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Ameliorative helping and the transgression-compliance phenomenon.

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AMELIORATIVE HELPING AND THE
TRANSGRESSION-COMPLIANCE PHENOMENON

A Thesis Presented

By

Gregory M. Chilenski

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University of Massachusetts in partial fulfillment
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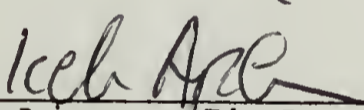
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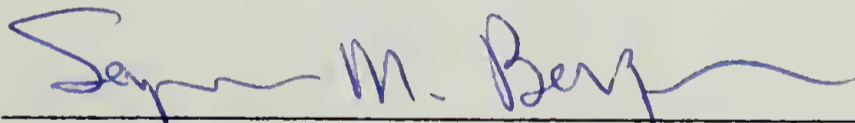
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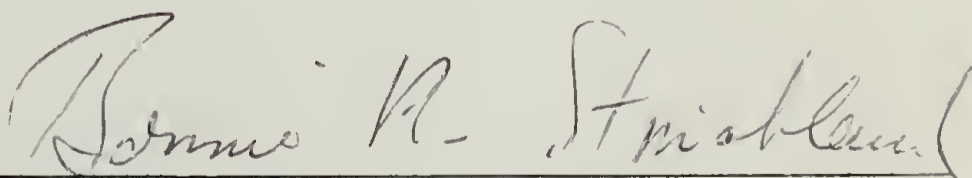
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A person's increased willingness to be helpful after he has transgressed against another is a strikingly consistent observation recorded in many studies. The most frequently cited explanation for this phenomenon is that by helping, people are attempting to alleviate a feeling of guilt which has been created in them by engaging in the prior harmful behavior. This literature was reviewed, and it was concluded that scant direct evidence exists in support of this explanation. It was hypothesized that the best single predictor for increased helpfulness after transgression is whether or not the requested help would be ameliorative of the harm previously done. An experiment was conducted in which helping was either ameliorative or not, and the actor's responsibility for the harm done was either high or low (a 2 x 2 design).

Helping occurred more when the effects were ameliorative of the harm done. While subjects' feelings of responsibility were successfully varied, feelings of guilt did not reflect the same directional differences as would be predicted by a mediation by guilt explanation. Neither guilt nor responsibility affected helping. Implications for the internal and ecological validities of transgression-compliance research were discussed. Further research on these and related questions was proposed.

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C H A P T E R I
INTRODUCTION AND REVIEW

This thesis deals with an aspect of people's reactions to their own transgressions; specifically, their subsequent compliance to requests for help. Interest in the transgression-compliance phenomenon can be viewed as a subset of a broader area of interest designated as pro-social behavior in which the concern is to understand the basic processes underlying social cohesion: cooperation, helping in emergencies, altruistic practices and behavior, sharing, and compliance to and with general norms and rules.

A large number of experimental studies of the transgression-compliance phenomenon were based on the assumption that the reduction of guilt is the central mediating variable which determines the typically observed relation between harming another and subsequently helping the victim or some other person. However, recently this general hypothesis has been modified by a number of limiting conditions, experimentally derived. The present paper takes the position that continued search for limiting conditions is an inadequate approach to the problem because the guilt hypothesis and its attendant methodology in use are themselves

inadequate to explain the transgression-compliance phenomenon which is observed in the laboratory. The tacit assumption of the guilt hypothesis is that people are basically hedonistic and self-serving organisms. This tacit assumption leads all too easily to the formulation (which has been broadly generalized beyond the scope of the research on which it is allegedly based) that people help others in order to help themselves feel better, i.e., to feel less or not guilty.

This idea can be faulted on both conceptual and methodological grounds. The thrust of the present argument is that the assumption made about the presence of guilt in the transgression-compliance situation imposed an unnecessary limitation on the research conducted in support of that assumption. The phenomenology of the actor was, as a result, almost completely overlooked. In addition, a review of the relevant literature reveals evidence for at least three variables which could account for a good deal of the findings without reference to guilt feelings. If these variables were to be added to the guilt hypothesis as limiting conditions the result would be to limit it almost out of the realm of meaningfulness. In this same literature, in the rare occurrences when guilt was directly assessed, little support for its existence was obtained. The experiment re-

ported in this paper tested an hypothesis derived from the literature review concerning the effects of the ameliorative nature of the requested help on the amount of help given by a transgressor. At the same time the guilt hypothesis was directly tested.

The typical transgression-compliance experiment uses a laboratory setting in which a subject is induced to "harm" another person and then is observed in order to determine the effects of harm-doing on the subject's subsequent helping. Frequently, the initial harm-doing is an integral part of some ostensible experimental procedure, as, for example, when a subject is instructed to administer shock to another for errors on a learning task. Sometimes a variant of this is employed so that events are arranged in which subjects believe they are the cause of accidental harm, for example, as in the accidental destruction of a laboratory apparatus. Accidental harm-doing is also employed in the few field studies conducted on this topic. Least often used is a procedure which enables an experimental subject to cheat on a paper-and-pencil test, the harm presumably being done to the experimenter because of the importance of the test for research purposes.

Subsequent helping in the laboratory setting is almost always presented to subjects as something they can do for another which is not within the normal procedures of any

experiment. For example, they might be asked to participate in another study just as a favor to the experimenter or to a friend of the experimenter. Or they might be asked to volunteer time or money to a charitable or social interest organization. In the field studies involving accidental harm-doing, they are exposed to a stranger in mild distress, for example, a woman who drops a bag of groceries.

The results of these procedures have generally shown that when an actor harms another he is more likely to comply with a request for help (and in a few cases more likely to help spontaneously) than if no harm is previously perpetrated. But this general statement must be qualified immediately. Not all combinations of the above operationalizations of harm-doing and of helping will yield this effect. To anticipate the literature analysis which follows, there seem to be at least three additional factors which have an influence on whether the transgression-compliance effect of more helping is strongly produced: whether the harm done is legitimate or illegitimate according to the situation in which it occurs, whether the help will undo the harm that was done, and who the requester of help is.

While the procedures used in these studies are thus easily summarized, the explanations that have been proposed for the observation that harming increases subsequent help-

ing are not. Yet this much can be said: concepts such as guilt, embarrassment, self-esteem maintenance and social consistency, as they have been applied to the transgression-compliance phenomenon, all are explanations based on a model using a single mediating factor. While the discussion below will be directed mainly at the inadequacies of the guilt explanation, the observations and conclusions arrived at apply to each of these other explanatory concepts as well. For a large majority of studies do not provide enough information about mediating events in order to make a decision in favor of any one of them as a plausible explanation. Where the pertinent data do exist, they support a different explanation along cognitive, multi-factor lines.

