168$ and is dependably negative (the 95% confidence interval is $-.274, -.061$). When one varies but the other does not, the mean r is $.024$ (the 95% confidence interval is $-.106, .154$). Despite the very small number of cases, these two means are nearly significantly different ($p < .12$). In short, it appears that both requester variations and beneficiary variations influence DITF effects, such that effects are especially reduced when both the requester and the beneficiary change from the first request to the second.

Joint operation of moderators. It is worthwhile to consider the joint operation of the five centrally relevant moderator variables (requester variation, beneficiary variation, prosocialness of requests, communication medium, and time interval between requests). This package of variables is obviously informative about the conditions under which DITF effects are to be expected. There is a notable difference between the mean DITF effect obtained under optimal conditions (roughly .15) and that obtained under suboptimal conditions (roughly .04). Indeed, the joint operation of the five key moderating factors is such as to make positive DITF effects unlikely to occur under suboptimal circumstances. When any of the five key moderators was not optimal for DITF effects, the obtained mean effect size was not significantly positive.

At the same time, no value of any one of the five key moderator variables is apparently absolutely necessary to produce DITF effects. Positive (though not necessarily dependably positive) DITF effects have been observed when different persons made the two requests (Foss & Dempsey, 1979, Experiment 2), when the two requests were not both prosocial (Mowen & Cialdini, 1980, Study 1, very large condition), when a delay intervened between the requests (Foss & Dempsey, 1979, Experiment 2), when the two requests had different beneficiaries (Grace et al., 1988), and when the requests were not made face-to-face (Cann et al., 1975, no-delay condition). That is to say, it may be possible to produce DITF effects under suboptimal conditions. Indeed, significantly positive DITF effects have been observed with implementations using nonprosocial requests (Mowen & Cialdini, 1980, large) and with implementations using telephoned requests (e.g., Harari, Mohr, & Hosey, 1980). Similarly, even apparently optimal conditions are no guarantee of positive DITF effects. A number of cases with optimal conditions nevertheless produced negative (though not necessarily dependably negative) effect sizes (Even-Chen et al., 1978, moderate; Katzev & Brownstein, 1989, math test; Schwarzwald et al., 1979, 100-10, 50-15, 60-15, 100-15, and 100-20 conditions; Wang et al., 1989, \$50). Thus, although one cannot confidently specify either necessary or sufficient conditions for obtaining DITF effects, it is nevertheless possible to identify a number of important moderating factors that influence the size of DITF effects.

Magnitude and Robustness of DITF Effects

Magnitude. DITF effects are genuine, as evidenced by the dependably positive overall mean effect size and by the observation of individual cases in which dependably positive effects obtain. But these effects may not appear to be especially large. The overall mean correlation is about .10, and even under optimal conditions the mean correlation is only about .15. Under anything less than optimal conditions, the mean drops substantially, to about .04.

But this appearance may be deceiving. For example, a correlation of .15 corresponds to the difference between a control condition in which 42.5% comply with the target request and a DITF condition in which 57.5% comply (which is a 35% increase in compliance). Even a correlation of only .10 represents the difference between a control condition with 45% compliance and a DITF condition with 55% (a 22% increase in compliance). Thus, although the obtained mean correlations may seem minute when judged against the magnitude of correlations expected in (for example) inter-item reliability assessments, in fact the observed effects are not entirely trivial. (For a general discussion of effect sizes, see Rosenthal, 1994; for some specific discussions concerning the understanding of effect size magnitudes, see Abelson, 1985; Cooper, 1981; Haase, Ellis, & Ladany, 1989.)

Moreover, it is not clear that it is realistic to expect dramatically larger mean effects in social influence research. Consider, for example, that the mean difference in persuasive effectiveness (expressed as r) between one-sided and two-sided messages has been reported as .04—increasing to .08 if only refutational two-sided messages are considered (Allen, 1991). The mean correlation between fear-appeal manipulations and behavior has been estimated variously as .10 (Boster & Mongeau, 1984) and .17 (Sutton, 1982), and that between fear-appeal manipulations and attitude as .21 (Boster & Mongeau, 1984) and .18 (Sutton, 1982). The mean persuasive effect (as r) of variations in the timing of communicator identification has been reported as .26 (O'Keefe, 1987). Segrin's (1993) review of the influence of various nonverbal elements on compliance reported mean correlations of .23 (for gaze), .21 (touch), .18 (interpersonal distance), and .16 (apparel).

