

The Effect on Liking of Underrating or Overrating Another

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People undoubtedly feel others deserve a correct and fair evaluation. This experiment tested the hypothesis that if an individual discovered he had accorded another less respect than the other deserved, he would attempt to make up for this injustice by a subsequent, temporary overestimation of the other. If he discovered he had accorded the other more respect than the other deserved, he would subsequently underestimate the other. It was expected that this "over-compensation" would occur even if the individual's unjust evaluation had been entirely private. This hypothesis was supported.

In the experimental situation, both commitment to the initial unjust evaluation and responsibility for the unjust evaluation were held at a very low level. We felt these restrictions might be necessary to demonstrate the over-compensation response we were proposing.

It was also proposed that as subjects became increasingly committed to their initial unjust evaluation, they would attempt to compensate for an initial injustice less and less often, and would increasingly attempt to justify their initial misjudgment. This expectation was not confirmed. Moderately committed subjects demonstrated as much, or more, over-compensation than uncommitted subjects. It was suggested that perhaps even the most committed groups were not in fact very committed to their initial unjust evaluations, which were anonymous.

Festinger (1957) proposed that if a person's cognitions (his beliefs about himself, about his behavior, or about the environment) were inconsistent with each other, he would be motivated to reduce this inconsistency. Researchers interested in interpersonal relations quickly saw and tested the implications of Festinger's theory for interpersonal relations. On the basis of dissonance theory, they hypothesized that if an experimenter persuaded a subject to behave in a harsh way, or in an exceptionally generous way toward someone, subjects should tend to change their attitudes so that they would be consistent with their be-

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havior. If a subject harmed another, the subject would be expected to justify his actions and thus come to dislike the person he had harmed. If the subject did a favor for another, the subject would be expected to convince himself that the other deserved his kindness and thus to increase his liking for the other. In addition, researchers also hypothesized that the more choice the subject felt he had about whether to treat someone harshly or generously, the more a subject would tend to like someone he had helped and to dislike someone he had harmed.

The prediction that if one chooses to hurt someone else, he will subsequently denigrate the person he has harmed has been supported experimentally several times; e.g., by Davis and Jones (1960) and Glass (1964). In addition, Berkowitz (1962) cites studies (designed to test other hypotheses) which demonstrate that men who counteraggress against a partner come to dislike him more than do men who are not given the opportunity to counteraggress (Berkowitz, Green, and Macaulay, 1962); and that students report increased feelings of hostility toward a stimulus person after verbally aggressing against him (Kahn, 1960).

The prediction that if one chooses to do a kindness for someone else, he will subsequently increase his liking for the other he has benefited, has *not* been successfully demonstrated. However, experimental tests of this hypothesis are in progress (Jecker,² and Hastorf and Regan³).

The consistency of the above findings could very easily lead us to conclude that individuals always denigrate those they hurt and glorify those they benefit.

The only reluctance we had in accepting such a generalization was that we could think of a few situations in which people behaved in opposite ways; glorifying those they had harmed and denigrating those they had benefited. For example, we could think of an instance when a teacher had considered punishing a young student only to discover that the student was undeserving of punishment. The teacher felt so guilty about her error that she went out of her way to make things up to the student, mentally over-emphasizing his goodness and importance. Berkowitz (1962) speaks about reactions of this type when he discusses guilt arousal following aggression.⁴ We could also think of an instance when

²Personal communications from Dr. Jon Jecker, University of Texas.

³Preliminary information available from Dr. Albert Hastorf, Stanford University.

⁴Berkowitz, however, is not very specific about when such reactions will occur. Although he notes that guilt arousal sometimes causes a "reaction-formation" in which the aggressor leans over backwards to avoid any semblance of future aggression, he also stresses that the same feeling of guilt may also enhance the aggressor's hatred for the victim. He suggests that which reaction occurs depends on the strength of the individual's moral standards condemning aggression.

a young woman had treated a young man affectionately and generously only to discover that he had been "putting her on," pretending to be more interested in her welfare than he really was. In such an instance, we felt the girl would become so angry and indignant that she would exaggerate the young man's shortcomings. Certainly, we thought she would like him less than if she perceived his manipulative intent from the start.