Preliminary considerations. Brock (1969) focused on transgression-compliance research which addressed itself to the guilt hypothesis and reviewed that literature. He concluded that a guilt explanation was premature at that time because of the concept's lack of precision, because independent checks on the guilt manipulations were lacking, and because certain results did not fit derivations from a guilt explanation. He did not make clear how the last of these conclusions could be supported in light of the first. That is, precise derivations cannot be made (and hence cannot be disconfirmed) from a concept which lacks the necessary precision in the first place. Nevertheless, his

critique of the methodological errors is sound. With few exceptions, publications since Brock's review suffer from the same lack of manipulation checks which he noted.

Despite ambiguity over predictions in specific situations, one prominent researcher in this area, Freedman, is fairly clear about what guilt in this context means and what it is thought to be related to.

The notion that guilt will lead to pressures toward expiation probably goes as far back as the concept of guilt itself. Presumably when someone feels that he has done something wrong there will be a tendency for him to make up for his wrongful deed. He can do this by subjecting himself to punishment or by doing something good to balance the bad. Either of these processes might lead to increased compliance if the request is appropriate. Given the opportunity to engage in some extremely unpleasant behavior, the guilty person should be more likely to agree than the nonguilty because the former can view it as his good deed for the day which will make up for the bad deed about which he feels guilty. This line of reasoning leads to the hypothesis that guilt will lead to greater compliance in a wide variety of situations. (Freedman, Wallington & Bless, 1967, p. 117.)

Although unattributed, the source of these ideas seems clear, for the sense of the remarks indicates some sort of equilibrium model very similar to the Freudian position on neurotic guilt (Freud, 1949). To paraphrase: harm doing causes guilt, guilt feels bad and the person will soon do something to feel better. Freedman's formulation relies on the Freudian-like concept of social man as a self-serving organism. This position as expressed here (and Freedman's

is the most concise statement of it in the transgression-compliance literature), disregards the actor's relations to others who might be significant in the situation, such as the subject's partner or the experimenter. Not surprisingly, the methods used to test the guilt hypothesis also disregard the possibility that the typically used transgression-compliance laboratory procedures might mean something different to the participating subject. To anticipate the review which follows, the subject may believe he is merely following legitimate instructions and not really harming anyone. The lack of manipulation checks in these studies seems to result from the theoretical stance adopted in them and not from a lack of methodological sophistication.

Experimental evidence (Weber & Cook, 1972) and theoretical considerations from several sources (e.g., Alexander & Knight, 1971; Goffman, 1959; Harre & Secord, 1973) converge to suggest that the laboratory setting and situations typically used in this research should be considered a dynamic social situation in which subjects do not necessarily perceive the "stimulus situation" in the same manner as does the experimenter. For example, the experimenter assumes that the subject must feel his harm-doing is not excusable, but the subject may see it as excusable. A further consideration, expanded upon by Harre & Secord,

which motivated the following literature analysis is that if subjects do perceive the situations (especially the transgression procedures) differently from experimenter expectations, their subsequent actions could have for them a meaning very different from the one given it by the experimenter.

These ideas can be reformulated in the form of three questions: 1) What evidence is there to support the claim that what subjects believed they were doing was "transgressing-feeling guilty-complying"? 2) What other conjectures about what they experienced can be made from the results? 3) What other evidence can be gathered in support of these conjectures?

But these questions form only part of the problem. A further complication sets in when in these experimental situations the person of the experimenter is more than just the individual who sets up the conditions for the subjects. When the experimenter is also involved directly as the one transgressed against or the one subsequently helped, results must be interpreted in light of the evidence (e.g., Rosenthal & Rosnow, 1969; Tedeschi, Schlenker & Bonoma, 1971) that the intrusion of the person of the experimenter is an independent factor in addition to any factor deliberately manipulated in the procedures.

It is not being claimed here that the presence of the experimenter is confounded with the effect of prior transgression on subsequent helping; for if the experimenter is present he is so in all conditions of a given study. Rather, the point to be made concerns the elaboration of a theory of transgression-compliance and the ecological validity of the findings. Insofar as the presence of the experimenter serves in all conditions to elevate the magnitude of compliance, it is important to understand by what process(es) his presence has this effect. If this effect could be achieved by any other person who, for the subject, is an influential communicator of the norms of the situation, then the theory which accounts for the transgression-compliance effect should take this factor into account. If, on the other hand, the effect of the presence of the experimenter is due only to the special experimenter-subject interaction as a laboratory artifact, then the validity of generalizing the laboratory findings to life situations is put in doubt.

The task of the present review, then, is not only to answer the above three questions but to do so while trying to separate from other effects those effects possibly caused by the involvement of the person of the experimenter. It will be argued that such an analysis leads to a more adequate explanation of the transgression-compliance phenomenon.

A review. The five studies which provided reports of manipulation checks will be discussed in greater detail below. All experiments included in the present review are listed in Table 1. Table 1 indicates for each experiment the nature of the transgression manipulation used and the nature of the dependent measure used to operationalize helping. The procedures of those experiments which are without manipulation checks (the first nine listed and the two field studies) were described in a general way above (pp. 3-4), and little space will be expended in further discussion of them. (See Table 1.)

Examination of Table 2 will reveal several other things about these experiments. First, experiments listed in Table 2 have been classified according to whether or not the harm done was excusable or not. Two distinct features of many of the transgression procedures may be expected to have this effect. First, in a number of instances subjects were performing actions which were ostensibly a normal part of a legitimate experimental procedure, e.g., shocking a learner for errors. Second, in other cases subjects were involved in what must have been for them an unavoidable accident. In these latter cases the experiments' procedures require that no matter what a subject did, an "accident" befall him.

Table 1

Transgression procedures and helping
measures for each study reviewed

Study	Transgression	Helping
Brock & Becker, 1966*	E's apparatus blows up when touched by S	none
Wallace & Sadalla, 1966*	Apparatus blows up	S volunteers to participate in more experi- ments, will re- ceive electric shock
Darlington & Macker, 1966*	S fails to win points for a needy C, C wins points for S	donate blood
Freedman, Wallington & Bless, 1967*	S lies to E, denies knowledge of test	volunteer for more experi- ments
Freedman, <u>et al.</u> 1967*	S spills file cards, ruining them for use	volunteer for more experiments
Freedman, <u>et al.</u> 1967*	S spills file cards, ruining them for use	volunteer for more experiments
Carlsmith & Gross, 1969*	teacher-learner situa- tion with simulated shock	S agrees to make phone calls for a "Save the Redwoods" campaign
Carlsmith & Gross, 1969*	teacher-learner with shock	"Save the Redwoods"
McMillan, 1971	S lies to E, denies knowledge of test	volunteer for more experiments