In short, other meta-analytic estimates of the effects of various factors on the success of social influence efforts should lead one to expect effects not larger than ones equivalent to a correlation of .30. Cohen's (1988) diffidently offered conventional labels would have a correlation of .50 be a "large" effect, .30 a "medium" effect, and .10 a "small" effect. By this yardstick, factors affecting the outcomes of social influence efforts appear (on the basis of meta-analytically derived estimates) to have no better than small to medium-sized mean effects. Understood in this context, the observed magnitude of DITF effects is not remarkably small.

One should also remember that it is possible in individual implementations to obtain rather impressive DITF effects: effect sizes in excess of .40 were obtained by Abrahams and Bell (1994, large), Cantrill (1985, donation-novelty), Miller et al. (1976), Schwarzwald et al. (1979, 30-10, 40-10, 20-15, 30-15, and 30-20 conditions), and Shanab and O'Neill (1982, very large). A correlation of .40 is equivalent to the difference between a control condition that produces 30% compliance and a DITF condition that yields 70% compliance (which is a 133% increase in compliance). Although DITF effects this large are rare (occurring in about 10% of the cases), a number of different

investigators have produced them—although, notably, almost always under conditions that were optimal with respect to the five key moderator variables.

Robustness. Even though there is a significantly positive overall DITF effect, there is also substantial heterogeneity among the effect sizes. Indeed, even under optimal conditions, there is significant variability among the observed effect sizes. What this suggests is that, even though there may be a dependably positive DITF effect under these optimal conditions, one should also expect quite a bit of variability in the success of DITF implementations even in optimal circumstances.

One reaction to such heterogeneity might be to suppose that the phenomenon in question must be poorly understood. Such a reaction would be encouraged by a supposition that the point of meta-analytic research is the establishment of sets of homogeneous effect sizes; from this vantage point, heterogeneity in a collection of effect sizes is something to be squeezed out by ever-finer effect size categorization (see, e.g., Dillard et al., 1984, pp. 465-466). But one might alternatively take heterogeneity to be a fact about the phenomenon. We are accustomed to thinking about the mean effect size as a fact about a given phenomenon, but we might consider effect size variability in a similar way. So, for example, two different compliance techniques might vary not only in their mean effects but in the degree of variability to be expected across implementations. That variability, like the mean effect, is simply one aspect of the phenomenon.

Considered in this way, one interesting fact about DITF effects is their heterogeneity. DITF effects do not appear to be the sort of thing easily produced on demand; there is no hard-and-fast set of requirements for obtaining DITF effects. On the contrary, DITF effects look to be relatively fragile rather than robust. After all, even when experimental conditions are optimized with respect to five demonstrably relevant moderating variables, the size of the observed DITF effect can still vary greatly. In fact, even under apparently optimal conditions, dependably *negative* DITF effects have been observed (e.g., Even-Chen et al., 1978, moderate).

Explaining DITF Effects

Any satisfactory explanation of DITF effects will need to be able to account for the roles of observed moderator variables. The present review, by providing stronger evidence than was previously available about the operation of suspected moderators, thus specifies constraints to be met by any proposed explanation.

The best-known DITF explanation, the reciprocal-concessions account, receives little support in these findings. The reciprocal-concessions explanation leads to the expectation that larger concessions will produce larger DITF effects. However, in this review, as in Fern et al.'s (1986) review using

different procedures, we did not find larger concessions to be dependably associated with larger DITF effects.

Moreover, these results suggest roles for two other moderators that are not obviously encompassed by a reciprocal-concessions account. The first is the prosocialness of the requests. There is no obvious reason concessions involving prosocial requests should receive greater reciprocity than concessions involving nonprosocial requests. (Prosocial requests might produce greater compliance than nonprosocial requests, but the issue here is different; it concerns not the difference between prosocial and nonprosocial request compliance rates, but the difference between prosocial and nonprosocial request DITF effect sizes.) Indeed, if anything, given a general familiarity with the existence of bargaining in commercial enterprises (e.g., labor-management negotiation), the reciprocal-concessions account might expect that nonprosocial requests would more easily be perceived as fitting a bargaining/negotiation frame (compared with nonprosocial requests), and hence might predict *larger* DITF effects for nonprosocial requests than for prosocial requests.

The other moderator variable that seems troublesome for the reciprocal-concessions explanation is variations in the identity of the beneficiary. The reciprocal-concessions account can easily explain why variations in the identity of the requester will influence DITF effects (because with different requesters, the pressure to reciprocate a concession vanishes), but it does not explain why changes in the beneficiary of the request should have the observed effects.

These results also offer little confirmation for the perceptual-contrast and self-presentation explanations. The perceptual-contrast explanation does not appear to provide any reason why the prosociality of the requests should influence DITF effect sizes. And neither of these explanations appears to offer any obvious account of why beneficiary variations should influence DITF effects.

