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ABSTRACT

A study continued a series of empirical investigations into the psychological criteria people use to determine whether or not to make particular arguments. Two hundred volunteers enrolled in a required public-speaking course (1) responded to several demographic questions, (2) described the persuasive choices they would make in the scenario provided, and (3) completed a self-monitoring measure of selection of argument strategies. In investigating the subjects' reasons for refusing to use a number of previously identified compliance gaining strategies, the study concluded that about 25% of the refusals were justified because the strategy simply would not work. Fifteen percent of refusals were made because respondents felt the strategies were negative or objectionable. Twenty-five percent of the rejections were due to person-centered concerns such as self-image or another's welfare. Finally, over one third of the strategies were rejected on the basis of being false or irrelevant. The results suggest that strategies should provide options to function in a given situation. (Tables of findings are included.) (DF)

COGNITIVE EDITING OF ARGUMENT STRATEGIES

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Abstract

This report improves upon the authors' previous investigations of naive actors' argument standards in two ways: (a) by using multiple operationalizations of each compliance gaining strategy; and (b) by using a checklist to obtain subjects' rationales for not using various arguments. Overall results correspond generally to those of the earlier studies: effectiveness concerns account for 23.5% of rejections, principled objections to specific strategies for 15.7%, person-centered issues for 26.1%, and discourse competence rules for 34.6%. Results also support the need to use several examples of each strategy. Situational differences have some significant effects on subjects' responses. The self-monitoring scale has serious psychometric problems, and does not predict use of rejection criteria.

COGNITIVE EDITING OF ARGUMENT STRATEGIES

This paper continues a series of empirical investigations into the psychological criteria people use in judging whether or not to make particular arguments (Hample, 1984; Hample & Dallinger, 1985). By studying the reasons people give for refusing to advance various compliance gaining strategies, we can describe the evaluative systems used in self-editing of arguments.

The two previous investigations provided respondents with lists of possible arguments which might be made in various situations. Subjects were asked to explain why they chose not to endorse the rejected strategies. These rationales were coded into several categories. Two codes ("it won't work", and "it might boomerang") represent straightforward effectiveness judgments, and accounted for about 5% of all rejections in Hample (1984), and 10% in Hample and Dallinger (1985).[1] Sometimes strategies were rejected because respondents felt the message to be objectionable on its face (e.g., "threats are nasty"). This sort of criterion accounted for 20% of rejections in the first study, and about 10% in the second. The third main group of codes is person-centered, and indexes issues such as violation of self-image, injury to other, and harm to the relationship between the persuader and target. About half the rejections in study one, and a third of those in study two, were classified as person-centered. The last main group of codes has to do with discourse competence—truth, relevance, and novelty of the arguments. About a quarter of the rejections in the first study, and nearly half in the second, were based on these criteria.

These results, which reflect the argument editing criteria of naive

social actors, are of considerable theoretical importance because of their resemblance to some of the argument standards appearing in our professional literature. The last two main groups of codes (person-centered and discourse competence), in particular, seem to support prominent strands of thought regarding human communication.

The use of person-centered criteria in situations having an overt persuasive goal underscores the fact of multiple intents in interactions (Delia & Clark, 1979; Tracy, 1984). Even when assigned to select persuasive strategies, respondents keep in mind subordinate intents (see Hample, in press). They attend to the other's face, to self-image, and to the relationship in general, even while searching for messages which will influence the other. Tracy, Craig, Smith and Spisak (1984) observe that compliance gaining research has not paid much attention to these other interactional goals. A few pertinent findings have appeared, of course—people weight benefit to other more heavily than benefit to self (Boster & Stiff, 1982), and awareness of identity management issues correlates with face-supportive messages (Kline, 1981)—but these sorts of concerns are as alien to the general compliance gaining research tradition as Tracy and her colleagues indicate. The present series of studies, however, provides clear evidence for the centrality of personal and interpersonal goals in persuasive situations.

The discourse competence criteria also have some special importance. Various philosophers (Grice, 1975; Habermas, 1979, ch. 1) have described the prerequisite conditions for competent communication. Although our respondents have certainly not shown sensitivity to all the preconditions noticed by these philosophers, subjects have repeatedly indicated the

importance of the truth and relevance (and, to a far lesser extent, the novelty) of a message as basic criteria in deciding whether or not a given strategy ought to be used. These sorts of findings offer some empirical mooring for several important philosophical critiques of ordinary communication.