Table 1 (continued)

Study	Transgression	Helping
Regan, J., 1971	E's machinery fouls up after S is induced by C to handle it	volunteer for more experi- ments
Cialdini, Darby & Vincent, 1973	teacher-learner with shock	volunteer for another experi- ment for friend of E
Geer & Jarnecky, 1973	teacher-learner with shock	reaction time in turning off C's shock
Studies reporting manipulation checks:		
Brock & Buss, 1964*	teacher-learner with shock	none
Heilman, Hodgson & Hornstein 1972	careless ruining of an experiment	reporting the accident, would aid the victim
Katz, Glass & Cohen, 1973	teacher-learner with shock	none
Noel, 1973	S gives false negative feedback to C	volunteer time to a social welfare program
Wallington, 1973	S lies to E, denies knowledge of test	none
Konecni, 1972	pedestrian accident (field study) (no manipulation check)	pick up spilled computer cards
Regan, Williams & Sparling, 1972	S "breaks" E's camera while taking his picture (field study)	pick up spilled merchandise

* Indicates a study reviewed by Brock, 1969.

Thus, in the former cases the apparent legitimacy of their actions suggests that little or no guilt may have been experienced by subjects. Indeed, in a great deal of research on obedience to authority (Milgram, 1974), not a single instance is reported in which "teaching" subjects expressed a sense of guilt over shocking a confederate, even when the confederate received shock which appeared to cause him great harm. In the cases described above as accidental, subjects, by definition, should not have perceived the effects of their actions as intended by them; so again the experience of guilt should be trivial, at best.

The procedures of each experiment have therefore been classified in Table 2 as either legitimate or illegitimate and as intended or unintended. Legitimacy and/or unintentionality should result in subjects' actions being perceived by themselves as excusable and therefore these procedures should be considered unlikely to have adequately generated significant guilt feelings. Since few of the experiments report manipulation checks for guilt, it seems appropriate to raise strong doubts as to the presence of the phenomenon on this basis alone.

In addition to the excusable or unexcusable nature of the transgression procedures, three other factors appear to run through the experiments in a somewhat systematic fashion. These have also been indicated in Table 2. Some-

times the requested help will undo or partially undo the harm that was just done by the subject. For example, a subject who has just wrecked a graduate student's file card system to be used in writing a dissertation is asked to volunteer time in helping that same student. This factor is identified as ameliorative (A) or not ameliorative (NA) helping. The second factor is whether the person to be helped is the victim of the original harm or a third party, independent of whether the actual helping is ameliorative or not. In the above example it could be that some other graduate student needs volunteer time. This factor is identified as original victim harmed (V) or a third party (3 P). Finally, and theoretically independent of the first two factors, is whether the person making the request for help is the experimenter or someone who is associated with the experimenter on the one hand, or someone not related to the person of the experimenter at all (E or NE).

A brief summary of the significance of these factors as they influence the validity of the guilt mediation hypothesis follows. Most important, it is proposed that the ameliorative nature of the consequences of helping is the most powerful determinant of the level of help offered by a transgressing subject. Second, since the focus of the guilt hypothesis is intrapersonal, the best test of it is in conditions in which the third person, a stranger in most

cases, is helped. Only five experiments report results for this type of condition; and in one of them helping was not significantly different from the helping of subjects in a non-transgression control group. Third, when the experimenter or a person associated with him makes the request, something which occurs in over half the experiments, there is a strong likelihood that this serves to elevate helping overall. This must suggest concern over the ecological validity of the findings, since the objective levels of helping involved are relatively small. Indeed, actual helping behavior, as opposed to intentions to help, is rarely measured.

The dependent measures of each experiment have been classified in Table 2 as to whether their operationalization involved assessments of beliefs and attitudes (e.g., derogation of self or victim), intention to help, or actual helping behavior. As can be seen, only six of the sixteen experiments listed actually used a behavioral measure of subsequent helpfulness. Insofar as the measures of intention obtained did not measure levels of specificity with respect to target, time, situation, and behavior (Fishbein & Ajzen, 1975), the results of the ten experiments which only obtained vague promises of help should be given less weight than results based on actual behavior. In general, intentions are not the same as helpfulness. (See Table 2.)

Table 2

Summary of unanalyzed but possibly significant factors for each study reviewed

Study	Nature of Transgression	Nature of help (ameliorative or not)	Who was aided (victim or third party)	Requester (E-related or not)	Dependent measure (belief or attitude; intention; behavior)
Brock & Becker, 1966	illegitimate unintentional excusable	NA	V	E	behavior
Wallace & Sadalla, 1966	illegitimate unintentional excusable	A	V	E	intention
Darlington & Macker, 1966	legitimate unintentional excusable	NA	3P	NE	intention
Freedman, Wallington & Bless, 1967	illegitimate intentional not excusable	NA	3P	E	intention
Freedman, <u>et al.</u> , 1967	illegitimate unintentional excusable	A	V	E	intention
Freedman, <u>et al.</u> , 1967	illegitimate unintentional excusable	A	V	E	intention

Table 2 (continued)

Study	Nature of Transgression	Nature of help (ameliorative or not)	Who was aided (victim or third party)	Requester (E-related or not)	Dependent measure (belief or attitude; intention; behavior)
Carlsmith & Gross, 1969	legitimate intentional excusable	NA	V	NE	intention
Carlsmith & Gross, 1969	legitimate intentional excusable	NA	V	NE	intention
McMillan, 1971	illegitimate intentional not excusable	A	V	E	intention
Regan, J., 1971	illegitimate intentional not excusable	A	V	E	behavior
Cialdini, Darby & Vincent, 1973	legitimate intentional excusable	NA	3P	E	intention
Geer & Jarnecky, 1973	legitimate intentional excusable	A	V	NE	behavior
Brock & Buss, 1964	legitimate intentional excusable	-	-	-	beliefs & attitudes
Heilman, Hodgson & Hornstein, 1972	illegitimate unintentional excusable	A	V	E	behavior

Table 2 (continued)

Study	Nature of Transgression	Nature of help ameliorative or not)	Who was aided (victim or third party)	Requester (E-related or not)	Dependent measure (belief or attitude; intention; behavior)
Katz, Glass & Cohen, 1973	legitimate intentional excusable	-	-	-	beliefs & attitudes
Noel, 1973	legitimate intentional excusable	NA	3P	NE	intention
Wallington, 1973	illegitimate intentional not excusable	-	-	-	beliefs & attitudes
Konecni, 1972	illegitimate unintentional excusable	A	V	NE	behavior
Regan, Williams & Sparling, 1972	legitimate unintentional excusable	NA	3P	NE	behavior

Table 3 is a 2 x 2 classification of experiments which used some measure of helping (either intention or behavior) as the main dependent variable. Procedures have been classified according to whether the transgression could plausibly be seen as excusable or unexcusable and whether the subsequent helping measure involved ameliorative helping or not. The remainder of this discussion will focus on the entries in this table.