In these above examples, we do *not* mean simply that a person who realizes he has made a mistake will then adjust his evaluation of the other so that the evaluation is as correct and realistic as possible. We are proposing much more than that: We are proposing that if a person discovers he has misjudged another, he will *overreact* in an effort to correct his mistake. The teacher will like the injured pupil more than an objective outside observer would, and more than she would had she known of his innocence from the start.

The individual will hate the false friend more than would an objective outside observer and more than the individual would had he known of his friend's manipulative intent from the start.

We are labeling this proposed process "over-compensation."

What would account for such "over-compensation" reactions as we have just described? It is clear what a person gains by reducing dissonance—he gains consistency. But what does a person gain by temporarily overrating someone he previously harmed and underrating someone he unjustly benefited? It is difficult to say, and yet we felt sure such over-compensation does occur.

Perhaps, we finally decided, individuals feel that they should accord another exactly as much liking and respect as he "deserves." If a person discovers he has markedly underrated another, he might feel guilt at his injustice. If a person discovers he has markedly overrated another, he might feel indignation at the unjust benefit the other has received. In either case, the person's first thought might be that he wants to make things up to the erroneously evaluated individual. One way of compensating another is to do something kind (or cruel) to him. A second way to compensate him is to like him (if we wish to reward him) or dislike him (if we wish to punish him). If one discovers he has rated another too harshly, he can compensate the other by temporarily according him more liking and respect than he normally would. If one discovers he has rated another too generously, he can pay him back by temporarily according him less liking and respect than he normally would.

We would like to hypothesize that a person will temporarily underrate someone he discovers he has previously judged too generously and will temporarily overrate someone he discovers he has previously judged too critically. In testing this hypothesis, we knew we had to impose one

restriction on our experimental design: subjects could not be committed too strongly to their initial unjust evaluations. From the dissonance research cited earlier, we know that if an individual is made to feel personally responsible for an unjust action, if there is no chance of his making up for the unjust action, and if there is no possibility of his explaining to the unjustly treated other why he did what he did, the subject will engage in behavior quite opposed to that which we are proposing. Thus, in this first experiment, we were careful to keep commitment to an initial unjust evaluation at a low level.

To test the above hypothesis, it was necessary to run two experimental groups and two control groups.

In the first experimental group, subjects were provoked into evaluating a stimulus person very *harshly*. (We will call this group E_{ith} . *ith* indicates *initially too harsh*.) Subsequent information made them realize that their initial judgment was unfair; that the stimulus person deserved a much more positive evaluation. We predicted that E_{ith} subjects would attempt to compensate for their initial injustice, by subsequently increasing their liking for the other.

In the control group for E_{ith} (which we will call C_{ith}) subjects received exactly the same information about the stimulus person that experimental subjects received. However, they were not provoked into initially judging the stimulus person too harshly; their initial judgment was fair and accurate. We predicted that control subjects (who had not been initially too harsh) would have no need to compensate for their initial injustice, and thus no need to increase their liking for the stimulus person. Therefore, on a second rating, we would expect experimental subjects (E_{ith}) to like the stimulus person *more* than do control subjects (C_{ith}).

In the second experimental group, subjects were provoked into evaluating a person very *generously*. (We will call this group E_{itg} . *itg* indicates *initially too generous*.) Subsequent information made them realize that their initial judgment was unfair; that the stimulus person deserved a much more negative evaluation. We predicted that E_{itg} subjects would attempt to compensate for their initial injustice by subsequently decreasing their liking for the other. As before, in the control group for E_{itg} (which we will call C_{itg}), subjects received exactly the same information about the stimulus person that experimental subjects received. However, they were not led into initially judging the stimulus person too generously; their initial judgment was fair and accurate. We predicted that control subjects (who had not been initially too generous) would have no need to compensate for their initial injustice and thus no need to decrease their liking for the stimulus person. Therefore, on a second

rating, we would expect experimental subjects (E_{itz}) to like the stimulus person *less* than do control subjects (C_{itz}).