The main purpose of this report is to improve upon the methodology of the earlier studies in several respects. A second goal is to determine the relationship between self-monitoring (Snyder, 1974) and use of the editing codes. The following two sections detail our rationales.

Methodological Considerations

We feel the need to address three methodological issues here. The first deals with a potential objection to the whole series of investigations, and the other two concern design improvements over the two earlier studies.

The first issue is our use of lists of compliance gaining strategies. Rather than have subjects invent their own possible arguments, we have argued that a better procedure is to supply them with prepared lists. Our reasoning on this issue has been detailed in the earlier papers, and we will not repeat it here. However, assuming that we are justified in our decision to provide respondents with a list of potential messages, which one should we use? We have chosen to use the Marwell and Schmitt (1967) list, which has been attacked by several scholars on the grounds that it was generated *a priori*, rather than having been elicited from naive actors (Wiseman & Schenck-Hamlin, 1981; Cody, McLaughlin & Jordan, 1980).

Although the Marwell and Schmitt list omits strategies which appear in

other formulations (see Wheeless, Barraclough & Stewart, 1983), Boster (1985, p. 580) judges that overall, "the content of the various lists is remarkably homogenous." Boster, Stiff and Reynolds (1985) have shown that both the Marwell/Schmitt and the Wiseman/Schenck-Hamlin lists are unidimensional. Therefore, each message type is simply another item for a general compliance strategy choice scale. Additional items (i.e., strategy types) will no doubt improve the overall reliability of the general scale, but will not necessarily alter its fundamental meaning. For normal compliance gaining research, therefore, the researcher's decision to use one list rather than another, or perhaps to combine several lists, is hardly critical to the validity of results.

In our research, this issue is even less pressing. Unlike the customary compliance gaining studies, ours are not designed to describe the types of messages people will or will not make. Instead, we merely wish to know the reasons why some arguments are rejected. Our omission of a strategy type is important only if that strategy would be rejected on grounds different than those for the other strategies. So we consider our use of argument lists prepared to correspond with Marwell and Schmitt's recommendations to be entirely appropriate.

The second issue we should discuss here is multiple operationalization. Jackson and Backus (1982; also see Tracy, et al., 1984, pp. 515-518) argue forcibly that the results of many compliance gaining studies, especially those claiming to find situational effects on likelihood of strategy endorsement, are irretrievably marred by the researchers' common use of a single example to represent a category of situations, and of a single

example to represent a category of strategies. On its face, this is a compelling argument. Further, when Jackson and Backus used four different operationalizations of each strategy, situational effects all but disappeared (the effect size was only about .03). Correlations between replication lists ranged from .24 to .65. Clearly, if we are to generalize about situations, we must have samples of situations to represent each situation condition. Similarly, our theories about strategy types must be based upon samples of each strategy.

Our previous work is susceptible to these criticisms. Neither study used more than one example of a strategy for any given situation. The second investigation made use of four situations, and found very weak relationships between situation and the editing criteria used to justify rejection of the strategies. Since pragmatic considerations of sample size and length of experimental booklet seemed to require us to choose between multiple operationalizations of situation and strategy, we chose here to use several examples of each strategy, on the grounds that situational effects appear to be quite small. This design change ought to improve our standing to generalize about the relationship between editing criteria and specific kinds of arguments.

This was not the most important revision in our procedures for this report, however. Here, for the first time, we provide respondents with a checklist of rejection rationales. The earlier studies requested that subjects write open-ended justifications for their decisions not to endorse various messages. This change in procedure is the last design issue we wish to address here.

Open-ended responses have the obvious merit that subjects express

themselves freely, rather than having their rationales filtered through a checklist. However, this technique has several costs as well. For one thing, our debriefings of subjects led us to believe that a few subjects endorsed more strategies than they might have otherwise, in order to escape the effort of justifying another rejection. Subjects in the debriefing periods seemed to be forthright with us about this, and indicated that the problem was rare, but we were still concerned. Secondly, open-ended responses must be coded. Quite apart from the tedium involved, we were only marginally successful in doing this coding. Percentage of coder agreement was in the range 70-80%, which, though tolerable, is not very impressive considering the limited number of categories in the system, and the fact that the coders wrote the system. We felt that subjects could code their rationales as well as we could, so in this study we converted our coding directions into a multiple choice checklist for subjects to use. An advantage of this new procedure is that it allows us to obtain more data from each subject in the same amount of time, since respondents can check off a rationale more quickly than they can write one.