Where transgression procedures involve the subject in unexcusable harm doing it would be expected that guilt induction is strongest. Only three experiments fall into this category and two of them used procedures in which helping was ameliorative of the harm done, confounding the hypothesized effects of guilt with those likely to be due to amelioration. From this point of view, only the first of three studies reported by Freedman, Wallington & Bless (1967) appears to be a completely adequate test of the guilt hypothesis. Of the twelve experiments which used transgression procedures which involve excusable harm doing, six used ameliorative helping procedures. Therefore, there remains only the task of explaining why in five out of the six remaining experiments in the excusable/not ameliorative cell increased helping occurred after transgression as compared with non-transgression or control groups. (See Table 3.)

Table 3

Classification of experiments according to whether transgression is excusable or not excusable and helping is ameliorative or not ameliorative

<u>Transgression</u>	<u>Helping</u>	
	Ameliorative	Not Ameliorative
Excusable	Wallace & Sadalla, '66	Darlington & Macker '66
	Freedman, Wallington & Bless, '67; #2 & #3	Cialdini, Darby & Vincent, '73
	Heilman, Hodgson & Hornstein, '72	Carlsmith & Gross, '69; #1 & #2
	Geer & Jarnecky, '73	Noel, '73
	Konecni, '72	Regan, Williams & Sparling, '72
Not Excusable	McMillan, '71	Freedman, Wallington & Bless, '67; #1
	Regan, '71	

Subjects in both the Darlington & Macker (1966) and the Cialdini, Darby & Vincent (1973) studies were asked to help by an associate of the experimenter. There is evidence to justify concern over this observation which is more specific to this paradigm than the general issue of the enhancing effects of the person of the experimenter. In a replication of the Freedman, et al. (1967) harm-doing by lying study, Rivera, Silverman, Chilenski & Tedeschi (1976) found that regardless of whether the subject had transgressed or not, when asked to volunteer time for studies in another department, those asked by the same experimenter with whom they had interacted all along complied significantly more than those who were asked by an "emergency substitute" experimenter who entered immediately prior to the request. The fact that the experiments were unassociated with the subject's present one presumably eliminated the ameliorative nature of the help. The measure of compliance was their stated intention to participate in another study. Those authors interpreted the results in terms of the subjects' attempts to manage a favorable impression of themselves before the experimenter who was more significant to them; but this need not concern the present discussion. The point is that in a typical guilt-compliance setting, the presence of the person of the experimenter seems to be enough to produce a compliance effect, for this main effect for

experimenter was independent of whether the subject had transgressed or not. The lie-no lie conditions did not produce a significant difference in degree of compliance.

As for the remaining experiments in the upper-right cell, subjects in both experiments by Carlsmith & Gross (1969) aided their original victims and so even though the help was not objectively ameliorative, subjects could have still perceived it as such.

From a total of nineteen experiments, the results of which are commonly taken as support for the guilt mediation explanation of the transgression-compliance phenomenon, only two (Freedman et al., 1967; Regan, Williams & Sparling, 1972) appear to be reasonably exact tests of the concept; and neither of these report results of manipulation checks.

With regard to the infrequent use of behavioral measures of helpfulness, Rivera et al. (1976) observed volunteering subjects to see if they did show up for their experiment. There were no effects of the independent variables on actual show-up rates. This suggests that the results of those experiments which confirmed the guilt hypothesis on the basis of measures of intentions to help must be considered extremely limited support.

Studies reporting manipulation checks. In a study by Heilman, Hodgson & Hornstein (1972) subjects were led to believe that they had caused either great or slight harm:

when they sat down at a table the dropleaf collapsed, spilling photographic slides belonging to another experimenter, one with whom the subject was not involved. Using notes placed in strategic locations, ostensibly left by the victim, subjects were informed as to the degree of importance of the slides (great or slight harm) and whether or not a timely report of the accident by them would allow for the damage to be undone (rectifiable or unrectifiable harm). The dependent measure was whether the subject reported the accident or not. All subjects were alone when the accident occurred, and they believed that they were unidentifiable as harm-doers if they left without reporting. In this 2 x 2 design, it was found that great harm-doing produced significantly more reporting than did slight harm-doing. When harm-doing was great, reporting occurred more often when the harm was rectifiable than when it was not, an interaction effect.

These results indicate the roles of severity of harm and of the ameliorative nature of the consequences of helping in determining the strength of the transgression-helping effect. Interestingly, reporters did not also volunteer time to the victim experimenter for another, unrelated, project. This is evidence that it was the ameliorative consequences of reporting which was significant, and not helping as such.

A second experiment replicated these findings and included a clever form of manipulation check. After the report of the accident (or after the subject had left the building without reporting), subjects were led to believe that the study was over and had been concerned with the effects of accidents on memory. In the guise of a recall test subjects were asked to report the contents of the notes which had conveyed the manipulations. In all conditions reporters were better able to recall the details of the notes than were non-reporters. Finally, fifteen percent of the total sample (evenly distributed across all conditions) became a self-selected control group by virtue of their not reading the notes at all. None of these reported the accident.

It seems, then, that much as in the case of helping, it is not harm-doing or even accidental harm-doing as such which is sufficient to produce the usual effect; but in addition to this, the meaning for the subject of both the harm-doing and the consequences of helping must also be taken into account.

A study by Katz, Glass & Cohen (1973) is notable here for the reason that the bogus teacher-learner paradigm with simulated shock was the transgression procedure used. Questionnaire data was also collected to check on felt guilt and responsibility for the pain of the confederate.

It was found that guilt varied directly with the degree of shock administered, although responsibility did not vary with intensity. Additionally, there was no correlation between guilt ratings and victim derogation, the major dependent measure of interest in this study. This is further support for the argument that the main consequences of interest should be the subsequent ameliorative actions of the harm-doer. A cognitive component presumably associated with a feeling of guilt, i.e., the knowledge that harm was done, apparently does not serve as partial mediator of just any post-transgression response but only of those responses classified as ameliorative. Subjects in this experiment could not do anything to change or make compensation for the suffering of the victim and so guilt was unrelated to the irrelevant response allowed them by the experiment.