In summary then, our prediction is that on a second rating, E_{ith} subjects will like the stimulus person *more* than do C_{ith} subjects, and that E_{itz} subjects will like the stimulus person *less* than do C_{itz} subjects.

PROCEDURE

Subjects were 312 students from the junior and senior classes of Dickinson Central High School in North Dakota. They were run in groups of approximately 30 in September, 1963, and September, 1964.

We needed to provide a rationale to induce the students to honestly evaluate a stimulus person. The following explanation was chosen: The experimenter² explained that she was interested in studying the kinds of first impressions certain people make on others. Then the following fictitious study was described to students: We supposedly had gotten the names of several boys who were going to transfer from one Minneapolis school to another. We interviewed students at the old school to find out how they felt about this boy, then we studied how students at the new school formed first impressions of him, first by hearing about him from the few "kids" who knew him and then by meeting him themselves. We claimed to have conducted this study because we were interested both in how people formed impressions and in how much they had to know about a person before they could form an accurate impression. Since we didn't want our findings to be limited just to students from Minneapolis, to students who perhaps already had a fairly good idea of what students at the boy's old school were like, we wanted the Dickinson students to give us their honest impressions of the boy.

Assignment of Subjects to Experimental Conditions

Subjects were told we would give them individual booklets which contained several interviews with Jim's fellow students. These booklets also would ask questions about their own reactions to Jim.

Although all booklets appeared to be identical, they were not. Which booklet the subject received determined whether he was assigned to experimental condition E_{ith} or E_{itz} or control condition C_{ith} or C_{itz} .

E_{ith} and C_{ith} Groups. Interview 1: In the booklets E_{ith} subjects received, the first interview they read was designed to provoke them into feeling dislike for Jim. In this interview, a fellow student of Jim's named Jane described Jim as "a cruel, sadistic, hoodlum." She then explained, in detail, the basis of her accusation. A gang of boys at school thought it was funny to tease and kill cats. They used to threaten to torture cats just to make little kids scream. Jane realized that Jim was a member of this show-off group when she heard that once, on the playground, in front of the first graders, he had killed a cat. In lurid detail she discussed how the tortured cat must have felt and how first graders felt about witnessing such cruelty.

Before booklets were passed out, the experimenter had requested subjects to think about their impressions of Jim after reading each page. It was hoped that after reading Interview 1, E_{ith} subjects would form a harsh opinion of Jim.

²Darcy Abrahams and Zita Brown served as experimenters.

Page *two* of the booklet was designed to make E_{1th} subjects realize that their initial evaluation of Jim had been too harsh. Page two contained a warning that some of the information Jane had provided in Interview 1 was false. This lengthy warning explained that although Jim had killed the cat on the playground, he had done so in the belief that it had rabies. How he came to the conclusion that the cat was rabid was explained in detail. Jane's negative evaluation of Jim, then, was mostly due to the fact that she had misinterpreted his reason for killing the cat.

At this point, E_{1th} subjects should be feeling they evaluated Jim *too harshly*. We predicted that this feeling would lead them to a subsequent overevaluation of Jim. We measured E_{1th} subjects' evaluations in the next section of the booklets. In this section, subjects were asked to express their honest reactions to Jim. The questionnaire stressed that we were interested in how they really felt, and not how they thought they were supposed to feel. Questions were included to measure the following things:

1. How much the subjects liked Jim personally. Questions asked how much they liked Jim, how mean they thought he was, and whether or not they thought he was sensitive to the feelings of others.

2. How subjects perceived the cat killing: e.g., did they think the cat had rabies? Did they think it was a good idea to kill the cat in front of the children? Did they think that animals are less sensitive to pain than humans?

3. How much information subjects believe a person should have before he commits himself to liking or disliking someone.

After completing the questionnaire, subjects then read some additional interviews about Jim. Interviews two through four indicated that Jim was generally a fairly good person.

Finally, subjects answered a final question as to how much they liked Jim.