Self-Monitoring

The primary goal of the earlier studies was to describe the criteria people use in editing their arguments. Since we now consider that we have at least a general description of these cognitive editing standards, we wish to begin exploring the variables which may influence use of these criteria. In coding the data for the earlier reports, we repeatedly noticed that some respondents seemed to have pronounced preferences for certain kinds of rationale. One person, for instance, might use reasons such as "this is

inconsistent with my self-image" nearly half the time, while other subjects would not use this or any other person-centered criterion at all. We therefore decided to test individual difference variables in the hope of explaining these response patterns.

We chose to test the effects of self-monitoring (Snyder, 1974) on criteria choice. We expect self-monitoring to predict subjects' preference for person-centered criteria (those involving self-image, face support for the other, and preservation of the interpersonal relationship). Snyder (1974, p. 536) summarizes his results as follows: "Out of a concern for social appropriateness, the self-monitoring individual is particularly sensitive to the expression and self-presentation of others in social situations and uses these cues as guidelines for monitoring and managing his own self-presentation and expressive behavior." People low in self-monitoring, Snyder adds, are substantially less concerned with interpersonal appropriateness, and tend simply to say what is on their minds. Douglas (1983) reports that high self-monitors are more likely than low self-monitors to generate friendly comments in a simulated interaction, and this supports Snyder's general findings. We expect, therefore, that people high in self-monitoring will use the person-centered rationales more often than low self-monitors.

Method

Subjects. Respondents were 200 volunteers enrolled in the university-required public speaking course. They received extra credit for completing the questionnaire. 64% were male, and 36% were female. 97% were sophomores, juniors or seniors, and their mean age was 21.

Procedure. Subjects received a booklet containing three tasks. The first asked for responses to several demographic questions. Second, subjects were given a situation and asked to describe the persuasive choices they would make in it. The final part of the booklet contained the self-monitoring instrument. Subjects required about half an hour to complete the booklet.

Situation. Three situations were randomly distributed among respondents. The three were duplicated from Hample and Dallinger (1985). Here are the paragraphs describing the situations:

(1) You have been dating the same person now for about two years and you are thinking about getting engaged. Christmas vacation is coming up soon and you want your boyfirend/girlfriend to come home with you. S/he initially disagrees but you are still trying to convince him/her.

(2) You have been living with your roommate in an apartment for several months. You generally take turns cleaning the place up and now it is his/her turn, but the apartment is in a real mess and s/he hasn't done any cleaning for several days. You want him/her to clean up.

(3) You have been living in an apartment with two friends for the school year and now, since school is over for the year, you are getting ready to move out. The landlord has come over to inspect the place, and you are trying to convince him to return your deposit money. Your roommates had to work so they aren't there with you.

Each student responded to one situation.

Strategies. Each situation paragraph was followed by forty-eight "things you could say or do." These potential messages were developed to represent Marwell and Schmitt's sixteen compliance gaining strategies. Three different examples were included for each strategy—two new and one from the earlier study (Hample and Dallinger, 1985). For example, for the landlord situation, the promise strategies were:

1) You tell him that if he gives you back your deposit you will recommend his apartments to your friends next year.

2) You promise to rent from him again if he will refund your money.

3) You offer to move out a few days early so he can do maintenance if he will return your deposit.

Strategies were presented in random order in the questionnaire. A different random order was used for each situation.