Unfortunately, mean values for guilt or responsibility were not reported by Katz, et al., so there is no indication of the levels of these phenomena the subjects were experiencing. But a study by Brock and Buss (1964), which used the same teacher-learner procedure, did report the mean levels. In this study subjects were made to feel more or less justified in their actions as teacher (i.e., shocking the confederate) by either replicating the usual procedure and "randomly" determining that the subject was to be the teacher (low justification) or pretending that the subject

was selected as the better teacher between the two on the basis of a phoney paper-and-pencil measure of teaching ability (high justification). Intensity of shock (high or low) was also manipulated as a between subjects variable. After the usual errors and shocks were experienced, subjects filled out some questionnaire items which included reports of guilt and estimates of harm done to the confederate. Subjects in the low justification conditions reported more guilt and estimated more harm done than did subjects in the high justification conditions. Inspection of the mean values for guilt are of special interest, however. The overall mean value for guilt was below the neutral point of the scale; that is, overall, subjects reported little or no guilt. The difference in guilt across levels of justification seems to be due more to the difference between high and low shock levels, for in the high shock conditions the means for low and high justification are separated by only .3 of a point on a 50-point scale.

It seems, then, that where the subject of a guilt-compliance procedure is chosen to deliver shock to another in the setting of a legitimate experiment, randomly chosen or not, it is unlikely that much guilt is being experienced.

Noel (1973) induced subjects to believe that they voluntarily gave another subject false derogative feedback about an interview they had just taken part in. Subjects'

willingness to volunteer to make phone calls for an anti-pollution campaign was assessed as the dependent measure. Checks ascertained that subjects felt badly after giving the feedback and felt they had harmed the other. Nevertheless, no relation was obtained between this form of transgression and the measure of cooperation. It should be noted that the request did not come from the victim or from the experimenter or from anyone connected with either of them. When the experiment was repeated using instead a request to help a blind student by recording readings, again there was no effect on compliance to this request due to the transgression. While the behaviors requested of the subjects in these two cases would have been helpful to someone, they had no relation to the harm done the victim or to the subject's continuing relationship to the experimenter since requests occurred outside of the experimenter's apparent knowledge. The results indicate that there is something special about being asked for help by a victim or by an experimenter. If this is so, then it is up to the guilt theorist to explain what the presence of either of these persons has to do with the internal state of the subject and why a third party should not have this effect as well. Of course, it has already been shown that there is little or no guilt experienced in these experiments anyway.

Finally, Wallington (1973) operationalized harm-doing by allowing subjects to lie about prior knowledge of a test and used a number of self-report questions to assess elation, social affection, vigor, fatigue, sadness and positive and negative self-evaluation. None of these variables, some of which might be expected to be highly related to guilt, showed a relation to the lie/no lie manipulation.

A summary. Heilman et al.'s results indicate the importance of the meaning given to the harm done by the subject and the essential role played by the ameliorative nature of the consequences of helping. The results of Katz et al. indicate that even though a subject may feel guilty for having harmed another, this feeling does not necessarily lead to the performance of just any subsequent action in order to feel less guilty (i.e., victim derogation). Brock & Buss's results cast doubt on the claim that subjects feel guilty when engaged in shocking another as a legitimate part of an experimental procedure. Noel's results suggest that even if a subject feels badly about a transgression, he or she does not necessarily help a total stranger (as guilt theorists would argue) but only, as in most studies, the original victim or the experimenter. Finally, Wallington's results indicate an absence of "bad feelings" in the lie procedures used in one of the two studies whose results were not easily reinterpreted in the previous discussion which

focused on Table 3 (p. 21), i.e., Freedman et al. (1967). In conclusion, the weight of evidence is decidedly against a guilt mediation explanation and in favor of one which incorporates the consequences of helping and the expectations of others in the situation; in this case the victim and the experimenter.

Hypotheses and overview. These considerations lead directly to the following hypotheses. First, while subjects in a transgression procedure which is an ostensible part of a legitimate experiment may report some discomfort at their own actions toward a confederate, their responses to a question specifically probing for guilt feelings will show no guilt is experienced. Second, despite the absence of guilt there is still some harm being done; so, the transgression-compliance effect can be reproduced and will depend on the ameliorative nature of the help requested, more help occurring when it is ameliorative than when it is not.

These hypotheses were tested in the following experiment. Subjects participated in the standard teacher-learner situation (minus electric shock) and penalized a confederate for errors. Subsequently, they were given the opportunity to help either their original victim or a stranger on a task which would aid the requester in recouping losses incurred in the bogus learning task. The difference between these two conditions is that with the original victim, help-

ing would undo the harm done by the subject whereas with the stranger it would undo harm done by someone else. As a test of the hypothesis concerning the absence of guilt, two conditions were included in which subjects' feelings of responsibility for the harm done were manipulated. By experiencing a certain degree of choice about some of the procedures, they were made to feel more responsibility and by absence of choice they were made to feel less.

In this 2 x 2 design subjects either had more or less control over the harm done the learner, and they were asked for help which would help recoup losses by either their own original victim or a stranger.

C H A P T E R I I

AN EXPERIMENT

Method.

Personnel. Subjects were recruited from first and second year psychology courses on a voluntary basis and were paid \$2.50 for approximately an hour's participation. A total of 44 subjects (11 per cell) were included in the final analyses. Data from two other subjects were discarded randomly in order to equalize the cell n's. Confederates were five female undergraduates recruited from an honors section of social psychology. Each was trained in both confederate roles and was randomly assigned to roles and conditions. Each was paid \$2.00 for each subject they encountered.

Procedure. Subjects were told that they were participating in a study which was one in a series investigating how people feel about using highly structured teaching and learning techniques and how these feelings influence their subsequent performance on a related task. Care was taken to make procedures seem plausible by describing the ostensible practical implications of the research. Subjects were given these instructions along with the confederate. A

rigged random drawing was then held which determined that the confederate would be the learner in the first task. The subject was informed that he or she would be the teacher. The experimenter then asked the confederate-learner if she would agree to submit to a procedure in which she could lose some of her money, the \$2.50 which she had been promised for participation as a subject. She paused and then agreed. The experimenter then asked both subject and confederate for their consent to participate in the study as it had thus been described. Subjects expected that after the first task in which they were teachers, the two of them would be given an additional task which would be worked on independently by the subject and the confederate. Subjects therefore expected no further procedural interaction with the confederate once the first learning task was completed.

In general, the task involved a 20 trial anagrams solving procedure. The teacher-subject was to present each of 20 five-letter anagrams to the confederate-learner one at a time. The subject was to time the confederate each trial and allow her 20 seconds for an anagram's solution. Subjects kept a record of the procedures and penalized the confederate for errors by removing dimes for errors from her pay. Variations in this occurred depending on the Choice or No Choice conditions.

