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THE EFFECT OF EQUITABLE PREMANIPULATION ATTITUDES ON SUBSEQUENT ATTITUDE CHANGE AND RECALL UNDER FORCED COMPLIANCE VERSUS INTERPERSONAL SIMULATION AND DIFFERENTIAL DEMAND CONDITIONS

by

Barry Hall Kinney

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1971
I hereby recommend that this dissertation prepared under my direction by Barry Hall Kinney entitled The Effect of Equated Premanipulation Attitudes on Subsequent Attitude Change and Recall Under Forced Compliance versus Interpersonal Simulation and Differential Demand Conditions, be accepted as fulfilling the dissertation requirement of the degree of Doctor of Philosophy.

Dissertation Director Date

After inspection of the final copy of the dissertation, the following members of the Final Examination Committee concur in its approval and recommend its acceptance:

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ABSTRACT

The present study investigated the differential effects of forced compliance and interpersonal simulation formats on subsequent attitude change and the recall of initial attitudes. Levels of justification for compliance were manipulated within each of the formats, and accuracy versus consistency recall demand conditions were manipulated both in the experimental, and in the control, groups. One hundred Ss were equated for premanipulation attitudes on the subject of student violence and were randomly assigned to each of the experimental and control groups. Significant differences in both attitude change and accuracy of recall between the two format conditions were interpreted as evidence supporting the hypotheses that counterattitudinal behavior would lead to greater attitude change than would the interpersonal simulations, and, because of that attitude change, Ss who engaged in counterattitudinal behavior would recall their initial attitudes less accurately than would Ss who did not engage in such behavior.
CHAPTER I
INTRODUCTION

Statement of the Problem and Plan of Dissertation

Statement of the Problem

Since the early reports of Kelman (1953), Janis and King (1954), and King and Janis (1956), two major schools of research have addressed themselves to the effects of counter-attitudinal behavior on changes in the referent attitude. The "dissonance" approach, founded by Leon Festinger in 1957, has generally been placed in opposition to the "incentive" approach, advocated by Janis, King, Gilmore, and their associates (e.g., Janis and Gilmore, 1965; Janis and King, 1954; King and Janis, 1956).

Festinger's (1957) theory of cognitive dissonance, very simply stated, maintains that inconsistency among related beliefs—and between beliefs and actions—produces internal, motivational tension called dissonance. Dissonance motivates one to regain cognitive consistency, often by means of an attitude change that resolves the inconsistency. Dissonance arousal is greater, according to the dissonance position, when sufficient but minimal justification or incentive is given for counterattitudinal behavior. Since the dissonance arousal is greater under those conditions, the attitude change required for dissonance reduction
must, itself, be greater. It is precisely on that point that the incentive advocates differ with the dissonance advocates. The incentive theorists (e.g., Janis and Gilmore, 1965) hold to the more "obvious" claim that, under most circumstances, attitude change will follow counter-attitudinal behavior to an extent positively related to the amount of justification or incentive given for the counter-attitudinal behavior. The more justifying reasons given for the counterattitudinal behavior, for example, the more attitude change would be expected to follow.

The great majority of the research reports addressed to the issue of justification or incentive effects has provided support for the dissonance position. That is, most studies have found the "inverse-incentive" or "inverse-justification" effects to hold. That body of research will be reviewed in the following chapter.

A more recent challenge to the dissonance position has been presented by the work of Bem (e.g., 1965, 1967a). Bem's self-persuasion model has offered an alternative explanation of the inverse-incentive phenomena found in many of the forced compliance studies. Bem maintains that a subject who engages in counterattitudinal behavior does not experience an increase in tension (dissonance arousal) that requires reduction through attitude change. According to Bem, the subject simply "updates" his own information regarding his own attitudes by inferring his attitudinal
stance from his new behavior. The process of updating requires, however, that the stimulus situation is one that has been associated with truth-telling, rather than with lying. Bem uses Skinner's (1957) distinctions between "mands" and "tacts" to aid in the discrimination of truth- and lie-telling situations. A "mand" is a verbal response that is under the specific control of some portion of the environment (e.g., "That's a butterfly."). Statements made in situations with high mand characteristics, even though the statements may be disguised as tacts (e.g., a celebrity on television says, "I like Sanka coffee."), tend to hold less credibility than statements made under conditions with primarily tact characteristics (e.g., a man says to his wife, "I like Sanka coffee."). Situations with primarily tact characteristics are very similar to those whose stimulus characteristics are said to be necessary for dissonance arousal (e.g., Carlsmith, 1968). For example, according to the dissonance position (e.g., Brehm and Cohen, 1959), justification or incentive for counterattitudinal behavior should be minimal for the greatest dissonance arousal to take place. Bem (1965, 1967a) argued that minimizing the justification or incentive for counterattitudinal speeches, for example, merely increases the tact characteristics—and, thereby, the credibility—of that communication. Bem further argued that the credibility is increased both for the audience and for the communicator,
himself. The controversy regarding the self-persuasion interpretation of the dissonance advocates' data has continued. At this point in time, both the dissonance advocates and the self-persuasion advocates seem to have agreed that each position holds some territory not held by the other, and that the choice of one position over the other in areas of overlap has become primarily a matter of loyalty (e.g., Linder and Jones, 1969; Bem and McConnell, 1970). A more detailed review and discussion of the self-persuasion controversy will be presented below in Chapter III.

Areas of ambiguity in the dissonance versus self-persuasion controversy remain, however, and those areas provide the questions to which the present paper is addressed.

1. Many of the studies providing early support for the dissonance position have been seriously criticized for their methodological inadequacies, including the failure of some to accurately assess premanipulation attitudes of the participating subjects (e.g., Chapanis and Chapanis, 1964).

2. The interpersonal simulations employed by Bem and his associates to provide support for the self-persuasion position have been criticized by members of the dissonance camp (e.g., Piliavin et al., 1969) as unfair replications of early forced compliance
experiments. They were said to be unfair particularly with regard to the omission of the original S's premanipulation attitudes in the input statements of the simulations.

3. Although Bem and McConnell (1970), in an experiment designed to respond to the dissonance advocates' criticism of the input statements, stated that the "demand characteristics" of their 1970 study should have been felt in the direction of accuracy of recall rather than consistency of recall, such demand thrust was not assessed or demonstrated. The simple statement that such was the thrust of the demand characteristics of their study seems to be insufficient evidence to that effect.

4. A fourth area of ambiguity in the controversy between the self-persuasion model and the dissonance model concerns the effect of Bem's interpersonal simulations on the attitude statements of the subject-observers who participate in the simulations. It seems possible that inducing an S to estimate the attitudes of other Ss on current issues of concern and importance would have the effect of "updating" the attitudinal positions of the subject involved.
In an attempt to resolve some of the ambiguity involved in the Bem-dissonance controversy, the present experiment did:

1. Equate all groups for the Ss' premanipulation attitudes.

2. Assess the recall of premanipulation attitudes following the experimental manipulations.

3. Provide for clear demand characteristics for both accuracy and consistency of recall of premanipulation attitudes, assessing differences in recall as a function of varied demand.

4. Assess the attitudes of both forced compliance Ss and interpersonal simulation Ss.

In addition to the major experimental treatments listed above, certain steps were taken to avoid experimenter effect or bias (e.g., Rosenthal, 1963, 1966) and to assess the phenomenologies of the Ss regarding the experimental procedures:

5. The person administering the treatments was "blind" regarding the purposes, hypotheses, and background of the experiment. He was unaware, as well, of the group assignments of the various Ss.

6. A post-experimental questionnaire was administered to all Ss to determine their notions regarding:
   a. The purpose of the experiment.
b. The hypotheses of the experimenter.

c. The manner of administration of the experiment.

d. Their change, if any, in attitudes.

e. Their reactions to any perceived deceptions.

A detailed Methods section, in Chapter IV, provides a rationale for, and a discussion of, the above points.

Plan of Dissertation

Following a brief summary of the background of research in counterattitudinal behavior, the remainder of Chapter II is devoted to the more common and serious methodological inadequacies of the experiments reviewed.

Chapter III presents and discusses Bem's self-persuasion model and its impact on the dissonance position.

Chapter IV presents the major hypotheses of the dissertation, and contains the Methods section.

Chapters V and VI present the Results of the experiment and a Discussion and Conclusions section, respectively.
CHAPTER II

BACKGROUND OF STUDIES IN COUNTERATTITUDINAL BEHAVIOR

Overview

The effects of counterattitudinal behavior on subsequent changes in the referent attitude have been widely researched under the labels "role-playing," "forced-compliance," and, more recently, "self-persuasion." While the labels generally aid in discrimination among the researchers allied in various theoretical camps, they have not proven to be helpful in the categorization of the behaviors induced or methods employed in the experiments. For example, Janis and Gilmore (1965) and Elms and Janis (1965) call the behaviors induced in their experiments "role-playing" whereas the actual tasks employed in the studies require the Ss to write essays. On the other hand, investigators labeling their research "forced-compliance" (e.g., Brehm, 1959; Festinger and Carlsmith, 1959) use a role-playing task as the induced counterattitudinal behavior.