Response Checklists. For each strategy, respondents were asked to indicate if they would use the strategy, and if not, to circle the letter of the item which came closest to describing their reason for not choosing to use it. Each possible message, therefore, was followed by the nine available responses, presented in a multiple choice format. These choices were developed to represent the most important of the thirteen codes in Hample (1984) and the eleven in Hample and Dallinger (1985). The booklet introductions explained the choices with abbreviated versions of the earlier studies' coding systems. Here are the full explanations from the booklet introductions:

a. I would use this one. This means that you would be willing to say or do whatever is indicated. You may accept as many of the 48 as you wish.

b. No: This wouldn't work. You reject this approach because it would fail, or even perhaps backfire.

c. No: This is too negative to use. You prefer not to use this one because is too high pressure--a distasteful threat or bribe, perhaps.

d. No: I must treat myself positively. You might later regret using this approach, or it doesn't match your self-image.

e. No: I must treat the other positively. You feel that this approach might hurt the other's feelings—perhaps make him/her feel guilty or mad.

f. No: I must treat our relationship positively. You reject this approach because it might injure the relationship between you and the other person.

g. No: This is false. You consider that this approach is false or impossible or easily refuted.

h. No: This is irrelevant. This approach seems irrelevant, either to you or to the other person.

i. No: Other. You wouldn't use this approach, but for reasons other than letters b through h. Please briefly indicate your reason.

Only the underlined portions appeared as choices in the actual booklets, but respondents were allowed to refer back to the elaborations as often as they wished. Subjects were instructed to choose only one response for each strategy. Only about 2% of all responses fell into (i), the "other" category; this suggests that the checklist has reasonably detailed coverage of the main rejection rationales.

Self-Monitoring. The third part of the questionnaire consisted of the twenty-five item self-monitoring scale. Items were taken from Snyder (1974). Rather than using true/false as the possible responses, as Snyder does, we provided a five point scale ranging from ENTIRELY TRUE to ENTIRELY FALSE for each item. Items were presented in Snyder's order.

As a matter of procedure, we factor analyzed the self-monitoring scale. With 200 subjects, we ought to have a fairly stable correlation matrix, and should obtain generalizable results. The factor analyses were extremely disappointing, however. Here are the percentages of variance explained by each of the first four factors: I (14.7%), II (11.9%), III (6.3%), and IV (5.8%). Altogether, these account for less than 40% of total scale variance.

Nor were the rotated factors themselves satisfactory. Using the .60/.40 factor loading rule of thumb, only two of the twenty-five items

qualify in the two-factor solution; only two for the three-factor solution; and only four for the four-factor solution. If only a single factor is extracted, only two items have loadings of .60 or more. Dillard, Hunter and Burgoon (1984) have also factor-analyzed this scale. They report four first order factors. Although they do not report factor loadings, our inferences from their Table 4 are that, at most, only four or five of the items in their study meet the usual .60/.40 standard.

Since our results do not justify subdividing the scale into constituent scales (because the "constituent scales" have too few items), we simply summed the whole scale, reflecting item scores where appropriate. This is the customary procedure. Our internal reliability was adequate (Cronbach's alpha = .71). Nonetheless, we confess to very serious reservations about the quality of this standard measuring instrument.

Results

Multiple Operationalizations of Strategy. Our data offer a rare opportunity to study whether different examples of each compliance gaining strategy elicit different responses. Jackson and Backus (1982) report some results of this kind, using four different examples of each strategy. However, one of their four examples was a general statement describing the strategy, rather than an actual argument; this feature makes their results a little difficult to interpret.

Table 1 reports comparisons among the three examples of each strategy, summed across situations. Each version of a strategy is associated with

INSERT TABLE 1

each other version. Only three of the forty eight comparisons are not significant. Cramer's V measures the strength of relationship within each pair; generally, this statistic is between .25 and .30. Thus, while each message is clearly associated with the others which operationalize the same strategy, strength of association is only moderate. These results support the recommendations of Jackson and Backus, to the effect that several examples are needed to operationalize each compliance gaining strategy.

Editing Criteria Patterns. Tables 2 and 3 display the general patterns of editing criteria usage. Table 2 shows the mean number of times each

INSERT TABLES 2 & 3

criterion was selected, as well as the percentages of use. About 30% of the strategies were accepted, suggesting that the messages were fairly plausible. Simple effectiveness judgments (code b) accounted for about a quarter of the total rejections. Principled objections to particular kinds of strategies (code c) generated 15% of the reasons for rejecting arguments. The person-centered criteria (codes d, e, and f) together account for a little more than a quarter of all rejections. The discourse competence categories (g and h) were used to justify a little more than a third of the rejections. In spite of the upward bias built into the coding system used in the two earlier studies (see footnote 1), these results are fairly comparable to those.