Table 1 contains a résumé of the order in which experimental subjects received information and assessment questions.

Control subjects (C_{1th}) received exactly the same information as did E_{1th} subjects, with one exception. We wanted C_{1th} subjects' initial evaluation of Jim to be fair and accurate. Thus, control subjects were given the warning not to take Interview 1 at face value *before* reading Interview 1.

Actually, two C_{1th} groups were run. In the regular Control group, subjects were simply told once, before reading Interview 1, that they should not take the interview at face value; that Jim had in fact killed the cat because he believed it was rabid.

The regular control group had one flaw as a control group, however. Control subjects read the warning not to believe Interview 1 immediately *before* they read Interview 1. Then they expressed their opinions about Jim. Experimental subjects read the warning not to believe Interview 1 a little later (after reading Interview 1). Then they expressed their opinions about Jim. It was, of course, crucial that control and experimental subjects should receive the warning and the interview in a different order. However, it was not crucial, nor even desirable, that a different amount of time should elapse between reading the warning and filling out the evaluations. Unfortunately, order and time are necessarily confounded in our regular control group.

If we, in fact, discover that E_{1th} subjects express more liking for Jim than do C_{1th} subjects, it would be possible to argue that E_{1th} subjects simply expressed greater liking because they remembered the warning better. We didn't feel this explanation was plausible. Nonetheless, to eliminate the above possibility, we de-

ecided to run a second control group (Double Warning Control) in which the subjects were required to read the warning *twice*: once before reading Interview 1 and again after reading Interview 1. Unfortunately, this control group also had certain flaws; in fact, it seemed to be a less satisfactory control than the regular control. Asking subjects to read a message for a second time, only a minute after they just finished reading it, is somewhat peculiar. It is likely to make subjects feel that we are unusually concerned that they feel that Jim was a "good" boy. However, it does eliminate the confounding of time with order which exists in the regular control group. To eliminate alternative interpretations, we chose to run *both* the regular control group and the double control group.

Readers may review the order in which material was provided to control subjects by turning to Table 1.

TABLE 1
ORDER IN WHICH INFORMATION AND QUESTIONS WERE PRESENTED TO SUBJECTS

Condition	Types of information				Subjects' opinions assessed	Interviews 2-4	Final liking assessment
	Warning not to believe Interview 1	Misleading Interview 1	Warning not to believe Interview 1				
Experimental group		×	×	×	×	×	
Regular control group	×	×		×	×	×	
Double warning control group	×	×	×	×	×	×	

E₁₁₂ and C₁₁₂ Groups. In the booklets *E₁₁₂* subjects received, the first interview they read was designed to lead them into feeling great liking and admiration for Jim.

In Interview 1, a schoolmate of Jim's named Jane, described him as a "really great person." Jane then went on to explain the basis of her evaluation. A rabies scare had existed in their hometown. A kindergartener had been bitten by a rabid cat on her way to school, and students had been warned to keep a lookout for the cat and to warn the principal immediately if it was spotted in the school area. Jane said that that same afternoon, Jim had seen a cat near the little children and had tried to get it to follow him. When the cat refused to follow him, and had continued threatening the children, Jim took off his shirt and threw it around the cat. Jane didn't know whether or not he had killed the cat but she interpreted Jim's action in glowing and heroic terms.

It will be recalled that before booklets were passed out, subjects were asked to think about their impressions of Jim after reading each page. It was hoped that after reading Interview 1, *E₁₁₂* subjects would be evaluating Jim very generously.

Page two of the booklet was designed to make *E₁₁₂* subjects realize that their initial evaluation of Jim had been too generous. Page two contained a warning that some of the information Jane had provided in Interview 1 was false. This warning

explained that Jim had *not* caught and killed the cat in the belief that it was rabid and a danger to the little children. He had confessed his true motivation to the school principal: He had bet some boyfriends \$5 that he could catch the cat in less than five minutes. However, he found it was more difficult to catch than he had expected; it continued to slip out of his grasp. Angry and frustrated, he took out his pocket knife and stabbed the cat. Jane's positive evaluation, then, was seen to be a function of her misinterpretation of Jim's motives in killing the cat.