Whereas the tasks employed in the counterattitudinal behavior studies provide some basis for categorization, the issues addressed by the various theoretical camps provide a clearer basis for meaningful delineation of crucial—versus
trivial—differences among the various approaches to this content area. Those issues delineate the areas of contention between the dissonance advocates and those theorists holding to the incentive approach, and have generally fallen into three categories: (1) justification effects, (2) incentive effects, and (3) choice or volition effects. On the first two of those issues, the incentive theorists and those advocating the dissonance approach clearly differ. The advocates of the incentive position generally predict that the relationship between amount of justification or incentive for the counterattitudinal behavior induced, and consequent attitude change, will be positive. The dissonance theorists predict that, given the proper conditions for dissonance arousal (e.g., Carlsmith, 1968), there will be a negative relationship between the amount of justification or incentive given for the counterattitudinal behavior and the amount of consequent attitude change. Both positions seem to predict that the amount of perceived choice in compliance to the experimenter's requests will be positively related to the amount of consequent attitude change.

**Review of Investigations in Counterattitudinal Behavior**

The behaviors induced in the experiments discussed in the present paper generally fall into two categories: (1) role-playing, and (2) essay-writing. The example of a
study in each of those categories serves to illustrate the
typical format employed.

Role-Playing

In a study designed to test the dissonance posi-
tion's negative-incentive effect, Festinger and Carlsmith
(1959) paid Ss either $1 or $20 to tell someone else that a
purportedly boring task was actually quite enjoyable and
interesting. The Ss were induced to engage in the "boring"
task and were then told that a co-worker of E's had failed
to appear for an interview and that the S's help would be
appreciated. The S was asked to tell "the next subject"
(actually a confederate of E's) that the task had been
interesting and enjoyable. The Ss who were paid $1 for
telling the other person that the task was interesting later
rated the task as more enjoyable than did the Ss in the $20
condition or those in the control condition. Festinger and
Carlsmith interpreted those results as supporting the dis-
sonance position by showing a negative incentive effect.

Essay-Writing

Cohen (cited in Brehm and Cohen, 1962) induced Ss,
supposedly initially opposed to the New Haven Police actions
during a student disturbance at Yale University, to write
essays supporting the position of the police. Incentive was
manipulated by offering the Ss either $.50, $1, or $5 for
compliance with E's request to write the essays. After
having written the essays, the Ss were asked to state their opinions regarding how justified the police actions were. Cohen's data indicated that the dissonance prediction of a negative incentive effect was supported. That is, there appeared to be a negative relationship between the amount of incentive given and the degree to which the Ss considered the police actions as justified.

The remainder of the experiments addressed to the effects of counterattitudinal behavior on attitude change will be reviewed with the focus of the review on the issues addressed by each of them.

Justification for Compliance

Cohen, Brehm, and Fleming (1958) divided 92 Ss into high or low justification groups and induced them to write essays taking positions against their own attitudes regarding the admission of female students to Yale. The Ss in the high justification group were given several more reasons for compliance than were those in the low justification group. The Ss in the low justification group tended to move more in their attitude statements toward the position advocated in their essays than did the Ss in the high justification condition. Those results were seen as supporting the inverse-justification position predicted by dissonance theory.
Rabbie, Brehm, and Cohen (1959), in the same manner as in the Cohen et al. (1958) study, manipulated justification for counterattitudinal essay-writing. The findings were also essentially identical with those of the Cohen et al. (1958) study: the less the justification given for the counterattitudinal behavior, the greater the attitude change.

A portion of Brock and Blackwood's (1962) experiment was addressed to the differential effects of high and low justification. Half of the Ss were given several justifying reasons for writing counterattitudinal essays and half of the Ss were given only minimal justification. Changes in the Ss' opinions in the expected direction were significantly more pronounced in the low justification condition than they were in the high justification condition.

The general finding of the above experiments seems quite clear: in accord with predictions of the dissonance position, an inverse justification effect was found in all of the studies. That is, the less the justification given for counterattitudinal behavior, the greater the subsequent change in attitude.

Incentive

The amount of incentive given for compliance to an experimenter's request to engage in counterattitudinal behavior may be viewed as a special case of the amount of
justification given. Rather than providing the _Ss_ with more or fewer justifying reasons for compliance, the experimenters investigating the effects of varied incentive for counterattitudinal behavior simply offer more or less money for compliance. The dissonance position's predictions regarding incentive effects are identical to those regarding justification effects: the more incentive given for compliance, the less the consequent attitude change.

The Festinger and Carlsmith (1959) experiment has been described above. In summary, their _Ss_, after having engaged in an ostensibly boring task, were asked to lie to confederates of the experimenter to the effect that the task was actually interesting and enjoyable. Incentive was manipulated by paying the _Ss_ either $1 or $20 for compliance. As predicted, the _Ss_ in the one collar condition rated the "boring" task as more interesting than did the _Ss_ in the twenty-dollar condition, following the experiment.

Cohen's (cited in Brehm and Cohen, 1962) study addressed itself to the incentive issue with a greater variety of different incentives for compliance. Cohen's _Ss_ were offered $.50, $1, $5, or $10 for writing essays purportedly counter to their attitudes regarding the actions of the New Haven Police during a recent student disturbance at Yale University. The data, showing an inverse incentive effect, generally, were seen as lending support to the dissonance position. Cohen's assumption regarding the _Ss'
initial attitudes is somewhat questionable, and will be further discussed below in the Comments section of the present chapter.

Two experiments have revealed data supporting both a positive- and a negative-incentive effect.

Carlsmith, Collins, and Helmreich (1966) developed an experimental format designed to separate the effects of role-playing, essay-writing, and incentive. Half of their Ss were induced to tell "another subject" that a presumably boring task was fun and interesting, while the remainder of the Ss wrote essays to the same effect. The Ss were paid either $.50, $1.50, or $5 for engaging in the behaviors requested of them. Control Ss worked on the "boring" task and completed the same posttest as the one the experimental Ss were given. Under the role-playing (face-to-face) conditions, the Ss provided data showing an inverse incentive effect. Data from Ss in the essay conditions, however, revealed a positive incentive effect.

Helmreich and Collins (1968) induced Ss to deliver speeches advocating compulsory birth control, a presumably counterattitudinal position. The Ss were paid either $.50 or $2.50 for making either anonymous audio- (low commitment condition) or identified video-tape (high commitment condition) recordings, ostensibly against their initial attitudes regarding compulsory birth control. An interaction effect appeared such that, in the low-commitment conditions, the Ss
paid more money supported compulsory birth control on the posttest to a greater degree than did _Ss_ paid less. In the high-commitment condition, the opposite was found to be the case.

Two experiments conducted by advocates of the incentive position have provided some support for that model.

Elms and Janis (1965), in an attempt to separate the effects of sponsorship variables and incentive variables, asked _Ss_ to write essays in favor of sending American students to study the Soviet system of government and the history of Communism in a Russian university for four years. In what Elms and Janis called the negative sponsorship conditions, the Soviet Embassy was said to be the sponsor of the essay-writing while, the positive sponsorship conditions had the U. S. State Department as the supposed sponsor. Incentives were manipulated by paying the _Ss_ from $.50 to $10. Under the "positive sponsorship" conditions, higher incentives were significantly related to more positive attitudinal statements with regard to the position advocated in the essays. No significant relationship was found between the incentive conditions and the attitude statements under the negative sponsorship conditions.

Janis and Gilmore (1965) induced _Ss_ to write essays in favor of requiring all college undergraduates to have additional courses in mathematics and science. Janis and
Gilmore assumed that such an educational policy was generally unpopular. Sponsorship variables were manipulated by telling the Ss in the "negative" sponsorship conditions that the essays were sponsored by a textbook firm and by telling the Ss in the "positive" sponsorship conditions that the sponsor was a public welfare representative. Incentive conditions were manipulated by paying the Ss either $1 or $20 for writing the essays. Although no significant differences were found between the high and low incentive groups, the maximum amount of "attitude change" was found in the group given the larger amount of money under the favorable sponsorship conditions.

In summary, while there is some evidence (Elms and Janis, 1965) that, under very specific conditions, a positive incentive effect may occur, it seems generally to be the case that the inverse incentive effect predicted by dissonance theory is found.

Choice

Both incentive and dissonance advocates have predicted that maximal attitude change will occur under conditions of high (perceived) choice.

Kelman (1953) manipulated choice in an essay-writing task by emphasizing to the high choice groups that they could refuse to write the essays. Kelman found, as predicted, that, while compliance was highest in the low choice
groups, attitude change was greatest in the high choice groups.

Brehm and Cohen (1959) manipulated choice in the same manner as Kelman (1953). They also manipulated "relative deprivation" by telling Ss that they had been randomly chosen to be deprived of the "usual reward" of either $1 or $10 for engaging in a supposedly boring task. An inverse incentive prediction was made; that is, greater relative deprivation was expected to arouse greater dissonance and lead to greater positive evaluation of the "boring" task. Brehm and Cohen predicted that such dissonance arousal and attitude change would take place only under the high choice conditions. Their data revealed trends to the effect that data from Ss in the low choice conditions showed the inverse incentive effect that was expected to appear only under the high choice conditions. The Ss in the high choice conditions, as well, tended to show greater liking for the task if they were in the groups that were more deprived. The Brehm and Cohen (1959) study suffers from some of the methodological and statistical inadequacies to be discussed below, in the Comments section of this chapter.

Brock (1962) induced non-Catholic Yale students to write essays entitled "Why I Would Like To Become a Catholic," under high and low choice conditions. Half of the Ss were confronted with the discrepancy between their attitudes and their behavior (confrontation condition). As
Brock predicted, trends were found in the direction of higher attitude change among the Ss in the high choice conditions. Brock also found an interaction of choice with confrontation conditions that was significant and indicated to Brock that, under high choice conditions, awareness of the meaning of a counterattitudinal behavior and deliberation on its implications increased attitude change. Under low choice conditions, however, Brock found that emphasis of the attitude-behavior discrepancy reduced attitude change.