Choice conditions. The subject was presented with a list of 100 anagrams and was told to select 20 for the learner to solve. Subjects were told that the anagrams on the list were randomly mixed in terms of their difficulty (which was true) and that they were to use their own judgement as to which ones to use. Subjects then had the procedures described to them and were told that when the learner failed to solve an anagram within the time limit, that was to be considered an error and the subject should decide whether or not to remove a dime from the learner's pay for that error. Again, subjects were given no guidance as to how to decide about removing a dime, but they were told that this was their only means of influencing the learner's performance. After the subject had selected the 20 anagrams the learning trials began and the confederate always made 10 errors. The subject chose freely at each error whether to penalize the confederate or not.

No Choice conditions. Subjects were simply given 20 anagram cards and were instructed to use these for the learning task. Additionally, they were instructed to penalize each error made by the learner by removing a dime from her pay. Subjects in these conditions were yoked to subjects in the Choice conditions in terms of the number of dimes removed by them. Once a Choice subject had been run, the number of dimes freely removed by him or her was deter-

mined to be the number of errors to be made by the yoked confederate in the next No Choice condition (instead of the standard 10 errors made by all confederates in Choice conditions). If subject S-1 in the Choice condition penalized 7 out of the standard 10 errors, then subject S-2 in the No Choice condition experienced a learner who made 7 errors out of the 20 trials. These 7 errors would, due to the instructions, be automatically penalized by the subject.

All other procedures were kept the same for all subjects except for those specifically mentioned above. Once the experimenter had given the instructions for the anagrams task, he left the room and allowed the subject to carry out the anagrams task on his or her own. When it had been completed all subjects were given a questionnaire to fill out (as was the confederate, in order to keep the cover story going). They were told that the experimenter needed this information to prepare part two of the study for them. He reminded them that part two involved working alone. Once again the experimenter left the room. At this point the Victim-Stranger manipulation occurred.

Victim conditions. The experimenter returned to collect the questionnaires and announced that he would be gone for a few minutes while preparing part two of the study. He told the confederate that if she wished she could work on an additional task while he was absent and if she

did, he would return the money she had lost in the anagrams procedure. The confederate agreed, and the experimenter left the room for 10 minutes. After 3 minutes the confederate said to the subject (having studied the task for that length of time), "Boy, this looks hard. Do you think you could help me with it?" Confederates said no more than this to the subject and allowed the subject to do as little or as much as she or he chose to do.

Stranger conditions. When the experimenter returned to collect the questionnaires he brought with him confederate number two, whom he instructed to have a seat and to wait. The experimenter collected the questionnaires and said that he would be gone for a while preparing part two of the study. The learner-confederate interrupted at this point, asking how much longer the experiment would take. She claimed that she could not stay for the entire experiment because she just today remembered that she had a counseling appointment which she could not break. The experimenter made another appointment for her to return and complete part two of the study and assured the subject that this did not change anything for him or her, since part two was worked on individually. When the learner-confederate had picked up her money (minus the losses) and left, the experimenter turned to the second confederate.

The stranger-confederate reminded him that she was there in order to regain the money she had lost in a previous anagrams session. She reminded him that he had offered her this opportunity when she had lost her money. The experimenter acted as though he now remembered, gave her the task and left, saying that he had to look up how much she had lost and also telling the subject that he would return with part two shortly. After 3 minutes, the stranger-confederate made the same request as the one made by the learner-confederate in the Victim conditions.

In all conditions, the task involved making up anagrams according to a rigid procedure which was described on a sheet of instructions. First, a six-letter word was to be looked up in a dictionary. Then its frequency was to be found in Thorndike & Lorge's Teacher's Word Book of 30,000 Words (1944). Finally, as the anagram was being created, the frequency of each pair of letters was to be checked in Underwood & Schulz's Meaningfulness and Verbal Learning (1960, appendix D). The words and the bi-grams had to meet a maximum frequency criterion, otherwise the confederate was to start over again. The instructions asked that as many as possible be done. In all conditions, after 10 minutes of the experimenter's absence had elapsed, he returned, the confederate left, and the subject was extensively debriefed. All deceptions were revealed at this time.

Questionnaire and the dependent measure. Seven-point scales were used to assess subjects' feelings of choice, evaluations of the experience, and responsibility for the learner's outcome.¹ Choice 3 (3 questions): Did your own role in the anagrams portion of this experiment provide you with any choice which seemed acceptably good to you? To what extent did you, in your role, experience a meaningful choice about what you would do as teacher? To what extent do you feel your decisions as teacher reflect on your own personal assessment of the overall situation? Evaluation (3 questions): How attractive did you find the prospect of selecting the anagrams to be worked on? (Choice conditions only.) How attractive did you find the prospect of removing a dime for an error? Rate how attractive or unattractive you think the learner views his/her outcome in the anagrams procedure. Responsibility (5 questions): How strongly are you associated with the events which brought about the learner's outcome? How instrumental were you in bringing about the learner's outcome? How foreseeable was the learner's outcome to you? How much did you intend to bring about the learner's outcome? How justified was your action related to the learner's outcome? In addition, subjects indicated a self evaluation consisting of 4 bi-polar scales:² Satisfied . . . unsatisfied. Responsible . . . not responsible. Blameworthy . . . not blameworthy. Guilty . . . not guilty.

When the confederate who made the request left the lab she filled out a checklist which described the subject's helping behavior. The categories of this checklist were, "subject read the task instructions; subject gave advice, instructions, or directions to the confederate; subject used the task materials; subject created or wrote down anagrams."

Results

Two-by-two analyses of variance (Instructional Set: Choice/No Choice by Person Helped: Victim/Stranger) were conducted on the responses to each of the above questions as well as on the measure of helping and on the number of dimes taken by the subjects during the anagrams procedure. Subjects on the average took away forty cents overall; there were no significant differences between conditions for the amount of money taken away from the learner ($\bar{X} = 4.05$ dimes). Thus, the yoking procedure succeeded in controlling this variable between conditions.

The four-point scale derived from the helped confederate's checklist description of the subject's helping behavior formed a Guttman scale with a coefficient of reproducibility of .94 and a coefficient of minimal marginal reproducibility of .39. Analysis of variance here yielded a main effect for the identity of the requester (the person

helped) such that the victim was aided more than the stranger ($F = 13.573$; $p < .001$ ³). There was no effect for Instructional Set, nor was there an interaction effect. Table 4 presents the means for these results. (See Table 4.)

Subjects' estimates of the learner's attraction to her own outcome, an indirect index of how much harm the subjects thought they had committed, showed a main effect for the instructions variable such that Choice subjects estimated a lower evaluation by the learner of her outcome ($\bar{X} = 3.1818$) than did No Choice subjects ($\bar{X} = 4.4545$; $F = 8.133$, $p < .007$).