At this point, subjects in the E_{1tz} condition should be feeling they evaluated Jim *too generously*. We predicted that this feeling would lead them to a subsequent underevaluation of Jim. We measured E_{1tz} subjects' evaluations in the next section of the booklets. (The questions included in this section are described in detail above.) After subjects had answered the 15 questions concerning their reactions to Jim and their perception of the cat killing, they were asked to read some additional interviews concerning Jim. Interviews two through four pointed out some slightly negative aspects of Jim's personality. Finally, subjects answered a final question about their liking for Jim.

Control subjects (C_{1tz}) received exactly the same information as did E_{1tz} subjects, with one exception. We wanted C_{1tz} subjects' initial evaluations of Jim to be fair and accurate. For this reason, control subjects were given the warning not to take Interview 1 at face value *before* Interview 1. For reasons discussed earlier, two control groups (a regular control group and a Double Warning Control group) were run. Subjects in the regular control group read the explanation that Jim really killed the cat in an attempt to win a bet just *once*, immediately before reading Interview 1. Subjects in the Double Warning Control condition read the warning not to take Interview 1 at face value *twice*—once before and again after reading Interview 1.

Table 1 provides a review of the order in which materials were provided to experimental and control subjects.

Four conditions, other than those already described, were also run. These conditions were run in order to answer some specific questions to be discussed later in the paper.

RESULTS

Manipulation Check

We have assumed that immediately after reading Interview 1, *experimental* subjects shared Jane's erroneous evaluation of Jim. Since control subjects were warned not to take Interview 1 seriously, we would not expect them to share Jane's erroneous evaluation.

Two extra groups were run to check the above assumption. The first group was comparable to E_{1tz} . However, immediately after reading Interview 1, subjects were asked to fill out the 15-item assessment questionnaire. As expected, these subjects do express great dislike for Jim and his action. For example, in answer to the question, "How much do you like him?" Subjects in this group rate him 2.5 on the average. (In between "I dislike him very much" and "I dislike him fairly much.") The second group was comparable to the E_{1tz} group. However, immediately after reading Interview 1, subjects filled out the 15-item assessment

questionnaire. As expected, subjects in this condition do express moderate liking for Jim. Subjects' ratings average 5.7 ("I like him fairly much.") It is clear from comparing the answers of subjects in these two additional groups, that subjects will accept the misinformation Jane presents, and will base their initial evaluation of Jim on this information.

Test of Hypothesis

We predicted that subjects who realized they had been unjust in their initial evaluation of Jim would subsequently over-compensate for their injustice. Thus, E_{1th} subjects should like Jim more than do control subjects (C_{1th}) and E_{1tz} subjects should like Jim less than do control subjects (C_{1tz}). Statistically, the way in which Jim was misjudged should interact significantly with whether or not subject misjudged the boy himself, in determining liking. When we look at Table 2, we see that our hypothesis is clearly supported by the data.

TABLE 2
SUBJECTS' LIKING FOR STIMULUS PERSON

Liking for Jim	Boy initially evaluated too harshly			Boy initially evaluated too generously		
	Experi- mental group	Regular control	Double warning control	Experi- mental group	Regular control	Double warning control
Expected reaction	Strong liking	Moderate liking		Strong dislike	Moderate dislike	
How much do you like Jim? ^a	.62 ^c	-.07	.56	-.82	-.04	-.30
Jim is <i>not</i> mean.	1.09	.70	.96	.12	.79	.70
He is sensitive.	-.44	-.56	-.44	-.94	.10	-.21
How much do you like Jim? ^b	1.50	1.24	1.33	-.53	.04	-.01
Total Index of Liking	2.76	1.31	2.40	-2.18	.89	.17
N	(34)	(35)	(35)	(34)	(35)	(35)

^a Ss ratings on first assessment questions.

^b Ss ratings after reading Interviews 2-4.

^c The more positive the mean, the more Ss like Jim.