In a later experiment, Brock (1968) attempted to separate the relative effects of volition and justification for compliance on subsequence attitude change. He induced Ss under either high or low choice conditions to engage in a supposedly boring task. Justification was manipulated by offering either few or many reasons to the Ss for their compliance. The Ss in the high volition conditions reported greater enjoyment of the "boring" task than did Ss in the low volition conditions. Only under low volition conditions did Ss show an inverse justification effect.

**Comments on Methodological Inadequacies**

**Summary of the Chapanis and Chapanis (1964) Critique**

Chapanis and Chapanis (1964) attacked the early forced compliance studies on several grounds. A summary of
the areas of concern and some examples of methodological flaws of studies in each area will follow.

1. Confounded Variables. Chapanis and Chapanis (1964) questioned whether "dissonance" alone was generated in the experiments in forced compliance. For example, in the Aronson and Mills (1959) initiation experiment, according to Chapanis and Chapanis, the Ss may well have experienced relief, pleasure, or a sense of accomplishment at having successfully passed the "initiation" which took the form of inducing female Ss to read sexual material to a male experimenter. The possibility that the Ss in the Festinger and Carlsmith (1959) study were incredulous of the experimental procedure was raised by Chapanis and Chapanis as another example of the confounding of internal states like "dissonance," "relief," or "incredulity."

2. Experimental Anomolies. A procedural anomaly most seriously criticized by Chapanis and Chapanis (1964) was the relatively common practice in forced compliance studies of arbitrarily eliminating the data of Ss who failed to perform according to the hypothesis. At least seven of the early forced compliance studies were cited by Chapanis and Chapanis as having discarded from 10% to 65% of the cases involved. Another flaw found by Chapanis and Chapanis in several of the forced compliance studies was that "... authors tend to present results as significant
and as supporting the dissonance theory prediction when the probabilities are greater than the usually accepted value of .05 [p. 20]." Calling that flaw straining for significance, Chapanis and Chapanis pointed out a closely related problem, capitalizing on chance. Several studies were cited in which the main effects were ignored while several interaction effects were analyzed or in which control conditions produced data as significant as those found in the experimental conditions. The Brehm and Cohen (1959) experiment will serve to exemplify: Choice conditions were analyzed separately for each of five sections of an introductory psychology class; on the basis of their medians on the perceived choice rating scale.

Separate interaction t's were calculated for each of the five sections. The N's in each cell were very small, ranging between 3 and 10 with an average of about 7. The probability values for these 5 interaction t's showed that one was significant, two tended to significance, and two were nonsignificant (one was actually a reversal). Here again the authors' failure to compute and report the results of an overall test make it exceedingly difficult for readers to interpret their findings (Chapanis and Chapanis, 1964, p. 19).

In addition to the specific flaws listed above, Chapanis and Chapanis cited twelve studies in which (1) control groups were missing, (2) overall significance of main effects was not presented, (3) groups had not been equated for initial attitudes, or (4) two or more of the above criticisms held.
Further Comment and Critique

Many studies in counterattitudinal behavior have been reported since the Chapanis and Chapanis (1964) review, and some that were reported earlier than 1964 were omitted from that review. The presentation of some examples of experimental problems found in those studies seems warranted.

Experimental and Statistical Anomolies

Davis and Jones (1960), investigating the effects of choice on re-evaluation of a stimulus person (SP), conducted an experiment that suffered from one of the more common flaws cited by Chapanis and Chapanis (1964); namely, that of the arbitrary rejection of cases. Twelve of 52 cases were dropped from statistical analysis because, of the 12 Ss, "6 of these were suspicious that the SP was not as presented, and 6 either balked at the task presented them or later indicated that they were subjectively in a different condition from that implied by the induction [Davis and Jones, 1960, p. 403]." While it is unfortunate that the experimental manipulations failed to convince these Ss, it seems procedurally questionable to reject their data; at least, if authors modify conventional procedural groundrules, one would expect them to replicate their results on a new sample before claiming significant results.

Freedman (1965) attempted to replicate the Aronson and Carlsmith (1963) study and claimed that the results of
his study supported his hypothesis. Actually, however, none of the differences between the groups approached significance.

Helmreich and Collins (1968) reported that, after five previously unsuccessful attempts to obtain a negative incentive effect finding, they managed to find significant results in the predicted direction. As was the case with the Brehm and Cohen (1959) experiment, the possibility of alpha error in the Helmreich and Collins (1968) study ought not to be ignored.

Kiesler and De Salvo (1967) contended that their hypothesis had been supported by their results even though most of their treatments produced nonsignificant results and those findings which they reported as significant had \( p \) values ranging from .06 to .10.

Assumptions Regarding Premanipulation Attitudes

In many of the studies addressed to the effects of counterattitudinal behavior on subsequent attitude change, either (1) the initial attitudes of the Ss were assumed by the experimenters rather than assessed; thus, the "counterattitudinal behavior" was not clearly counter to the attitudinal positions of the Ss; or (2) "inherently" aversive, boring, or embarrassing tasks were simply assumed to be such without assessment, often, of the Ss' views of
the tasks, nor by prior determination of task-valance on a separate, standardization, sample.

1. One of the major problems to be addressed by the present study is the assumption, rather than the assessment, of premanipulation attitudes in many of the studies in counterattitudinal behavior; a flaw that seems strangely common in a literature addressed to attitude change. Some examples follow.

Kelman (1953) assumed that Ss in his sample preferred a certain kind of comic book to another. That assumption was made by inference from preliminary interviews with children who "were comparable to our Ss [p. 191]." Cohen (cited in Brehm and Cohen, 1962) assumed that his sample of Yale students held the same anti-police attitude that was held by a majority of the Yale student body. In both these cases, one would need a large standardization pre-sample which demonstrated very small attitude variability before it would be sufficient to conclude that a random (usually small) subset of Ss from a pre-measured general population could be expected, simply by their membership in the larger population, to hold a given pre-experimental attitude position. It would, of course, seem easier for research purposes to merely obtain pre-experimental attitude judgments from the Ss to be studied,
which pre-measures, further, would permit matching of treatment condition groups for attitudinal position.

Janis and Gilmore (1965) assumed that students were initially opposed to adding science and mathematics requirements to undergraduate curricula. Linder and Jones (1969) assumed that students at Duke University were initially more in favor of a capitalistic system than to democratic socialism. In view of the involvement of many college students in anti-establishment movements in recent years, the Linder and Jones assumptions seem questionable.

2. Several of the studies in this content area used tasks that were supposed to be aversive to the Ss. In many cases, the aversiveness of the tasks was assumed, but not assessed. Two examples follow:

Aronson and Mills (1959) assumed that reading "sexy" material would be "embarrassing" to Ss. Actually, however, their Ss (all females) may well have found sexual verbalizations in the presence of a male experimenter quite titillating. Festinger and Carlsmith (1959) assumed that a simple motor task would be "boring and uninteresting."

The assumption of Festinger and Carlsmith (1959) seems more reasonable than does that of Aronson and Mills (1959). The point, however, remains that the demonstration of attitude change following counterattitudinal behavior
ought to include the assessment of the attitude before, and the attitude after, the experimental manipulations.
CHAPTER III

BEM'S SELF-PERSUASION MODEL

Overview

The data presented in reports of forced compliance experiments have been subjected to serious question and have even been viewed as predominantly artifactual by some investigators (e.g., Chapanis and Chapanis, 1964; Rosenberg, 1965).

While not necessarily accepting all of the dissonance data as valid, Bem (1965, 1967a) has taken an entirely different approach by questioning the underlying assumptions of dissonance theory. Referring to his position as "radical behaviorism," Bem offers an alternative explanation of the forced compliance results, especially those results of studies addressed to the effects of varied justification or incentive. Bem's self-persuasion model holds that people may learn to label their own attitudes as they learn to infer the attitudes of others; i.e., they may observe their own behavior and infer therefrom their attitudes.

Self-descriptive statements that appear to be exclusively under the discriminative control of private stimulation may, in fact, remain under the control of the same public events which members of the community themselves must use in "inferring" the individual's inner states. In
our well-fed society, for example, it is not uncommon to find a man consulting his wrist watch to answer the question, "Are you hungry?" (Bem, 1965, p. 199).

Bem continues:

... when the answer to the question, "Do you like brown bread?" is, "I guess I do, I'm always eating it," it seems unnecessary to invoke a fount of privileged self-knowledge to account for the reply. In this example, it is clear that the discriminative stimuli controlling the attitude statement reside in the individual's overt behavior; indeed, the man's reply is functionally equivalent to the reply his wife might give for him: "I guess he does, he is always eating it."

It is the major thesis of this report, then, that an individual's belief and attitude statements and the beliefs and attitudes that an outside observer would attribute to him are often functionally equivalent in that both sets of statements are "inferences" from the same evidence: the public events that the socializing community originally employed in training the individual to make such self-descriptive statements (pp. 199-200).

Bem uses Skinner's (1957) differentiation between "mands" and "tacts" (see above, pp. 3-4) to provide the basis for his interpretation of data from many of the forced compliance experiments. Bem relates the problem of mand-tact discrimination to communicator credibility:

It is clear, then, that in attempting to infer a speaker's "true" beliefs and attitudes, the listener must often discriminate the mand-tact characteristics of the communication. This is, in fact, an important dimension of "communicator credibility." A communicator is credible to the extent that his communication is discriminated as a set of tacts, and his credibility is vitiated to the extent that he appears to be manding in the form of disguised tacts (Bem, 1965, p. 201).
A major tenet of Bem's self-persuasion model is that a credible communicator is not only likely to convince others, he is just as likely to convince himself that he believes what he is saying if he views his own communication as a set of tacts rather than mands. That is so because his inferences regarding his attitudes and internal states are a function of essentially the same discriminative stimuli as others' inferences about them; that is, his behavior and its context.