Table 3 breaks category usage down by strategy. Inspection of the table reveals that category usage is somewhat dependent on strategy. Code b

is used with fairly consistent frequency throughout the strategy list, but other rationales are more variable. Relevance (code h), for instance, is quite important for positive altercasting and positive esteem, but not very pertinent to decisions about threat, aversive stimulation, or moral appeal.

Several strategies--threat, aversive stimulation, negative altercasting, and negative esteem--are commonly rejected because they seem too negative, but this code (c) is rarely used for other types of message. The person-centered criteria are fairly evenly dispersed throughout the strategy list, suggesting that the specific content, rather than the message type, is the critical feature here. The only exceptions to this generalization are that threats and aversive stimulation are often thought to endanger relationships (code f). The discourse competence codes (g and h) are often used to reject most of the strategies: promise, positive expertise, negative expertise, pregiving, debt, positive self-feeling, negative self-feeling, positive altercasting, negative altercasting, positive esteem, and negative esteem.

In sum, the data reveal an overall pattern of editing criteria usage which is fairly similar to those found in earlier studies. Several of the criteria are often applied to most of the strategies, while others are mainly used for a handful of argument types. These latter results are obviously quite dependent on the exact content of the sample strategies we provided, but having three examples for each message type allows some confidence in these outcomes.

Situation. Tables 4 and 5 are informative with regard to the effects of situation on editing criteria usage. Table 4 reports results of analyses

INSERT TABLES 4 & 5

of variance, using situation as the independent variable and frequency of code usage as the dependent variable. Situation had a significant but weak effect on acceptance of strategies, replicating Jackson and Backus' (1982) results. As for the rejection codes, situation had a significant impact for only three of the seven criteria, and effect sizes range from .06 to .11. Though these results offer some support for the conclusion that situation affects rejection category usage, the effect sizes are small.

Table 5 reports the results of a large number of contingency tests which relate situation to rejection criteria usage for each example of each strategy. In half the analyses, all nine responses (codes a-i) are used, and in the other half, only the substantive rejection criteria are involved (i.e., codes b-h). Situation has a significant impact on rejection/endorsement choices for nearly all the message examples, failing to achieve significance in only seven of the forty eight tests. The average Cramer's V for these comparisons is .33, indicating moderate levels of association. Situation has less effect on the substantive rejection codes, however. There, twenty two of the forty eight tests are not significant, including all three examples of the liking and debt strategies, and two of the three examples of threat, pregiving, positiv self-feeling, negative self-feeling, and positive esteem. The mean Cramer's V for these associations is .30. While this still indexes moderate associations, the relationships do not appear for all strategies.

The effect of situation on rejection category usage is very weak when code selection is summed across strategies, as in Table 4. When broken down

on a strategy-by-st ··· basis, however, the effect is moderately strong. But situation is inconsistent in its effects on substantive rejection codes, and seems particularly irrelevant to reasons for rejecting threat, liking, pregiving, debt, positive self-feeling, negative self-feeling, and positive esteem. People's reasons for rejecting these latter strategies appear to have little to do with si uation.

Self-Monitoring. We expected self-monitoring to correlate positively with use of the person-centered codes (i.e., codes d-f). We conducted several analyses to test this possibility. First, we correlated self-monitoring scores with the frequencies with which each subject used each of the response codes. Self-monitoring does in fact correlate significantly with frequency of endorsement ($r=.22$, $df=194$, $p<.01$), but the effect size is less than .05. Self-monitoring was not significantly associated with any of the rejection codes, however, in spite of our healthy sample size.

Our other set of analyses consisted of chi-squareds, using a median split on self- nitoring as the independent variable, and the response codes as dependent variables. Separate analyses were conducted for each message example, for all nine codes, and for only the seven substantive rejection codes. (The design is analogous to that presented in Table 5, except that self-monitoring replaces situation.) Of the ninety-six analyses, four were statistically significant—about what chance would predict if the variables were completely unrelated.

These results make it plain that self-monitoring is irrelevant to use of cognitive editing standards.

Discussion

Though our first effort to explain individual differences in the use of rejection rationales failed, in other respects this investigation has been useful.