In the questionnaire which followed Interview 1 and the relevant warning that Jane's information was false, subjects were asked how much they liked Jim, whether or not they agreed Jim was a mean person, and whether or not they felt he was "sensitive to the feelings of others." After he read interviews two through four, the subject was asked, once again, how much he liked Jim. Answers to these four questions were summed together to form a single index of liking.

On the Liking Index subjects who were themselves initially unfairly harsh, express more liking for Jim than do subjects in both control conditions. Subjects who were themselves initially too generous, express more dislike for Jim than do subjects in both control conditions. This interaction is significant ($F = 8.76$, $p < .01$, with 1 and 202 *df*). When we examine the significance of the four questions making up the index individually we see that all are significant. Interaction F s for questions one through four are 5.35, 5.15, 4.88 and 4.18, respectively ($p < .05$ with 1 and 202 *df* in all cases).

Control Groups. We decided to run both a regular control group and a double warning control group for each experimental group because each control group alone had certain flaws. Let us then examine the regular and the double warning control groups to see if there are any significant differences between them on the crucial liking questions. Data show these control groups do not differ in the liking they produce for Jim. (The same index discussed earlier is, of course, our measure of liking.) The proper statistic to determine whether one type of control group is more similar to the experimental conditions than another is an interaction F . This F is insignificant. ($F = 1.66$, n.s., with 1 and 202 *df*).

When we look at the mean liking for Jim in the various control conditions, however, we notice that there is a slight, though insignificant, tendency for the double warning groups to be more similar to the experimental groups than are the regular control groups.

What do the data look like when we compare the experimental groups to each of the control groups, separately? When we eliminate the double control group from our analysis and look only at the difference between E_{1th} and regular C_{1th} and between E_{1tg} and regular C_{1tg} , we see that the Interaction F is significant ($F = 10.30$, $p < .01$ with 1 and 202 *df*). When we eliminate the regular control group from our analysis and look only at the difference between E_{1th} and double C_{1th} and between E_{1tg} and double C_{1tg} , we see that the Interaction F approaches significance. ($F = 3.72$, $p < .06$ with 1 and 202 *df*).

The fact that the double control group is more similar to the experimental group than is the regular control group seems reasonable. In the double control group, the subjects read the warning message, which contradicted Interview 1, *twice*. It may be that they are learning and accepting this message more or that they feel (since we repeat the same warning twice) that we want them to strongly reject Interview 1. If either possibility were true, the double control subjects would be expected to look more like experimental subjects than do single control subjects.

Since each control group taken alone supports our hypothesis at $p < .01$ or $p < .06$, we felt that we no longer had to worry about the slight

difference between the two control groups, and did not really have to come to a decision as to which was the more adequate control.

Supplementary Questions

The questionnaire included several questions not directly related to subjects' liking for Jim:

1. Several questions were included to assess whether or not subjects' perception of the *act* Jim performed was different in different conditions. Such questions as whether or not the subject thought the cat had rabies, whether *killing* the cat was a good thing, whether the cat should have been killed *in front of the children*, and whether or not animals were as sensitive to pain as humans were, were asked. When we examine the interaction between conditions to see if experimental groups differ from control groups in their perception of the same act, we see that they do not. The interaction *F*s for the above questions are .03, 3.99, .88, and .46, respectively. From looking at these questions then, it seems that there is little evidence that the over-compensation reaction leads subjects to alter their perception and evaluation of the *act*, even though it does lead them to have a different reaction to Jim.

ADDITIONAL QUESTIONS INVESTIGATED

Permanence of the Over-Compensation Effect

A question that will undoubtedly occur to the reader is "how much over-compensating does a person have to do, in order to make up for an initial misjudgment?" Does a person only have to over-compensate once, or does the feeling that one owes the unjustly treated person something persist? In order to get some minimal information concerning this question, we asked subjects on two occasions about their liking for Jim. After discovering they had initially misjudged Jim, subjects did demonstrate over-compensation for this misjudgment on the assessment questionnaire. Subjects then read Interviews two through four and were asked, on the basis of this new information, to express a final estimate of their liking for Jim. In spite of the fact that they had already "over-compensated" for their initial unjust thoughts, subjects continued to over-compensate on this second occasion. So subjects' tendency to over-compensate was at least consistent enough to last through two evaluations. We have no other information on how long this tendency persists.