In his 1965 report, Bem reinterpreted the results of several of the early forced-compliance experiments, beginning with the 1959 Festinger and Carlsmith study. Bem noted that an outside observer of that experiment, who heard an individual making very favorable statements about the motor tasks employed, and who knew that the individual had been paid $1 or $20 for making the statements, would be likely to interpret the $20 communication as a mand. Having thus discriminated the mand-tact characteristics of the communications, the outside observer would then assume that the communication was not actually descriptive of the motor task but was, instead, a mand for $20. He would further assume that the person making the communication probably felt about the task as would a member of the control group; that is, as any person chosen at random. Although the $1 communication, too, had some mand characteristics, the outside observer would be far more prone to judge it, than
he would the $20 communication, as a tact. By definition, then, the $1 communication would be judged to be more credible and to be more likely reflective of the true belief of the person making it. At this point, Bem suggests, placing the hypothetical observer and the experimental subject in the same skin results in the findings reported by Festinger and Carlsmith (1959). That is, a subject in their $1 condition, more than one in the $20 condition, heard himself describe a task under conditions generally associated in his past with tacting rather than manding. He then persuaded himself that the motor task must not have been so bad after all. He saw himself communicating in the context of stimuli that generally had set the occasion for truthful communication. One of the experiments reported in Bem's (1965) paper was directly addressed to the effect of varying contexts on self-persuasability. The Ss who were pre-trained to lie in the presence of a colored light (lie-light) and tell the truth in the presence of a light of a different color (truth-light) were then induced to rank cartoons (previously rated as "neutral") as "extremely funny" or "extremely unfunny." Those cartoons so ranked in the presence of the truth light, when later rated by the Ss, appeared significantly more distant from the "neutral" mark than did those rated in the presence of the lie-light. The

\(^{1}\text{E.g., perhaps as a token offered for participation in the research.}\)
Ss, according to Bern, who heard themselves remark on the cartoons in a setting (truth-light on) that had been associated with telling the truth were more persuaded by their own statements than those Ss hearing themselves under conditions setting the occasion for lying (lie-light on).

The Interpersonal Simulations

Bern's first interpersonal simulation was an attempt to replicate the Cohen (cited in Brehm and Cohen, 1962) experiment using subject-observers whose task was to estimate the attitude statements of the original Cohen Ss. Cohen's incentive conditions, which were less dissimilar across the conditions than were the Festinger-Carlsmith (1959) conditions, were designed to rule out the possibility that the inverse incentive effect found by Festinger and Carlsmith might have been a function of the experiment's lack of credibility. Bern noted that his explanation of the inverse incentive results was similar to Cohen's communicator credibility explanation and attempted to simulate Cohen's experiment using subject-observers. Bern randomly divided sixty undergraduates into "50¢," "$1," and "control" conditions. From a description of the Cohen study, each of the subject-observers was to estimate the "actual opinion" of a student willing to write an essay in favor of the police actions during the Yale student disturbance. Depending on the group to which the subject-observer had
been assigned, he was told that the student writing the essay was paid $.50 or $1, or was merely asked to estimate the opinion of a student chosen at random. The estimates given by the subject-observers of Cohen's Ss' opinions closely matched Cohen's data on their actual opinion responses. It is in the above manner that Bem's subject-observers take the place of the hypothetical outside-observer discussed above in regard to the Festinger-Carlsmith (1959) data.

Bem (1965) also simulated an experiment reported by Brehm (cited in Brehm and Cohen, 1962) in which Brehm had Ss commit themselves to lengthy periods of food deprivation and report their intensity of hunger feelings. Bem's subject-observers closely matched the estimates of the amount of hunger reported by the original Ss.

In a later report, Bem (1967a) described an interpersonal simulation of the Festinger-Carlsmith (1959) study. Bem's subject-observers successfully estimated the differential ratings of the "boring" task by the original Ss. The experimental format of that simulation was essentially identical to those reported in Bem's (1965) earlier report.

Critique of the Interpersonal Simulations

Criticism of the interpersonal simulations as evidence supporting Bem's self-persuasion model was rapid in appearance. Mills (1967) argued that Bem's interpersonal
replications had failed to demonstrate that the subject-observers could estimate changes in the attitudes of the Ss in the original experiments.

Jones et al. (1968) held that Bem had "loaded" the simulations by failing to mention in the "input statements" the premanipulation attitudes held by the original Ss, and pointed out that, when such information is provided to the subject-observers, they fail to accurately estimate the postmanipulation attitudes of the Ss in the original experiments. The Jones team cited several simulations in which the premanipulation attitudes of the original Ss are included in the input, and in which the observers failed to reproduce the results of the study simulated.

Piliavin et al. (1969) replicated Bem's 1965 simulations with two major modifications. First, they presented "a more exact" script of the Cohen (cited in Brehm and Cohen, 1962) study including mention of the Ss' premanipulation attitudes. Secondly, they asked their subject-observers to write essays explaining why they estimated the performances of Cohen's Ss as they had. The authors found that their subject-observers failed to perform as did Cohen's Ss or as did Bem's subject-observers, and they viewed this discrepancy as evidence that Bem's simulations were faulty. The authors further argued that money was not a salient dimension in the subject-observers' estimates of the attitudes of Cohen's Ss. Instead, many of their
subject-observers cited "biased scanning" (vide Janis, 1968) of new information as the most relevant dimension for their estimates of attitudes of the original Ss.

The Piliavin group joined Jones et al. (1968) in pointing to Bem's failure to include the premanipulation attitudes of the original Ss in the simulation input statements provided to the subject-observers.

Linder and Jones (1969) successfully replicated Bem's (1965) experiment using truth- and lie-lights to demonstrate the stimulus control of self-persuasion. Linder and Jones pointed out, however, that their results were significant in conditions providing the illusion of choice to the Ss, but not in other conditions. The authors viewed this finding as lending support to the dissonance position because of the choice manipulation. They conceded, however, that both their dissonance-derived model and Bem's self-persuasion model could account for their results.

... indeed, it is not at all clear that dissonance theory and an amended self-judgment model will lead to different predictions in any forced compliance experiment or in any instance of counter-attitudinal advocacy (p. 481).

Bem's Response

Bem (1967b) claimed that his critics had missed the point of his interpersonal simulations when they mentioned the failure of the simulations to demonstrate that the subject-observers could estimate attitude change. Attitude
change, according to Bem, "is a psychological reality only for an experimenter who can compare Ss' ratings with those of a control group [p. 537]." The real task of the subject-observers, Bem argued, is to "stand in" for the original Ss and reproduce phenomenologically functional relationships in the original experiments.

Furthermore, Bem responded to the question of the appropriate input statements for the simulations by pointing out that, unless the observers are permitted to hear or view the counterattitudinal behavior of the original Ss, the premanipulation attitudes are not appropriate portions of the input statements. The original Ss, according to Bem, have "updated" their attitudes by means of having engaged in the counterattitudinal behavior, rendering their premanipulation attitudes non-salient. As evidence for that claim, Bem and McConnell (1970) presented data showing that experimental Ss failed to recall accurately their premanipulation attitudes in a forced compliance study. In fact, the Ss incorrectly recalled their premanipulation attitudes as being identical with their postmanipulation attitudes. Citing a study by Jones, Bem (1968) pointed out that subject-observers could replicate the dissonance experiments' findings when given the initial attitudes of the original Ss, if the subject-observers were permitted to actually observe the original Ss engaging in the counterattitudinal behavior. The Jones finding, and the others cited above,
Bem suggests, indicates that when Bem's critics replicate the simulation experiments, and use the correct input information, they do obtain results virtually identical to those found in the Bem simulations.
CHAPTER IV

METHODS AND HYPOTHESES

Method

1. Subjects: One hundred undergraduate students in psychology classes at The University of Arizona were matched for initial attitudes on an issue regarding violence in student demonstrations, and were randomly assigned to eight experimental, and two control, groups.

2. The experimental design: For the assessment of attitude change, a 2 x 2 factorial design was employed in which the factors were: (a) forced compliance versus interpersonal simulation, (b) high versus low justification, and (c) accuracy versus consistency recall demand characteristics. The experimental groups were randomly assigned to eight experimental, and two control, blocks, with n = 10 in each block.

3. The dependent variables included: (a) the amount of attitude change in each group, (b) the accuracy of recall of initial attitudes for each group, (c) the estimated scale scores of forced compliance Ss by the subject-observers in the interpersonal
simulation conditions, and (d) information regarding the phenomenologies of all Ss with regard to attitude change and experimenter bias.

4. The experimental sequence: During the first week of class in the Fall, 1970 semester, the premanipulation attitude questionnaires (see Appendix C) were administered by a psychology graduate student to all students in several psychology 1-B quiz sections, who were gathered together in a large lecture setting. The students were asked to identify their questionnaires with their names. They were told that the questionnaires represented an "attempt to assess current student attitudes regarding various social issues of importance." (All verbal and written instructions are presented in Appendices A and B.) From the results of the initial attitude questionnaires, those Ss who were to participate in the experiment were selected; that is, those who held exactly the same attitudinal position on the issue selected.