Our use of multiple operationalizations for each compliance gaining strategy provides a more representative set of stimuli than are available for single-operationalization studies. We found moderate associations within our triads of messages. This means that, while the example messages were associated strongly enough to justify our claim that they are representing the same strategy, the individual stimuli are different enough that multiple operationalization is essential. We therefore endorse the methodological recommendations of Jackson and Backus (1982), and offer additional empirical support for them.

The patterns of editing criteria usage obtained here are roughly similar to those of the two earlier studies. The results of the present investigation deserve more credence, however, for several reasons. First, this study uses multiple operationalizations of the strategies, as discussed above. Secondly, since subjects chose the most meaningful response category, and did not settle close decisions by automatic use of the higher category as was the case in the earlier studies, this study's results do not have the upward bias which was built into the previous investigations' outcomes.

Results show that, as before, the person-centered and discourse competence rationales are extremely important. Here, they account for more than 60% of all rejections. These results are of special theoretical interest, for they point to use of multiple goals in these simulated

interactions. Even when assigned to persuade another person, our respondents respected (presumably subordinate) goals involving self-image, face support for other, relationship preservation, truth of utterance, and relevance of utterance. This is fairly clear evidence that any single-goal analyses of communicative intents are quite likely to be wrong.

Some of the rejection rationales are used consistently for most of the strategies, but others are concentrated in the responses to a few message types. This suggests that type of strategy may be quite important to the overall pattern of our results. Consequently, we need to include some of the compliance gaining strategies omitted from Marwell and Schmitt's list in future work.

Although we did not attempt to vary stimulus situations systematically on any particular dimensions, we provided persuasion settings that seem to vary in the intimacy of target and in the duration of relationship. These are two situational variables that have received some attention in compliance gaining research. Our results show statistically significant, but minor, effects of situation on overall use of rejection codes. When we conducted message-by-message analyses, however, we obtained slightly more impressive effects for situation. On a message-by-message basis, situation is usually a significant predictor of endorsement and rejection choices, though the effect is weaker when only the rejection codes are used in the analyses. The effect size of these relationships is moderate.

We quite clearly failed to find any relationship between self-monitoring and any of the rejection codes. We did find a small association between self-monitoring and endorsement, such that high self-monitors use

more messages. However, we found no influence of self-monitoring on any measures of editing criteria usage. The psychometric properties of the self-monitoring scale are quite unsatisfactory. For these reasons, we will look elsewhere for explanations of individual editing differences.

Conclusions

This study investigated people's reasons for refusing to use various of Marwell and Schmitt's compliance gaining strategies. About a quarter of the refusals are justified by the simple claim that the strategy will not work. A further 15% are due to respondents' feelings that the strategies are unacceptably negative, or objectionable on their face. A fourth of the rejections are due to person-centered concerns: with self-image, with the other's welfare, or with the continuation of the interpersonal relationship. More than a third are based on discourse competence considerations, namely, that the message is false or irrelevant. We also report evidence that strategies ought to be multiply operationalized, and that generalization requires situational variance.

Footnote

[1]We built an upward bias into both the coding systems we used in the earlier studies. Given a rationale which could be coded in two ways (e.g., "This wouldn't work because it's a lie" could be coded as "won't work" or as "not true"), we always chose the more sophisticated code. We felt this upward bias to be justified for two reasons: (a) it afforded an unambiguous way to resolve coding problems; and (b) it allowed subjects' responses to be coded in such as way as to reflect their highest plausible level of sophistication. In increasing order of sophistication, the general code categories are: effectiveness, objection to types of strategy, person-centered, and discourse competence. Thus, the results being reported above probably over-represent the person-centered and discourse competence codes, and under-represent the effectivenss and principled objection criteria.