The three questions assessing choice correlated significantly ($r = +.31$ to $+.45$, all p 's $< .04$, two-tailed tests), and so these responses were combined to form one overall index of the degree of experienced choice. Scores could range from 3 (none at all) to 21 (very much). Analysis of variance of this index revealed a trend toward a main effect for Instructions ($F = 3.669$, $p < .06$) such that Choice subjects reported more choice ($\bar{X} = 13.7273$) than did No Choice subjects ($\bar{X} = 11.2273$).⁴

Degree of association was highly correlated with degree of instrumentality ($r = +.66$, $p < .001$)⁵ and with degree of responsibility ($r = +.32$, $p < .03$); instrumentality was in turn correlated with foreseeability ($r = +.37$, $p < .01$).

Table 4

Mean level of helping in each condition and the marginals. Data are scaled 1 to 4 with a higher number indicating more helping. Cell $n = 11$.

<u>Instructional</u> <u>Set</u>	<u>Person Helped</u>		
	Victim	Stranger	
Choice	3.1818	2.0000	2.5909
No Choice	3.2727	1.6364	2.4545
	3.2272	1.8182	

These scores were combined into a single overall index of felt responsibility which could range from 4 (none at all) to 28 (a high degree). Analysis of variance revealed a main effect for instructions ($F = 4.950$, $p < .03$) such that Choice subjects reported feeling more responsible for the learner's outcome ($\bar{X} = 17.8636$) than did No Choice subjects ($\bar{X} = 14.5909$). No other significant main effects, nor any interaction effects, were revealed by the analysis of variance on any other variable.

Table 5 presents the Pearson r correlation coefficient matrix for all subjects' responses on selected dependent measures. Intercorrelations among the questionnaire items assessing choice and those assessing responsibility have already been discussed. The two overall indices for these variables have been included in the matrix of Table 5. It should be noted that helping did not significantly correlate with any of the variables.

The number of dimes taken and, more especially, the estimate of the learner's attraction to or liking for her outcome can be viewed as an estimate of the degree of harm inflicted on the learner in the subject's view. As can be seen from Table 5, the former (dimes) was negatively related to how justified the subject felt ($r = +.32$, $p < .04$) and to reported satisfaction ($r = -.37$, $p < .01$). Thus, the more

dimes the subject took, the less justified and satisfied he or she felt. The estimation of the learner's attraction to her outcome was positively related to satisfaction ($\underline{r} = +.36, \underline{p} < .02$), and negatively related to blameworthiness ($\underline{r} = -.31, \underline{p} < .04$), guilt ($\underline{r} = -.44, \underline{p} < .003$), and responsibility ($\underline{r} = -.31, \underline{p} < .02$). Thus, the more damage that was thought to have been caused, the more responsible, blameworthy and guilty, and the less satisfied the subject felt. Finally, choice was positively related to intentionality ($\underline{r} = +.35, \underline{p} < .02$), which suggests that subjects inferred their own intention to cause the learner's outcome from the magnitude of the damage done and from their instructionally induced perceptions of choice.

Discussion

The first hypothesis predicted that while subjects may report mild discomfort after the anagrams procedure, they would report no guilt feelings. This was confirmed. Overall, subjects reported themselves to be "not blameworthy" ($\bar{X} = 2.8864$) and "not guilty" ($\bar{X} = 3.0000$) despite the fact that they did feel slightly responsible for the learner's outcome ($\bar{X} = 4.7045$) which they imagined the learner did not like ($\bar{X} = 2.2045$).

Table 5

Pearson r correlation coefficients for selected variables. All tests of significance are 2-tailed. For all r 's, $df = 42$ pairs.

degree of choice	no. dimes taken	S's estimate of C's attraction to outcome	responsibility	intentionality	how justified	how satisfied	how blameworthy	how guilty
choice	-.26	.05	.04	.35*	.23	.23	-.20	-.16
dimes		-.28	.05	-.25	-.32*	-.37#	.11	.19
estimate of learner's attr.			-.36*	.03	.06	.36*	-.31*	-.44#
responsibility				.26	-.17	.05	.36*	.33*
intentionality					.13	-.06	.22	.20
how justified						.02	.02	-.02
how satisfied							-.39#	-.44#
how blame-worthy								.81@
how guilty								

* indicates $p < .05$

indicates $p < .01$

@ indicates $p < .001$

It may be concluded that a simple guilt explanation is heavily overstated when it is applied to post-transgression compliance where the transgression is a legitimate part of an experimental procedure. This conclusion is further supported by the observation that while the Choice-No Choice manipulation succeeded in creating expected differences in degree of choice and responsibility experienced, as well as in estimates of the learner's liking for her outcome, guilt and blameworthiness were not so affected. These procedures were included as a direct test of the guilt hypothesis, and while they clearly had an effect on some aspects of the subject's experience, they did not produce the effects required by a strict guilt mediation explanation. This includes, but should not be thought of as limited to, the lack of a main effect for helping due to the manipulation of the choice instructions. It is only fair to add, however, that there was a tendency for helping and guilt to be positively related ($\underline{r} = +.27, \underline{p} < .08$).

A limitation to the inferential value of the guilt and blameworthiness data must be mentioned. These data are, in one sense, manipulation checks. As such, they show that the present operations failed to create the appropriate internal feeling-states which would provide an adequate test of the guilt hypothesis. Therefore, the first hypothesis could be seen as untested and unconfirmed.

Two objections can be raised concerning this criticism. First, the present experiment represents the first attempt to directly assess the presence of guilt feelings in the transgression-compliance situation. If this experiment failed to create the appropriate state, then all others like it, namely, all those in which a subject's harm doing is part of a legitimate experiment, have probably failed to do so, as well. This still leaves open the question of guilt's presence in the lie-telling and the accidental damage situations, however (e.g., Freedman, et al., #1, 1967; Regan, Williams & Sparling, 1972). Further study would be required before it could be concluded that guilt is an important mediating variable in these situations.

Second, manipulation checks are generally thought of as incidental data which assess subjects' ability to recognize or recall the objective events which transpire in an experimental procedure. These subject reports are for the most part veridical (see Fishbein & Ajzen, 1975, pp. 135-143). The present assessments of guilt and blameworthiness asked for a report on their feelings which resulted from those objective events. Therefore, the inference made here, that little or no guilt was experienced, is an inference about psychological states based not on manipulation checks but on subjects' own inferences about their own states.

The second hypothesis predicted that despite the absence of guilt feelings and the lack of relation between guilt and helping, helping would occur as a function of the ameliorative nature of its consequences. The Victim-Stranger conditions were included in order to test this hypothesis. The results support this interpretation. Subjects aided the original victim much more than they did the stranger, and it should be noted that the requested aid required the same activity of all subjects and would have had the same effect for the recipients. The difference is due, then, to the fact that in the Victim conditions, subjects were undoing the harm they themselves had caused, whereas in the Stranger conditions they were not, but were only undoing harm caused by someone else.