As was the case in the Bem and McConnell (1970) experiment, one week's delay was allowed between the administration of the initial questionnaires and the second experimental sessions. At that time, the experimental Ss were administered either the forced compliance treatments or the interpersonal
simulation treatments. The controls received no treatment between their initial, and postmanipulation, questionnaires. The postmanipulation questionnaires were given, following the treatments, by the same graduate student who administered the initial ones, and who administered the treatments to the experimental Ss. Following the postmanipulation questionnaires, post-experimental questionnaires were administered.

5. Description of treatments:

a. Forced compliance: The Ss assigned to the forced compliance treatments were asked, under two levels of justification—either one or four justifying reasons for the counterattitudinal essay writing—to write short, one-paragraph essays, "including at least three reasonable points," contrary to their initial attitudinal position on the issue.

b. Interpersonal simulation: The Ss assigned to the interpersonal simulation conditions were asked to estimate the "actual opinion" of an S in one or the other of the two justification conditions in the forced compliance format.

c. Recall-demand conditions: After having completed the postmanipulation questionnaires, each S was asked to recall his initial attitude
regarding the same issue. The manner in which he was asked to do so depended on which of the two recall-demand conditions he had been assigned to: accuracy-demand or consistency-demand. The Ss assigned to the accuracy-demand conditions were asked to accurately recall their initial attitudes. In the consistency-demand conditions, the Ss were told, "We are interested in the degree to which students are consistent in their attitudes." They were then asked to recall their initial attitudes.

d. Control conditions: Each control group was given the pre- and the post-manipulation questionnaires and was asked to recall initial attitudes under one of the two recall-demand conditions delineated above.

6. The postexperimental questionnaire: A postexperimental questionnaire was administered to all Ss following their participation in the experiment. A copy of that questionnaire is presented in Appendix E. For the purposes of the present study, two major questions asked on the questionnaire were of primary interest:

a. Perceived attitude change. In order to explicate the status of attitude change in the phenomonologies of the Ss, it was considered
important to determine each S's perception of his own attitude change, or lack of change.

b. Perceived experimenter effects or bias.

Although precautions were taken to avoid experimenter bias—for example, by having a "blind" experimenter present all verbal instructions—it was considered important to check on the S's perceptions of the experiment with particular focusing on any perceived bias or interference from the experimenter.

Hypotheses

1. Main effects: It was hypothesized that attitude change would be greater under the forced compliance conditions than under the interpersonal simulation conditions because, under the latter conditions, no counterattitudinal behavior took place and little attitude change was expected to result from merely reading about engaging in counterattitudinal behavior. It was further hypothesized that greater attitude change would occur under the low justification conditions than under the high justification conditions. That prediction clearly followed from the majority of the reported results of experiments in both the forced compliance and the interpersonal simulation formats which showed that greater
attitude change often had been found under conditions of minimal justification for the counterattitudinal behavior. The third main effect hypothesis was that the accuracy of recall of initial attitudes would be greater under the accuracy-recall-demand conditions than under the consistency-recall-demand conditions. Such an hypothesis seemed to follow logically from the expected effect of the demand characteristics. A fourth hypothesis, relating solely to the interpersonal simulation portion of the experiment, was that the subject-observers who estimated the opinions of Ss in the low justification condition of the forced compliance format would estimate those opinions as closer to the attitude expressed in the counterattitudinal essays than would be the case with subject-observers who were to estimate the attitudes of Ss in the high justification conditions. That prediction was made because it clearly followed from the reported results of earlier interpersonal simulations.

2. Interaction effects: Three interaction effects were anticipated: one for attitude change and two for recall error scores. An interaction between the type of experimental format (forced compliance and interpersonal simulation) and the justification
level (high and low) was expected such that, under the high justification conditions, a similar and low amount of attitude change would occur in both the forced compliance and the simulation formats. Under the low justification conditions, however, significantly more attitude change was expected to occur in the forced compliance format than in the simulation format. This result was hypothesized because the justification manipulations were expected to be more powerful in the forced compliance format, in which actual counterattitudinal behavior took place, than in the simulation conditions, in which the Ss merely read about the counterattitudinal behavior of Ss in the forced compliance format. The first interaction effect predicted, regarding accuracy of recall, followed directly from the preceding interaction hypothesis regarding attitude change. It was hypothesized that the type of experimental format (forced compliance and interpersonal simulation) and the justification level (high and low) would interact such that, under the high justification conditions, a similar and high degree of accuracy of recall would be found for both types of experimental format. Under low justification conditions, however, accuracy of recall was expected to be significantly lower for Ss in the forced compliance conditions
than for those in the simulation conditions, because, as was predicted above, greater attitude change would occur in the forced compliance format, under low justification conditions, than would occur in the simulation format. The greater the attitude change, the more interference with accurate recall of initial attitudes was to be expected. The second recall interaction effect predicted was one between the type of experimental format (forced compliance and interpersonal simulation) and the type of demand characteristics employed (accuracy-demand and consistency-demand) such that both experimental formats were expected to allow a similar level of recall accuracy under the accuracy-recall-demand conditions, whereas, under the consistency-demand conditions, less accurate recall of initial attitudes was expected in the forced compliance format than in the simulation format. The reason for that prediction was the same as that for the first predicted recall interaction effect, namely, that greater attitude change, and more recall interference, was expected to occur under the forced compliance than under the simulation conditions. The third recall interaction effect hypothesized was an interaction between the demand conditions (accuracy and consistency) and the
justification conditions (high and low). Under the accuracy-demand conditions, each level of justification was expected to allow similar recall accuracy. Under the consistency-demand conditions, the high justification conditions were expected to allow greater accuracy of recall than were the low justification conditions, because of the expected greater attitude change in the low justifications which were expected to create greater dissimilarity between initial and postmanipulation attitudes. Thus, the greater similarity between the initial and the postmanipulation attitudes expected under the high justification conditions were expected to allow Ss in those conditions to respond consistently and accurately to a greater extent than would be the case for Ss in the low justification conditions. That is, while the Ss in the low justification conditions were presumably responding to demand characteristics that subtly request them to incorrectly recall their initial attitudes as consistent with their current (i.e., postmanipulation) ones, the Ss in the high justification conditions would be responding to a demand to correctly recall their initial attitudes as consistent with their current ones.
CHAPTER V

RESULTS

Attitude Change

The primary question to be answered by the present results concerned the relative amount of attitude change in the various experimental groups. A secondary, but nonetheless important, question concerned the conditions under which attitude change was significantly greater than any changes seen in the control groups. Table 1 presents a summary of the results of a 2 x 2 analysis of variance on attitude change scores, comparing the two format conditions (forced compliance and interpersonal simulation) and the two levels of justification (high and low). It was predicted that the Ss in the low justification conditions would change in their attitudinal positions more than would Ss in the high justification conditions. While the difference in the amount of attitude change found in the two justification conditions was in the predicted direction, that difference was nonsignificant. As can be seen in Table 1, the predicted greater attitude change among Ss in the forced compliance format conditions, compared to Ss' attitude change in the interpersonal simulation conditions, was found
Table 1. 2 x 2 factorial analysis of variance summary table: attitude change scores.

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>Mean square</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total</td>
<td>79</td>
<td>.505</td>
<td></td>
</tr>
<tr>
<td>Between</td>
<td>3</td>
<td>1.513</td>
<td></td>
</tr>
<tr>
<td>A. FC x IPS</td>
<td>1</td>
<td>3.613</td>
<td>7.7666*</td>
</tr>
<tr>
<td>B. Low x High Justification</td>
<td>1</td>
<td>.313</td>
<td>.6719</td>
</tr>
<tr>
<td>A x B</td>
<td>1</td>
<td>.612</td>
<td>1.3168</td>
</tr>
<tr>
<td>Within</td>
<td>76</td>
<td>.465</td>
<td></td>
</tr>
</tbody>
</table>

*p < .007.

to be significant (p < .007). Thus, as predicted, those Ss who actually performed counterattitudinal behavior changed more in the direction of the position argued in the counterattitudinal essays than did those Ss who merely read about the forced compliance manipulations and estimated the attitudes of the forced compliance Ss.

The hypothesized interaction effect between the experimental format type (forced compliance and interpersonal simulation) and the justification levels (high and low) was in the predicted direction. That is, within the interpersonal simulation condition, both the Ss in the high

1. Although the direction of all significant results obtained had been hypothesized in advance, the significance levels reported throughout this paper are based on two-tailed probability estimates.
and those in the low justification conditions changed very little in attitudinal position whereas, in the forced compliance format, the Ss in the low justification condition changed in attitudinal position more than did the Ss in the high justification condition. The difference in attitudinal change between the Ss in the low justification condition and those in the high justification condition (within the forced compliance format) was substantial, as seen in Table 2, but failed to reach significance.

Table 2. Mean attitude change scores of groups in 2 x 2 analysis of variance.