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Table 1

Association between Replicated Strategies

Strategy	1st and 2nd Version		1st and 3rd Version		2nd and 3rd Version	
	χ^2	V	χ^2	V	χ^2	V
Promise	173.4***	.33	158.3***	.32	185.0***	.34
Threat	130.4***	.29	107.5***	.26	98.3**	.25
Positive Expertise	115.4***	.27	150.6***	.31	153.9***	.31
Negative Expertise	116.5***	.27	63.4	.21	94.9***	.26
Liking	145.8***	.30	105.9***	.26	172.4***	.33
Pre-giving	146.9***	.30	137.8***	.30	105.2***	.26
Aversive Stimulation	218.5***	.37	140.5***	.29	170.6***	.33
Debt	55.9	.19	88.0*	.24	227.6***	.38
Moral Appeal	139.8***	.29	89.8**	.24	152.8***	.31
Positive Self Feeling	99.8**	.25	90.9**	.24	146.8***	.30
Negative Self Feeling	94.7**	.25	86.5*	.23	228.1***	.38
Positive Altercasting	99.4**	.25	124.6***	.28	93.6**	.24
Negative Altercasting	186.8***	.35	129.8***	.29	134.1***	.29
Altruism	91.0**	.26	64.1	.22	111.8***	.27
Positive Esteem	115.5***	.27	133.9***	.29	92.8**	.24
Negative Esteem	127.5***	.30	146.4***	.30	109.2***	.28

*** $p<.001$, ** $p<.01$, * $p<.05$

Table 2

Means of Total Use of Nine Response Types

Response	Mean	% of all codes	% codes 2-8
a. yes	10.955	30.5	n/a
b. no, won't work	7.260	15.9	23.5
c. no, too negative	4.900	15.9	15.7
d. no, don't hurt self	2.440	5.3	7.9
e. no, don't hurt other	2.705	5.9	8.8
f. no, don't hurt relationship	2.890	6.3	9.4
g. no, false	4.710	10.3	15.2
h. no, isn't relevant	5.985	13.1	19.4
i. other	.935	2.0	n/a

Table 3

Frequencies and Percentages of Response Types by Separate Strategies

Strategy	a	b	c	d	e	f	g	h	i	Total
Promise	205 (35)	101 (17)	56 (9)	56 (9)	9 (2)	18 (3)	48 (8)	86 (15)	14 (2)	593
Threat	107 (18)	111 (19)	128 (22)	36 (6)	27 (5)	74 (13)	45 (4)	49 (8)	15 (3)	592
Positive Expertise	283 (48)	93 (16)	25 (4)	14 (2)	13 (2)	15 (3)	72 (12)	70 (12)	9 (1)	594
Negative Expertise	173 (29)	111 (18)	63 (11)	16 (3)	25 (4)	51 (9)	73 (12)	75 (13)	8 (1)	595
Liking	272 (46)	109 (18)	25 (4)	44 (7)	15 (3)	16 (3)	37 (6)	60 (10)	17 (3)	595
Pregiving	146 (25)	140 (24)	48 (8)	59 (10)	22 (4)	23 (4)	36 (6)	98 (16)	23 (4)	595
Aversive Stimulation	61 (10)	142 (24)	116 (20)	53 (6)	30 (5)	101 (17)	47 (8)	27 (5)	16 (3)	593
Debt	236 (40)	88 (15)	52 (8)	23 (4)	26 (4)	37 (6)	51 (9)	71 (12)	11 (2)	595
Moral Appeal	271 (46)	74 (12)	51 (9)	22 (4)	36 (6)	32 (5)	48 (8)	46 (8)	14 (2)	594
Positive Self Feeling	158 927)	168 (28)	34 (6)	22 (4)	25 (4)	20 (3)	81 (14)	79 (13)	7 (1)	594
Negative Self Feeling	96 (16)	142 (24)	58 (10)	16 (3)	68 (11)	41 (7)	96 (16)	66 (11)	10 (2)	593
Positive Altercasting	153 (26)	93 (16)	43 (7)	20 (3)	53 (9)	32 (5)	41 (7)	148 (25)	10 (2)	593

Strategy	a	b	c	d	e	f	g	h	i	Total
Negative Altercasting	111 (19)	96 (16)	110 (19)	19 (3)	73 (12)	51 (9)	56 (9)	67 (11)	9 (2)	592
Altruism	290 (49)	100 (17)	20 (3)	43 (7)	20 (3)	10 (2)	41 (7)	64 (11)	5 (1)	593
Positive Esteem	148 (25)	110 (19)	41 (7)	18 (3)	36 (6)	20 (3)	82 (14)	121 (20)	16 (3)	592
Negative Esteem	81 (14)	115 (19)	110 (19)	27 (5)	63 (11)	37 (6)	88 (15)	70 (12)	3 (1)	594

Table 4

The Effects of Situation on Respondents' Use of Response Codes

Codes of Responses	F	p	R^2
a. yes	4.794	.009	.046
b. no, won't work	6.304	.002	.060
c. no, too negative	6.735	.001	.064
d. no, don't harm self	1.009	.367	.010
e. no, don't harm other	2.337	.099	.010
f. no, don't harm relationship	12.006	.001	.109
g. no, false	1.613	.202	.016
h. no, isn't relevant	.963	.384	.010
g. other	1.698	.186	.017

Note. Degrees of freedom for all tests was 2/199.