By contrast, these results do provide some evidence supporting the usefulness of a guilt-based analysis of DITF effects. First, this review has provided better evidence for the operation of previously suggested moderator variables than the expected effects of which can be explained by a guilt-based analysis. As discussed previously, the guilt-based account explains the role of requester variations by suggesting that a second request that comes from a different requester does not offer the same guilt-reduction possibilities as a second request from the same person whose request was just declined. The observed effect for prosocialness is explained as a consequence of persons' feeling greater guilt when declining prosocial requests than they feel when declining nonprosocial requests. And the observed time-interval effect is seen to arise because with increased delay between the requests, any induced guilt has a greater opportunity to dissipate.

Second, two new moderating variables suggested by the guilt-based analysis (beneficiary variations and communication medium) are apparently re-

lated to the size of DITF effects. However, these findings do not (and cannot) represent decisive evidence for a guilt-based explanation. These same moderating factors might be explained equally well in other ways; for example, the reciprocal-concessions explanation might suggest that face-to-face interactions would engender greater pressure toward reciprocity than would telephone interactions. As several commentators have observed, when meta-analytic research identifies relevant moderating factors, it is not uncommon for multiple plausible models to be consistent with the data in hand (e.g., Cook et al., 1992, pp. 181-184; Hale & Dillard, 1991). Thus these findings concerning new moderator variables do not uniquely support a guilt-based analysis. Nevertheless, a guilt-based analysis did lead to the identification of these new moderating factors, and the present results are entirely consistent with the expectations of a guilt-based account.

In any event, the identification of these new moderator variables places additional constraints on any satisfactory explanation of DITF phenomena, as any suitable explanation will need to address how and why the various moderator variables produce their observed effects. And the guilt-based explanation does appear well suited to accommodate the observed pattern of effects and particularly the apparent role of beneficiary variations. Moreover, the apparently fragile character of DITF effects seems consistent with the evanescent nature of an emotional state such as guilt.

Future Research

Future DITF research might usefully examine more closely the possible role of guilt in DITF processes. Two broad lines of work recommend themselves. The first is the examination of the effects of additional possible moderator variables that might be derived from the guilt-based account. For example, O'Keefe and Figgé (1997) have suggested that, because guilt arises from a person's holding him- or herself responsible for some negatively evaluated outcome, anything that reduces a person's sense of responsibility for first-request refusal will reduce the amount of guilt experienced (and correspondingly affect the success of the DITF strategy). As just discussed, however, there is no reason to suppose that any such possible moderating variable would be uniquely associated with guilt-based processes.

A second, and empirically more compelling, avenue for research would involve the direct assessment of guilt to see whether guilt arousal and reduction patterns obtain in the ways expected by a guilt-based explanation. As several commentators have noted, it is important to have direct assessment of whatever mediating state is posited by a DITF explanation (Abrahams & Bell, 1994; Dillard, 1991). In the specific case of guilt various assessment procedures are possible, but assessing guilt is not entirely unproblematic (see Bozinoff & Ghingold, 1983; Kugler & Jones, 1992; O'Keefe & Figgé, 1997).

No matter the specific direction of future primary research, it might be noticed that the present results underscore the difficulties and delicacy of producing satisfactory primary-research evidence concerning DITF and any proposed mediating state. DITF effects are elusive, there is no recipe to guarantee their production, and they are commonly of sufficiently small magnitude as to make power requirements potentially daunting (particularly for designs seeking comparisons between different DITF conditions).¹¹ Useful primary research on DITF effects will plainly require a skilled experimental hand.

Some Methodological Considerations

The present meta-analysis is distinctive in some methodological respects, most notably in using a random-effects analysis. The same considerations that underwrite the choice of random-effects analyses in primary-research designs that contain message replications underwrite the choice of random-effects analyses in meta-analytic research covering message replications. Fixed-effects analyses (e.g., of the sort reported by Fern et al., 1986) cannot provide evidence bearing on general claims about classes of messages. Where meta-analytic work in communication is motivated by an interest in generalizing about message categories, random-effects analyses should be preferred (with message as the unit of analysis).

But the present analysis is also distinctive in reporting confidence intervals around means. Faced with a set of effect sizes, many meta-analysts in communication appear concerned with reporting whether the collection of effect sizes is homogeneous and what the mean effect size is. The assumption seems to be that if a set of effect sizes is homogeneous, then the observed mean effect size is the population effect size; this assumption is mistaken, however. A set of effect sizes can be homogeneous and have a nonzero mean, but that mean can still not be significantly different from zero.¹² (For an example, see the DITF cases with some time interval between requests, as summarized in Table 1.2.) Routine reporting of confidence intervals would of course make individual meta-analyses more informative, but it also would facilitate comparison across meta-analyses.