<table>
<thead>
<tr>
<th>Group</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>Forced compliance</td>
<td></td>
</tr>
<tr>
<td>High justification</td>
<td>.60</td>
</tr>
<tr>
<td>Forced compliance</td>
<td></td>
</tr>
<tr>
<td>Low justification</td>
<td>.90</td>
</tr>
<tr>
<td>Interpersonal simulation</td>
<td></td>
</tr>
<tr>
<td>High justification</td>
<td>.35</td>
</tr>
<tr>
<td>Interpersonal simulation</td>
<td></td>
</tr>
<tr>
<td>Low justification</td>
<td>.30</td>
</tr>
</tbody>
</table>

In order to compare the two format conditions (forced compliance and interpersonal simulation) with the control conditions, a one-way analysis of variance was conducted with three pooled groups: (1) forced compliance, (2) interpersonal simulation, and (3) controls. The forced
compliance conditions were pooled by collapsing the justification levels and the accuracy-consistency demand levels. The collapsing of the justification levels was deemed warranted because the justification manipulations did not, in the above described analysis, account for significant variance in attitude change scores either separately or in an interaction effect with the format conditions. The accuracy-consistency demand conditions were collapsed because they were irrelevant to attitude change, and are addressed only to attitude recall differences; in fact, they follow, in the experimental sequence, the postmanipulation attitude questionnaire. The interpersonal simulation conditions were pooled in the same manner as were the forced compliance conditions; and for the same reasons. The two control conditions (accuracy and consistency recall-demand conditions) were pooled because they were found not to differ significantly either in terms of attitude change scores or in terms of attitude recall-error scores.

Table 3 presents a summary of the one-way analysis of variance comparing the three pooled groups (forced compliance, interpersonal simulation, and controls). The groups were found to vary significantly with regard to attitude change scores (p < .004). In order to make specific comparisons among the three groups, the Tukey technique (Myers, 1966; Winer, 1962) was employed. That particular technique for ad hoc comparisons was employed for
Table 3. One-way analysis of variance summary table: attitude change scores: Forced Compliance vs. Interpersonal Simulation vs. Controls.

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total</td>
<td>99</td>
<td>.4541</td>
<td></td>
</tr>
<tr>
<td>Groups (FC x IPS x C)</td>
<td>2</td>
<td>2.4675</td>
<td>5.980*</td>
</tr>
<tr>
<td>Error (groups)</td>
<td>97</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*p < .004.

two reasons. First, the Tukey technique is considered by Winer (1962) to be conservative enough to prevent the serious "alpha slippage" often found when individual comparisons are made among several groups. Secondly, the Tukey technique is considered, again by Winer (1962), to be powerful enough to allow nonspurious results to attain acceptable levels of significance. The results of the comparisons by the Tukey technique indicated that the Ss in the forced compliance conditions produced significantly higher attitude change scores than did Ss in either the interpersonal simulation conditions (p < .05) or those in the control conditions (p < .01). There was no significant difference between the attitude change scores of the Ss in the interpersonal simulation conditions and those of the Ss in the control conditions. Thus, the primary hypothesis of the present study is seen as having been confirmed. That
is, Ss who actually engaged in counterattitudinal essay-writing (in the forced compliance conditions) changed from their initial attitudinal positions significantly more than did Ss (in the interpersonal simulation conditions) who merely read about other Ss' performing counterattitudinal behavior. Furthermore, those Ss who engaged in counterattitudinal behavior changed in attitudinal position significantly more than did control Ss who were merely asked for their attitudes at two different times.

Recall Error Scores

An issue subsidiary to the attitude change question, but important with regard to the controversy regarding Bem's interpersonal simulations (e.g., Bem and McConnell, 1970; Jones et al., 1968), is that of the relative accuracy of recall of initial attitudes among the Ss assigned to the various treatments in the present experiment. Table 4 presents a summary of an overall 2 x 2 x 2 factorial analysis of variance in which the three factors compared were: (1) format conditions (forced compliance and interpersonal simulation), (2) levels of justification (high and low), and (3) recall-demand conditions (accuracy and consistency). The expected main effect, in which Ss in the accuracy-recall-demand conditions were expected to make fewer errors in recall of initial attitude than did Ss in the consistency-recall-demand conditions, was in the
Table 4. 2 x 2 x 2 analysis of variance summary table: attitude recall error scores.

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total</td>
<td>79</td>
<td>.430</td>
<td></td>
</tr>
<tr>
<td>Between</td>
<td>7</td>
<td>.641</td>
<td></td>
</tr>
<tr>
<td>A. FC x IPS</td>
<td>1</td>
<td>2.813</td>
<td>6.844*</td>
</tr>
<tr>
<td>B. High x Low Justification</td>
<td>1</td>
<td>.013</td>
<td>.0305</td>
</tr>
<tr>
<td>C. Accuracy x Consistency</td>
<td>1</td>
<td>.113</td>
<td>.2746</td>
</tr>
<tr>
<td>A x B</td>
<td>1</td>
<td>.012</td>
<td>.0305</td>
</tr>
<tr>
<td>A x C</td>
<td>1</td>
<td>.612</td>
<td>1.4949</td>
</tr>
<tr>
<td>B x C</td>
<td>1</td>
<td>.612</td>
<td>1.4949</td>
</tr>
<tr>
<td>A x B x C</td>
<td>1</td>
<td>.313</td>
<td>.7627</td>
</tr>
</tbody>
</table>

*p < .011.

predicted direction, but the difference was nonsignificant. It was further expected that, due to greater expected attitude change under the low justification conditions, compared to the high justification conditions, that Ss in the high justification conditions would make fewer errors in recall of initial attitudes. Again, while the results were in the predicted direction, they were far from significant. A third expectation with regard to attitude recall-error scores was that the Ss in the forced compliance conditions, because of their expected greater attitude change, would
make more errors in attempting to recall their initial attitudes than would the Ss in the interpersonal simulation conditions. That expectation was confirmed by the analysis presented in Table 4; the significance level was beyond \( p = .011 \).

An interaction effect between the experimental format conditions and the justification levels was predicted such that, under the high justification conditions, a similar and high degree of accuracy of recall would be allowed under both format conditions, while, under the low justification conditions, more attitude change would occur in the forced compliance groups than would be seen in the simulation groups, leading to greater accuracy of recall of initial attitudes in the latter groups. While the results were in the expected direction, the interaction effect predicted was nonsignificant.

An interaction effect was predicted between the recall-demand conditions and the justification levels such that, under the accuracy-demand conditions, each level of justification was expected to allow similar, and high, accuracy of recall. Under the consistency-demand conditions, the high justification conditions, which were expected to produce less attitude change, were predicted to allow greater accuracy of recall of initial attitudes than the low justification conditions. That interaction effect was not found, even as a tendency, due primarily, it seems,
to the almost complete ineffectiveness of the demand condition manipulations.

In order to compare the two format conditions with the control conditions, a one-way analysis of variance of recall-error scores was conducted with three pooled groups: (1) forced compliance conditions, (2) interpersonal simulation conditions, and (3) control conditions. The justification and recall-demand conditions within the three groups were pooled, in this case, in a manner identical with the pooling of the same three groups for the analysis of variance of attitude change scores described above. The results of the analysis of variance for the three groups are presented in Table 5. The three groups were found to differ significantly (p < .005) from one another in recall error scores. Employing the Tukey technique, an ad hoc series of specific comparisons was conducted. Those comparisons revealed that the recall error scores of the Ss in the forced compliance conditions were significantly higher than those of Ss in either the interpersonal simulation conditions (p < .05) or those of Ss in the control conditions (p < .01). There was no significant difference between the recall error scores of the Ss in the pooled interpersonal simulation conditions and those of Ss in the pooled control conditions. Those results are seen as confirming the expectation that the Ss in the forced compliance conditions would recall their initial attitudes less accurately than
Table 5. One-way analysis of variance summary table: attitude recall error scores: Forced Compliance x Interpersonal Simulation x Controls.

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total</td>
<td>99</td>
<td>.3890</td>
<td></td>
</tr>
<tr>
<td>Groups</td>
<td>2</td>
<td>2.0675</td>
<td>5.834*</td>
</tr>
<tr>
<td>Error (groups)</td>
<td>97</td>
<td>.3544</td>
<td></td>
</tr>
</tbody>
</table>

*p < .005.

would the Ss in the interpersonal simulation conditions or those in the control conditions. That expectation was held because of the expected differences in attitude change among the groups and the interference effect of that attitude change on the recall of initial attitudes. It is interesting to note that 87% of all the Ss who incorrectly recalled their initial attitudes incorrectly recalled them as being identical with their postmanipulation (then current) attitudinal positions.

The Interpersonal Simulation Scores

It was predicted that subject-observers in the low justification simulation conditions would tend more often to view the attitudes of the Ss in the forced compliance procedures as more favorable to the position argued in the essays than would subject-observers in the high
justification simulation conditions. The expected difference, while nonsignificant, was in the predicted direction. That is, subject-observers who were asked to estimate the actual opinions of an S willing to write a counterattitudinal essay tended to estimate that S's attitude as being closer to the attitude expressed in the essay if that S had written the essay under low justification conditions rather than under high justification conditions. Although the predicted difference in estimated attitude scores between the high and the low justification simulation groups was nonsignificant, it should be noted that the actual differences between the attitude change scores of the Ss in the forced compliance conditions—which were being simulated—were also nonsignificant and were in the same direction as the estimate-score differences.

Impressions from the Postexperimental Questionnaire

Perceived Attitude Change

In the interest of clear illustration, the Ss were divided into two groups: (1) those who changed in attitudinal position, and (2) those who did not change their attitudes. Table 6 presents a summary of the Ss' answers to question 5 on the postexperimental questionnaire ("Were you aware of any attitude change on your part . . .?"). Of

---

1. The postexperimental questionnaire appears in Appendix E.
Table 6. Answers to the question: "Were you aware of any attitude change on your part? If so, in which direction?"