Does the Subject's Tendency to Over-Compensate for an Initial Injustice Generalize to Others Besides the Misjudged Victim?

It could be argued that both C_{11a} and E_{11a} subjects perceived that Jim had been evaluated too harshly by Jane, and thus, in some sense, they had received a lesson that it is not always right to accept, at face value,

the negative things people say about others. If such a lesson was provided, its impact on experimental subjects should have been especially strong, for experimental subjects also received a demonstration that *they* were capable of underrating others.

In the same way, C_{112} and E_{112} subjects also may have learned a lesson: that it is not always right to take the positive things people say about others at face value. The demonstration to experimental subjects, who had been shown *they* were susceptible to misjudging others, may have been especially strong.

It is possible, on the basis of the above reasoning to argue that what we have been calling an "over-compensation" tendency—(the tendency of experimental subjects to make up for an initially unjust rating of another), is in fact something much simpler. Perhaps experimental subjects have simply become careful not to rate *anyone* too harshly (or too generously) since they have seen how easy it is for themselves to be misled by too harsh (or too generous) information.

To check on such an alternative explanation, we included three test questions in the questionnaire provided to subjects.

1. How much do you like Jane?

2. How much do you agree or disagree with the statement: "One should never *really dislike* someone unless he has a tremendous amount of information about that person?"

3. How much do you agree or disagree with: "One is foolish to *really like* someone before he has a tremendous amount of information about that person?"

If instead of specifically trying to over-compensate for their misjudgment of Jim, experimental subjects are just *generally* being especially cautious not to judge others too harshly (in the case of E_{112} subjects) or too generously (in the case of E_{112} subjects), we would expect experimental conditions to interact exactly in the same way when subjects are evaluating Jane, whom they did not initially misjudge, as when they are rating Jim, whom they did misjudge. When we look at the data, we see that experimental subjects are obviously *not* demonstrating the same "over-compensation" response in rating Jane, that they demonstrated in rating Jim.⁶ (Interaction $F = .18$, with 1 and 202 *df*).

Similarly, on the two questions, "One is foolish to *really like* someone before he has a tremendous amount of information about that person" and "One should never *really dislike* someone unless he has a tremendous

⁶It should be noted that the average liking for Jane when she says untrue but good things about Jim (5.24) is higher than the rating given to Jim when he actually did the good thing she describes (4.87, all three conditions averaged). In addition, the dislike of Jane when she says untrue but bad things about Jim (3.91) is greater than the ratings given to Jim when he actually did the bad things described (4.11, all three conditions averaged).

amount of information about that person," there are also no significant between-condition differences.

It appears, then, that the above alternative explanation is not an adequate one.

Does Increased Commitment to an Initially Unjust Evaluation Make Subjects Less Likely to Over-Compensate for the Misjudgment?

Earlier, we speculated that if we allowed the subject to become too strongly committed to his initial unjust evaluation of the other, we might *not* be able to demonstrate that he would lean over backwards to compensate for an initial injustice. We speculated that strong commitment to an initial misjudgment would probably lead subjects to try to *justify* the misjudgment, rather than to try to compensate for it.

To get some information about whether or not increased commitment produced a decreased tendency in subjects to over-compensate for initial misjudgments and an increased tendency to justify their initial misjudgment, four experimental groups were run.

1. *A small commitment group.* This group received exactly the same information and questionnaires as did E_{111} subjects. However, *in addition*, immediately *after* reading Interview 1, they answered two questions concerning their liking for Jim and their reaction to his killing the cat. Answering these questions thus made these subjects slightly more committed to their unjust evaluation than were E_{111} subjects (who had only misjudged Jim in thought, not in writing).