Summary

This random-effects meta-analysis of DITF research provides evidence supporting more confident generalizations about the role of several moderator variables than were possible based on previous reviews. Variations in the identity of the requester, the identity of the beneficiary, the prosocialness of the requests, the medium of communication, and the time interval between requests all appear to influence the size of DITF effects; variations in concession size do not. DITF effects are small in absolute terms but not remarkably small in the context of other effect sizes concerning social influence. How-

ever, there is substantial variability in DITF effects, even under optimal conditions. These findings are not easily reconciled with most proposed explanations of DITF effects, but they appear consistent with a guilt-based account.

NOTES

1. Dillard et al. (1984, p. 483) noted that requester identity and delay were confounded, hence they analyzed only delay variations.
2. A reader quite properly cautioned that this parallel is imperfect because two meta-analyses of a given research area rely on the same (or at least overlapping) data (unlike the case of two primary-research reports based on entirely independent data). But those familiar with (for example) the difficulties of extracting effect sizes from research reports will also appreciate that when different meta-analysts independently figure effect sizes, code study characteristics, and undertake statistical analyses, convergence of results is by no means guaranteed. Still, the caveat is well-taken, and the parallel to primary research ought not be overinterpreted.
3. One cannot be entirely certain on this point because Fern et al. (1986) were not sufficiently explicit about the procedures followed.
4. This suggestion does not appear to be entirely well-founded. The finding that a set of effect sizes is homogeneous does not mean it is impossible for a significant moderator to be at work within that set. See Cook et al. (1992, pp. 313-314) or Hall and Rosenthal (1991, p. 440).
5. Fern et al.'s (1986) inclusion criteria were such as to exclude DITF cases if the sponsoring organization was different in the two requests (see p. 147). This might well have had the effect of excluding cases in which the beneficiary of the second request was different from that of the first request.
6. This expectation is obviously not unique to a guilt-based account. Any number of interpersonal processes might be expected to be engaged more intensely in face-to-face interaction than in mediated communication. But the distinctly interpersonal character of guilt does recommend examination of the possible effect of the communication medium on DITF effects.
7. For this reason, a fixed-effects analysis is naturally more powerful than its random-effects counterpart—because the fixed-effects analysis tests a less demanding hypothesis (a hypothesis about the particular collections of messages, as opposed to a hypothesis about the larger classes of messages). Greater statistical power is sometimes touted as an advantage of fixed-effects analyses, but obviously this is no boon unless the hypothesis of interest concerns the particular collection of messages—in which case a fixed-effects analysis would be preferred not for reasons of power but for reasons of appropriateness.
8. As Jackson (1992) points out, in a fixed-effects analysis, a result such as this one “does not establish a categorical difference, even for the messages actually included in the sample.” She notes that “any differences between the two concrete groups may reflect nothing more than case-to-case differences occurring even within categories,” and hence “the observation that the two concrete groups of messages differ [in a fixed-effects analysis] does not justify the conclusion that the categories differ” (p. 95).
9. When multiple within-study concession size comparisons were available, we used the comparison between the largest and smallest concessions. For example, the comparison derived from a study with small, medium, and large initial request conditions (compared with a given target request) was based on the comparison of the small and large conditions.
10. An alternative procedure is to compute separate effect sizes for the two investigations and then to produce an *n*-weighted average; where raw frequencies were unavailable, we followed this procedure. However, we preferred cumulation based on raw frequencies because

such a procedure minimizes data manipulation and provides consistency with the procedure we followed when collapsing across irrelevant experimental manipulations within a given study.

11. Consider an experimental design hoping to compare two DITF conditions in which different-sized effects are expected. The current results can provide estimates of the relevant population effect sizes: The mean observed DITF effect size under optimal conditions is approximately $r = .15$, and under suboptimal conditions is approximately $r = .04$. To have power of at least .50 (.05 alpha, two-tailed test) in detecting differences of this magnitude (i.e., to have a 50% chance of returning a statistically significant difference between conditions), more than 1,300 participants would be required (more than 650 in each of the two conditions). Even with 2,000 participants, the design's power will be only .65. In the cases reviewed here (as summarized in Table 1.1), the median number of participants was 71.

12. Jackson (1991) has addressed this matter in the context of considering meta-analytic methods for the analysis of primary-research designs containing message replications. Specifically, she points out that testing for homogeneity but failing to test the significance of a mean effect size is (expressed in parallel ANOVA terms) the equivalent of testing the significance of a Treatment $\times$ Replications interaction but not the significance of the treatment main effect. A nonsignificant Treatment $\times$ Replications interaction is no guarantee of a significant treatment main effect.

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Note: Asterisks indicate studies included in the meta-analysis.

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