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>No</th>
<th>No Answer</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subjects who changed in attitudinal position</td>
<td>7</td>
<td>25</td>
<td>7</td>
<td>39</td>
</tr>
<tr>
<td>Subjects who did not change in attitudinal position</td>
<td>2</td>
<td>49</td>
<td>10</td>
<td>61</td>
</tr>
<tr>
<td>Totals</td>
<td>9</td>
<td>74</td>
<td>17</td>
<td>100</td>
</tr>
</tbody>
</table>

the 39 Ss who changed in attitude, 7 failed to answer that question. Of the remaining 32 Ss who changed in attitudinal position, 80% reported no awareness of any attitude change on their parts.

Perceived Experimental Bias

Table 7 presents a summary of Ss' answers to questions 6, 7, and 8 of the postexperimental questionnaire; which questions all dealt with perceived experimenter bias or effect. Of the 92 Ss who answered those questions, one answered "yes" to question 6 and stated, "I felt I was biased by signing my name . . . ." Two Ss answered "yes" to question 7; each of them questioned whether they had actually been free to refuse to participate in the
Table 7. Answers to questions 6, 7, and 8 on the post-experimental questionnaire, regarding whether Ss perceived experimenter bias or interference.

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>No</th>
<th>No Answer</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subjects who changed</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>in attitudinal position</td>
<td>2</td>
<td>34</td>
<td>3</td>
<td>39</td>
</tr>
<tr>
<td>Subjects who did not</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>change in attitudinal</td>
<td>1</td>
<td>55</td>
<td>5</td>
<td>61</td>
</tr>
<tr>
<td>position</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Totals</td>
<td>3</td>
<td>89</td>
<td>8</td>
<td>100</td>
</tr>
</tbody>
</table>

experiment. All other Ss answered "no" to all three of those questions.

Perceived Purpose of Experiment

Table 8 presents a summary of Ss' responses to question 1 of the postexperimental questionnaire ("What do you think was the purpose of this experiment?"). Six Ss either failed to answer the question or made responses that were illegible. Of the remaining 94 Ss, 57 stated that they thought the experiment had to do with attitude change and 37 stated that they thought the experiment was a poll whose aim was to assess student feelings regarding violence.
Table 8. Subjects' stated beliefs regarding the purpose of the present experiment.

<table>
<thead>
<tr>
<th>Purpose Cited</th>
<th>Per Cent of Ss Responding</th>
</tr>
</thead>
<tbody>
<tr>
<td>Attitude change or attitude consistency</td>
<td>57</td>
</tr>
<tr>
<td>Measure or poll of student opinions regarding violence</td>
<td>37</td>
</tr>
<tr>
<td>Other, or no answer</td>
<td>6</td>
</tr>
</tbody>
</table>
CHAPTER VI

DISCUSSION AND CONCLUSIONS

Attitude Change

The present findings are seen as providing clear evidence for the contention that the induction of counter-attitudinal behavior will lead to a change in the referent attitude. A strong, while nonsignificant, tendency of the low justification conditions—within the forced compliance format—to effect greater attitude change (as indicated by higher change scores) than was seen in the high justification conditions seems to provide further support for the inverse incentive position held by the advocates of both the dissonance and the self-persuasion positions.

In view of the difficulty in interpretation of the findings of many of the previous experiments in counter-attitudinal behavior—due to their various methodological anomalies, discussed above—the present findings are seen as providing comparatively clearer evidence regarding the effect of counterattitudinal behavior on subsequent attitude change. That claim is made for several reasons. First, unlike many of the previous forced compliance studies, the present experiment included both the assessment and the equating of premanipulation attitudes of all Ss in each
Even some of the later forced compliance studies (e.g., Janis and Gilmore, 1965; Linder and Jones, 1969) failed to include the assessment of initial attitudes, leaving open the possibility that the "counterattitudinal" behavior was not, in fact, counter to the relevant attitudes of the Ss involved. Since, in the present study, all Ss were matched for their initial attitude scores, the mean initial attitude scores of the various groups were, of course, identical—not "closely matched"—and the variances of the various groups were identical, as well.

Secondly, the manner of data analysis employed in the present study, especially with regard to the application of the Tukey technique in specific comparisons, compares the present results favorably with those of previous experiments in this content area. As noted above, some forced compliance researchers (e.g., Brehm, 1960; Brehm and Cohen, 1959) reported significant results after having run several specific comparisons employing "interaction t tests." Some had first tested for overall significance and some had not, in any case, as Chapanis and Chapanis (1964) put it:

Making a number of ordinary t tests on the same set of data, without a prior overall test of the null hypothesis, can be misleading. The principal difficulty is that in making such multiple comparisons the experimenter is allowing himself a number of opportunities to find an event (significance) which normally occurs

1. Refer to footnote, Appendix C.
infrequently. As a result, the usual t tables underestimate the true probabilities, that is, the probabilities obtained suggest a level of significance which is higher than warranted. Another way of saying it is that if, out of several subgroups, one finds one or two t's significant, he is, in effect, capitalizing on chance (p. 18).

Chapanis and Chapanis were referring, in part, to a statistical anomaly to which some statisticians (e.g., Winer, 1962) refer to as "alpha slippage." Alpha slippage, according to Winer (1962) and Myers (1966), is entirely avoided by employing the Tukey technique of specific comparisons.

Thirdly, in contrast to several of the previous studies in forced compliance (e.g., Brehm, 1960; Cohen et al., 1958; Davis and Jones, 1960), the present design did not allow for the de-selection of Ss who had failed to perform according to hypothesis. That is, the de-selection of Ss in the present study, for the purpose of creating equal cell n's, was performed with several precautions: (1) the de-selection was conducted before inspection of the data; (2) the de-selection was random; and (3) the de-selection was conducted by a person other than the experimenter; one who had knowledge neither of the nature of the experiment nor of the scores of the Ss. Thus, one of the most serious, and common, pitfalls marked by Chapanis and Chapanis (1964) in the literature on forced compliance was avoided.
In summary, even though the attitude change results of the present experiment seem to replicate those of most of the previous forced compliance studies, the avoidance, in the present experiment, of certain methodological anomalies is seen as providing results that are somewhat less equivocal.

**Attitude Recall Errors**

The present finding that Ss in the forced compliance conditions recalled their initial attitudes significantly less accurately than did Ss in either the interpersonal simulation conditions or in the control conditions is seen as evidence that Ss who engage in counterattitudinal behavior, and consequently change their attitudinal positions, fail more typically to recall accurately their initial attitudes than do Ss who have not engaged in such counterattitudinal behavior.

Further support is seen in the present recall-error results for Bem and McConnell's (1970) contention that incoming data from the behavior of an S who engages in counterattitudinal behavior will "... 'update' his information on his attitude, replacing any prior information to the contrary [p. 24]." Bem's (1968) earlier argument that initial attitudes are not salient in the phenomenologies of Ss who have undergone intervening counterattitudinal behavior seems, by the same token, to be supported by the
present finding that approximately 80% of the Ss who changed in attitudinal position were unaware of any attitude change on their parts. Furthermore, the present finding that 87% of the Ss who incorrectly recalled their initial attitudes recalled them as being identical with their postmanipulation (then current) attitudes provides substance for Bem's (1967b) claim that perceived attitude change "... is a psychological reality only for an experimenter who can compare S's rating with those of a control group [pp. 536-537]."

It could be speculated that those Ss were merely attempting to appear to be consistent in their attitudinal positions. However, in the light of the present finding that the accuracy-consistency manipulations had no significant differential effect on the accuracy of recall of initial attitudes, assigning a consistency motive to those Ss would seem a purely speculative gesture. As Bem (1970) put it, even if some Ss did feel a demand for consistency between their pre- and postmanipulation attitudes, "One could even argue that ... they could not implement their desire to look consistent because they couldn't remember their original attitudes ... ."

The Interpersonal Simulation

While the scale scores of the Ss in the forced compliance conditions, estimated by the subject-observers in the simulation conditions were not significantly
different as a function of the high-low justification manipulations, they did vary in the predicted direction. That is, subject-observers who were asked to estimate the actual opinions of Ss in the low justification condition of the forced compliance format, more often estimated those opinions to be congruent with the opinions expressed in the counterattitudinal essays than did subject-observers who were asked to estimate the opinions of Ss in the high justification condition of the forced compliance format. That the simulation estimate differences were nonsignificant may have two contrary implications.

First, it might be assumed that the simulations failed to provide further support for Bem's (e.g., 1965) claim that outside observers can estimate another's opinions by inferring them from that person's behavior. As was the case in the "countersimulations" conducted by Bem's critics (e.g., Linder and Jones, 1969; Piliavin et al., 1969), the present simulation did not succeed in showing a significant difference between the estimated opinions of Ss in the low justification conditions and those in the high justification conditions. Even so, a failure to successfully replicate Bem's simulations with other simulations may simply imply that the latter efforts employed faulty "input" statements. As Abelson (cited in Bem and McConnell, 1970) noted, "Ironically, what [Bem's] detractors should now really be doing if they must still simulate is to replicate [his]
outcome with clearly bad descriptions to the observer, rather than to reverse [his] outcome with purportedly good descriptions [p. 30]." In other words, a successful simulation replication with obviously faulty input statements would seem to damage Bem's simulation formats more than would failures to replicate with ostensibly correct inputs.