Important to note here is the absence of a direct check on whether subjects actually perceived their respective situations in these ways. This does not carry the same force, however, as the similar lack with respect to checks on guilt in prior studies. A subject's inferences about his feelings of guilt and about the ameliorative consequences of helping differ in two ways. The locus of one is internal and the other's is external. The inference of guilt is the identification of a response to a situation and the inference of ameliorative consequences is the recognition of the stimulus situation itself. A further study of this problem could be done, using subjects who read scenario descriptions of the

different conditions. If these subjects could recognize the objective difference between the Victim and Stranger conditions with respect to their consequences for amelioration, then further support for hypothesis two would be provided. It is less certain, however, that a similar approach could be used with respect to guilt since it is likely that the identification of this internal response requires experiencing the events as the subjects in the behavioral study did. Whether scenario reading subjects could replicate behavioral subjects' responses about guilt or not, these results would not help to ascertain the presence of the behavioral subjects' responses of guilt to the stimulus situations described by the legitimate transgression and the ameliorative consequences of helping.

It could be argued that a subject's feeling of embarrassment (Apsler, 1975), being confounded with the ameliorative nature of the consequences of helping in the present operations, is what caused the results. That is, embarrassed subjects who are confronted with their original victim will be more likely to help in order to remove their shame or embarrassment in the victim's eyes. It is unfortunate that no questions about embarrassment or shame were asked which would provide data which directly addresses this objection. Nevertheless, subjects overall reported

feeling satisfied ($\bar{X} = 5.0000$) and this feeling seems somewhat antithetical to feelings of embarrassment. Therefore, while not conclusive, the available evidence seems not to support this objection.

It is fairly clear that explanations of the transgression-compliance phenomenon based solely on such concepts as guilt, "negative state relief" (e.g., Cialdini, Darby & Vincent, 1973), self-esteem maintenance (McMillan, 1971), and general arousal mediated by responsibility (Geer & Jarnecky, 1973) would not predict the present results on helping due to the ameliorative nature of the consequences. The results do appear, however, to be consistent with Brock's (1969) hypothesis of a need for consistency of fate control over another. In this analysis the helpful or harmful features of the actions performed are not as important as the fact that the present subjects in the Victim conditions were acting on a need to maintain equivalent fate control over the same target person. But this position ignores the evidence that all subjects (and Choice subjects more than the others) do recognize a harmful effect on the learner and do feel responsible for it. The consistency explanation does not delve deeply enough into the subject's own experience.

The present study demonstrates that the more finely the experience of a subject is investigated, the more pre-

cise a prediction of helping after transgression can be made. The identity of the requester, and consequently who's harm doing is to be undone, will determine whether much helping will occur in spite of the possible strength of the covert elements.

A second explanation consistent with the results is that subjects in the Victim conditions have established and have been forced to maintain a minimal relationship with their partners. Subjects in the Stranger conditions, however, are able in a sense, to "start from zero" with their new partners. This idea predicts greater subsequent involvement with the original partner than with the stranger on almost any dimension which is reasonably appropriate or normative. Since the only opportunity for subsequent involvement in the present study is the request for help, the results on this variable are only spuriously related to the prior transgression and the ameliorative consequences of helping. Control conditions in which subjects interact with their partners without taking money away would have been useful for addressing this alternate explanation. The establishment of such a minimal relationship during the anagrams task would lead to a pattern of results between a same-partner and a different-partner control group which would be identical to the present results.

Unfortunately, these data are not available. If, in a further study, subjects' levels of helping in the control conditions are equivalent and in turn are equivalent to the help given in the Stranger conditions, then this alternative explanation will have been disconfirmed, and the presnet hypothesis further supported.

Two important results of the present paper are to describe the degree to which the literature on transgression-compliance lacks internal validity, and to clarify what some overlooked sources of variation are in the typical procedures. Yet how generalizable, how ecologically valid, are these research results? The most striking inadequacy of this whole line of research (the present experiment included) is that it sheds little light on the nature of these phenomena in established relationships. All subjects, confederates, and experimenters have been complete strangers to one another in all the reports to date. If we are interested in these processes because they shed light on mechanisms of socialization and social cohesion, then they must be investigated among pairs and groups of people who are already in relationships with one another; whether these relationships be personal, institutional, or a combination of these. For it is rare outside the laboratory that strangers come into contact for a long enough time to allow processes described here to be initiated or affected;

and people mostly interact with people they already know. Given the institutional embeddedness of the experimenter-subject and the subject-subject relationships, it seems that the more immediate steps along this line could be taken by looking at people in other kinds of institutional relationships; in the classroom, the workplace, or perhaps in the helping professions.

Research on the transgression-compliance effect has diminished in the last six to eight years after a short peak of interest. Perhaps this is partly due to a feeling that the phenomenon had been adequately established and accounted for. Certainly, another reason is the general switch to research on social perception and judgement in general. This paper has attempted to show that what was assumed to occur in the research reviewed is only part of the story. In addition it is one step in the direction of bringing behavior, in this case post-transgression behavior, back to social perception research. The elements for this desirable union are present: responsibility and choice inferences, costs and benefits of action; it is a small step to move from an actor's own post-transgression acts to those of observers or victims.

Some interesting questions for future research along these lines concern the perceptions and attributions of third-party observers to the transgression-compliance

situation. For example, is post-transgression helping seen as an "admission of guilt"? If so, what effect will this have on observers' reactions to the actor? Will an attempt to ameliorate harm done in turn serve to ameliorate an observer's assignment of blame or punishment? Or will post-transgression helping only be seen as a confession? Additionally, what is the transgressor's "implicit attribution theory" about what observers will infer about him and how might this affect his own post-transgression responses?

The closing of the "attribution loop," i.e., from actor to observer and back to the actor again, via the investigation of an actor's implicit theories of how others will view his actions, is a tantalizing follow-up to the present integration of the transgression-compliance literature. It is one that seems an appropriate extension of the attribution literature as a whole.

FOOTNOTES

1. For these questions, 1 represents no amount of the entity described, 7 represents an extremely high amount, and 4 represents a neutral point.
2. The four bi-polar scales ranged from +3 to -3.
3. For all \underline{F} 's, $\underline{df} = 1, 40$.
4. In the separate analyses of variance for each of the three questions assessing choice, all \underline{F} 's < 1 except for two. These were the \underline{F} 's for instructional set (choice/no choice) on the "good choice" question ($\underline{F} = 3.617, \underline{p} < .064$) and on the "personal assessment" question ($\underline{F} = 1.820, \underline{p} < .185$).
5. For all \underline{r} 's, $\underline{df} = 42$.

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