Table 5

Tests of the Contingency between Situation and Response Code for Each Message

	Codes a–i			Codes b–h		
Strategy	χ^2	p	V	χ^2	p	V
Promise 1	62.4	.00	.39	25.1	.01	.36
Promise 2	55.8	.00	.38	21.2	.05	.29
Promise 3	55.5	.00	.37	54.2	.00	.43
Threat 1	28.5	.03	.27	15.8	.20	.2[illegible]
Threat 2	43.4	.00	.33	36.6	.00	.33
Threat 3	22.4	.13	.24	10.4	.58	.19
Pos Exp 1	28.9	.02	.27	20.4	.06	.30
Pos Exp 2	42.4	.00	.32	23.3	.03	.34
Pos Exp 3	38.0	.00	.31	23.7	.02	.36
Neg Exp 1	81.2	.00	.45	37.2	.00	.37
Neg Exp 2	51.4	.00	.36	34.8	.00	.33
Neg Exp 3	20.1	.13	.23	11.8	.46	.22
Liking 1	68.6	.00	.42	13.6	.32	.27
Liking 2	21.4	.17	.23	6.7	.88	.17
Liking 3	23.7	.09	.24	15.8	.20	.28
Pregiving 1	48.4	.00	.35	24.8	.02	.35
Pregiving 2	31.7	.01	.28	17.4	.14	.24
Pregiving 3	25.1	.07	.25	20.1	.06	.25
Aversive 1	55.8	.00	.37	41.8	.00	.34
Aversive 2	28.8	.03	.27	24.8	.02	.27
Aversive 3	42.6	.00	.33	31.9	.00	.31
Debt 1	44.9	.00	.34	9.0	.69	.24
Debt 2	69.9	.00	.42	18.6	.10	.26
Debt 3	40.7	.00	.32	19.8	.07	.28
Moral App 1	59.5	.00	.39	10.8	.56	.22
Moral App 2	77.8	.00	.44	22.0	.04	.33
Moral App 3	47.5	.00	.35	22.5	.03	.35
Pos Self 1	39.5	.00	.32	30.1	.00	.32
Pos Self 2	60.9	.00	.39	20.3	.06	.29
Pos Self 3	26.1	.05	.26	18.1	.11	.25

Strategy	Codes a-i χ^2	p	V	Codes b-h χ^2	p	V
Neg Self 1	68.1	.00	.41	51.5	.00	.40
Neg Self 2	35.9	.00	.30	19.0	.09	.24
Neg Self 3	23.8	.09	.25	20.0	.07	.25
Pos Alter 1	82.3	.00	.46	52.8	.00	.44
Pos Alter 2	19.5	.25	.22	9.9	.63	.19
Pos Alter 3	46.6	.00	.34	42.0	.00	.37
Neg Alter 1	59.2	.00	.39	22.8	.03	.27
Neg Alter 2	32.9	.01	.29	24.1	.02	.28
Neg Alter 3	72.9	.00	.43	49.0	.00	.40
Altruism 1	97.9	.00	.50	25.7	.01	.38
Altruism 2	53.1	.00	.37	20.6	.06	.31
Altruism 3	43.3	.00	.33	27.0	.01	.37
Pos Esteem 1	27.9	.03	.27	25.5	.01	.29
Pos Esteem 2	43.9	.00	.33	15.8	.20	.23
Pos Esteem 3	40.3	.00	.32	10.9	.54	.20
Neg Esteem 1	46.0	.00	.34	35.7	.00	.33
Neg Esteem 2	25.5	.03	.25	13.9	.31	.20
Neg Esteem 3	39.4	.00	.32	33.5	.00	.32