A second possible interpretation of the finding that the simulation-estimated differences in attitude change between the high and low justification levels were non-significant is this: The simulation was successful in that the subject-observers' estimates of the attitudinal positions of the Ss in the forced compliance format were in the same direction as the actual opinion statements of the forced compliance Ss. That is, low justification Ss, in the forced compliance format, held postmanipulation attitudes more congruent with the position argued in the essays than those of Ss in the high justification condition, and the subject-observers in the simulation conditions correctly estimated the direction of that difference. Both the actual attitude differences between Ss in the low and the high justification conditions of the forced compliance format and the differences between the estimates of the subject-observers of those attitudes, were in the predicted direction, and both differences were nonsignificant.
Implications Regarding the Bem-Dissonance Controversy

While Bem's interpersonal simulation technique seems to remain unchallenged by the results of the present simulation attempt, the results of the present attitude-recall experiment seem actually to support Bem in his arguments with his detractors (e.g., Bem and McConnell, 1970; Linder and Jones, 1969) regarding the salience of premanipulation attitudes of Ss in their postmanipulation phenomenologies. Even that support, however, ought not to be construed as a clear or direct confrontation between Bem's self-persuasion model and that held by the dissonance advocates: "Dissonance theory . . . is not embarrassed by the finding that subjects fail to recall their initial attitudes; in fact it could be argued that forgetting an earlier conflicting attitude is itself a mode of dissonance reduction [Bem and McConnell, 1970, p. 30]."

The present findings reconfirming the efficacy of counterattitudinal behavior in the modification of related attitudinal statements are also not embarrassing to either camp; both the self-persuasion advocates and the dissonance advocates would have--and have--predicted an identical outcome. In summary, it seems still to be the case that a choice between the dissonance and the self-persuasion explanation of the changes in attitudinal statements
following counterattitudinal behavior remains one of "... loyalty or aesthetics [Bem and McConnell, 1970, p. 30]."

**Ecological Validity: Further Implications**

The efficacy of counterattitudinal behavior in the subsequent change of a referent attitude, in a predictable direction is a well-documented phenomenon that has potential implications beyond the scope of a controversy between the advocates of dissonance theory and those of self-persuasion.

"Ecological validity" (vide Brunswik, 1956; Friedman, 1967) refers to the generalizability of experimental findings to non-experimental situations. An example of the ecological validity of the present findings clearly presents itself in the area of psychotherapy. In many psychotherapeutic endeavors, for example, one of the goals of therapy is the modification of the client's attitudes; either toward himself (his self-perception or self-image), toward others, or toward some other portion of his psychological environment.

The results of the present experiment, and those of other studies in counterattitudinal behavior suggest rather strongly that an excellent method of attitude change would be to induce a client to engage in behavior counter to his (presumably) maladaptive attitudes. The literature on efforts in systematic desensitization of phobic problems (e.g., Lazarus and Rachman, 1960; Rosenthal, 1968; Wolpe,
1958) and the work on therapeutic role-playing (e.g., Lazarus, 1966; Rosenthal and Myer, in press; Wolpe and Lazarus, 1966) both provide convincing evidence to the effect that the induction of behavior counter to the client's attitudes (fears, expectancies, self-percepts, etc.) predictably leads to a modification of those attitudes.

Furthermore, it seems likely that, in the more traditional forms of insight therapy which are aimed primarily at revision of a client's attitudes regarding himself (e.g., Rogerian, Ego-analytic, and Freudian psychodynamic therapies) the use of counterattitudinal behavior in effecting changes in the client's attitudes might have wide applicability. For example, inducing a client to attempt novel, and more adaptive, behaviors in a situation previously associated with failure or pain, would seem likely to result in a modification of the client's attitude toward himself and toward the situation itself.

More systematic research seems warranted into attempts to modify attitudes outside the traditional experimental settings in order to establish the ecological validity of the attitude change potential of counterattitudinal behavior.
APPENDIX A

VERBAL INSTRUCTIONS

A. Instructions read, in the first session, for the pre-
manipulation questionnaire:

In an attempt to assess current student attitudes
regarding various social issues of importance, we are
requesting your assistance for a few minutes. On the sheets
being passed out to you now, please include your name and
matriculation number, and indicate your answers to the five
questions by marking on the horizontal lines as indicated in
the written instructions. While your participation is
entirely voluntary, your assistance is considered quite
important and will be greatly appreciated. Please fill out
the questionnaires and pass them to the aisles when you have
finished. Thank you for your assistance.

B. Instructions read, in the second session, for the
treatments and postmanipulation questionnaires:

We are requesting a few minutes of your time once
again for our research project. While participation is
voluntary, your assistance is very important and will be
greatly appreciated.

Please put your names, and this section number, on
each sheet (in case they're separated). The names will not
be seen by anyone other than those of us directly involved
in this research, and no one but us will see any identified
data.

The results of the experiment will be available in
summarized form in a few weeks. For any of you who might be
interested, ask your T.A. for the results.

Please respond to each question briefly, and in
order, but as completely as you can.

Again, thanks for your assistance.
C. Instructions read after the postexperimental questionnaires had been collected:

Please don't discuss this experiment with any other students in Psychology for a week. Thank you.
APPENDIX B

WRITTEN INSTRUCTIONS

A. Instructions for forced compliance Ss; low justification:

This week, we are collecting arguments for and against the various positions expressed in the questionnaires given last week. Each participant is being asked to write a short essay on one of the issues. In the space below, please write one paragraph including at least three reasonable points for the contention that violence in student protests might be justified under some conditions.

B. Instructions for forced compliance Ss; high justification:

In order to develop balanced positions on the various issues presented on our questionnaire, and to discover novel and reasonable arguments for both sides of each issue, we are asking each participant to write a short essay on one of the issues. In the interest of fair and open dissent, as well as intellectual stimulation, we are asking some participants to write essays for positions that they may be opposed to. In the space below, please write one paragraph including at least three reasonable points for the contention that violence in student protests might be justified under some conditions.

C. Instructions for interpersonal simulation Ss; low justification:

This week, some participants have been asked to write brief essays justifying violence in student protests under some conditions. Most students were initially opposed to the position. The participants who were asked, agreed to do so and wrote the essays. The scale shown below was used to assess the students' opinions on the issue. From the above description, please estimate as well as you can the actual opinion of a student who was willing to write the
essay. Indicate your estimate by drawing a line through the appropriate point on the scale below.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / / Completely very quite somewhat little not at all

Justified

D. Instructions for interpersonal simulation Ss; high justification:

This week, some participants have been asked to write brief essays justifying violence in student protests under some conditions. Most students were initially opposed to the position. The participants were given several reasons (e.g., the values of open mindedness, fairness, intellectual stimulation, and free and open dissent) for writing the essays. The participants who were asked, agreed to do so and wrote the essays. The scale shown below was used to assess students' opinions on the issue. From the above description, please estimate as well as you can the actual opinion of a student who was willing to write such an essay. Indicate your estimate by drawing a line through the appropriate point on the scale below.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / / Completely very quite somewhat little not at all

Justified
APPENDIX C

PREMANIPULATION QUESTIONNAIRE

Name__________________________ Age_________ Sex______
Quiz Section Number_________ Matric Number________________

Please indicate your response to each of the following questions by marking a vertical line on the horizontal scales, above the appropriate answers.

1. To what extent do you feel American involvement in the war in Indochina is justified in terms of American self-protection?

/ / / / / / /
Completely very quite somewhat little not at all Justified

2. How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / / /
Completely very quite somewhat little not at all Justified

3. To what extent do you feel that a direct U.S. military intervention in the Middle East would be justified at the present time?

/ / / / / / /
Completely very quite somewhat little not at all Justified

4. How justified do you consider the expenditures on space exploration to be, compared to other national priorities?

/ / / / / / /
Completely very quite somewhat little not at all Justified
5. To what extent do you consider the current emphasis on ecological problems to be justified, compared to other social issues?

<table>
<thead>
<tr>
<th>Completely</th>
<th>very</th>
<th>quite</th>
<th>somewhat</th>
<th>little</th>
<th>not at all</th>
</tr>
</thead>
</table>

Justified

Note.—All Ss marked answer 6 in item 2; that is, all Ss replied to the question, "How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protests?" with the answer, "not at all."
APPENDIX D

POSTMANIPULATION QUESTIONNAIRE

A. Consistency demand:

Please indicate your current attitude by marking above the appropriate answer on the scale below.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / 
Completely very quite somewhat little not at all

Justified

We are interested in the degree to which students are consistent in their attitudes. Please indicate your attitude on the same issue as you marked it last week in the Psychology 1-B lecture session.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / 
Completely very quite somewhat little not at all

Justified

B. Accuracy demand:

Please indicate your current attitude by marking above the appropriate answer on the scale below.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

/ / / / / 
Completely very quite somewhat little not at all

Justified

75
Please attempt to accurately recall your attitude on the same issue as you marked it last week in the Psychology 1-B lecture session.

How justified do you feel the tactics of violence in student protests have been, compared to the tactics of peaceful protest?

<table>
<thead>
<tr>
<th>Completely</th>
<th>very</th>
<th>quite</th>
<th>somewhat</th>
<th>little</th>
<th>not at all</th>
</tr>
</thead>
</table>

Justified
APPENDIX E

POSTEXPERIMENTAL QUESTIONNAIRE

Please answer the following questions very briefly.

1. What do you think was the purpose of this experiment?

2. What hypotheses do you think were held by the experimenters?

3. Do you feel they were supported in those hypotheses?

4. Did you recall your initial (last week's) attitude accurately?

5. Were you aware of any attitude change on your part? If so, in which direction?

6. Do you feel that you were biased by any of the things we did or requested? If so, please explain.

7. Did you receive any interference by any of us in your free choice in your participation in the experiment? If so, please explain.

8. Do you feel that you were misled in any way by the experimenters? If so, please explain.

Thank you very much for your aid.
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