2. *Moderate commitment group.* This group also received exactly the same information and questionnaires as did E_{111} subjects. However, *instead* of filling out the 15-item questionnaire concerning their reaction to Jim *after* reading the true explanation of his behavior, they filled it out *before* reading the warning.

We felt that answering 15 detailed questions made moderate commitment subjects more committed to their unjust evaluations than were slight commitment subjects, who answered only two questions.

Groups 3 and 4. A slight commitment and a moderate commitment group were run in which subjects committed themselves slightly or moderately to an unjustly *generous* evaluation of Jim.

The reader should remember that these four commitment groups are exploratory groups. They have obvious inadequacies. In addition, fewer subjects were run in the small and moderate commitment groups than were run in the regular experimental conditions.⁷

⁷Since the commitment groups were included in this study for exploratory purposes, only 25-27 subjects were run in the Small and Moderate Commitment conditions.

Our prediction is as follows: The more committed subjects are to their initial misjudgment of Jim, the less they will over-compensate for their misjudgment of him.

Table 3 presents the final estimate of their liking for Jim by subjects in the various commitment and control conditions. We will see from this table that these exploratory groups provide no data to support our speculation that subjects would over-compensate less as they became increasingly committed to an initial misjudgment. When subjects were

TABLE 3
LIKING FOR JIM IN VARIOUS COMMITMENT CONDITIONS

Condition	(N)	Liking for Jim after reading Interviews 2 4
Boy initially evaluated too harshly		
No commitment (experimental E _{th} condition)	(26)	1.62 ^a
Small commitment	(26)	1.27
Moderate commitment	(26)	1.46
Combined control groups (regular and double warning C _{th} conditions)	(41)	(1.26)
Boy initially evaluated too generously:		
No commitment (experimental E _{tg} condition)	(25)	-.26
Small commitment	(25)	-.38
Moderate commitment	(27)	-1.35
Combined control groups (regular and double warning C _{tg} conditions)	(51)	(.03)

^a The means for the no commitment and combined control groups differ from those presented in Table 2, because only those subjects run in 1963 are included in Table 3. (The small and moderate commitment conditions were not run in 1964.)

initially too harsh in their evaluations of Jim, commitment subjects do seem to over-compensate slightly less, but this trend is neither systematic nor significant. When subjects were initially too generous in their evaluations of Jim, commitment subjects show significantly *more* over-compensation than do the regular (no commitment) experimental subjects.

If *anything*, the evidence suggests that increased commitment will produce increased over-compensation. If all the three experimental groups (No Commitment, Small Commitment, and Moderate Commitment) that were run for a given story concerning Jim are combined and compared to the control groups (Single Warning and Double Warning) run for that same story, the over-compensation effect discussed earlier is still clearly demonstrated (Interaction $F = 8.43$, $p < .01$ with 1 and 2.37 *df*).

How can we account for the fact that commitment groups do not seem to be different in any theoretically consistent way from the no commitment (experimental) groups? There are many possibilities—the most obvious being that commitment is not a crucial variable in determining when over-compensation and when dissonance reduction occurs. Our best guess, however, is that the level of commitment that we aroused in this experimental setting was so minimal that commitment groups were not essentially different from the no commitment ones. A subsequent experiment in which commitment subjects are really committed—in which they feel the conclusion they came to was their own responsibility, in which their answers are identified as theirs and their views known to others, or when their actions have some important consequences—will have to be conducted in order to get some information as to whether degree of commitment to the original position determines whether a subject shows an over-compensation or a dissonance reduction reaction. Such an experiment is reported in the study following this one.

In addition, we need some evidence as to which conditions, if any, will induce subjects to make up for unjust *behavior* and which conditions will even induce them to *compound* their injustice to another in an attempt to justify their initial misjudgment. Davis and Jones (1960) demonstrated that if people anticipate being able to see the victim and “explain” their harsh behavior, they will not denigrate him after injuring him. It may also be that if the subject is going to be forced by circumstance to interact with another, he will also “over-compensate” and attempt to restore an equitable relationship rather than reduce dissonance by justifying his unjust behavior.

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